Rebel on the Canal: Disrupted Trade Access and Social Conflict in China, 1650–1911

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December 18, 2021

Abstract

This paper examines the effects of the abandonment of China's Grand Canal — the world's largest and oldest artificial waterway — which served as a disruption to regional trade access. Using an original dataset covering 575 counties over 262 years, we show that the canal's abandonment contributed to the social turmoil that engulfed North China in the nineteenth century. Counties along the canal experienced an additional 117% increase in rebelliousness after the canal's closure relative to their non-canal counterparts. Our findings highlight the important role that continued access to trade routes plays in reducing conflict.

Keywords: Trade Access; Conflict; Transportation Infrastructure; China

JEL Classification Numbers: D74, N75, O18.

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The question of whether access to trade opportunities enhances or undermines social stability fuels a long-running and controversial debate in economics and other social sciences. Easy access to trade may boost income, secure employment, and hence stablize the society, yet it may also fuel social unrest by offering a larger prize to seize (Becker, 1968; Grossman, 1991). There is limited causal evidence that informs us about this debate and the findings are ambiguous. Our paper approaches 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 functioning of the canal was maintained by the government, which used 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. The official transportation of grain taxes also contributed directly to commercial prosperity as the official boats were allowed to carry duty-free commodities during their services. In 1826, a severe breach of the canal prompted the government to experiment with a sea-transportation of tribute rice, which led eventually to the closure of the canal, and abruptly 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. Second, there is rich information available on social unrest in China over a long 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 and Watkins, 2007; Djankov and Reynal-Querol, 2010; Kung and Ma, 2014; Janus and Riera-Crichton, 2015; Jha, 2013).

[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." (Smith, 1776)

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 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 difference-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 starting point of the canal's decline, as the treatment date, and have verified that this choice is consistent with the pattern in the data. We find no anticipatory increase in rebellions before the initiation of the reform. Our findings indicate a higher number of rebellions associated with the canal's closure: compared with distant counties, canal counties experienced a 0.0380 increase in rebellions per million population after the reform than before. This effect corresponds to a 117% increase over the sample mean (0.0330), a finding that is significant at the 5% level. The estimates are robust to controlling for various confounding factors, including geography, demography, climate, agriculture, and pre-treatment level of rebelliousness. We allow the effects of all control variables to vary across time. We have also tested that the results are not subject to specific modeling choices regarding sample selection, outcome construction, and standard error adjustments.

The baseline model with the binary treatment variable is then extended to account for more flexible treatment intensity measures along various dimensions. We first explore, among canal counties, variations in the extent to which the canal matters for the county, both geographically and economically. We show that the reform's impact on rebellions is proportional to the length of the portion of the canal contained within a county (normalized by the county's land size), and to the share of market towns within 10 kilometers from the canal in 1820. We then turn to examine the potential range of the canal's impact by estimating a spatial gradient model for the distances from the canal. We find a decreasing effect with distances from the canal and the impact spills over to counties up to $150km$ away from the canal.

One central concern to our identification is whether canal counties and non-canal counties are comparable to each other. While we have controlled for the pre-treatment level of rebelliousness interacted with year dummies and the set of period-specific covariates, one might still worry about the possible existence of unknown or unobservable characteristics pertaining to the canal, possibly leading to a differential reaction to common shocks. We apply three independent approaches to delve into addressing this inherently challenging problem. First, we show that our findings survive two alternative estimation techniques that accommodate systematic differences between the treated and the control groups: changes-in-changes

(Athey and Imbens, 2006) and synthethic control method (Abadie and Gardeazabal, 2003; Abadie, Diamond and Hainmueller, 2010). Second, we find no placebo treatment effects along other transportation routes that might have similar influences as the canal's historical presence but did not suffer much from its closure. Finally, we examine two prominent events in the mid-19th century — the Opium War and the Taiping Rebellion — and find that canal counties did not reacted more violently to these events. These pieces of evidence collectively support a causal interpretation of our findings that the increased rebelliousness in canal counties since 1826 is likely attributable to the loss of the canal.

We discuss potential mechanisms through which the canal closure might destabilize society. First, we investigate whether the reduction in repressive capacity following the closure could explain our findings. We evaluate this interpretation using two different approaches and do not find evidence in support of it. We then examine the alternative mechanism that the closure deprived canal counties of their access to regional trade. Specifically, we find that: i) the closure slowed down the development of market towns around the canal, ii) the effects are much smaller for counties with access to alternative trade routes, and iii) the canal stopped to make an effect in risk-mitigation, a role that trade usually plays upon negative shocks (Burgess and Donaldson, 2010). While suggestive and inconclusive individually, these findings collectively point towards the loss of trade access as a likely channel through which the closure impeded political stability. We lay out additional statistical results consistent with the historical narrative that urban unemployed workers (e.g., sailors and dockworkers) might be the major group who suffered and rebelled after the closure. We briefly discuss the potential long-term implications of this narrative.

Our work contributes most directly to the long-standing debate over the implications of trade access 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 (e.g., Hirshleifer, 1989; Grossman, 1991; McGuirk and Burke, 2020). Empirical studies — in light of the endogeneity concerns — rely mostly on trade volatility shocks at the intensive margin, which, however, produce rather mixed results (see Dube and Vargas (2013) for a destabilizing effect and Bazzi and Blattman (2014) for a null effect). 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. [2] Moreover, we distinguish from previous work by studying an urban (rather than rural) setting in which the economy depends on internal (rather than

[2] Berman and Couttenier (2015), while again focusing on price-volatility shocks, suggests 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.

international) trade. In particular, we focus on the roles of unemployed urban workers who earned a living in the circulation and exchange of commodities. This explanation presents a nuanced but intriguing distinction from the existing narratives, in which trade volatility mainly affects the profitability for the producers (or the survivability of the consumers). 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 trade integration and liberalization.[3]

More broadly, this study contributes to the large body of literature on economic shocks and civil conflicts (see Miguel, Satyanath and Sergenti (2004) and Miguel (2005) for seminal contributions, and Blattman and Miguel (2010) for a 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, Feigenberg and Teshima (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), Donaldson (2018), and Banerjee, Duflo and Qian (2020)). 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 (Perlman and Schuster, 2016; Burgess and Donaldson, 2010).

Finally, our work also sheds light on the chronic social disorder that afflicted nineteenth-

[3] In this sense, our work echoes Amodio, Baccini and Di Maio (2021), which shows that trade restrictions (often motivated by security concerns) increases the episodes of political violence in localities where the restricted sectors concentrated.

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 (Esherick, 1988; Perry, 1980; Liu, 2007). 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 time. [4]

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 II we present the data. We formalize our empirical strategy in Section III and demonstrate the baseline results. Section IV delves into the addressing the central concern regarding the causal interpretation. In Section V we discuss the possible mechanisms and their implications, and Section VI concludes.

I Background

A The Grand Canal

The 1,776 kilometers 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). 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 (See Appendix B.1 for institutional details). In the early nineteenth century, approximately 3.5 million piculs of rice (roughly 560 million pounds) were delivered

[4] 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).

to the capital annually (Huang, 1918). Maintaining the canal was therefore one of the most crucial tasks for the Qing government (Leonard, 1988; Hummel, 2010; Cheung, 2008).

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 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.

B Abandonment of the Grand Canal

The Grand Canal was gradually abandoned by the government in the nineteenth century. The triggering event was 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 the canal. In response, the government launched a first experiment in 1826 to explore an alternative route for tribute grain transportation through the East China Sea. The experiment was successful and demonstrated the feasibility and efficiency of sea-shipping. However, the reformers' efforts to urge the continuation of the sea route were strongly resisted by vested interest groups associated with canal transportation. For this reason, the operation of the canal was restored in the following year, and the government continued its investment in restoring and maintaining the canal for at least another two decades. It was not until the late 1840s that the government took further steps toward a permanent reform. Canal shipping of tribute rice ceased to operate after 1855; since then the government stopped the canal's maintenance (Fairbank, 1978; Leonard, 2018). As a result, many sections of the canal became so clogged that they were no longer navigable at all by the end of the nineteenth century. The government officially announced the canal's closure in 1901 (Li and Jiang, 2008).

The 1826 experiment, while temporary, marked a milestone of the destiny of the canal.

After the success of this experiment, sea transportation became, for the first time during the Qing dynasty, a viable option for tribute rice transportation, which could be adopted whenever needed. That is, the possibility of a total closure became real and foreseeable, even though it was delayed by the existence of vested interested groups. Meanwhile, the condition of the canal started worsening, and its usage declined. Figure 2 shows the amount of tribute rice transported via the canal. The data are unfortunately sparse and noisy, but there is a suggestive downward trend since 1826. If one ignores the fitted line and looks directly at the data themselves, we see that, despite the initial restoration, there was considerable fluctuation in the following years. In particular, we observe a sharp decline in canal transportation in the early and mid-1830s. This pattern is consistent with the custom revenue reports at the Huai'an depot, a lynchpin of the Grand Canal, in which the reported trade volume dropped by about 30% between 1818 and 1831 (von Glahn, 2018). Thus, the decline of the canal likely started since as early as 1826, before subsequent steps toward a total closure were put forward. For this reason, we consider 1826 experiment as the treatment date for our analysis.

To assess the extent to which the 1826 experiment may be motivated by existing or anticipated rebellions associated with the canal, we look into the parliamentary debates on sea versus canal shipping. The main reason adduced by the reformers in support of sea-shipping was its cost-efficiency (faster, cheaper, and laborsaving). The reformers did not consider social disorder as a ground for abandoning the canal. Rather, the opponents frequently referred to social instability concerns as a claim against sea-shipping (Ni, 2005). Thus, it seems unlikely that the 1826 experiment was a response to past or future rebellions.[5]

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. The situation is particularly severe for the northern sections of the canal, as the canal's operation in the north relied more heavily on the effectiveness of its maintenance. As the canal's navigability declined, commodities had 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. 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).

[5]We also conduct a brief survey of other historical events that might be associated with social instability, including the White Lotus Rebellion (1794–1804), the Miao Rebellion (1795–1806 and 1854–1872), the Opium War (1839–1842), and the Taiping Rebellion (1851–1864). None of these events occurred around the 1826 period, and they mostly affected regions far from the canal. To the extent of there being other events we are unaware of, Section B presents a set of placebo tests to mitigate the concern.

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.

II Data

We construct an original panel dataset from several historical sources spanning the 1650 – 1911 period. 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 stable during the study period (relative to 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).

A Rebellions

Our primary dependent variable is the number of rebellions reported in each county and year, normalized by the county's initial population size. We construct this measure as follows. First, we count 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 the most reliable and comprehensive information on social unrest during the Qing Dynasty (Yang, 1975; Kung and Ma, 2014). Meticulously compiled by the Qing Court, it details the times and places of all rebellions during this period. We focus our analysis on the *onset* of rebellions, excluding the continuation of existing rebel groups that may spread across multiple regions or last for years (see Appendix C for details). During our sample period, there were a total of 1,144 reported rebellion onsets (4.37 annually). The average number of rebellions onsets that a county experienced in a year is 0.0076.

We then normalize the count of rebellions by the population size at the beginning of the period. We take 1600 as the reference year, for which Cao (2001) provides an estimate of the population size at the prefecture level. To estimate the county-level population in that year, we leverage an additional data source from Liang (1980) which estimates the county-level households in 1546. We impute the 1600 population in a county by allocating the prefecture's 1600 population according to the distribution of households within that prefecture in 1546.[6] This normalized rebellion measure is available for 536 counties in our sample.

B Treatment Intensities

We construct four measures of treatment intensity. For our baseline analysis, we employ a binary measure based on the geographic adjacency to the canal. Counties bordering or containing a portion of the canal are considered in the treated group, while those away from the canal are considered in the control group. We assign the values based on a spatial join of the county boundaries and the canal's path, both from CHGIS (2007). In our sample, 73 out of the 575 counties are assigned to the treated group (canal counties).

For the more extensive analyses, we construct three intensity measures that exploit additional variations within the baseline treated and control groups. The first two measures consider the extent to which the canal matters geographically and economically for a canal county. A county's geographic dependence on the canal is proxied by the length of the portion of the canal the county contains. The length of the canal segment contained in a county is 32.45 km on average, with the longest one running 91.44 km. The economic dependence is proxied by the share of towns (serving as local markets) within 10 kilometers from the canal. The mean value conditional on being along the canal is 50%. Lastly, we compute the distance from a county's administrative center to the canal to measure the spatial gradient of the canal's impact beyond county boundaries. The average distance from the canal is 118 km in the sample, while the farthest county is 499 km away. All measures are constructed by applying the spatial join and the proximity analysis techniques to historical shapefiles provided by CHGIS (2007).

[6] More specifically, the 1600 population size of county i in prefecture p is given by:

$$population_{i,1600} = \frac{households_{i,1546}}{\sum_{j \in p} households_{j,1546}} \times population_{p,1600}$$

C Control Variables

We include the following controls to alleviate the concern about omitted variable bias:

Geography We include two geographical measures in our analysis. We first include a county's land size which comes from the county-level shapefiles in CHGIS (2007). The average land size of a county in our sample is $1622km^2$. The spatial distribution of land size is depicted in Appendix Figure C3. The second 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, DeGloria and Elliot, 1999). Grid-cell elevation per 30×30 arc seconds is obtained from GTOPO30 (US Geological 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 C4, with a mean of 16.92 and a standard deviation of 19.53.

Demography We include the initial population density in 1600 as a control for demography. The variable is constructed from Cao (2001) and Liang (1980) following the procedure described above. The spatial distribution of this initial population measure is depicted in Appendix Figure C5.

Climate We consider climate shocks as another fuse of rebellion (Miguel, Satyanath and Sergenti, 2004; Miguel, 2005; Hsiang, Meng and Cane, 2011; Hsiang, Burke and Miguel, 2013). We measure climate shocks using 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 assign grid-cell temperatures to counties in our sample based on the cell's coverage and define a temperature anomaly as a temperature that was beyond one standard deviation of the mean. Such temperature anomaly occurred approximately every three years in our sample. The alternative measure of climate shocks is the historical presence of extreme drout and flooding 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 Appendix Figure C6. We do not see any evidence of climate shock specific to the canal area or around 1826.

Agriculture It is well documented that social conflicts are also subject to agricultural productivity (see, for example, Jia (2014) and Iyigun, Nunn and Qian (2017)). We consider the impacts of both traditional and New World crops. For traditional crops, we include the suitability index for wetland rice and wheat, the two main crops in our sample area (Talhelm et al., 2014). The information is extracted from IIASA/FAO (2012). The spatial distribution of the suitability for each of the two crops is depiected in Appendix Figure C7. For New World crops, we consider the planting duration of two most prominent New World crops in China: maize and sweetpotato. The information comes from Chen and Kung (2016) and Jia (2014), respectively. Figure C8a shows the year when the two crops were first adopted, which does not appear to depend on the canal. Figure C8b calculates the number of counties in which the crops have been adopted for each year. It appears that the spread of the crops does not coincide with the canal's decline.

Table 1 summarizes the sources of and descriptive statistics for all the variables used in our analysis. Appendix Table A1 presents a comparison between the canal and non-canal counties in terms of the covariates. We notice that canal counties and non-canal counties are not systematically different in most dimensions. The two exceptions are initial population density and terrain ruggedness. While these variables are time-invariant and therefore their effects should be captured by the county fixed effects, one might worry about their effects differing in the two periods. Therefore, we will control for the period-specific effects of all these covariates in our empirical analysis.

D Suggestive Evidence

Before proceeding to the formal analysis, we provide some descriptive evidence to help place our findings in context. Figure A1 shows the distribution of rebellions over time. It shows clearly that the frequency of rebellions significantly increases following the abandonment of the canal: from 1.35 annually before 1825 to 10.55 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 3 shows the distribution of rebellions in the pre-abandonment period, while the right panel shows the distribution in the post-abandonment 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.

III Empirical Strategy and Results

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

A Empirical Strategy

Our empirical strategy follows the standard DID approach. We compare the relative changes in the frequency of rebellions in counties through which the canal runs relative to distant counties. The model specification takes the following form:

$$Y_{ct} = \beta AlongCanal_c \times Post_t + \delta_c + \sigma_t + \chi_{ct} + \varepsilon_{ct} \tag{1}$$

where c indexes counties and t indexes years. $AlongCanal_c$ is a dummy variable that equals one if a county contains a canal stretch and zero otherwise. Hence, the treated group comprises canal counties while the control group comprises other (non-canal) 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 frequency of rebellions. We expect the coefficient 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. Year fixed effects control for any secular patterns of rebellions that similarly affect all regions. We include pre-treatment rebelliousness times year dummies to account for the possibility that counties prone to disruptions may have differential reactions to common events. The model also takes into consideration province-specific year dummies as well as the prefecture-specific time trends. The identification relies on the assumption that there are no other omitted variables or events beyond those we have controlled that coincide with the reform and affect social unrest. We should not take this assumption for granted because China experienced many notable events during the nineteenth century (for example, the Opium War and the Taiping Rebellion). We address this issue in Section IV.

We define the pre- and post-reform periods by the first sea transportation experiment in 1826 for three reasons. First, the 1826 experiment, though temporary, marked a milestone of the destiny of the canal, which might change the expectations and behaviors of all parties involved, especially those forward-looking merchants. These changes likely affected canal trade even after the government restored the operation of the canal in the following decades. Second, we observe a downward trend in canal usage following the 1826 experiment (see Section I for details), which indicates a gradual decline in the canal economy since then. Moreover, the 1826 experiment provides a cleaner setting for identification, whereas those later dates could be endogenous to socioeconomic conditions that were shaped, perhaps in the first place, by the 1826 experiment. Therefore, we believe that 1826 would be a reasonable treatment date choice for identification purposes.

We confirm that this choice of treatment date is consistent with patterns in the data. Specifically, we estimate a fully flexible decade-by-decade estimating equation that takes the following form:

$$Y_{ct} = \sum_{\tau=-50}^{70} \beta_\tau AlongCanal_c \times Decade_\tau + \delta_c + \sigma_t + \chi + \varepsilon_{ct} \tag{2}$$

where all variables are defined as in Equation (1). The only difference from Equation (1) is that in Equation (2), rather than interacting $AlongCanal_c$ with a post-reform indicator variable, we interact the treatment variable with each of the decade fixed effects (relative to 1826), treating the period more than 50 years before 1826 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 4 plots the estimates of Equation (2). A clear pattern emerges from the figure. The difference between the treated and control groups is constant over time and small in magnitude before 1826. After the 1826 experiment, we first observe an increase in conflict in the first 10 years (1826–1835) and a drop in the next 10 years (1836–1845). The rise and fall of conflict alongside the canal during this period (an inversed-V shape) corresponds to the drop in canal usage in the early 1830s and the rebound around the 1840s (a V-shape, as shown in Figure 2), suggesting that conflict reacted to changes in canal usage during this period. The difference between canal and non-canal counties increased again after the late 1840s when further steps toward a total closure were in place. The effect peaked 40 years after the first experiment (up to 1865), and converged from the 1870s onwards. This pattern is consistent with a gradual decline of the canal starting in 1826, which confirms our choice of 1826 as the treatment date.

The point estimates shown in Figure 4 also suggest no differential trends between the two groups before 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 $AlongCanal_c \times Year_t$ captures the difference in time trends between the treated and control groups. We summarize the results in Table 2, with different sets of controls included to derive the table columns' results. 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 likely satisfied.

B Baseline Estimates

We present our baseline estimates derived from Equation (1) in Table 3, where the dependent variable is the frequency of rebellions normalized by population, calculated as the inverse hyperbolic sine of the value. The four columns reflect varying combinations of controls. For column (1), we control for county and year fixed effects. This specification allows us to rule out all time-invariant county features and year shocks that unanimously affect all regions. For column (2), we include the pre-treatment measure of rebellions (the total number of rebellions per capita in the pre-treatment period, with the arcsinh transformation) interacted with year dummies. This specification allows common shocks to have differential effects in counties that were already more prone to disruptions in the first period. [7] For column (3), we add province-year fixed effects to take into account differential time effects across provinces. For column (4), we add prefecture-specific year trends to account for differences in regional trends. For column (5), we further include the set of control variables introduced in Section C; we allow these covariates to have differential effects in the pre- and post-reform periods by interacting each of them with the post-reform indicator. For each column, we report two sets of standard errors. First, the standard errors in the parentheses are clustered at the county level, as counties represent the lowest and foundational administrative division with complete bureaucratic systems and function independently of one another Ge (1997). Second, we report in square brackets the Conley standard errors following the approaches

[7] We would like to deliver our deep gratitude to the anonymous referees for suggesting this excellent specification.

suggested in Conley (1999) and Conley (2008), assuming a cutoff window of 500km and allowing a serial correlation across all 262 years.[8]

The results we obtain across all specifications are positive and significant, suggesting that more rebellions occurred in canal counties after the abandonment. For example, the point estimator reported in column (1) is 0.0380, which represents an $exp(0.0380) - 1 = 0.0387$ increase in rebellions per million population. This effect corresponds to a 117% increase from the sample mean (0.0330) and is significant at the 5% level. The estimated coefficients reported in columns (2) through (5) exhibit magnitudes and significance levels similar to those reported in column (1).

Robustness In the online Appendix, We show that our baseline estimates are robust to alternative modeling choices regarding the sample selection, outcome measures, and standard error adjustments. First, we present the estimated coefficients from different sampling methods in Appendix Table A2. Panels A through E present estimates using 20-, 100-, and 200-year windows as well as the full 262-year window. In each panel, we report the estimated treatment effects for samples consisting of: counties in canal prefectures (column 1), counties within 100km, 150km, and 200km from the canal (columns 2–4), and all baseline counties (column 5). We also confirm that our estimates are robust to aggregating the number of rebellions at the prefecture-level (column 6).

Second, we show in Appendix Table A3 that our estimates do not depend on how we construct the outcome variable. For panel A, we normalize the number of rebellions by population in 1820, the latest pre-reform year for which population is available. The estimated coefficients are smaller than those in the baseline — which mechanically reflects the population growth over the two centuries — but all are significant at similar levels. For panel B, we normalize the number of rebellions by a yearly population imputed using linear interpolation. For panel C, we normalize the number of rebellions by land area instead of population. For panel D, we use the unnormalized number of rebellions as the dependent variable. For panel E, we apply the same normalization method as in the baseline but forgo the arcsinh transformation. Our estimates are robust to all these alternative specifications.

Finally, in Appendix Figure A2, we experiment with the Conley standard errors using a range of bandwidths for distance (50, 100, 200, 500, 1000, 2000km) and time (20, 100, 200, 262 years) cutoffs and plot the t-statistics derived from each of those combinations. The specifications in the four panels correspond to the five columns in Table 3. We see that the t-statistics are consistent and significant at conventional levels regardless of how we adjust

[8] The estimations with high dimensional fixed effects are implemented using the Stata package REGHDFE, which is developed by Correia (2014). The Stata package for implementing Conley standard errors is provided by Hsiang (2010)

for standard errors. It is worth pointing out that the cutoffs we report as the baseline produce the most conservative set of Conley standard errors.

C Treatment Intensities

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 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. These restrictions are perhaps arbitrary. On the one hand, some canal counties may rely more heavily on the canal and therefore hit more severely by its abandonment. On the other hand, the impact of the canal's closure may spill over to nearby non-canal counties due to decreased market access. This section lifts the restriction of binary treatment and allows more flexible treatment intensity measures along various dimensions.

Intensities among canal counties We start by exploring how the treatment intensity may differ among canal counties. We consider two intensity measures: a geographic measure and an economic measure. The geographic measure is defined as the length of the portion of the canal contained within the county normalized by the county's land size. It captures the extent to which the canal matters geographically for the county. The economic measure is defined as the share of 1820 towns (serving as local marketplaces) within 10 kilometers from the canal.[9] It captures the extent to which the county's economy depended on the canal's functioning on the eve of the closure. We expect counties that depend more on the canal (either geographically or economically) to be more severely affected by its abandonment.

We confirm these two hypotheses by estimating the following equations:

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

$$Y_{ct} = \sum_{k=0}^{K} \beta_k IntensityGroup_c^k \times Post_t + \delta_c + \sigma_t + \chi + \varepsilon_{ct} \tag{5}$$

where Equation (4) assumes a linear function of treatment intensity (measured either way) while Equation (5) uses a more flexible specification to estimate a separate coefficient for each intensity group (indexed by k). For both intensity measures, we expect the coefficient β estimated from Equation (4) to be positive and the coefficients β_ks estimated from Equation (5) to be larger for more intensively affected groups.

[9] The results are robust to using alternative bandwidths, e.g., 5km or 20km. We thank the anonymous referees for suggesting this measure.

We estimate Equation (4) for both intensity measures and report the results in the first two columns of Table 4. Column (1) estimates rebelliousness as a linear function of canal length per $100km^2$ (the geographic intensity). Column (2) estimates rebelliousness as a linear function of within-$10km$ town share (the economic intensity). The results confirm our hypotheses that the reform's impact increases with the extent to which the county depends on the canal (both geographically and economically).

To further illustrate these results, Figure A3 plots, for both intensity measures, the estimate from Equation (5). Panel (a) of Figure A3 shows the flexible estimates of our geographic measure of treatment intensity, for which we group canal counties by the length of the canal they contain (normalized by land size). The reference group consists of counties away from the canal. We find that counties containing less than $2km$ per $100km^2$ ($32km$ for a county of average size) do not appear more rebellious than non-canal counties. Overall, the figure suggests larger effects in counties relying more on the canal geographically, though the relationship is not monotonic.

Turning to the economic measure of treatment intensity, panel (b) of Figure A3 plots the flexible estimates from Equation (5) for which we group canal counties by the share of towns within $10km$ of the canal. The reference group is the set of counties away from the canal. The figure shows that the impact of the canal's closure increases proportionally to the share of canal towns of the county. In particular, canal counties with dispersed town distribution do not appear more rebellious than non-canal counties. Taken together, Figure A3 suggests that the canal's abandonment matters primarily for canal counties geographically or economically dependent on the functioning of the canal.

Spillover to non-canal counties The canal's influence is not necessarily restricted to canal counties. It could naturally spill over to non-canal counties through the market access channel (Donaldson and Hornbeck, 2016). Therefore, the canal's abandonment might have also distorted non-canal counties, and the effect should depend on the county's distance from the canal. To verify this hypothesis, we exploit variations in treatment intensity as a function of distance from the canal:

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

$$Y_{ct} = \sum_{k=0}^{K} \beta_k Distance_c^k \times Post_t + \delta_c + \sigma_t + \chi + \varepsilon_{ct} \tag{7}$$

where we multiply $Post_t$ with $Distance_c$, the distance to the canal, and each of the 25-km distance intervals, $Distance_c^k$, respectively. The estimated coefficient β from Equation

(6) represents the linear change in rebelliousness as a function of distance. We expect it to have a negative sign since farther-away counties should be less responsive. Equation (7) estimates the treatment effects for each of the 25-km distance intervals, for which the reference group consists of counties more than 400km away from the canal. We expect the estimated coefficients β_ks to be decreasing with distances.

We present the estimated coefficients from Equation (6) in the last column of Table 4. The result confirms our hypothesis that the effect of the canal's abandonment on rebelliousness decreases with a county's distance from the canal and is significant at the 1% level. Figure A4 plots the estimates from Equation (7) for each of the 25km distance intervals. As expected, we find the estimated coefficients decreasing with distances from the canal. The figure is also informative about the range of the canal's impact: counties more than 150km away from the canal do not appear more rebellious than counties farther away. Therefore, we consider 150km as the spatial range of the canal's impact.

Taken together, the estimates from more flexible treatment measures are consistent with our baseline findings from a binary treatment variable. They suggest that counties more dependent on the canal (either geographically or economically) were hit most severely by its closure, and the effects might have spilled over to a range of 150km.

IV Addressing Additional Concerns

In the previous section, we provide empirical evidence that there were more rebellions in the areas close to the canal in the years after 1826. While we have verified that there were no differential trends in rebellions between canal counties and non-canal counties before the reform, additional caution should be taken when interpreting the increases in rebellions as caused by the loss of the canal. The major threat to this causal interpretation resides in the fact that China entered a rather turbulent period in the mid-19th century. Thus, one may be concerned about other national-level events differentially affecting canal counties if canal counties happened to be more susceptible to rebellions during an age of increasing unrest.

This section delves deeper into this broad concern about differential reactions to common events, focusing on the most worrisome and challenging scenario in which the differential reactions were caused by unknown or unobservable characteristics pertaining to the canal.[10] We adopt three independent approaches to diagnosing the extent to which this scenario might have distorted our results. First, we apply two alternative estimation techniques that accommodate the possibility of latent differences between the treated and the control groups:

[10] We would like to deliver our deepest gratitude to the coeditor, Esther Duflo, and two anonymous referees for pushing us in this direction, and for their helpful suggestions and guidance for addressing the issue.

change-in-change (Athey and Imbens, 2006) and synthetic control (Abadie and Gardeazabal, 2003; Abadie, Diamond and Hainmueller, 2010). Second, we consider placebo treatments to counties that might be inherently similar to canal counties but did not actually experience a loss of the canal. Finally, we devote particular attention to two major distortions in mid-19th century China that might coincide with the advancement of the reform — the Opium War and the Taiping Rebellion — and examine the extent to which they might have affected canal counties differentially.

A Alternative Estimation Techniques

Our standard difference-in-differences estimation requires the average treatment effect to be constant across all observations. One potential violation of this homogeneous treatment effect assumption is that areas prone to rebellions before the intervention might be more susceptible to other national distortions in the post-reform period, thus causing a plausibly differential reaction to some common shocks. Our baseline estimation attempts to alleviate this concern within the difference-in-differences framework by controlling for the flexible time effects of pre-treatment rebelliousness. This section extends the methodology by employing two alternative estimation procedures that econometrically account for systematic differences between the treated and the control groups.

Changes-in-changes The first technique we employ is the changes-in-changes (CIC) method developed by Athey and Imbens (2006), which generalizes the standard difference-in-differences model. Instead of comparing the mean outcome in the treated and the control groups, the CIC estimator recovers the whole distribution of the counterfactual outcome and non-parametrically estimates the quantile treatment effects on the entire distribution by comparing the counterfactual distribution to the actual second-period distribution for the treated group. The method does not make assumptions on the functional form of the outcome distribution. Therefore, it allows the two groups to differ in the distribution of unobservable characteristics in arbitrary ways as long as the distribution is time-invariant within each group. In particular, it accommodates the possibility that the treatment effect might differ systematically across groups.

We follow the two-step procedure suggested by Athey and Imbens (2006) to estimate the quantile treatment effects on the distribution. The first step uses the parametric ordinary least square to partial out the effects of the covariates. In the second step, we apply the unconditional CIC estimator to the residuals to estimate a quantile treatment effect.[11] The

[11]We implement the estimation using the Stata package provided by Melly and Santangelo (2015).

effect at each quantile is estimated by comparing the changes of the treated distribution at that quantile to changes of the control distribution at a possibly different quantile that takes the same pre-treatment outcome value. Thus, the treatment effect at each quantile is estimated from observations with comparable pre-treatment outcome values.

We display the estimated quantile treatment effect and the bootstrapped standard errors in Appendix Figure A5. Two important patterns emerge. First, we find that the canal's abandonment has a significant positive effect on the entire distribution. This finding is consistent with our standard differences-in-differences estimation results in the baseline. Second, we notice that the effect is mostly decreasing in the quantile level. In particular, the increase in rebellions is significantly larger at the left tail of the conditional distribution, such that most of the changes between the two groups come from counties that were peaceful in the pre-reform period. This pattern lays to rest any concerns that certain areas might be prone to rebellions in both periods, in which case we would have expected larger increases in inherently unstable counties at the right tail of the distribution.

Synthetic control method The second approach we employ is the synthetic control method (SCM) proposed in Abadie and Gardeazabal (2003) and Abadie, Diamond and Hainmueller (2010). Unlike the CIC estimator, which allows the distribution of unobservables to differ across groups in arbitrary ways, the SCM deals with the comparability problem by constructing a synthetic control group that closely matches the treated group. Specifically, it applies a data-driven approach to search for a weighted combination of control units that approximates the evolution of the outcome variable for the treated group before the intervention. The evolution of the outcome variable for the resulting synthetic control group is thus considered a projection of the counterfactual of what would have been observed for the affected unit in the absence of the intervention.

The SCM was originally developed for comparative case studies with one single treated unit. We follow the procedure suggested by Cavallo et al. (2013) to extend it for our context with multiple treatment units. Specifically, we construct a synthetic match for each canal county using a weighted combination of non-canal counties in the donor pool following the standard SCM approach. [12] We repeat this procedure for all canal counties and aggregate the individual estimates to compute the average treatment effect. In doing so, we compare every canal county to a synthetic counterfactual that matches the evolution of its first-period outcome in calculating the average effect. We follow the randomization inference procedure to compute the p-values by comparing the estimated main effect to the distribution of placebo

[12] The SCM requires that units in the donor pool should not be affected by the treatment (Abadie, 2021). However, we have found that the canal might have a spillover effect to counties up to 150km away (see Figure A4). Therefore, we restrict the donor pool to non-canal counties at least 150km away from the canal.

effects. (See Cavallo et al. (2013) and Galiani and Quistorff (2017) for the technical details.)

We plot the estimated results in Figure A6. Panel (a) depicts the evolution of rebelliousness for the treated and the synthetic control groups. Panel (b) shows the differences between the two groups (the treatment effects) over time, along with the p-values from the randomization inference. The pattern revealed by the synthetic control is consistent with our previous findings that canal counties started to have more rebellions following the canal's abandonment in 1826. It is worth noting that we see a surge in rebellions in the synthetic control group approximately 30 years after the 1826 closure. This increase coincides with a politically unstable period for the country in the mid-19th century. It is opposite to what would have been observed if the scenario of canal counties being more susceptible to overall unrest was factual.

Taken together, Both techniques represent generalizations of the standard difference-in-differences methodology by flexibly allowing for systematic differences between the treated and the control units. The results consistently show that our findings are not subject to the potential concern of comparability between canal counties and non-canal counties. Furthermore, both exercises reveal additional patterns suggesting it unlikely that our results are driven by a common event differentially affecting canal counties previously prone to distortions.

B Placebo Tests

Another related concern is that our results might reflect the effect of the canal's historical presence rather than its abandonment. This concern arises because canal counties may differ from non-canal counties in many attributes. While we have controlled for the period-specific impact of a large set of covariates in the baseline, one might still worry about the possibility of other perhaps unknown or unobservable differences associated with the canal's historical presence. In this section, we attempt to tackle this issue by implementing two placebo designs. The idea is to approximate the hypothetical treatment effects on canal counties had the canal continued to operate.

Our first exercise leverages the historical context that not all portions of the canal were abandoned to the same extent. In particular, the part of the canal north of the Yellow River suffered more severely from the reform as it became silted and almost unnavigable due to the lack of consistent maintenance. In particular, much of the maintenance effort was focused on cleaning the silt and sendiments flushed into the canal from the Yellow River. continued to serve in transporting Jiangsu's and Zhejiang's tribute rice to Shanghai, the starting point of sea transportation. On the other hand, the part of the canal south of the Yellow River continued to function relatively well for two reasons: first, it continued to serve in transporting Jiangsu and Zhejiang's tribute rice to Shanghai, the starting point of sea transportation; second, there was far less maintainance effort required to sustain the navigability of the canal in the south. Therefore, while the historical presence of the canal was held constant across the two sections, the southern part of it suffered minimally from its closure. This fact makes the southern part of the canal a potentially suitable placebo group that could be used to separate the effect of the canal's abandonment from other confounding factors associated with the historical presence of the canal. If our findings reflect the canal's historical presence rather than its closure, we would expect a similar response in both portions of the canal. If, on the contrary, it is the abandonment of the canal that really mattered, we would expect a minimal effect in its southern part.

To examine the differential effects of canal closure in the north and the south, we introduce a triple interaction term, $AlongCanal_c \times Post_t \times North_c$, where $North_c$ is an indicator that takes the value of one if the county is located to the north of the Yellow River. The dual interaction term, $AlongCanal_c \times Post_t$, thus captures the effect of canal closure in the south, whereas the triple interaction term estimates the difference in treatment effects between the north and the south. The total treatment effects for canal counties in the north can be calculated by adding up the coefficients for the two terms. We have also included the interaction between the post-reform dummy with an indicator of northern counties, $North_c \times Post_t$, which captures the average difference between counties to the north and the south of the Yellow River, so that we are comparing counties along the northern (southern) part of the canal only to other counties in the north (south).

Table 5 presents the results. A few observations are worth noting. First, we find that the distortion effects of the canal's abandonment come exclusively from its northern part. For the southern part, the effect is close to zero, and in some specifications is even negative. The differences between the two coefficients are significant at the 1% level across all specifications. Second, the estimated coefficient for $North_c \times Post_t$ is not significantly different from zero in our most comprehensive specifications, suggesting that non-canal counties in the north and those in the south are not statistically different from each other. This result helps to rule out any alternative explanations referring to regional differences between North and the South (e.g., climate, geography, etc.). Overall, the findings support the role of the canal's abandonment over that of its historical presence.

In the second exercise, we examine more broadly the placebo treatment effects on counties along other rivers and transportation arteries. We consider four placebo designs: the Yangtze River, the Yellow River, the coast, and the courier routes. These groups are chosen in such a way that each of them played a similar role to the canal in terms of facilitating transportation and regional trade. If there are any unobservable characteristics associated with the canal's historical presence (rather than its abandonment) that explain our findings, we should expect a comparable effect along these alternative routes as they likely possess similar influences on their neighboring regions to the canal's historical presence.

We summarize the placebo treatment effects along these alternative transportation routes in Table 6. The first four columns examine each placebo treatment group separately, whereas the last column examines a summary measure of belonging to any of the four placebo treatment groups. We do not find that any of these placebo treatments generate a significant increase in rebellions in the post-reform period. Therefore, the possibility of differential reactions due to the historical presence of the canal is a relatively unlikely scenario.

We view the two placebo exercises in Table 5 and Table 6 as complementary to each other. They collectively depict a comprehensive picture that, among the main transportation networks in the region we study, the distortionary effects only appear to emerge along the northern part of the canal — the portion that was completely abandoned after the reform. The pattern suggests that it was the canal's abandonment that really mattered.

C Major Historical Events

The two approaches we have applied provide extensive evidence that our findings unlikely arise from a common event differentially affecting canal counties. Yet, given the reform's gradual process, one might be curious about how the canal's abandonment interacted with the many other distortionary events in mid-19^{th} century China. To this end, we directly examine two significant events that occurred during the process of the reform: the First Opium War (1840–1842) and the Taiping Rebellion (1851–1864). We discuss in detail how these events might affect our estimates for the canal's closure.

It is worth first pointing out that both events started beyond our sample area but moved toward it in their later stages. The First Opium War between Britain and China 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 Taiping army 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 skewed our results in several ways. The immediate concern is that our measure of rebellions might capture simply the campaigns of the British and Taiping armies. This is unlikely for three reasons. First, we have restricted our analysis to rebellions known to have broken out locally and excluded the actions of existing rebel groups (see Appendix C for details regarding how we distinguish 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. [13] Our results are driven primarily by counties in the north (as shown in Table 5), 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 A2, 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. The results are reported in Panel A, Table 7. For columns (1), we exclude counties where battles between the British Army and the Qing government took place (i.e., the Battles of Zhapu, Zhenhai, Zhenjiang, Ningbo, Cixi, and Wusong). 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 column (2), however, the results we report include estimated coefficients that are even larger when we exclude the Taiping-affected regions (defined as places occupied by the Taiping group or those in which the battles took place). Consequently, our results are not subject to inaccurate information collected in the occupied regions.

[13] The British Army's campaign ended after it occupied the city of Zhenjiang (where the canal and the Yangtze River intersect), which forced the Chinese 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 defeated by the Qing imperial forces in 1855.

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 7. The result reported in columns (3) indicates that the Opium War produced little interaction with our estimates, which is consistent with our previous analysis. For the Taiping Rebellion (columns (4)), 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 typically found in histories of China (e.g., Martin, 1996). On top of everything, we do not find evidence that canal counties reacted more violently to these national distortions.

Remark In this section, we delve into the central concern of differential reactions to common events (during a rather unstable historical period). The concern arises from the fact that unmodeled national events may have occurred during the reform's gradual process and that canal and non-canal counties seem to differ in some aspects. We apply three independent approaches to address this inherently challenging problem. First, we show that our findings survive the alternative estimation techniques that allow for systematic differences between the treated and the control groups. Second, we explore the placebo treatment effects along other transportation routes that might have similar influences as the canal's historical presence but did not suffer from its abandonment. We find no effects in these placebo treatments. Finally, we directly examine two main events that distorted the country in the mid-19th century and do not find that canal counties reacted more violently to either of them. Taken together, these findings give us sufficient confidence to interpret the increased rebellions in canal counties as a causal effect of the loss of the canal.

V Discussion of Mechanisms

The previous sections provide plausibly causal evidence that associates the abandonment of the Grand Canal with subsequent rebellions in counties close to the canal. This section discusses the potential mechanisms through which the canal closure impeded political stability. We consider two alternative explanations of our findings: reduction in state repressive capacity and loss of access to trade. We use a variety of approaches to evaluate these two mechanisms and find the evidence most consistent with the loss of trade interpretation. We further delve into the specific aspect of income shocks induced by the loss of trade and discuss its long-term implications.[14]

A State Repressive Capacity

One possible channel through which the canal's abandonment could have led to more rebellions is the weakening of the government's repressive capacity (or effort) as the canal loses its significance. This change 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 state's repressive capacity.

The first approach explores additional variations in the importance of political control before the canal's closure. If the canal's closure weakens the government's repressive capacity along the canal, we would expect the effect to be more substantial in counties that require more intensive controls. We employ two measures of such political importance. The first measure (denoted as $Soldiers$) considers the size of the pre-assigned military establishment in the 1750s(Luo, 1984) — a direct proxy for the repressive capacity as well as the government's perception of the county's military significance. The second measure (denoted as $PrefectureCapital$) is whether a county served as the prefecture-level administrative center — i.e., the administration that operated at a higher level on the political hierarchy. We interact our treatment intensity variable, measured by the length of canal per 100 km^2, with each of these measures to investigate whether the effects are particularly intensified in counties that were deemed important to the government regarding its repressive force during the canal-transportation era.

The results are reported in the first two columns of Table A4. 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). These estimates suggests that repressive capacity may play a limited, if any, role in boosting the subsequent rebellions.

[14]For the analyses in this section, We measure the canal's impact by the length of canal (per 100 square kilometers), which provides more variations in treatment intensity.

Our second approach is to examine two placebo outcomes that measure the military actions of existing rebel groups. Specifically, we extract records in *Qing Shilu* that document events in which an existing rebel group attacked or retreated (often after being defeated by the government) into a county. These military decisions should be responsive to changes in state capacity. If the canal's closure reduced the state's repressive capacity along the canal, we would expect the rebels to increasingly view canal regions as easier targets or safer retreats and attack or flee into these places more often.

We summarize the results in the next two columns of Table A4. Column (3) examines the effects of the canal's closure (using the intensity measure) on the frequency at which a county was attacked by existing rebel groups. Contrary to the declined state capacity interpretation, we do not find an increase in canal county's being attacked. Rather, the effect is negative and significant at the 5% level. This negative effect might reflect a decreased reward for occupying a canal county after the reform. Column (4) examines whether the rebels tended to retreat or flee into a canal county, for which decision military security was the top concern. We find no evidence that canal counties became safer retreats for the rebels. Taken together, the evidence does not support the potential drop in state repressive capacity along the canal as a major channel that explains the increased rebellions after the reform.

B Trade Access

Our preferred interpretation of the association between the loss of the canal and subsequent rebellions is that the reform deprived regions with direct access to this established trade route of that access. In previous analyses, we have seen several pieces of evidence consistent with this interpretation: (*i*) the effects are proportional to the share of market towns distributed directly next to the canal; (*ii*) the effects are not constrained by county borders and may spill over, possibly as a result of decreased market access, to a range of $150km$ away; (*iii*) most of the effects come from the northern part of the canal for which the access to trade was completely disrupted following the reform, whereas the southern part of it for which the access to trade was less disrupted did not appear to become more rebellious; (*iv*) we do not observe similar effects along other major trade routes in the region under study that did not experience a closure.

Here we lay out three additional pieces of evidence in support of this loss of trade access interpretation. The first exercise directly examine the development of market towns for canal counties and non-canal counties. Specifically, we consider the number of market towns in each county observed for 1820 and 1911 and regress this figure on the canal's intensity (length per 100 km^2). The estimated coefficient are reported in the first column of Table A5. The result suggests that the canal's closure significantly hampered the development of market towns along the canal over the nineteenth century, which directly indicates the reform's disruptive effect on regional trade.

We next 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 canal's intensity with a county's access to this alternative route of transportation. The results are presented in Column (2) of Table A5. 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 earlier results. 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. We interpret this result as supportive evidence for the channel of disrupted trade-route access.

Finally, we examine the canal's role in mitigating the risks of conflict from climate shocks, a role that trade usually plays (Burgess and Donaldson, 2010). Specifically, we interact the canal's intensity with a dummy variable indicating the years with temperature anomaly. The result is reported in the third column of Table A5. The interaction term, $Canal \times TemperatureAnomaly$, represents the differential effects of a climate shock on social conflict for canal counties and non-canal counties when the canal was functioning well. We find a mitigation effect of the canal on weather-induced social conflict, a finding consistent with the role that trade often plays according to Burgess and Donaldson (2010). The coefficient on the triple interaction, $Canal \times TemperatureAnomaly \times Post$, suggests, however, that the mitigation effect no longer existed after the canal's closure. This piece of evidence, though mostly indirect, is suggestive that the canal's closure might have contributed to subsequent rebellions by disrupting trade access that the canal once facilitated.

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.

C Further Discussion

In previous sections, we have provided substantial evidence that disrupted trade access might serve as the main channel through which the canal's closure triggered rebellions. In this section, we attempt to go one step further to investigate the types of income shocks induced by the loss of trade. We consider two possible channels. First, the loss of trade access might increase transaction cost of agricultural products, leading to the welfare loss of consumers and (or) producers. Second, the canal's closure might hit urban workers employed in trade-related jobs (sailors, dockworkers, traders, etc.).

We examine the first channel through two lenses. First, we examine changes in grain prices in response to the canal closure. If the closure increased the transaction cost of agricultural products, we would expect an increase in grain prices. [15] For the first column of Table A6 we regress grain price on the abandonment of the canal. The coefficient is positive, but is nonsignificant at conventional levels. This finding suggests that the transaction of agricultural goods does not seem to be the main channel.

The second approach is to investigate whether the impact of the canal's closure varies across regions where suitability to crop plantations varies. If the transaction of agricultural products 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 A6. While the main effects of the closure remain significant, we find no heterogeneous effects across levels of crop suitability. Therefore, the potential channel of agricultural production and transaction is not supported by the data.

We then turn to examining the second channel that the closure increased rebellions by unemployed workers in urban settings. It is difficult to disentangle the income loss of urban workers from other aspects of trade because these factors often move together in most regions. Our approach is to find an area in which the extent of income loss was minimal despite the loss of trade aceess due to the canal closure. We consider the areas around the intersection of the Grand Canal and the Yangtze River as an ideal subsample for this purpose. The reasons are twofolds. First, sailors and dockworkers require specific types of human capital, and the switching cost might be high if they wanted to find other jobs once canal trade declined. Second, the Yangtze River should not gain additional trade after the canal closure, because the river runs in a different direction from the canal and serves different regions. Areas close to the intersection between the two waterways thus constitute an subsample in

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

which the loss of canal introduced a large trade shock with a minimal impact on the income of dockworkers. Figure A7 plots the estimated coefficients using subsamples 100, 200, 300, and 400km away from the Yangtze River. We find that the point estimates increase with the distance to the Yangtze River (despite the wide confidence intervals that prevent us from making any conclusive inference). We consider this result as suggestive evidence that urban unemployed workers might be the major group who suffered and rebelled.

While our approach is tentative and experimental, the findings are consistent with historical narratives that sailors' and dockworkers' associations around the canal developed into gangsters and secret societies as the canal economy declines.[16] These organizations were often tightly organized and eventually became hotbeds for recurring and persistent turmoil over a long period. Historians have described the potential association between the canal's closure and the emergence of organized gangs and violent events over several decades (e.g., Martin, 1996; Liu, 2007). This historical narrative is also consistent with other studies (quantitative or qualitative) on urban trade shocks and organized crimes in distinct settings, including Dell, Feigenberg and Teshima (2019) for Mexico, Haller (1971) for Chicage, and Critchley (2008) for New York City.

We further explore this idea using a cross-sectional exercise. Specifically, we exploit 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 Table A7, demonstrate a high concentration of these early gang members around the canal. The result, 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.

VI 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 were

[16] One prominent example is Cao Bang, an official labor union once approved by the government, which controlled tens of thousands of grain junks and over $50,000$ registered members by the late 18^{th} century. It was also recognized as the main precursor of the Green Gang following the decline of the canal economy.

geographically and economically more dependent on the canal's well-functioning. Our work emphasizes the importance of continued access to trade routes in promoting peace, a classic notion 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. Our work highlights the loss of socioeconomic opportunities as a leading force in promoting the persistent and recurring insurrections that plagued the end of Imperial China.

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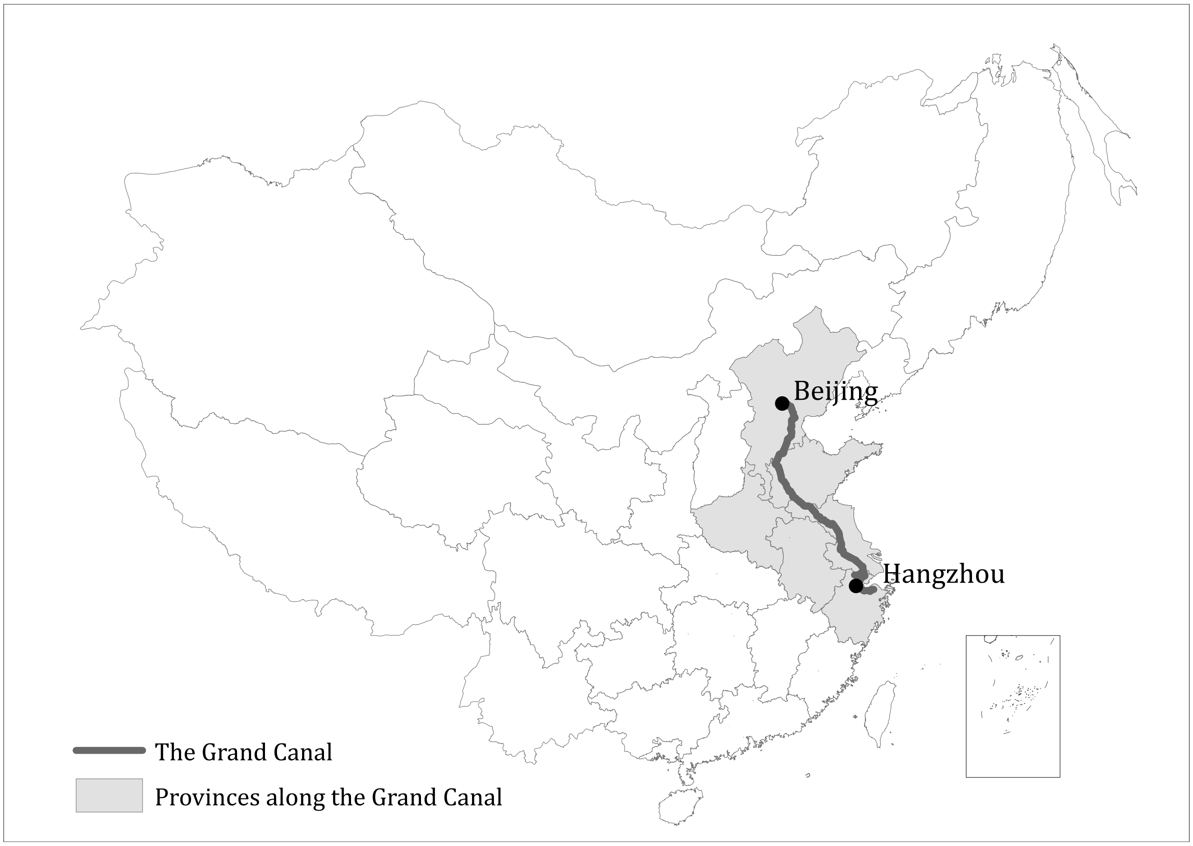
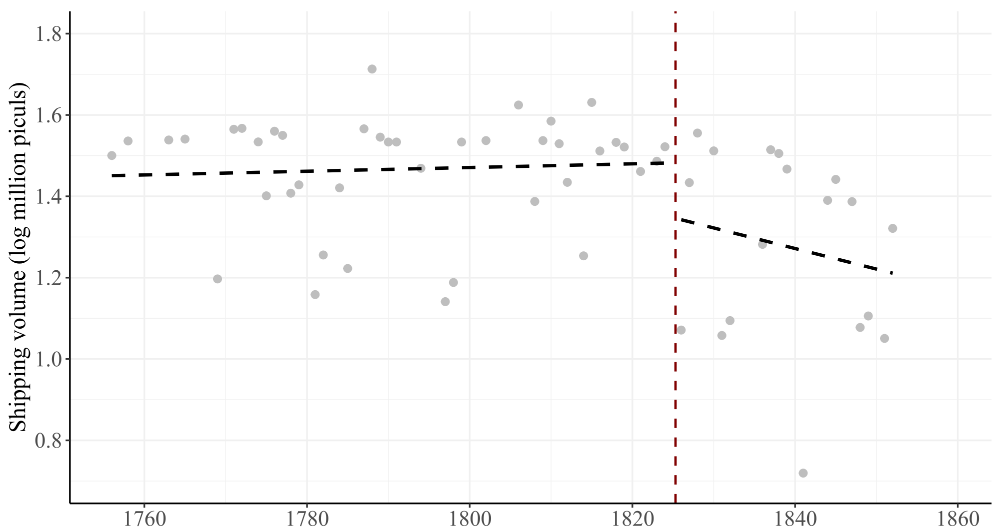
Figures and Tables

Figure 1: Location of the Grand Canal



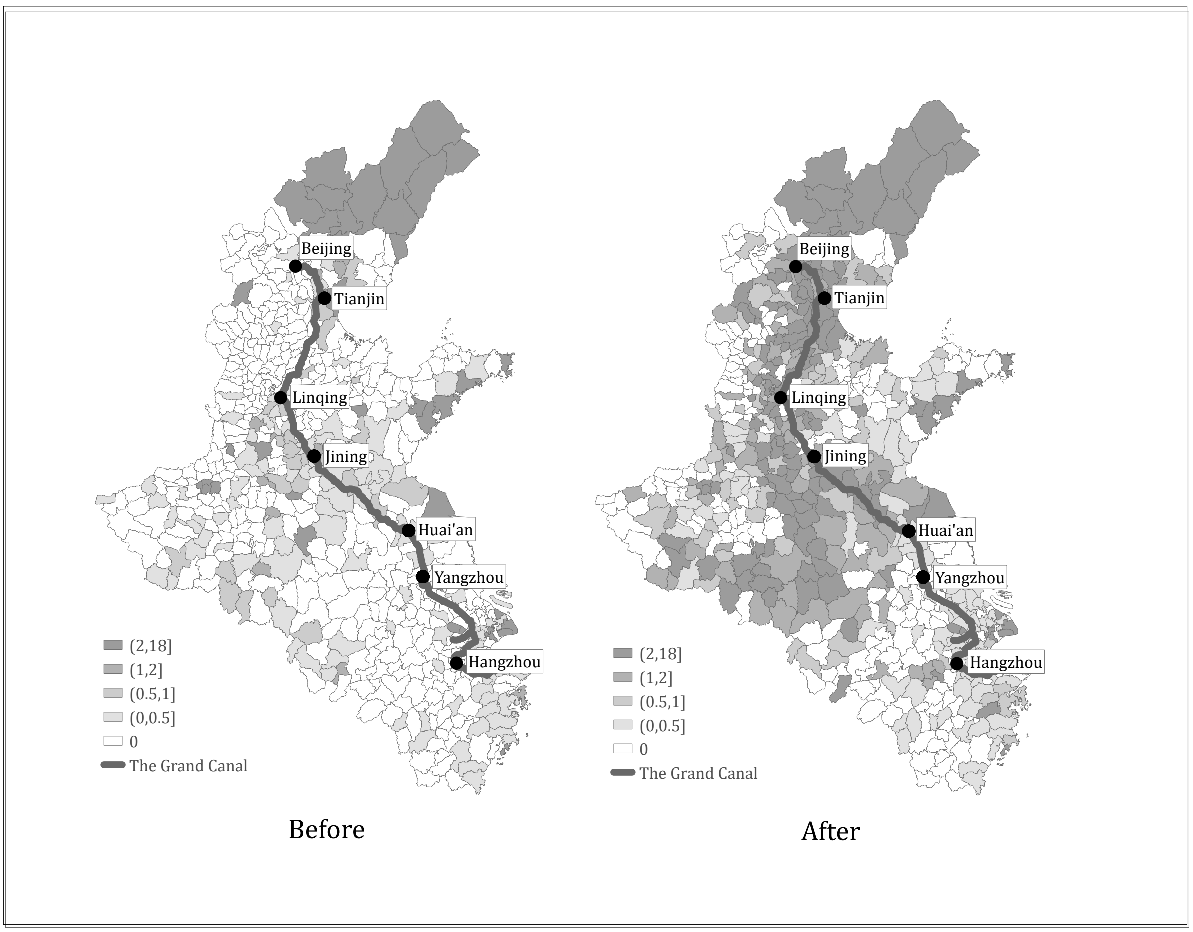
Note. The figure depicts the location of the Grand Canal and the six provinces adjacent to it, which is the sample region for our study.

Figure 2: Canal usage measured by tribute rice transportation



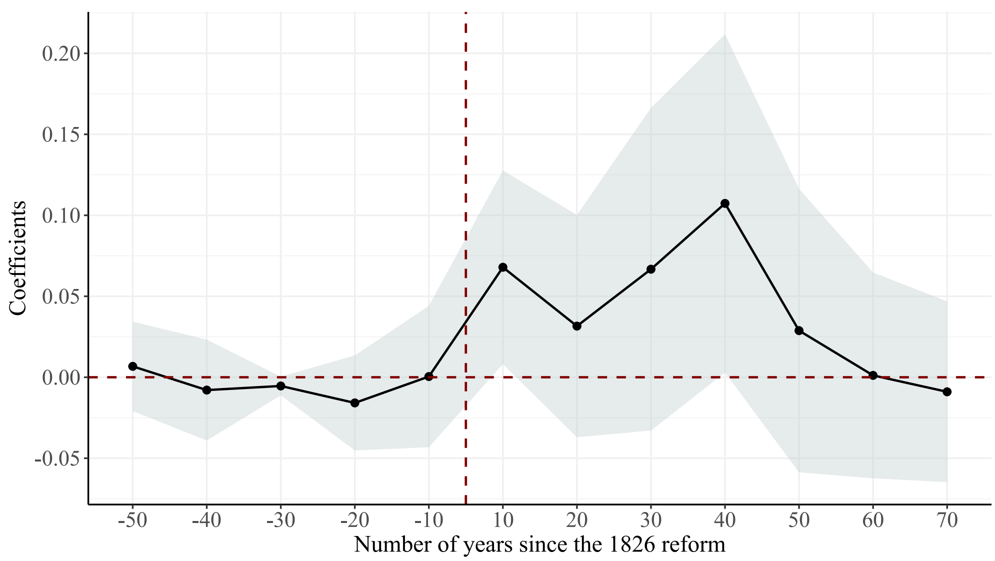
Note. The figure depicts the amount of tribute grains transported via the canal between 1755 and 1860, calculated based on information from Li and Jiang (2008), pages 38–42. The vertical solid line marks the 1826 sea-shipping experiment. The fitted lines depict the linear trend of grain shipping before and after that date.

Figure 3: The spatial distribution of rebellions before and after the abandonment



Note. The figure depicts the spatial distribution of rebellions before and after 1826.

Figure 4: Canal closure and rebellions: event study



Note. The figure depicts the differences in rebellions between canal versus non-canal counties before and after the 1826 reform. The markers and capped spikes represent the OLS estimators and 95% confidence intervals based on standard errors clustered at the county level. The dependent variable is the inverse hyperbolic sine transformation of the number of rebellions normalized by 1600 population. The dashed vertical line represents the 1826 treatment date, and the periods are grouped every 10 years relative to 1826 (i.e., represents the 1816–1825 period, represents the 1826–1835 period, etc..) The reference groups are the years more than 50 years before 1826. The regression considers county fixed effects, year fixed effects, pre-treatment rebelliousness × year fixed effects, and province × year fixed effects.

Table 1: Data sources and summary statistics

	Source	Obs.	Mean	S.D
Outcome				
Number of rebellions per million population	1	140,432	0.4604	7.1172
Treatments				
Being Along the Grand Canal	2	150,650	0.1270	0.3329
Length of canal per 100 km^2	2	150,650	0.4065	1.3768
Share of towns within 10km of the canal	2	138,598	0.0684	0.2105
Distance from the Grand Canal (km)	2	150,650	118.0353	113.3476
Controls				
AREA	2	150,650	1,622.2486	1,528.0102
Ruggedness Index	5	150,650	72.7510	97.5940
Temperature Anomaly	3	150,650	0.3459	0.4757
Flood	4	150,650	0.0743	0.2623
Drought	4	150,650	0.0976	0.2968
Population density	6	145,934	54.0430	167.1533
Year of Maize Adoption	4	147,506	1,718.4050	95.5303
Year of Sweet Potato Adoption	4	60,522	1,755.0130	51.1259
Suitable for wetland rice	8	150,650	0.0661	0.2484
Suitable for wheat	8	150,650	0.4139	0.4925
Supplements				
Imperial Soldiers Stationed	7	150,650	154.2104	345.5507
Prefecture Capital	2	150,650	0.1391	0.3461
Number of Attacking Cases	1	150,650	0.0054	0.0822
Number of Retreating Cases	1	150,650	0.0036	0.0690
Number of Towns and Local Markets	2	1,150	12.4957	10.7650
Along the Qing Courier Routes	2	150,650	0.2800	0.4490
Average Grain Price (Liang/KCal)	4	91,110	0.5980	0.1856
Senior Green Gang Members	9	150,650	0.2435	1.0582

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. Liang (1980)
7. Luo (1984)
8. FAO (2012), GAEZ: http://gaez.fao.org/Main.html#
9. Encyclopedia of the Green Gang (*Qingbang Tongcao Huihai*)

Table 2: Canal closure and rebellions: pre-treatment trends

	Dependent Variable: Rebellions			
	(1)	(2)	(3)	(4)
$AlongCanal \times Year$	0.0003	-0.0000	-0.0002	-0.0003
	(0.0006)	(0.0006)	(0.0006)	(0.0007)
	[0.0007]	[0.0007]	[0.0007]	[0.0008]
Constant	-0.0523	0.0255	0.0753	0.0804
	(0.1400)	(0.1372)	(0.1522)	(0.1689)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Pre-reform rebellion × Year FE	No	Yes	Yes	Yes
Province × Year FE	No	No	Yes	Yes
Prefecture Year Trend	No	No	No	Yes
Mean of the Dependent Variable	0.0198	0.0198	0.0198	0.0198
No. of Observations	26,800	26,800	26,800	26,800
No. of Counties	536	536	536	536
Adjusted R-squared	0.0133	0.0355	0.0473	0.0495

*Note.*The sample consists of 536 counties in the six provinces around the canal from 1776 to 1825. The dependent variable is the inverse hyperbolic sine transformation of the number of rebellions normalized by 1600 population. *AlongCanal* is an indicator that equals one if the county is adjacent to the canal. *Post* is an indicator that equals one in and after 1826. Standard errors in parentheses are clustered at the county level. Standard errors in square brackets are Conley standard errors robust for spatial correlation, assuming a cut-off window of 500 km and a serial correlation of 262 years.

Table 3: Canal closure and rebellions: baseline estimates

	Dependent Variable: Rebellions				
	(1)	(2)	(3)	(4)	(5)
Along Canal × Post	0.0380	0.0369	0.0453	0.0427	0.0340
	(0.0166)	(0.0172)	(0.0173)	(0.0172)	(0.0166)
	[0.0165]	[0.0167]	[0.0167]	[0.0168]	[0.0168]
Constant	0.0313	0.0314	0.0310	0.0311	-0.0268
	(0.0007)	(0.0007)	(0.0008)	(0.0007)	(0.0195)
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Pre-reform rebellion × Year FE	No	Yes	Yes	Yes	Yes
Province × Year FE	No	No	Yes	Yes	Yes
Prefecture Year Trend	No	No	No	Yes	Yes
Controls × Post	No	No	No	No	Yes
Mean of the Dependent Variable	0.0330	0.0330	0.0330	0.0330	0.0330
No. of Observations	140,432	140,432	140,432	140,432	140,432
No. of Counties	536	536	536	536	536
Adjusted R-squared	0.0253	0.0322	0.0471	0.0497	0.0509

Note. The sample consists of 536 counties in the six provinces around the canal from 1650 to 1911. The dependent variable is the inverse hyperbolic sine transformation of the number of rebellions normalized by 1600 population. *AlongCanal* is an indicator that equals one if the county is adjacent to the canal. *Post* is an indicator that equals one in and after 1826. The control variables include: land size (ln), terrain ruggedness, an indicator of temperature anomaly, an indicator of drought, an indicator of flooding, population density in 1600, an indicator of maize adoption, an indicator of sweetpotato adoption, an indicator of being suitable for wheat, and an indicator of being suitable for wetland rice. Standard errors in parentheses are clustered at the county level. Standard errors in square brackets are Conley standard errors robust for spatial correlation, assuming a cut-off window of 500 km and a serial correlation of 262 years.

Table 4: Canal closure and rebellions: treatment intensities

	Dependent variable: Rebellions		
	(1)	(2)	(3)
Canal length (per $100km^2$) × Post	0.0200		
	(0.0104)		
	[0.0094]		
Canal town share × Post		0.0770	
		(0.0292)	
		[0.0269]	
Distance to canal × Post			-0.0142
			(0.0038)
			[0.0046]
Constant	0.0315	0.0315	0.0561
	(0.0007)	(0.0007)	(0.0063)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Pre-reform rebellion × Year FE	Yes	Yes	Yes
Province × Year FE	Yes	Yes	Yes
Prefecture Year Trend	Yes	Yes	Yes
Mean of the Dependent Variable	0.0330	0.0334	0.0322
No. of Observations	140,432	129,166	130,476
No. of Counties	536	493	498
Adjusted R-squared	0.0497	0.0515	0.0485

Note. The sample consists of 536 counties in the six provinces around the canal from 1650 to 1911. The dependent variable is the inverse hyperbolic sine transformation of the number of rebellions normalized by 1600 population. *Canal Length* (per $100km^2$) is the length of the canal portion (in km) contained with the county's boundary, divided by the size of the county (in $100km^2$). *Canal Town Share* is the share of towns within 10 kilometers away from the canal, measured in 1820. *Distance to canal* is the distance from a county's geological center to the canal. *Post* is an indicator that equals one in and after 1826. Standard errors in parentheses are clustered at the county level. Standard errors in square brackets are Conley standard errors robust for spatial correlation, assuming a cut-off window of 500 km and a serial correlation of 262 years.

Table 5: Canal closure and rebellions: north versus south

	Dependent Variable: Rebellions			
	(1)	(2)	(3)	(4)
$AlongCanal \times Post \times North$	0.0991	0.0998	0.0907	0.0687
	(0.0274)	(0.0284)	(0.0286)	(0.0279)
	[0.0287]	[0.0291]	[0.0284]	[0.0277]
Along Canal × Post	-0.0144	-0.0144	-0.0065	0.0020
	(0.0092)	(0.0092)	(0.0105)	(0.0095)
	[0.0119]	[0.0119]	[0.0113]	[0.0106]
$North \times Post$	0.0282	0.0281	0.0197	0.0221
	(0.0083)	(0.0083)	(0.0221)	(0.0245)
	[0.0098]	[0.0097]	[0.0246]	[0.0263]
Constant	0.0256	0.0256	0.0271	0.0268
	(0.0018)	(0.0018)	(0.0044)	(0.0049)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Pre-reform rebellion × Year FE	No	Yes	Yes	Yes
Province × Year FE	No	No	Yes	Yes
Prefecture Year Trend	No	No	No	Yes
Mean of the Dependent Variable	0.0330	0.0330	0.0330	0.0330
No. of Observations	140,432	140,432	140,432	140,432
No. of Counties	536	536	536	536
Adjusted R-squared	0.0263	0.0332	0.0475	0.0499

Note. The sample consists of 536 counties in the six provinces around the canal from 1650 to 1911. The dependent variable is the inverse hyperbolic sine transformation of the number of rebellions normalized by 1600 population. $AlongCanal$ is an indicator that equals one if the county is adjacent to the canal. $Post$ is an indicator that equals one in and after 1826. The northern and southern parts are defined by the canal's intersection with the old course of the Yellow River. $North$ is an indicator of whether a county is located north to the old course of the Yellow River. Standard errors in parentheses are clustered at the county level. Standard errors in square brackets are Conley standard errors robust for spatial correlation, assuming a cut-off window of 500 km and a serial correlation of 262 years.

Table 6: Canal closure and rebellions: placebo treatments

	Dependent Variable: Rebellions				
Placebo treatment:	(A)	(B)	(C)	(D)	(E)
	(1)	(2)	(3)	(4)	(5)
Along × Post	-0.0496	0.0484	0.0044	-0.0045	0.0034
	(0.0142)	(0.0308)	(0.0102)	(0.0094)	(0.0092)
	[0.0209]	[0.0264]	[0.0118]	[0.0096]	[0.0090]
Constant	0.0335	0.0319	0.0328	0.0334	0.0325
	(0.0002)	(0.0007)	(0.0004)	(0.0009)	(0.0014)
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Pre-reform rebellion × Year FE	Yes	Yes	Yes	Yes	Yes
Province × Year FE	Yes	Yes	Yes	Yes	Yes
Prefecture Year Trend	Yes	Yes	Yes	Yes	Yes
Mean of the Dependent Variable	0.0330	0.0330	0.0330	0.0330	0.0330
No. of Observations	140,432	140,432	140,432	140,432	140,432
No. of Counties	536	536	536	536	536
Adjusted R-squared	0.0496	0.0496	0.0495	0.0495	0.0495

Note. The sample consists of 536 counties in the six provinces around the canal from 1650 to 1911. The dependent variable is the inverse hyperbolic sine transformation of the number of rebellions normalized by 1600 population. *Along* is an indicator that equals one if the county is adjacent to the specific transportion arterial specified at the top of each column: (A) Yangtze River, (B) Yellow River, (C) Coast, (D) Courier routes, and (E) Any of the four. *Post* is an indicator that equals one in and after 1826. Standard errors in parentheses are clustered at the county level. Standard errors in square brackets are Conley standard errors robust for spatial correlation, assuming a cut-off window of 500 km and a serial correlation of 262 years.

Table 7: Canal closure and rebellions: major distortions

	Dependent Variable: Rebellions			
	Panel A		Panel B	
	(1)	(2)	(3)	(4)
Canal × Post	0.0438	0.1032	0.0437	0.1034
	(0.0175)	(0.0382)	(0.0174)	(0.0365)
	[0.0171]	[0.0367]	[0.0170]	[0.0356]
Opium Battlefield × Post			-0.0230	
			(0.0198)	
			[0.0179]	
Canal × Opium Battlefield × Post			-0.0339	
			(0.0284)	
			[0.0256]	
Taiping × Post				-0.0018
				(0.0171)
				[0.0222]
Canal × Taiping × Post				-0.0928
				(0.0391)
				[0.0388]
Constant	0.0313	0.0362	0.0312	0.0315
	(0.0007)	(0.0009)	(0.0008)	(0.0024)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Pre-reform rebellion × Year FE	Yes	Yes	Yes	Yes
Province × Year FE	Yes	Yes	Yes	Yes
Prefecture Year Trend	Yes	Yes	Yes	Yes
Mean of the Dependent Variable	0.0077	0.0087	0.0077	0.0077
No. of Observations	138,860	79,648	140,432	140,432
No. of Counties	530	304	536	536
Adjusted R-squared	0.0499	0.0689	0.0497	0.0500

Note. Panel A excludes counties that suffered from battles During the Opium War (column 1) or the Taiping Rebellion (column 2). Panel B examines the heterogeneous effects in those regions. The sample consists of 536 counties in the six provinces around the canal from 1650 to 1911. The dependent variable is the inverse hyperbolic sine transformation of the number of rebellions normalized by 1600 population. *Canal* is an indicator that equals one if the county is adjacent to the canal. *Opium Battlefield* is an indicator that equals one if the county was one of the battlefields during the First Opium War. *Taiping* is an indicator that equals one if the county was one of the battlefields during the Taiping Rebellion. *Post* is an indicator that equals one in and after 1826. Standard errors in parentheses are clustered at the county level. Standard errors in square brackets are Conley standard errors robust for spatial correlation, assuming a cut-off window of 500 km and a serial correlation of 262 years.