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Circle of fortune: The long-term impact of Western customs institution in China☆

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ABSTRACT

This paper examines the long-run effect of the customs institution founded by the Chinese Maritime Customs (CMC), a foreign-run customs agency established in the mid-19th century in China. A historical natural experiment is exploited to identify a causal effect: in 1902, the CMC took over a number of Chinese Native Customs stations, for which it established clear procedural guidelines and improved transparency and efficiency, while the other Native Customs stations remained under Chinese authority. Using the historical criterion that determined which Native Customs stations were taken over – those located within a 25 km radius of a CMC customs station – as a source of exogenous variation in the quality of the local customs institution, I show that counties historically affected by the CMC institution are more developed today relative to nearby but unaffected areas. Moreover, today's business and bureaucracy in the affected counties suffer less from corruption, suggesting that the long-run effect of institutions can be explained by the cultural norms it fostered in the local society.

1. Introduction

Recent studies on the long-run economic effect of colonial rules stress their influence on today's formal institutions (Acemoglu et al., 2001, 2002; Engerman and Sokoloff, 1997). Nevertheless, colonial institutions were also associated with the cultural norms brought by the early European settlers; values such as the attitude towards the rule of law and the appropriate role of government were essential to the functioning of initial institutions (Nunn, 2012). In light of a growing literature that identifies a cultural effect of historical institutions (Becker et al., 2016; Chen et al., 2020; Guiso et al., 2016; Voigtlander and Voth, 2012, among others), colonialism may similarly affect long-run economic development through its transmission of embedded European cultural norms. This paper examines such an institution operated by the Chinese Maritime Customs (CMC) — a foreign-run customs agency founded in China in the mid-nineteenth century.

China offers an intriguing and appropriate testing ground for investigating the question at hand. China's economy has expanded rapidly since 1978 and currently ranks as the world's largest trading country and the second-largest economy in terms of GDP. While the Reform and Opening-Up in the late 1970s is typically given full credit for the alleged "China's growth miracle", recent studies indicate that China's economic success has much deeper historical roots (Brandt et al., 2014; Chen et al., 2022; Keller et al., 2011), one of which could be the forced opening of Chinese treaty ports in the mid-nineteenth century. By exploiting the externally-imposed trade opportunity and drastic institutional change in the treaty port era as a quasi-experiment, this paper examines the institution of the CMC that governed trade in treaty ports.

The CMC administered customs stations in Chinese treaty ports with an institution historically known for valuing honesty and penalizing corruption (Dai, 1993; Drew, 1913; Foster Hall and Bickers, 2015). From this vantage point, my results highlight a causal relationship between the CMC institution and contemporary economic performance in Chinese regions as measured by nighttime light luminosity. Moreover, I demonstrate that the CMC institution fostered local norms of

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honesty and lawfulness that persisted in the long run. Specifically, business and bureaucracy in places affected by the CMC institution are less corruptible today, suggesting a genuine channel of cultural transmission that explains the persistent effect of a long-gone colonial institution.

Identifying the effect of the CMC institution is challenging because treaty ports might have had various long-run effects on local economies. Thus, a simple comparison between all Chinese regions with and without a CMC customs station would present misleading results. To address this concern, I employed a novel identification strategy based on a historical quasi-experiment: in 1902, CMC customs stations took over nearby stations previously run by the Native Customs — the Chinese customs authorities administering inland trade. Under the CMC rules, the affected Native Customs stations established clear procedural guidelines and improved transparency and anti-corruption measures. Exploiting the variation in the takeover of Native Customs stations enables me to identify the CMC institutional effect in non-treaty port regions. By using a sample of counties neighboring CMC customs stations and fixed effects of CMC neighborhoods, my results show that the CMC institution contributes to a 48% increase in the level of nighttime light; these results are robust even if the distance to the nearest treaty port is controlled for.

The takeover of Native Customs stations can be associated with numerous unobserved factors. Thus, I construct an instrumental variable using the historical criterion that determined which Native Customs stations were taken over, namely, those located within a 25 km radius of a CMC customs station; those located further away remained under Chinese control. This setup allows me to use the share of a county's area covered by *CMC circles* – circular areas around each CMC station with a radius of 25 km – as an instrumental variable. I show, through a series of sensitivity checks, that the IV is orthogonal to pre-colonial economic development and treaty port agglomerations. The instrumented results are consistent with the OLS estimations and robust to a battery of robustness tests that take into account outliers, alternative variables, more demanding sampling procedures, and so forth.

Next, I examine the channel(s) through which the CMC institution indirectly or directly affected today's economic outcomes. First and foremost, I reason that the CMC institution, established to formalize Chinese inland trade, bred a long-lasting culture of honesty and lawfulness that restricted corruption at the local level. Indeed, evidence from a Chinese firm-level data set, the Investment Climate Survey, shows that firms located in counties where the CMC took over Native Customs stations are less corruptible today. Furthermore, the CMC institution explains the anti-corruption effect of a contemporary judicial policy. Starting in 2014, Chinese courts are forced to publish judicial documents on a governmental website. This policy improves judicial transparency in China and thus increases the cost of corruption. Using the number of published corruption cases at the local court level between 2008 and 2019, I show that counties affected by the CMC institution reported fewer corruption cases after 2014 than other counties. This regional variation in corruption suggests a strong anti-corruption effect of the CMC cultural legacy.

I also test for direct economic channels. Using the data from the Annual Survey of Industrial Firms (ASIF) in 2007, I examine inbound foreign direct investment (FDI) at the industry-county level. After applying the IV strategy and industry fixed effects, my results show that industries received more FDI in counties affected by the CMC institution than in other unaffected places. For the rest of the analysis, I investigate other plausible mechanisms, such as institutional quality, education, Confucianism, and conflicts. By using rich historical and contemporary data sources to measure the aforementioned factors, I do not find that the CMC institution generated a long-run effect through these channels.

The remainder of the paper is organized as follows: Section 2 reviews the literature. Section 3 describes the historical background. In Section 4, the data and the empirical strategy are presented. Section 5 shows the baseline results. Section 6 reports the results using the IV strategy. Section 7 shows robustness checks. Section 8 tests the channel of cultural transmission. Section 9 examines alternative mechanisms. Section 10 concludes.

2. Literature

This paper contributes to a large literature that considers institutions as a fundamental cause of long-term economic development (Acemoglu et al., 2001, 2002; Easterly and Levine, 2003; Rodrik et al., 2004). For example, colonial trade institutions played an important role in leading to the current economic prosperity in Latin America. Alvarez-Villa and Guardado (2020) show that colonial ports in Mexico demonstrate less poverty and higher tax revenue than neighboring non-port regions. Ellingsen (2023) finds that lifting restrictions on trade with Europe facilitated market integration and urbanization of a port in the Spanish Empire. My paper demonstrates that the *quality* of trade institutions matters as well by comparing two co-existing inland trade systems that were organized by the same population and served the same purpose, differing only in their administration and procedures of tax collection. Moreover, different from most previous studies that measure institutions as a package of state-level laws and bureaucracy, my paper examines a specific institution with a clear feature that was conducive to economic prosperity.

My paper also adds to the analysis of cultural persistence stemming from institutions in the past. Recently, a growing number of studies examine the effect of historical indigenous institutions on culture. For example, Becker et al. (2016) find that the Habsburg Empire's reputable governance fostered people's trust in public services today. Guiso et al. (2016) show that achieving self-governance in the Middle Ages is important in explaining the current level of social capital in Italian cities.[1] Another stream of studies investigates cultural traits stemming from externally imposed institutions – which are often measured by colonial rules – and finds mostly a "negative" effect of disrupting indigenous traditions. For instance, Nunn and Wantchekon (2011) show that the slave trade decreased trust levels in contemporary African countries. Okoye (2021) documents a similar trust-reducing effect in today's African countries caused by missionary activities under indirect colonial rule. In this light, my paper broadens the spectrum of colonial legacies by revealing a "positive" cultural trait of honesty and lawfulness, which is essential in explaining the level of corruption (as in Fisman and Miguel (2007) and Barr and Serra (2010)). A more recent study, Lowes and Montero (2021), show that the colonial concessions in Congo decreased today's governmental quality due violence and indirect rule, but they also fostered a pro-social cultural norm as a substitute of worse institutions. My paper does not find such a substitution effect: unlike the concessions in Congo, the CMC in China promoted domestic institutional quality, enforcing an equilibrium with compatible cultural norms. This finding sheds new light on the interaction between historical institutions and culture.

Third, this paper speaks to the literature on the historical roots of corruption. Following Becker et al. (2016), my results support the notion that good institutions are essential for the development of a long-lasting anti-corruption culture. Moreover, my results complement the findings of Angeles and Neanidis (2015), who show that the presence of European settlers increased corruption in countries where they constituted a small group of ruling elites. According to my analysis, the CMC institution managed to reduce the corruption level today even though China's colonial era was marked by an extractive colonial system and powerful European elites. One plausible explanation is that the Europeans did not maintain the existing extractive institution – the Native Customs system – to exploit the local population,[2]. Instead, the transmission of the CMC institution formalized the inland trade system

[1] In regions other than Europe, similar studies have also been conducted on historical institutions in Asia (Chen et al., 2020; Dell et al., 2018), and Latin America (Lowes et al., 2017).

[2] As argued by Acemoglu et al. (2002), European colonialism led to the formation of extractive institutions or the maintenance of existing institutions in formerly rich regions.

and benefited a larger share of the indigenous population. Another salient finding of my paper is that the colonial cultural legacy explains why a national policy might curb corruption more effectively in some places than in others, adding to the analysis of the effect of anti-corruption institutions (Bobonis et al., 2016; Chen and Kung, 2019; Ferraz and Finan, 2008; Olken, 2007).

Finally, my analysis is related to a growing body of research on the economic impact of treaty ports and the Western presence in historical China (Bai and Kung, 2015; Jia, 2014a; Keller et al., 2017; Long et al., 2019). The article closest to my paper is Jia (2014a), who documents the long-run effects of treaty ports on economic growth. My paper focuses on one of the important, if not the most important, institutions established in treaty ports, which is the one governing customs and trade. My empirical design allows me to identify the CMC institutional effect while excluding confounding factors stemming from treaty ports. On a broader scale, my findings improve the understanding of China's economic success after 1978. While previous analysis shows that China's modern economic prosperity is likely to be the continuation of human capital accumulation (Chen et al., 2022), business development (Ma, 2008), and trade (Keller et al., 2011) that already took place in the mid-nineteenth century, my paper brings new insight by focusing on the role of culture.

3. Historical background

In this section, I briefly introduce the CMC and the takeover of Native Customs stations. A detailed description of the Native Customs system and the historical scenario of the takeover can be found in Appendix B.

3.1. The Chinese maritime customs service

Since 1757, the Qing government of China had maintained a "Sea Ban" policy and forbade trade in all port cities except for Guangzhou. China's self-imposed economic isolation, however, soon collapsed after its defeat in the First Opium War (1839–1842). With the signing of the Nanking Treaty in 1842, China was forced to open up treaty ports for foreign trade and settlements. The first five treaty ports were Guangzhou, Xiamen, Fuzhou, Ningbo, and Shanghai, with Shanghai becoming the major port of trade. Foreign trade in China was initially organized by the Chinese Native Customs, but its weak institutions led to inconsistent customs treatments among stations and low tariff revenues collected from trade. In 1856, foreign consuls in Shanghai founded the CMC to overcome a temporary vacuum of trade authority caused by the devastating civil war of the Taiping Rebellion. After the war, the CMC persisted and continued to govern Chinese foreign trade.

The establishment of the CMC marked the beginning of China's dual-customs system: the CMC regulated foreign trade brought by Westerners or those who traveled by steamships, whereas the Native Customs processed trade conducted by traditional Chinese vessels, mostly referring to inland trade. A more fundamental difference between the two systems, however, is that the CMC was operated by Westerners. The top position of the CMC, the Inspector General (IG), was mostly occupied by a Briton, who further appointed commissioners to administer customs stations in treaty ports; these higher-ranking officials were usually also foreigners.

The CMC was operated under modern customs rules, which had three distinct features compared to the historical Chinese ones. First was its transparency. The CMC published clear tax codes that were consistent across all treaty ports in China. Second, the CMC had strict administrative procedures. It tightly controlled revenues and expenditures with a European bookkeeping system and offered constant customs treatment for travelers and businessmen. Last but not least, the personnel policy of the CMC was (arguably) free from nepotism and corruption. Passing the customs exam was mandatory before accepting a position in the CMC, regardless of the applicant's nationality and family background. Once becoming an employee of the CMC, honesty was valued as a top virtue while corruption was punished by an immediate discharge.

3.2. Takeover of the native customs

In 1900, China was defeated by an alliance of eight Western countries.[3] Through signing the *Boxer Protocol*, foreign powers imposed a payment of 450 million HK. taels of silver[4] to the Chinese government (Spence, 1991). To ensure that the payment would be properly funded, the protocol permitted the CMC to take over the Native Customs stations from Chinese authorities and collect revenues of Chinese inland trade.

Initially opposed by the Chinese officials, the plan of taking over Native Customs went through a series of discussions between the Chinese government and the CMC. With the agreement made in 1901, each CMC station began to administer Native Customs stations within a radius of 50 *li*[5] at the beginning of 1902, while Native Customs stations outside this radius remained under Chinese authority. According to the record by Huang (1917), 22 CMC stations took over 24 Native Customs stations and 120 substations in total. To efficiently collect revenues from China's inland trade, the CMC transplanted most of its administrative concepts to the Native Customs stations it took over, which included establishing written tax rules, simplifying the administrative procedures, and tackling the problem of corruption and excess staff in Native Customs stations.

Qualitative historical evidence suggests that the transmission of the CMC institution was a success in increasing the inland trade tax collected by the affected Native Customs stations.[6] For example, the station of Santuao collected an annual revenue of 11,000 HK. taels before the takeover; in 1906, that figure had increased to 80,000 HK. taels. The trade revenue collected in two Native Customs stations in Fujian province (Min and Xiamen) doubled after 1902, and Chinese officials reported that the reforms of Native Customs were necessary and effective (Dai, 1989).

3.3. The fall of the CMC system

After the fall of the Qing dynasty in 1911, the CMC continued to manage foreign trade and collect tariff revenues. In 1931, the government abolished all Native Customs stations, whereas the intra-50-*li* stations were completely annexed by the CMC. The CMC survived the Second Sino-Japanese War (1937–1945) and persisted until the foundation of the People's Republic of China in 1949.

Based on the historical experience of having business activities under a stable customs institution, rather than a corruptible one, I hypothesize that the CMC rules positively affected regional economic development today. Furthermore, I argue that norms of honesty and lawfulness – the cultural foundation of the CMC administration – were fostered by the CMC rules and persistently restrict corruption and illegality in local business and bureaucracy, even if the CMC institution ceased to exist after 1949.

4. Data and empirical approach

4.1. Sample

The main data set in this analysis is based on historical counties in the late Qing period of China, and it is accessible through the China

[3] These countries are the United Kingdom, France, the United States, Japan, Russia, Germany, Italy, and Austria-Hungary.

[4] HK. tael was a currency measurement used by customs stations, measuring the weight of silver. One HK. tael of silver weighed approximately 37 g. 450 million Hk. taels was approximately $330 million under the exchange rate at the time.

[5] *Li* is Chinese mile. Fifty *li* are approximately 25 kilometers.

[6] The comparison of revenues collected by Native Customs stations before and after the takeover is only documented in a few CMC archives. In most cases, Native Customs did not have an official bookkeeping system before 1902.

Table 1
Descriptive statistics.
Sources: A: NOAA, B: Huang (1917), Hamashita (1989), and Qi (2004), C: CHGIS V.5, D: Cao (2000), E: CGIAR-CSI, F: Nunn&Puga (2012).

Variable	Definition	Source	Mean	St.d	Obs
(1) Long-run Economic Development					
Light0010	Average nighttime light 2000–2010 (in log)	A	1.436	1.212	116
(2) CMC institution					
CMC	Any Native Customs station taken over by the CMC (dummy)	B	0.138	0.346	116
Total station	Number of total Native Customs stations taken over by the CMC	B	0.310	0.927	116
Main station	Number of Native Customs main stations taken over by the CMC	B	0.043	0.204	116
Sub station	Number of Native Customs sub stations taken over by the CMC	B	0.233	0.204	116
(3) Pre-colonial and geographic conditions					
Dis_CMC	Distance to the nearest CMC station (km)	C	58.002	27.787	116
Dis_capital	Distance to the nearest prefecture capital (km)	C	49.064	26.248	116
Size	County area size (in log)	C	7.447	0.611	116
Density1820	Population density in 1820 at the prefecture level (person/km^2)	D	196.140	161.767	116
Dis_Yangtze	Distance to Yangtze river (km)	C	403.911	338.182	116
Dis_coast	Distance to the coast (km)	C	224.895	282.450	116
Dis_canal	Distance to the Grand Canal (km)	C	606.137	450.875	116
Latitude	Latitude in degree	C	28.258	4.915	116
Longitude	Longitude in degree	C	115,826	4.090	116
(5) IV Specifications					
CMC coverage	Share of area size covered by the CMC Circle	C	0.089	0.154	116
Elevation	Average elevation (in log)	E	−4.739	1.489	116
Ruggedness	Average terrain ruggedness (in log)	F	1.966	1.347	116
River density	Small river density (km/km^2)	C	0.041	0.024	116

Historical Geographic Information System version 6 (CHGIS, 2016), published by the Harvard Yenching Institute. My baseline sample includes counties neighboring a CMC station that took over at least one Native Customs station. To construct the sample, 21 CMC stations that took over Native Customs stations are identified by referring to the books of CMC (1907) and Huang (1917).[7] Then, I match each of the stations to a county where it was located (called the CMC county), including only the direct neighboring counties of each CMC county into my sample. Using a sample of neighboring counties allows me to compare regions with similar geographic and pre-colonial socioeconomic characteristics that differ only in the customs institution. However, the results remain qualitatively unchanged by using a larger data set covering more than 1,500 counties.[8] Finally, I exclude counties where Western powers or the Chinese governments established treaty ports. By doing so, my results are unlikely driven by factors associated with treaty ports such as international trade and foreign investment.

To sum up, my sample includes 116 counties in 11 provinces. Fig. 1 illustrates the baseline sample on a map of historical China, where CMC stations that took over at least one Native Customs station are labeled. Additionally, I mark CMC stations that *did not* take over any Native Customs on the map, which will be examined in a robustness check. The following sections introduce the variables used in this study. The data sources, definitions, and summary statistics of these variables are reported in Table 1.

4.2. Economic development

Long-term economic development is measured by average nighttime light luminosity. The National Oceanic and Atmospheric Administration (NOAA) processed the data from weather satellites of the U.S. Air Force (the Defense Meteorological Program Operational Line-Scan System, or DMSP-OLS). The data are recorded in satellite images taken between 20:30 and 22:00 local time from 1992 to 2012. Each data set contains pixels that represent the light luminosity from 0 to 63 at a resolution of 30 arc seconds (roughly 1 km^2 at the equator). In this paper, I compute the average luminosity across all pixels in each county for every year between 2000 and 2010 and then calculate the county's average luminosity over the period of 2000–2010.

4.3. Takeover of the native customs

I measure the transmission of the CMC institution at the county level with a dummy variable that equals one if any Native Customs station was taken over by the CMC in that county. To construct this variable, I locate all Native Customs stations that were taken over by the CMC using the book of Huang (1917) and two contemporary studies: Hamashita (1989) and Qi (2004). Then, I geo-match these Native Customs stations to the counties in my sample. As a result, 36 Native Customs stations are identified and matched with 16 neighboring counties. The comparison group hence refers to counties where: (1) Native Customs stations did not exist, or (2) the CMC did not take over Native Customs stations.[9]

4.4. Control variables

Treaty port spillover effect. The effect of the CMC institution might simply capture the spillover effect of treaty ports. Considering that the level of the spillover effect is a function of the distance to a CMC station, I control for the distance of each county to the nearest CMC station (*Dis_CMC*) in all regression analyses. In the robustness checks, I include other geographic factors that are associated with the treaty port spillover effect.

Pre-colonial economic conditions. The initial economic conditions are plausibly correlated with both today's economic development and the takeover of Native Customs stations, causing a classic omitted variable bias. To address this concern, I control for: (1) the distance to the

[7] I exclude the CMC station in Kiaochow (in Shandong province) from the analysis. Kiaochow became a German colony in 1898, and its local institutions were vastly different from other Chinese regions. This exclusion results in 21 CMC stations used in this analysis instead of 22 stations mentioned in Section 3.

[8] The analysis can be found in Appendix C.

[9] Unfortunately, data on the location of Native Customs stations (including all substations) at the county level are not available, hence differentiating the two types of counties in the comparison group is not possible. Nevertheless, in the robustness check I include the number of Native Customs stations at the *prefecture* level as an additional control variable and the main results still hold.

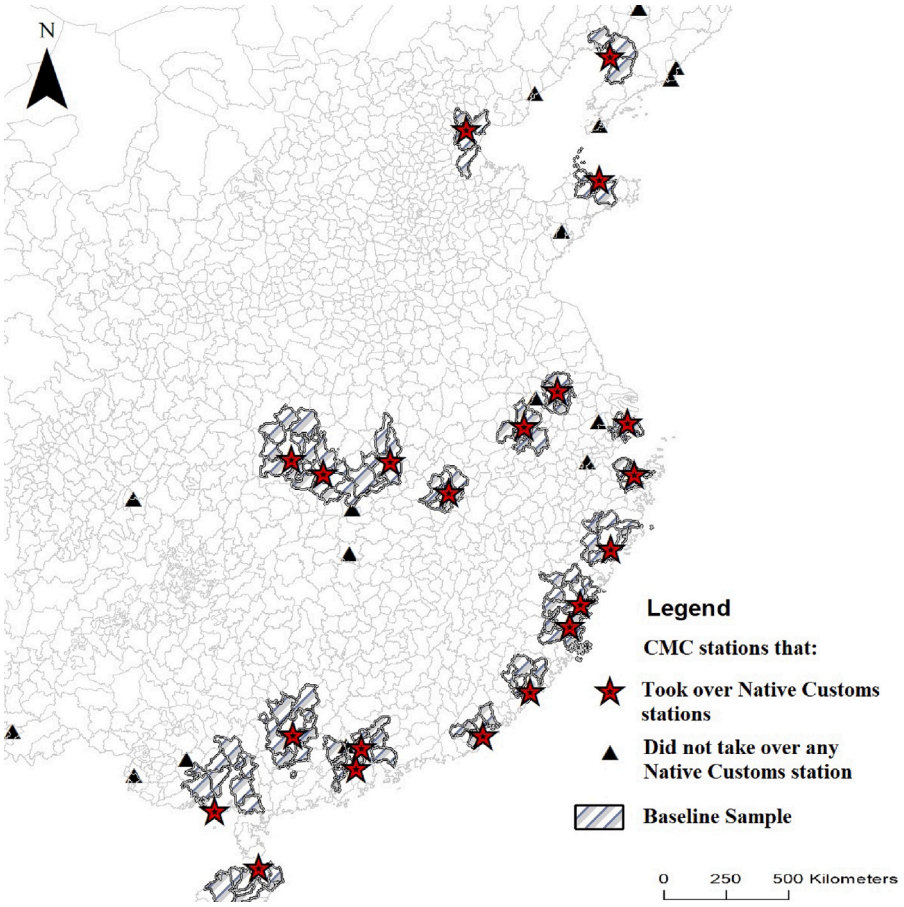

Fig. 1. Treaty ports and sample counties.

nearest prefecture capital; and (1) population density in 1820 at the prefecture level, which is documented by Cao (2000).

Geography and locations. I control for the area (size) of a county because larger counties are potentially more rural and less developed. Access to waterways and maritime transportation are captured by the distance to the coast, to the Yangtze River, and to the Grand Canal; the Yangtze River is the most navigable waterway in China, whereas the Grand Canal was the major waterway for grain transportation. Other unobserved geographic characteristics are captured by latitude and longitude.

4.5. Baseline regression

The county-level regression equation is as follows:

$$Y_i = \beta_0 + \beta_1 CMC_i + \mathbf{X}_i\gamma + \delta_j + \epsilon_i \quad (1)$$

Y_i is the average nighttime light luminosity of county i from 2000 to 2010. CMC_i is the main explanatory variable and is equal to one if at least one Native Customs station was taken over by the CMC in county i after 1902. To ensure that counties influenced by the CMC institution are compared to similar but unaffected regions, I include CMC fixed effects, as denoted by δ_j, for the group of counties neighboring a CMC county j.[10] Thus, my analysis controls for the difference in economic development among treaty port regions. Finally, the error term is denoted by ϵ_i.

[10] For a county neighboring more than one CMC station, I assign the closest CMC station to that county.

5. Results

Table 2 reports the baseline results. In column (1), a simple bivariate estimation with CMC fixed effects shows that the CMC institution is positively and significantly associated with today's economic prosperity. The coefficient of *CMC* remains highly significant after controlling for the distance to the nearest CMC station (column (2)), suggesting that the CMC institution explains the long-run economic development beyond a potential spillover effect of treaty ports. Next, pre-colonial economic conditions and geography are included (columns (3) and (4)) and the coefficient of *CMC* remains significant at the five percent level. Regarding the magnitudes, the preferred specification in column (4) implies that the nighttime light luminosity in counties affected by the CMC institution is greater than the luminosity of other counties by approximately 48%, which explains one-third of the standard deviation of the nighttime light.

As for the spillover effect of treaty ports, *Dis_CMC* is negatively associated with nighttime light, suggesting a positive spillover effect stemming from treaty ports. However, when the full set of geographic controls is included in column (4), the spillover effect is no longer statistically significant. Regarding other control variables, pre-colonial development (as measured by distance to a prefecture capital and prefecture population density) is positively associated with today's economic development, whereas a county's area is negatively related to contemporary development. Finally, Appendix C reports the analysis using a larger sample, and the results remain qualitatively similar.

Using a sample of neighboring counties may raise concern that the estimated effect of the CMC institution is driven by spatial autocorrelations in the regression residuals. However, the insignificant Moran test statistics reported in each column in Table 2 suggest that spatial

Table 2
Baseline results.

Dependent variable: Average nighttime light 2000–2010, in log				
	(1)	(2)	(3)	(4)
CMC	0.811***	0.616***	0.513***	0.392**
	(0.152)	(0.159)	(0.150)	(0.173)
Dis_CMC		−0.010**	−0.007**	−0.004
		(0.003)	(0.003)	(0.003)
Dis_capital			−0.006***	−0.008***
			(0.002)	(0.003)
Density1820			0.002**	0.002*
			(0.001)	(0.001)
Size			−0.221*	−0.180
			(0.123)	(0.121)
Latitude				0.191
				(0.222)
Longitude				0.536**
				(0.208)
Dis_Yangtze				−0.001
				(0.002)
Dis_coast				−0.002
				(0.002)
Dis_canal				0.005*
				(0.002)
Observations	116	116	116	116
R-squared	0.496	0.826	0.707	0.778
Moran test *p*-value	0.689	0.694	0.962	0.611
CMC fixed effects	Yes	Yes	Yes	Yes

Notes: County level regressions with CMC fixed effects. Robust standard errors are reported in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1.

autocorrelation is not a critical issue here. In Appendix D, I discuss the spatial autocorrelation in more detail and show the robustness of my results under different spatial specifications.

Another concern is that extreme values of average nighttime light intensity may exist, and thus my results might be driven by particular observations. Reported in Table A.1, I address the concern of outliers by employing the robust estimation of the baseline model in column (1). Next, I implement a quantile regression model that estimates the conditional median of the average nighttime line over my explanatory variables (column (2)), which provides estimates more robust against outliers. Finally, I take a more direct approach and exclude observations outside the top and bottom 0.5 percentile (column (3)) and one percentile (column (4)) from my sample. The results show that the coefficient of *CMC* remains statistically significant.

6. Instrumented evidence

Whether a Native Customs station was taken over by the CMC is related to a variety of unobserved factors. First, counties affected by the takeover might had been systematically different than the unaffected ones before 1840. Thus, directly comparing the two types of counties may result in biased OLS estimates. Second, the baseline effect might capture the impact of other institutional changes unrelated to the CMC, such as the radical institutional changes during the Republic era (1911–1949) and after 1949. Finally, the variable *CMC* may suffer from measurement errors such that the takeover of a Native Customs stations was inaccurately recorded, attenuating the OLS estimates downward. To deal with these issues, I employ an instrumental variable strategy based on the radius of 25 km around a CMC station for determining the takeover of Native Customs stations, thereby exploiting an exogenous variation in the change in customs institutions at the county level.

6.1. Definition and setup

As described in Section 3, a Native Customs station was subject to the takeover if its distance to the nearest CMC station was smaller than 25 km. Thus, a circular area around each CMC station with a radius of 25 km, called the *CMC circle*, should determine which Native Customs stations were taken over by the CMC: the ones that fell inside the CMC circle. Following this logic, the likelihood that a Native Customs station in a county was taken over should increase with the share of that county's area covered by a CMC circle, premised on the notion that Native Customs stations did not cluster in specific areas. In effect, substations of a Native Customs station were usually established in a scattered way outside the treaty port, spreading at a large distance from the main stations. For example, substations of the Min Customs in Xiamen spread more than a thousand kilometers away from the main station (Qi, 2004). One reason behind such a distribution was to prevent smuggling and tax evasion.

Therefore, I use the share of a county's area covered by the CMC circle, as denoted by *CMC coverage*, to instrument the endogenous *CMC* with the following first-stage regression:

$$CMC_i = \theta_0 + \theta_1 CMC\ coverage_i + \mathbf{X}_i\pi + \delta_j + \epsilon_i \quad (2)$$

To ensure that the IV does not simply capture other unobserved geographic factors, **X** includes additional geographic variables. First, elevation and terrain ruggedness are controlled for to capture general agricultural potential and transportation conditions. They are obtained from the digital elevation model from NASA's Shuttle Radar Topography Mission (SRTM) and Nunn and Puga (2012), respectively. Second, I control for small river density; a denser river network is plausibly correlated with better waterway transportation. Finally, I additionally include a quadratic polynomial of latitude and longitude (x^2, xy, and y^2).

6.2. Testing exclusion restrictions

For the IV strategy to identify a causal relationship, the 25 km radius must be exogenous. For example, if the CMC officials knew the location of every Native Customs station and intended to take over the ones that performed better by fabricating the condition of the takeover, the IV strategy would not be valid. However, as presented in Appendix B, this is unlikely to be the case. First, the radius was unilaterally provided by top Chinese officials who had little incentive to hand over important Native Customs stations to a foreign-run customs agency, and thus, the CMC circle was unlikely to create a "catchment area" of better-developed regions. Second, the CMC officials also considered the circle

as an arbitrary boundary, indicating that the CMC could not determine which Native Customs stations to take over.

Given the arguably arbitrary nature of the 25 km radius, the advantage of using *CMC coverage* as an IV is that, after controlling for the distance to the nearest CMC station and other co-determinants of long-run development, *CMC coverage* depends primarily on the relative position between a county and a CMC station, which is exogenously determined by geography. However, the IV could be correlated with other co-determinants of long-term development. Here, I provide three pieces of evidence to show that this is very unlikely.

6.2.1. Pre-colonial development

To perform the first test, I regress proxies of pre-colonial development on *CMC coverage*, controlling for the distance to the nearest CMC station; a significant coefficient of *CMC Coverage* implies that the instrument affects economic development through other channels. The first two columns of Table 3 test for pre-colonial urban development. In column (1), urbanization is measured by a dummy variable of pre-1840 urban status generated by Rozman (1973), whereas column (2) uses the distance to the nearest prefecture capital as the outcome variable. The results show that *CMC coverage* is uncorrelated with the two measurements.

To measure the agricultural side of the Chinese pre-1840 economy, I report results regarding a variety of crops grown in China measured by their suitability indexes. First, traditional Chinese staple foods, namely wheat, rice, and foxtail millet, are examined (columns (3)–(5)). Second, I examine the two major New World crops of maize and sweet potatoes (columns(6) and (7)) because the adoption of New World crops in China was associated with higher population growth (Chen and Kung, 2016) and fewer peasant revolts (Jia, 2014b). Third, sugarcane, tea, and cotton are typical Chinese cash crops and could be strongly correlated with economic prosperity (columns(8)–(10)). The IV, however, is unrelated to any of these measurements.

The level of the treaty port spillover effect can be affected by many factors. In particular, how close a neighboring county is geographically close to, and therefore economically connected with, a treaty port can be particularly relevant. In columns (11)–(14), I test if the IV is associated with the connectivity between a CMC county and its neighboring counties. In column (11), the outcome variable *Courier connect* equals one if a neighboring county is connected to a CMC station via a historical courier road. In the next two columns, *Small(Major) river connect* equals one if a small(major) river in a neighboring county also went across a CMC county, whereas *Any river connect* in column (14) switches on if either of two aforementioned variables equals one. Finally, a county that shared a longer boundary with a CMC county could be more closely connected to a treaty port. Thus, I calculate the share of the joint boundary with adjacent CMC counties for each county in my sample, namely *% share boundary*, and use it as the dependent variable in column (15). The empirical results, however, show that the IV is associated with none of the connectivity measurements. Summing up, the IV is orthogonal to a wide range of variables that measures pre-colonial development and a county's closeness to a CMC station and thus confidently supports the exclusion restriction.

6.2.2. Ring-level analysis

The second test for the exogeneity of the CMC circle is through the comparison of areas barely inside and outside the boundary of the circle. Specifically, I draw an inner ring area and an outer ring area alongside the boundary of each CMC circle. An outer ring area expands from 25 km to 28 km beyond the circle, while an inner ring area goes from 22 km to 25 km within the circle. Thus this exercise examines a sample of 42 ring areas drawn from the 21 CMC stations.

Pre-colonial development at the ring level is measured by the number of towns in 1820. To account for the fact that inner rings are on average smaller than outer rings, I calculate the density of historical towns (number of towns per 100 km^2) as the outcome variable. The main independent variable, *inner*, equals one if the ring is an inner ring. As reported in column (1) of Table 4, the difference in town density between inner and outer ring areas is statistically insignificant. The results are robust to the inclusion of a ring's area (column (2)) and port-level characteristics, such as whether the treaty port was a prefecture capital (column (3)) and the coordinates of the treaty port (column (4)), thus reaffirming previous findings that the IV is exogenous[11].

Table 3
Testing exclusion restrictions.

DV	Urban	Capital	Millet	Wheat	Rice
	(1)	(2)	(3)	(4)	(5)
CMC Coverage	−0.048	0.018	−0.010	−0.505	−0.190
	(0.187)	(0.219)	(0.723)	(0.532)	(0.400)
R-squared	0.027	0.007	0.024	0.013	0.010
	Maize	Sweet potato	Sugar	Tea	Cotton
	(6)	(7)	(8)	(9)	(10)
CMC Coverage	0.622	−0.135	−0.829	−0.0608	0.412
	(0.583)	(0.626)	(0.953)	(1.209)	(0.541)
R-squared	0.012	0.093	0.069	0.137	0.006
	Courier connect	Small river connect	Major river connect	Any river connect	% share boundary
	(11)	(12)	(13)	(14)	(15)
CMC Coverage	−0.042	−0.170	−0.247	−0.420	0.053
	(0.352)	(0.377)	(0.327)	(0.361)	(0.109)
R-squared	0.002	0.006	0.111	0.070	0.078
Observations	116	116	116	116	116

Notes: County level OLS regressions. All estimations control for the distance to the nearest CMC stations (*Dis_CMC*). Standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table 4
Ring-level analysis.

DV: Town density in 1820 at the ring level				
	(1)	(2)	(3)	(4)
Inner ring	0.149	0.148	0.164	0.162
	(0.146)	(0.151)	(0.148)	(0.150)
Size		−0.012	0.131	0.119
		(0.265)	(0.276)	(0.283)
Capital			−0.243	−0.246
			(0.154)	(0.155)
Latitude				0.00140
				(0.0166)
Longitude				0.0196
				(0.0166)
Observations	42	42	42	42
R-squared	0.025	0.025	0.086	0.118

Notes: Ring level regressions. Each observation is either an inner ring area or an outer ring area based on a CMC circle. The dependent variable is the density of towns in 1820. *Inner ring* is the indicator for inner ring area. *Size* is the size of a ring area in log form. *Capital* indicates whether the ring is drawn from a prefecture capital. *Latitude* and *Longitude* are geographic coordinates of the treaty port from which the ring is drawn. Robust standard error are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

[11] A CMC circle determined which Native Customs station was to be administered by the CMC but not necessarily the area in which the station operated. Thus, I do not hypothesize that *contemporary* economic prosperity differs at the boundary of a CMC circle. In Appendix E I employ a *fuzzy* regression discontinuity design that exploits the discontinuity in the probability of taking over Native Customs stations at the boundary of a CMC circle.

Table 5
Placebo analysis.

Dependent variable: Average nighttime light 2000–2010, in log				
	(1)	(2)	(3)	(4)
Placebo coverage	0.627	−0.675	−0.740	−0.465
	(0.950)	(0.884)	(0.878)	(0.875)
Observations	82	82	82	82
R-squared	0.788	0.834	0.846	0.878
Spillovers	No	Yes	Yes	Yes
Socioeconomic controls	No	No	Yes	Yes
Geographic controls	No	No	No	Yes
CMC fixed effects	Yes	Yes	Yes	Yes

Notes: County level regressions with CMC fixed effects. Sample includes counties neighboring treaty ports that did not take over any Native Customs stations. *Spillovers* indicates the inclusion of *Dis_CMC*. *Socioeconomic controls* include *Dis_Capital*, *Density 1820* and *Size*. *Geographic controls* include *Latitude*, *Longitude Dis_Yangtze*, *Dis_Coast*, and *Dis_Canal*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

6.2.3. Placebo analysis

A final threat to the IV strategy is that *CMC coverage* might proxy for other treaty port-specific characteristics that are unrelated to the CMC institution. For example, if the IV is correlated with the agglomeration force of treaty ports, the takeover of Native Customs stations might not be the only channel through which the IV had an effect. To address this concern, I established a placebo sample of counties adjacent to CMC stations that did *not* take over any Native Customs station.[12] By drawing *placebo circles* with a radius of 25 km around these CMC stations, I calculate the share of area of each county in the placebo sample covered by the *placebo circles* and estimate the following reduced-form regression equation:

$$Y_i = \sigma_0 + \sigma_1 Placebo\,coverage_i + \mathbf{X}_i\rho + \delta_j + \epsilon_i \quad (3)$$

The dependent variable is the nighttime luminosity, and the control variables in $\mathbf{X}$ are the same as in Eq. (2). The independent variable of interest, *Placebo coverage*, captures the geographic coverage of a CMC station that did not take over any Native Customs station in the surrounding area. Thus, a positive and significant coefficient σ suggests that the IV might have an effect on economic development other than taking over Native Customs stations, which invalidates the exclusion restriction.

The results in Table 5, however, show that the *Placebo coverage* is in effect unrelated to long-run development. When additional control variables are included in columns (2)–(4), the coefficient even becomes negative, which further rejects the possibility that the IV had a direct economic effect. Combining all evidence from previous analyses leads to the conclusion that the IV is unlikely to have an effect through channels other than the takeover of Native Customs stations, and the exclusion restriction is thus likely to hold.

6.3. Instrumented results

Table 6 reports the results of the IV estimation. Starting with the first-stage regressions, column (1) includes only CMC fixed effects whereas column (2) includes the full set of control variables. In both columns, *CMC coverage* positively and significantly predicts the takeover of the Native Customs stations at the county level. The coefficient 1.002 in column (1) indicates that a one standard deviation increase in *CMC coverage* increases the probability of taking over Native Customs stations by 15 percentage points; in column (2), the estimated coefficient of *CMC Coverage* changes only slightly after including control variables. Moreover, the first-stage F statistics reported in both columns are greater than 10, indicating that a weak IV bias is unlikely to be a major issue.

Table 6
Instrumented results.

DV:	CMC		Nighttime light			
Estimation	First stage		Reduced form		Second stage	
	(1)	(2)	(3)	(4)	(5)	(6)
CMC Coverage	1.002***	1.022***	2.054***	1.153**		
	(0.308)	(0.277)	(0.502)	(0.474)		
CMC					2.049***	1.129**
					(0.451)	(0.452)
Dis_CMC		0.002*		0.001		−0.001
		(0.001)		(0.004)		(0.003)
F-stats	10.551	13.582				
Observations	116	116	116	116	116	116
R-squared	0.454	0.588	0.809	0.895	0.717	0.853
Controls	No	Yes	No	Yes	No	Yes
CMC fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: County level IV regressions with CMC fixed effects. Columns (1)–(2) report first-stage results, columns (3)–(4) report reduced-form results, and columns (3)–(4) report second-stage results. Control variables include: *Dis_CMC*, *Dis_Capital*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Next, I present reduced-form results in columns (3) and (4). As indicated by the positive and significant coefficients of *CMC Coverage* in both columns, the more a county's area covered by a CMC circle, the higher the nighttime light intensity today. Finally, the second-stage IV estimates are reported in columns (5) and (6). After instrumenting *CMC* with *CMC coverage*, I show that the takeover of Native Customs stations positively and significantly affects long-run economic development. In column (6), for example, the coefficient of instrumented *CMC* is 2.049 and significant at the one percent level. Including the full set of control variables reduces the coefficient to 1.129 (column(6)), but it remains significant at the five percent level.

The estimated effect of the CMC institution is on average larger in the IV estimations than in the OLS specifications, indicating a potential underestimation in baseline regression. As discussed earlier, measurement errors could be a plausible explanation, which can be alleviated by introducing an IV with errors orthogonal to the errors in *CMC*. Another concern is the violation of exclusion restrictions, but this is unlikely to be the case given the analysis in the previous subsections. Finally, the IV regression in this analysis might estimate a local average treatment effect (LATE). The LATE captures the effect on "compliers", i.e., a subset of counties affected by the CMC institution simply due to a high *CMC coverage*. For instance, counties with higher *CMC coverage* might have multiple Native Customs stations taken over by the CMC. Hence, it is not surprising that the IV estimates are larger than the OLS estimates.

7. Robustness checks and extensions

In this section, I examine whether the results are sensitive to outlier treaty ports, alternative measurements of key variables, and modifications to the sample.

7.1. Outliers

Are the results potentially driven by particular treaty ports that developed faster than other cities? To address this concern, I consider Shanghai, Tianjin, Guangzhou, and the "first wave" treaty ports[13] as outliers and exclude counties neighboring these treaty ports in my analysis.

[12] The location of these stations is labeled in the map presented in Fig. 1.

[13] These include Guangzhou, Xiamen, Fuzhou, Ningbo, and Shanghai.

Table 7
Excluding outliers.

Dependent Variable: Average nighttime light 2000–2010, in log					
Outliers:	Baseline	Shanghai	Tianjin	Guangzhou	First-wave
	(1)	(2)	(3)	(4)	(5)
CMC	1.129**	1.115**	0.984**	1.176***	2.127***
	(0.452)	(0.461)	(0.394)	(0.450)	(0.665)
Observations	116	111	112	111	91
R-squared	0.853	0.826	0.864	0.850	0.722
Controls	Yes	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes	Yes

Notes: County level IV regressions with CMC fixed effects. In columns (2)–(4), neighboring counties of Shanghai, Tianjin, and Guangzhou are excluded from the sample, respectively. In column (5), neighboring counties of Guangzhou, Xiamen, Fuzhou, Ningbo, and Shanghai are excluded. Control variables include: *Dis_CMC*, *Dis_Capital*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table 8
Sensitivity to alternative variables.

Panel A: Alternative measurements of *CMC*				
	Baseline	Number of Native Customs	Number of Main stations	Number of Sub stations
	(1)	(2)	(3)	(4)
CMC	1.129**	0.315**	2.231*	0.407**
	(0.452)	(0.147)	(1.261)	(0.194)
R-squared	0.830	0.861	0.797	0.867
Panel B: Alternative measurements of pre-colonial development				
	Prefecture Capital (0/1)	Town Density 1820	Urban pre 1840 (0/1)	Number of Native Customs
	(5)	(6)	(7)	(8)
CMC	1.053**	1.101**	1.063**	1.092**
	(0.481)	(0.529)	(0.477)	(0.467)
Pre-colonial development	0.0972	−5.123	0.0360	−0.0410
	(0.227)	(20.58)	(0.174)	(0.0275)
R-squared	0.855	0.851	0.854	0.855
Observations	116	116	116	116
Controls	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes

Notes: County level IV regressions with CMC fixed effects. Column (1) is the baseline IV regression. Columns (2)–(4) use the number of all Native Customs stations, the number of main stations and the number of sub stations that were taken over by the CMC as the independent variable, instrumented by *CMC coverage*. Columns (5)–(8) replace *Dis_Capital* with prefecture capital dummy, town density in 1820, city dummy defined by Rozman (1973), and the number of pre-colonial Native Customs stations at the prefecture level, respectively. Control variables include: *Dis_CMC*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

First, Shanghai was the most populous treaty port in China and processed the majority of China's trade (CMC, 1904; Keller et al., 2011). Moreover, the International Settlement of Shanghai was the most developed Concession in China,[14] which was also considered to have a long-lasting impact on Shanghai's urbanization and economic prosperity (Fei, 1991; Ma, 2008). In column (2) of Table 7, I exclude counties neighboring Shanghai. The estimated coefficient of the *CMC*, however, decreases slightly and remains significant.

Second, Tianjin was the largest treaty port in northern China due to its vital strategic position and well-established trade network along the Hai River. In column (3), I exclude counties neighboring Tianjin from the sample, and the estimated coefficient reduces to 0.984 with the statistical significance unchanged.

Third, Guangzhou was the only port that permitted foreign trade during the Sea Ban period (1757–1842). With the experience in trade accumulated for nearly a century before the forced openness, Guangzhou may exhibit an initial advantage in trade after 1842. In column (4), I exclude counties neighboring Guangzhou, and the estimate slightly increases to 1.176 and remains significant. Even if all of the counties affected by the three aforementioned treaty ports – Shanghai, Tianjin, and Guangzhou – are simultaneously excluded, the effect of the CMC institution remains stable (not shown in this table).

Finally, the first five coastal treaty ports that opened in 1842 are considered outliers for their pre-1840 population size (Jia, 2014a; Bai and Kung, 2015) and access to maritime transportation. In column (5), I exclude all counties neighboring the "first wave" treaty ports. The estimated coefficient increases to 2.127 and becomes significant at the one percent level.

7.2. Alternative variables

This subsection examines whether the results are sensitive to the inclusion of alternative variables. First, I use the nighttime light in years between 2000 and 2010 separately as independent variables. As reported in Table A.2, the estimated coefficient of *CMC* is positive and significant. In addition, the size of the coefficient does not increase over time, suggesting that my baseline result does not capture a growing trend of China's regional economy during the period of the study.

Second, I check whether using alternative measurements of the CMC institution makes a difference. In panel A of Table 8, I use the total number of Native Customs stations taken over in a county in column (2), and the estimated coefficient remains positive and significant. Taking over one more Native Customs station increases today's nighttime luminosity by approximately 37%. Columns (3) and (4) use the number of main and substations taken over by the CMC, respectively. While the estimated coefficient is positive and significant for both measurements, the effect is stronger when taking over a main station is considered as a valid treatment; this is not surprising as main stations administered the majority of trade and were subject to stricter regulations under the CMC.

Third, I show that the results are robust to different measurements of pre-colonial development by replacing *Dis_Capital* with alternative variables. In column (5) of Table 8, I replace *Dis_Capital* with a dummy variable for prefecture capital. In column (6), I use historical town density. In column (7), I use a dummy variable for indicating urban areas generated by Rozman (1973). Lastly, I control for the number of pre-1840 Native Customs stations at the prefecture level (column (8)); information on pre-colonial Native Customs stations is obtained from a large collection of gazetteers at the prefecture, county, and customs levels. The results show that the coefficient of *CMC* remains robust with all alternative measurements of pre-colonial development.

Fourth, using alternative measurements of treaty port spillovers does not affect my main results. For example, in column (1) of Table A.3, I measure the distance to the nearest treaty port from the political center, rather than the geographic centroid, of a county.[15] Next, I consider only the distance to the closest first-wave treaty ports (column (2)) and concessions (column (3)). In column (4), I use a

[14] Concessions were foreign enclaves inside Chinese cities. There were 10 cities where Concessions were formally established: Tianjin, Shanghai, Suzhou, Zhenjiang, Jiujiang, Hangzhou, Xiamen, Wuhan, Chongqing, and Guangzhou. See Fei (1991) for a more detailed description of Concessions in China.

[15] The political center of a county is also called county seat. In case a county seat is not identified in CHGIS (2016), I assign the contemporary city center of the county as the political center.

contemporary measurement of the distance variable by obtaining the shortest driving distance between a county and its closest treaty port city from the *Baidu Map* website. Regardless of the choice of the distance variable, the coefficients of the CMC remain significant.

In columns (5)–(7), I allow for the spillover effects of treaty ports to be nonlinear. I use tertile dummies of the distance variable in column (5), quartile dummies in column (6), and quintile dummies in column (7). The results show a monotonic decrease in luminosity as counties' distance from a CMC station increases. More importantly, the estimated effect of the CMC institution remains positive and significant to the inclusion of different sets of quantile dummies. The overall findings in Table A.3 suggest that the effect of CMC institution is independent from the treaty port spillover effects.

Finally, my results are robust to the inclusion of additional control variables. In Table A.4, I include variables used in Section 6.2 one at a time in columns (1)–(13). Specifically, agricultural suitability indexes are included in columns (1)–(8), and the connectivity between a CMC county and a neighboring county is controlled for in columns (9)–(13). In column (14), all aforementioned control variables are included.[16] The results show that the coefficient of *CMC* remains positive and significant in all estimations.

7.3. Samples and sample balance

Using alternative samples does not affect my main findings. First, I restrict the sample to counties where Dis_CMC is between the 5th and 95th percentile. Second, I use a sample of counties where CMC circles crossed, i.e. counties where *CMC coverage* is greater than zero. Lastly, I examine whether my main results are robust after merging the baseline and placebo samples used in Section 6.2. In the combined sample, I code the variable *CMC* as zero for counties that appear only in the placebo sample. Reported in Table A.5, the coefficient of CMC remains positive and statistically significant in all columns, which reaffirms the robustness of my results to using alternative samples.

Counties affected by the CMC institution may exhibit a different geographic and socioeconomic profile than other regions, potentially biasing the results. As shown in Table A.6, the affected counties were on average closer to CMC stations than the other counties. Moreover, they were smaller in area and closer to prefecture capitals and the coast (insignificant differences are not reported). To address this concern, I improve the balance of my sample by matching counties affected by the CMC institution with the unaffected ones based on the variables listed in Table A.6.

By applying radius and kernel matching techniques with the common support condition, Table A.6 yields reassuring outcomes, as both matching methods generate samples of highly comparable counties, and the differences in all matching variables between the two groups of counties become insignificant and quantitatively small.[17] In addition to applying PSM, I employ entropy balancing following Hainmueller (2012)[18] in column (6), and the balance of the sample improves even more. Next, I re-estimate the IV regression with the weights generated by the aforementioned balancing techniques. Doing so enables me to obtain estimates that are robust to both observed heterogeneity and endogeneity. Columns (1) and (2) of Table A.7 report the results of IV estimation using the kernel matching technique. The estimated coefficient of *CMC* in column (2) is 0.632, which is smaller than the IV estimate in Table 6, indicating that county-level heterogeneities might account for part of the CMC institutional effect. Nevertheless, the effect remains significant at the five percent level. Using a radius matching technique and entropy balancing yields a quantitatively similar result.

7.4. Short-run effect of the CMC institution

In this subsection, I investigate the short-run economic impact of taking over Native Customs stations. By formalizing the procedure of tax collection and improving transparency in the administration, the CMC institution greatly reduced transaction costs of inland trade regulated by Native Customs stations. Thus, the taking over of Native Customs stations in 1902 may very well have increased the transportation of goods between treaty ports and interior regions with a subsequent impact on international trade.

To test this hypothesis, I examine a panel data set at the CMC station-year level. In particular, I obtain CMC export statistics documented by 29 CMC customs stations[19] from 1898 to 1900. Then, I employ a diff-in-diff framework to identify the causal impact of taking over Native Customs stations on trade with the following equation:

$$Y_{it} = \tau_0 + \tau_1 T_i * P_t + \tau_2 D_{it} + \tau_3 S_j * P_t + \tau_4 M_j * P_t + \tau_5 R_j * P_t + I_i + I_t + I_{rt} + \epsilon_{it} \quad (4)$$

where subscripts i, t, and j represent a CMC customs station, a year, and a prefecture where the station is located, respectively. Y is the value of goods exported through a customs station to foreign countries in a year measured by *HK Tael*. T is a dummy variable that equals one if a CMC station ever took over Native Customs stations. P is an indicator that switches on after 1920 — the year after which some of the Native Customs stations were taken over by the CMC. D is the duration of time since the opening of the CMC station. Next, I control for common determinants of trade. S is the population size at the prefecture level in 1880. M is the distance to the closest provincial capital and captures access to the domestic market. R captures natural endowments and represents the agricultural suitability of cotton and tea. These indicators are interacted with the treatment dummy P. Finally, I control for customs, year, and region[20] times year fixed effects; these are denoted by I_i, I_t, and I_{rt}, respectively.

As reported in column (1) of Table 9, CMC stations that took over Native Customs stations after 1902 processed on average more exports compared to other CMC stations as demonstrated by the positive and significant coefficient of $T * P$. Controlling for other co-determinants of trade – population size, access to the domestic market, and endowment – increases the coefficient from 1.211 to 1.407 (column (4)). To examine whether the growth of trade between the two types of CMC stations had already diverged before 1902, I use placebo treatment years of 1901 and 1900 in columns (5) and (6). The results, however, show no significant differences, indicating that the common trend assumption is likely to hold. To sum up, my results suggest that taking over Native Customs stations had an immediate effect on international trade.

8. Channel of honest and lawful norms

This section shows that the persistent effect of the CMC institution can be explained by the long-lasting norms of honesty and lawfulness it fostered. Operated under the CMC rule, the Native Customs stations abolished some of the old practices, such as buying off tax collectors or middlemen for a favorable tax rate. Instead, being honest and lawful was encouraged in the new system, which restricted wrongdoing

[16] Only *Any river connect* is controlled in column (14) due to apparent collinearity with *Major river connect* and *Small river connect*.

[17] In particular, the differences in area size and longitude demonstrate the greatest reduction of around 100%.

[18] Entropy balancing uses a smooth reweighting scheme that matches the distribution of covariates between the treatment and control group for all predetermined moments. Thus, it provides a highly-balanced data set while preventing potential information loss (e.g., assigning a weight of zero).

[19] Among all 37 CMC stations for which trade statistics are available, I exclude four stations opened after 1901, three stations operated in colonies, and one station operated in Tibet.

[20] Regions are defined by the CMC and are categorized as: northern ports, southern ports, coastal ports, Yangtze ports, and inland ports.

Table 9
The effect of CMC institution on historical trade.

Dependent variables: Export in log						
	Baseline				Placebo	
	(1)	(2)	(3)	(4)	(5)	(6)
T*P	1.211*	1.225*	1.317**	1.407**	1.063	0.591
	(0.625)	(0.641)	(0.656)	(0.652)	(0.690)	(0.800)
Duration	Yes	Yes	Yes	Yes	Yes	Yes
S*P	No	Yes	Yes	Yes	Yes	Yes
M*P	No	No	Yes	Yes	Yes	Yes
R*P	No	No	No	Yes	Yes	Yes
Customs FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region*Year	Yes	Yes	Yes	Yes	Yes	Yes
Observations	258	258	258	258	258	258
R-squared	0.263	0.275	0.277	0.295	0.273	0.264

Notes: Diff-in-diff regressions using a panel data set at the customs station-year level. T equals one if a CMC customs station has ever taken over any Native Customs station, and P stands for years after 1902. S is the population density of the prefecture where the CMC station was located. M is the distance to the closest provincial capital. R represents agricultural suitability of cotton and tea at the prefecture level. All regressions include customs, year, and region times year fixed effects. Standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

of public officials and businessmen and facilitated a stable business environment for the Chinese population. This local equilibrium of social interactions could be highly persistent even if the formal CMC institution ceased to exist after 1949. In the rest of this section, I demonstrate the persistence of such a cultural trait measured by corruption outcomes. Moreover, I show that the underlying cultural legacy fostered the anti-corruption effect of a contemporary judicial policy.

8.1. Firm-level evidence

Customs institutions matter for firm behavior and business-government relationships. Thus, I estimate the impact of the CMC institution on restricting corruption and illegality at the firm level. Specifically, I hypothesize that firms in places that were historically affected by the CMC institution are less likely to use corruption or other illegal means to achieve material or non-material gains.

To test this hypothesis, I use the Investment Climate Survey conducted jointly by the World Bank and the National Bureau of Statistics of China. To match firms in the survey to historical counties in my baseline sample, I use the first survey round conducted between 2002 and 2004 for its larger sample size and finer geographic reference,[21] resulting in a sample of approximately 950 firms used in this exercise.

The first outcome variable, *Informal payments*, is a dummy variable equal to one if the answer to the question *"Is there a need for informal payment to staff from the banks or loan-providing institutions?"* is yes. The second outcome variable is based on the question *"How many days does the GM or Vice GM spend on the government assignments and communications per month?"*.[22] While options are categories of different ranges of days, I create a dummy indicating less than a week per month for a better interpretation of the results. Time spent with officials can be a good indication of corruption because building networks with officials takes time and usually relies on illegal payments. My third outcome variable is from the question *"Does your company usually sign formal contracts with the client/supplier?"*. The use of informal contracts is usually motivated by illegal purposes such as corruption and tax evasion, which strongly signifies the illegality of a firm. Here, I code a company as using formal contracts if it usually signs formal contracts with both clients and suppliers.

To examine the effect of the CMC institution at the firm level, I regress the aforementioned outcome variables on *CMC*, which equals one if a firm is located in a county historically affected by the CMC institution. It is further instrumented by *CMC coverage*. I also control for firm-level characteristics such as firm age, number of employees, net assets, shares of state capital, shares of foreign capital, and sector dummies.[23] To address the possibility that city-level policies and investment environment affect firms' behavior, I control for city dummies in addition to CMC fixed effects.

As reported in columns(1)–(3) of Table 10, the CMC institution is negatively associated with firms' illegal behaviors and corruption today. Specifically, in places affected by the CMC institution, firm managers are less likely to make informal payments (column (1)), less likely to spend longer time with governmental officials (column (2)), and more likely to use formal contracts in business (column (3)). These results are robust to the inclusion of firm-level controls and city fixed effects.

The effect of the CMC institution might be more salient on domestic firms than on foreign firms because the former should had have a longer exposure to the local culture. Additionally, the effect on domestic private firms can be of particular interest. Private firms usually face tighter regulations and more red tape than state-owned or foreign firms, making the use of corruption a common practice to evade legal restrictions (Jiang and Nie, 2014; Poncet et al., 2010). However, such firms may be able to more flexibly adapt to the local culture and business environment, exhibiting a stronger anti-corruption norm. To define the ownership type of firms, I use the information on a firm's actual capital shares; a firm is considered to be a domestic firm if the domestic capital shares of the firm dominate its foreign capital shares, while private firms are those whose private shares dominate other shares.

In columns (4)–(9) of Table 10, I find that the effects of the CMC institution on firms are indeed heterogeneous. Columns (4)–(6) show that the CMC institution significantly reduced firms' corruption behavior when examining a sample of domestic firms. In particular, compared to effect in the full-sample estimates, the effect is stronger in reducing interactions with officials (column (5)) and the use of informal contracts (column (6)). Further examining a sub-sample of private firms provides a sharper result: columns (7)–(9) show that the estimated coefficient of CMC increases in both magnitude and statistical significance. Summing up, ownership type may be important to explain the heterogeneous effects of the CMC institutions on firms' corruption.

8.2. CMC institution, judicial policy, and anti-corruption

In this subsection, I show that the cultural legacy of the CMC institution is important to explain the anti-corruption effect of today's judicial policy. Since assuming power in 2012, Chinese President Xi Jinping has promoted transparency in administration and the judicial system.[24] As a major component of the judicial reform under Mr. Xi's leadership, the Chinese Supreme Court announced a regulation in 2014 that forced local courts to publish verdicts and other types of judicial

[21] The first round covers 12,400 firms, while the second round, also called the Enterprise Survey, covers only 2,700 firms and no longer provides the location of firms at the county level.

[22] As documented in the survey, "government assignments and communications" refers to maintaining the relationship with governmental officials, consolidating and submitting various reports or statements, etc.

[23] Employees, assets, and capital shares are calculated as the yearly average between 2002 and 2004.

[24] According to the speech Mr. Xi made on 23 February 2013 in the meeting of the Politburo, the highest policy-making body of the Chinese Communist Party, "judicial professionals should keep close contact with the people, standardize judicial practices, and promote the degree of judicial transparency".

Table 10
The effect on Firms' corruption and illegality.

	Full sample			Domestic firms			Private firms		
DV:	Informal payment	Fewer gov. interactions	Formal contracts	Informal payment	Fewer gov. interactions	Formal contracts	Informal payment	Fewer gov. interactions	Formal contracts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CMC	−0.228**	0.143*	0.294*	−0.210**	0.228*	0.416*	−0.491**	0.269**	0.710***
	(0.103)	(0.0821)	(0.154)	(0.0925)	(0.127)	(0.225)	(0.248)	(0.131)	(0.269)
Observations	878	934	951	624	673	682	324	349	355
R-squared	0.091	0.100	0.161	0.106	0.128	0.192	0.177	0.262	0.245
CMC fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Firm-level IV regressions using data from World Bank's Investment Climate Survey 2005. Sample includes firms located in counties of the baseline sample. Control variables include: *Dis_CMC*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Firm controls include: firm age (in log), average employment 2002–2004 (in log), average net assets 2002–2004 (in log), industry dummies, shares of state capital, and shares of foreign capital. Robust standard errors adjusted for clustering at the county level are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

decisions on a governmental website: *wenshu.com*. On this website, judicial documents can be searched by keywords and downloaded for free.[25] The publication of judicial documents has attracted great attention from society: by 2020, the website had published over 100 million documents and was visited over 50 billion times.

The compulsory publication of judicial documents could potentially deter corruption by increasing legal transparency and the cost of corruption. Thus, I would expect corruption to decrease after 2014. However, this effect might be confounded by two factors: (1) the overall increase in all types of legal cases after the policy was enforced in 2014 and (2) the increase in corruption cases in particular due to the anti-corruption campaign after Mr. Xi assumed power in 2012.[26] Thus, simply observing the trend of reported corruption before and after 2014 would not identify the effect of improving transparency on corruption.

To address this problem, I employ a diff-in-diff model with the CMC institution as a source of identification. The rationale of the identification is as follows: in places with a strong norm of honesty and lawfulness, the anti-corruption effect of the policy may be more pronounced. For example, the database may be visited more often by the residents in such places, potentially strengthening local public surveillance. Local judges and prosecutors may also be more conscientious with corruption cases, further reducing the chance of exposed corrupt officials or businessmen escaping punishment. Lastly, the open records of corruption could be evaluated more negatively in places with stronger norms of honesty, increasing the costs of corruption. Summing up, counties affected by the CMC institution (and hence maintain a stronger norm of honesty until today) should report less corruption than unaffected counties after the policy was enforced in 2014.

Using the information from *wenshu.com*, I collect 4,864 corruption cases that were trialed between 2008 and 2019 in 145 county-level courts located in my sample of historical counties.[27] I choose the courts at the lowest tier because highly localized corruption (or petty corruption) is more likely to reflect local norms than higher-profile corruption cases (or major corruption cases) ruled by a higher-level court. I then aggregate the number of corruption cases for each court and year and normalize the number of cases for each court-year cell by the population of the county ruled by that court. Since no official and publicly available data set can provide the population covering all years and counties in my sample in a systematic manner, I use population data provided by *WorldPop*.[28].

Fig. 2 illustrates how reported corruption cases grew over the period 2008–2019. Noticeably, a tremendous increase in the number of cases can be observed after 2014, which is not surprising because the publication of judicial documents was enforced in 2014. To identify the anti-corruption effect of the policy, I categorize the courts in my sample into two groups: those located in historical counties affected by the CMC institution ($CMC = 1$) and those located in unaffected counties ($CMC = 0$). As demonstrated in Fig. 2, there was hardly any difference in the number of reported corruption cases between the two groups of courts before 2014; after 2014, they diverged and followed vastly different growth trends. This reassures that the parallel pre-trend assumption is likely to hold. To examine the link between the historical CMC institution and corruption before and after the mandatory publication policy in a formal manner, I estimate the following panel IV regression:

$$Y_{it} = \kappa_0 + \kappa_1 CMC_i * Post2014_t + \kappa_2 Dis\_CMC_i * \delta_t + \delta_i + \delta_t + \delta_{jt} + \delta_{kt} + \epsilon_{it} \quad (5)$$

The outcome variable of Eq. (5) is the number of corruption cases per capita reported by court i in year t. The variable of interest is the interaction term $CMC_i * Post2014_t$. CMC equals one if court i is located in a county affected by the CMC institution (a historical county where CMC equals one), while $Post2014$ switches on for the period 2014–2019. To account for the endogeneity associated with CMC, $CMC_i * Post2014_t$ is instrumented by $CMC\,coverage_i * Post2014_t$ in a first-stage regression. Moreover, interaction terms between *Dis_CMC* and a full set of year dummies are included to capture any time-variant effect of treaty port spillovers.

δ_i and δ_t represent court and year fixed effects, which control for time-invariant court-specific characteristics as well as secular time

[25] In particular, cases can be searched by dates, courts, causes, litigants, and judges, among other legal factors. Court cases that are associated with classified information (such as state secret or adolescent crimes) or settled out of court are exempted from this regulation, as they are defined by law as cases not suitable for publication.

[26] Operated by a strong anti-corruption agency, Mr. Xi's ongoing anti-corruption campaign had busted more than 600,000 officials as of 2021. Unlike his predecessors, Mr. Xi also targeted standing committee members of Chinese Communist Party (CCP) Politburo, who are usually considered top politicians in China. See Chen and Kung (2019), Kong et al. (2020), and Xu and Yano (2017) for more details on the impact of Mr. Xi's anti-corruption campaign.

[27] Local courts in China are categorized, from high to low, as provincial-level high courts, prefecture-level intermediate courts, and county-level grassroots courts. Courts that were established after 2014 are not included in this analysis.

[28] The data set records the population count at the grid level (at a resolution of 3 arc seconds, c.a. 100 m at the equator), relying on two county-level population census rounds in China (2000 and 2010) and linear interpolations for the other years. The aggregated population for 2000 and 2010 are adjusted by the estimates provided by the United Nations. For details, please see https://www.worldpop.org. I sum up the population count for each county using a map of the contemporary county boundaries.

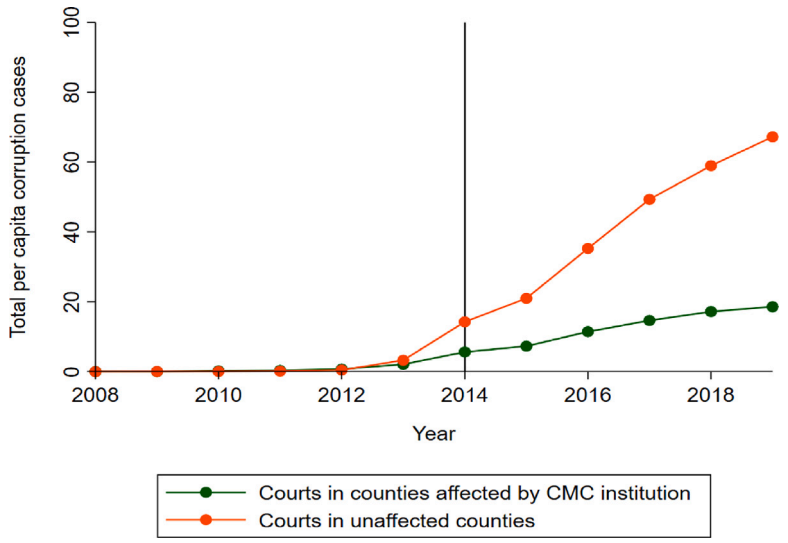

Fig. 2. Cumulative corruption cases in the period 2008–2019.

Table 11
CMC cultural legacy and anti-corruption policy.

Dependent variable: Number of reported corruption cases per 10,000 people					
	(1)	(2)	(3)	(4)	(5)
CMC*Post2014	−0.076**	−0.085**	−0.094**	−0.049**	−0.060***
	(0.035)	(0.038)	(0.043)	(0.024)	(0.026)
Observations	1,740	1,740	1,740	1,740	1,740
Number of courts	145	145	145	145	145
Dis_CMC*Year	No	Yes	Yes	Yes	Yes
Rank*Year	No	No	Yes	No	Yes
Province*Year	No	No	No	Yes	Yes
Court&Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Diff-in-diff regressions using a panel data set at the court-year level. The unit of observation is a court-year. Sample includes all local courts located in counties of the baseline sample. *Post* equals one for the period after 2014. *CMC* Post2014* is instrumented by *CMC coverage*Post2014* in all regressions. *Rank* represent administrative level of the county where a court is located (city district, county-level city, and county). Robust standard errors adjusted for clustering at the court level are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

trends at the national level (e.g. Mr. Xi's anti-corruption campaign after 2012). Finally, province-year fixed effects (δ_{jt}) and administration rank-year fixed effects (δ_{kt}) are used to capture the differences in anti-corruption efforts and law enforcement that are associated with provinces and administrative ranks.[29]

Table 11 presents the results. Particularly, the estimation in column (1) uses court and year fixed effects while column (2) additionally controls for the interactions of *Dis_CMC* and year dummies. In both columns, I find that counties affected by the historical CMC institution reported fewer corruption cases after policy was enforced in 2014. Columns (3)–(5) further demonstrate that the results are robust to the inclusion of province-year and rank-year fixed effects; in column (5), for example, the estimated effect of $CMC_i * Post2014_t$ is −0.6 and is significant at the five percent level.

Could $CMC_i * Post2014_t$ capture a growing difference in the trend of reporting corruption before 2014? In Fig. 2, I show that the pre-2014 difference in reporting corruption was very small. To address this concern more formally, I re-estimate Eq. (5) using placebo treatment years ranging from 2011 to 2013; a significant result from the placebo regression indicates that the baseline effects might capture a pre-trend before the policy was actually implemented. As reported in Table A.8, however, I find that the placebo effects are statistically insignificant and quantitatively very small.

9. Testing alternative mechanisms

9.1. Foreign direct investment

Have regions affected by the CMC institution attracted more FDI today and hence developed better than other places? While this channel can be studied by examining FDI inbound at the county level, various unobserved factors might influence the location choice of FDI and hence bias the results. To address this concern, I exploit the fact that industries have different demands and attractiveness to FDI and establish an industry-county data set of receiving FDI. Doing so allows me to compare the same industry in regions affected and unaffected by the CMC institution.

I use the information from the Annual Survey of Industrial Firms (ASIF) in 2007 to construct the data set. The survey is conducted by the National Bureau of Statistics (NBS) of China to collect data on the universe of more than 300 thousand industrial firms (defined as firms in energy, manufacturing, and mining industries) with annual sales above

[29] From the highest to the lowest, county-level administrative units in today's China are ranked as follows (from high to low): city districts, county-level cities, and counties.

Table 12
CMC institution and FDI.

Dependent variables: Foreign direct investment				
Weights	Size of employment		Size of fixed assets	
	(1)	(2)	(3)	(4)
CMC	5.018***	7.114**	5.472***	8.993***
	(1.263)	(3.268)	(1.771)	(3.355)
Controls	No	Yes	No	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes
Observations	2,550	2,550	2,550	2,550
R-squared	0.619	0.628	0.634	0.608

Notes: Industry-county level IV regressions with CMC and industry fixed effects. Columns (1) and (2) use the number of employees of industry-county pairs as weights, columns (3) and (4) uses the value of fixed assets (in Yuan) of industry-county pairs as weights. Control variables include: *Dis_CMC*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

5 million Chinese Yuan. The data set reports the industry code and location of each firm, enabling me to match firms to counties in my baseline sample. Aggregating the matched firms by industry and location yields the data set of 2,550 industry-county pairs.[30] To examine the effect of the CMC institution on FDI, the following equation is estimated:

$$Y_{ic} = \mu_0 + \mu_1 CMC_c + \mathbf{X}_c\zeta + \delta_i + \delta_j + \epsilon_{ic} \quad (6)$$

where Y_{ic} is the FDI received by industry i in county c. I transform the FDI using the inverse hyperbolic sine transformation (asinh), which has the advantage of being continuous at zero (Bellemare and Wichman, 2020). CMC equals one if the industry is located in a county affected by the CMC institution, and it is instrumented by *CMC coverage*. I control for the CMC fixed effect (δ_j) as usual and δ_i is the industry fixed effects which allow me to identify the causal effect of the CMC institution on FDI.

Table 12 reports the results. I use the number of employees (columns (1) and (2)) and the value of fixed assets (columns (3) and (4)) of industry-county pairs as weights to account for the vast difference in the size of industry-county pairs in my sample.[31] In column (1), the coefficient of instrumented CMC is positive and significant at the one percent level, suggesting that the CMC institution had a positive effect on FDI. After including county-level control variables in column (2), the coefficient increases to 7.1 and remains highly significant. Columns (3) and (4) reestimate Eq. (6) using the value of fixed assets as weights, and the results remain very similar. These findings suggest that the CMC institution may affect modern economic outcomes through its influence on FDI.

9.2. Legal institutions

Colonialism may have a persistent effect on the functioning of contemporary formal institutions (Acemoglu et al., 2001, 2002, 2005). In this subsection, I examine the Chinese judicial system established during the Republic period of China (1911–1949). Taking the idea of the separation of powers into account, the central government progressively established independent courts in Chinese regions, replacing the Qing system under which local governors played a dominant role in both the administration and judiciary.

Could the radical change in the judicial system during the Republic era of China explain part of the long-run effect of the CMC institution? For instance, the transmission of the CMC institution improved the inland trade conditions, fostering demands for formal rules that would impartially handle business disputes. In the long run, places with independent courts might develop a better rule of law. To test this hypothesis, I collect data from a judicial statistic yearbook in 1930 published by the Ministry of Justice (*Minguo shijiu niandu sifa tongji*) and generate a dummy variable equal to one if a county had established a local court by 1930. The results reported in column (1) of Table A.9, however, suggest that the CMC institution is uncorrelated with the establishment of historical courts.

Using a contemporary measurement of legal institutions does not support the hypothesis either. Obtaining and utilizing data of governmental expenditure in 2000 from the Yearbook of Fiscal Statistics on Prefectures and Counties (*Quanguo Dishixian Caizheng Tongji Ziliao*), I find no relationship between the CMC institution and judicial expenditures today (columns (2) and (3)). In summary, my evidence is consistent with the argument that formal colonial rules in China were mostly abolished after 1949, and that the persistence of formal institutions is unlikely a channel here (Jia, 2014a; Long et al., 2019). However, culture as a part of informal institutions might persist and affect economic development today.

9.3. Modern human capital

Colonial investments and missionary activities could have generated a profound impact on educational outcomes and economic prosperity (Bai and Kung, 2015; Huillery, 2009; Caicedo, 2019; Waldinger, 2017). Hence, it is important to test whether the CMC institution positively affected long-run economic development through modern human capital.

To measure historical human capital in China, I use data from Stauffer (1922), which is a national survey conducted by Protestant missionaries in China in the 1920s. The survey provides the number of Protestant converts, Protestant church school students, and government school students in 1920 at the county level (all normalized by 10,000 people in 1920). I also measure contemporary education by the average years of schooling reported in the population census of the year 2000. Table A.10 shows that the CMC institution cannot predict schooling in the historical and contemporary period, suggesting that the transmission of a customs institution induced only very limited demands for higher education at the local level,[32] and no human capital persistence can be observed.

9.4. Confucianism

This subsection examines the connection between the CMC institution and Confucianism, which plays a vital role in Chinese society. I measure Confucianism by its cultural,[33] educational, and meritocratic perspectives.

First, Confucian culture can be proxied by the presence of Confucian temples. These shrines were constructed as early as the West Han Dynasty (206 B.C.–A.D. 9) for worshiping Confucius and commemorating his deeds and greatly promoted Confucianism (Chow, 1996; Kung and Ma, 2014). I obtain the list of Confucius temples in China

[30] The data set includes 39 industries.

[31] Weighted regressions assign greater weights to industry-county pairs with a larger size. Smaller industry-county pairs, which are potentially poorly measured and subject to measurement errors, are assigned smaller weights. My results stay qualitatively similar when using unweighted regressions.

[32] Notwithstanding the non-results, CMC indeed contributed to China's modern education at the national level. It actively co-organized the School of Combined Learning (Tongwen Guan) with the Chinese government. Established in Beijing, it offered instructions in European languages (primarily English) and modern science such as chemistry and astronomy.

[33] Confucianism has been shown to place great emphasis on subordination (Kung and Ma, 2014) and informal institutions (Chen et al., 2022). Its cultural traits have had a long-lasting effect on firm and individual behavior today (Chen et al., 2019; Du, 2015).

from the official website *chinakongmiao.org* and calculate the distance from each county to the closest Confucian temple. Second, places with stronger Confucian norms may also have established more Confucian academies (*Shuyuan*), which captures the educational function of Confucianism. Hence, I calculate the number of Confucian academies in each county using the data from the book of Ji (1996). Finally, the Chinese meritocracy during the Qing period was largely formed by those who held a *Jinshi* title - a title awarded to individuals who achieved the highest rank in the empirical civic exam (the *Keju* exam). The exam focused primarily on testing the interpretation of Confucian classics.[34] I obtain the name list of *Jinshi* during the Ming and Qing period (c.1368–1905) from China Biographical Database (2021), which collects biographical data of historical individuals from Zhu and Xie (1980) and local gazetteers.

Reported in Table A.11, I find that the CMC institution is unrelated to the distance to the nearest Confucian temple or the number of Confucian academies (columns (1) and (2)). Moreover, it is not associated with the number of *jinshi*, which is transformed by the inverse hyperbolic sine function in column (3) and normalized by the number of settlements in a county in column (4). The overall results suggest that the long-lasting impact of the CMC institution is independent of the legacy of Confucianism in China.

9.5. Conflicts

Finally, I test whether conflicts may be a relevant factor in this analysis by using the data set on battles and wars obtained from Miller and Bakar (2022), who list important battles (together with the name, type, date, and location of the battle) around the globe from 1468 B.C. to the present day based on Jaques (2007) and historical studies on individual countries. I use a sub-sample of the database and focus on battles that took place in my baseline counties of China during and after the Qing period.

First, I investigate whether the long-run economic effect of the CMC institution can be explained by the conflict history of a county, which is measured by a county's exposure to the Taiping Rebellion — the deadliest civil war in human history (Ho, 1959). Next, I examine whether wars and battles were more likely to occur in places where Native Customs stations were taken over. In particular, I count the number of battles that took place after 1902 (and before 1949) for each county in my baseline sample. Moreover, I differentiate post-1902 battles by examining civil wars and wars involving foreign powers. The former include the 1911 revolution that marked the end of China's imperial rule and the Second Kuomintang-Communist Civil War between 1927 and 1949, after which the People's Republic of China was founded. Wars involving foreign powers comprise the Russo-Japanese War (1904–1905) and the Second Sino-Japanese War during the WWII. In column (1) of Table A.12, I find that the coefficient of *CMC* is still highly significant after controlling for *Taiping*, which equals one if a county was exposed in the Taiping Rebellion. Interestingly, the coefficient of *Taiping* is positive and significant, which supports the findings of Xu and Yang (2018) that the Taiping Rebellion had a long-run pro-development effect by strengthening state capacity and local institutions. Reported in columns (2)–(4), I do not find any significant correlation between the CMC institution and armed conflicts in historical China.

Considering the importance of the CMC institution in commerce, trade, and public life, it might be relevant to investigate whether the takeover of Native Customs stations led to unrest and protests. Therefore, I collect data from the book of Ding and Zhang (1982) who recorded all revolts and protests (violent or nonviolent) during the period 1902–1910 from a large collection of archived newspapers and gazetteers. Reading through the description of all events, it seems that the takeover of Native Customs stations did not result in substantial social instability: across the total of more than a thousand incidents in the list, I find only two protests directly against the takeover of Native Customs stations.

Despite the qualitative evidence, I digitize Ding and Zhang (1982) and calculate the number of incidents at the county level depending on their type and motivation. Reported in Panel A of Table A.13, I examine whether the CMC institution is correlated with the total number of incidents (column (1)) or violent incidents[35] in particular (column (2)). Columns (3) and (4) examine whether the takeover of Native Customs stations is associated with more strikes organized by workers (column (3)) and business owners (column (4)). In panel B, I distinguish incidents driven by different identifiable motivations, such as harsh tax rates (column (5)) and other economic-related motivations (e.g., workers demanding higher wages or peasant revolts because of poverty). Columns (7) and (8) examine incidents that were driven by anti-Western sentiments and opposition to churches. The results reported in Table A.13 show that the CMC institution is not correlated with any of my conflict measurements.

10. Conclusion

This paper shows that the transmission of the CMC institution to China generated a long-lasting impact on China's regional economic development. The causal relationship is identified by focusing on a sample of neighboring counties of CMC stations and by exploiting the exogenous variation in the institutional change with a historical natural experiment. My empirical findings are robust to a number of modifications to the sample, variables, and empirical strategies. To explore plausible channels of persistence, I show that the CMC institution transmitted the norm of honesty and lawfulness, which reduced corruption at the local level.

By showing that the colonial institutions fostered the transmission of embedded values and hence long-run economic development, my study follows Nunn (2012) and contributes to the debate on the "colonial origins of comparative development" (Acemoglu et al., 2001; Glaeser et al., 2004). In particular, I examine a historical context where formal colonial rules ceased to exist and the role of human capital is rather limited, shedding light on the importance of cultural norms and informal institutions.

More broadly, my results imply that the improvement of institutional quality could have a long-run economic effect not only directly but also indirectly through its profound cultural impact. Similar to most studies that examine a historical natural experiment, my analysis exploits counterfactuals that are otherwise unavailable in most contemporary contexts and thus is concerned with the implications for more effective policy-making in modern times.

Data availability

Data will be made available on request.

Appendix A. Additional regression tables

See Tables A.1–A.13

[34] For further evidence, Bai and Jia (2016) show that the abolition of the *Keju* system had a large impact on political stability. Chen et al. (2020) show that *jinshi* density had an effect on the number of high-ranking officials during the Republic period (1911–1949).

[35] Violent incidents refer to entries with words such as "casualty", "destroy", "violent suppression", and so forth. Incidents conducted by known violent groups are counted as violent incidents as well.

Table A.1
OLS results excluding outliers.

Dependent variable: Average nighttime light 2000–2010, in log				
	(1)	(2)	(3)	(4)
CMC	0.373*	0.488**	0.396**	0.362**
	(0.193)	(0.233)	(0.176)	(0.178)
Observations	116	116	114	112
Controls	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes

Notes: County level regressions with CMC fixed effects. Robust regression results are reported in columns (1) and quantile regression results based on conditional median are reported in column (2). Then, I restrict my sample to counties with their nighttime light between the top and bottom 0.5 percentile in column (3) and one percentile in column (4). Control variables include *Dis_Capital*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, and *Dis_Canal*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table A.2
Estimations using annual nighttime light.

Dependent variable: Nighttime light in log					
Year:	2000	2001	2002	2003	2004
	(1)	(2)	(3)	(4)	(5)
CMC	0.871**	1.087**	1.021**	1.208***	1.177***
	(0.418)	(0.468)	(0.420)	(0.462)	(0.445)
Controls	Yes	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes	Yes
Period:	2005	2006	2007	2008	2009
	(6)	(7)	(8)	(9)	(10)
CMC	1.361***	1.245**	1.052**	1.077**	1.278**
	(0.506)	(0.490)	(0.457)	(0.481)	(0.549)
Controls	Yes	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes	Yes
	2010				
	(11)				
CMC	0.871**				
	(0.418)				
Controls	Yes				
CMC fixed effects	Yes				

Notes: County level IV regressions with CMC fixed effects. Control variables include: *Dis_CMC*, *Dis_Capital*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table A.3
Alternative measurement of treaty port spillover effects.

Dependent variable: Average nighttime light 2000–2010, in log							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CMC	1.030**	1.287***	1.178**	1.015**	0.972**	0.917*	0.866**
	(0.486)	(0.478)	(0.467)	(0.421)	(0.489)	(0.491)	(0.433)
Dis_CMC	−0.003	0.003	−0.001	−0.004**			
	(0.003)	(0.003)	(0.002)	(0.002)			
Dis_CMC (2nd group)					−0.090	0.034	−0.127
					(0.146)	(0.167)	(0.153)
Dis_CMC (3rd group)					−0.165	−0.219	−0.139
					(0.185)	(0.169)	(0.192)
Dis_CMC (4th group)						−0.443**	−0.272
						(0.206)	(0.197)
Dis_CMC (5th group)							−0.535**
							(0.234)
Observation	116	116	116	115	116	116	116
R-squared	0.798	0.770	0.781	0.800	0.801	0.814	0.812
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: County level IV regressions with CMC fixed effects. In column (1), Dis_CMC is measured by the distance from the political center of a county to the nearest treaty port. In column (2) and (3), Dis_CMC measures the distance to the closest first-wave treaty ports (Guangzhou, Xiamen, Fuzhou, Ningbo, and Shanghai) and Concessions (Tianjin, Shanghai, Suzhou, Zhenjiang, Jiujiang, Hangzhou, Xiamen, Wuhan, Chongqing, and Guangzhou). Column (4) uses the shortest driving distance to the nearest treaty port city derived from *Baidu Map*. Control variables include: *Dis_Capital*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parenthesis.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table A.4
Estimations with additional control variables.

Dependent variable: Average nighttime light 2000–2010, in log					
Add. control	Foxtail millet	Wheat	Rice	Maize	Sweet
	(1)	(2)	(3)	(4)	(5)
CMC	0.963**	1.005**	1.070**	1.027**	1.123**
	(0.448)	(0.469)	(0.452)	(0.453)	(0.450)
Add. control	−0.180**	−0.101	−0.105	−0.139	−0.0226
	(0.0888)	(0.0898)	(0.132)	(0.102)	(0.107)
Controls	Yes	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes	Yes
	Sugar	Tea	Cotton	% shared boundary	Courier connect
	(6)	(7)	(8)	(9)	(10)
CMC	1.087**	0.920**	1.019**	1.141**	1.047**
	(0.465)	(0.432)	(0.429)	(0.456)	(0.420)
Add. control	−0.267	0.382**	−0.200*	−0.448	0.184
	(0.188)	(0.165)	(0.115)	(0.413)	(0.149)
Controls	Yes	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes	Yes
	Small river connect	Major river connect	Any river connect	All	
	(11)	(12)	(13)	(14)	
CMC	1.091**	1.096**	1.129**	0.781*	
	(0.427)	(0.455)	(0.453)	(0.404)	
Add. control	0.144	0.0883	0.00848		
	(0.131)	(0.141)	(0.132)		
Controls	Yes	Yes	Yes	Yes	
CMC fixed effects	Yes	Yes	Yes	Yes	

Notes: County level IV regressions with CMC fixed effects. Control variables include: *Dis_CMC*, *Dis_Capital*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table A.5
Estimations with alternative samples.

Dependent variable: Average nighttime light 2000–2010, in log				
	(1)	(2)	(3)	(4)
CMC	1.129**	1.139***	0.934**	1.051*
	(0.452)	(0.423)	(0.448)	(0.571)
Observations	116	104	59	187
R-squared	0.853	0.858	0.481	0.373
Controls	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes

Notes: County level IV regressions with CMC fixed effects. Column (1) is the baseline IV regression. Column (2) includes counties whose distance to the nearest CMC stations falls in the 5th and 95th percentile. Column (3) uses a sample of counties where a CMC circle crossed. Column (4) combines the baseline and placebo sample. Control variables include: *Dis_CMC*, *Dis_Capital*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table A.6
Balancing checks.

	No matching			Kernel matching	Radius matching	Entropy balancing
	Mean CMC (1)	Mean Non-CMC (2)	Mean difference (3)	Mean difference (4)	Mean difference (5)	Mean difference (6)
Dis_CMC	38.813	61.073	−22.260***	−2.746	−2.309	−0.001
Dis_Capital	30.309	52.065	−21.755***	−2.974	−2.922	−0.000
Size	7.151	7.495	−0.344**	−0.003	0.027	−0.000
Longitude	117.816	115.508	2.308**	0.010	−0.083	0.000
Coast	102.162	233.532	−142.370*	−7.429	−1.979	−0.005

Notes: The table presents mean comparisons of variables between counties where *CMC*=1 and 0. Columns (1)–(3) show the results without using weights. Columns (4), (5), and (6) assign weights generated from the Kernel matching, radius matching (caliper=0.1), and entropy balancing, respectively. All reported models keep the entire counties from the baseline sample (116 counties), and the sum of weights is 32.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table A.7
Estimations using IV and sample balancing techniques.

Dependent variable: Average nighttime light 2000–2010, in log						
Method	PSM (kernel)		PSM (radius)		Entropy balancing	
	(1)	(2)	(3)	(4)	(5)	(6)
CMC	1.746***	0.632**	1.746***	0.666***	2.024***	0.761**
	(0.387)	(0.252)	(0.391)	(0.252)	(0.552)	(0.363)
Observations	116	116	116	116	116	116
R-squared	0.703	0.917	0.708	0.920	0.614	0.908
Controls	No	Yes	No	Yes	No	Yes
CMC fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: County level IV regressions with CMC fixed effects and weights generated from kernel matching (columns (1) and (2)), radius matching (columns (3) and (4)), and entropy balancing (columns (5) and (6)). Control variables include: *Dis_CMC*, *Dis_Capital*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parenthesis.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table A.8
CMC cultural legacy and corruption, placebo tests.

Dependent variable: Number of reported corruption per 10,000 people				
Placebo treatment Year:	Baseline	2013	2012	2011
	(1)	(2)	(3)	(4)
CMC*Post	−0.060**	−0.025	−0.015	−0.009
	(0.026)	(0.024)	(0.020)	(0.018)
Observations	1,740	1,740	1,740	1,740
Dis_CMC*Year	145	145	145	145
Rank*Year	Yes	Yes	Yes	Yes
Province*Year	Yes	Yes	Yes	Yes
Court&Year FE	Yes	Yes	Yes	Yes

Notes: Diff-in-diff regressions using a panel data set at the court-year level. Sample includes all local courts located in counties of the baseline sample. *Post* equals one for the period after the placebo treatment year. *CMC*Post2014* is instrumented by *CMC coverage*Post2014* in all regressions. *Rank* represent administrative level of the county where a court is located (city district, county-level city, and county). Robust standard errors adjusted for clustering at the court level are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table A.9
CMC and legal institutions.

DV:	Court in 1930 (0/1)	Judicial expenditure in 2000 (per capita, in log)	Share of judicial expenditure
	(1)	(2)	(3)
CMC	−0.179	0.0725	0.0269
	(0.242)	(0.256)	(0.0176)
Observations	116	116	116
R-squared	0.643	0.800	0.311
Controls	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes

Notes: County level IV regressions with CMC fixed effects. Dependent variables are: the establishment of a modern court by 1930 (dummy), per capita judicial expenditure in 2000, and the share of judicial expenditure to the total governmental expenditure in 2000, respectively. Control variables include: *Dis_CMC*, *Dis_Capital*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table A.10
CMC institution and modern human capital.

DV:	Protestant converts	Church school students	Government Gov. school students	Years of schooling in 2000
	(1)	(2)	(3)	(4)
CMC	−13.65	−0.336	−3.501	0.080
	(10.71)	(6.822)	(84.088)	(0.063)
Observations	116	116	116	116
R-squared	0.435	0.506	0.514	0.530
Controls	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes

Notes: County level IV regressions with CMC fixed effects. Dependent variables in columns (1)–(3) are: the number of Protestant converts, church school students, and governmental schools in 1920 normalized by 10,000 people. In column (4) the dependent variable is average years of schooling in 2010. Control variables include: *Dis_CMC*, *Dis_Capital*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table A.11
CMC institution and confucianism.

DV	Ln (Distance to Confucian temple)	# Confucian academies	*Jinshi* (in asinh)	*Jinshi* density
	(1)	(2)	(3)	(4)
CMC	−0.348	−1.569	1.330	2.686
	(0.439)	(1.545)	(1.072)	(2.776)
Controls	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes
Observations	116	116	116	116
R-squared	0.586	0.353	0.749	0.532

Notes: County level IV regressions with CMC fixed effects. Dependent variables in columns (1)–(4) are: the natural log of distance to the nearest Confucian temple, the number of Confucian academies, the number of *Jinshi* taking inverse hyperbolic sine transformation, and the number of *jinshi* per town in a county. Control variables include: *Dis_CMC*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table A.12
CMC institution and armed conflicts.

DV	Nighttime light	Post-1902 conflicts	Civil wars	Wars involving foreign powers
	(1)	(2)	(3)	(4)
CMC	1.091***	0.163	0.163	−0.014
	(409)	(0.287)	(0.287)	(0.039)
Taiping Rebellion	0.324***			
	(0.074)			
Observations	116	116	116	116
R-squared	0.333	0.465	0.465	0.379
Controls	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes

Notes: County level IV regressions with CMC fixed effects. Dependent variables are the average nighttime light between 2000 and 2010 in log (column (1)), the number of battles after 1902 (column (2)), and number of civil wars after 1902 (column (3)), and the number battles against foreign powers after 1902 (column (4)). Column (1) controls for the number of battles fought during the Taiping rebellion. Control variables include: *Dis_CMC*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Table A.13
CMC institution, protest and revolts.

Dependent variables: Number of incidents				
Panel A: Types of incidents				
	Total incidents	Violent incidents	Strikes (Labor)	Strikes (Business)
	(1)	(2)	(3)	(4)
CMC	1.928	0.513	0.250	0.823
	(2.408)	(1.547)	(0.218)	(0.667)
Observations	116	116	116	116
R-squared	0.368	0.404	0.258	0.247
Panel B: Motivations				
	Taxation	Economic	Anti-Western	Anti-church
	(5)	(6)	(7)	(8)
CMC	0.930	−0.281	0.199	0.141
	(0.757)	(0.854)	(0.220)	(0.208)
Observations	116	116	116	116
R-squared	0.320	0.360	0.402	0.383
Controls	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes

Notes: County level IV regressions with CMC fixed effects. The dependent variable is the number of incidents. Panel A differentiates incidents by types (total incidents, violent incidents, and strikes) while panel B categorizes incidents by their motivation (protest against taxation, economic conditions, Western colonial powers, and churches.) Control variables include: *Dis_CMC*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, *Dis_Canal*, *Elevation*, *Terrain Ruggedness*, *River Density*, and the squared term and the interaction of *Latitude* and *Longitude*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Appendix B. Additional historical background

Native customs in China

Trade in the Qing period of China (c.1644–1912) was processed by the Native Customs, the existence of which can be traced back to the Ming Dynasty (1368–1644). Before the Opium War in 1840, the Chinese government had established 40 Native Customs stations, with the station of Guangzhou (Canton) being the only one processing international trade (Qi, 2004; Deng, 2007). In the late Qing period, the customs system was generally characterized as inefficient and corrupt (Qi, 2004; Liao, 2010). As cited in Wright (1950, p747), Sir Robert Hart, the second Inspector-General (IG) of the CMC, described the Native Customs stations as:

> "...widely scattered, decentralized, and semi-independent trade-taxing establishments, each with its own tariffs, and each with its own excessive staff, every man of whom had his own vested interests to defend".

Corruption arose as each station applied its own tax rates and administrative procedures. These informal rules were often "stored in the head" of the tax collectors, making tax payments a matter of private bargains (CMC, 1907, p6). Moreover, because all customs stations were obliged to submit the entire amount of tax revenues to the central government, the superintendents (who were directors of Native Customs stations) had created a variety of fees and fines to generate a sizable income off the record (Qi, 2004). In Xiamen (Amoy), for example, the superintendent collected nearly $30,000 from fees and fines per year, whereas his salary was only $3 per month (CMC, 1907, p80).

Corruption also stemmed from nepotism. Family members of the superintendents usually occupied the most important positions. Many other positions were often taken by people who held inherited rights to the proceeds from working in the Native Customs stations without any duty to perform. In Tianjin, for instance, it was reported that "several hundred persons" held such rights, and "no less than 10,000 people" depended on their connections with the Native Customs (CMC, 1907, p6).

The takeover of native customs stations

To ensure that the Chinese government was financially capable of delivering the payment imposed by the *Boxer Protocol*, the Western powers further required the CMC to collect revenues from "the Native Customs, administered in the open ports by the Imperial Maritime Customs" (Sun, 2007). Ambiguity arose since the protocol could be explained either in a way that the CMC administered *all* Native Customs stations or that the CMC only administered the Native Customs stations within the areas of the treaty ports. In a letter written to the IG, Robert Hart, the Chinese representatives (Prince *Ch'ing*, the prime minister, and *Li Hongzhang*, the governor of Zhili province) argued that "most of the Native Customs are set inland" and were hence "inconvenient for Commissioners to manage" (General Administration of Customs, 2003; Tsai, 2008). They further claimed that:

> "it should be clearly ruled that those inland Native Customs intra-50-*li*[36] will be handled simultaneously by the Commissioners. Those which are extra-50-*li* should still be exclusively managed by the superintendents of that port in order to make a clear boundary".

Hart and other CMC commissioners questioned whether the radius of 50 *li* was a reasonable parameter; Hart argued that the radius was arbitrary (General Administration of Customs, 2003, p462), whereas the commissioner of Shanghai Customs, Herbert E. Hobson, claimed that "the 50-*li* radius proviso is found to be a stumbling block, fatal to the collection of anything like the revenue which should be forthcoming" (CMC, 1907, p47). Nevertheless, the takeover was swiftly carried out based on the radius of 50 *li*. By the end of 1902, most of the intra-50-*li* Native Customs stations, including the substations, were officially administered by the CMC

To efficiently collect revenues from China's inland trade, the CMC transplanted most of its administrative concepts to the Native Customs stations it took over. This was done by dispatching a foreign clerk and a Chinese assistant to supervise a series of reforms in each affected Native Customs station. Foremost was the establishment of written tax rules: the CMC published a tax code for inland trade based on the old Chinese ones issued in 1786 — the latest version that the CMC could find in the Native Customs archives (CMC, 1907). Furthermore, duty memos were made publicly available to the merchants, ensuring that "a merchant was enabled to tell at a glance what he had to pay on any goods to any place" (CMC, 1907, p83). To tightly monitor inland trade revenues, a system of recording the exact duties and fees was implemented, and the figures were further published in the CMC statistical reports.

Second, the CMC simplified the administrative procedures. Realizing that various fees usually caused delays, disputes, and corruption, the CMC carefully revised these charges and aggregated them into one fixed payment, releasing the local merchants from burdensome extractions (CMC, 1907; Dai, 1989). Moreover, stricter rules were applied to the cargo-examination process, which forbade abuse: goods must be measured by their actual weights, and tax collectors could not coerce or detain travelers, nor could they ask for bribes (CMC, 1907; Tsai, 2008).

Last but not least, the CMC tackled the problem of excess staff in Native Customs stations. From 1901 to 1906, the CMC station in Shanghai reduced the total number of employees in its two Native Customs stations from 140 to 61, and the station in Santuao reduced the number from 600 to 70 (CMC, 1907, p46, p67). Although discharging employees was still challenging in some places such as Jiujiang and Wuhu, this CMC reform starting from 1902 was the first systematic crackdown on nepotism in the Native Customs system in the history of Qing China (Qi, 2004).

[36] *Li* are Chinese miles. Fifty *li* are approximately 25 kilometers.

The takeover of the Native Customs stations triggered strong responses from the local society. While some cooperative superintendents and merchants showed appreciation for the improvements, others protested the loss of their vested interests (CMC, 1907). In Xiamen, the radical reform of the Native Customs stations resulted in a riot. Nevertheless, "one provision of undoubted value" remained in the new agreement reached after the riot, which was the amalgamation of fees and the publication of tax rates (CMC, 1907, p83).

Appendix C. Baseline estimations with extended sample

As discussed in Section 4, my analysis focuses on a sub-sample of highly comparable counties that reliably overcome the identification challenge stemming from the vast heterogeneities across Chinese regions. However, it may also be relevant to test my baseline model with the full-county sample. To that end, I extend the baseline sample by including more than 1,500 counties for which I collect data from the same source of the baseline analysis (listed in Table 1). I exclude the most western and northern regions such as Tibet, Xinjiang, and Mongolia due to data limitations. Nevertheless, the extended sample covers about 94% of the total population in China as measured in 1820 (Cao, 2000).

Table C.1 reports the results after reestimating Eq. (1) with the extended sample. Particularly, I replace CMC fixed effects with province fixed effects to better capture unobserved regional heterogeneities in this setup. The estimated coefficient of *CMC* in columns (1) and (2) suggest that the positive correlation between the CMC institution and long-term economic prosperity still holds. However, such result may be biased due to unobserved county-specific characteristics. In columns (1)–(3) of Table C.2, I report the mean comparison between control variables, and the results show that counties affected by the CMC institution were vastly different from other unaffected places,[37]. To further account for the heterogeneities across the counties, I apply propensity score matching (PSM) based on variables listed in Table C.2. After applying the kernel and radius matching (with a caliper of 0.1), columns (4) and (5) show that the difference between the two types of counties reduces tremendously. It is worth noticing that the number of observations decreases to fewer than 500 counties when PSM is applied, suggesting that the full-county sample is poorly balanced.[38] Using the weights generated by PSM, columns (3) and (4) in Table C.1 report positive and significant coefficients of *CMC* as well.

Table C.1
Full county estimations.

Dependent variable: Average nighttime light 2000–2010, in log				
Method	OLS		Kernel matching	Radius matching
	(1)	(2)	(3)	(4)
CMC	1.267***	0.699***	0.301**	0.425***
	(0.162)	(0.129)	(0.118)	(0.113)
Observations	1,861	1,561	490	486
R-squared	0.692	0.592	0.812	0.826
Controls	No	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes	Yes

Notes: County level regressions with province fixed effects. Columns (3) and (4) use weights generated from kernel matching and radius matching (calibor = 0.1). Control variables include *Dis_Capital*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, and *Dis_Canal*. Robust standard errors are reported in parentheses.
*** p < 0.01 ** p < 0.05 * p < 0.1.

[37] The only variable that does not show a significant difference is *Yangtze*. Hence, I do not include this variable in the matching process.

[38] As presented in Section 4.1 no counties in my baseline sample are excluded after applying the same PSM approach.

Table C.2
Full county balancing checks.

	Unmatched			Kernel matching	Radius Matching
	Mean CMC	Mean Non-CMC	Mean difference	Mean difference	Mean difference
	(1)	(2)	(3)	(4)	(5)
Dis_CMC	39.36	256.2	−216.8***	−25.41	−24.09**
Dis_capital	30.41	48.86	−18.45***	−4.201	−2.028
Density1820	205	136.6	68.40**	31.32	9.239
Size	7.14	7.453	−0.313	0.222	0.0353
Latitude	27.79	31.2	−3.405***	0.609	0.300
Longitude	117.8	111.9	5.891***	−0.417	−0.136
Coast	91.78	448.7	−356.9***	50.53	6.881
Canal	428.3	670.6	−242.3*	−31.34	−10.78
Observations			1,561	490	486
Sum of weight				26	18

Notes: The table presents mean comparisons of variables between counties where *CMC*=1 and 0. Columns (1)–(3) show the results without using weights. Columns (4) and (5) show results using weights generated from the Kernel matching and radius matching (caliper=0.1), respectively.
*** p < 0.01 ** p < 0.05 * p < 0.1.

Appendix D. Spatial regressions

The long-term effect of the CMC institution on economic development may be driven by spatial autocorrelations in the residual, which, according to Kelly (2019), might severely inflate the t-statistic of the regression estimates. On the other hand, Voth (2021) critically reviews Kelly (2019) and argues that more sophisticated specifications — such as including control variables, clustering standard errors based on spatial relationships, and applying regional fixed effects, substantially decrease the impact of spatial noise.

To examine the existence of spatial autocorrelation in my analysis, I conduct Moran tests using the regression residuals in columns (1)–(4) of Table 2 with a contiguity spatial matrix. A Moran test is a two-dimensional analogue of Durbin-Watson tests that detects autocorrelation in regression residuals, and a contiguity spatial matrix assigns equal weight to all direct neighbors and zero weight to other places. The Moran test statistic turns out to be insignificant even if my regression includes only CMC fixed effects (column (1)), suggesting that regional fixed effects indeed play a major role in reducing the level of spatial autocorrelation, as discussed in Voth (2021).

To formally address the issue of spatial autocorrelations, Table D.1 presents regression results using different spatial models. To begin with, I use standard errors clustering at the CMC level (column (1)) and the Conley (1999) standard errors adjusting for spatial correlation with 25 km (column (2)). In particular, the Conley standard error is not sensitive to a specific choice of distance threshold: using a range of thresholds between 20 and 80 km in increments of 5 km, Fig. D.1 shows that the newly computed Conley standard errors are well below the robust standard error of my preferred specification (reported in column (4) in Table 2).

For the rest of Table D.1, a spatial error model (columns (3) and (4)) and a spatial lag model (columns (5) and (6)) are estimated based on the method of generalized spatial two-stage least-squares (GS2SLS). In particular, a contiguity spatial matrix and a reverse-distance spatial matrix are applied for both spatial models. In all estimations, the coefficient of *CMC* is significantly positive, suggesting that spatial autocorrelation is not a critical issue in my analysis.

Table D.1
Baseline results adjusting for spatial autocorrelation.

Dependent variable: Average nighttime light 2000–2010, in log						
	Cluster	Conley	Spatial error		Spatial lag	
			Contiguity	Distance	Contiguity	Distance
	(1)	(2)	(3)	(4)	(5)	(6)
CMC	0.392*	0.392***	0.381**	0.355**	0.387**	0.361**
	(0.224)	(0.148)	(0.148)	(0.148)	(0.150)	(0.146)
Spatial error			0.074	−4.221**		
			(0.243)	(1.915)		
Spatial lag					−0.052	−0.919**
					(0.132)	(0.428)
Observations	116	116	116	116	116	116
Controls	Yes	Yes	Yes	Yes	Yes	Yes
CMC fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: County level regressions with CMC fixed effects. Control variables include: *Dis_CMC*, *Dis_Capital*, *Density 1820*, *Size*, *Latitude*, *Longitude*, *Dis_Yangtze*, *Dis_Coast*, and *Dis_Canal*. Column (1) reports standard errors clustered at the CMC level. Column (2) reports standard errors assuming spatial autocorrelation within approximately 25 km (within grid of 0.225 degree latitude and longitude). Columns (3) and (4) report results generated by the spatial autoregressive model (SAR) with a spatially lagged error term. In particular, the contiguity matrix and the inverse-distance matrix are applied in columns (3) and (4), respectively. Columns (5) and (6) report results of a SAR model taking into account the spatial lag of the dependent variable, i.e., the nighttime light.
*** p < 0.01 ** p < 0.05 * p < 0.1.

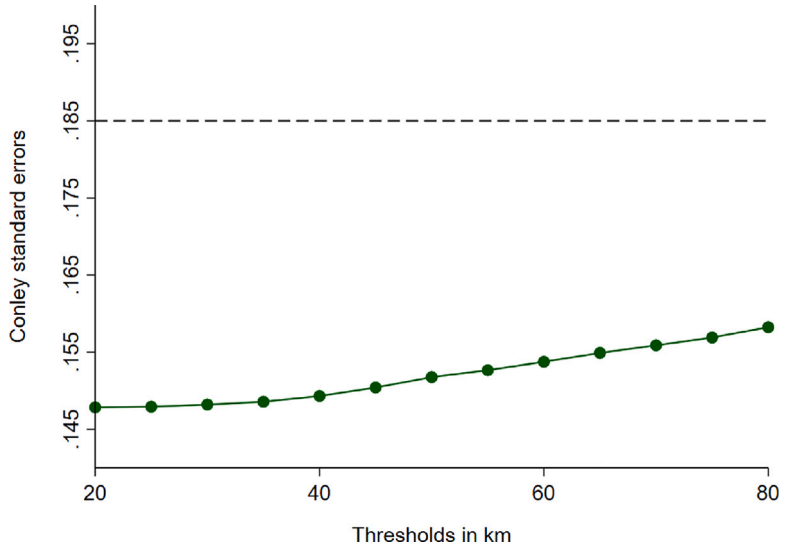

Fig. D.1. Conley standard errors with different distance thresholds.
Notes: This graph presents Conley standard errors of the coefficient of CMC after estimating Eq. (1) using a range of thresholds between 20 and 80 km in increments of 5 km. The dashed line stands for the robust standard error without addressing spatial autocorrelation.

Appendix E. Spatial regression discontinuity

At a first glance, the radius of 25 km allows me to exploit the discontinuity in the nighttime light of small areas at the boundary of a CMC circle. However, such a sharp RDD analysis is inappropriate for my study: unlike the boundary of a state or a catchment area within which a homogeneous set of institutions and laws is enforced,[39] the CMC circle determined only the selection of Native Customs stations to be taken over by the CMC. It had no further relevance *after* the takeover because the affected Native Customs station did not necessarily operate within the circle. Thus, I do not assume any discontinuity regarding institutional quality (and economic development thereafter) at the boundary of a CMC circle.

To exploit the CMC circle as a source of identification, I rely on a fuzzy RDD analysis using the baseline sample of neighboring counties. Similar to a sharp RDD analysis, a fuzzy RDD identifies a causal effect by restricting the sample to highly comparable regions and by controlling for the smooth effect of geographic location. However, it differs by utilizing a first-stage regression to exploit the discontinuity in the probability of receiving treatment at a geographic boundary. Then, the predicted treatment from the first-stage regression is considered to have an exogenous effect on the outcome variable in a second-stage regression.

The first-stage equation of the fuzzy RDD is presented in Eq. (7), CMC_i equals one if the CMC took over at least one Native Customs station in county i. *Treatment* equals one if at least one historical town in county i was located inside a CMC circle.[40] *Dis_circle* captures smooth geographic variation, for which I first compute the shortest distance of every town in a county to a CMC circle, called D. Then, for every county where *Treatment* equals one, *Dis_circle* takes the maximum value of D among towns in the overlaying area between this county and a CMC circle. For each county where *Treatment* equals zero, *Dis_circle* takes the minimum value of D for all towns in this county. Fig. E.1 explains these key variables using a group of hypothetical neighboring counties as an example.

$$CMC_i = \eta_0 + \eta_1 Treatment_i + \eta_2 f(Dis\_circle) + \mathbf{X}_i \upsilon + \delta_j + \epsilon_i \tag{7}$$

$$Y_i = \lambda_0 + \lambda_1 \hat{CMC}_i + \lambda_3 f(Dis\_circle) + \mathbf{X}_i \phi + \delta_j + \epsilon_i \tag{8}$$

Eq. (8) is the second stage regression where Y_i stands for the average nighttime light. $\hat{CMC}_i$ is the predicted treatment computed in the first stage. $\mathbf{X}$ is a set of controls used in the baseline OLS estimation, and δ_j is the CMC fixed effect. In addition to the linear distance polynomial, I test for quadratic polynomials and flexible functions that interact distance terms with *Treatment*. Moreover, instead of choosing an optimal bandwidth for the RDD analysis, I report the results in a more transparent way by employing a continuous range of bandwidth. Measured by *Dis_circle*, the bandwidth ranges from 8 km – below which the number of observations is too low to allow for a regression analysis – to 20 km, beyond which the sample size grows only by including counties where no towns were covered by a CMC circle.[41]

To identify a causal relationship with an RDD analysis, the CMC circle should be irrelevant to economic development in the absence of the takeover. While my analysis using the ring-level data set (Section 6.2) support this assumption, here I show that other co-determinants of development, especially pre-colonial economic conditions, vary smoothly at the threshold. Specifically, I estimate a standard sharp RDD model with variables measuring pre-colonial economic development on the left-hand side and the variable of interest, *Treatment*, on the right-hand side. The equation also controls for the linear distance term (*Dis_circle*), the distance to the nearest CMC station (*Dis_CMC*), and CMC fixed effects. Fig. E.2 visualizes the estimation results for each outcome variable, using a continuous range of bandwidth between 8

[39] For example, the *Mita* boundary in Peru (Dell, 2010), the boundary of the Habsburg Empire (Becker et al., 2016), and the boundary of the Roman part of historical Germany (Wahl, 2017), among others.

[40] I use historical towns to determine the geographic relationship between a county and a CMC circle for the following reasons. First, data on the exact location of Native Customs substations do not exist, and therefore I use the location of towns as a proxy. Second, no county fell completely inside a CMC circle, thus a conventional definition of "being inside the treatment area" does not apply properly in my case. Finally, using historical towns ensures a smooth measurement of the distance variable at both sides of a CMC circle.

[41] These counties tend to be far from a CMC circle and are unlikely to be comparable to the rest of the sample.

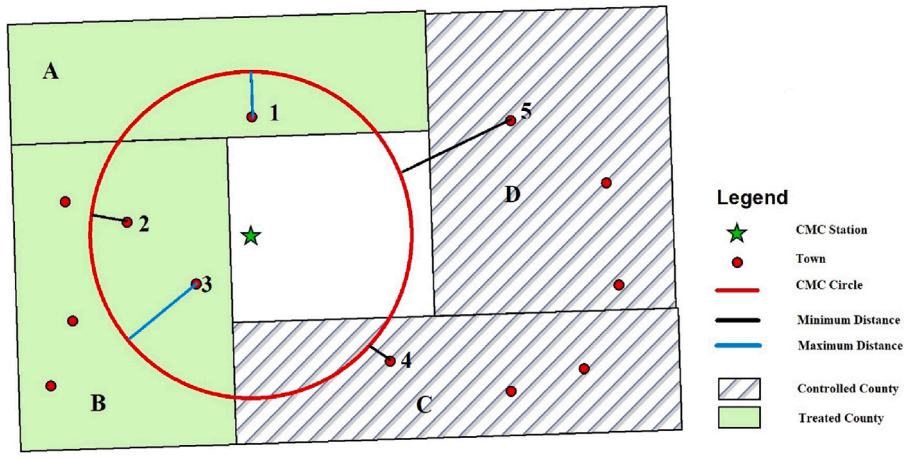

Fig. E.1. Example for the fuzzy RDD analysis.
Notes: In this example, *Treatment* equals one for all green counties (County A and B) and zero for all shaded counties (County C and D). In county A, *Dis_circle* is measured from town 1. In county B, *Dis_circle* is measured from town 3, which is larger than the distance measuring from town 2. In county C and D, *Dis_circle* is measured from town 4 and 5, respectively.

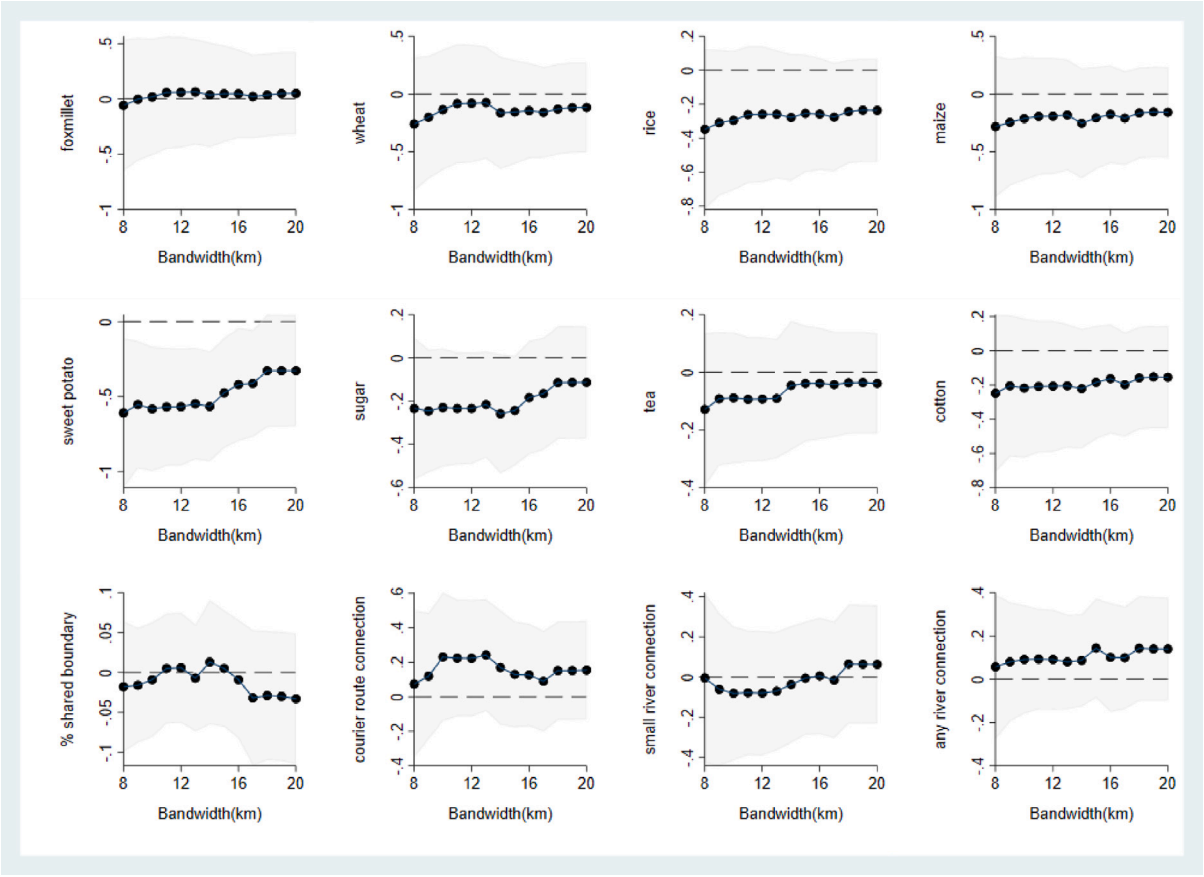

Fig. E.2. Balancing tests for the RD analysis.
Notes: The graphs plot estimated coefficients of *Treatment* against different bandwidths. Outcome variables are: agricultural suitability of foxtail millet, wheat, rice, maize, sweet potato, sugarcane, tea, cotton, as well as share of joint boundary, courier connection, small river connection, and any river connection. All estimations control for *Dis_CMC*, *Dis_circle*, and CMC fixed effects. The gray area indicates 95% confidence interval.

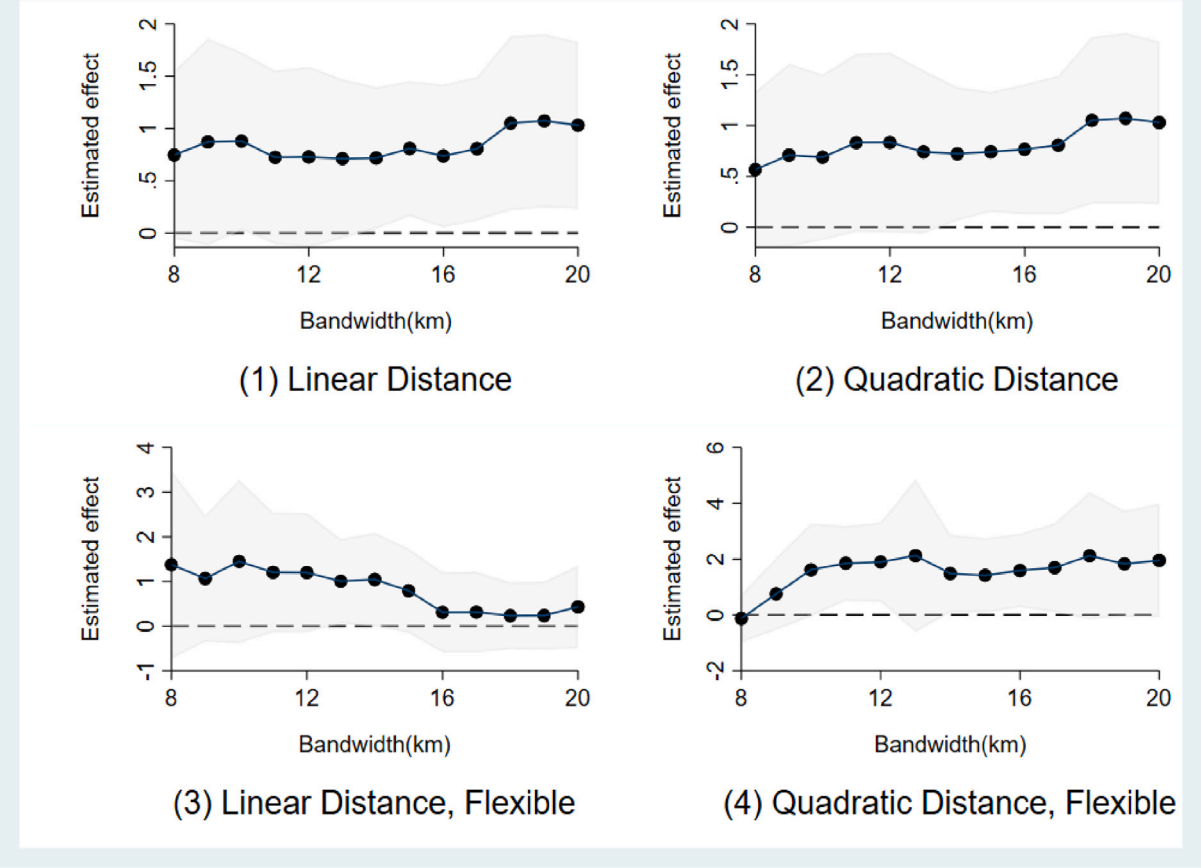

Fig. E.3. Fuzzy RD results.
Notes: The graphs plot coefficients of *CMC* against different bandwidths. Panels (2) and (4) use a quadratic distance term instead of a linear one, where panels (3) and (4) use a flexible distance term. All regressions control for a set of baseline OLS control variables and the CMC fixed effects. The gray area indicates 95% confidence interval.

and 20 km. A bandwidth of 8 km, for example, includes counties with *Dis_CMC* smaller than or equal to 8 km on both sides of the CMC circle. Reassuringly, I do not find any discontinuity for most of the pre-colonial development measurements across all bandwidths,[42] which suggests that the preconditions for a spatial RDD analysis are likely to hold.

Finally, Fig. E.3 reports the second-stage fuzzy RDD results using the average nighttime light for the period 2000–2010 as the dependent variable. Similarly, I plot the coefficients of *CMC* against the full range of bandwidth, with 95% confidence interval indicated by the gray area. The results show that counties affected by the CMC institution have a higher level of nighttime light luminosity than the unaffected ones. For example, in panel (1), the estimated coefficients are significant at the ten percent level across all bandwidths. Switching to a quadratic distance term (panel (2)) does not alter my findings in any significant way. The results are also robust, to a lesser extent, to the inclusion of flexible distance terms. For instance, panel (3) includes a flexible linear polynomial and finds a significant estimated effect at a bandwidth between 11 and 15 km. Panel (4) uses a quadratic flexible polynomial, and the results are significant at a broader bandwidth range of 10 to 20 km.

To sum up, the RDD analysis provides supportive evidence to my baseline findings that the CMC institution had a long-run impact on China's regional economic development.

[42] One outlier could be the suitability of sweet potatoes; counties where *Treatment* equals one had, if anything, lower suitability level for growing sweet potatoes. However, my fuzzy RDD results remain unchanged when I include sweet potato suitability as an additional control variable.

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