Robin Hood on the Grand Canal

Disrupted Trade Access and Social Conflict in China, 1650–1911 *

Yiming Cao †

Department of Economics

Boston University

Shuo Chen ‡

Department of Economics

Fudan University

August 7, 2020

Abstract

This paper examines the effects of the closure of China's Grand Canal — the world's largest and oldest artificial waterway — which served as a permanent shock to regional trade access. Using an original dataset covering 575 counties over 262 years, we show that the canal's closure led to social turmoil that engulfed North China in the nineteenth century. Counties along the canal experienced an additional 136% increase in the number of rebellions after the canal's closure relative to their non-canal counterparts. We explore several prominent mechanisms that potentially explain our results and find the most support for disrupted trade access, especially in urban areas. Our findings thus highlight the important role that continued access to trade routes plays in reducing conflict — a classic conjecture that has rarely been directly tested in a causal context.

Keywords: Trade Access; Conflict; Transportation Infrastructure; China

JEL Classification Numbers: O13, O17, D74, H56, N45, N95, Q34.

*Helpful and much appreciated suggestions, critiques and encouragement were provided by: Ying Bai, Samuel Bazzi, Eli Berman, Travers Child, Zhao Chen, Zhiwu Chen, Esther Duflo, Thiemo Fetzer, Raymond Fisman, Martin Fiszbein, Oded Galor, Yu Hao, Jiashun Huang, James Kung, Xiaohuan Lan, Pinghan Liang, Ruobing Liang, Gedeon Lim, Chicheng Ma, Robert Margo, Tianguang Meng, Nathan Nunn, Nancy Qian, Zheng Song, Shangjin Wei, Austin Wright, Chenggang Xu, and Noam Yuchtman; participants in the 2016 NBER Chinese Studies Group meeting, the 4th International Symposium on Quantitative History, NEUDC 2016 at MIT, the NBER Summer Institute 2017, and the ASSA Annual Meeting 2018; and seminar participants at Boston University, Fudan University, Harvard University, Jinan University, Peking University, Shandong University, the Southwestern University of Finance and Economics, and Tsinghua University. This work was supported by the National Natural Science Foundation of China (71933002) and the Innovation Program of Shanghai Municipal Education Commission (2017-01-07-00-07-E00002). We alone are responsible for any remaining errors.

†Address: 270 Bay State Road,Boston, MA 02215, USA, e-mail: *yiming@bu.edu*.

‡Address: 600 GuoQuan Road, Yangpu District, Shanghai, 200433, China, e-mail: *cs@fudan.edu.cn*.

Peace is a natural effect of trade.

— Montesquieu (1748)

1 Introduction

The question whether trade access enhances or undermines social stability — a notion that dates back to Montesquieu — fuels a long-running and controversial debate in economics and other social sciences. Yet there is little empirical research that directly examines this relationship in a causal context. Our paper addresses this question by analyzing the abandonment of China's Grand Canal — a plausibly exogenous policy shock that dramatically disrupted regional trade access — and its consequences for the rebellions that followed. In doing so, we also shed fresh light on chronic social disorder in nineteenth-century North China — a pivotal episode in Chinese history that until now has not been subjected to careful statistical analysis.

China's Grand Canal is the world's largest and oldest artificial waterway. For over 800 years it facilitated inland navigation and promoted the commercial prosperity of its neighboring markets. [1] The reliable functioning of the canal was maintained by the government, which relied on the canal to transport its grain taxes; this ensured that the canal was in good condition to be used also by private interests, for both trade and pleasure. [2] Starting in 1826, however, the government decided to shift its grain-tax transportation method from the canal to the East China Sea. This reform, while implemented progressively, led eventually to the complete closure of the canal and abruptly — although unintentionally — deprived the cities with direct access to this established trade route of that access. Social unrest followed shortly thereafter, which has been linked via anecdotal accounts to the canal's closure. This link has never, however, been systematically tested.

The historical context is well-suited to examining the consequences for social instability of disrupted trade access. Our setting offers three main advantages. First, the decision that led to the eventual abandonment of the canal was neither trade-oriented nor motivated by existing or anticipated rebellions, thus providing a plausibly exogenous shift in trade access. [3] Second, there is rich information available on social unrest in China over a long

[1] Adam Smith, in his work *The Wealth of Nations*, refers to China's Grand Canal as affording "an inland navigation much more extensive than that either of the Nile or the Ganges, or, perhaps, than both of them put together."

[2] In fact, the official transportation of grain taxes contributed greatly to commercial prosperity as the official boats were also allowed to carry some duty-free commodities.

[3] In particular, the decision was made in direct response to a temporary malfunction of the canal and arguably motivated by the preferences and political considerations of a newly enthroned emperor. See Section 2 for details.

period of time, which allows us to observe the entire reform process and examine both the short- and long-term consequences of disrupted trade access. Third, by focusing on a particular region in China, our setting is less subject to the many factors that would otherwise confound identification in cross-country settings, including ethnicity as well as institutional and cultural norms (Hegre and Sambanis, 2006; Laitin, 2007; Djankov and Reynal Querol, 2010; Kung and Ma, 2014; Janus and Riera-Crichton, 2015; Jha, 2013).

We construct an original dataset covering 575 counties over 262 years (from 1650 to 1911), extracted from archival records officially compiled by the Qing court. The records provide detailed information on the location and time of all rebellion onsets throughout the Qing Dynasty. We focus our analysis on the six provinces around the canal basin — a highly populated area that accommodated 15% of the world's population in 1820.

We begin our analysis with a standard differences-in-differences strategy. We compare changes in the number of rebellions in counties through which the canal ran or with direct access to the canal — hereafter "canal counties" — relative to those that occurred far from it. We choose 1826 — the first year of the reform — as the cutoff based on the pattern in the data, and have verified that there is no anticipatory increase in rebellions prior to the reform. Our findings indicate a higher number of rebellions associated with the canal's closure: compared with distant counties, canal counties experienced 0.0101 more rebellions after the reform than before. This effect corresponds to a 136% increase over the sample mean (0.0074), a finding that is significant at the 1% level. The estimates are robust to using various alternative specifications of the model, including the presence of rebellions as the outcome measure, as well as various combinations of fixed effects, imposing alternative distributional assumptions on the error term, and using a randomization-inference approach.

The baseline model with the binary treatment variable is then generalized to allow for greater variation in treatment intensity. We first show that the treatment effects are proportional to the length of the canal contained within a county. This finding is consistent with the historical background, which suggests that people involved in canal sailing suffered directly from the canal's abandonment. We also observe that the effects spread beyond the county boundaries and decreased with distance up to 150km from the canal. This pattern suggests that the reduction in general market access might also contribute to the effects we observe. Finally, we explore the heterogeneous treatment intensities across separate segments of the canal, and find that most of the effects come from the northern sections. This is not surprising, given that the northern areas suffered to a greater extent from the reduction in transportation and inadequate maintenance.

We conduct a number of robustness checks to address potential threats to our baseline estimation. The first focuses on the validity of the initial sample selection. We explore a variety of alternative approaches to defining the sample and conclude that our results are not driven by any specific choices regarding the sample. Our second robustness check considers the per capita measures of rebellions. While we do not have comprehensive population data at the county level, we show that our results are robust to normalizing the number of rebellions by the imputed population under a variety of reasonable assumptions. To address omitted variable bias, we include a set of additional controls that are common in the conflict literature (weather, geography, agricultural technology and culture) and our results remain largely unchanged. Finally, we discuss how the canal's abandonment interacts with two other major events during this period: the First Opium War and the Taiping Rebellion. We show that our results are unlikely to simply be capturing the impact of these confounding events.

We explore several mechanisms through which the canal's closure could potentially destabilize society: a reduction in repressive capacity, deterioration in agricultural productivity, and disruption of trade access. While the patterns we observe in the data are difficult to reconcile with the first two possibilities, we present evidence suggesting that the loss of trade access likely plays a role. Specifically, we find that: i) the canal's closure largely impedes the development of local markets, ii) the destabilizing effects are stronger in more densely urbanized places, and iii) access to alternative trade routes mitigate the destabilizing effects of the canal's closure. While none of these pieces of evidence is conclusive on its own, collectively they present a pattern suggesting that loss of access to trade was a channel through which the canal's closure destabilized society.

Our work contributes most directly to the long-standing debate over the implications of trade for political and social stability. The relationship is ambiguous in theory, hinging to a large extent on whether access to trade increases the availability of resources over which rivals fight or discourages citizens from participation in soldiering. [4] Empirical studies — in light of the endogeneity concerns — rely exclusively on trade volatility shocks at the intensive margin, which, however, produce rather mixed results. [5] Our work thus provides a unique contribution to the debate by focusing on the extensive margin, in which we find that the loss of trade access destabilizes society. [6] While we focus on a historical context that facilitates a causal interpretation, the implication of the study may be potentially pertinent to contemporary policy-making, especially in an era of significant backlash against global trade integration.

[4]See, for example, Hirshleifer (1989) and Grossman (1991) for the theoretical accounts, and McGuirk and Burke (2020) for an attempt to separate the two forces.

[5]See, for example, Dube and Vargas (2013) for a destabilizing effect and Bazzi and Blattman (2014) for a null effect.

[6]Berman and Couttenier (2015), while again focusing on price-volatility shocks, suggest that their impact depends to a large extent on geographical adjacency to seaports. They do not, however, explicitly evaluate changes in seaport access at the extensive margin.

More broadly, this study contributes to the large body of literature on economic shocks and civil conflicts (see Miguel et al. (2004) and Miguel (2005) for seminal contributions, and Blattman and Miguel (2010) for a recent review). Existing work, while applying effective identification strategies for causal studies, focuses on an extremely narrow subset of shocks — weather and price fluctuations — that are transitory and hit mostly rural areas. The implications of these studies may not be immediately generalizable to other increasingly prominent settings, such as one with a permanent shock hitting urban markets, in which case individuals have more outside options and may adjust to changes in their expectations. We therefore contribute to this literature by characterizing the dynamics of conflicts in response to a permanent negative trade shock plausibly hitting urban sectors, in which we see a pattern that consists of an immediate and sharp increase in an outcome followed by a convergence on a new, slightly higher equilibrium level. Our emphasis on the role of urban workers who found themselves unemployed by the trade disruption also echoes Dell et al. (2019) on the violent consequences of trade-induced worker displacement in Mexico as well as historical case studies of the origin of mafia-like activities in Chicago and New York City around the early twentieth century (Haller, 1971; Critchley, 2008).

Given our focus on the Grand Canal, we also contribute to the literature on the role of transportation infrastructure — the emphasis of which is mainly on roads and railways (see, for example, Fogel (1979) and Donaldson (2018)). We focus, instead, on a prevalent and longstanding means of transportation that, despite its advantages in terms of cleanliness and cost-efficiency, has been largely overlooked in the literature. Moreover, in contrast to the extensive body of work that evaluates the economic outcomes of such infrastructure in terms of productivity and income, our work is one of the small number of papers that illustrate their broader political and social implications. [7]

Finally, our work also sheds light on the chronic social disorder that afflicted nineteenth-century North China — an episode of pivotal importance in Chinese history. In particular, we focus on a key region that has been characterized as the home of persistent and recurrent turmoil for over a century, including a series of notable events such as the Nian Rebellion, the Boxer Rebellion, and the Green Gang. The area was also an early base for the Communist revolution. [8] This aligns our work closely with that of Bai and Jia (2016): both studies attribute the insurrections towards the end of Imperial China to the loss of economic opportunities; whereas they focus more specifically on a small group of elites who participated in the 1911 revolution, we shed light on the dynamics of social disorder over a longer period of

[7]For example, Perlman and Schuster (2016) examine the effects of rural delivery roads on voters' behavior in the US; Burgess and Donaldson (2010) suggest that access to railroads helps to mitigate famines in India.

[8]There have been extensive historical accounts of various social economic environments that might have contributed to these events. See, for example, Esherick (1988), Perry (1980) and Liu (2007).

time. [9]

The remainder of the paper is organized as follows. In the next section we present background information about the Grand Canal and its abandonment. In Section 3 we present the data. We formalize our empirical strategy in Section 4 and demonstrate the baseline results. Section 5 offers a set of robustness checks that address potential challenges to the results. In Section 6 we discuss the possible mechanisms and their implications, and Section 7 concludes.

2 Background

2.1 The Grand Canal

The 1,776 km Grand Canal is the longest and oldest artificial waterway in the world. Located in the north-eastern and central-eastern plains of China, it links Beijing in the north with Hangzhou in the south (see Figure 1). [10] The earliest parts of the canal were constructed in the fifth century BC, and the various sections were integrated into a nationwide system during the Sui Dynasty (581–618 AD). The scale of the Grand Canal was unparalleled in its time (Elvin, 1973). More than 126 million people lived in the six provinces the canal traveled through in 1820, which accounted for about 15% of the world's population.

The canal was originally constructed to secure Beijing's food supply. As the empire's capital and most populous city, Beijing had a population of over one million in 1820. Rice production was, however, clustered in the south, which featured abundant, fertile land and suitable weather (rain and sunshine) for agriculture. The Chinese government therefore adopted the "tribute grain" system to transport grains produced in the south to the north of the country via the Grand Canal. [11] In the early nineteenth century, approximately 3.5 million piculs of rice (roughly 560 million pounds) were delivered to the capital annually (Huang, 1918). Maintaining the canal was therefore one of the most crucial tasks for the Qing government (Hummel, 1991; Leonard, 1988; Cheung, 2009).

The canal also benefited adjacent regions by facilitating regional trade and providing job opportunities. The government allowed the grain junks to carry an estimated 200 million pounds of duty-free commodities annually in the early nineteenth century (Ni, 2005). Popular

[9] Historical revolts and conflicts in China have also been associated with other factors such as climate (Bai and Kung, 2011; Chen, 2015), agricultural technology (Jia, 2014) and social norms (Kung and Ma, 2014).

[10] The canal cuts across four provinces (Zhili (now Hebei), Shandong, Jiangsu, and Zhejiang) and runs very close to Henan and Anhui

[11] See Appendix A.1 for more historical background regarding the Tribute Grain system and the transportation of grains via the canal.

commodities ranged from bamboo, woods, paper, china and silk to pears, jujube and walnuts. Private junks also used the canal extensively for trade, travel and pleasure (Gandar, 1894; Hinton, 1952). As the only north–south waterway in east China, the canal facilitated the transportation of over 10 million piculs (roughly 1.5 billion pounds) of commodities each year. Moreover, transportation and trade along the canal created a wide range of jobs for urban areas. Workers were hired either by the government for sailing, boat construction, and canal maintenance or by the private sector in restaurants, hotels, and commercial services.

The Grand Canal thus boosted the economy along its route and created large commercial cities. For example, Linqing was a minor county before its construction. It developed into a trade center by the early Qing Dynasty, and was promoted to a municipality in 1777. The prosperity of the corridor was also reflected in its population density, which by 1820 was 45% higher in canal prefectures than in non-canal prefectures.

2.2 Abandonment of the Grand Canal

The government abandoned the Grand Canal during the second half of the Qing Dynasty. Beginning in 1826, the government decided to replace the conventional route of grain transportation via the canal with an alternative route through the East China Sea. Historians have posited two reasons for this decision. First, it was precipitated by the canal's breach at its junction with the Yellow River following severe storms and flooding in 1825, which halted the government's grain shipments through this route. Second, it reflected the personal preferences of the Daoguang Emperor, who succeeded the Jiaqing Emperor in 1820. [12] There is no evidence to suggest that the reform was motivated by previous or anticipated rebellions associated with the canal (see Appendix A.3 for further discussions).

It is worth noting that the reform was implemented gradually and involved frequent controversies. [13] The first phase, which began in 1826, was confined to a portion of the grains collected in Jiangsu Province. [14] It was then expanded to all grains collected in Jiangsu and Zhejiang Provinces in 1852, which together comprised about half of the tribute rice. The

[12] While flooding problems recurred during the Jiaqing Era (1760 – 1820), the former emperor Jiaqing — like his predecessors — was highly skeptical of the sea and expressly forbade any proposals related to sea transportation. This was an important obstacle to the development of sea transportation until the Daoguang Era (1820–1850). Appendix A.2 briefly discusses evolution of the early emperors' attitudes and policies toward the sea.

[13] There were extensive discussions and debates over the closure of the canal throughout the process, which reflected many contemporary political, social and economic considerations. We conducted a brief survey of the arguments in these discussions and found no indication that social instability was a motivation for advancing the reform. See Appendix A.3 for details.

[14] Despite the reduced amount of tribute grains transported, the canal's operation was mostly sustained during this period as the government continued its investment in restoring and maintaining it (Leonard, 2018).

canal's usage continued to decline in the face of the expansion of sea transportation and negligence. By the end of the nineteenth century, many sections — especially those in the north — were no longer navigable. The government officially announced the canal's closure in 1901.

The canal's abandonment necessarily deprived the canal cities of their access to this established trade route. The immediate losses came from the disappearance of grain boats and the tax-free commodities they carried. The canal's private use was also disrupted because it was in a state of disrepair. This situation forced most of the commodities to be transported by land, which was nearly ten times more costly in pre-modern China (Watson, 1972; Shiue, 2002), leading to a dramatic reduction in regional trade access. As a result, workers who lived by the canal lost their jobs. As shown in Figure 2, regions in which the canal was located did not recover from this recession until the early twentieth century. The population of Linqing — the most representative city in the canal's rise and fall — fell from over 200,000 in the late eighteenth century to fewer than 50,000 by the early twentieth century (Cao, 2001).

There is considerable anecdotal evidence that the closure of the canal was associated with subsequent social disorder in the region. Historians have documented that unemployed workers who lost their livelihoods following the closure — especially those directly involved in grain transportation and commercial services — contributed significantly to the formation and development of many of the groups of gangsters and rebels in the late Qing Dynasty, including the Nian Rebellion (Perry, 1980), the Boxing Rebellion (Esherick, 1988) and the Shanghai Green Gang (Martin, 1996). Folk wisdom also implies the potential existence of link between the canal's closure and social disorder. A popular ballad from nineteenth-century Shandong Province lamented the destructive consequences of closing the canal in the line "*broken the boat, disordered the world*" (Ni, 2005). Our paper offers the first systematic evaluation of the hypothesis implied in these anecdotes.

3 Data

We construct an original panel dataset from a number of historical sources spanning the period 1650 – 1911. Our dataset, which covers 575 counties in six provinces through which the Grand Canal ran (or to which it was adjacent), allows us to empirically test the effects of the canal's abandonment on social instability. We conduct our empirical analyses at the county (*xian*) level, which gives us two advantages. First, the administrative boundaries between counties remained quite stable during the study period (relative to those of provinces and prefectures) (Ge, 1997). Second, by examining the most disaggregated administrative division in historical China, we are able to assess the considerable heterogeneity that is likely to exist at higher levels (e.g. in provinces and prefectures). [15]

3.1 Rebellions

Our main dependent variable is the number of rebellions reported in each county and year. This information comes from *Qing Shilu* (Veritable Records of the Qing Emperors), the official record of imperial edits and official memorials about events of national significance. According to Chinese historians, *Qing Shilu* is a unique source that provides complete and systematic information on social unrest during the Qing Dynasty (Kung and Ma, 2014). Meticulously compiled by the Qing Court, it details the times and places of all rebellions during this period. To identify the link between local economic shock and social instability unambiguously, we focus our analysis on the onset of rebellions (although most of them are small in scale), excluding the continuation of existing rebel groups that may spread across multiple regions or last for years. The detailed coding procedure and the confounding complications are discussed in Appendix B. One type of measurement error that could potentially arise from our data-collection procedure is the double counting of a single event if it is reported multiple times (without evident linkages). To avoid this possibility, we construct both binary (presence) and the quantitative (number) measures of rebellions. During our sample period, there were a total of 1, 141 reported rebellions (4.35 annually). The sample means of the presence and number of rebellions are 0.0073 and 0.0076, respectively. [16]

3.2 Intensity of the Canal's Impact

We determine the intensity of the canal's impact by reference to the geographic locations of the affected counties. The locations are obtained from the digital maps available on the China Historical GIS Website. [17] We employ both discrete and continuous measures of intensity. The discrete measure is a dummy variable indicating whether the canal runs through (or is adjacent to) a county. In our sample, the canal runs through (or is adjacent to) 73 of the 575 counties. The continuous measures of intensity are defined in two ways: the distance from a county's administrative center to the canal and the length of the canal contained

[15] For example, Cao County experienced 21 rebellions during the Qing Dynasty, while the most stable county in the same prefecture experienced only three.

[16] Another measure of rebellion that might be worth considering is per capita rebellions normalized by population. Unfortunately, we cannot effectively construct such a measure because we lack population data at the county level. We can observe the population only at the prefecture level and for only six individual years (1600, 1776, 1820, 1851, 1880 and 1910). As we will elaborate later in Section 5.2, this imperfection in the data should not undermine the essence of our findings. In particular, we will show the robustness of our results when normalizing rebellions by imputed population under reasonable assumptions.

[17] http://www.fas.harvard.edu/~chgis/

within the county's boundary. The average distance from the canal is 118 km in the sample, while the farthest county is 499 km away. The length of the canal segment contained in a county is 32.45 km on average, with the longest running 91.44 km.

3.3 Control Variables

We include the following controls that we extract from the conflict literature to eliminate certain omitted variables:

Climate The first control variable we consider is climate shocks (Miguel et al., 2004; Miguel, 2005; Hsiang et al., 2011, 2013). We obtained climate information from two independent sources. One is the historical temperature reconstructed by Mann et al. (2009) at 5×5 arc degrees based on $1,209$ geological proxy records over the past $1,500$ years (based e.g. on tree-rings, coral, sediment, etc.). We assigned grid-cell temperatures to counties in our sample and define a temperature anomaly as a temperature that was beyond one standard error of the mean for all years. In our sample, an anomalous temperature was recorded every three years in a county. The other source is the presence of extreme drought and flood, as compiled by Chen and Kung (2016). A representative county in our sample experienced extreme drought every 10.24 years and extreme flooding every 13.44 years. We plot the spatial and chronological distribution for each of the three climate measures in Figure C2. We do not see any evidence of climate shock specific to the canal area or around 1826.

Geography We include two geographical measures in our analysis. The first is the terrain ruggedness index suggested by Nunn and Puga (2012), which is based on the square root of the sum of the squared differences in elevation between one central grid cell and the eight adjacent cells (Riley et al., 1999). Grid-cell elevation per 30×30 arc seconds is obtained from GTOPO30 (Survey, 1996). For each county, the ruggedness index is constructed by computing the mean of all grid-cells contained within it. The spatial distribution of the ruggedness index is depicted in Figure C3, with a mean of 16.92 and a standard deviation of 19.53. Second, we include two geographical measures that could prove relevant in our historical context: the distance to the Yellow River (the site of the breach of 1825) and the distance to the coast (the alternative grain transportation post reform route). The average distance from the river and from the coast is 297 km and 200 km, respectively.

Technology Jia (2014) suggests that social conflicts are also subject to technological changes in the agricultural sector, especially the introduction of New World crops (Jia, 2014; Iyigun et al., 2015). Such changes may also lead to inconsistent estimates to the extent that crops spread along the canal. Therefore, we also controlled for the planting duration of maize and sweet potato, the two most important New World crops in China (Jia, 2014; Chen and Kung, 2016). Figure C4a shows the year when the two crops were first adopted, which does not appear to depend on the canal. Figure C4b calculates the number of counties in which the crops have been adopted for each year. Again, the spread of the crops does not coincide with the reform.

Culture The literature maintains that culture is another factor that underpins violence (Jha, 2013; Voigtländer and Voth, 2012; Grosjean, 2014). In China, Confucianism represents one type of cultural norm for alleviating conflict (Kung and Ma, 2014). Therefore, we include the number of *jinshi* — the highest attainable qualification under China's civil exam, which focused on Confucianism — as a measure of Confucian culture. [18] In our sample, the average number of *jinshi* is 0.16 per county year. The spatial and chronological distribution of *jinshi* is depicted in Figure C5a, and does not appear to be associated with the reform.

Table 1 summarizes the sources of and descriptive statistics for all the variables used in our analysis. In addition to the main variables noted above, we also used additional variables to help us distinguish between the mechanisms that potentially explain our findings. The sources of and descriptive statistics for these variables are listed as "Supplements" in Table 1.

3.4 Suggestive Evidence

Before proceeding to the formal analysis, we provide some descriptive evidence to help place our findings in context. Figure 4 shows the distribution of rebellions over time. It shows clearly that the frequency of rebellions significantly increases following the abandonment of the canal: from 1.37 annually before 1825 to 10.47 annually afterwards. The number keeps increasing until the peak occurs in 1861, in which year a total of 66 rebellions take place, after which the number of rebellions does not fall until the 1870s. The spatial distribution of the rebellions also reveals a potential relationship between the canal's abandonment and social instability. The left panel of Figure 5 shows the distribution of rebellions in the pre-abandonment period, while the right panel shows the distribution in the post-abandonment

[18] It is worth noting that the number of *jinshi* can be used to capture the accumulation of human capital and the presence of elites. Insofar as mastery of Confucian classics was the sole criterion for *jinshi* selection, we believe it provides a good proxy for the intensity of Confucian culture. It is beyond the scope of this paper to disentangle its effects from those of other confounding factors.

period. The color intensity represents the number of rebellions reported. Before the abandonment, the rebellions are less frequent but more widely dispersed. Afterwards, the total number of rebellions increases, and, more importantly, the relative change is greater in areas located closer to the canal. This evidence of temporal as well as spatial distribution suggests that the abandonment of the canal may have contributed to the overall increase in rebellions in the nineteenth century.

Table 2 translates the above pattern into a more precise but naïve calculation. We compare the relative change in the number of rebellions before and after the canal's abandonment in both areas and perform a standard t-test. Consistent with our previous observation, rebellions are more frequent in canal counties and in post-abandonment years: the frequency of rebellions increases by 0.0240 and 0.0146, respectively, for canal and non-canal counties after the canal's closure in 1826. The relative change is 0.010 higher at the 1% significance level for canal counties, accounting for a 135% change relative to the sample mean of 0.0074.

4 Empirical Strategy and Results

In this section we estimate the impact of the abandonment of the Grand Canal on rebellions. Section 4.1 characterizes our DID strategy and validates the identification assumptions. Section 4.2 presents our baseline estimates of binary treatment effects. We extend our analysis to allow for greater variation in treatment intensity in Section 4.3.

4.1 Empirical Strategy

Our empirical strategy follows the standard DID approach, in which we compare the relative change in the number of rebellions in counties through which the canal runs relative to counties that are distant from it. The model specification takes the following form:

$$Y_{ct} = \beta Bordering_c \times Post_t + \delta_c + \sigma_t + \chi_{ct} + \varepsilon_{ct} \qquad (1)$$

where c indexes counties and t indexes years. [19] The outcome of interest, denoted Y_{ct}, is the number of rebellions recorded in county c in year t. $Bordering_c$ is a dummy variable that equals one if a county contains a stretch of the canal and zero otherwise. Hence, the treated group comprises canal counties while the control group comprises other (non-canal)

[19] We use a linear frequency model because the coefficient and marginal effects of the interaction term are not readily interpretable in nonlinear settings such as Poisson or negative binomial regressions (Ai and Norton, 2003; Puhani, 2012) and the incremental effects of the interaction term coefficient are not estimable when we have county and year fixed effects in nonlinear models.

counties. $Post_t$ is a dummy variable that equals one for the years after the abandonment. The equation also contains controls for county and year fixed effects, δ_i and σ_t; χ_{ct} denotes other time-variant controls. The coefficient of interest in equation (1) is β, the estimated impact of the canal's abandonment on the number of rebellions. The coefficient is expected to be positive, which would suggest a greater increase in the number of rebellions in canal counties.

The estimation strategy has all the advantages and potential pitfalls of standard DID estimators. County fixed effects control for all time-invariant factors that differ between counties (such as location, size and topography). Year fixed effects control for any secular patterns of rebellions that affect all regions in a similar way (such as an overall change in control of the government). The time-variant controls in the baseline include the interaction of the province and year dummies and the linear time trends at the prefecture level (along with other controls for robustness checks). The identification relies on the assumption that there are no other events beyond those for which we have controlled that occur simultaneously with the reform and affect social unrest. We should not take this assumption for granted because China experienced a number of changes during the nineteenth century (in particular, the Opium War and the Taiping Rebellion). We address this issue in Section 5.

The empirical design requires that we pick a cutoff in the timeline that defines the pre- and post-reform periods. As discussed in Section 2, although the first phase of the reform began in 1826, much of it was accomplished gradually over the next 30 years. This suggests that the reasonable cutoffs could range from the 1820s to the 1850s. Rather than assume a specific cutoff *a priori*, we rely on the data to determine the most appropriate cutoff that is consistent with the pattern in the data. Specifically, we estimate a fully flexible decade-by-decade estimating equation that takes the following form:

$$Y_{ct} = \sum_{\tau=1780}^{1880} \beta_\tau Bordering_c \times Decade_\tau + \delta_c + \sigma_t + \chi + \varepsilon_{ct} \quad (2)$$

where all variables are defined as in Equation (1). The only difference from Equation (1) is that in Equation (2), rather than interacting $Bordering_c$ with a post-reform indicator variable, we interact the treatment variable with each of the decade fixed effects, treating the years before 1780 as the reference group. The estimated vectors of β_τ reveal the differences between the treated and control counties during each decade. If, for example, the canal's abandonment increases rebellions, then we would expect the estimated β_τ to be constant over time for years before the reform took effect. We would also expect the coefficients to increase as the reform advanced.

Figure 6 plots the point estimates of Equation (2) and their 95% confidence intervals. A clear pattern emerges from the figure. The difference between the treated and control groups is constant over time and small in magnitude before the 1820s. It begins to increase in the 1820s, reaches its peak in the 1850s and starts to converge from the 1860s onwards. This pattern is consistent with the timeline that we have documented in the historical background, suggesting that the reform starts to take effect immediately during the first phase, and that its impact increased as the reform advanced. Therefore, we choose 1826 (when the reform was initiated) as the cutoff in our analysis.

The point estimates and confidence intervals shown in Figure 6 also suggest that there are no differential trends between the two groups prior to the reform, which is the key assumption of our identification. We formally test this assumption for additional verification by restricting our sample to the 50 years before the reform and estimate a variant model of Equation (1):

$$Y_{ct} = Bordering_c \times Year_t + \delta_c + \sigma_t + \chi_{ct} + \varepsilon_{ct}. \tag{3}$$

In this model, the coefficient β of the interaction term $Bordering_c \times Year_t$ captures the difference in time trends between the treated and control groups. We summarize the results in Table 3, with varying combinations of fixed effects included to derive the results reported in the columns of the table. [20] Across all columns the differences are tiny and statistically nonsignificant, which confirms that there are no pre-existing differential trends in the data and that the model's identification assumption is satisfied.

4.2 Baseline Estimates

We present our baseline estimates derived from Equation (1) in Table 4, where the dependent variable is the number of rebellions. The four columns reflect varying combinations of fixed effects. For column (1), we control for county and year fixed effects. This allows us to rule out all time-invariant county features (e.g. location) and year shocks that unanimously affect all regions (e.g. the overall control of the government). We then include province-year fixed effects to rule out differential time effects across provinces and report the results in column (2). For column (3) we include prefecture-specific year trends to account for differences in regional trends. For column (4) we include all sets of fixed effects simultaneously. For each column, the standard errors reported in the parentheses are clustered by county. [21] To account for potential spatial correlations among neighboring counties, we also include

[20] The combinations of fixed effects reflected in the columns are analogous to those in our baseline estimates, which we present in Table 4.

[21] We also allow standard errors to be clustered by prefecture to account for within-prefecture correlations. The results are reported in Table C1. The standard errors are larger, yet our estimates remain significant at the 10% or higher level.

Conley standard errors in square brackets, following the approaches suggested in Conley (1999) and Conley (2008).

The results obtained across all specifications are positive and significant, suggesting that a large number of rebellions are associated with the canal's abandonment. For example, the point estimator reported in column (1) represents 0.0101 more rebellions experienced by canal counties (compared with what their non-canal counterparts experienced) after the reform (relative to before). This effect corresponds to a 136% increase from the sample mean (0.0074), and is significant at the 1% level. The estimated coefficients reported in columns (2) through (4) exhibit magnitudes and significance levels that are similar to those reported in column (1). [22]

The analysis is replicated in Table C2 using binary outcome measures to avoid double counting issues in the data. The dependent variables are coded 1 if there were any rebellions that year and 0 otherwise. The estimated coefficients and standard errors are almost unchanged from those reported in Table 4, which confirms that our results are immune to potential counting problems.

Finally, the types of standard errors we obtained in our baseline require technical assumptions about the distribution of the error term. We run a Monte Carlo simulation that works out the sampling distribution by fitting the model to "placebo laws" that do not impose any specific assumptions on the error distribution. To do this, we first pick the set of canal counties at random, holding the total number of canal counties constant. Thus, the total number of canal counties is the same as the actual number, but the distribution is randomly generated. We then estimate Equation (1) using the simulated data to obtain the placebo treatment effect. We replicate this exercise repeatedly and compare the treatment effect in the baseline with the distribution of the randomly generated placebo treatment effects (the placebo law distribution). Figure 7 plots the distribution of t-statistics for the placebo treatment effects after $1,000$, $3,000$, $5,000$ and $10,000$ iterations. The vertical lines mark the location of the estimated coefficient of the actual treatment (as in Column (1), Table 4). The share of placebo estimates that is larger than the actual coefficient ($P(\hat{\beta} \leq \beta)$) can be interpreted as analogous to a p-value. It suggests the probability that a randomly assigned treated group could present the same effect as (or be larger than) the actual treated group. As such, we can reject the null of no treatment effect at about the 1% level of significance. This confirms our baseline findings without relying on assumptions about the shape of the error distribution.

[22] We also note that the Conley standard errors reported in square brackets are much smaller than those clustered at the county level, suggesting the potentially negative spatial auto-correlations between neighboring counties. This could be explained by the movement from other places to a single county of people considering rebelling. Our estimation would be even more significant after accounting for this possibility.

4.3 Heterogeneous Treatment Intensity

One limitation of our empirical study is that we lack well-defined treatment and control groups. At baseline we specify a binary treatment variable and draw natural comparisons between the canal counties and the rest of the counties. In doing so we implicitly assume that the treatment is uniform and confined by the counties' boundaries. This assumption might, however, overlook the heterogeneity in treatment intensity arising from a rich historical background. Our discussion of the historical background in Section 2 suggests several channels that could generate such heterogeneity. In this section, we generalize the specification by allowing for greater variation in treatment intensity implied by history without distinguishing between these non-exclusive channels.

We begin by exploiting the fact that the immediate losses caused by the reform were levied on those who were directly involved in the canal-related sailing industry. This suggests that, for canal counties, the treatment intensity should be proportionate to the length of the portion of the canal contained within the county. We confirm this by estimating the following specifications:

$$Y_{ct} = \beta Length_c \times Post_t + \delta_c + \sigma_t + \chi + \varepsilon_{ct} \tag{4}$$

$$Y_{ct} = \sum_{\iota=0}^{80} \beta_\iota Post_t \times Length_c^\iota + \delta_c + \sigma_t + \chi + \varepsilon_{ct} \tag{5}$$

where Equation (4) assumes a linear function of canal length while Equation (5) uses a more flexible specification to estimate a separate coefficient for each length interval. The coefficient β estimated from Equation (4) represents the relative change in the number of rebellions per 10 kilometers of canal. It is expected to be positive, because counties containing longer segments should be treated more intensively if sailing matters. As a further extension, the β_ι coefficients derived from Equation (5) estimate the treatment effects for each of the 10-km intervals with counties located away from the canal (i.e., the baseline control group) as the reference group. Accordingly, we would expect these estimates to be increasing with ι's.

The coefficients estimated from Equation (4) with the baseline fixed effects are reported in the first two columns of Table 5. As expected, the estimated coefficient β suggests an increase of 0.003 rebellions per 10 kilometers of canal, which is significant at the 5% or higher level. This means that counties with an additional 25 km of canal running through them experienced doubled rebellions relative to the sample mean. [23] To further illustrate these results, Figure 8 plots the coefficients along with the 95% confidence intervals estimated

[23]To confirm robustness, we also analyze models in which the length of the canal is normalized by the size of the county (i.e., a density measure). The results, which are available from the authors, are consistent with our non-normalized estimates.

from Equation (5). The coefficients increase with the length of the canal, but do not reach statistical significance until a length of 40 km. Therefore, in counties through which only small portions of the canal run, the number of rebellions is not statistically different from the number of rebellions in counties located away from it. In other words, the treatment effects we observe at baseline come primarily from counties that contain longer sections of the canal.

We have also suggested in the historical background that the reform may also hit neighboring counties by restricting their access to markets (Donaldson and Hornbeck, forthcoming). As a result, the treatment may not be restricted to canal counties; rather, the impact may spread beyond county boundaries, with its intensity decreasing with distance from the canal. This prompts us to exploit variations in treatment intensity as a function of distance:

$$Y_{ct} = \beta Distance_c \times Post_t + \delta_c + \sigma_t + \chi + \varepsilon_{ct} \tag{6}$$

$$Y_{ct} = \sum_{\rho=0}^{400} \beta_\rho Post_t \times Distance_c^\rho + \delta_c + \sigma_t + \chi + \varepsilon_{ct} \tag{7}$$

where $Post_t$ is interacted with $Distance_c$, the distance to the canal, and each of the 25-km distance intervals,$Distance_c^\rho$, respectively. [24] The estimated coefficient β from Equation (6) represents the relative change in rebellions per kilometer away from the canal, which is expected to be negative as the impact diminishes. Equation (7) estimates the treatment effects for each of the 25-km distance intervals; counties located 400 km away from the canal serve as the reference group. We expect their estimates to be smaller over longer distances.

In the last two columns of Table 5 we present the estimated coefficients from Equation (6). Consistent with our expectations, the estimates suggest that approximately 0.005 fewer rebellions occur per 100 km away from the canal, a finding that is significant at the 1% level. Figure 9 plots the estimated coefficients from Equation (7) for each of the distance intervals. The coefficients decrease steadily and remain significant up to 150 km away from the canal. Because counties located more than 150 km from the canal do not experience more rebellions than do those that are farther away, we interpret 150 km as the range of the canal's impact.

Finally, we explore the heterogeneous treatment intensity across separate sections of the canal, as regions in the north suffered more severely from negligence. To this end, we construct two section-specific treatment variables to separately estimate the effects along

[24] Distance is measured from the county's administrative center to the canal. The results are also robust to using the shortest distance between the county boundary and the canal (available upon request).

separate sections of the canal:

$$Y_{ct} = \beta_1 AlongNorthCanal_c \times Post_t + \beta_2 AlongSouthCanal_c \times Post_t + North_c \times Post_t + \delta_c + \sigma_t + \chi + \varepsilon_{ct} \quad (8)$$

where $AlongNorthCanal_c$ and $AlongSouthCanal_c$ are two indicators that take the value of 1 if county c is located along the northern (or southern) part of the canal. [25] We also include interaction between the indicators of northern counties and post-reform periods, $North_c \times Post_t$, which captures the average difference between the north and the south, so that we are comparing counties along the northern (southern) part of the canal only to other counties in the north (south).

The results are reported in Table 6. We first notice that most of the results we observed at baseline come from counties in the northern sections of the canal: the magnitude is larger and highly significant across all specifications. The effects in the south are positive but cannot be statistically distinguished from those observed in other counties in the south. This is not surprising, for two reasons related to the historical background. First, because tribute grain (and other commercial activities in general) was not transported solely via the canal south of the Yellow River, the abandonment of the canal had a comparatively minor impact there. Second, the canal's navigability was mostly preserved in the southern portion, so trade access there was barely impeded. We also find that in the northern portion away from the canal more rebellions ocurred than in non-canal counties in the south (despite the smaller magnitude and lack of statistical significance in the strictest specification). This is consistent with our previous results indicating that the reform also spread beyond county boundaries into nearby non-canal counties, perhaps via the market access channel.

Overall, we conclude that the heterogeneous patterns we observe in the data are consistent with the differential intensity of treatment suggested in the historical background.

5 Robustness

This section offers a set of robustness checks that address the possible threats to our baseline estimation. We show that it is relatively unlikely that our results are biased by sample selection, measurement errors, or omitted variables, either in the literature or in the historical context.

[25] We define the northern and southern parts of the canal by its intersection with the Yellow River. Counties north of the Yellow River were more intensely affected by the canal's abandonment because transportation in the northern sections relied more heavily on the canal, and maintenance was a larger problem in the north Yellow River sections

5.1 Alternative Sample Selection

Our empirical strategy requires us to choose the control groups that are sufficiently comparable to the treated groups yet are relatively immune to the reform. Insofar as the economic impact of the canal is not confined to administrations in direct geographical contact with the canal (as we have seen in Figure 9), there is a tradeoff between choosing counties that are close enough to those located near the canal and those located relatively farther away and thus less likely to be affected by the canal's abandonment. Similar issues arise in our choice of the time window of analysis: we would like a window wide enough to enable us to investigate the long-term dynamics, but not so wide that we are detecting the impact of other (irrelevant) events.

Instead of claiming that our current selection is optimal, we show in Table 7 that our empirical findings are robust to a variety of alternative selections. Panels A through E present estimates using 50-, 100-, 150-, and 200-year windows as well as the full 262-year window. In each panel, we report the estimated treatment effects from several alternative samples. To obtain the results reported in column (1), we restrict our sample to canal prefectures, which covers 190 counties (about a third of the original sample). Columns (2) – (4) present the results from a distance-based sample selection that includes counties within 100 km, 150 km and 200 km of the canal. For column (5) we use the full sample of 575 counties. Finally, for column (6), we aggregate our measures at the prefecture level and compare canal prefectures with those located away from it. In all these analyses, we include county(prefecture) fixed effects, year fixed effects, and province × year fixed effects, and cluster the standard errors at the county (prefecture) level. Overall, the estimates are quite robust and are not subject to the specific sample. [26]

5.2 Per capita Measure of Rebellion

Another limitation of our analysis is that we lack county-level population data. As explained in Section 3, population data are available only at the prefecture level and for six points in time (1600, 1776, 1820, 1851, 1880 and 1910). This prevents us from effectively constructing a per capita measure of rebellions. However, we do not believe this limitation alters our findings. The historical evidence suggests that regions located close to the canal experience greater population losses in the post-reform period. Conditional on county fixed

[26]We do observe that the estimates reported in the first two columns are much smaller and sometimes not statistically significant at conventional levels when we apply severe restrictions on the size of the sample. This is probably because the effects spread beyond county boundaries via the market access channel. If we focus on the specifications that include all counties within 150km of the canal (the scope of the spread of the impactas suggested in Figure 9), both the magnitude and the level of significance are highly robust across all sampling methods.

effects (that capture each county's average population over the period), omitting yearly population would overestimate the difference in rebellions between the two groups in the first period and underestimate the difference in the second period, leading to an overall underestimation of the difference-in-differences effects. More formally, we show that our findings are robust to normalizing the number of rebellions by imputed population under reasonable assumptions.

The imputation takes two steps. First, we calculate the yearly population at the prefecture level assuming linear changes in each of the intervals between the six available years. [27] We then impute the county-level population by assuming a Zipf distribution within each prefecture and allocate more inhabitants to larger counties. [28] We also consider the worst-case scenario in which canal counties had the highest populations (regardless of size) and assign all the largest imputed numbers to those counties. While the imputation could be inaccurate, it gives us a sense of the robustness of our results when using per capita outcomes.

Table 8 summarizes the results obtained where the dependent variables are the number of rebellions normalized by (imputed) population. In the first two columns, we show estimates from the more highly aggregated prefecture-level analyses, which compare canal prefectures with non-canal prefectures. [29] While aggregating at the prefecture level discards all within-prefecture variation, it allows us to use the relatively more accurate measure of population obtained from the first step of imputation (assuming only linear population changes to occur between years). We find a positive and significant effect of 0.024 more rebellions per million inhabitants in canal prefectures after the reform. The coefficient represents a 93% increase over the sample mean, which is lower than what we estimated at baseline. [30] For the next four columns, we employ less highly aggregated data with additional assumptions regarding within-prefecture population distributions. For columns (3) and (4) we assume a Zipf distribution as well as that larger counties house more residents. The coefficient we estimate from this exercise is 0.037, representing a 131% relative increase. This increase is larger than those in our prefectural-level estimates and is almost identical to baseline. For the last two columns, we consider the worst-case scenario in which all populated counties are distributed along the canal. The relative increase in rebellions would have been 89% under this scenario. We interpret this finding as the lower bound of our estimates in per capita

[27] Our results are robust to allowing polynomial or other smooth changes to occur between years.

[28] More specifically, we assume the population in the N^{th} most populated county pop_N within a prefecture to be $pop_1^{-\alpha}$, where the parameter α is the average of what we estimate using prefecture-level data in each of the six observable years.

[29] For prefectural analyses using the number of rebellions as the outcome without normalization, see Table 7 in the previous section.

[30] It is not surprising that our prefecture-level analysis yields a milder effect, as it discards all within-prefecture variation.

terms. Given these results, we believe the lack of population data does not undermine the validity of our findings.

5.3 Additional Controls

Conflict has been attributed in the literature to many factors, including climate shocks (Miguel et al., 2004; Miguel, 2005; Hsiang et al., 2011, 2013), geography (Nunn and Puga, 2012), technological changes (Jia, 2014), and culture (Jha, 2013; Voigtländer and Voth, 2012; Grosjean, 2014). If changes in these factors coincide with the abandonment of the canal, it could cause our estimates to be systematically biased. To address this concern, we include a variety of control variables for each of these factors to examine the robustness our of results.[31]

Table 9 summarizes the results. In the first three columns we report results obtained by including controls for three types of climate shocks: extreme temperature, droughts, and floods. Consistent with findings reported in the literature, we find positive effects for all three measures. The significance level is somewhat marginal, presumably because the region's climate is relatively homogeneous. Our main variable of interest, $Canal \times Post$, remains unchanged.

For columns (4) – (6) we control for a set of geographical measures, each interacted with the *Post* dummy that allows us to analyze differential effects before and after the reform. The first geographical measure is the ruggedness index proposed in Nunn and Puga (2012). Consistent with their findings, we observe in our data that rebellions after the reform tend to take place in less rugged areas. The canal's effect is reduced by 24% conditional on land ruggedness, but is still positive and significant. Next, we include controls for distances to the Yellow River and the coast, the two geographical destinations relevant in our historical context. We find that the frequency of rebellions decreases with distance to the Yellow River, but the effect of the canal is larger when the Yellow River is accounted for. We do not find that distance to the coast affects rebellions after the reform, because the ocean shipping route did not make frequent stops along the coast and therefore could not deliver as many economic benefits as the canal.

To obtain the results reported in columns (7) and (8) we consider the effects of technological changes in agriculture, measured by the introduction of maize and sweet potatoes, the two most important New World crops in historical China (Jia, 2014; Chen and Kung, 2016). Both coefficients are positive but neither is statistically significant. For column (9) we

[31] We do not include these controls in our baseline specifications for two reasons: first, some of these variables are not available for all counties in our sample, which could make the sample unbalanced; second, we consider multiple measures for each of the factors, which could cause co-linear issues or work as bad controls. Therefore, we decided to include these control variables to check robustness.

control for the number of *jinshi* (successful civil exam candidates) as a measure of Confucian culture; it does not have a significant impact.

Our main variable of interest — the effect of the Grand Canal — is positive and significant across all specifications. More importantly, the estimated coefficients are almost identical to our baseline estimation, suggesting that our findings are not biased by omitting any of these control variables. This is reconfirmed in column (10), where the reported results were derived after controlling for all the factors mentioned above.

5.4 Other Historical Events

We also explore how our findings interact with other major events in historical China, in particular the First Opium War (1840–1842) and the Taiping Rebellion (1851–1864), which occurred during the canal's abandonment and coincided with the major advancements of the reform. Both events started beyond but moved toward our sample area in their later stages. The First Opium War started in Guangdong Province in 1840, and most of its early campaigns took place around the Pearl River Delta in Guangdong and the southeastern coast. It was not until early 1842 that the British Army sought to cripple the finances of the Qing Empire by striking up the Yangtze River. After capturing Ningbo, Shanghai and Zhenjiang in July, the British fleet cut off the Grand Canal, effectively bringing the war to an end in August 1842. The Taiping Rebellion, on the other hand, overlaps more with our sampled area. It started in Guangxi Province in 1851 and moved along the Yangtze River into Anhui, Jiangsu and Zhejiang Provinces. The rebels captured the city of Nanjing in 1853, declaring it the capital of their kingdom, but failed in their effort to head north into Shandong and Zhili over the next two years. The tribute grain system was completely paralyzed during this period as the Taiping Army had taken control of the areas from which the grain was transported.

These two events might have contaminated our analysis in either of two ways. The immediate concern is that our measure of rebellions might capture simply the campaigns of the British and Taiping armies. Three reasons persuade us that this is not likely. First, we have restricted our analysis to rebellions known to have broken out locally and excluded the actions of existing rebel groups (see Appendix B for details regarding how we identify the two types). As a result, the British and Taiping campaigns should not be directly reflected in our accounting of rebellions. Second, although both the British and Taiping armies sought to take control of the area where canal transportation originated (in particular, the cities of Nanjing and Hangzhou), neither advanced their campaigns very far along the canal —

especially its northern portions. [32] Our results are driven primarily by counties in the north (as shown in Table 6), so the chance that they capture nothing but the British and Taiping campaigns should be relatively small. Third, as indicated by the results we present in Panel A, Table 7, our estimates are robust to restricting our sample to the 50-year window (1801–1850) before the Taiping Rebellion started.

A second concern is that the reported number of rebellions might be less accurate in regions affected by the Opium War or the Taiping Rebellion. The government might have limited information on social disorder in those occupied regions, raising the noise level in the numbers being reported. To address the concern that our results might have been biased by inaccurate information from those regions, we re-run our analyses while excluding counties directly affected by these events. [33] The results are reported in Panel A, Table 10. For columns (1) and (2), we exclude counties where battles between the British Army and the Qing government took place. It is not surprising that our results are almost unchanged, given that the Opium War affected only a small subset of our sample counties. The Taiping Rebellion was more influential, as nearly half of the counties in our sample were directly affected by that conflict. In columns (3) and (4), however, the results we report include estimated coefficients that are even larger when we exclude the Taiping-affected regions. As a result, we believe our results are not subject to inaccurate information collected in the occupied regions.

A third possibility is that the campaigns of the British and Taiping campaigns could have interacted with the reform to influence local rebel uprisings. Two possible channels are worth noting: 1) the British and Taiping campaigns could have encouraged the local population to also rebel against the government; 2) these campaigns could have recruited new rebels into their ranks and thus substituted out demand for local rebellion. To further explore whether it was the complementary effect or the substitution effect that played a major role, we tested triple interactions between the canal's abandonment and the occupied regions during the two events and report the results in Panel B, Table 10. The results reported in columns (5) and (6) indicate that the Opium War produced little interaction with our estimates, which is consistent with our previous analysis. For the Taiping Rebellion (the corresponding results

[32] The British Army's campaign ended after it occupied the city of Zhenjiang (where the canal and the Yangtze River intersect), which forced the government to enter negotiations. The Taiping rebel group launched its Northern Expedition in 1853, aiming to seize the capital of Beijing. They did not head north through the Grand Canal, however, because of the difficulties associated with crossing the Yellow River. Instead, they marched westward into Henan and Shanxi Provinces before turning north-east toward Beijing. The expedition was destroyed by the Qing Emperor in 1855.

[33] For the Opium War, the directly affected counties are defined by the sites where the battles took place (the Battles of Zhapu, Zhenhai, Zhenjiang, Ningbo, Cixi and Wusong). For the Taiping Rebellion, we define directly affected regions as those occupied by the Taiping group or where the battles took place.

are reported in columns (7) and (8)), we observe significant negative effects for regions that were directly affected. Our interpretation is that, instead of rebelling on their own, some people who were inclined towards rebellion might have joined the Taiping group once the campaign reached their area, which is consistent with arguments made by historians (Martin, 1996). [34]

6 Discussion of Mechanisms

The previous sections provide plausibly causal evidence that associates the abandonment of the Grand Canal with subsequent rebellions in the areas through which the canal runs. While this paper stresses the loss of trade access as a major contributor, there are several other mechanisms that could explain these results. For example, the government's repressive capacity around the canal might diminish as the canal loses its political importance in grain tax transportation (Fearon and Laitin, 2003; Besley and Persson, 2010); the agricultural productivity might also be affected — either positively or negatively — if, for example, the functioning of the canal also matters for the irrigation system. In this section, we evaluate these alternative mechanisms and find that neither of them is consistently supported by the data. More importantly, we provide some suggestive evidence that the loss of trade access is likely a channel through which the canal's closure destabilizes society.

6.1 State Repressive Capacity

One possible channel through which the abandonment of the canal could lead to more rebellions is the weakening of the government's repressive capacity (or effort) as the canal loses its significance. This could encourage more rebellions as the chances of success increase. The ideal approach to evaluating this mechanism would be to examine directly whether the repressive capacity decreases along the canal following its closure. We are however unable to directly observe any measures of repressive capacity at the county level that vary across time. Therefore, we employ two relatively indirect approaches to disentangle the potential role of the repressive capacity.

The first approach explores variations in the importance of political control prior to the canal's closure. If the canal's closure weakens the government's repressive capacity a-long the canal, we would expect the effect to be stronger in counties that require more intensive controls. We employ two measures of such political importance. The first measure

[34] Because we cannot distinguish between those who personally joined the Taiping army and those who made an alliance with Taiping, the smaller effects we observe in the Taiping regions could also be interpreted as resulting from inaccurate reporting in those areas

considers the size of the pre-assigned military establishment in the 1750s — a direct proxy for the repressive capacity, which also reflects the government's perception of the county's military significance. [35] The second measure is the presence of prefectural administrative centers — i.e., administrations that operate higher on the political hierarchy. We interact our post-reform indicator with each of these measures to investigate whether the effects are particularly intensified in counties that were previously important to the government in terms of its repressive force during the canal-transportation era. The results are reported in the first two columns of Table 11. The importance of political control, measured either way, does not exhibit a higher rate of rebellion in the post-reform period. Furthermore, we find no evidence that the reform produces more rebellions in regions that were previously more politically important to the government (suggested by the triple interaction terms). The nonsignificance of these estimates suggests that repressive capacity may play a limited, if any, role in boosting the subsequent rebellions.

Our second approach is to examine how the canal's closure affects the actions of existing rebel groups (recall that we consider only the new outbursts in our baseline). Specifically, we examine whether the rebels also tend to attack or retreat into canal counties — which should be observed if the reform reduces the county's repressive capacity — following the canal's closure. The results are summarized in the next two columns of Table 11. To obtain the results reported in column (3), we tested the effects on the frequency of attacks, whereas for the results reported in column (4) we tested the effects on the frequency of retreats by a rebel groups into other counties after being defeated elsewhere. [36] Both coefficients are relatively small compared with the sample mean, and neither is significantly different from zero. These results suggest that, while the abandonment of the canal increases the local onset of rebellions, it does not seem to make the canal counties appear more vulnerable to the rebels. Putting these findings together, we conclude that the decline in the government's repressive capacity is unlikely a primary mechanism that explains our findings.

6.2 Agricultural Productivity

Another possible effect of the closure is that it disrupted agricultural productivity along the canal, especially in the northern portions where adjacent farmland dry up. Such an effect could contribute to the subsequent rebellions by lowering the opportunity costs for peasants in rural areas. We evaluate this channel through two lenses. First, we examine changes in grain prices in response to the reform: If agricultural productivity is adversely affected by the

[35] The size of military establishment was pre-assigned during the 1750s and there were no substantial changes afterwards (Luo, 1984)

[36] The definition of and coding method used with these two variables are presented in Appendix B.

canal's closure, we would expect to observe an increase in local grain prices. [37] For the first column of Table 12 we regress grain prices on the abandonment of the canal. The coefficient is indeed positive, but it is nonsignificant at conventional levels. This finding suggests that agricultural productivity along the canal does not seem to suffer from the reform.

We then investigate whether the impact of the canal's closure varies across regions where suitability to crop plantations varies. If agricultural productivity plays a major role in stimulating rebellions, we should expect the effects to be stronger in areas that are more suitable for agriculture. Therefore, we multiply our baseline interaction with the suitability index for wheat and wetland rice, the two main crops in our sample area (Talhelm et al., 2014). The results are reported in the next two columns of Table 12. While the main effects of the reform remain significant, we find no heterogeneous effects across levels of crop suitability. Therefore, the potential channel of agricultural productivity is not supported by the data.

6.3 Trade Access

After ruling out confounding explanations based on repressive capacity and agricultural productivity, we provide three pieces of suggestive evidence that the rebellions associated with the canal's closure most likely reflect the loss of trade access, which hit primarily urban areas. We start by examining the development of local markets along the canal. Specifically, we consider the number of market towns in 1820 and 1911, and regress this figure on the abandonment of the canal. The estimated results are reported in the first column of Table 13. The results suggest that the development of local markets was significantly hampered following the abandonment of the canal, which indicates the disruption of regional trade after the abandonment of the canal.

We next investigate whether the effects of the reform are stronger in more urbanized areas. To this end, we multiply our baseline interaction by the share of urban areas in 1776 — the most recent pre-reform year for which we have this information — and summarize the results in the second column of Table 13. In this specification, the baseline interaction $Canal \times Post$ represents the treatment effect in the absence of urbanization; the coefficient is negative and significant at the 10% level, suggesting a negative effect in a hypothetical "absolutely rural" county with zero rate of urbanization. The triple interaction, $Canal \times Post \times 1776\ Urbanshare$, estimates the extent to which the difference between canal and non-canal counties depends on the level of urbanization. The estimated coefficient is positive and significant at the 1% level, suggesting larger treatment effects in more densely urbanized places. This finding indicates that the canal's closure causes rebellions particularly in those

[37] The data for grain prices are compiled by Chen and Kung (2016).

more urbanized areas.

Finally, we evaluate whether access to alternative trade routes could help to mitigate the destabilizing effects from of the canal's closure. In particular, we consider an alternative north-south land transportation route in North China that runs along courier roads. Specifically, we interact the reform dummy with an indicator denoting whether a county has access to this alternative route of transportation. The results are presented in Column (3) of Table 13. In this specification, the baseline interaction, $Canal \times Post$ estimates the incremental number of rebellions in canal counties with no access to the alternative courier roads, in which we find a positive and significant effect similar to our baseline estimates in terms of scale. The triple interaction term, $Canal \times Post \times Courier$, represents the relative change in the treatment effects in counties with access to the alternative route. We find a mitigating effect in the number of rebellions that is significant at the 5% level if a canal county also has access to the alternative route. In particular, having access to an alternative trade route appears to offset at least half of the rebellions caused by the abandonment of the canal.[38] We interpret this result as supportive evidence for the channel of disrupted trade-route access. [39]

To sum up, we conclude that the pattern we observe in the data is most consistent with a story according to which the canal's closure disrupts neighboring markets' access to this long-established trade route, leading to more rebellions in the following decades. While none of these pieces of evidence is sufficiently conclusive on its own, collectively they present a pattern suggestive of the loss of trade access as a channel through which the canal's closure destabilizes society.

6.4 Further Implications

The evidence discussed above suggests that disrupted trade access, particularly in urban areas, might be responsible for the chronic social disorder following the canal's closure. One further implication of this channel is that the violence — whether or not it is associated with trade disruption in urban areas — might tend to be more organized and perhaps, as a result,

[38]We also verified the canal's role in mitigating risk, the role that trade usually plays in the course of climate shocks, as suggested by Burgess and Donaldson (2010). Consistent with their argument, we find that the canal helped reduce conflict during extreme weather, but this effect was absent after its abandonment. The results are available upon request.

[39]An alternative interpretation of our findings regarding urban areas is that they reflect some sort of political grievance (blaming the government for the abandonment) rather than a decline in opportunity costs. To account for this possibility, we identify a set of regions that experienced mass killings when the Qing military took over their territory in the 1640s. We would expect stronger effects at the site of a mass killing if the impact we have observed can be attributed to such grievances. The results, while not reported, are inconsistent with this argument. We do not find a significant difference between places with and without a history of mass killing.

persistent. In particular, the existence of worker associations in urban sectors likely fosters the development of gangs and secret societies once access to trade opportunities has been lost; such an organization could potentially serve as a hotbed for recurring and persistent turmoil over a long period of time. [40] Indeed, historians have described the potential association between the canal's closure and the emergence of organized gangs and violent events over several decades, including the emergence of the Green Gang, the rise of the Communist revolution, and the prevalence of armed conflicts during the Cultural Revolution. [41]

While our data do not allow us to causally identify whether the canal's closure leads to these violent events, we employ three cross-sectional exercises to show that canal regions remain most susceptible to such organized violence even decades after the closure. The first exercise exploits variations in the emergence of the Green Gang, one of China's largest and most powerful organized crime groups in the early twentieth century. We obtain a list of high-ranking Green Gang members and correlate their distribution with the location of the canal. The results, presented in the first column of Table 14, demonstrate a high concentration of these early gang members around the canal. The second evaluation considers the rise of communism in southern China during the 1920s. We compile the year in which a county establishes its first Communist Party branch for each county in Anhui, Jiangsu and Zhejiang, the provinces with the earliest communist activities in our sample. In column (2) of Table 14 we present the correlations between the canal and the early development of the Communist revolution. On average, the Communist revolution emerges earlier in counties that were more intensively affected by the canal. Finally, in column (3) of Table 14 we report the results of estimating the correlation between the canal's intensity and the presence of armed conflicts (with deaths > 0) in the first five years of Chinese Cultural Revolution. [42] These results show that the regions around the canal experienced much more violence than the other regions during the Cultural Revolution. [43] Taken together these results, while not necessarily causal, suggest a pattern that the pre-reform labor organizations in urban regions could have been transformed into gangs or secret societies that perpetrated organized violence when the canal was abandoned, and thereby produce a persistent effect into the twentieth century.

[40] For example, Dell et al. (2019) shows that urban trade shocks are associated with organized crimes in Mexico. Historical case studies also find that trade disruption played a crucial role in the origin of mafia-like organizations in Chicago and New York City (Haller, 1971; Critchley, 2008).

[41] See, for example,Martin (1996), Perry (1980) and Liu (2007)

[42] We thank Andrew Walder for generously sharing his data.

[43] Walder (2014) estimated that the political upheaval resulted in 1.1 to 1.6 million deaths and 22 to 30 million direct victims of political persecution.

7 Conclusion

In this paper we examine the link between the closure of China's Grand Canal and the subsequent social turmoil in nineteenth century North China. Using an original dataset covering 575 counties over 262 years, we present plausibly causal evidence that canal counties experience more frequent rebellions than their non-canal counterparts after the canal's closure. Furthermore, we find that these effects are driven primarily by counties that contain longer sections of the canal and that the impact spreads over a distance of approximately 150km. We explore several prominent mechanisms that could potentially explain our findings, and find the most support for the loss of trade access through which the canal's closure destabilizes society.

Our work emphasizes the importance of continued access to trade routes in promoting peace — a classical notion dating back to Montesquieu — that has rarely been subject to direct statistical examination in a causal context. While we focus on a historical context that allows for a plausible causal interpretation, the implication of the study is likely pertinent to contemporary policy-making, especially in an era of significant backlash against global trade integration. We also shed fresh light on the chronic social disorder in nineteenth-century North China — a pivotal episode in Chinese history that features a series of notable events marked by turmoil, including the Nian Rebellion, the Boxer Rebellion, and the Green Gang as well as an early base area for the Communist revolution. Our work highlights the loss of social economic opportunities as a leading force in promoting the persistent and recurring insurrections that plagued the end of Imperial China.

References

Chunrong Ai and Edward C. Norton. Interaction terms in logit and probit models. *Economics Letters*, 80(1):123 – 129, 2003.

Ying Bai and Ruixue Jia. Elite Recruitment and Political Stability: The Impact of the Abolition of Chinas Civil Service Exam. *Econometrica*, 2016.

Ying Bai and James Kai-sing Kung. Climate Shocks and Sino-nomadic Conflict. *Review of Economics and Statistics*, 93(3):970–981, 2011.

Samuel Bazzi and Christopher Blattman. Economic Shocks and Conflict: Evidence from Commodity Prices. *American Economic Journal: Macroeconomics*, 6(4):1–38, 2014.

Nicolas Berman and Mathieu Couttenier. External shocks, internal shots: The geography of civil conflicts. *The Review of Economics and Statistics*, 97(4):758–776, 2015. doi: 10.1162/REST_a_00521. URL https://doi.org/10.1162/REST_a_00521.

Timothy Besley and Torsten Persson. State Capacity, Conflict, and Development. *Econometrica*, 78(1):1–34, 2010.

Christopher Blattman and Edward Miguel. Civil War. *Journal of Economic Literature*, 48 (1):3–57, 2010.

Robin Burgess and Dave Donaldson. Can Openness Mitigate the Effects of Weather Shocks? Evidence from India's Famine Era. *The American Economic Review*, pages 449–453, 2010.

Shuji Cao. *The Population History of China*, volume 5. Fudan University Press, Shanghai, 2001.

Qiang Chen. Climate Shocks, State Capacity and Peasant Uprisings in North China during 25C1911 CE. *Economica*, 82(326):295–318, 2015.

Shuo Chen and James Kai-sing Kung. Of Maize and Men: the Effect of a New World Crop on Population and Economic Growth in China. *Journal of Economic Growth*, 21(1):71–99, 2016.

Sui-wai Cheung. Construction of the Grand Canal and Improvement in Transportation in Late Imperial China. *Asian Social Science*, 4(6):p11, 2009.

C. Chi. *Key Economic Areas in Chinese History as Revealed in the Development of Public Works for Water-control*. A. M. Kelley, 1936.

T. G. Conley. GMM Estimation with Cross Sectional Dependence. *Journal of Econometrics*, 92(1):1–45, 1999.

Timothy G. Conley. *Spatial Econometrics*. Palgrave Macmillan, Basingstoke, 2008.

D. Critchley. *The Origin of Organized Crime in America: The New York City Mafia, 1891–1931*. Routledge Advances in American History. Taylor & Francis, 2008.

Melissa Dell, Benjamin Feigenberg, and Kensuke Teshima. The violent consequences of trade-induced worker displacement in mexico. *American Economic Review: Insights*, 1(1): 43–58, 2019.

Simeon Djankov and Marta Reynal Querol. Poverty and Civil War: Revisiting the Evidence. *Review of Economics and Statistics*, 92(4):1035–1041, 2010.

Dave Donaldson. Railroads of the Raj: Estimating the Impact of Transportation Infrastructure. *The American Economic Review*, 2018.

Dave Donaldson and Richard Hornbeck. Railroads and American Economic Growth: a "Market Access" Approach. *Quarterly Journal of Economics*, forthcoming.

Oeindrila Dube and Juan F Vargas. Commodity Price Shocks and Civil Conflict: Evidence from Colombia. *The Review of Economic Studies*, 80(4):1384–1421, 2013.

M. Elvin. *The Pattern of the Chinese Past: A Social and Economic Interpretation*. Stanford University Press, 1973.

Larry G. Epstein. A definition of uncertainty aversion. *The Review of Economic Studies*, 66 (3):579–608, 1999.

Joseph Esherick. *The Origins of the Boxer Uprising*. Univ of California Press, 1988.

James D Fearon and David D Laitin. Ethnicity, Insurgency, and Civil War. *American Political Science Review*, 97(01):75–90, 2003.

Robert William Fogel. Notes on the Social Saving Controversy. *The Journal of Economic History*, 39(01):1–54, 1979.

D. Gandar. *Le Canal Imperial: Etude Historique et Descriptive*. Kraus Reprint, 1894.

Jianxiong Ge. *Changes in Boundaries and Administrative Divisions in Chinese History*. The Commercial Press, 1997. ISBN 9787100024259.

Pauline Grosjean. A History of Violence: The Culture of Honor and Homicide in the US South. *Journal of the European Economic Association*, 12(5):1285–1316, 2014. ISSN 1542-4774. doi: 10.1111/jeea.12096.

Herschell I Grossman. A General Equilibrium Model of Insurrections. *The American Economic Review*, pages 912–921, 1991.

Mark H. Haller. Organized Crime in Urban Society: Chicago in the Twentieth Century. *Journal of Social History*, 5(2):210–234, 1971.

Hϕvard Hegre and Nicholas Sambanis. Sensitivity Analysis of Empirical Results on Civil War Onset. *Journal of Conflict Resolution*, 50(4):508–535, 2006.

Harold C. Hinton. The Grain Tribute System of the Ch'ing Dynasty. *The Far Eastern Quarterly*, 11(3):339–354, 1952.

Jack Hirshleifer. Conflict and rent-seeking success functions: Ratio vs. difference models of relative success. *Public Choice*, 63(2):101–112, 1989. ISSN 00485829, 15737101. URL http://www.jstor.org/stable/30025171.

Solomon M Hsiang, Kyle C Meng, and Mark A Cane. Civil Conflicts are Associated With the Global Climate. *Nature*, 476(7361):438–441, 2011.

Solomon M. Hsiang, Marshall Burke, and Edward Miguel. Quantifying the Influence of Climate on Human Conflict. *Science*, 341(6151), 2013.

H. Huang. *The Land Tax in China*. Columbia university, 1918.

A.W. Hummel. *Eminent Chinese of the Ch'ing Period: 1644-1912*. SMC publ., 1991.

Murat Iyigun, Nathan Nunn, and Nancy Qian. The Long-run Effects of Agricultural Productivity on Conflict, 1400-1900. 2015.

Thorsten Janus and Daniel Riera-Crichton. Economic Shocks, Civil War and Ethnicity. *Journal of Development Economics*, 115:32–44, 2015.

Saumitra Jha. Trade, Institutions, and Ethnic Tolerance: Evidence from South Asia. *American Political Science Review*, 107(4):806C832, 2013. doi: 10.1017/S0003055413000464.

Ruixue Jia. Weather Shocks, Sweet Potatoes and Peasant Revolts in Historical China. *The Economic Journal*, 124(575):92–118, 2014.

James Kai-sing Kung and Chicheng Ma. Can Cultural Norms Reduce Conflicts? Confucianism and Peasant Rebellions in Qing China. *Journal of Development Economics*, 111(0): 132–149, 2014.

David D Laitin. *Nations, States, and Violence*. OUP Oxford, 2007.

Jane Kate Leonard. 'Controlling from Afar' Open Communications and the Tao-Kuang Emperor's Control of Grand Canal-Grain Transport Management, 1824-26. *Modern Asian Studies*, 22(4):665–699, 1988.

Jane Kate Leonard. *Stretching the Qing Bureaucracy in the 1826 Sea Transport Experiment*. Brill, Leiden, The Netherlands, 2018. ISBN 978-90-04-38458-3.

C. Liu. *Peasants and Revolution in Rural China: Rural Political Change in the North China Plain and the Yangzi Delta, 1850-1949*. Routledge studies in the Chinese economy. Routledge, 2007. ISBN 9780415421768. URL https://books.google.com/books?id=YOh8cbTo3SOC.

Ergang Luo. *Lu ying bing zhi (Book of the Green Standard Army)*. Zhonghua Shuju, 1984.

Michael E Mann, Zhihua Zhang, Scott Rutherford, Raymond S Bradley, Malcolm K Hughes, Drew Shindell, Caspar Ammann, Greg Faluvegi, and Fenbiao Ni. Global Signatures and Dynamical Origins of the Little Ice Age and Medieval Climate Anomaly. *Science*, 326 (5957):1256–1260, 2009.

B.G. Martin. *The Shanghai Green Gang: Politics and Organized Crime, 1919-1937*. University of California Press, 1996.

Eoin McGuirk and Marshall Burke. The economic origins of conflict in africa. *Journal of Political Economy*, 2020. doi: 10.1086/709993. URL https://doi.org/10.1086/709993.

Edward Miguel. Poverty and Witch Killing. *The Review of Economic Studies*, 72(4):1153–1172, 2005.

Edward Miguel, Shanker Satyanath, and Ernest Sergenti. Economic Shocks and Civil Conflict: An Instrumental Variables Approach. *Journal of Political Economy*, 112(4):725–753, 2004.

H.B. Morse. *The Trade and Administration of China*. Longmans, Green and Company, 1913.

Yuping Ni. *Grain Transportation in the Qing Dynasty and Social Change (Chinese Edition)*. Shanghai Bookstore Publishing House, 2005.

Nathan Nunn and Diego Puga. Ruggedness: The Blessing of Bad Geography in Africa. *Review of Economics and Statistics*, 94(1):20–36, 2012.

Elisabeth Ruth Perlman and Steven Sprick Schuster. Delivering the Vote: The Political Effect of Free Mail Delivery in Early Twentieth Century America. *Journal of Economic History*, 2016.

Elizabeth Perry. *Rebels and Revolutionaries in North China, 1845-1945*. Stanford University Press, 1980. ISBN 9780804766524.

A KUHN Philip. *Soulstealers: the Chinese sorcery scare of 1768*. Harvard University Press, 2009.

Patrick A. Puhani. The treatment effect, the cross difference, and the interaction term in nonlinear difference-in-differences models. *Economics Letters*, 115(1):85 – 87, 2012. ISSN 0165-1765.

Shawn J. Riley, Stephen D. DeGloria, and Robert Elliot. A Terrain Ruggedness Index that Quantifies Topographic Heterogeneity. *Intermountain Journal of Sciences*, 5(1 - 4):23 –27, 1999.

Carol H. Shiue. Transport Costs and the Geography of Arbitrage in Eighteenth-Century China. *American Economic Review*, 92(5):1406–1419, December 2002. doi: 10.1257/000282802762024566.

Donald Sturgeon. Chinese Text Project: a dynamic digital library of premodern Chinese. *Digital Scholarship in the Humanities*, 08 2019. ISSN 2055-7671. doi: 10.1093/llc/fqz046. URL https://doi.org/10.1093/llc/fqz046. fqz046.

US Geological Survey. GTOPO30, 1996.

T Talhelm, X Zhang, S Oishi, C Shimin, D Duan, X Lan, and S Kitayama. Large-scale Psychological Differences within China Explained by Rice versus Wheat Agriculture. *Science*, 344(6184):603–608, 2014.

Nico Voigtländer and Hans-Joachim Voth. Persecution Perpetuated: The Medieval Origins of Anti-Semitic Violence in Nazi Germany. *The Quarterly Journal of Economics*, 127(3): 1339–1392, 2012. doi: 10.1093/qje/qjs019.

F. Wakeman. *The Great Enterprise: The Manchu Reconstruction of Imperial Order in Seventeenth-century China*. California University, 1985. ISBN 9780520048041.

Andrew G. Walder. Rebellion and repression in china, 1966c1971. *Social Science History*, 38(3-4):513C539, 2014. doi: 10.1017/ssh.2015.23.

A.J. Watson. *Transport in Transition: the Evolution of Traditional Shipping in China*. Michigan abstracts of Chinese and Japanese works on Chinese history. Center for Chinese Studies, University of Michigan, 1972. ISBN 9780892649037.

Baojiong Zhu and Peilin Xie, editors. *Ming-Qing Jinshi Timing Beilu Suoyin (Official Directory of Ming-Qing Imperial Exam Graduates)*. Shanghai: Shanghai Guji Chubanshe, 1980.

Figures

Figure 1: Location of the Grand Canal

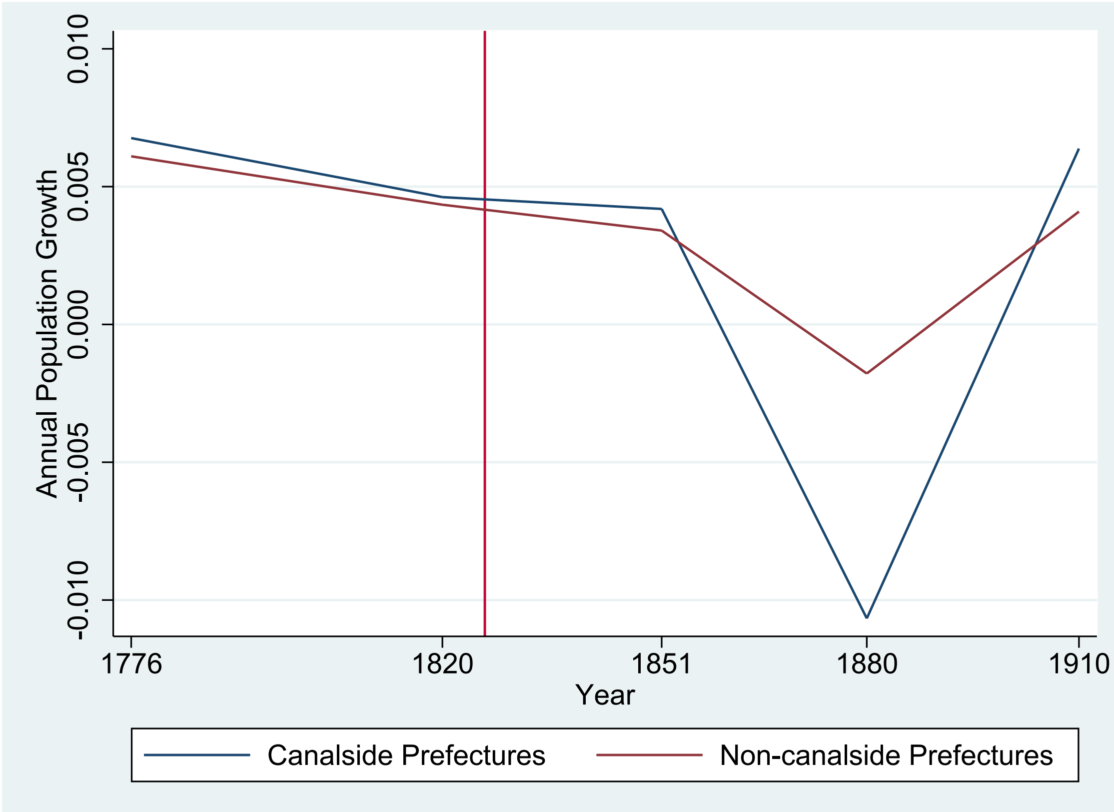


Figure 2: Annual Population Growth at the Prefecture Level

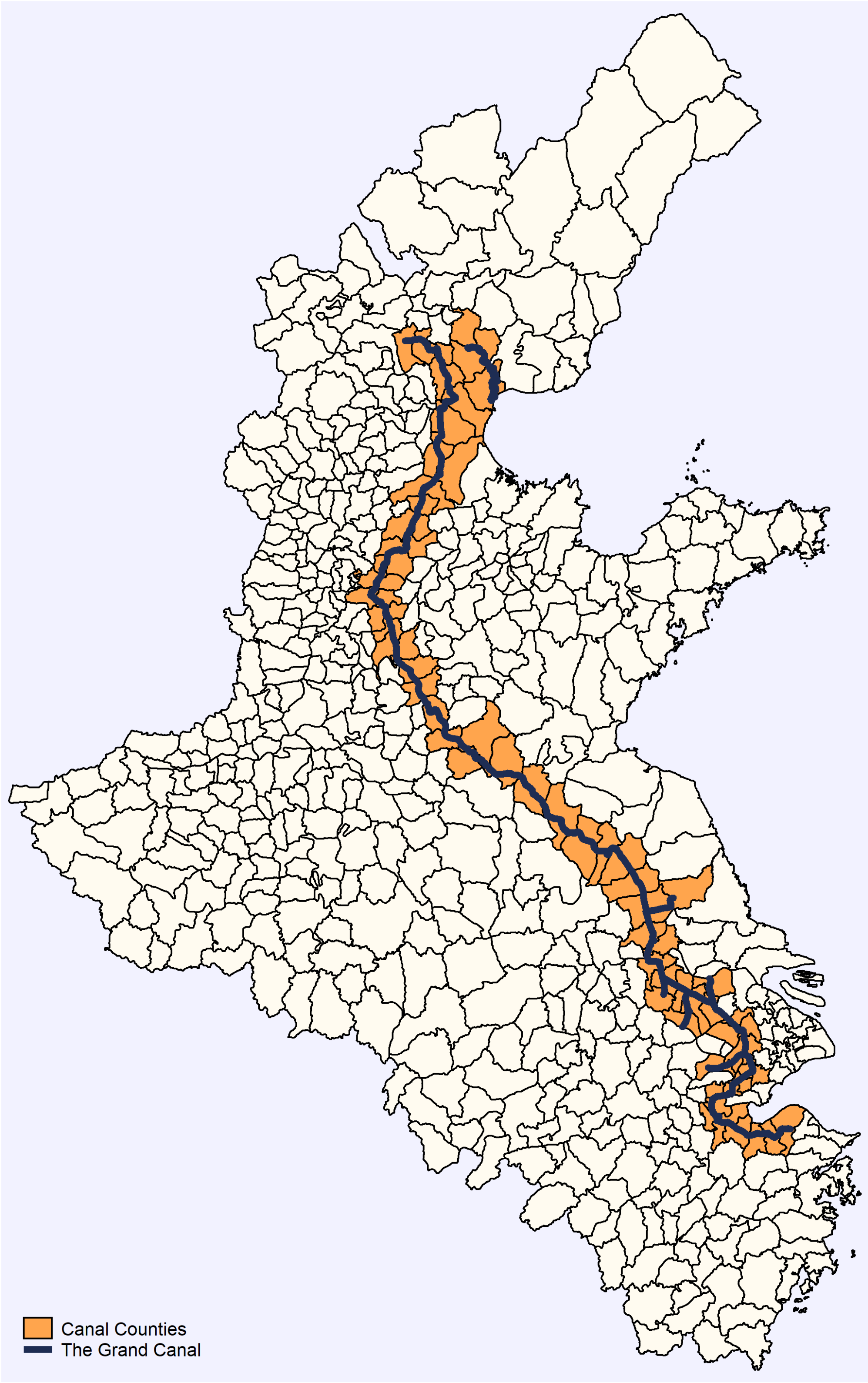


Figure 3: Counties Contained in the Sample

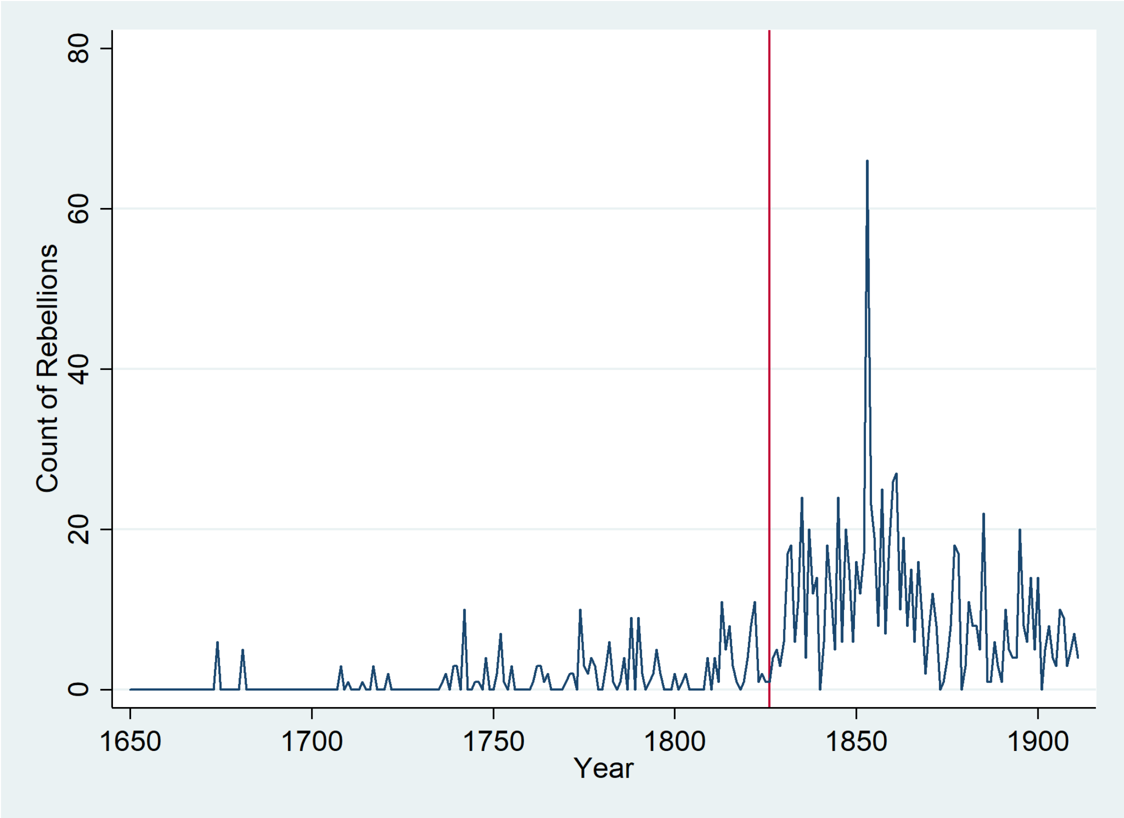


Figure 4: The Dynamics of Rebellions over Time

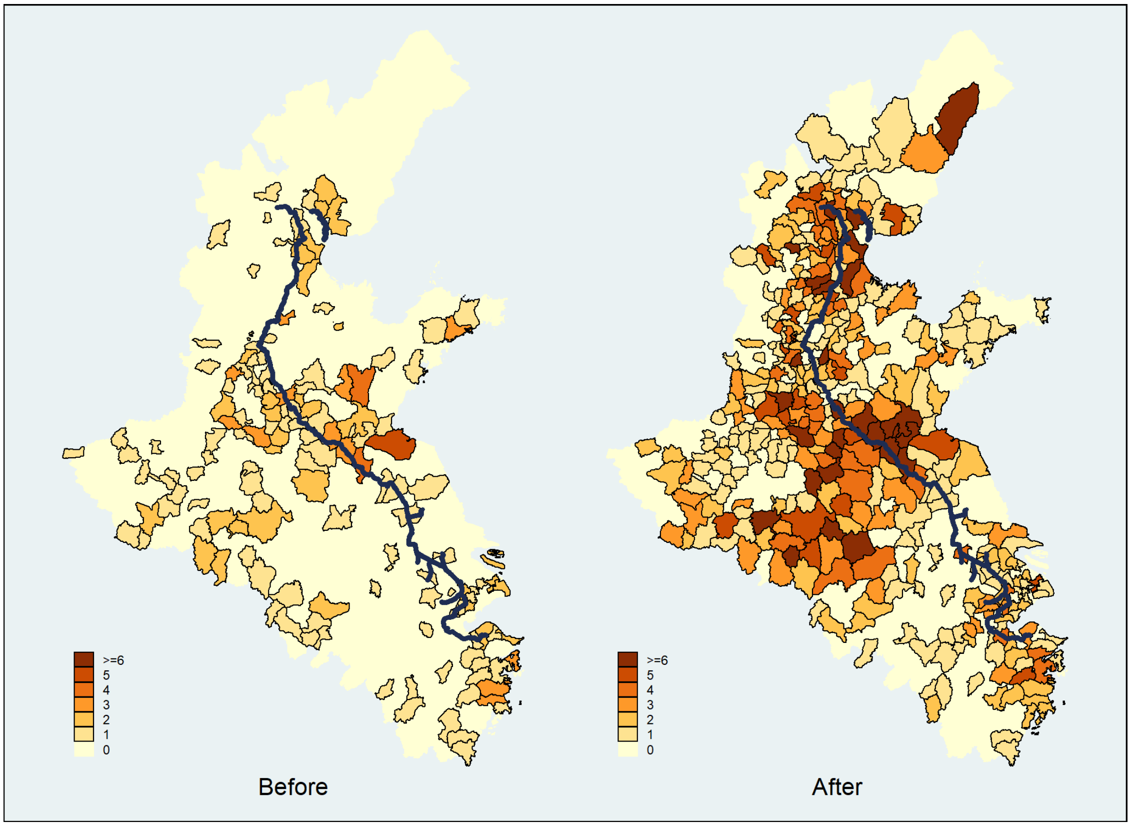


Figure 5: The Spatial Distribution of Rebellions before and after the Abandonment

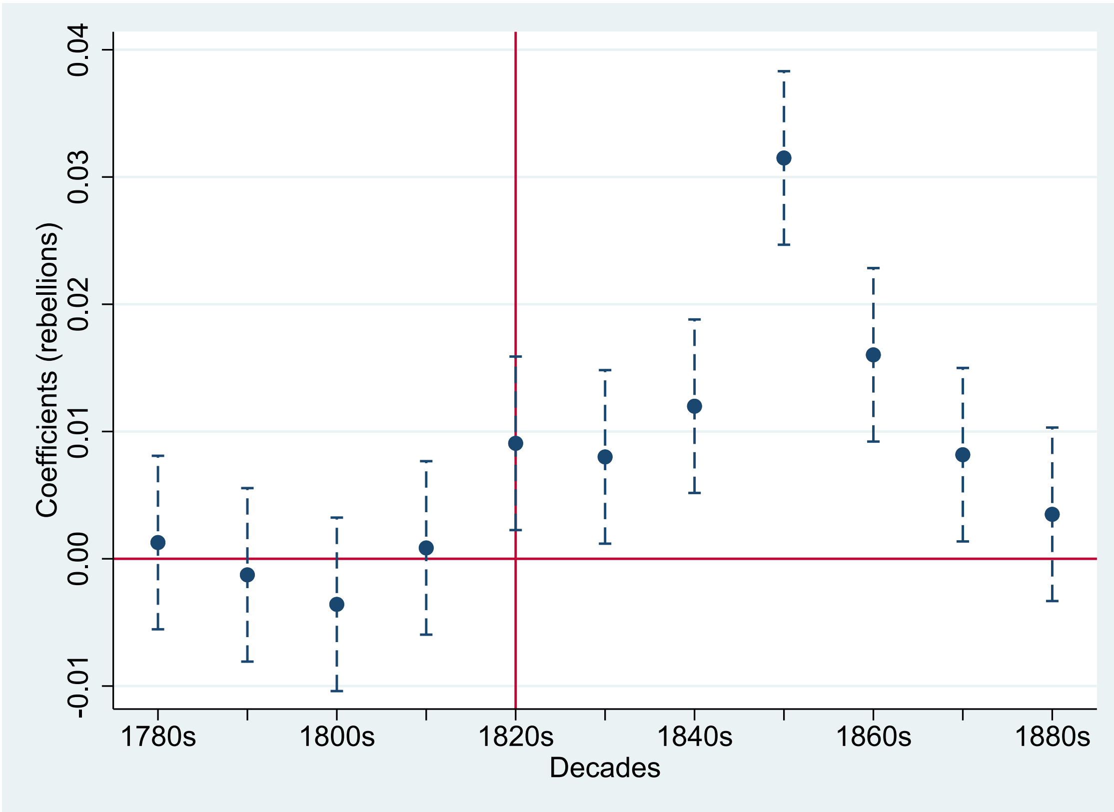


Figure 6: Flexible Estimation of the Treatment Effects by Decades

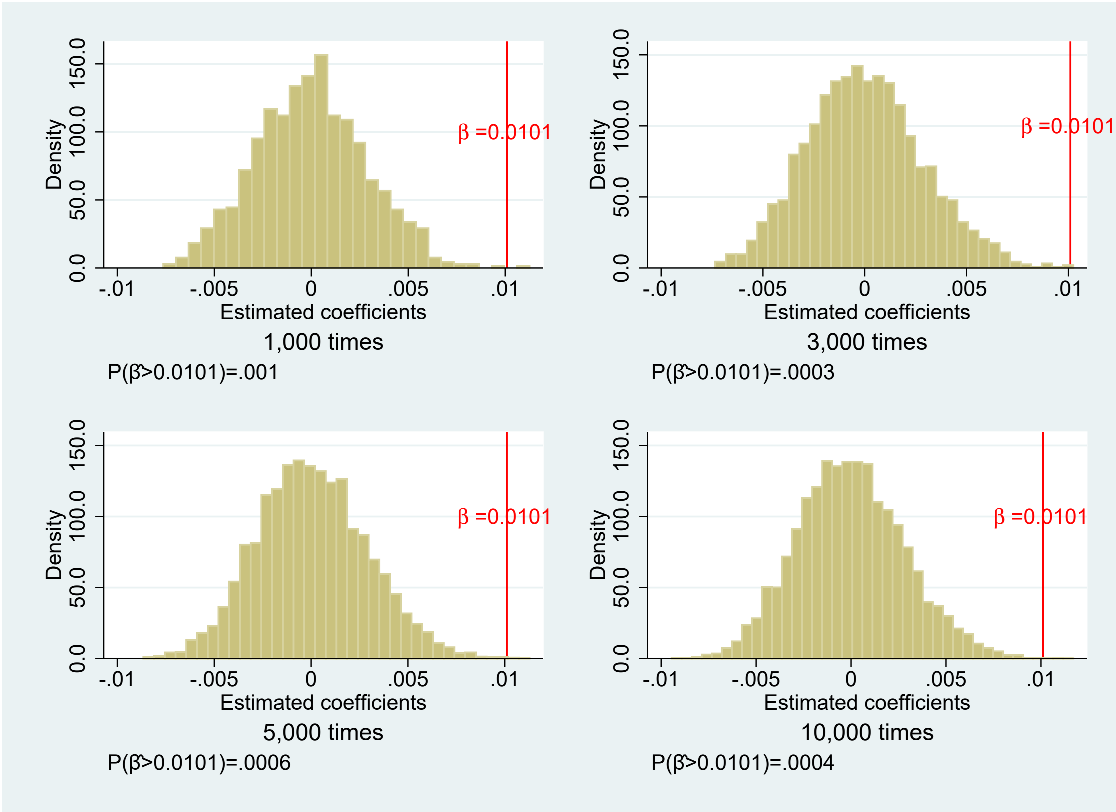


Figure 7: The Distribution of t-statistics from Randomly Assigned Placebo Treatment Effects

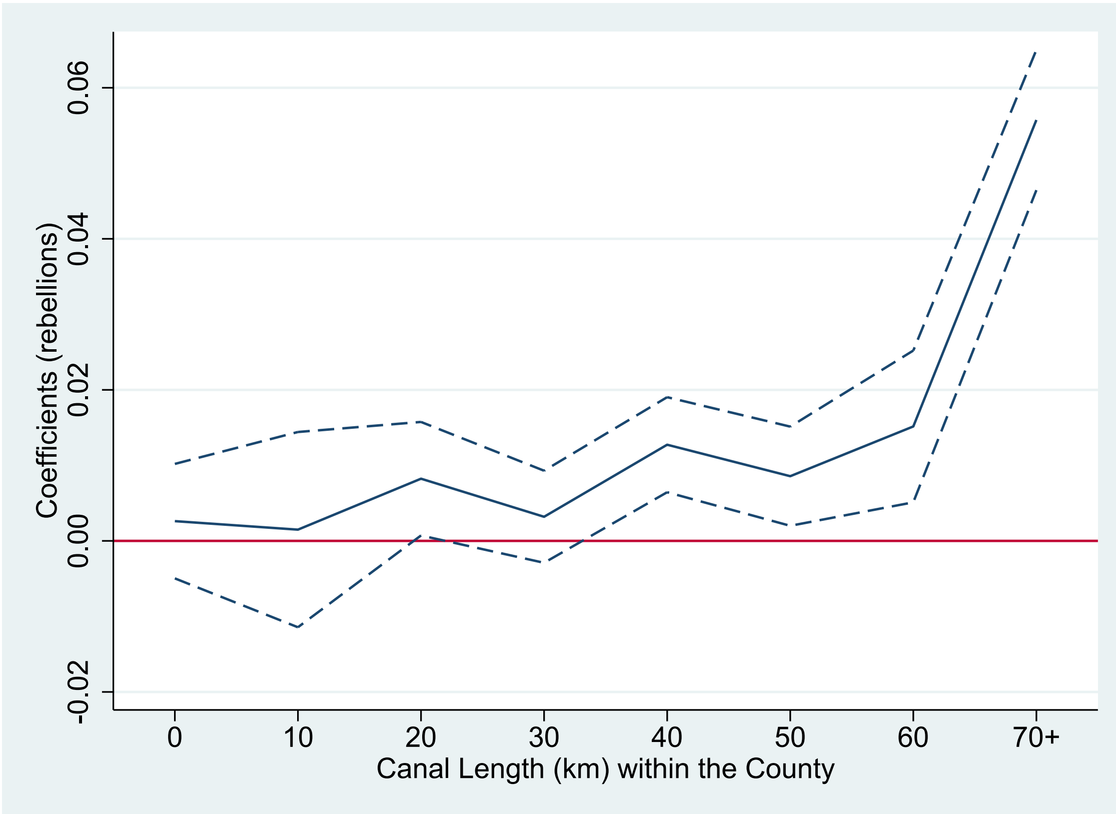


Figure 8: More Flexible Estimation of the Treatment Effects by Canal Length

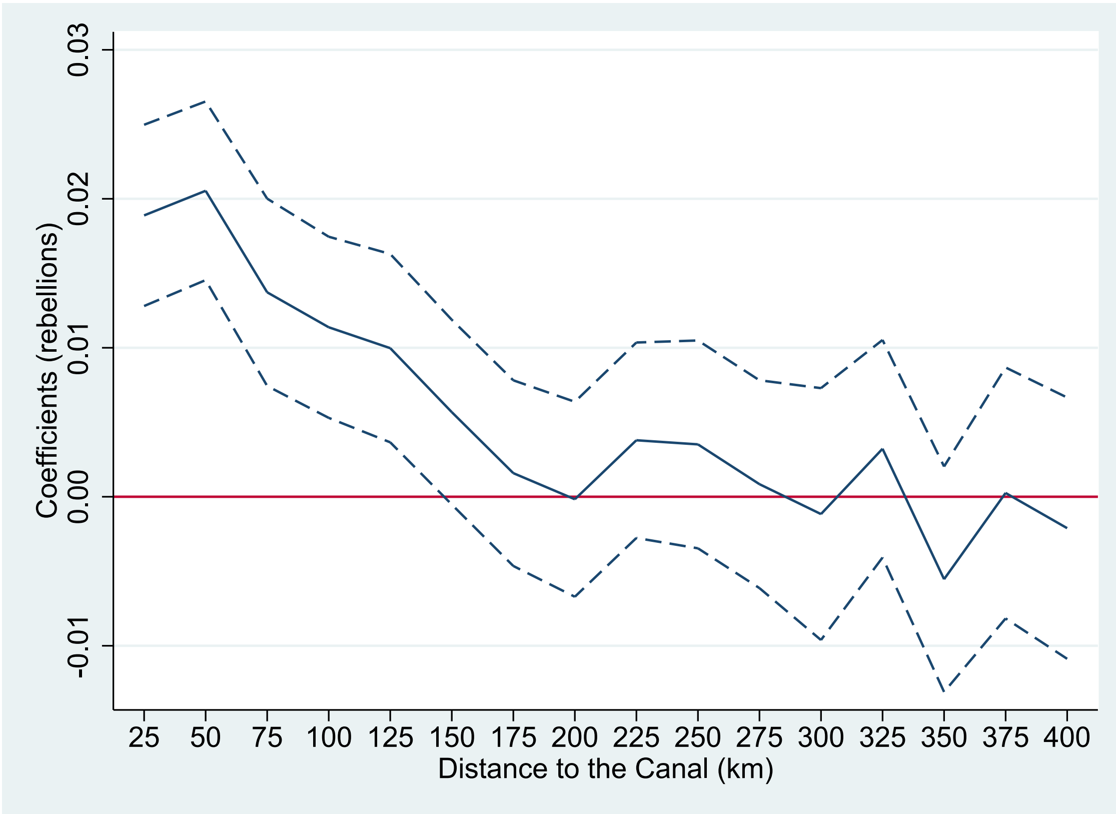


Figure 9: More Flexible Estimation of the Treatment Effects by Distance to the Canal

Tables

Table 1: Descriptive Statistics

	Source	Obs.	Mean	S.D
Outcomes				
Presence of Rebellions (Onset)	1	150,650	0.0072	0.0843
Number of Rebellions (Onset)	1	150,650	0.0074	0.0881
Treatments				
Being Along the Grand Canal	2	575	0.1270	0.3332
Length of Canal within Boundary (km)	2	575	4.1196	13.3217
Distance from the Grand Canal (km)	2	575	118.0353	113.4459
Controls				
Temperature Deviated from 1961-2006 Mean	3	143,838	-0.1954	0.3343
Drought	4	150,650	0.0976	0.2968
Flood	4	150,650	0.0743	0.2623
Ruggedness Index	5	575	72.7510	97.6787
Distance from the Yellow River (km)	2	575	296.8347	267.3429
Distance from the coast (km)	2	575	199.5409	169.9313
Year of Maize Adoption	4	563	1,718.4050	95.6149
Year of Sweet Potato Adoption	4	231	1,755.0130	51.2365
jinshi	6	150,650	0.1563	0.8558
Supplements				
Imperial Soldiers Stationed	7	575	154.2104	345.8505
Prefecture Capital	2	575	0.1391	0.3464
Number of Attacking Cases	1	150,650	0.0054	0.0822
Number of Retreating Cases	1	150,650	0.0036	0.0690
Average Grain Price (Liang/KCal)	4	91,110	0.5980	0.1856
Suitability Index for Wheat (Irrigation, M Input)	8	575	4,793.0057	1,836.9174
Suitability Index for Wetland Rice (Irrigation, M Input)	8	575	3,758.3572	1,231.6994
Number of Towns and Local Markets	2	1,150	12.4957	10.7650
1776 urbanshare	4	563	8.6055	3.7947
Along the Qing Courier Routes	2	575	0.2800	0.4494
Mass Kill during the Qing Invasion	9	575	0.0226	0.1488
Senior Green Gang Members	10	575	0.2435	1.0592
Year of First Communist Party Branch	11	569	1,928.2583	5.2102
Armed conflict with non-zero death toll, 1966 - 1971	12	569	0.6432	0.4795

Data Sources:

1. Veritable Records of the Qing Emperors (*Qing Shilu*)
2. Harvard Yenching Institution (2007), CHGIS, Version 4.
3. Mann et al. (2009)
4. Chen and Kung (2016)
5. Nunn and Puga (2012)
6. Zhu and Xie (1980)
7. Luo (1984)
8. FAO (2012), GAEZ: http://gaez.fao.org/Main.html#
9. Wakeman (1985)
10. Encyclopedia of the Green Gang (*Qingbang Tongcao Huihai*)
11. Local gazettes
12. Walder (2014)

Table 2: Comparing the Number of Rebellions between the Treatment and the Controls

	1825 and before	1826 and after	Difference
Canal	0.004	0.029	0.025***
	(0.001)	(0.002)	(0.001)
Non-canal	0.002	0.017	0.015***
	(0.001)	(0.001)	(0.001)
Difference	0.002**	0.012***	0.010***
	(0.001)	(0.001)	(0.001)

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 3: Time Trends for the Number of Rebellions Before the Reform

	Dependent Variable: Number of Rebellions			
	(1)	(2)	(3)	(4)
Along Canal × Year	0.0001	0.0001	0.0001	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Constant	-0.0188	-0.0099	-0.0295	-0.0195
	(0.0345)	(0.0357)	(0.0401)	(0.0401)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Province × Year FE	No	Yes	No	Yes
Prefecture Year Trend	No	No	Yes	Yes
Mean of the Dependent Variable	0.0043	0.0043	0.0043	0.0043
No. of Observations	28,750	28,750	28,750	28,750
No. of Counties	575	575	575	575
No. of Clusters	575	575	575	575
Adjusted R-squared	0.0152	0.0258	0.0198	0.0301

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors, in parentheses, are clustered at the county level. The length and distance measures are rescaled to 10 km.

Table 4: The Effects of the Canal's Abandonment on the Number of Rebellions

	Dependent Variable: Number of Rebellions			
	(1)	(2)	(3)	(4)
Along Canal × After Abandonment	0.0101***	0.0110***	0.0087**	0.0089**
	(0.0037)	(0.0038)	(0.0037)	(0.0037)
	[0.0026]	[0.0022]	[0.0027]	[0.0023]
Constant	0.0070***	0.0069***	0.0070***	0.0070***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Province × Year FE	No	Yes	No	Yes
Prefecture Year Trend	No	No	Yes	Yes
Mean of the Dependent Variable	0.0074	0.0074	0.0074	0.0074
No. of Observations	150,650	150,650	150,650	150,650
No. of Counties	575	575	575	575
No. of Clusters	575	575	575	575
Adjusted R-squared	0.0251	0.0405	0.0279	0.0432

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors, in parentheses, are clustered at the county level. Standard errors in square brackets are Conley standard errors robust for spatial correlation.

Table 5: Continuous Treatment Effects of the Abandonment of the Canal on the Number of Rebellions

Measures of Treatment Intensity:	*Dependent variable:* Number of Rebellions			
	(1)	(2)	(3)	(4)
Canal Length × After Abandonment	0.0034***	0.0030**		
	(0.0012)	(0.0013)		
Distance to Canal × After Abandonment			-0.0004***	-0.0006***
			(0.0001)	(0.0001)
Constant	0.0069***	0.0070***	0.0090***	0.0101***
	(0.0002)	(0.0002)	(0.0003)	(0.0005)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Province × Emperor FE	No	Yes	No	Yes
Prefecture Year Trend	No	Yes	No	Yes
Mean of the Dependent Variable	0.0074	0.0074	0.0071	0.0071
No. of Observations	150650	150650	139384	139384
No. of Counties	575	575	532	532
No. of Clusters	575	575	532	532
Adjusted R-squared	0.0254	0.0433	0.0252	0.0431

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors, in parentheses, are clustered at the county level. The length and distance measures are rescaled to 10 km. Number of years since the reform is measured in decades.

Table 6: Treatment Effects of the Abandonment of the Canal on the Number of Rebellions by Section

	Dependent Variable: Number of Rebellions			
	(1)	(2)	(3)	(4)
Along North Canal × Post Reform	0.0161***	0.0162***	0.0114**	0.0113**
	(0.0056)	(0.0057)	(0.0057)	(0.0057)
Along South Canal × Post Reform	0.0017	0.0027	0.0038	0.0048
	(0.0031)	(0.0033)	(0.0031)	(0.0032)
North × Post Reform	0.0051***	0.0059*	0.0052**	0.0035
	(0.0018)	(0.0034)	(0.0021)	(0.0035)
Constant	0.0061***	0.0059***	0.0061***	0.0064***
	(0.0004)	(0.0006)	(0.0004)	(0.0006)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Province × Year FE	No	Yes	No	Yes
Prefecture Year Trend	No	No	Yes	Yes
Mean of the Dependent Variable	0.0074	0.0074	0.0074	0.0074
No. of Observations	150,650	150,650	150,650	150,650
No. of Counties	575	575	575	575
No. of Clusters	575	575	575	575
Adjusted R-squared	0.0256	0.0407	0.0281	0.0432

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors, in parentheses, are clustered at the county level.

Table 7: The Effects of the Canal's Abandonment using Alternative Sampling Methods

	Dependent Variable: Rebellions					
	County Sample within:					Prefecture
	Prefecture	100km	150km	200km	All	Sample
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: 50-year window (1800 – 1850)					
Along Canal × After Abandonment	0.006	0.006	0.009*	0.010*	0.010**	0.106*
	(0.00556)	(0.00518)	(0.00511)	(0.00506)	(0.00500)	(0.0573)
Observations	9500	15650	19800	22450	28750	3950
	Panel B: 100-year window (1775 – 1875)					
Along Canal × After Abandonment	0.009*	0.010*	0.013**	0.014***	0.015***	0.150**
	(0.00504)	(0.00505)	(0.00498)	(0.00492)	(0.00484)	(0.0634)
Observations	19000	31300	39600	44900	57500	7900
	Panel C: 150-year window (1750 – 1900)					
Along Canal × After Abandonment	0.006	0.007$^+$	0.010**	0.011**	0.011***	0.123**
	(0.00446)	(0.00443)	(0.00436)	(0.00431)	(0.00424)	(0.0568)
Observations	28500	46950	59400	67350	86250	11850
	Panel D: 200-year window (1711 – 1911)					
Along Canal × After Abandonment	0.006$^+$	0.006$^+$	0.009**	0.010**	0.010***	0.115**
	(0.00395)	(0.00390)	(0.00384)	(0.00379)	(0.00372)	(0.0525)
Observations	38000	62600	79200	89800	115000	15800
	Panel E: all years (1650 – 1911)					
Along Canal × After Abandonment	0.007*	0.007*	0.009**	0.010***	0.011***	0.114**
	(0.00401)	(0.00398)	(0.00391)	(0.00386)	(0.00379)	(0.0533)
Observations	49780	82006	103752	117638	150650	20698
County FE	Yes	Yes	Yes	Yes	Yes	
Prefecture FE						Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: $^+$, *, **, and *** denote significance at the 15% 10%, 5%, and 1% levels, respectively. Standard errors, in parentheses, are clustered at the county level. The length and distance measures are rescaled to 10 km. Number of years since the reform is measured in decades.

Table 8: The Effects of the Canal's Abandonment Accounting for Population

	Dependent Variable: Rebellions per million population					
	Prefecture Level		County Level (Imputed)			
			Based on Area		Lower Bound	
	(1)	(2)	(3)	(4)	(5)	(6)
Along Canal × After Abandonment	0.0240*	0.0283*	0.0371**	0.0413**	0.0254*	0.0286*
	(0.0140)	(0.0159)	(0.0160)	(0.0162)	(0.0148)	(0.0150)
Constant	0.0236***	0.0232***	0.0267***	0.0265***	0.0271***	0.0269***
	(0.0013)	(0.0014)	(0.0007)	(0.0007)	(0.0006)	(0.0006)
Prefecture FE	Yes	Yes	No	No	No	No
County FE	No	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × Year FE	No	Yes	No	Yes	No	Yes
Mean of the Dependent Variable	0.0258	0.0258	0.0283	0.0283	0.0282	0.0282
No. of Observations	19,785	19,785	146,744	146,744	146,744	146,744
No. of Prefectures	76	76				
No. of Counties			563	563	563	563
No. of Clusters	76	76	563	563	563	563
Adjusted R-squared	0.0497	0.0860	0.0165	0.0296	0.0169	0.0301

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors, in parentheses, are clustered at the county level.

Table 9: The Effects of the Canal's Abandonment including Multiple Controls

	Dependent Variable: Number of Rebellions									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Canal × Post	0.0100***	0.0101***	0.0101***	0.0076**	0.0109***	0.0099***	0.0101***	0.0101***	0.0101***	0.0078**
	(0.0037)	(0.0037)	(0.0037)	(0.0038)	(0.0036)	(0.0037)	(0.0037)	(0.0037)	(0.0037)	(0.0038)
Climate:										
Temperature Deviation	0.0031									0.0015
	(0.0024)									(0.0024)
Drought		0.0017*								0.0020**
		(0.0009)								(0.0009)
Flooding			0.0018*							0.0019*
			(0.0010)							(0.0010)
Geography:										
Ruggedness × After				-0.0000***						-0.0000**
				(0.0000)						(0.0000)
Distance to Yellow River × After					-0.0000***					-0.0000***
					(0.0000)					(0.0000)
Distance to the Coast × After						-0.0000				-0.0000**
						(0.0000)				(0.0000)
Technology:										
Maize Adopted							0.0004			0.0018*
							(0.0010)			(0.0010)
Sweet Potato Adopted								0.0006		0.0004
								(0.0012)		(0.0011)
Culture:										
jinshi									0.0002	0.0003
									(0.0002)	(0.0003)
Constant	0.0077***	0.0068***	0.0068***	0.0080***	0.0083***	0.0071***	0.0067***	0.0068***	0.0069***	0.0086***
	(0.0005)	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0007)	(0.0003)	(0.0002)	(0.0010)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of the Dependent Variable	0.0073	0.0072	0.0072	0.0072	0.0072	0.0072	0.0072	0.0072	0.0072	0.0075
No. of Observations	143,838	150,650	150,650	150,650	150,650	150,650	147,506	147,506	150,650	141,218
No. of Counties	549	575	575	575	575	575	563	563	575	539
No. of Clusters	549	575	575	575	575	575	563	563	575	539
Adjusted R-squared	0.0259	0.0251	0.0251	0.0255	0.0255	0.0251	0.0255	0.0255	0.0251	0.0269

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors, in parentheses, are clustered at the county level.

Table 10: The Interaction between the Canal's Abandonment and Other Major Wars

	Dependent Variable: Numer of Rebellions							
	Panel A: Excluding Suffering Counties				Panel B: All Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Along Canal × After Abolishment	0.0106***	0.0093**	0.0239***	0.0196**	0.0106***	0.0092**	0.0239***	0.0197**
	(0.0037)	(0.0038)	(0.0083)	(0.0083)	(0.0037)	(0.0038)	(0.0083)	(0.0082)
Opium Battlefield × After					-0.0045	0.0026		
					(0.0106)	(0.0115)		
Canal × Opium Battlefield × After					-0.0150	-0.0159		
					(0.0120)	(0.0127)		
Taiping × After							-0.0051***	-0.0015
							(0.0018)	(0.0036)
Canal × Taiping × After							-0.0178**	-0.0166*
							(0.0089)	(0.0088)
Constant	0.0069***	0.0070***	0.0077***	0.0078***	0.0070***	0.0070***	0.0076***	0.0072***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0005)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Province × Year FE	No	Yes	No	Yes	No	Yes	No	Yes
Prefecture Year Trend	No	Yes	No	Yes	No	Yes	No	Yes
Mean of the Dependent Variable	0.0074	0.0074	0.0082	0.0082	0.0074	0.0074	0.0074	0.0074
No. of Observations	149,078	149,078	88,294	88,294	150,650	150,650	150,650	150,650
No. of Counties	569	569	337	337	575	575	575	575
Adjusted R-squared	0.0252	0.0429	0.0320	0.0629	0.0252	0.0432	0.0257	0.0433

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors, in parentheses, are clustered at the county level.

Table 11: Testing Shocks to State Repressive Capacity

	Dependent Variable:			
	Baseline Outcome		Placebo Outcome	
	Rebellions (Onset Case)		Attack Case	Retreat Case
	(1)	(2)	(3)	(4)
Canal $\times$ Post	0.0003**	0.0003***	0.0000	0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0000)
Soldiers $\times$ Post	-0.0003			
	(0.0002)			
Canal $\times$ Post $\times$ Soldiers	0.0000			
	(0.0000)			
Prefecture Capital $\times$ Post		0.0049		
		(0.0030)		
Canal $\times$ Post $\times$ Capital		-0.0003		
		(0.0002)		
Constant	0.0123***	0.0125***	0.0137***	0.0040***
	(0.0004)	(0.0004)	(0.0003)	(0.0002)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean of the Dependent Variable	0.0074	0.0071	0.0054	0.0036
No. of Observations	150,650	143,052	150,650	150,650
No. of Counties	575	546	575	575
Adjusted R-squared	0.0254	0.0246	0.0959	0.0785

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The treatment is measured by the length of the canal within a county. Standard errors, in parentheses, are clustered at the county level.

Table 12: Testing the Shocks to Agricultural Productivity

	Dependent Variable:		
	Grain Price	Number of Rebellions	
	(1)	(2)	(3)
Canal × Post	0.0003	0.0003**	0.0003**
	(0.0002)	(0.0001)	(0.0001)
Canal × Post × Wetland Rice Suitability		0.0001	
		(0.0003)	
Canal × Post × Wheat Suitability			0.0001
			(0.0002)
Constant	0.4372***	0.0121***	0.0132***
	(0.0109)	(0.0004)	(0.0004)
Dual Interactions	No	Yes	Yes
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean of the Dependent Variable	0.5980	0.0074	0.0074
No. of Observations	91,110	150,650	150,650
No. of Counties	560	575	575
Adjusted R-squared	0.7062	0.0254	0.0258

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The treatment is measured by the length of the canal within a county. Standard errors, in parentheses, are cluster at the county level.

Table 13: Testing Shocks to Trade Accessibility

	Dependent Variable:		
	Town Number	Number of Rebellions	
	(1)	(2)	(3)
Canal × Post	-0.1234***	-0.0021	0.0028*
	(0.0389)	(0.0026)	(0.0014)
Canal × Post × 1776 Urbanshare		0.0373	
		(0.0272)	
Canal × Post × Courier			-0.0015
			(0.0020)
Constant	1.2222***	0.0120***	0.0117***
	(0.0292)	(0.0005)	(0.0006)
Dual Interactions	No	Yes	Yes
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean of the Dependent Variable	2.1228	0.0074	0.0074
No. of Observations	1,104	147,506	150,650
No. of Counties	575	563	575
Adjusted R-squared	0.6523	0.0254	0.0250

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels respectively.

Table 14: Testing the Persistent Effects of the Reform

	Dependent variable:		
	Green Gang High-ranking Members (early 20^{th} century)	Year of Communism Emergence (1920 – 1949)	Armed conflict (1966 – 1971)
	(1)	(2)	(3)
Canal	0.0305***	-0.0327*	0.0027*
	(0.0098)	(0.0189)	(0.0015)
Prefecture FE	Yes	Yes	Yes
Mean of the Dependent Variable	0.3533	1926.7339	0.6603
No. of Observations	317	109	312
Adjusted R-squared	0.1712	0.0597	0.1450

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels respectively. The treatment is measured by the length of the canal within a county.

A More on the Historical Background

This section offers additional historical background on sea transportation reform and the abandonment of the Grand Canal in 1826. We start by introducing the tribute grain system in the Qing Dynasty, when the Grand Canal played a central role. This is followed by a brief discussion of the alternative sea route. We conclude the section with a discussion of the potential motivations for the reform.

A.1 The Tribute Grain System and the Grand Canal

The tribute grain system had been operated in ancient China from as early as the first century AD to address the spatial mismatch between the production and consumption of rice. For most of China's history, the political and economic center was located in the north part of the country. Many of its residents were members of the court, official personnel, scholars, and imperial soldiers and their families (Morse, 1913; Chi, 1936), and did not produce their own rice. As shown in Figure A1, however, most of the rice-producing regions were concentrated in the south, particularly in the middle-lower Yangtze River plain. This required the government in each era to collect rice taxes from the south and transport them to the capital in the north. Most of China's natural rivers run west-to-east, though, and the cost of land transportation (via humans or animals) was at least tenfold the cost of water transportation (Shiue, 2002). Therefore, governments throughout China's history invested massive resources in constructing and maintaining the Grand Canal — the only artificial waterway that linked the south to the north.

The Qing Dynasty inherited the Ming Dynasty's tribute grain system. The government collected rice taxes from eight provinces (plotted in Figure A2) in the central and southern parts of China. The circled area in Figure A2 highlights the intersection between Jiangsu and Zhejiang, the most productive area in the region, which contributed more than 50% of the rice collected. The grains collected from Hubei, Jiangxi and Anhui were first transported via the Yangtze River to Huai'an, where the Yangtze River and the Grand Canal intersect. They were then delivered to Beijing via the canal together with the Jiangsu and Zhejiang grains.

A.2 The Alternative Sea Route

While the government had used the Grand Canal as the exclusive route for tribute grain transportation, an alternative had always existed — the sea route. The sea route was first explored in the Yuan Dynasty (1271 – 1368), to connect Shanghai and Tianjin through the Yellow Sea and the Bohai Sea (see Figure A3). The technical skills needed for sea shipping were acquired long before the establishment of the Qing Dynasty. For example, seven maritime treasure voyages took place between 1405 and 1433 that reached as far as the Arabian Peninsula and East Africa. In addition, the same chartered ships that were later used in sea transportation had already been used by private agents at the beginning of the Qing Dynasty. In the late eighteenth century, there were 3,000 such ships, twice as many as were used in the first sea shipping experiment.

Despite the readily-available technology, the early Qing emperors generally strongly objected to the sea transportation. In 1656, the Shunzhi Emperor issued the sea ban that prohibited any private maritime trading. [44] This ban was reinforced by the "Great Clearance" in 1662 that required all coastal residents to destroy their property and move 20 km inland. While the ban was lifted in 1684, subsequent emperors continued imposing strong restrictions on maritime trade and rejected all proposals to transport grain by sea. In 1816, the Jiaqing Emperor enacted an order that strictly prohibited any discussion of sea transportation (Ni, 2005).

Historians have highlighted four reasons behind the emperors' conservative attitudes towards the sea. The first motivation was related to national security, to protect against threats from overseas. This was the strongest justification for the sea ban enacted in the Ming and early Qing dynasties. Second, after centuries of maritime isolationism, the emperors were generally ignorant about the ocean. This prompted them to avoid all dealings with the sea as a result of ambiguity (uncertainty) aversion (Epstein, 1999). Third, Confucian culture highly valued obedience on the part of emperors to the time-honored rules of their fathers. [45] Any emperor who wanted to alter the established convention without a sufficiently strong justification would incur a high reputation cost. Fourth, such a would-be reformer would have to overcome resistance from strong vested interests and entrenched bureaucrats who benefited from maintaining the tribute grain transportation along the Grand Canal. These personal and political constraints kept sea transportation off the agenda until the early nineteenth century.

A.3 From the Canal to the Sea

The first sea transportation was implemented in 1826 following a natural disaster and a turnover of emperors that altered the associated constraints. The trigger of the reform process was the breach of the Gaojia Dam, the embankment dam at Hongze Lake near the intersection of the Yellow River and the canal, which made the nearby part of the canal too clogged to navigate. [46] While the breach was temporary, it prompted Daoguang — the newly-enthroned middle-aged emperor — to seriously consider the alternative that had been previously rejected by his father (the Jiaqing Emperor). The decision to shift to sea transport was taken for both personal and political reasons. On the personal side, the Daoguang Emperor was more unconventional and open to making changes. He launched a series of reforms during his reign that altered the time-honored rules of his forefathers, including allowing private mining, introducing competition in the salt industry, and redressing Qianlong's literary inquisition. On the political side, launching the sea transportation reform allowed the new emperor to dismantle the old patronage networks and bring in more of his own trusted aides. As summarized in Table A1, the officials who played a significant role in implementing the

[44] The sea ban itself was a continuation of what had been enacted for over 200 years during the Ming Dynasty.

[45] According to Confucius, emperors should behave as models of filial piety, and one of the criteria for that ideal is not altering the conventions of their deceased parents.

[46] The Gaojia Dam was designed to store water in Hongze Lake to flush out the sediments carried into the canal by the Yellow River. The canal became silt-clogged by the lack of water flow after the breach of the dam.

reform were generally much younger than the opposition, and most of their careers flourished during Daoguang's reign.

Importantly, there is no historical evidence to suggest that the reform was inspired or advanced by any actual or anticipated rebellion along the canal. We surveyed the variety of reasons adduced by officials in support of the reform throughout the process. The main argument for sea transportation was its efficiency: it was faster, less expensive, and required less labor. Supporters never mentioned concerns about social instability as a motivation for the reform. In fact, opponents frequently raised concerns about potential disorder against the reform. In one memorial to the Daoguang Emperor, the opponent pointed out that "the sailors would definitely cause trouble" if transportation via the canal was abandoned.

We close this section by comparing the alternative motivations for initiating and advancing the reform. We collected all the proposals ever raised for advancing sea transportation throughout the Qing Dynasty, and regress their emergence on the candidate triggers of the reform: natural disasters, emperor turnovers and previous rebellions. [47] The results depicted in Figure A4 suggest that proposals for sea transportation were more likely to be raised during river breaches (when canal transportation became more costly) and emperor turnovers (when the reform's political benefits increased). The coefficient on lagged rebellions is, if anything, negative and insignificant. This is consistent with our historical narrative that the dam breach and emperor turnover were the major forces that triggered the reform, and that past rebellions along the canal were not a major consideration.

[47] The regression is conditional on emperor fixed effects so we are comparing variations within each emperor's reign.

A.Figures

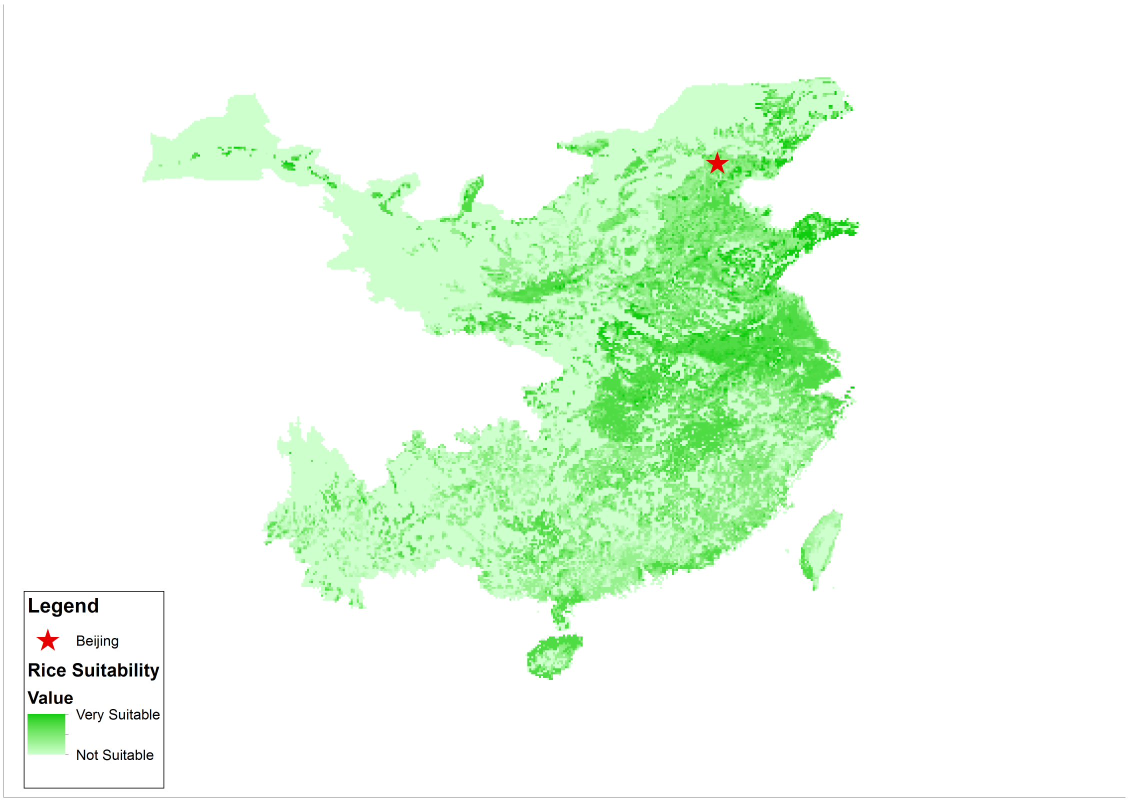


Figure A1: Suitability Index for Wetland Rice (irrigation, medium input)

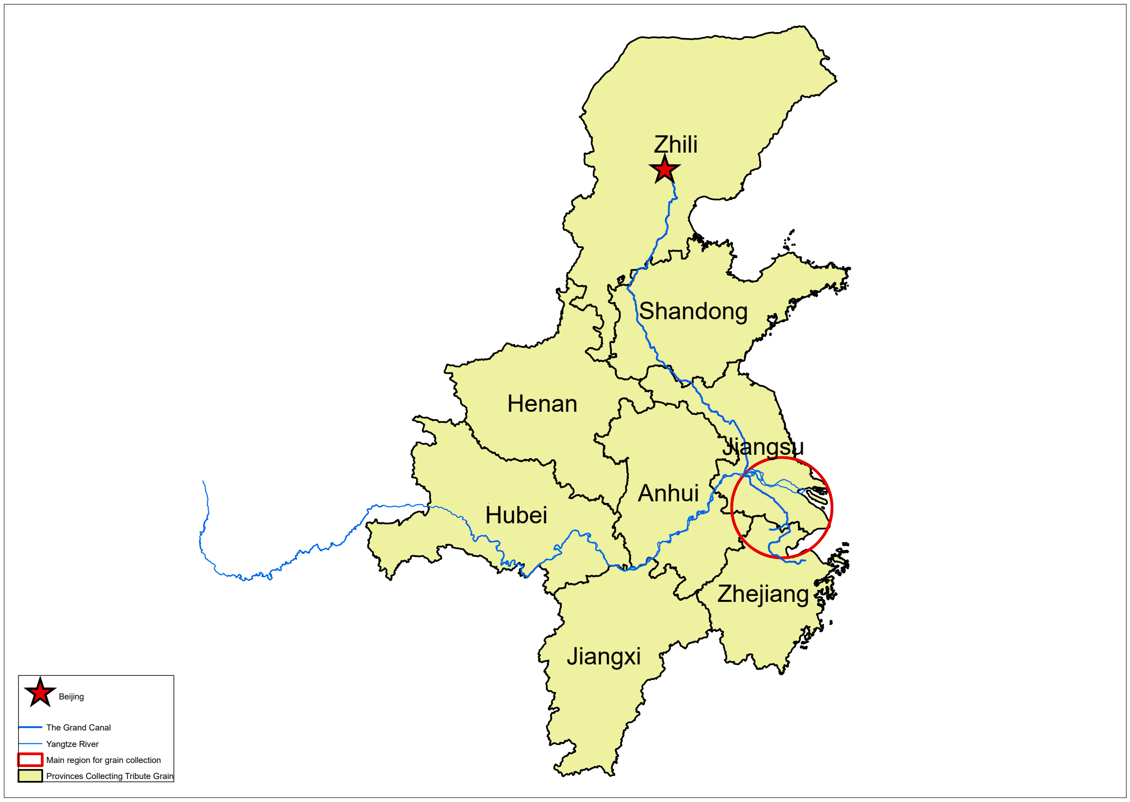


Figure A2: Sources and Shipping Routes of tribute rice in the Qing Dynasty

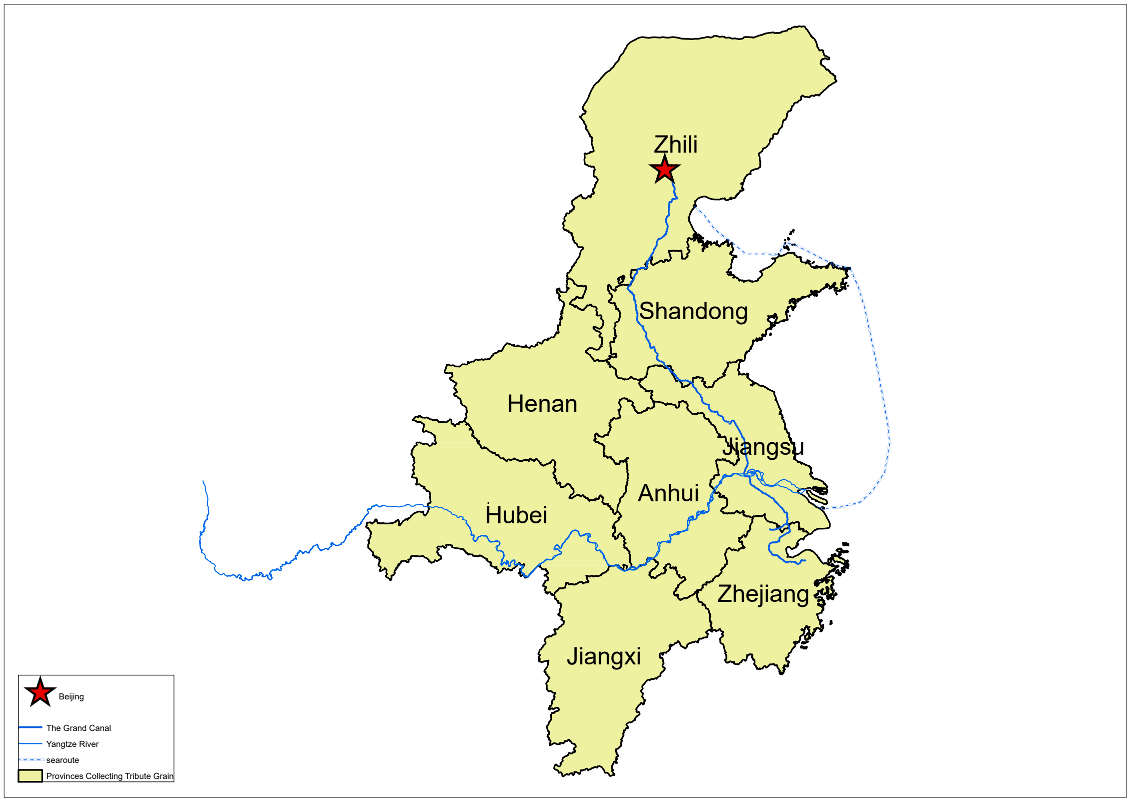


Figure A3: The Sea Route for Grain Transportation

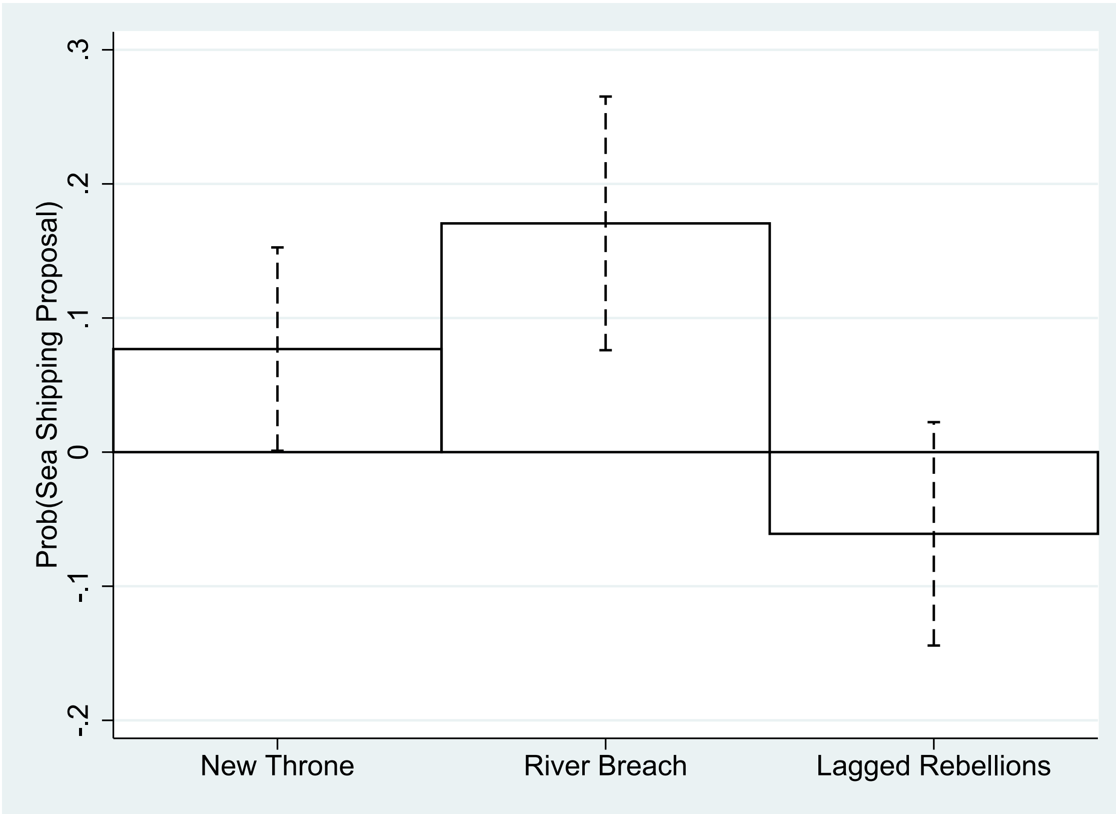


Figure A4: The probability of sea transportation proposals

A.Tables

Table A1: The leading officials for and against sea transportation

Attitude	Name	Birth and Death	Age in 1825	Career after Reform
For	Yinghe	1771–1840	55	Demoted
	Qishan	1776–1854	50	Promoted
	Shu Tao	1779–1839	47	Promoted
	Changling He	1785–1848	41	Promoted
Against	Yuanyu Wei	1767–1825	59	Dead
	Yuting Sun	1752–1834	74	Demoted
	Jian Yan	1757–1832	69	Demoted
	Shicheng Zhang	1762–1830	64	Demoted

Note: Yinghe was demoted in 1827 for misconduct irrelevant to the grain transportation.

B Additional Data Description

B.1 Coding Method

This section summarizes the coding method of our dependent variable: the number of rebellions. We start by describing the structure and content of *Qing Shilu* (The Veritable Records of Qing Emperors). We then describe the detailed steps taken to locate and code the relevant records and provide illustrative examples.

Qing Shilu is a collection of 13 books (see Figure B1 for a photo of its appearance), each corresponding to one of the 13 emperors in Qing China. The books consist of the words, orders, and activities of the emperors documented by the court on a daily basis. To the best of our knowledge, it is a unique data source that systematically tracks the universe of rebellions throughout the Qing Dynasty.

The original books of *Qing Shilu* are very hard to read because of their traditional format (right-to-left, vertical writing) and traditional Chinese language. To facilitate the task, we obtained the digitized text of the books available at *Chinese Text Project* [48] and collected the information in the following steps:

Step 1 We identified the items in the books that are related to rebellions by looking for the keyword "fei" (bandits), which is the most common term used by the Qing government to refer to the rebels. [49] A typical record starts by describing the activities of the rebels followed by the emperors' instructions on how to address them. Specifically, it would mention where the rebels originated, where they were headed, and where they were stationed.

Step 2 We extract the following information through a thorough reading of the texts: i) year of the event reported, ii) counties involved, iii) the activities taking place. [50] For events that involve multiple counties, we identify the associated activities separately for each county (i.e., we have activities for each event-county pair).

Step 3 We pinpoint the counties' location by matching their names to the administrative boundaries of the counties as of 1820. [51]

Step 4 For each event-county record, we categorized the associated activities into five groups according to the descriptions of the event: *onset*, *attacking*, *defending*, *stationing*, and *retreating*. Specifically, *onset* refers to cases where the rebel group did not exist previously and

[48] *https://ctext.org*. See Sturgeon (2019) for a description of the project.

[49] The Qing government often referred to rebel groups according to their identity (usually the location or the leader's surname) followed by the keyword "fei". For example, "yue fei" refers to rebel groups from Guangdong and Guangxi (also named "yue"); "cuan fei" refers to rebels moving around ("cuan").

[50] We keep track only of the records that involve at least one county in our sampled area (i.e., the six provinces close to the canal) and do not have information about rebellions taking place outside this area.

[51] The county boundaries were relatively consistent throughout the Qing Dynasty (Ge, 1997) despite the frequent adjustments in prefectural and provincial boundaries. In the rare cases when the names did not match (often due to changes in names, merges and splits), we relied on online searches to link the county names mentioned in the records to 1820 counties.

started to rebel locally. This is often identified by phases such as "hu you" (suddenly there is), "shu qi" (raise their flag), "qi shi" (starts rebelling), etc.. *Attacking* refers to cases where the rebel group already exists and is trying to attack **another** county. *Defending* refers to cases where the rebel group already exists and is being repressed by the government. *Stationing* refers to cases where the rebel group already exists and is staying in one county without other military action. Finally, *Retreating* refers to cases where the rebel group already exists and is retreating to a different county (often after being defeated by the government). [52]

Step 5 Finally, for each county-year we count the number of events by action type and construct a balanced panel where the value of 0 is assigned to county-year pairs with no reports of a specific type of action. We also generate for each action type a dummy variable indicating the presence of the specific type of action in the county year.

Although the books of *Qing Shilu* are the most reliable source available for information on rebellions in the Qing Dynasty, the fact that they are not statistical books in standard format posed some complications for our data-collection process. Such complications, if not handled properly, could have affected the accuracy of the data collected. We made every effort to address these complications. First, while most of the events were reported in the year in which they occurred, in some cases they were reported one or more years later, especially if an event took place at the end of the year but was reported at the beginning of the next year. While we rely primarily on the year of reporting to document time, we identify phrases such as "last year" and "back in *some specific year*" to make corresponding corrections.

Second, the records for some years are known to be inaccurate. For example, the cases reported in 1768 are mostly miscarriages of justice in which the innocent people were accused and interrogated during the government's campaign against rumors of sorcery. [53] The reports in 1818 are mixed up with many previous events over the previous decades as a result of a backlog clearing campaign. Therefore, we discard all cases reported in 1768 and 1818 to ensure that our results are not biased by the distortion. [54]

Third, it is not uncommon in *Qing Shilu* for one event to be reported and discussed multiple times, which could have caused serious double-counting problems. However, when the record refers to an event that was already reported, it typically starts with an indicator phrase such as "as reported before". We use such phrases to identify and discard duplicate reports to minimize the risk of double counting. We also discard cases with phrases that imply within the text that they are explicitly connected to previous ones (e.g., one is a continuation of the other, or initiated by the same leader, or there is some sort of collusion between the rebels, etc.). [55]

[52]Unfortunately, our data do not allow us to further distinguish among various types of rebellions, e.g., food riots or political grievances.

[53]In spring 1768, mass hysteria broke out over rumors that sorcerers were roaming the country, cutting off men's braids and stealing their souls. During the campaign against the rumor, people brought false charges against socially marginalized people, and officials extracted confessions of sorcery from the innocent under torture. (Philip, 2009)

[54]We also verified that our findings are scarcely affected by including these cases.

[55]It is nevertheless possible that the two events are implicitly connected in an unobservable manner that is not recorded, causing the potential double-counting problem in the data. We address this concern by also

Fourth, in cases where the rebels were reported to have spread across multiple counties, we code their actions in each county separately. [56]

Finally, the cases reported in the books might also capture battles between the Qing government and its major enemies (e.g., the British army, the Taiping army, and the Nian army). Unfortunately, our data source does not provide us with enough information to identify whether a case actually belongs to any of these events. However, since these events generally started from outside our sample period and lasted for a few years, most of the associated actions would be categorized as *attacking*, *defending*, *retreating*, or *stationing*. Therefore, when we focus our analysis on the *onset* measure, there is little chance that it could directly capture the campaigns of these historical events.

using the binary indicator of the presence of rebellions in our analysis.

[56] To illustrate, consider a group of rebels that started in county A, attacked counties B and C, and retreated into county D after being repressed. In our data set, county A will receive 1 count of *onset*, counties B and C will each receive 1 count of *attacking*, and county D will receive 1 count of *retreating*.

A.Figures

Figure B1: Photograph of *Qing Shilu*

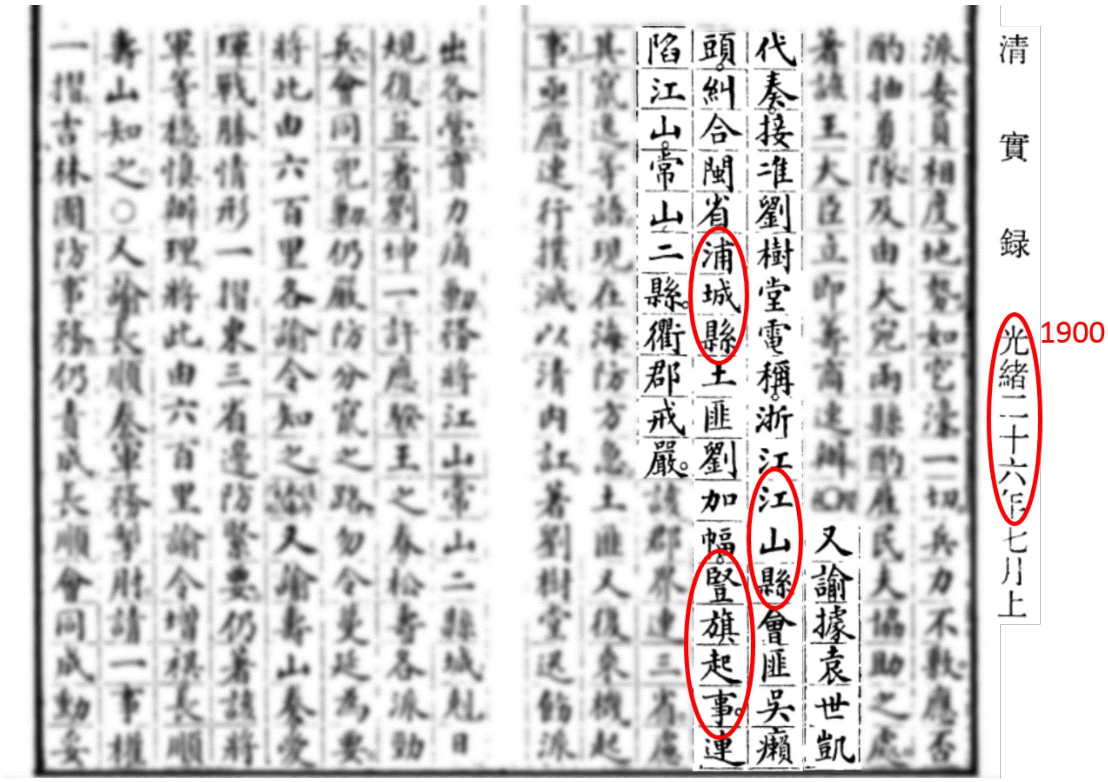


Figure B2: Coding Method

July, Guangxu 26 (1900):

> *A telegraph report from Shutang Liu suggests that a rebel group led by Laitou Wu and Jiafu Liu has formed in Jiangshan County and Pucheng County. They have captured Jiangshan County and Changshan County. Keep alert!*

Guangxu Shilu (vol. 266)

C Supplementary Results

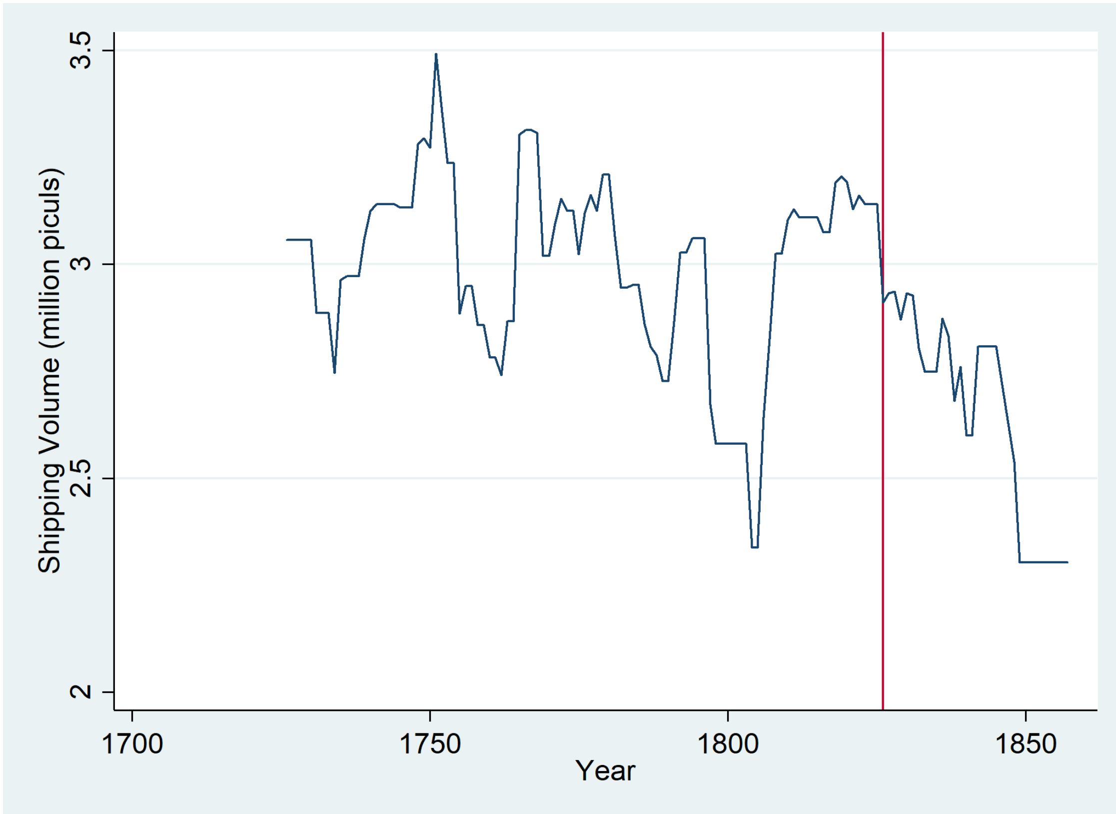
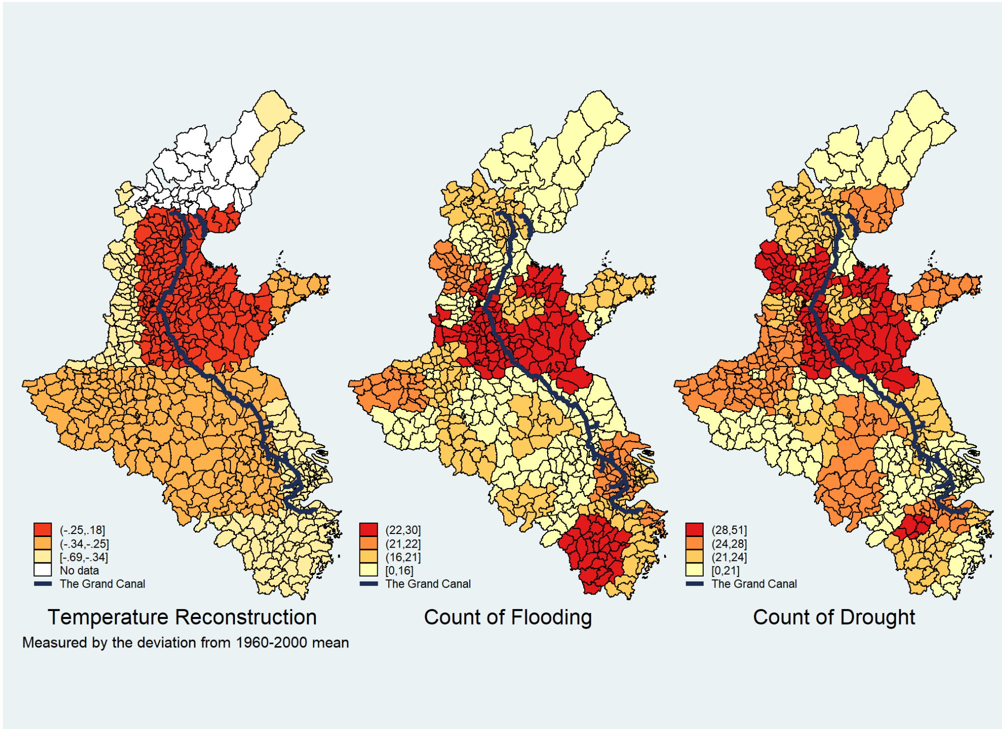
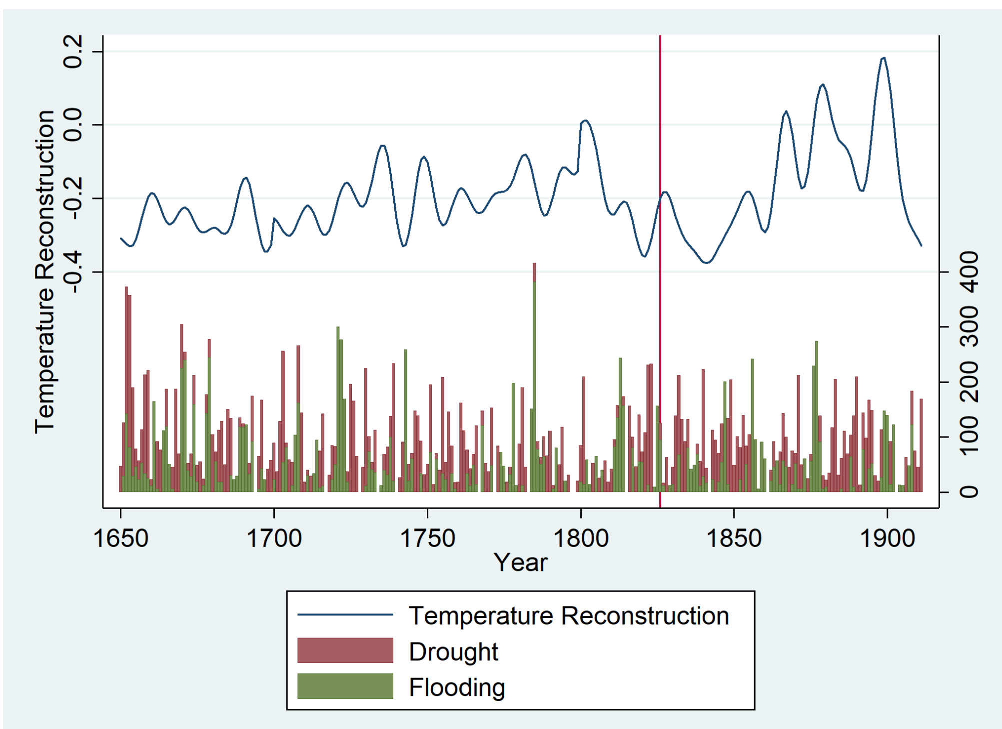


Figure C1: Grain Shipping Volumes



(a) Across Regions



(b) Over Time

Figure C2: Distribution of Climate Measures

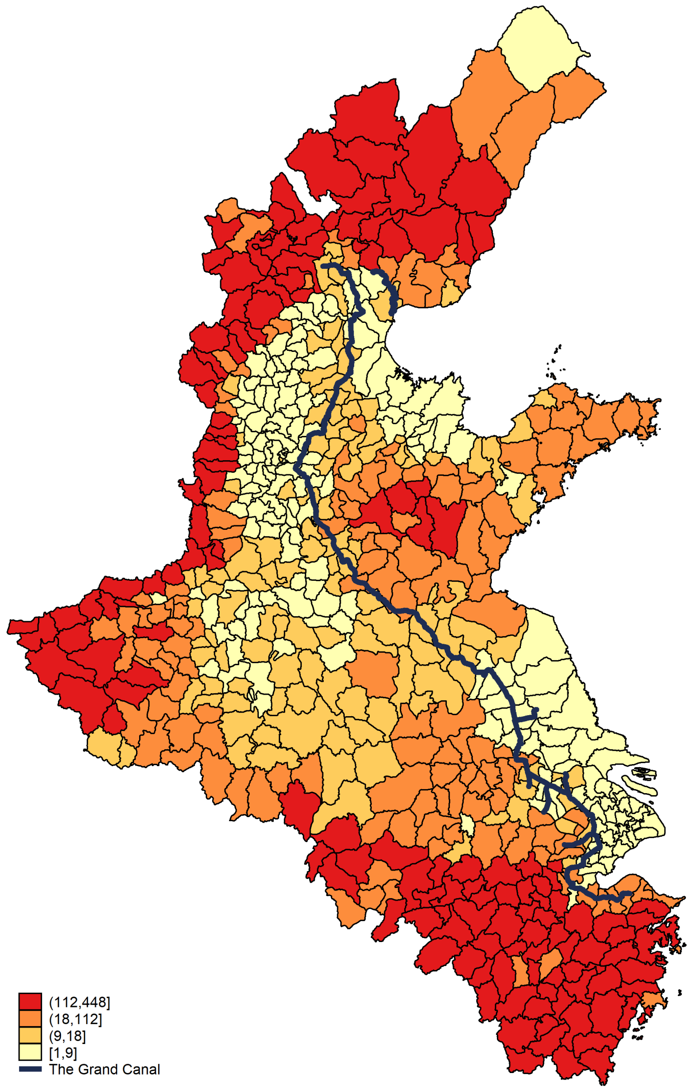
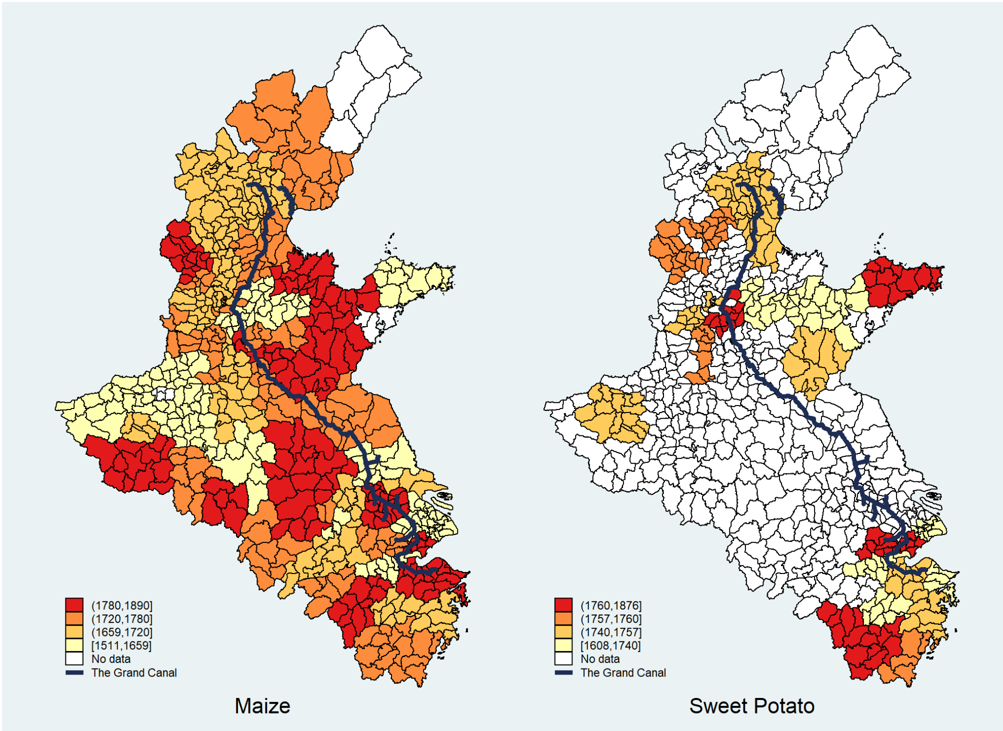
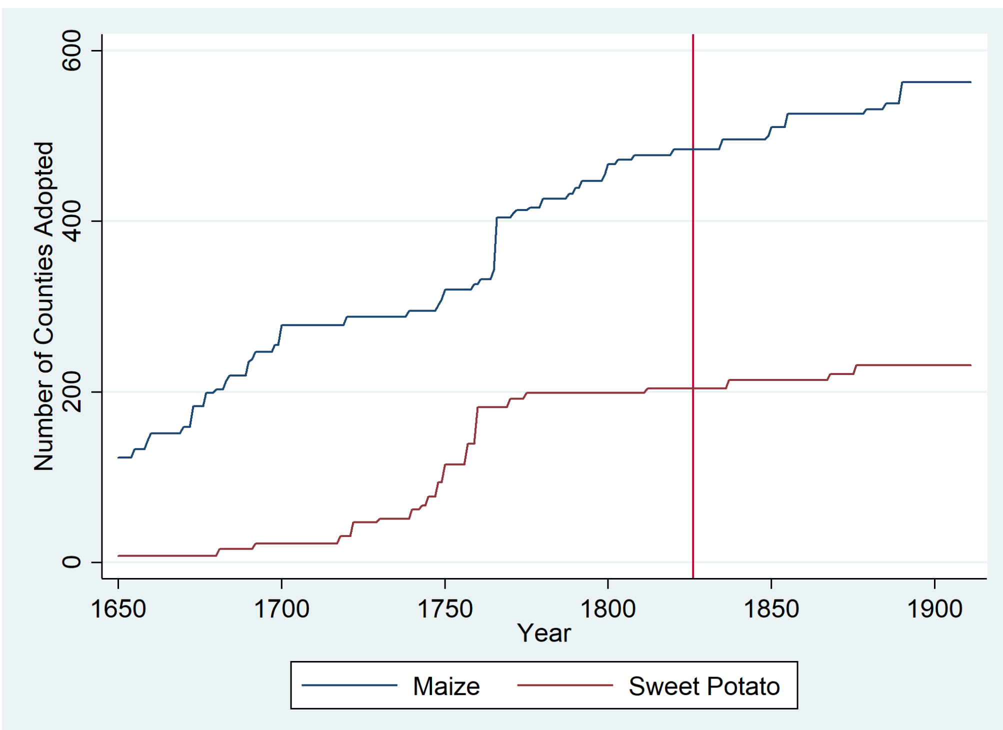


Figure C3: Spatial Distribution of the Ruggedness Index

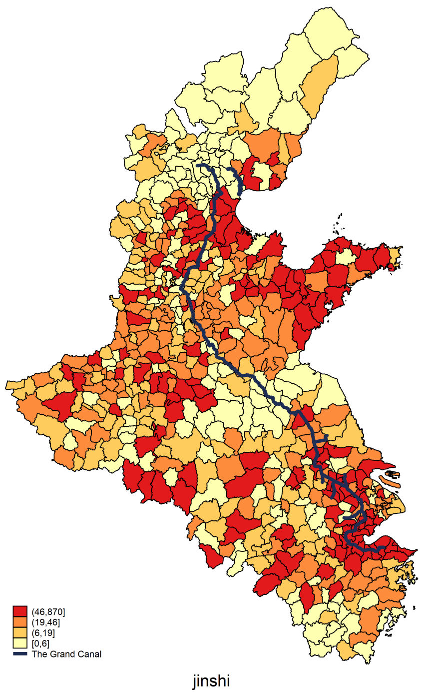


(a) Year of Adoption

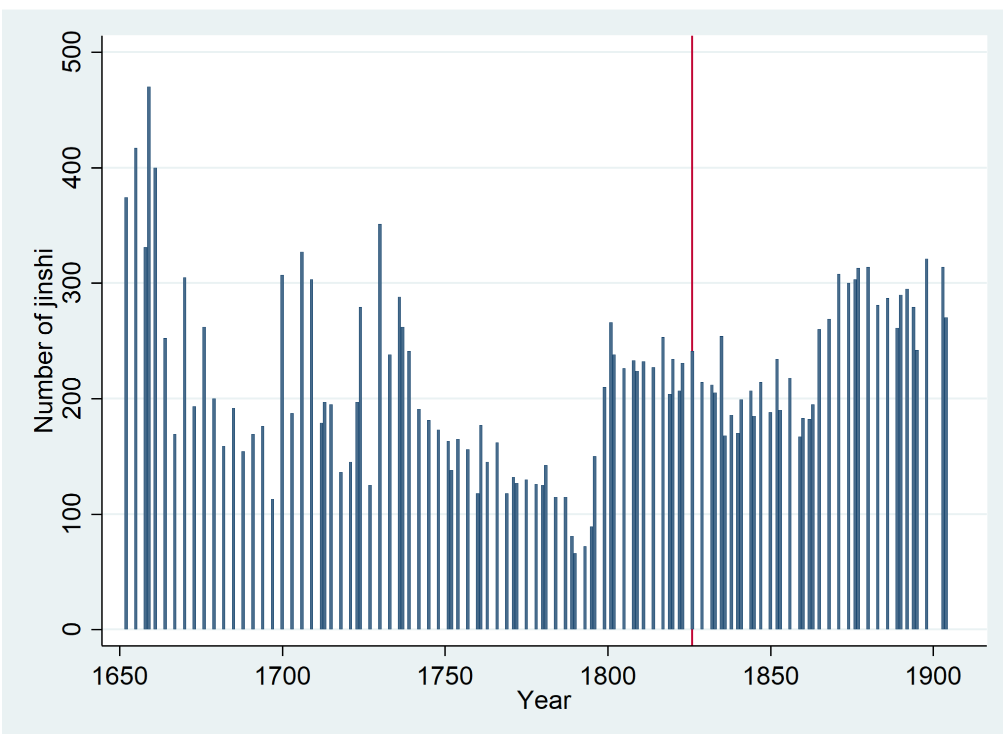


(b) Number of Counties Adopted

Figure C4: The Spread of New World Crops



(a) Across Regions



(b) Over Time

Figure C5: The Distribution of *jinshi*

Table C1: The Effects of the Canal's Abandonment on the Number of Rebellions Clustered at the Prefecture Level

	Dependent Variable: Number of Rebellions			
	(1)	(2)	(3)	(4)
Along Canal × After Abandonment	0.0101**	0.0110***	0.0087**	0.0089**
	(0.0045)	(0.0041)	(0.0042)	(0.0040)
	[0.0026]	[0.0022]	[0.0027]	[0.0023]
Constant	0.0070***	0.0069***	0.0070***	0.0070***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Province × Year FE	No	Yes	No	Yes
Prefecture Year Trend	No	No	Yes	Yes
Mean of the Dependent Variable	0.0074	0.0074	0.0074	0.0074
No. of Observations	150,650	150,650	150,650	150,650
No. of Counties	575	575	575	575
No. of Clusters	79	79	79	79
Adjusted R-squared	0.0251	0.0405	0.0279	0.0432

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels respectively. Standard errors in parentheses are clustered at the prefecture level. Standard errors in square brackets are Conley standard errors that are robust to spatial correlation.

Table C2: The Effects of the Canal's Abandonment on the Presence of Rebellions

	Dependent Variable: Presence of Rebellions			
	(1)	(2)	(3)	(4)
Along Canal × After Abandonment	0.0097***	0.0106***	0.0080**	0.0083**
	(0.0035)	(0.0036)	(0.0035)	(0.0036)
	[0.0025]	[0.0021]	[0.0025]	[0.0021]
Constant	0.0068***	0.0067***	0.0068***	0.0068***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Province × Year FE	No	Yes	No	Yes
Prefecture Year Trend	No	No	Yes	Yes
Mean of the Dependent Variable	0.0072	0.0072	0.0072	0.0072
No. of Observations	150,650	150,650	150,650	150,650
No. of Counties	575	575	575	575
No. of Clusters	575	575	575	575
Adjusted R-squared	0.0239	0.0385	0.0265	0.0410

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Standard errors in parentheses are clustered at the county level. Standard errors in square brackets are Conley standard errors that are robust to spatial correlation.