**The London School of Economics and Political Science**

# *Essays on Development Economics and Japanese Economic History*

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A thesis submitted to the Department of Economics of the London School of Economics for the degree of Doctor of Philosophy, London, July 2017

# Declaration

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# Acknowledgment

I am sincerely grateful to my advisors, Prof. Robin Burgess and Dr. Greg Fischer, for their continuous support of my PhD study. They guided my development to start my career as a researcher; without their guidance and continuous feedback, this PhD would not have been achievable. I am also grateful to the following researchers for their comments on my papers: David Atkin, Oriana Bandiera, Gharad Bryan, Maitreesh Ghatak, Vernon Henderson, Janet Hunter, Yukinobu Kitamura, Masayuki Kudamatsu, Takashi Kurosaki, Gerard Padró i Miquel, Tetsuya Okazaki, Gianmarco Ottaviano, Hitoshi Shigeoka, Yasuko Takatsuki, Kensuke Teshima, Shintaro Yamaguchi, and Kazuki Yokoyama. I also would like to pay respect to Hidehiko Ichimura and Yasuyuku Sawada, who supported me even after my graduation at the University of Tokyo.

Furthermore, I am surrounded by great friends. My colleagues and friends at the LSE, including Pedro Alves, Michel Azulai, Diego Battiston, Florian Blum, Shiyu Bo, Hao Dong, Miguel Espinosa, Hanwei Huang, Dana Kassem, Yu-Hsiang Lei, Nicola Limodio, Yatang Lin, Stephan Maurer, Tsogsag Nyamdavaa, Frank Pisch, Federico Rossi, Francesco Sannino, Arthur Seibold, Xuezhu Shi, Munir Squires, Eddy Tam, Lisa Windsteiger, and Giulia Zane gave many comments on my research and supported me during my PhD study in London through conversations over a cup of coffee or pint of beer. Takeshi Aida, Jun Goto, Nobuyoshi Kikuchi, Yuhei Miyauchi, and Mari Tanaka were colleagues when I was at the University of Tokyo and supported me from other institutions to which they subsequently moved. Kohei Kawaguchi shared his post-graduate experience at both the University of Tokyo and LSE with me, and we had many discussions about our research projects. Takahiro Yamamoto provided the impetus to start researching Japanese economic history. Takubo Junpei, Tomohito Honda, Shouta Tojima, and Yoshimasa Katayama provided excellent research assistance. My old friends, Kenichi Koizumi and Michihiro Nakamura, helped me with their programming skills for my data collection.

The Nakajima Foundation financially supported my PhD study. The Institution of Economic Research at Hitotsubashi University provided me with invaluable funding and access to archives, which was necessary for my data collection. In addition, for giving me permission to use the data, I greatly appreciate Yutaka Arimoto, Kaori Ito, Asuka Imaizumi, Tetsuji Okazaki, Kentaro Nakajima, and Tomohiro Machikita, who digitized a part of the data used in this thesis with support from the JSPS KAKENHI Grant-in-Aid for Scientific Research (B) 21330064.

I owe thanks to John Curtis, Jane Dickson, Rhoda Frith, Gisela Lafico, Akemi Okayasu, Mark Wilbor, and many librarians for their help throughout the process.

This work has benefited from comments in various seminars and conferences, including STICERD, 15th EUDN PhD Workshop on Development Economics, NEUDC 2016, SAEe 2016, Kyoto Summer Workshop on Applied Economics, Hayami Conference, and those at the LSE, Kobe University, Osaka University, The University of Queensland, Tsukuba University, Kindai University, and Hitotsubashi University.

Finally, I express appreciation to my parents and in particular my partner, Saeko, for her patience and encouragement during the last 6 years.

# Abstract

This thesis consists of three independent chapters on development economics and Japanese economic history.

The first chapter analyzes the effect of railroad construction in the Meiji period (1868–1912) on technology adoption and modern economic development. By digitizing a novel data set that measures the use of steam engines at the factory level and determining the cost-minimizing path between destinations as an identification strategy, I find that railroad access led to the increased adoption of steam power by factories, which in turn induced structural change and urbanization. My results support the view that railroad network construction was key to modern economic growth in pre-First World War Japan.

The second chapter analyzes the effect of time horizon on local public investment in the Edo period (1615–1868). I use a unique event in Japanese history during this period to identify the effect. In 1651, the sudden death of the executive leader of the Tokyo government reduced the transfer risk of local lords, especially for insiders, who supported the Tokyo government during the war of 1600. Using a newly digitized data set and a difference-in-differences strategy, I find that after 1651, regions owned by insiders increased the number of public projects more than regions owned by the other lords. I discuss other possible channels to interpret the effect of tenure risk, but I find no strong support for these alternative channels and conclude that the results support a longer time-horizon effect.

The third chapter provides more general background and a complete description of the data availability in Japan in the 17th–20th centuries, to discuss future research directions. It would aid reexamination of the history of Japan and other East Asian countries, which have experienced different economic and political paths.

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# Preface

This thesis consists of three independent chapters on development economics and Japanese economic history.

The first chapter analyzes the effect of railroad construction in the Meiji period (1868–1912) on technology adoption and modern economic development. Railroad access can accelerate technological progress in the industrial sector and therefore, can induce structural change and urbanization, the two common features of modern economic growth. I examine this particular mechanism by digitizing a novel data set that measures the use of steam engines at the factory level, allowing me to observe the diffusion of steam power directly, and I analyze the effect of railroad access on the adoption of steam power. To overcome an endogeneity problem, I determine the cost-minimizing path between destinations, and use this to construct an instrument for railroad access. I find that railroad access led to the increased adoption of steam power by factories, which in turn reallocated labor from the agricultural sector to the industrial sector, thereby inducing structural change. In addition, the railroad network reversed the mean decline in population growth, eventually leading to urbanization. My results support the view that railroad network construction was key to modern economic growth in pre-First World War Japan.

The second chapter analyzes the effect of time horizon on local public investment in the Edo period (1615–1868). The political economy literature recognizes that a longer time horizon of government is one of the key determinants of local public goods investment and the origin of states. I use a unique event in Japanese history during this Edo period to identify the effect. In 1651, the sudden death of the third Shogun, the executive leader of the Tokyo government, gave rise to a political crisis and reduced the transfer risk of local lords, especially for insiders, who supported the Tokyo government during the war of 1600. To exploit this regime change to identify the causal impact of tenure risk on public goods provision, I digitize various data sources, such as the records of public investment in irrigation and other agricultural infrastructure. By a difference-in-differences strategy, I find that after 1651, regions owned by insiders increased the number of public projects financed by local lords more than regions owned by the other lords. I discuss other possible policies that might induce bias in this reduced-form effect, such as in-kind intergovernmental transfers and other possible ways to interpret the effect of tenure risk. However, I find no strong support for these alternative channels and conclude that the results support a longer time-horizon effect.

The third chapter provides more general background and a complete description of the data availability in Japan in the 17th–20th centuries, in order to discuss future research directions. It aids understanding to reexamine the history of Japan and other East Asian countries, which have experienced different economic and political paths. I first explain the political and economic development in the Edo and Meiji periods. Then, I explain various data sets to track the changes in economy and politics, focusing on important data sets with disaggregated information. In addition, I present descriptive statistics to capture the process of development, mainly using data sets not used in the other chapters. Then, I conclude with a discussion of future research directions.

# Chapter 1

# Railroads, Technology Adoption, and Modern Economic Development: Evidence from Japan

## 1.1 Introduction

Transportation infrastructure is considered to be one of the most important public goods in developing countries. In 2015, the World Bank was financing 197 transportation projects, accounting for 21 percent of its total lending.[1] The bank's annual report for 2015 shows that the transportation sector was consistently one of the three largest recipients of lending from 2011 to 2015. In the United States and Western Europe, railroad network construction was associated with an increase in economic growth. Rostow (1960) argues that through increased market integration and higher demand for industrial goods, railroads were a key factor behind the take-off from pre-industrialized economies.[2]

This study investigates the effect of railroads on technology adoption in the industrial sector,[3] structural change, and urbanization. The result of Bustos (2011) suggests that railroad network construction should increase the adoption of new technology because of the increased economies of scale. Alvarez-Cuadrado and Poschke (2011) presents evidence that the productivity growth in the manufacturing sector relative to the agricultural sector is responsible for the structural change in the countries that transitioned between 1840 and 1920. However, the cause of this productivity growth, its effect on structural change, and even its consequences for the overall economy are not well understood. Apart from structural change, the other common feature of modern economic development is urbanization, which is only possible if the population dynamics diverge from the pattern of mean reversion seen in agricultural economies. Desmet and Rossi-Hansberg (2009) and Michaels et al. (2012) show that the adoption of new technologies and structural change broke mean reversion in the United States.

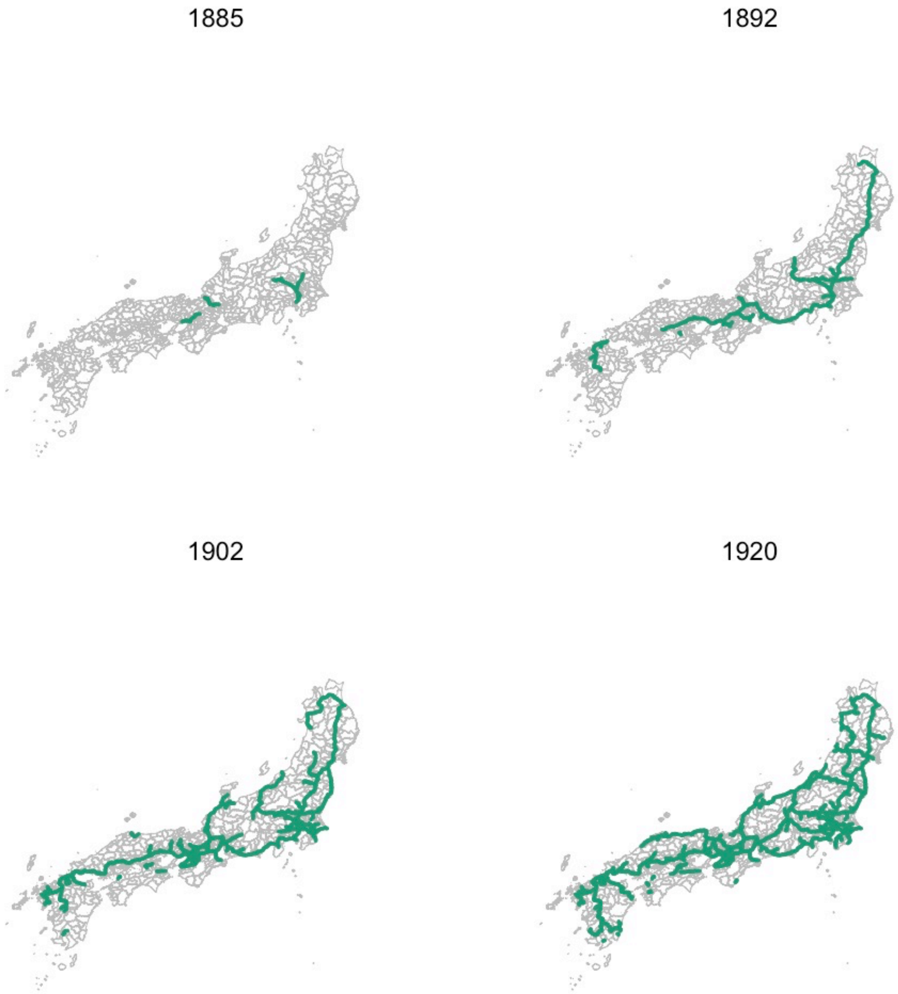
In particular, I consider the case of Japan in the late 19th and early 20th centuries. During this time, Japan expanded its railroad network, adopted many Western technologies, and experienced modern economic development. The Japanese government, following its modernization and centralization through the Meiji Restoration in 1868, started constructing a railroad network in the 1870s. By 1900, this network reached around 60 percent of the counties, as shown in Figure 1.1. Figure 1.2 shows that during this time, the adoption of steam power proceeded at a faster pace than the growth in the the agricultural sector's

[1] http://www.worldbank.org/en/topic/transport/overview.

[2] This claim is tested by Fogel (1964). He uses sub-national price and trade flow data for the United States and concludes that the impact of railroads was not as large as expected. Donaldson (2017) test whether the price of agricultural land increased around the construction of railroads, as trade theory predicts. They conclude that structural market access, as derived from a model a la Eaton and Kortum (2002), increased the land price and that without railroad access, the price would be 64 percent lower. If this effect could be mitigated by the feasible extension of waterways and road improvement, then the loss would be in the range of 13 to 20 percent.

[3] In this paper, I abstract from the service industry and use the "industrial sector" and "non-agricultural sector interchangeably.

Figure 1.1: Railroad Network

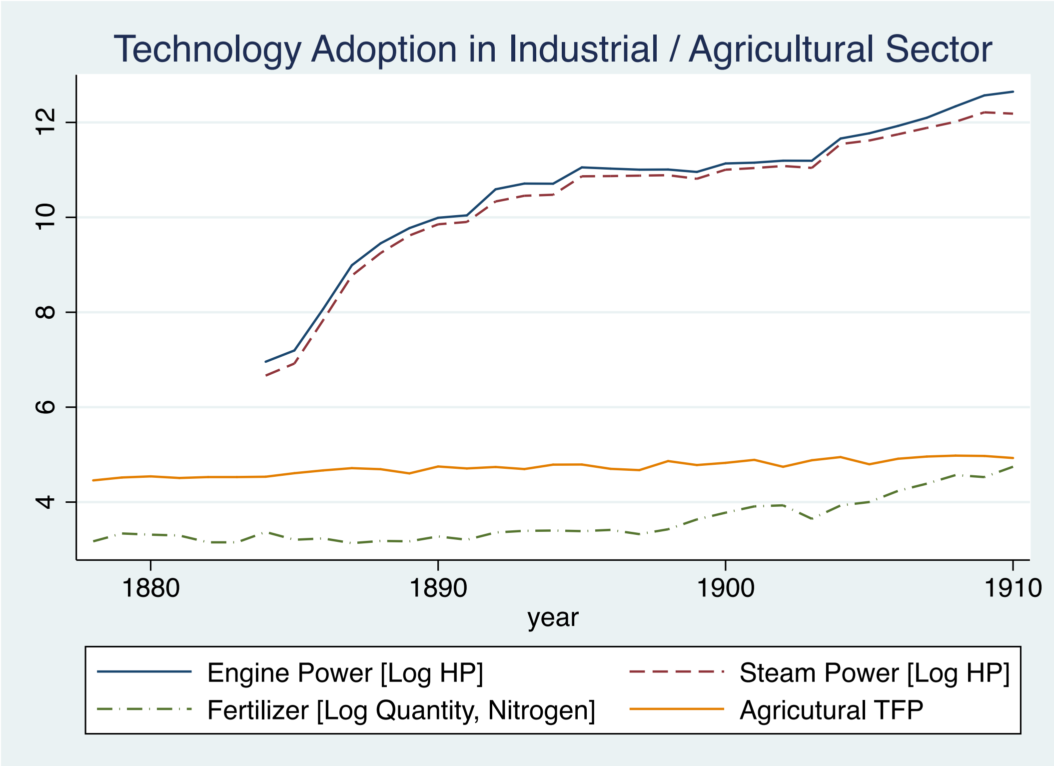



The green lines represent those that existed in each year. See section 1.3.1 for the source

total factor productivity and the use of fertilizers, which was the primary new technology in the agricultural sector. Similarly, Figure 1.3 illustrates that the price of industrial goods decreased relative to that of agricultural goods. Figure 1.4 shows that during this time Japan also experienced structural change and Figure 1.5 urbanization. These changes resulted in the rapid growth in GDP. Figure 1.6 illustrates that in 1868, Japanese GDP was far lower than that of the United States or the United Kingdom, but that it began to increase thereafter.

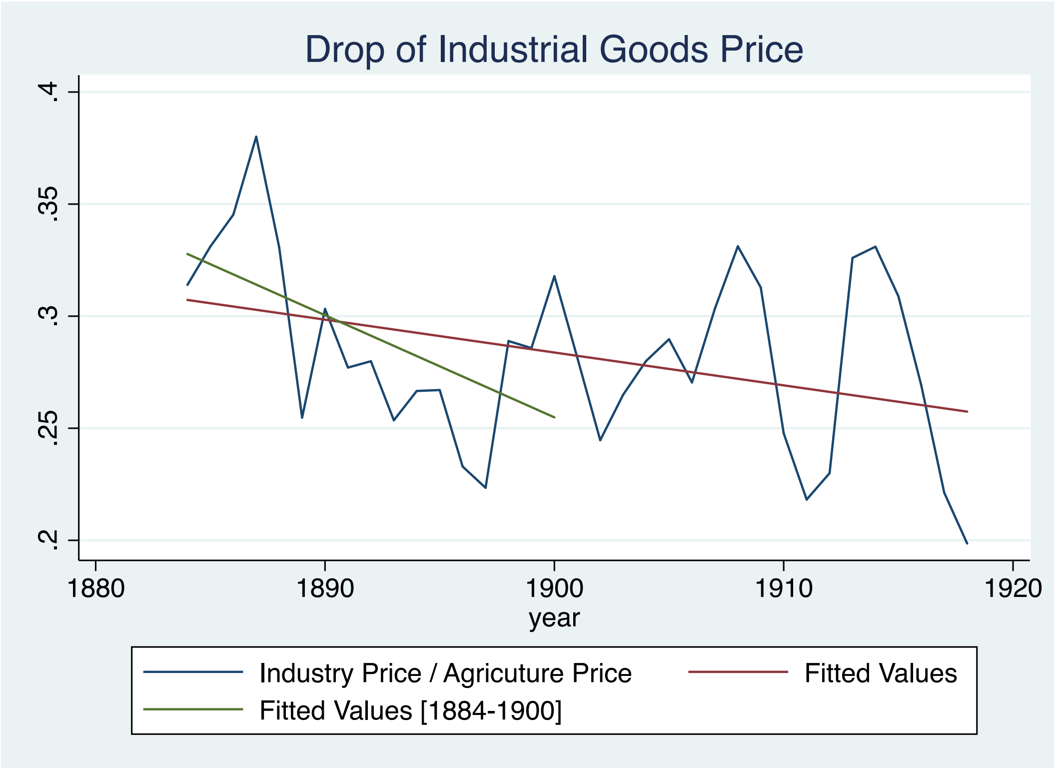
To facilitate my analysis, I digitize several novel data sets. I construct geographical data showing the railroad network expansion from the 1870s to 1910s based on current network data and documents to identify the opening year of each section. To measure technology adoption, I digitize repeated cross-sectional factory data, which contain ad-

Figure 1.2: Adoption of New Technology in the Industrial and Agricultural Sectors




Source: Minami (1979) for the steam power and Umemura et al. (1965) for the fertilizers and total factor productivity. These show aggregated numbers.

Figure 1.3: Relative Price (Industrial/Agriculture)

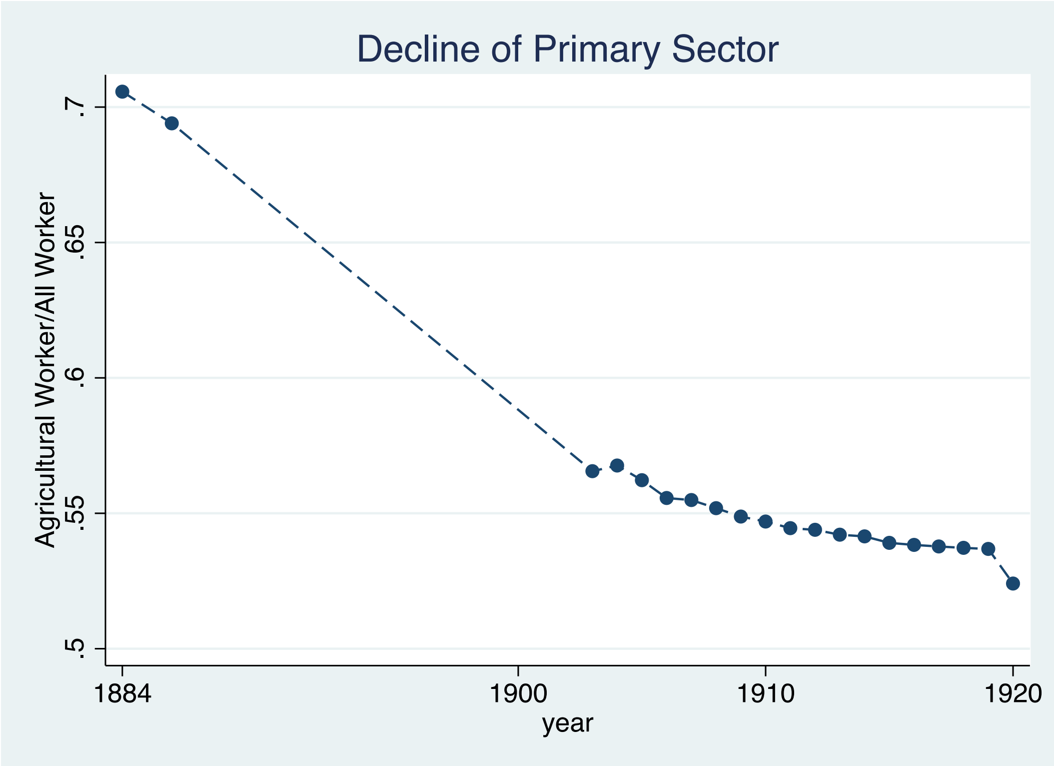



Source: Umemura et al. (1965) as used in Alvarez-Cuadrado and Poschke (2011)

dresses, the types of products manufactured, and information on the number of engines and total horsepower for each power source (steam and water). This approach allows me to construct a county-level panel data set with concrete measures of technology adoption and productivity. In addition, I digitize county-level population and sector-wise worker data for my sample period to analyze the population dynamics and structural change.

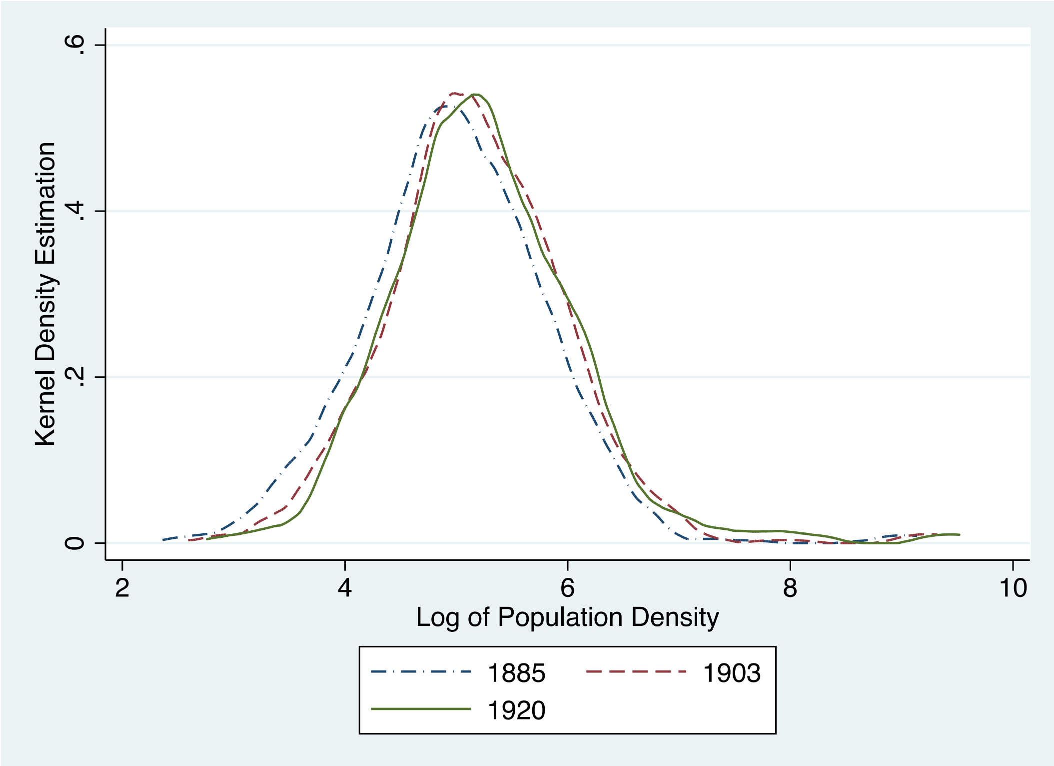
Railroad construction is an endogenous choice by the government, and this endogeneity could bias the ordinary least squares (OLS) estimates. Moreover, the direction of the bias is unclear ex ante. The government may choose to favor areas where the economic benefits would be the greatest. Alternatively, it may choose to build railroads in areas with low economic potential as a form of redistribution. I use an instrumental variable (IV) to overcome this endogeneity problem. I construct the cost-minimizing route between destinations, using slope information to determine the construction costs incurred by the government. This is a standard type of identification strategy in this literature, as used in Atack et al. (2008b), Banerjee et al. (2012), Hornung (2012), and Faber (2014). It exploits that the government's motivation to connect the destinations and make the network is exogenous to the local economy. I then use the distance from the cost-minimizing path as my instrument for growth in railroad access, while conditioning on local geography and other variables to satisfy the assumption of quasi-random assignment. My difference-in-

Figure 1.4: Decline in the Primary Sector, 1885–1920



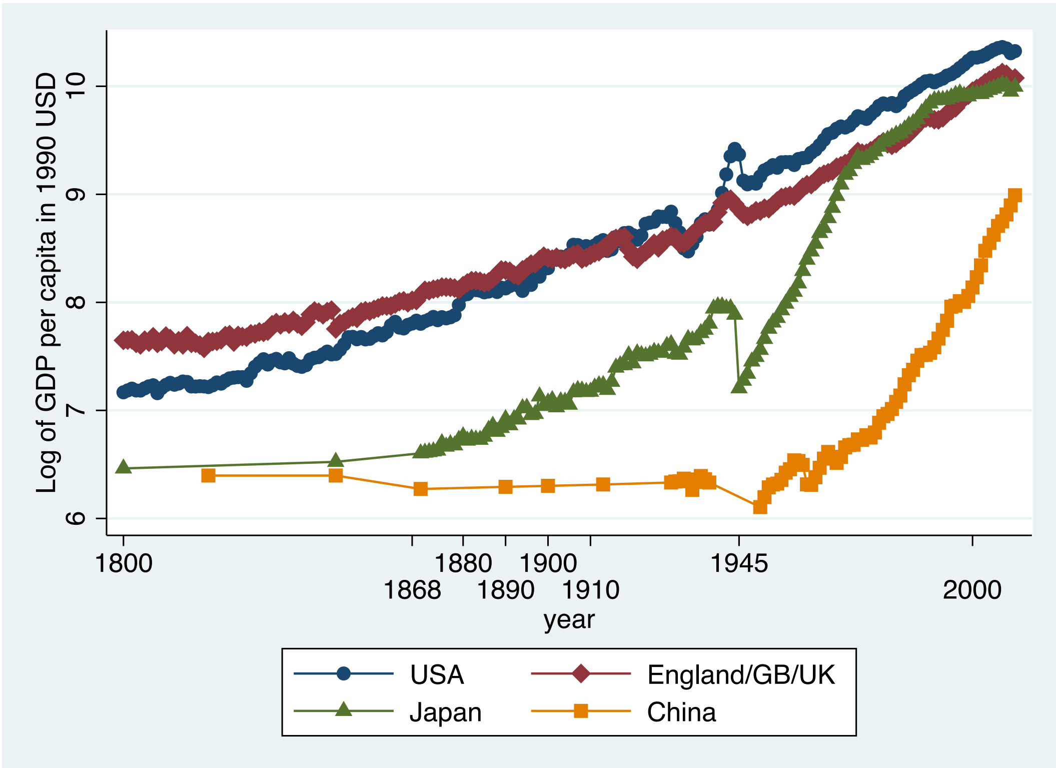

Source: *Meiji iko Hompo Shuyou Keizai Tokei (Major Economic Statistics After the Meiji Period)* published by the Bank of Japan in 1999

Figure 1.5: Population Density in 1885, 1902, and 1920




Notes and Source: The data are based on the 1885 and 1902 population statistics and 1920 census. Section 1.3.2 provides more detailed information on the data source.

Figure 1.6: Log of GDP per Capita, 1800–2000




Source: The Maddison Project, `http://www.ggdc.net/maddison/maddison-project/home.htm`, 2013 version.

differences (DID) with IV estimates imply that having railroad access by 1892 led to an increase in steam power of about 300 horsepower at the county level between 1888 and 1902.[4] This fact means that railroad access growth between 1888 and 1892 accounted for 67 percent of steam power growth between 1888 and 1902.

I also analyze the impact of gaining access to the railroad network earlier on structural change and urbanization. By using census data from 1920 and agricultural worker data from 1885, I estimate the long-term impact of having railroad access by 1892 on the share of workers in the primary sector. Conditional on the share of the population working in the agricultural sector in 1885, I find that early adopters have a 6.7 percent lower share of workers in the primary sector. To analyze the link between railroad access and the population dynamics, I estimate the impact of railroad access on population growth, allowing for dependence on initial density. I find that without railroad access by 1892, counties with a log initial population one standard deviation above the average have 7.2 percent lower growth between 1885 and 1902 compared with counties at the mean. This pattern of mean reversion is broken by railroad access. If a county gained railroad access by 1892, this 7.2 percent decline becomes a 1.9 percent decline. When I analyze population growth between 1885 and 1920, the estimates for early adopters even become positive. The estimated effect is 11.3 percent compared with minus 16.9 percent for counties with no railroad access by 1892. This finding is consistent with a positive feedback mechanism generating urbanization.

When combined, these results imply that railroad construction was the key factor behind both structural change and urbanization. My results provide further implications for the more general question of why the industrial sector in Japan adopted Western technology so quickly and developed so rapidly after the Meiji Restoration. This study suggests one explanation, namely the presence of a centralized government receptive to Western technology, which enabled these changes by rapidly constructing a railroad network.

This study contributes to the literature on infrastructure, technology adoption, structural change, population dynamics, and urbanization as well as on the economic history of Japan and the industrial revolution. In terms of the effects of railroads,[5] recent studies such as Atack et al. (2008b), Atack et al. (2010), Burgess and Donaldson (2010), Herrendorf et al. (2012), Duranton and Turner (2012), Jedwab and Moradi (2013), Fajgelbaum and Redding (2014), Donaldson and Hornbeck (2016), Donaldson (2017), and Berger

[4] In 1902, the average steam power in factories was about 32 horsepower. According to the instruction manual of the Toyoda power loom, which was invented in 1898 as the first Japanese power loom, factories could handle 20 power looms per unit of horsepower (`https://www.toyota.co.jp/jpn/company/history/75years/text/taking_on_the_automotive_business/chapter1/section1/item6.html`.

[5] Atkin and Donaldson (2015) finds that in Ethiopia and Nigeria, trade costs are four to five times larger per unit of distance than those in developed countries.

and Enflo (2017) investigate the effect of railroads on the number of factories,[6] price equalization, famine, the price of agricultural land, specialization, structural change, and the (urban) population.[7] Authors such as Baum-Snow (2007), Duflo and Pande (2007), Jensen (2007), Michaels (2008), Goyal (2010), Dinkelman (2011), Rud (2012), Duranton et al. (2014)f, Lipscomb et al. (2013), Kline and Moretti (2014), Faber (2014), and Agrawal et al. (2016) study other types of infrastructure such as highways, dams, electricity, regional development plans, and mobile phones.[8] On the contrary, my study focuses on the particular mechanism of why railroad construction can induce modern economic development by examining the effect on technology adoption.

Research on international economics also examines technology adoption. Since railroads reduce transportation costs, gaining access to a railroad network will be conceptually similar to a reduction in tariffs or trade liberalization. The study most similar to mine is Bustos (2011), which studies the impact of MERCOSUR on investment in technology by Argentinian firms.[9]

Turning to the structural change literature in macroeconomics, as noted above, Alvarez-Cuadrado and Poschke (2011) shows empirically that the nature of structural change altered between 1920 and 1960. Before 1920, structural change was driven by an increase in the relative productivity of non-agricultural industries, which led to a labor pull effect. Since 1960, however, structural change has been driven by productivity improvements in agriculture, which have led to a labor push effect. This can be formulated as operating through an income effect, as in Gollin et al. (2002) and Kongsamut et al. (2001), or through a relative price effect as in Ngai and Pissarides (2007). On the contrary, recent empirical studies that use within-country exogenous variation find that growth in agricultural productivity is the cause of structural change. Foster and Rosenzweig (2008), Marden (2015), Bustos et al. (2016), and Kitamura (2016) find this to be the case for

[6]Factory production may be a new technology as well, although the former is defined as an establishment with more than 15 workers, and therefore does not measure the change in management methods. In addition, even among factories, the steam-powered ones had higher labor productivity than that of the non-steam-powered ones, as shown by Atack et al. (2008a). Thus, it is worth considering this margin of steam power adoption.

[7]Kim (2005) and Herrendorf et al. (2012) use a rural/urban classification in their analysis of urbanization, without discussing Gibrat's Law. However, such a classification depends on the country or context, meaning that a discussion based on population dynamics will be more general.

[8]Faber (2014) uses an identification strategy similar to the one adopted in this study in the context of highway construction in China. The Chinese government aimed to connect all local capitals and cities with a single highway network. As an instrument, he uses geographical information to find the cost-minimizing path between each possible combination of cities and then chooses routes to connect all cities at the lowest cost. In my case, the Japanese government merely wanted to connect specific destinations. In particular, it aimed to connect Tokyo with the northern and western parts of Honshu, Aomori, and Niigata. I find no anecdotal evidence of their intention to directly connect every destination to every other destination.

[9]To be precise, her measure of investment in technology is R&D spending, whereas mine is total horsepower, which is an outcome of investment in new technology.

post-Second World War India, China, Brazil, and Japan, respectively. All these studies examine eras and countries, with Alvarez-Cuadrado and Poschke (2011) finding that technological progress in agriculture drives structural change, or they are out of the sample of Alvarez-Cuadrado and Poschke (2011) entirely. This study thus examines why the relative productivity of non-agricultural industries was rising before 1920 and whether this rise induced any structural change.

The fourth strand of the literature to which this study is linked includes studies of population dynamics and urbanization. In these works, Gibrat's Law, which asserts that population growth rates are constant irrespective of the differences in the initial population density, is one of the stylized facts.[10] However, Dittmar (2011), Glaeser et al. (2014), and Chauvin et al. (2017) find that it does not hold and population dynamics shows mean reversion in the United States and Europe in pre-modern era, and India and China from 1980 to 2010. Studies such as Giesen and Südekum (2014) and Desmet and Rappaport (2017) investigate which factors (e.g., city age) explain these differences in population growth rates. Desmet and Rossi-Hansberg (2009) and Michaels et al. (2012) argue that industry-specific technological progress is crucial in that sufficiently fast technological progress will break the pattern of mean reversion in the population as better technology attracts migration. My study tests this idea by investigating whether gaining access to a railroad network and the subsequent adoption of new technology breaks mean reversion in the population dynamics.

Finally, this study is linked to the literature on the economic history surrounding steam power and the industrial revolution. The diffusion pattern of steam power and its effect on factory-level productivity and urbanization in the United States have been studied by Atack et al. (1980), Kim (2005), and Atack et al. (2008a). Other related studies consider the industrial revolution in Japan. The research most similar to mine is Tang (2014), which analyzes the impact of railroad access on the number of firms and their capital stocks. The present study differs from Tang (2014) in that I use various county-level outcomes while Tang (2014) uses prefecture-level data and construct an instrumental variable to identify the impact. I find a significant positive average effect on technology adoption using the data from 1888 to 1891 and from 1888 to 1902 while he finds no significant average effect on firm capital using the data from 1884 to 1893. County-level data enable us to estimate negative externalities or reallocation effect that will lead to a upward bias in the main specifications in the both papers, but I did not find a negative neighborhood effect and find similar point estimates when using specifications using a railroad access dummy at the county level and prefecture level. I also construct a larger set of variables

[10]Gibrat's Law is linked theoretically to Zipf's Law, another stylized fact concerning the size distribution of cities. Gabaix and Ioannides (2004) describes the related theory and Giesen and Südekum (2011) provides empirical evidence.

to see the effect of railroads on structural change and urbanization. Minami (1977) and Minami (1979) describe the diffusion of new power sources by using prefecture-level data and estimate the impact on productivity through growth accounting. However, the link between the diffusion of steam power and railroad access is not studied. Kiyokawa (1995) provides an overview of technology adoption in pre-Second World War Japan, covering both the agricultural and the non-agricultural sectors.

The remainder of this paper proceeds as follows. Section 1.2 discusses why the construction of a railroad network can accelerate technology adoption, structural change, and urbanization. Section 1.3 provides an overview of the history of railroads and steam technology in Japan, describes the data sources used in the study, and explains the construction of the instrument. Sections 1.4 and 1.5 present my estimation results, and Section 1.6 concludes.

## 1.2 Conceptual Framework

In this section, I first explain the market access channel, which is the main channel through which railroad access could have increased technology adoption in the industrial sector. I then describe how this could facilitate structural change and urbanization. I also discuss two other potential channels, the migration and adoption cost channels, and explain how it is possible to distinguish them from the main channel in my data. In addition, I describe how the existence of financial frictions and the type of market integration, foreign or domestic, could lead to heterogeneous outcomes. I use the intuition developed in this section as the basis for the formal statistical tests in Section 1.4. The discussion in this section is informal, but I include a formal model for the main channel in the Appendix.

To understand the market access channel, consider a firm in a monopolistically competitive industry, which exports goods to many regions. To export its goods, the firm has to pay for the iceberg trade costs. When railroads are constructed, these trade costs decrease. The firm would then reduce its price,[11] and facing constant elasticity of substitution (CES) consumer demand, it increases its sales. Suppose the firm can pay a fixed cost to adopt a new technology that lowers its marginal cost. As in Bustos (2011), firms that have access to a railroad network and produce more output will be more likely to adopt this new technology.[12]

Alvarez-Cuadrado and Poschke (2011) show that in the presence of an income effect

[11] We have the same implication when firms can use inputs with a lower price. I include this as a part of the market access channel.

[12] David (1975) discusses a similar mechanism to explain why railroads lead to a gain in productivity at the factory level. However, he explains this by using the notion of learning-by-doing rather than technology adoption. He also points out that this gain is not accounted in the social saving estimated by Fogel (1964).

in the industrial sector, the adoption of new technology by industrial firms will increase the share of total workers employed in this sector, thereby implying structural change. This effect is known as labor pull. Most of the empirical literature examines labor push, where productivity growth in the agricultural sector leads to industrialization. Alvarez-Cuadrado and Poschke (2011) show that the labor pull effect was dominant in developed countries before 1920. As seen above, this is also true in the case of Japan.[13]

Technology adoption would also change the population dynamics. In an agricultural economy where technological progress is stagnant, more populated areas have lower population growth because of the mean reversion of agricultural productivity. In an industrial economy, the population dynamics of more populated areas are determined by the productivity of the industrial sector, which is more persistent because of technological progress. More populated areas may also have additional advantages in technology adoption, such as easier access to credit, which could lead to rapid population growth.

Access to a railroad network could affect technology adoption through three other channels. The migration channel may exist because railroad construction could affect migration costs. If railroads reduce migration costs, labor mobility may consequently increase. If areas with railroad access are more attractive to workers than those without, this could increase aggregated demand in the former areas. Sales of firms in these areas could increase as their home market will be larger. In addition, higher labor supply would lead to lower wages, which will be passed through to lower prices, further increasing sales. As mentioned above, higher sales for firms with access to the railroad network would mean that they are more likely to adopt new technologies. Note that the market access channel will also lead to an increase in migration through the labor pull effect; therefore, the market access and migration channels have broadly similar implications. However, the two differ in the timing of events. The market access channel implies that firms first adopt new technology, which is followed by an increase in the population. Under the migration channel, the order is reversed. It is thus possible to distinguish between the two channels by examining the timing of technology adoption relative to population growth.[14] The information channel may also exist because railroad construction would lower the communication cost and may induce technology adoption by sharing information such as the existence of steam power or its expected return. I test this channel by looking at the number of expositions, an important place to share information.

The final channel is the adoption cost channel. To adopt steam power, firms had to buy

[13]In the Appendix, I formally model both labor push and labor pull by combining Bustos (2011) and Alvarez-Cuadrado and Poschke (2011).

[14]Firms may adopt new technology in anticipation of future migration, which would render the timing under the migration effect ambiguous. However, the outcome variable that I examine is horsepower, a result of investment, and firms will have no incentive to increase horsepower before migration occurs.

machines from trading companies, which were mainly based in Tokyo or Osaka. Asajima (2001) documents that the Mitsui Trading Company, the dominant trading company in the late 19th century, imported and sold machines through its headquarters in Tokyo and a branch in Osaka. The introduction of a railroad network may thus have allowed firms based outside Tokyo and Osaka to access these trading companies more easily. In addition, Pomeranz (2001) shows that the gain in productivity by adopting new technology depends on the price of coal. By reducing transportation costs for coal, railroad network construction could increase the productivity gain from new technology for firms in areas far from coal producers.[15]

Other than these channels, Rostow (1960) suggests that railroad construction could affect the economy by increasing demand. Such construction requires industrial goods such as rails and locomotives. However, in this case most goods were imported from abroad, and thus the effect on domestic demand should be minimal. Moreover, if the decrease of trade cost is crucial as in the other channels, we will see smaller effects of railroad access in an area that did not decrease trade cost by railroad access because of a presence of rapid marine transportation.

In the presence of credit market frictions, the effect of access to a railroad network on technology adoption may be heterogeneous over areas. Suppose only urban areas have credit markets; then, firms in only these areas could invest in new technology.[16] Gaining access to a railroad network would then have a greater impact for more populated areas.[17]

The heterogeneous effects over industries are also important in order to distinguish whether the key mechanism is domestic integration or external integration. Although the conceptual framework abstracts from this, it would still affect the external validity of the results. For example, if the effect is found only in the exporting sector, then this suggests that country-level comparative advantage will fully determine the sector affected by railroad access.

[15] Coal was an export industry at that time, suggesting that the distance from the areas of coal production, and not ports, would be more appropriate.

[16] Nakamura (2010) shows that in Japan, the credit market for firms was based on face-to-face communication and networks among elites established during the Edo period. Ohnuki (2007) finds that interest rates were higher in rural than in urban areas in the early 1880s and 1890s, although the gap narrowed after the establishment of the Bank of Japan.

[17] When exporting has a fixed cost, only larger firms will choose to export. Bustos (2011) labels this as the firm selection effect. If larger firms tend to be located in areas with large populations, then this mechanism implies that the effect of railroad access on technology adoption will be stronger in urban areas. However, in the data I use, there is no significant relationship between population density and the average number of workers per factory. Therefore, this effect should be absent.

## 1.3 Background and Data

In this section, I first provide an overview of the history of railroad network expansion and explain how I exploit these historical facts in constructing my instrument. I then describe modern economic development in Japan. I also briefly discuss my data sources and explain the construction of my control and outcome variables. The Appendix contains a more detailed description of each of the original data sources. Note that most of the data set comes from hundreds of newly digitized documents through archival work. A unique feature of this data set is that I observe variables at a more disaggregated level (county level) than in previous studies (prefecture level). This approach thus enables me to analyze important economic issues such as heterogeneous impacts, externalities, and urbanization. Prefecture-level data will mask county-level variation when analyzing heterogeneous impacts and externalities. Such data are also unsuitable for analyzing urbanization if we are considering within-prefecture migration. Finally, I provide the descriptive statistics of my data set.

### 1.3.1 Development of the Japanese Rail Network

#### Background

Both railroads and steam power originated in the United Kingdom. The diffusion of these technologies to Japan was slow because of political circumstances. Consequently, adoption started in the late 19th century. From 1612 to 1853, the Tokugawa Shogunate severely restricted interactions with foreign countries in order to preserve political stability. In 1612, Christianity was outlawed, and in 1635 Nagasaki was designated as the only port open to Western ships.[18] In 1639, the Netherlands became the only Western country permitted to trade with Japan. Western knowledge could then diffuse to Japan only through the Netherlands. Throughout this period, Japan gradually adopted Western technology.

In the 1850s, the United States used military force to pressure Japan to open its ports to trade. Japan subsequently signed treaties with the United States, the United Kingdom, France, the Netherlands, and Russia. Political conflict over how to handle this foreign pressure resulted in a civil war, the collapse of the Tokugawa Shogunate, and, eventually, the establishment of the new Meiji Government in 1861. The civil war and threat of colonization by foreign powers led many political groups to attempt to adopt Western technology in the 1850s.

[18] Three other ports were open to Asian regions: Tsushima for Korea, Satsuma for Ryukyu, and Ezo for the Ainu people.

Railroad was one such technology. Japanese people first saw steam locomotives in the 1850s.[19] In 1869, the Meiji government created a railroad construction plan to link Tokyo and Kyoto. The first railway line opened in 1873. It was approximately 30 km in length and linked Tokyo with the largest port open to foreign trade, Yokohama. In 1874, a line linking Osaka, the second-largest city, and Kobe, another port open to foreign trade, was completed. Military expenditure related to the civil war in 1873 (the *Seinan Senso*) led to budget problems, and there was no further expansion of the railroad network in the 1870s.

In the 1880s, the railroad network began to expand. Following these fiscal problems, the Meiji government allowed the establishment of a private railroad company, *Nihon Tetsudo*, in 1881. This company was issued a license to build a line from Tokyo to the northern part of Honshu, the largest island in the Japanese archipelago. Despite being a private company, *Nihon Tetsudo* had close connections with the government,[20] and received financial support from it. In 1887, to further increase private investment in railroads, the government enacted a law allowing the private sector to establish railroad businesses. Under this law, private firms had to submit a proposal for a railway line to the government and obtain a license. The government also subsidized these private firms.[21] These policies enabled the railroad network to expand. The top-right panel in Figure 1.1 shows the railroad network in 1892. Accordingly, railway lines covered about 34 percent of the counties included in my sample.

The expansion continued even after the 1880s. In 1892, the central government enacted the Railway Construction Act (*Tetsudo Husetsuho*) to plan the construction of additional railroad lines. The law specified 33 lines,[22] and stated that a public railroad fund would finance their construction projects. Private sector firms were also allowed to build these lines on behalf of the government, conditional on obtaining a license. The routes were chosen based on predicted local economic benefits. A map of factors that predicted railroad revenues such as villages, industries, and other transportation was constructed.[23] The railroad network then expanded to cover the entire nation. In 1895, 2,126 thousand Yen was spent on railways through the public fund, constituting 2.5 percent of total government expenditure. By comparison, the total expenditure of the Ministry of Education

[19]By contrast, in the United Kingdom, the first commercial railroad with steam locomotives had opened in 1825.

[20]*Iwakura Toshimi*, who was appointed as *Udaijin* (literally "Minister of the Right") for the Council of State of the Meiji Government in 1871, served on the board of Nihon Tetsudo.

[21]Murphy et al. (1989) also has a view that subsidy to the railroad sector may be helpful for industrialization. This is because though it will be socially optimal to construct railroads, the fixed cost of railroads may be too high to construct railroads. In their model, the government also has a role in making a big push from no-industrialization to industrialization equilibrium by bringing coordination of investments of enough private factories, which is hard to test empirically though.

[22]This includes some lines that were already completed.

[23]See p. 123 in Vol. 3 of Nihon Kokuyu Tetsudo (1969).

was 1,153 thousand Yen and that of the Ministry of Agriculture and Commerce was 1,236 thousand Yen. The amount of freight traffic and number of cars used for freight services increased during the 1890s. Figure 1.7 shows that the growth rate of freight services was higher than that of passenger services.[24] The bottom-right panel of Figure 1.1 shows that by 1920, the rail network covered almost the entire country. Accordingly, the rail network decreased domestic trade costs. Indeed, Tetsudo-In (1916) shows that railroad access halved trade costs between Tokyo and Fukui, which is about 500 km on today's highway network. Before railroad network construction, this cost 132 yen and 10 days per 10 *kan* (37.5 kg). After railroad network construction, it became 62 yen and four days.[25] Tetsudo-In (1916) also states that following railroad access, people increased consumption by importing from other regions and domestic traders increased long-distance trade accordingly, which is consistent with the market access channel.

To construct my instrument, I exploit the government's policy objectives in the 1880s. Masaru Inoue, the director of the Ministry of Railways, was the driving force behind the government's railroad policy. When designing the line from Tokyo to Aomori, the northern part of Honshu, he stated that "We should not make side trips to consider local benefits." This statement implies that the government prioritized the connection from Tokyo to the end points of Honshu over the benefits to the local economy, suggesting exogeneity with respect to the economic conditions of the local areas along the route. In addition, technological constraints meant that the slope was an important consideration when planning routes. As pictured in Figure 1.8, construction was more difficult and costly in steeper areas. For example, the line connecting Tokyo with Western Honshu was initially planned to pass through steep terrain; however, after re-evaluation in 1885, the route was changed to pass through flatter areas as shown in Figure 1.9. A report comparing the two routes was prepared after this re-evaluation. This report estimated that the construction cost per mile of the original route was roughly double that of the final route. The slope was also significant for the operation of railway lines. Locomotives would run more slowly on slopes and were not sufficiently powerful to climb some steep slopes. The report estimated that a train on the original line would take one-and-a-half times as long as the revised route would, despite the length of the two routes being roughly the same.

Given this historical context, I construct the cost-minimizing paths from Tokyo to the prioritized destinations to use as an instrument for railroad network expansion from the 1880s to 1892. In 1881, *Nihon Tetsudo* prioritized a few destinations in its statement. The first objective of *Nihon Tetsudo* was to connect Tokyo and Aomori, the latter being

[24] At that time, most railroads carried both freight and passengers, whereas modern high-speed trains carry only passengers.

[25] Unfortunately, no comprehensive trade cost data before railroad network construction are available.

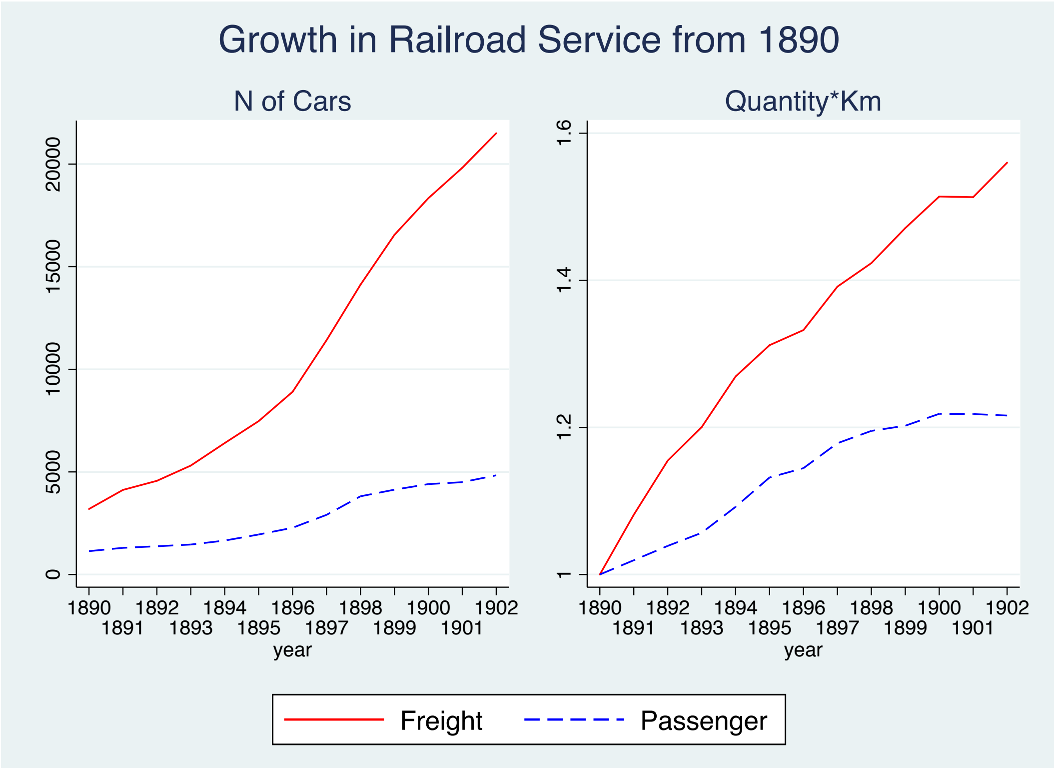
historically regarded as both the northern part of Honshu and the gateway to Hokkaido. The other two prioritized lines connected Tokyo and Niigata as well as Moji and Nagasaki. Niigata and Nagasaki were both ports open to Western trade, and Moji was the gateway port of Kyushu facing Shimonoseki.[26] To the west, Shimonoseki was prioritized by the government as the gateway port for the island of Kyushu.[27] To construct the cost-minimizing path, I use information on the slopes together with the algorithm proposed by Dijkstra (1959), which I explain below.

To be a valid instrument, the cost-minimizing path must satisfy the relevance, quasi-random assignment, and exclusion restrictions. Since cost was considered when the route was planned, the cost-minimizing path will predict the actual expansion of the network. Furthermore, we can easily test this by using the results of the first-stage regression. On the contrary, by construction, the cost-minimizing path will be correlated with local geography, which affects its own outcomes, and this violates the assumption of quasi-random assignment. Therefore, I use the distance from these paths *conditional on local geography including slope information*. Figure 1.10 illustrates this idea. As an example, I consider two potential paths from Tokyo to the destination in question, although there is an infinite set of potential. In this case, path B will be chosen as it passes through flatter terrain than path A does. If paths A and B are compared unconditionally, the assumption of quasi-random assignment will be violated as the flatter area may have better economic potential. By conditioning on local geography, the comparison is between counties with similar slopes and other geographic information such as distance to the coast, but one lies on path A and the other on path B. Therefore, the identification strategy relies on the assumption that the interaction of government's networking motivation (their destination choice) and other counties' geography is exogenous to the local economy. Moreover, before the railroad era, there were no attempts to connect Tokyo with other destinations by public expenditure on land transportation because of the absence of a powerful cen-

[26]The original statement by *Nihon Tetsudo* states that the line to Niigata will continue to Ushu, a historical province to the north of Niigata, comprising the modern Yamagata and Akita prefectures. However, the statement does not specify a city in this province as the intended destination. Therefore, I set Niigata as the destination. Niigata was the only foreign port open to Western trade on the Sea of Japan, rendering it a natural choice as the final destination. In addition, the fact that no exact destination in Ushu was specified indicates its subsidiary importance relative to Niigata. Similarly, the line between Moji and Nagasaki was originally intended to continue to Higo province, a historical province corresponding to today's Kumamoto prefecture. Assuming that the final destination is Kumamoto city, the line to Higo would be only 70 km in length and pass through five or six counties along the cost-minimizing path. Hence, excluding this branch will not affect the final results.

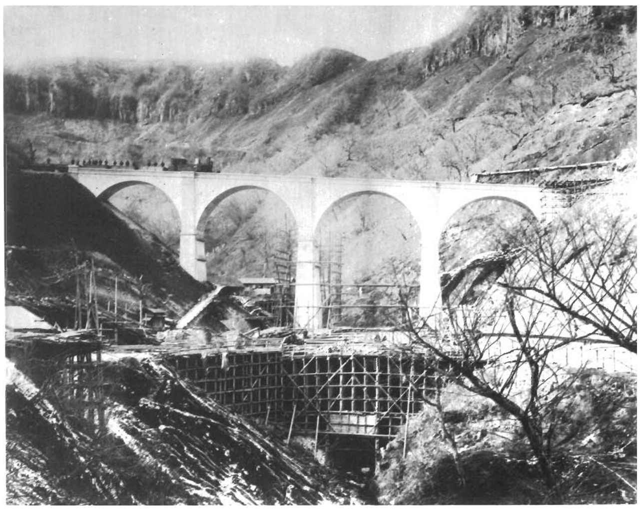
[27]In the statement by *Nihon Tetsudo*, Kyoto was specified as the destination of the line to the west from Tokyo. However, the government recognized that Shimonoseki was the important destination to the west. In 1887, to consider the proposal from the private sector to a line to Shimonoseki, Masaru Inoue stated in a document to the cabinet that the line to Shimonoseki is a part of prioritized network. In Table 1.A.12, I carry out robustness checks by excluding counties located between Kyoto and Shimonoseki.

Figure 1.7: Freight and Passenger Traffic by Railroad



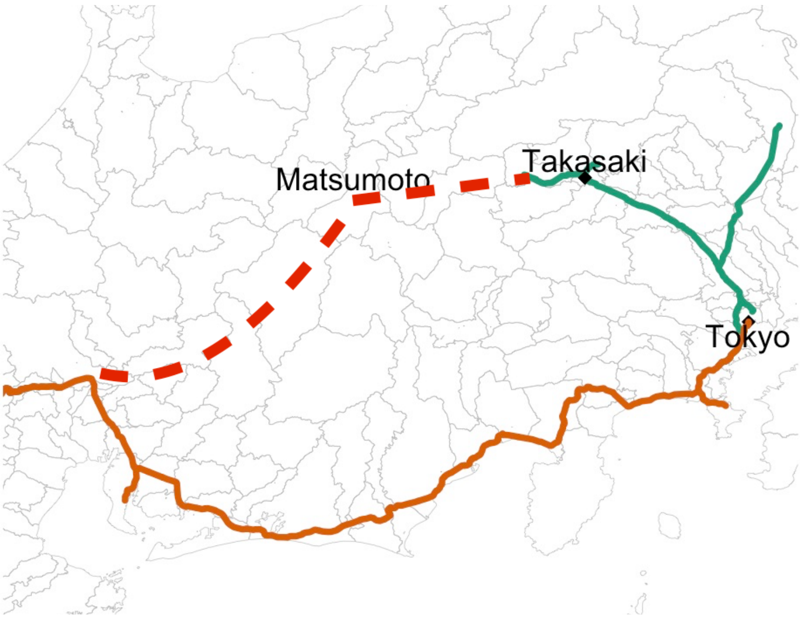

Source: *Meiji Iko Hompo Shuyo Keizai Tokei*(Nihon Ginko Tokeikyoku, 1997). In the left panel, the Y-axis depicts the total number of cars for freight (red and solid line) or passengers (blue and dashed line), divided by their level in 1890. In the right panel, the Y-axis shows million ton * km for freight and million passenger * km for passengers, divided by their level in 1890.

Figure 1.8: Construction Work in Steeper Areas (The *Usui* Pass)



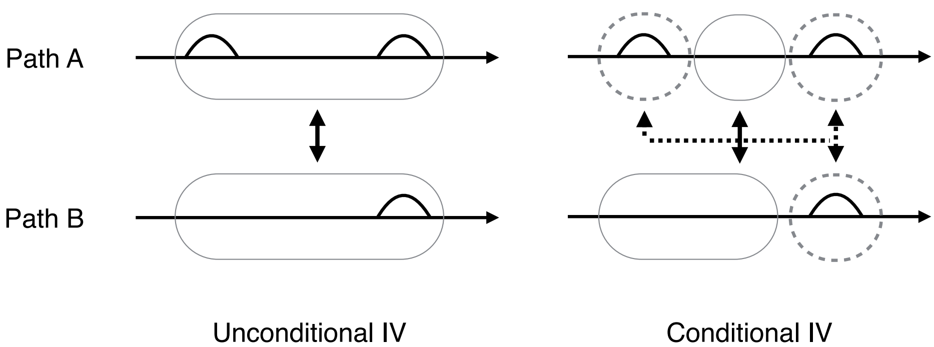
Source: *Nihon Kokuyu Tetsudo Hyakunen Shashinshi p44*

Figure 1.9: Abandoned Line due to Steepness




The green lines are those that existed in 1885. The red dotted lines depict the original plan to connect Tokyo with the west that was abandoned. The brown line depicts the line that subsequently connected Tokyo with the west.

Figure 1.10: IV Conditional on Local Geography



tralized government and the low level of technology in land transportation.[28] Therefore, conditional on local geography, the cost-minimizing path will not be associated with the economic variables before railroad construction started.

To further ensure quasi-random assignment, I estimate specifications including the initial population density and existence of traditional roads as additional controls. I show that these controls do not affect the results, suggesting that conditional on local geography, the instrument is plausibly exogenous. There may also be concerns related to the exclusion restriction. For example, the central government may allocate other kinds of public goods together with railroad construction. This would cause the effects of railroad access to be overestimated, although the reduced form will not be biased. To eliminate this possibility, I check for a relationship between the instrument and other kinds of public spending, such as investment in roads or irrigation. I also discuss other important policies and technological progresses during this time and argue that these do not affect the main result.

### Data

By using current railroad network data, I construct historical data for the rail network. The National Land Numerical Information download service[29] offers geographical data that include all railroad network and station data from 1950 onwards. However, it does not include the opening years for all railroads that opened before 1950 and therefore cannot be used to generate geographical data for the railroad network for each year of the sample period. Consequently, I use two additional official documentary sources, the *Nihon Kokuyu Tetsudo Hyakunenshi* (The History of Japan's National Railroad) and the *Nihon Tetsudoshi* (The History of Japanese Railroads) to retrieve the railroad opening years. After constructing the historical network data, I identify the year in which each county gained access to the railroad network.[30]

To calculate the cost-minimizing paths, I download 250 m mesh data for the average slopes from the National Land Numerical Information download service,[31] and calculate the cost-minimizing path using the algorithm in Dijkstra (1959).[32] My basic specification assumes that construction costs are a linear function of the average slope. Since the true

[28] Before railroads were constructed, marine transportation was the major transportation mode for long-distance freight.

[29] `http://nlftp.mlit.go.jp/ksj`.

[30] Throughout this paper, the variable railroad access is a dummy, which takes a value of one for a county-year when that county has any intersection with a 1 km buffer of railroad lines by the beginning of that year. I include the buffer since minor data issues lead some lines to pass outside the coast.

[31] For computational reasons, when rasterizing the mesh data, I set 0.002, which is around 1.7 km, as the mesh size.

[32] The Arcgis "cost" commands use this algorithm.

cost function is unknown, I also try a specification that includes a constant term and a specification that does not penalize a slope of less than 25 per mill. The results under these specifications are discussed in detail in the Appendix, but the form of the cost function does not affect the main results. After constructing the paths, I simply calculate the distance to each path from each county, which I then use as the cross-sectional variation of the instrument to predict growth in railroad access by 1892.

To control for the time-variant effect of traditional roads, I construct network data for traditional roads by connecting the towns that each route passed through with straight lines, referring to the *Nihon no Kaido Handbook* (Handbook of Japanese Traditional Roads). Although this is an approximation, since the towns are fairly close to one another, the measurement bias will be minimal at the county level. I also include a different dummy for the *Gokaido*, which are the five major routes specified and controlled by the Shogunate. The Shogunate directly specified checkpoints, pre-modern post stations, and lodging on the routes, and therefore these areas might have higher economic potential.

### 1.3.2 Steam Power and the Japanese Economy

#### Background

The diffusion of steam power to Japan was also gradual. In 1712, the British inventor Thomas Newcomen was the first to develop a practical steam engine.[33] James Watt, among others, subsequently attempted to improve the engine's thermal efficiency. In 1785, Edmund Cartwright used steam power as the basis for his power loom. Robert Stephenson later used steam to power locomotives on the Stockton and Darlington railway lines. Many industries thus began to use this new source of power.

In Japan, the Tokugawa Shogunate was the first to experiment with Western steam technology. In 1861, the Shogunate established a steam-powered metal-producing factory in Nagasaki. Throughout the 1860s, the Shogunate, local domains, and the Meiji government established similar factories in other industries such as textiles and mining (Minami, 1977); however, it was not until the 1880s that the private sector began to use steam engines.[34]

The adoption of steam power led to a significant increase in aggregate horsepower,

[33]The idea of steam power is not new. In the first century, Hero of Alexandria described a simple steam turbine known as the Aeolipile (Hero's engine).

[34]The fixed cost of adopting steam technology was significant, as I assume in the conceptual framework. In the pulp and paper industry, *Fuji Seishi*, the second-largest company at that time, estimated the cost of establishing a steam-powered factory as 32,919 Japanese yen in 1890. Unfortunately, it did not report any revenue. Nonetheless, by using the revenue of *Ouij Seishi* and relative production of the two companies, it estimated that of *Fuji Seishi* to be around 230,000 Japanese yen. The cost of this new factory would thus have been around 14.3 percent of annual revenue.

as can be seen in Figure 1.2. In 1884, the total horsepower was 1,051, which increased to 21,825 by 1890. In addition, as Figure 1.11 illustrates, the adoption of steam power showed a regional variation. Figure 1.2 also shows that technological progress in the agricultural sector, often considered to be the key factor behind structural change, did not occur during this period. Fertilizer consumption and total factor productivity in the agricultural sector did not increase as rapidly as horsepower did, and the next major technological breakthrough, a new high-yield variety of rice,[35] did not occur until 1910.[36] Consistent with these developments, Figure 1.3 shows that the relative price of industrial goods declined before 1940, particularly before 1900. Alvarez-Cuadrado and Poschke (2011) notes that this pattern can be observed for 11 countries that experienced structural change between 1840 and 1920. This fact implies that productivity growth is faster in the manufacturing sector than in the agricultural sector, consistent with the labor pull effect emphasized in Lewis (1954).

A stylized pattern of structural change and economic growth can also be seen. Agriculture's share of labor and value added decreased, while GDP per capita increased. Figure 1.4 shows the declining importance of the primary sector (agriculture and fisheries) after the 1880s. Figure 1.6 shows the growth in GDP per capita for Japan, with three other countries included for comparison purposes. Before 1868, both Japan and China lagged far behind the United States and the United Kingdom, Western countries that had already experienced industrial revolutions. After 1868, Japan began to grow, while China's growth was stagnant. Japan's catch-up period ended during the Second World War; however, it began to grow again afterward and surpassed the United Kingdom in 1980.[37]

The cross-sectional data also illustrate a regional pattern of technology adoption, structural change, and urbanization. Figure 1.12 plots the share of primary workers in a county against the log of total horsepower per factory worker and rice productivity per land. Both are negatively correlated with the primary industry's share of employment, with the slope being much steeper for log horsepower (on a standardized X-axis). This finding is consistent with labor pull rather than labor push. Figure 1.13 shows that a neg-

[35] The key scientific knowledge underlying this breakthrough, Mendelian inheritance, was re-discovered in 1900.

[36] Kiyokawa (1995) states that there were two other adoptions of major technology in agriculture. The first was a new type of cocoon that could grow after July. Its adoption rate was stagnant during the 1890s, although no data are available before this decade. Additionally, this cocoon does not belong to the agricultural sector as defined in most growth models, because there will be no subsistence level of consumption for this crop. Therefore, I interpret technology adoption in this industry similar to steam power adoption in the industrial sector, although most data do not treat it separately from non-cash crops such as rice. The second new technology was the non-human-powered agricultural machinery, although the adoption of this technology started in the 1900s.

[37] Hayashi and Prescott (2008) argues that pre-Second World War cultural factors explain this progress, while the war and resulting destruction of the country acted to constrain migration and hinder structural change.

ative correlation exists between the share of primary workers and the log of population density.

To summarize, there is a positive relationship between technology adoption in the industrial sector, structural change, urbanization, and economic growth from the 1880s onwards. This is true both in the time series and in the cross-sectional data. It is therefore important to examine the factors that induced technology adoption in the industrial sector.

### Data

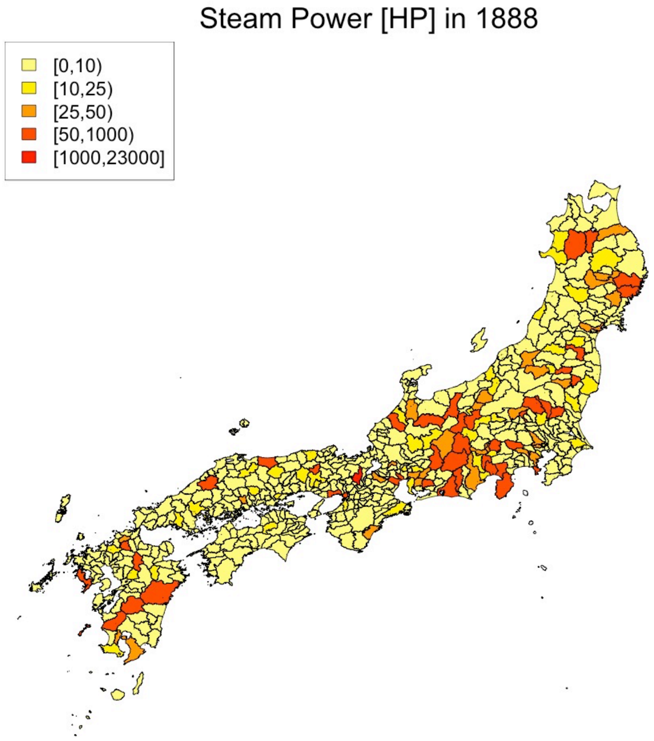
To capture technology adoption and economic development, I newly digitize three publication series and supplement these data with the two previously digitized series. I use two types of data: factory data, which allow me to observe technology adoption, and population data, which allow me to observe structural changes and urbanization.

To obtain factory data, I digitize two of the statistical series published by the central government, *Noshomu Tokeihyo* (Agriculture, Commerce and Manufacturing Statistics) and *Kojo Tsuran* (the Factory Catalog). These contain the total horsepower generated through steam power, the number of steam-powered factories, and the number of workers in steam-powered factories in 1888–1891, 1902, and 1919. These series also contain product information, which allows the data to be classified by industry. However, there is no reliable common identifier for individual factories, and thus I aggregate all the variables at the county level.

I digitize a similar local government publication, the *Huken Tokeisho* (Prefectural Statistics), from 1884 to 1902 to obtain the revenue, share capital, number of workers, product type, and location of each factory. Because this publication has many missing volumes, I do not use it in my main analysis. However, I use the profit data to check if the use of steam power is associated with improvements in revenue productivity.

For the population variables, I digitize several central government population publications: the *Nihon Teikoku Minseki Koguchi Hyo* (Table of Registered Population), *Nihon Teikoku Seitai Jinko Tokei* (Table of Population Statistics), and Population Census. I obtain population data for 1885, 1888–1898, 1903, 1908, and 1920. Data are also available for 1879, 1880, and 1882, which is before the IV affected railroad construction. However, these data use a slightly different definition for population, and thus I use it only for falsification test. The data for 1920 were collected by census, and they contain the number of workers in each sector. I use this data set to test for the effect of technology adoption on structural change. Before 1920, no central government publication recorded the number of workers in the agricultural sector. I obtain these data from a local government publication, the *Huken Tokeisho*, which recorded the number of workers in the agricultural sector around 1885. This is used as a control when testing for the effects on structural change.

Figure 1.11: Regional Pattern of Steam Power Usage between 1888 and 1902



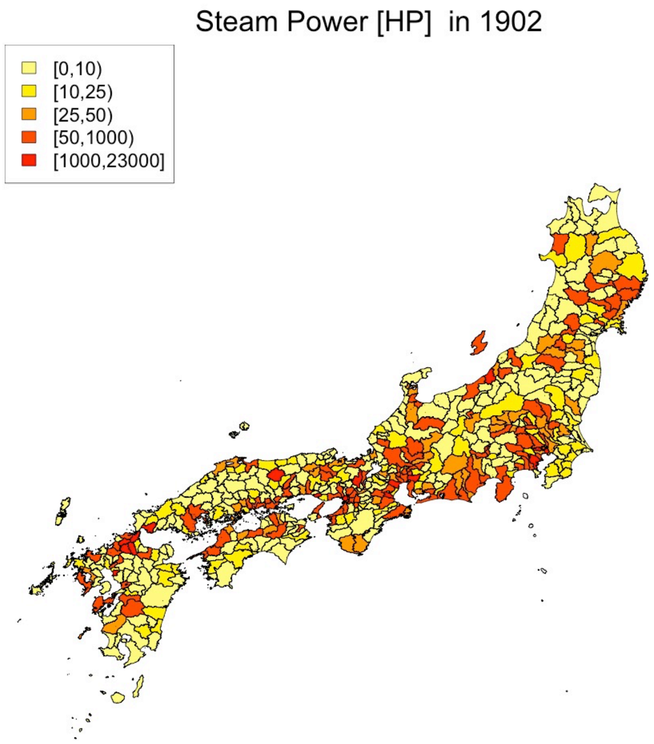
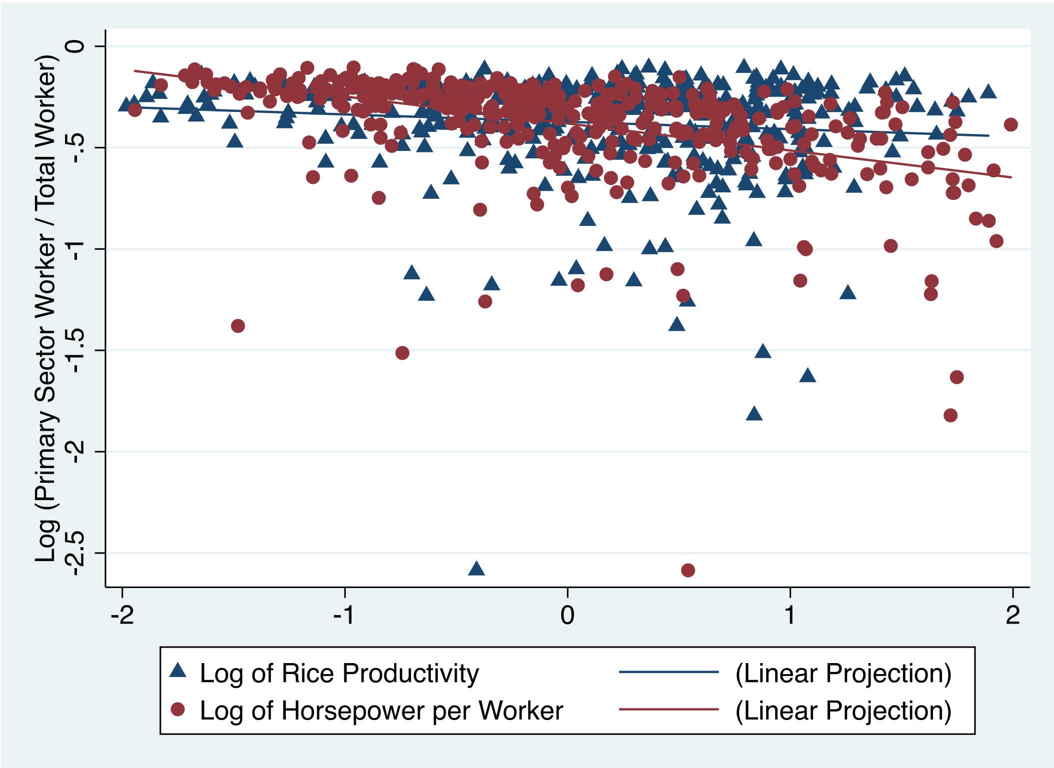
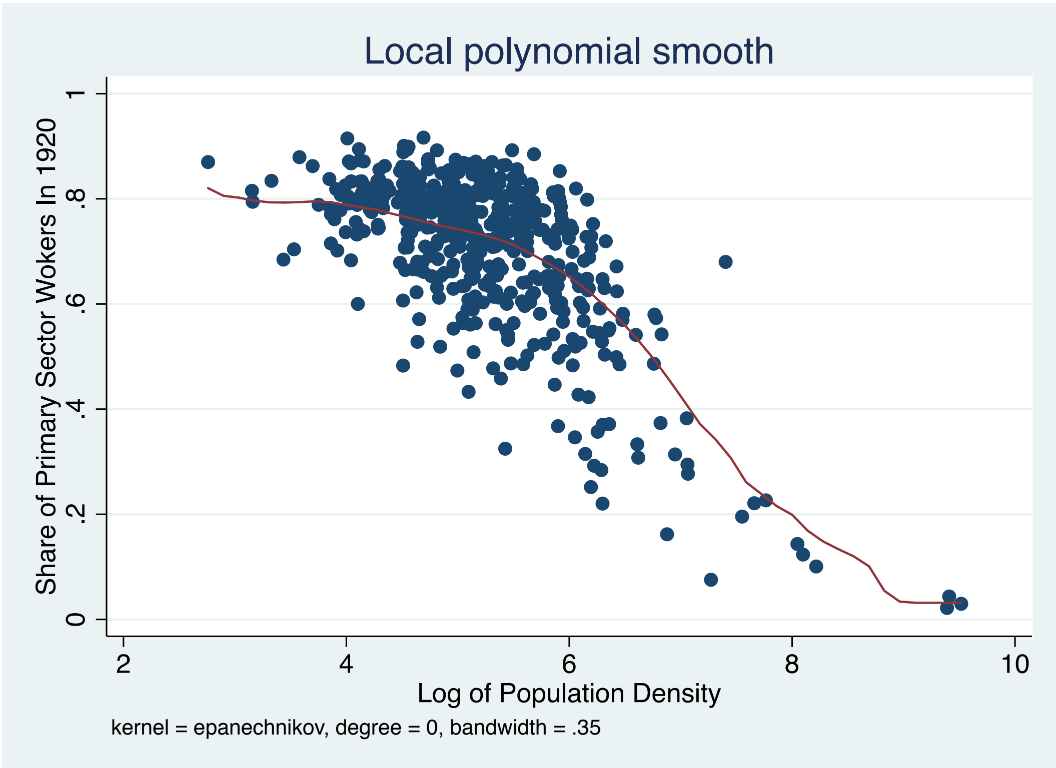



Figure 1.12: Technology in Factories and Primary Sector Employment Share in 1920




Notes and Source: Steam power data are from the 1919 Factory catalog and primary sector employment share data are from the 1920 census. To calculate total workers, I add agriculture, fisheries, manufacturing, and mining industry workers to divide the steam power and primary sector workers (agriculture and fisheries). Section 1.3.2 provides more detailed information on the data source. Rice productivity is the mean of the mesh-level crop suitability of rain-fed with low-fertilizer-type rice, obtained from the FAO GAEZ data. I use the same sample in the main analysis. For readability, the figure excludes samples with more than two standard deviations from the mean in either of the x variables.

Figure 1.13: Population Density and Primary Sector Employment Share in 1920




Notes and Source: Primary sector employment share is from the 1920 census. To calculate total workers, I sum agriculture, fisheries, manufacturing, and mining industry workers to divide the horsepower and primary sector workers (agriculture and fisheries). Section 1.3.2 provides more detailed information on the data source. The log of population density is also based on the Population Census.

### 1.3.3 Descriptive Statistics

Before moving onto the regression analyses, I show the descriptive statistics. Table 1.1 presents the descriptive statistics in three time periods: the initial stage in 1888, a middle stage in either 1902 or 1903, and an end stage in either 1919 or 1920.[38] Panel A shows that steam power was in limited use in 1888. Average total horsepower per county was 12.4 and the average number of steam-powered factories per county was only 0.58. There were two-thirds as many water-powered factories as there were steam-powered ones, but the former's total horsepower was 15 percent of that of the latter. Only 5.8 percent of the counties had railroad access. The average county population was 69,087 and the average population densioty was 245.84 per km$^2$. This level of population density is similar to that of Pakistan in 2014, which had a density of 240, and lower than that of Vietnam in 2014, where the density was 293.[39] Most counties had traditional roads, as shown in Figure 1.14, while the *Gokaido* did not reach the end points as the railroad network did after 1868.

By the middle stage, the average number of steam-powered factories had increased to 3.51 and the horsepower to 112.79, roughly 10 times that of the initial period. The number of water-powered factories had also increased, but their contribution to total horsepower was 22.62, much lower than that of steam-powered factories.[40] Population and railroad access both increased. About 64 percent of the counties now had railroad access.

In the end stage, electricity and gas emerged as new types of power sources. These new power sources were more important than steam power, in terms of both horsepower (621.57 vs. 1207.96) and the number of factories (8.17 vs. 20.1). I aggregate these two power sources when analyzing the long-term impacts later. The population also increased, with 79 percent of the counties having railroad access.[41]

## 1.4 Main Results

In this section, I first confirm that the introduction of steam power increases factory size and labor productivity by using the *Huken Tokeisho*. Then, I estimate the causal effect of railroad access on steam power adoption. I finally perform robustness checks and

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[38] Table 1.A.2 shows the relationship between the timing of obtaining railroad access county characteristics. Compared with counties that got railroad access before 1890, the late adopters tends to be less populated, far from the destinations, Tokyo or Osaka, main traditional roads.

[39] `http://data.worldbank.org/indicator/EN.POP.DNST`.

[40] The original data recorded the horsepower for "steam" and "other," not specifically water. However, given the context, it is appropriate to read this as water power. I therefore assume that this variable is consistent between the data in 1902 and before.

[41] To show the extent to which steam power had diffused in more detail, I present the descriptive statistics at the industry and factory levels in 1902, as in Table 1.A.3.

Figure 1.14: Traditional Roads Built before 1868

Source: Takeuchi (2006). The blue lines indicate traditional roads and the red lines are the *Gokaido*, namely the five major roads directly controlled by the Tokugawa Shogunate during the Edo period (1615–1868).

Table 1.1: Descriptive Statistics: County-level Data

| | mean | sd | min | max |
|---|---|---|---|---|
| *A. Initial Stage (1888)* | | | | |
| *1. Factory Data* | | | | |
| Steam Power [horsepower] | 12.24 | 63.27 | 0 | 878 |
| Water Power [horsepower] | 2.057 | 8.296 | 0 | 80 |
| N of Steam Power Engines | 1.209 | 7.158 | 0 | 110 |
| N of Steam-Powered Factory | .5770 | 2.471 | 0 | 36 |
| N of Workers in Steam-Powered Factory | 70.78 | 354.0 | 0 | 4917 |
| *2. Population Data* | | | | |
| Population | 68918 | 36176 | 13246 | 255985 |
| Population Density (/km2) | 243.6 | 665.9 | 10.60 | 9307.6 |
| *3. Other Data* | | | | |
| Railroad Access | .0616 | .2407 | 0 | 1 |
| Traditional Road | .8357 | .3709 | 0 | 1 |
| Main Traditional Road | .1376 | .3448 | 0 | 1 |
| *B. Middle Stage (1902 or 1903)* | | | | |
| *1. Factory Data* | | | | |
| Steam Power [horsepower] | 111.2 | 425.1 | 0 | 4107 |
| Water Power [horsepower] | 22.26 | 89.64 | 0 | 971 |
| N of Steam Power Engines | 5.924 | 14.45 | 0 | 155 |
| N of Steam-Powered Factory | 3.478 | 5.926 | 0 | 57 |
| N of Workers in Steam-Powered Factory | 336.7 | 740.1 | 0 | 7259 |
| *2. Population Data* | | | | |
| Population | 82831.4 | 45262.2 | 16275 | 439531 |
| Population Density (/km2) | 293.3 | 815.5 | 13.34 | 11160.8 |
| *3. Other Data* | | | | |
| Railroad Access | .6407 | .4803 | 0 | 1 |
| *C. End Stage (1919 or 1920)* | | | | |
| *1. Factory Data* | | | | |
| Steam Power [horsepower] | 621.6 | 2374.4 | 0 | 28129.5 |
| N of Steam Power Engines | 11.76 | 19.99 | 0 | 205 |
| N of Steam-Powered Factory | 8.172 | 14.90 | 0 | 186 |
| Elec. or Gas Power [horsepower] | 1208.0 | 8418.5 | 0 | 174151.9 |
| N of Elec. or Gas Power Engines | 46.79 | 130.75 | 0 | 1565 |
| N of Elec. or Gas-Powered Factory | 20.05 | 40.80 | 0 | 396 |
| N of Workers in Steam-Powered Factory | 1088.1 | 2319.0 | 0 | 26526 |
| *2. Population Data* | | | | |
| Population | 93255.3 | 76286.8 | 16364 | 1156973 |
| Population Density (/km2) | 342.61 | 1028.05 | 15.79 | 13599.4 |
| *3. Other Data* | | | | |
| Railroad Access | .7926 | .4058 | 0 | 1 |
| Observations | 487 | | | |

*Railroad Access* and *(Main) Traditional Road* are binary variables, which take the value of one if there are railroads or (main) traditional roads, respectively. In Panels B and C, only *Population Data* uses the data in 1903 and 1920, respectively, and *Factory Data* and *Other Data* use the data in 1902 and 1919, respectively.

heterogeneous impact analysis.

### 1.4.1 Preliminaries: Factory-level Analysis

To analyze the relationship between the introduction of steam power and factory size/labor productivity, I use the following specification:

$$Outcome_{it} = \alpha_1 Steam\ Power\ Fty._{it} + County * Year\ FE + County * Industry\ FE + u_{it} \quad , \tag{1.1}$$

where *Steam Power Fty.*$_{it}$ is a binary variable that takes a value of one if firm $i$ is using steam power at time $t$. Note that because the source data lack a common identifier for individual factories, I cannot include factory fixed effects. As an alternative, I include the interaction terms with the country and industry dummies.[42]

Table 1.2 presents the results for factory size and share capital. Columns (1) to (3) show the relationship between the adoption of steam power and number of workers. The estimates in column (1) indicate that factories that use steam power are roughly 58 percent larger than those that do not.[43] I find similar results for share capital, which is consistent with factories issuing stock to fund the adoption of new technology. Column (4) shows that steam-powered factories have 78 percent more share capital than those without steam power. Columns (5) and (6) show similar point estimates.

Table 1.3 shows the impact on labor productivity, measured by the log revenue per worker. Columns (1) to (3) use different combinations of fixed effects, and columns (4) to (6) add share capital as an additional control. Panel A uses all the available data. However, revenue is only observed for one year in some prefectures. In Panel B, I estimate the model by using the subset of prefectures where revenue data are available for multiple years. Both panels show a positive relationship between labor productivity and the use of steam power, even after controlling for share capital. In column (6), for example, steam-powered factories have 49.7 percent higher productivity than non-steam-powered ones have.

Overall, although these results are not free from selection bias, the introduction of steam power is associated with a larger number of workers, higher share capital, and higher labor productivity. All these results are consistent with the assumptions of this study.

[42] Only *Factory Catalog* has an industry classification. Hence, for the other data sources, I use product type to classify the factories.

[43] Atack et al. (2008a) estimates a similar equation by using U.S. data from 1880 and finds a coefficient of 1.281 for steam power and 0.760 for water power. When I include a water power dummy, both the estimated coefficients are smaller, but the difference between the two is similar.

Table 1.2: Factory-level Data: Steam Power and the Log of Workers or Share Capital

| | Log Workers | | | Log Share Capital | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Steam-Powered Fty. | 0.586*** | 0.593*** | 0.624*** | 0.787*** | 0.777*** | 0.731*** |
| | (0.0697) | (0.0651) | (0.0637) | (0.101) | (0.126) | (0.121) |
| County*Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County*Industry (First Digit) FE | No | Yes | No | No | Yes | No |
| County*Industry (Second Digit) FE | No | No | Yes | No | No | Yes |
| Observations | 7194 | 6299 | 6231 | 7427 | 6526 | 6478 |

The county-level cluster robust standard errors (SEs) are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *Steam-Powered Fty.* takes one if the factory is using steam power engines. Industries are classified into Chemical, Machines, Tobacco, Gas and Electricity, Spinning and Weaving, Metal, Mining, Food, and Miscellaneous as the *first digit*. There are 39 subcategories in the *second digit*.

Table 1.3: Factory-level Data: Steam Power and the Log of Labor Productivity

| | Log (Revenue / Labor) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Full Available Prefectures* | | | | | | |
| Steam-Powered Fty. | 0.847*** | 1.233*** | 0.767*** | 0.221** | 0.577*** | 0.410** |
| | (0.187) | (0.218) | (0.196) | (0.0760) | (0.108) | (0.143) |
| Log Share Capital/Workers | | | | 0.538*** | 0.504*** | 0.427*** |
| | | | | (0.0620) | (0.0771) | (0.0843) |
| *Panel B: Prefectures with Multiple Years* | | | | | | |
| Steam-Powered Fty. | 1.118*** | 1.235*** | 0.778*** | 0.417*** | 0.652*** | 0.497*** |
| | (0.205) | (0.248) | (0.163) | (0.108) | (0.0933) | (0.112) |
| Log Share Capital/Workers | | | | 0.490*** | 0.491*** | 0.414*** |
| | | | | (0.0764) | (0.0844) | (0.0880) |
| Observations in Panel A | 960 | 807 | 786 | 934 | 782 | 761 |
| Observations in Panel B | 724 | 724 | 724 | 724 | 724 | 724 |
| County*Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County*Industry (First Digit) FE | No | Yes | No | No | Yes | No |
| County*Industry (Second Digit) FE | No | No | Yes | No | No | Yes |

The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *Steam-Powered Fty.* takes one if the factory is using steam power engines. Panel A uses all the available sets of data, which uses the data in 1884–1893, while Panel B uses the data on prefectures only, which I can obtain for more than one time period. As a result, Panel B uses Osaka (1886–1888), Yamanashi (1885, 1887, 1888, and 1890), Hiroshima (1892–1893), Aichi (1886–1887), Shiga (1887–1888), Fukuoka (1887–1888), Nagasaki (1887–1890), and Tottori (1889–1891). See Table 1.2 for the description of the industry groups.

### 1.4.2 Main Result: Effect of Railroad on Technology Adoption in 1888–1902

Now, I analyze the impact of railroad access on technology adoption by using data from 1888 and 1902 or 1903.[44] Since the government constructs railroads endogenously, I rely on the IV explained in the previous section to estimate the causal impact of railroad access.

First, I examine the relationship between the actual railroad network and my instrument. Figure 1.15 plots both the railroad network and the cost-minimizing path, showing the relevance of the instrument graphically. To see how the relationship between the cost-minimizing and actual paths evolves over time, I non-parametrically estimate the relationship between the distance from the cost-minimizing path and railroad access in selected years. Figure 1.16 plots the resulting functions. In 1880, when only a small part of the railroad network had been constructed, there was no relationship. In 1888 and 1892, counties closer to the cost-minimizing path were more likely to have railroad access. Subsequently, governmental support of railway construction led to an increase in access for counties far from the cost-minimizing path. The line for 1902 shows that between 1892 and 1902, the same amount of railroad was constructed across all counties. By 1919, the gap in railroad access had disappeared and the function was flat. In Table 1.A.4, I show that this relationship is true even after including the controls.

I next estimate the impact of railroad access on various outcomes, using the cost-minimizing path as an instrument, as seen in Figure 1.16. The main specification is

$$\begin{aligned}
\textit{Outcome}_{it} =& \alpha_1 \textit{Railroad Access (-1892)}_{it} \\
&+ \textit{Local Geographical Controls}_i * \textit{Year FE} \\
&+ (\textit{Local Other Controls}_i * \textit{Year FE}) \\
&+ \textit{Year FE} + \textit{County FE} + e_{it} \\
\textit{Railroad Access (-1892)}_{it} =& \textit{Distance to the Path}_i * \textit{Year FE} \\
&+ \textit{Local Geographical Controls}_i * \textit{Year FE} \\
&+ (\textit{Local Other Controls}_i * \textit{Year FE}) \\
&+ \textit{Year FE} + \textit{County FE} + e_{it}
\end{aligned} \tag{1.2}$$

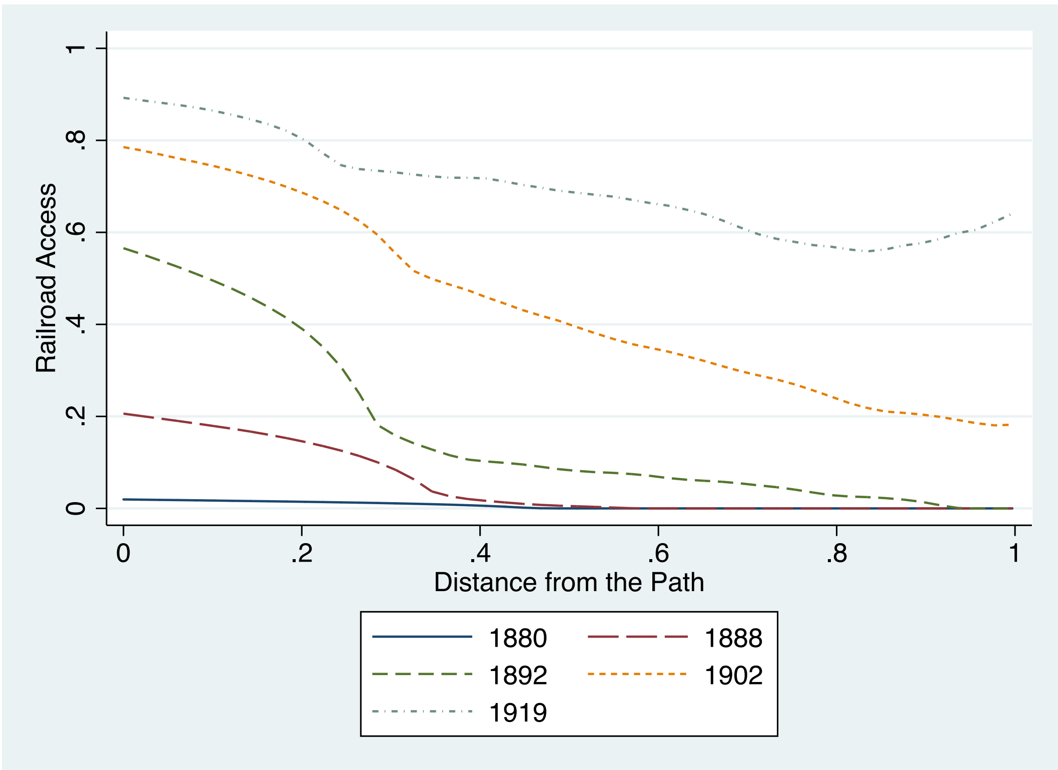
where *Railroad Access (-1892)*$_{it}$ is a binary variable. When $t \leq 1892$, it takes a value of one if county $i$ had railroad access in year $t$. When $t > 1892$, it takes the value it had at $t = 1892$. The treatment variable is defined differently from in the previous analysis. I

[44] I focus on the medium-term effect here, as I expect the effect of railroads to permeate gradually. This is because my outcome variable is the result of investment, unlike R&D expenditure.

Figure 1.15: Railroad Network in 1892 and Cost-Minimizing Paths


The green lines represent the railroad network in 1892 and the red lines depict the cost-minimizing path.

Figure 1.16: Unconditional Relationship Between IV and Railroad Access



construct it in this way since my instrument is related to railroad construction before 1892. This means that the treatment effect should be interpreted as the effect of having access to the railroad network before 1892, which will be less than the simple unconditional effect of railroad access.[45][46]

Table 1.4 presents the estimates of the effect of railroad access before 1892 on the adoption of steam power in a county. The top row shows the first stage result that if a county is 100km away from the path, it will get the access to the railroad between 1888 and 1892 about 30 percent point less likely than a county on the path. Panel A contains the estimated effect on steam power measured by its horsepower, with column (2) adding the initial population density and presence of traditional or main traditional roads, both of which are correlated with the instrument in the cross section as additional controls. In columns (3) and (4), I add the distance to the destinations as an additional control. The point estimate is around 300 in column (1), and the result is robust by including an additional control in columns (2) to (4). To understand the magnitude of the effect, I multiply the effect by the number of counties that obtained railroad access between 1888 and 1892 and find that the growth in railroad access between 1888 and 1892 accounts for 67 percent of the growth in steam power between 1888 and 1902. In Panel B, I use a slightly different measure of technology adoption, the number of steam power engines. In all specifications, the coefficients are significantly positive and the point estimates do not change substantially over the different sets of controls.[47] The point estimates are around 21 and through a similar calculation, the growth in railroad access between 1888 and 1892 accounts for 97 percent of the growth in the number of steam engines between 1888 and 1902. Although this calculation assumes that LATE is equal to ATT and ignores general equilibrium effects, overall, a large amount of technology adoption is explained by more widespread railroad access.

### 1.4.3 Robustness Checks

First, I analyze the direction of the bias of OLS. As shown in Table 1.A.6 in the Appendix, I perform the same regression analysis with Table 1.4 without the use of IVs. I

[45] I choose the distance from the cost-minimizing path as my instrument, rather than a binary variable taking a value of one for counties on the path, as the fit of the first stage is better. Figure 1.16 shows that the relationship is close to linear in 1892.

[46] This "hypothetical" path is a better instrument than government plans, which are commonly used in the literature, because it is constructed by using only geographical information, whereas government plans may depend on other economic or political factors. After controlling for local geography, the only variation in my instrument is whether the county lies on the cost-minimizing path, implying that the quasi-random assignment and exclusion restrictions are likely to be satisfied.

[47] Because the instrument is essentially cross-sectional, I also show the result using the outcomes in 1902 and railroad access in 1892 without using the panel structure of the data in Table 1.A.5. The results are robust with this modification, though point estimates of railroad effects decline.

Table 1.4: Main Result: Effect of Railroad Access on Technology Adoption in 1888–1902

| | DID-IV | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *First Stage: Railroad Access Dummy* | | | | |
| Distance from Path (100km) * 1892 | -0.308*** | -0.288*** | -0.313*** | -0.292*** |
| | (0.0477) | (0.0467) | (0.0487) | (0.0476) |
| *Panel A: Steam Power [HP]* | | | | |
| Railroad (-1892) | 299.0** | 304.3** | 256.3** | 255.5** |
| | (102.0) | (110.6) | (89.07) | (93.49) |
| *Panel B: Number of Steam Engines* | | | | |
| Railroad (-1892) | 21.16*** | 20.59** | 19.11*** | 18.25** |
| | (6.015) | (6.526) | (5.404) | (5.766) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | No | Yes | No | Yes |
| (Main) Traditional Road * Year FE | No | Yes | No | Yes |
| Dist. to Destinations * Year FE | No | No | Yes | Yes |
| First KP-stat | 41.52 | 38.15 | 40.91 | 36.48 |
| N | 956 | 956 | 956 | 956 |
| N County | 487 | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use the data in 1888 and 1902.

find that the OLS estimate is biased downwards, possibly because of the government favoring less developed areas. Agrawal et al. (2016) notes that this is a typical result in the transportation literature. Alternatively, the result may be due to the difference between ATT and LATE. The instrument captures the main lines for long-distance transportation, not branches, and these main lines may be better connected and offer better services than branches do.

In Table 1.A.7, I investigate the effect of railroad access on other outcomes, namely the number of (steam-powered) factories, proportion of the population working in (steam-powered) factories, and population, using the same sets of specifications as in Table 1.4. The results indicate that railroad access increased the total number of steam factories, population, and proportion of workers employed by steam-powered factories, but not the total number of factories or the proportion of workers employed in any factory, although some results are not significant in some specifications. This finding is consistent with railroad access accelerating technology adoption, but not entry into factory-based production.[48][49]

We can use the result on the number of steam-powered factories to study the effect of railroads on the productivity of factories. The results in column (6) of Table 1.3 imply that steam power raises productivity by 49.7 percent.[50] The estimates in column (1) of Panels A and B in Table 1.A.7 depict that growth in railroad access between 1888 and 1892 accounted for 29.5 percent point of the proportion of steam-powered factories in 1902. Together, these estimates imply that access to a railroad network increases factory productivity by 16.2 percent. This figure does not correct for selection effects, and therefore should be treated as an upper bound. However, it provides a rough estimate of the effect of railroads on productivity.[51][52]

[48] Although the proportion of people working in factories did not increase, this is not a measure of structural change. Matsumoto and Okuda (1997) and Nakamura (1971) note that there is a non-factory-based sector connected with the factory-based sector.

[49] As a robustness check, I include prefecture-level geographical controls and cluster robust SEs at the prefecture level, as county-level geographical controls may be too restrictive. Table 1.A.8 shows the results. The general pattern is similar to the results without prefecture-level controls.

[50] This is a comparison between steam power and non-steam power. When I include the water power factory dummy in the analysis, the difference between water power and steam power is 30 percent and that between human power and steam power remains at 50 percent, with both effects significant. I use a simple steam versus non-steam comparison because water power is only feasible near steep rivers and is unsuitable for industries that require a constant amount of energy input. Atack et al. (2008a) finds that the difference between steam power and water power, in terms of log revenue, for establishments with more than 16 employees is 0.181 without controlling for capital per worker, and 0.201 when controlling for capital per worker. The estimate with capital per worker is similar to my result. However, capital in Atack et al. (2008a) is defined as the actual capital invested, whereas capital in the present study refers to share capital.

[51] Bias may also arise because of the use of revenue productivity rather than physical productivity. Productivity will increase following the adoption of new technology, but a monopolistically competitive firm will reduce prices in response to the decrease in marginal costs and this will have a negative effect on revenue productivity. However, separating these effects is impossible in my data.

[52] Minami (1979) examines the adoption of the power loom by the Japanese textile industry and its effect on industry productivity. By using the cross-sectional differences in the adoption rate in 1924, he finds that

I next perform a falsification test to see the cross-sectional correlation between the instrument and pre-railroad characteristics in **??**. Panel A shows the correlation conditional on the same set of geographical controls in column (1) of Table 1.4. I only find correlations between the instrument and traditional road dummies, but the main results do not change by controlling them as we saw in Table 1.4. I see another falsification test to examine the pre-trend of economic growth, which is more relevant for DID analysis, my headline specification. Between 1879 and 1880, no county gained access to the railroad network,[53] and between 1879 and 1882, only six counties gained access. If the instrument is quasi-randomly assigned, it should have no impact on the population in this period. Panel B in Table 1.5 presents the estimation results. The dependent variable is population growth in 1879–1880 in columns (1) and (2) and 1879–1882 in columns (3) and (4). Similar to the main results, I estimate all models with and without additional non-geographical controls.[54] I find no significant correlation in any columns. This is not due to small sample size; When normalizing the population growth and the IV, the standard error becomes 0.04 in column (1) and 0.05 in column (3), so the coefficients are precisely estimated. The IV is not correlated with economic growth proxied by population growth before it could start to be affected by railroad construction.

I now test for the exclusion restriction. This restriction would be violated if being on the cost-minimizing path affects the outcomes through some channel other than railroad access. I test it informally by checking whether there is any effect of the IV on a potential channel, which might affect the outcomes. Specifically, I use prefecture-level data on government infrastructure spending, as the government may increase spending on other forms of public infrastructure such as roads and irrigation at the same time as constructing the railroad. I digitize its data from the Home Ministry's Statistical Yearbook and estimate the following specification.

in the cotton industry, a one percentage point increase in the adoption of the power loom led to an increase in labor productivity of 0.91 percent (0.59 percent conditional on capital per worker); in the silk industry, the estimated effect is 0.57 percent (0.12 percent conditional on capital per worker). However, by 1920 the dominant source of power was electricity, meaning that the effect of the power loom will not be the same as the effect of steam power.

[53] At this point, there were already short lines in the Tokyo and Osaka areas.

[54] Since I control for log area, log population is the same as log population density.

Table 1.5: Falsification Test: IV and Population, Traditional Road Access, and Population Growth

| | Log Population | | Traditional Road | |
|---|---|---|---|---|
| | 1879 | 1882 | All | Main |
| Panel A: *Cross-sectional Correlation* | (1) | (2) | (3) | (4) |
| Distance from Path (100km) | 0.120 | 0.108 | -0.123* | -0.0906+ |
| | (0.0785) | (0.0803) | (0.0572) | (0.0477) |
| Geographical Controls | Yes | Yes | Yes | Yes |
| N | 478 | 478 | 478 | 478 |

| | Log Population | | | |
|---|---|---|---|---|
| | 1879-1880 | | 1879-1882 | |
| Panel B: *Pre-trend Check* | (1) | (2) | (3) | (4) |
| Distance from the Path * 1879 | 0.00179 | 0.00144 | 0.0116 | 0.0144 |
| | (0.00169) | (0.00165) | (0.0210) | (0.0205) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Geographical Controls * Year FE | Yes | Yes | Yes | Yes |
| (Main) Traditional Road * Year FE | No | Yes | No | Yes |
| Initial Log Population * Year FE | No | Yes | No | Yes |
| N | 956 | 956 | 956 | 956 |
| N County | 478 | 478 | 478 | 478 |

*N* shows the sample size and *N County* shows the number of counties. The robust SEs are in parentheses in Panel A and the county-level cluster robust SEs are in parentheses in Panel B + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The sample size is different from that in the other analyses because I adjusted the border change before 1884 and also excluded two outliers ($\Delta$*Log Population* in 1879–1880 was 0.65 and 0.7, while the 99th percentile is 0.1) possibly due to measurement error.

$$\begin{aligned}
&\textit{Outcome}_{it} && (1.3)\\
&= \alpha_1 \textit{Prop of Railroad Access County (-1892)}_{it}\\
&+\textit{Prefecture Geographical Controls}_i * \textit{Year FE}\\
&+\textit{Year FE} + \textit{County FE} + e_{it}\\
&\textit{Prop of Railroad Access County (-1892)}_{it} && (1.4)\\
&= \textit{Ave. Distance from the Path}_i * \textit{Year Trend (-1892)}\\
&+\textit{Prefecture Geographical Controls}_i * \textit{Year FE}\\
&+\textit{Year FE} + \textit{County FE} + e_{it}
\end{aligned}$$

$Outcome_{it}$ is public infrastructure spending in prefecture $i$ for year $t$. For $t \leq 1892$, *Prop of Railroad Access County (-1892)* $_{it}$ is the proportion of counties with railroad access in prefecture $i$ for year $t$, while $t > 1892$ it takes its value at $t = 1892$. The same controls used in the county-level analysis, *Prefecture Geographical Controls*$_i$ and *Prefecture Geographical Controls*$_i$ are recalculated at the prefecture level. *Ave. Distance from the Path* $_i$ is the average of the distance from the cost-minimizing path, taken across counties in prefecture $i$. Instead of using year fixed effects, I use *Year Trend (-1892)*, which is the linear trend for $t \leq 1892$ and 1892 afterwards to be interacted with *Ave. Distance from the Path*$_i$ to increase the F-value in the first stage.

Table 1.6 shows the results. Columns (1) and (3) do not use any instrument for the railroad variable, while columns (2) and (4) do. Columns (1) and (2) use levels, while columns (3) and (4) use a log specification. The point estimates are negative and insignificant at the 10 percent level. Furthermore, the mean of *Public Expenditure*$_{it}$ is 514.0, meaning that the point estimates in columns (1) and (2) are not significant. Hence, it does not appear that the exclusion restriction is violated by public spending.

Further robustness checks are presented in the Appendix. In Table 1.A.9 and Table 1.A.10, I check for robustness to the choice of the cost function used to calculate the cost-minimizing path. In Table 1.A.11, I control for the distance to the destinations and exclude counties adjacent to the destinations to exclude the potential spillover effect from the destinations. In Table 1.A.12, I see if dropping subsamples affects the main results. The main results are robust to these modifications. In addition, Redding and Turner (2015) points out that the estimated treatment effect may just be a displacement effect. In other words, the estimated effect in the main result overestimates the effect because of negative externalities. To test this possibility, I first use a different treatment variable, namely *Railroad (-1892) within 50 km*$_{it}$, which takes one if *Railroad (-1892)*$_{it}$ takes one or *Railroad*

*(-1892)*$_{jt}$ of any neighboring counties within 50 km takes one. Of the counties that switch according to the new treatment variable between 1888 and 1902, 29 percent do so because of their neighbor's access. Hence, we see a huge drop in the coefficient when the displacement effect is large. However, when comparing columns (1) and (2) in Table 1.A.13, we do not see a huge difference in the effect on steam power. The result is similar in columns (4) and (5), using the number of steam engines, suggesting no displacement effect. I also assess the effect of having railroad access only in neighboring counties to capture local externalities directly. In columns (3) and (6), I use *Neighbor's Railroad within 50 km*$_{it}$, which takes one if *Railroad (-1892) within 50 km*$_{it}$ takes one and *Railroad (-1892)*$_{it}$ takes zero. I instrument this by *Adjacent to the Path*$_i$$*$ *Year FE*$_t$, where *Adjacent to the Path*$_i$ takes one if *Distance to the Path*$_i$ takes from 20 km to 50 km.[55] If there is a displacement effect, its coefficient should show a negative sign; however, columns (3) and (6) show no significant effects. Similarly, I use a prefecture-level treatment variable which takes one if any of the counties in a prefecture has railroad access. As in the case of including neighborhoods as treated group, we will see lower point estimates for prefecture-level estimates if there is displacement effect. However, Table 1.A.14 shows very similar estimates with column (1) and (2) in Table 1.4, which does not support displacement effect again. [56]

Another way in which to see the displacement effect would be to compare the counties on Shikoku Island, the island in the south without any destination shown in Figure 1.15, with the counties in the other three islands far from the path. If a displacement effect existed and only occurred within the islands, counties on Shikoku Island should have higher growth than counties far from the path on the other islands. I analyze this point in Table 1.A.16, finding no significant difference between them. Of course, displacement may occur across islands, but this is difficult to characterize without knowing the form of the entrepreneur's entry decision problem and difficult to observe in a small area such as Japan. In Table 1.A.17, I analyze which variables are affected in the short term, finding an effect on steam power, although its magnitude is much smaller than in the main result as expected.

I now investigate whether channels other than market access are responsible for the main results. The difference between the migration and market access channels is in the timing of the effects. Table 1.A.17 shows the short-term reduced-form effect of railroad access on technology adoption and the population. In the short term, the only significant

[55] Between 1888 and 1892, counties in this region were 3 percent (insignificant) more likely to gain railroad access than counties more than 50 km from the path, suggesting minimal bias from the direct impact of railroad access.

[56] This is related with the results in Tang (2014). See subsection 1.A.10 and Table 1.A.15.

Table 1.6: Exclusion Restriction: Does the Path Increase Other Public Goods?

| | Public Expenditure | | Log Public Expenditure | |
|---|---|---|---|---|
| | OLS | IV | OLS | IV |
| | (1) | (2) | (3) | (4) |
| Railroad County Prop (-1892) | -51740.7 | 80865.5 | -0.388 | -0.216 |
| | (123228.2) | (208140.6) | (0.239) | (0.527) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | Yes | Yes | Yes | Yes |
| Coastal * Year FE | Yes | Yes | Yes | Yes |
| (Main) Old Road * Year FE | Yes | Yes | Yes | Yes |
| First KP-stat | | 14.05 | | 14.09 |
| N | 504 | 504 | 490 | 490 |
| N Pref | 36 | 36 | 36 | 36 |

*N* shows the sample size and *N Pref* shows the number of prefectures. Prefecture-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use the data from 1889 to 1902. I excluded Tokyo, Osaka, Kyoto, Kanagawa, and Hyogo as outliers, but the qualitative results do not change by excluding them. *Public Expenditure* is in yen and the mean of *Public Expenditure* of the sample in this analysis is $514,013$.

effect is on technology adoption in Panel A and not on the population in Panel B.[57][58] Table 1.A.18 investigates whether the short-term impact is heterogeneous. The results imply that there was no urban-to-rural migration by railroad. Overall, these results indicate that migration due to railroad access did not occur before or at the same time as technology adoption, which is inconsistent with the migration channel being the main channel.

To examine whether the cost channel is driving my results as well as the heterogeneity of the effect of railroad access between rural and urban areas, I estimate a model allowing for heterogeneous impacts. Table 1.7 shows the results. Columns (1) to (3) present the estimates of the short-term effects using annual data from 1888 to 1891, and columns (4) to (6) present the estimates of the medium-term effects using data from 1888 and 1902, as shown in the main results. In both cases, the estimates indicate that the effect was stronger in counties with a higher population density. This finding is consistent with the presence of credit constraints in rural areas, and it implies that the distance from Tokyo and Osaka is not important, which is not what we would observe if the cost channel was important. In Table 1.A.19 of the Appendix, I show that the distance from areas where coal is produced is not significant, meaning that the coal price channel is not the main driving force.

Another possible channel is the information channel. The results in Conley and Udry (2010), Agrawal et al. (2016), and Catalini et al. (2016) suggest that introducing railroads to an area may transmit information such as the existence of steam engines and the profitability of the firms using them. I test this channel by looking at the impact of railroads on the number and total days of expositions held in a prefecture.[59] Expositions played a major role in sharing in the industrial sector (Kiyokawa, 1995), and railroads may have increased expositions by reducing the transportation costs incurred by speakers. By using the same specification as Table 1.6, Table 1.A.20 shows no effect of railroads on expositions. Further, factories often adopted steam power even without access to a railroad, which suggests that the effect through this channel is minor. In 1888, 101 counties contained at least one steam-powered factory and 82 of them did not have railroad access, implying that the information channel is not working in present context. Another po-

[57] Hornung (2012) estimates the effect of railroads on population growth for cities in Prussia from 1840 to 1864 by using a straight line between the cities as an instrument for the railroad. His estimates are similar to the ones presented here.

[58] Kim (1995) estimates the impact of steam power adoption on the location of firms in the United States between 1850 and 1880 and finds the effect to be negligible. The analysis in this case is, however, limited to a simple cross-sectional regression, with no IV or natural experiment. Atack et al. (2010) estimates the effect of railroads on population density and the rural population in the Midwest United States between 1850 and 1860 and finds a roughly 3 percent increase in both. In this case, the initial estimates are obtained by using a DID estimator and further estimates are obtained by using a congressional survey as an instrument. The results of the IV estimation imply that the DID is biased downwards, although the IV estimates are imprecise.

[59] We cannot observe more informal communication outside the expositions, which railroads might affect.

Table 1.7: Heterogeneous Impact Analysis on the Steam Power of the Non-Mining Industry

| | 1888-1891 | | | 1888-1902 | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Railroad (-1892) | 23.41 | $35.72^{+}$ | 20.94 | 83.39 | 83.96 | 62.50 |
| | (20.51) | (21.63) | (20.44) | (58.56) | (62.46) | (65.61) |
| Railroad * Initial Population Density | $44.77^{*}$ | 36.48 | $56.16^{*}$ | 129.6 | 148.6 | 120.6 |
| | (22.80) | (22.21) | (26.80) | (84.53) | (97.20) | (79.43) |
| Railroad * Distance to Tokyo | | 18.76 | | | -65.17 | |
| | | (20.42) | | | (83.43) | |
| Railroad * Distance to Tokyo or Osaka | | | 26.20 | | | -16.35 |
| | | | (19.21) | | | (58.71) |
| County FE and Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| (Main) Traditional Road * Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Dist. to Tokyo * Year FE | No | Yes | No | No | Yes | No |
| Dist. to Tokyo or Osaka * Year FE | No | No | Yes | No | No | Yes |
| First KP Stat | 6.835 | 3.521 | 5.726 | 20.42 | 6.718 | 7.124 |
| N | 1948 | 1948 | 1948 | 974 | 974 | 974 |
| N County | 487 | 487 | 487 | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use steam power in the non-mining industry because the mining industry occupies the largest share of horsepower, but its location is sensitive to the natural resource location, which is far from Tokyo or Osaka. This means that the coefficient of the interaction term for distance will be misleading if we include this industry. Because of the low F-value in the first stage, I estimate the same model with LIML as well, but the results do not change qualitatively.

tential channel is within-city commuting, as suggested by Duranton and Turner (2012). However, commuting by train would have been unaffordable for most workers. The average daily wage in 1902 was between 0.3 and 0.7 yen,[60] while the cheapest ticket from Tokyo (Shimbashi) to Yokohama, a commute of 30 km and 45 minutes, was 0.3 yen.[61] Therefore, the effect of within-region commuting is most likely to be small.

Now, to see whether the effect of railroads is well explained by trade cost reduction, I use a part of the line connecting Osaka and Shimonoseki, going through Sanyo-San'in region. In this region, railroads were constructed along the Seto Inland Sea, which is a calm sea and suitable for marine transportation. Even after railroad construction, trade cost from Osaka to Shimonoseki did not decrease well. In 1892, a newspaper reported that between the Seto Inland Sea has very calm tide and wave and the most frequent marine transportation service in the nation, the fee and speed of marine transportation there are better than those of railroad, and people use marine transportation monopolistically (Nihon Kokuyu Tetsudo, 1969). If trade cost is a key, we will see a lower effect of railroads for this section. Using similar specifications as in Table 1.4, Table 1.8 shows the heterogeneous impact analysis. The interaction terms have negative signs and almost cancel the main effects, though the significance of the interaction terms in Panel A is not high. Though we cannot distinguish the market access, cost of adoption, and information channels from this result, this is suggestive evidence that trade cost reduction, not the direct demand effect by railroad construction works, was a driving force of the railroad effect.[62]

Though it is hard to identify the operating channel precisely, the results presented so far suggests that the market access channel is likely to be the main force behind the effect of railroad access. It is also possible to discern whether the incentive to adopt new technology is driven by improved access to domestic or foreign markets. If domestic market integration is important, then railroad access should affect purely domestic industries. I focus on two domestic industries, brewing and printing, both of which have low export shares as they produce goods for the domestic market.[63] I then use steam power in these

[60]This estimate is obtained from Umemura et al. (1965).

[61]See Nihon Kokuyu Tetsudo (1969).

[62]Another possibility is to test market access channel is to construct market size explicitly as in Donaldson and Hornbeck (2016) based on a structural model. This approach does not work in my context because it is hard to construct a structural model to incorporate technology adoption with multiple asymmetric regions and an undulating geography and the lack of complete trade cost data before railroads both will prevent us from creating a meaningful measure of market size.

[63]I consider industries with more than 200 factories (2.6 percent of the total) in 1902, use estimates by Taguchi (1994) to calculate the export share of each, and select the industries with an export share of less than 10 percent. I construct the brewing industry as an aggregate of miso, soy sauce, sake, and beer manufacturers. The export share of this aggregate is 2.5 percent. No exports were recorded for printing. The tobacco industry also had an export share of 2.5 percent, but this was monopolized by the government in 1898, so I exclude it. The explosive material, mining, ceramic, and textile industries had more than 200 factories, but were not categorized as domestic.

Table 1.8: Heterogeneous Impact Analysis in Sanyo-San'in Region

| | DID-IV | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A: Steam Power [HP]* | | | | |
| Railroad (-1892) | 329.3* | 344.9* | 273.8* | 280.2* |
| | (138.6) | (159.4) | (113.8) | (128.9) |
| Railroad (-1892) * Sanyo-San'in Region | -447.0 | -467.9 | -415.7 | -424.5 |
| | (320.6) | (322.6) | (328.6) | (331.0) |
| *Panel B: Number of Steam Engines* | | | | |
| Railroad (-1892) | 26.80*** | 27.23** | 25.02*** | 25.12** |
| | (7.721) | (8.931) | (7.240) | (8.340) |
| Railroad (-1892) * Sanyo-San'in Region | -25.79** | -26.28** | -24.79** | -24.87** |
| | (8.401) | (9.489) | (8.262) | (9.313) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | No | Yes | No | Yes |
| (Main) Traditional Road * Year FE | No | Yes | No | Yes |
| Dist. to Destinations * Year FE | No | No | Yes | Yes |
| Sanyo-San'in FE * Year FE | Yes | Yes | Yes | Yes |
| First_F | 18.83 | 16.67 | 16.65 | 14.89 |
| N | 974 | 974 | 974 | 974 |
| N_g | 487 | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument.

industries as an additional outcome. I also show the results from the mining and textile industries, both export industries, for comparison purposes.[64]

Table 1.A.21 shows the estimates of the industry-specific effects. The specification is the same as that in column (1) of Table 1.4. Column (1) shows the result for the domestic industry, which I consider to be significant.[65] The other columns show the results for the mining and textile industries. The effect on the mining industry is huge and accounts for roughly two-thirds of the overall effect. The estimated effect for the textile industry, shown in column (4), is positive but insignificant. I also show the coefficient divided by the standard deviation of the outcomes in the row below the SE. These results indicate that the effect of railroad access affects both the domestic and the export industries in a similar extent.[66] The effect on the export industry will have an implication on external validity. If a country has a comparative advantage in the agricultural sector, then its railroads may affect that sector rather than the industrial sector.

I further consider other important policies or technological progress in my sample period to discuss whether these might affect my main result.[67] For the policies, I consider modern post offices, telegraphs, and telephones to be important upgrades in communication infrastructure after the Meiji Restoration because communication costs might affect technology adoption and the government might have a similar networking motivation for communication infrastructure as for transportation infrastructure. The modern post office was introduced in 1872 and covered the entire nation in 1873 in cooperation with village leaders, meaning that it will not affect my identification strategy. Similarly, the telegraph was introduced in 1869 between Tokyo and Yokohama and started to expand in 1874. By 1878, most of the capital cities of the prefectures had obtained telegraph stations, meaning that this would also not affect the main result.[68] The telephone network spread in 1898,

[64]In total, these two industries account for 50–60 percent of the total value of exports in the 1880s and 1900s. In the data from 1909, both industries also exhibit a high export rate, defined as the value of exports divided by the value of total production. The textile industry has a low export ratio, but this industry is difficult to distinguish from the yarn industry in my data. Hence, I work with their aggregate, which I define as the textile and related products industry.

[65]Tanimoto (2006) finds that firms in the domestic industry are run by local elites.

[66]Even without technology adoption in the export industry, structural change can occur through exposure to the foreign market and specialization in non-agricultural goods due to a comparative advantage. However, this would imply that the relative price of industrial goods increases after a county gains access to the railroad network, whereas the trend in the aggregated data is the opposite. Alvarez-Cuadrado and Poschke (2011) uses similar reasoning to argue that trade does not explain the decrease in the price of industrial goods and the increase in the industrial sector's share of workers. I also show more detailed export data in Figure 1.A.1. This shows growth in the openness and trade volume with developed countries of the metal and textile industries, but no trade growth in the agriculture and machinery industries. Figure 1.A.2 shows the industry-specific relative price data and confirms that the export industry also experienced a drop in prices over the sample period.

[67]The issue of specification error for the IV is covered in Figure 1.A.3, Table 1.A.9, and Table 1.A.10.

[68]Telegraphs were available even without telegraph stations through post offices covering the entire nation. In its public announcement, the Ministry of Technology instructed users on how to use telegraphs in

and hence this might have been affected by the cost-minimizing path. During that time, the telephone service was restricted within city areas, and thus I exclude seven counties that obtained telephone services by 1902 and estimate the same model as the main result, but the result does not change (See Table 1.A.22.). As an important industrial policy, the Meiji government established state-owned factories in the 1870s. These factories tried to adopt Western technologies to demonstrate them for the private sector. Hence, even though many of them were unprofitable, it might affect the main results. Table 1.A.23 shows the result without counties in which state-owned factories are located,[69] highlighting that the results do not change substantially. Further, the new government tried to improve the credit market and this might have affected technology adoption through credit constraints. However, it established an act to create a national banking system in 1876 and private banks covered the nation uniformly until the end of the 1870s. Accordingly, Table Table 1.A.24 implies that the instrument is not correlated with interest rates in the initial period and railroad construction does occur in areas where interest rates are falling, meaning that the credit market story cannot explain the main result. Steamship is another concern, because it became popular during the sample period and decreased trade costs as railroads did. However, this affected coastal areas more, and the cost-minimizing path is located inland on average. In addition, since the distance to the coast is controlled for in all the specifications, the adoption of steamships does not drive the main result.[70]

## 1.5 The Effect on Modern Economic Development

As discussed earlier, technological progress in the industrial sector induces structural change through the labor pull effect.[71] and urbanization by breaking the pattern of mean reversion in the population. In this section, I examine the effect of earlier railroad access on structural change and urbanization.

the absence of telegraph stations. Source: `http://dl.ndl.go.jp/info:ndljp/pid/796222`.

[69]The list of factories is provided by Kobayashi (1977).

[70]As another technological change in the transportation infrastructure, Chandler (1977) documents that some firms producing perishable products such as milk and meat adopted refrigerated railroad cars in the United States. However, refrigerated railroad cars are herein interpreted as the outcome of railroad access if factories invest as in the United States or a part of the treatment if introduced by railroad companies. In addition, this does not affect the main result because it was introduced to Japan in 1908.

[71]While railroads might affect industrialization through regional specialization, there is no reason to believe that railroad access will induce industrialization on average. Indeed, it might also induce structural change through the potential effect on agricultural productivity; however, this will be less important as shown in Figure 1.2 and Figure 1.3 in the present context. Railroad access did not induce technology adoption in the agricultural sector because farmers were credit constrained, new Western technology was not available, Western technology was not directly applicable to Japanese crops, and the technological gap between Japanese and Western technology in agriculture was not as large as that in the industrial sector. Hence, in this study, the labor pull effect serves as the main channel.

### 1.5.1 Structural Change

Here, I examine whether gaining railroad access induces structural change. I cannot analyze the short- and medium-term effects as sectoral employment data are unavailable. However, by using 1880's *Huken Tokeisho* and the 1920 Population Census, I can analyze the long-term effects. I use the following specification:[72]

$$\begin{aligned} \textit{Primary Worker/Total Worker}_i = \alpha_1 \textit{Railroad Access (-1892)}_i + \alpha_2 \textit{Local Controls}_i \\ + \alpha_3 \textit{Initial Agricultural Worker / Population}_i + e_i \end{aligned} \tag{1.5}$$

*Primary Worker/Total Worker*$_i$ is the primary sector's share of workers in county $i$ in the 1920 census. *Railroad Access (-1892)*$_i$ is a dummy variable, which takes the value of one if county $i$ had gained railroad access by 1892. As before, *Railroad Access (-1892)*$_i$ is instrumented by *Distance to the Path*$_i$. *Initial Agricultural Worker/Population*$_i$ is the share of agricultural workers in the total population of county $i$, calculated from the population data of 1885 and the number of agricultural workers in 1884, 1885, or 1886.[73] Although this control and outcome are not identically constructed, they are conceptually similar. Since the outcome variable is only available for 1920, this is a cross-sectional analysis. However, I control for the initial level of industrialization by using *Initial Agricultural Worker/Population*$_i$.

Table 1.9 presents the results. In column (1), I find a significant negative relation between the two, as expected. This finding is robust after controlling for the time-variant effect of traditional roads in column (2). In column (3), I also include the time-variant effect of traditional roads. While this loses significance, this is because of the higher SE rather than omitted variable bias. To confirm that railroad access affects the manufacturing sector rather than the service sector, I use the share of workers in the industrial sector as the dependent variable in columns (4) to (6) and estimate the effect with the same sets of controls as in columns (1) to (3). The size of the effect is similar to that in columns (1) to (3), suggesting that the negative effect on primary sector employment is due to the industrial sector rather than the service sector. In column (4), the magnitude is 5.7 percent, and this corresponds to a standard deviation of 0.36. This estimate may be attenuated since after 1900 steam power diffused widely and agricultural technology was upgraded, which induced widespread structural change and narrowed the gap in structural change

[72] See Figure 1.A.4 and Table 1.A.25 in Section 1.A.9 for the long-term effects on technology and the population.

[73] Since this variable is taken from the *Huken Tokeisho*, data availability varies by year and prefecture. To increase the sample size, I use the earliest available data from these three years. Even so, no data were available for 87 of the 487 counties used in the main estimation.

Table 1.9: The Effect on Structural Change in 1920

| | Primary/Total (Worker) | | | Industry/Total (Worker) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Railroad Growth 1885-1892 | -0.0675$^{+}$ | -0.0632$^{+}$ | -0.0640 | 0.0569$^{+}$ | 0.0529$^{+}$ | 0.0544 |
| | (0.0409) | (0.0369) | (0.0410) | (0.0324) | (0.0303) | (0.0342) |
| Initial Agri. Worker / Population | Yes | Yes | Yes | Yes | Yes | Yes |
| Sum of Slope | Yes | Yes | Yes | Yes | Yes | Yes |
| Log Flat Area | Yes | Yes | Yes | Yes | Yes | Yes |
| Log Area | Yes | Yes | Yes | Yes | Yes | Yes |
| Distance to Coast | Yes | Yes | Yes | Yes | Yes | Yes |
| Initial Population Density | No | Yes | Yes | No | Yes | Yes |
| Traditional Road | No | Yes | Yes | No | Yes | Yes |
| Main Traditional Road | No | No | Yes | No | No | Yes |
| First KP Stat | 62.57 | 62.26 | 55.75 | 62.57 | 62.26 | 55.75 |
| N | 400 | 400 | 400 | 400 | 400 | 400 |

Robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *N* shows the sample size. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. *Initial Agri Worker/Population* is calculated as agricultural workers divided by the population in 1885. Agricultural workers' data are obtained from *Huken Tokeisho*, and I use the earliest available data from 1884 to 1886 for each. Since this information is missing for some prefectures, 87 counties were dropped from the main results. Note that the mining industry is accounted for in the industrial sector.

with early adopters.

### 1.5.2 Urbanization

To analyze the impact on urbanization, I estimate the following model for two points in time, 1903 and 1920.

$$\begin{aligned} &\textit{Log of Population}_{it} - \textit{Log of Population}_{i,1885} \qquad (1.6)\\ &= \alpha_0 + \alpha_1 \textit{Log of Population Density}_{i,1885} + \alpha_2 \textit{Railroad Growth 1885-1892}_i \\ &+\alpha_3 \textit{Log of Population Density}_{i,1885} * \textit{Railroad Growth 1885-1892}_i + \alpha_4 \textit{Local Controls}_i + e_{it} \end{aligned}$$

where *Railroad Growth 1885-1892*$_i$ is *Railroad Access*$_{i,1892}$ - *Railroad Access*$_{i,1885}$. As in the previous analysis, this is instrumented by *Distance to the Path*$_i$. This variable captures the effect of railroads constructed as a matter of priority, and will have a long-term effect after 1982, when the new railroad law was enacted and counties that were not prioritized began to catch up. *Local Controls*$_i$ contains the set of geographical (and non-geographical) controls as in the other analysis. Because of the mean reversion effect in pre-modern agricultural economies, I expect $\alpha_1$ to be a negative coefficient. I expect $\alpha_2$

and $\alpha_3$ to be positive coefficients because of technology adoption in the industrial sector.[74] The parameter of interest is $\alpha_1 + \alpha_3 * \textit{Railroad Growth 1885-1892}_i$. This fact determines whether larger cities can grow at a quicker rate. A fatter tail on the right-hand side of the distribution of the log of population density, as shown in Figure 1.13, also determines how this differs between the early and the late adopters of railroads. According to Gibrat's Law, if the benchmark in the population dynamics of the modern economy holds for any sample, $\alpha_1$ and $\alpha_3$ will have a coefficient of zero.

Table 1.10 contains the results. To simplify the interpretation of the effects, I normalize all the controls. For all the years and specifications, $\alpha_1$ has a negative sign and the interaction terms, $\alpha_2$ and $\alpha_3$, have positive signs, as expected. Moreover, the estimated coefficient of $\textit{Log of Population Density}_{i,1885}$ when $\textit{Railroad Growth, 1885–1892}_i = 1$ is less negative and even becomes positive in some specifications. In columns (1) and (3), the overall effects are -0.0189 and 0.113, suggesting that the urbanization effect is stronger in the long term. I obtain similar estimates in columns (2) and (4), when access to (main) traditional roads is added as a control. These results are consistent with the idea that technology adoption and structural change caused by railroad access induced urbanization by breaking the pattern of mean reversion.[75]

## 1.6 Conclusion

The central contribution of this study is finding a causal impact of gaining access to a railroad network on technology adoption in the industrial sector, which is a key mechanism that explains why railroads were a major driver of structural change and urbanization. To achieve this, I digitize a novel county-level data set covering Japan from the 1880s to 1920s, a period that witnessed railroad network expansion, the transformation of the industrial sector by steam power, and structural change and urbanization. I start the analysis by using factory-level data and confirm that steam-powered factories are larger and more productive, which is consistent with the key assumption that steam power has a high fixed cost but a low marginal cost. Then, I perform a DID-IV estimation by calculating the cost-minimizing path between destinations, using the distance from this path to predict the growth in railroad access by 1892. I find that gaining railroad access by 1892 increased steam power in 1902. These estimates imply that railroad access growth from 1888 to 1892 accounts for 67 percent of the growth in steam power from 1888 to 1902.

[74] Michaels et al. (2012) shows that conditional on productivity, manufacturing areas should have higher population density because production is less land-intensive.

[75] If railroads affect the population dynamics by reducing migration costs, then the negative slope of $\textit{Log of Population Density}_{i,1885}$ would become *steeper* when interacted with railroad access. This is because the mean reversion process will be accelerated by costless migration. My estimate is precisely the opposite.

Table 1.10: The Effect on Urbanization in 1885–1903, 1920

| | Δ Log of Population Density | | | |
|---|---|---|---|---|
| | 1885-1903 | | 1885-1920 | |
| | (1) | (2) | (3) | (4) |
| Railroad Growth 1885-1892 | 0.0860** | 0.0788* | 0.169* | 0.141+ |
| | (0.0316) | (0.0336) | (0.0772) | (0.0825) |
| Railroad Growth 1885-1892 * Initial Pop. Density | 0.0531* | 0.0513* | 0.282*** | 0.275*** |
| | (0.0245) | (0.0239) | (0.0653) | (0.0630) |
| Log Initial Population Density | -0.0720*** | -0.0734*** | -0.169*** | -0.175*** |
| | (0.0117) | (0.0118) | (0.0304) | (0.0310) |
| Sum of Slope | Yes | Yes | Yes | Yes |
| Log Flat Area | Yes | Yes | Yes | Yes |
| Log Area | Yes | Yes | Yes | Yes |
| Distance to Coast | Yes | Yes | Yes | Yes |
| (Main) Traditional Road | No | Yes | No | Yes |
| First KP-stat | 36.21 | 33.93 | 36.21 | 33.93 |
| N | 480 | 480 | 480 | 480 |

*N* shows the sample size. Robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. The dependent variable is $\textit{Log of Population Density}_{1903} - \textit{Log of Population Density}_{1885}$ in columns (1) and (2) and $\textit{Log of Population Density}_{1920} - \textit{Log of Population Density}_{1885}$ in columns (3) and (4). I drop the counties whose log of population is more than 2.5 s.d. from the mean as outliers for this analysis. When I cluster the SEs at the prefecture level, the SEs for the interaction terms become 0.036, 0.034, 0.106, and 0.101.

The effect is smaller for less populated areas, suggesting its disadvantage for technology adoption such as credit constraints. Further, I observe the effect for both the domestic and the export industries, which highlights that railroad access integrated the regions internally and externally.

As additional results, I find that railroad access affects structural change and urbanization. As in the labor pull story, gaining railroad access by 1892 increased the share of workers employed outside the agricultural sector in 1920, and most of the effect is explained by the increase in workers in the manufacturing sector. It also breaks down the pattern of mean reversion in the population dynamics during 1885–1903 or 1920, which is an important change for larger cities to emerge. Overall, these results are consistent with the picture that railroads induce the two features of modern economic development, structural change and urbanization, through technological progress in the industrial sector.

These findings suggest a view on the history of the take-off of the Japanese economy. Before the 1850s, the diffusion of Western technology was mostly blocked by the restrictive trade policy of the Tokugawa Shogunate. Owing to foreign pressure from Western countries, however, the Shogunate was forced to open several ports for foreign trade in the late 1850s. However, the fear of colonization resulted in the collapse of the Shogunate in 1868. The new Meiji Government, a more centralized one than the Shogunate, tried to adopt Western technology to strengthen its state capacity to avoid colonization. The railroad network was one such advancement. This adoption would not have been possible without the Meiji government because constructing a railroad network requires significant government spending, coordination, and a positive attitude towards Western technology. Increased tax revenue would be another side of this success story and it is hard to quantify what would have happened to the economy if railroads had not had a positive impact on the economy because railroads raised more tax revenue by fostering the economy and the revenue was spent for further railroads construction. Still, the findings of this study support the view that the centralization and modernization of the government in 1868 aided Japan's economic development through starting railroad construction.

# Appendix

## 1.A Appendix

### 1.A.1 A Formal Model

#### Consumers

Consumers have the following utility function over two types of goods, $x$ and $y$. $x$ is the composite of industrial goods and $y$ represents agricultural goods. They have a CES preference over a continuous variety of $x$:

$$u = (1-\beta)\log y + \beta \log(x + \bar{x})$$
$$x = \left[\int_0^N x_i^\alpha di\right]^{1/\alpha}$$

The elasticity of substitution across varieties is given by

$$\sigma = \frac{1}{1-\alpha} > 1$$

As in the standard way, I can define $p_x$ as the aggregate price for aggregated goods $x$:

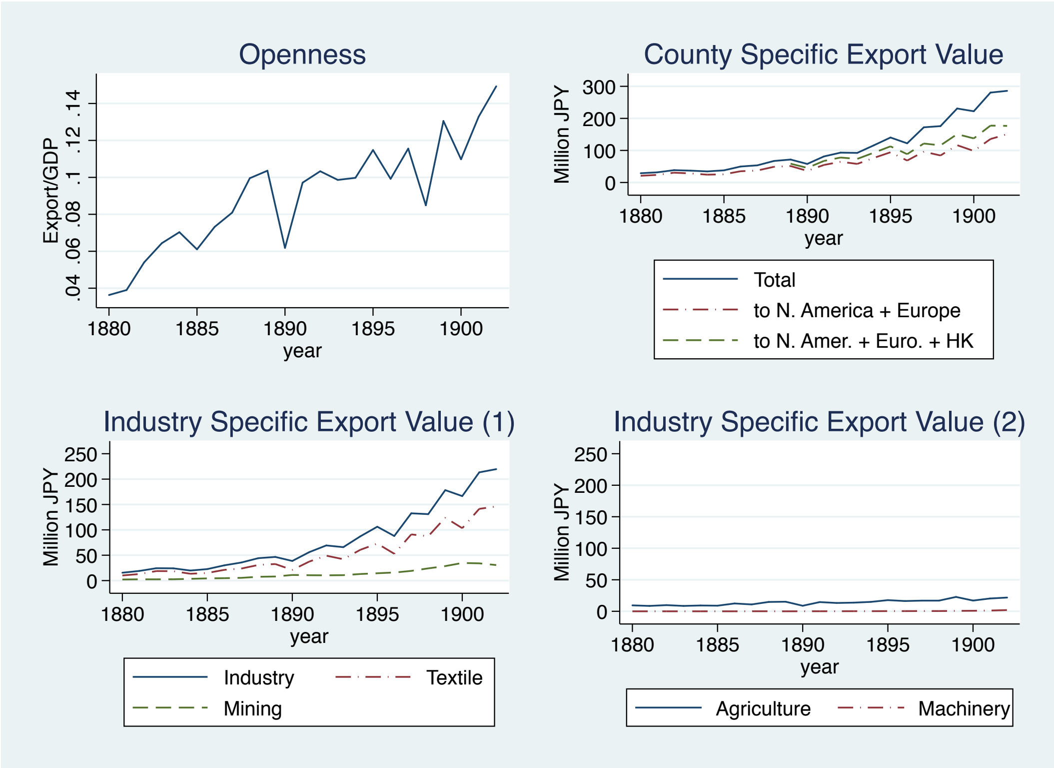
$$p_x = \left[\int_0^N p_i^{1-\sigma} di\right]^{1/1-\sigma}$$

The budget constraint is

$$p_x x + p_y y = E$$

where $E$ is total expenditure and wage is the numeraire.

Figure 1.A.1: Aggregated Data on Foreign Trade




Source: Umemura et al. (1965). The unit for values is million Japanese yen.

Given these, I obtain the following equation from the FOCs with respect to $x$ and $y$:

$$\frac{1-\beta}{\beta}p_x(x+\bar{x})+p_x x=E \implies x=\frac{E}{p_x}\beta-(1-\beta)\bar{x}$$

and demand for each variety $i$ is

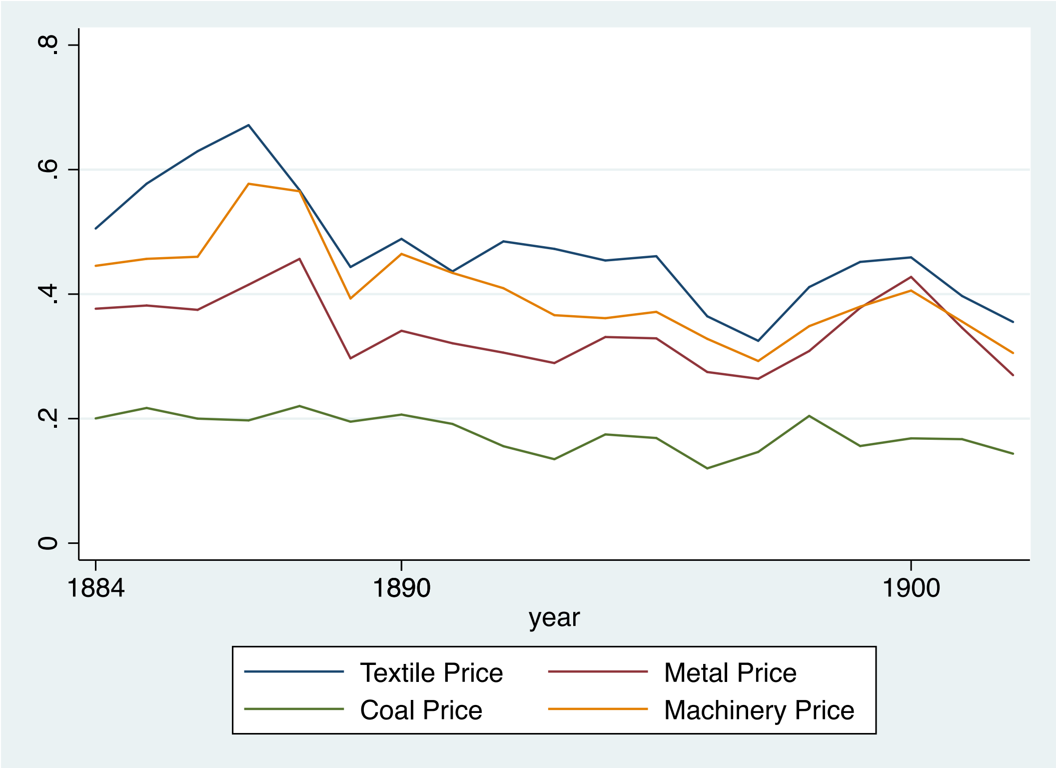
$$x_i=\left[\frac{E}{p_x}\beta-(1-\beta)\bar{x}\right]\left(\frac{p_i}{p_x}\right)^{-\sigma}$$

Consumers can decide in which sector to engage by using one unit of time.

### Production

The agricultural sector is competitive and has a production function, $AL_y^{\theta}$, where $A$ is its technology level, and I assume $0<\theta<1$. On the contrary, the industrial sector is characterized by monopolistic competition as in Bustos (2011). Firms are ex-ante homogeneous and have to pay fixed cost $f_e$ for entry. They draw their productivity $\phi$ from the Pareto distribution $G(\phi)=1-\phi^{-k}$ during entry. There are two types of technology in this sector, low (old) and high (new). The total cost function of production is given by the following:

Figure 1.A.2: Relative Price Data at the Industry Group Level




Source: Umemura et al. (1965). I obtained these figures by dividing the price index of each industry group by the price index of agricultural goods.

$$TC_l(q,\gamma) = \left(f + \frac{q}{\phi}\right)$$
$$TC_h(q,\gamma) = \left(f\eta + \frac{q}{\gamma\phi}\right)$$

where $\eta > 1$ and $\gamma > 1$. Further, they can export to the other symmetric region by paying $f_x$ with the iceberg cost $\tau$. Given demand $q(\omega) = E_x P^{\sigma-1} p(\omega)^{-\sigma}$ and pricing rules $p_l^d = w/(\rho\phi)$, I obtain the following profit equation:

$$\pi_l^d(\phi) = \frac{1}{\sigma} E_x (p_x \rho)^{\sigma-1} \phi^{\sigma-1} - f$$
$$\pi_h^d(\phi) = \frac{1}{\sigma} E_x (p_x \rho)^{\sigma-1} (\phi\gamma)^{\sigma-1} - f\eta$$
$$\pi_l^x(\phi) = (1 + \tau^{1-\sigma}) \frac{1}{\sigma} E_x (p_x \rho)^{\sigma-1} \phi^{\sigma-1} - (f + f_x)$$
$$\pi_h^x(\phi) = (1 + \tau^{1-\sigma}) \frac{1}{\sigma} E_x (p_x \rho)^{\sigma-1} (\phi\gamma)^{\sigma-1} - (f\eta + f_x)$$

where $\pi$'s subscript $l$ ($h$) denotes the case for choosing low (high) technology and superscript $d$ ($x$) denotes the case for not exporting (exporting).

**Equilibrium**

I can show the following by using the results of Bustos (2011).

**Proposition 1.** *1. (Sorting) Firms with $\phi < \phi^*$ will exit, firms with $\phi^* < \phi < \phi^x$ will neither export nor adopt high technology, those with $\phi^x < \phi < \phi^h$ will export and will not adopt high technology, and those with $\phi^h < \phi$ will export and adopt high technology.*

*2. (Technology adoption) $\phi^h$ is strictly decreasing in $\tau$.*

*3. (Industrial price index) $p_x$ is strictly increasing in $\tau$ if $\sigma$ is sufficiently large.*

*Proof.* (1) and (2) are the same as in Bustos (2011), because the endogenous variables $E_x$ and $L_x$ do not affect these results. Considering (3), since $E_x$ is endogenous, this differs from the original result, which simply claims that $p_x$ is strictly increasing in $\tau$. Accordingly, following the same step as in Bustos (2011), I obtain the following expression:

$$p_x \propto \Delta^{-1/k} (E\beta - (1-\beta)\bar{x} p_x)^{\frac{1}{1-\sigma}} \implies p_x (E\beta - (1-\beta)\bar{x} p_x)^{\frac{1}{\sigma-1}} \propto \Delta^{-1/k} \tag{1.7}$$

where

$$\Delta = 1 + \tau^{-k}\left(\frac{f_x}{f}\right)^{\frac{\sigma-1-k}{\sigma-1}} + (1+\tau^{1-\sigma})^{\frac{k}{\sigma-1}}(\gamma^{\sigma-1}-1)^{\frac{k}{\sigma-1}}(\eta-1)^{\frac{\sigma-1-k}{\sigma-1}} \tag{1.8}$$

The LHS is strictly increasing in $p_x$ as in the partial equilibrium when $\sigma$ is sufficiently large. If so, this $p_x$ is increasing in $\tau$ because $\tau$ only strictly increases the RHS through $\Delta$, as shown in Bustos (2011).[76] $\sigma$ is the key variable to predict whether the larger total expenditure on industrial goods is associated with a lower price more (or less) than proportionally. If $\sigma$ is sufficiently high, the income effect generated by the drop in the price of industrial goods will not dominate the partial equilibrium effect. I assume this fact throughout the paper.[77] □

The proposition claims that railroad access will increase $\phi_h$. Note that $1-G(\phi_h)$ is the probability of technology adoption at the firm level and the number of high technology firms is given by $M(1-G(\phi_h))$, which will be the aggregated-level variable, as observed in the data.[78] The following result ensues:

**Proposition 2.** *1. $M(1-G(\phi_h))$ is strictly decreasing in $\tau$ when $\bar{x}$ is sufficiently large.*

*Proof.* To see this, $(1-G(\phi_h))$ is strictly decreasing in $\tau$ because $\phi_h$ is strictly increasing in $\tau$. Therefore, it suffices to show that $M$ is decreasing in $\tau$ to obtain the result. Note that $M$ is $\frac{E_x}{\bar{r}}$, where $\bar{r}$ is the average revenue. By taking the log of $M$,

$$\frac{\partial \log M}{\partial \tau} = \frac{\frac{\partial E_x}{\partial \tau}}{E_x} - \frac{\frac{\partial \bar{r}}{\partial \tau}}{\bar{r}}$$

Next, I show that this is decreasing in $\bar{x}$. First, $-\frac{\frac{\partial \bar{r}}{\partial \tau}}{\tau}$ is constant about $\bar{x}$. Therefore, I focus on $\frac{\frac{\partial E_x}{\partial \tau}}{E_x}$. If I take the derivative with respect to $\bar{x}$,

$$\begin{aligned}\frac{\partial \log E_x}{\partial \bar{x}\partial \tau} &= \frac{\partial \frac{-(1-\beta)p_x}{E_x}}{\partial \tau} = \frac{-(1-\beta)}{E_x}\frac{\partial p_x}{\partial \tau} + \frac{(1-\beta)p_x}{E_x^2}\frac{\partial E_x}{\partial \tau} = \frac{-(1-\beta)}{E_x}\frac{\partial p_x}{\partial \tau} + \frac{(1-\beta)p_x}{E_x^2}(\beta-1)\bar{x}\frac{\partial p_x}{\partial \tau} \\ &= \frac{\partial p_x}{\partial \tau}\frac{1}{E_x^2}\left(-(1-\beta)(E\beta-(1-\beta)p_x\bar{x}) - (1-\beta)^2 p_x\bar{x}\right) = \frac{\partial p_x}{\partial \tau}\frac{1}{E_x^2}(-1+\beta)E\beta\end{aligned}$$

This is negative because $\beta < 1$ and $\frac{\partial p_x}{\partial \tau} > 0$. Note that when $\bar{x} = \frac{E\beta}{(1-\beta)p_x} - \epsilon$, $\frac{\partial M}{\partial \tau} = -\frac{\frac{E\beta}{p_x}+\epsilon(1-\beta)}{\bar{r}}\frac{\partial p_x}{\partial \tau} - \frac{\epsilon}{r^2}\frac{\partial r}{\partial \tau}$, and when $\epsilon \to 0$, since $p_x\frac{\partial p_x}{\partial \tau}$ is now constant about $\epsilon$ and the other

[76] I assume that $E\beta - (1-\beta)\bar{x}p_x$ is positive to exclude corner solutions and confirm that the LHS is strictly increasing in $p_x$.

[77] This also matches the pattern in the aggregated data.

[78] The aggregated amount of new technology will also behave similarly.

terms go to zero, it goes to a negative constant. Therefore, there must be a feasible $\bar{x}^*$ ensuring a positive demand for industrial goods such that when $\bar{x}^* < \bar{x}$, then $\frac{\partial M}{\partial \tau} < 0$.

□

This result has an intuition: if there is no income effect, $M$ will decrease because of the introduction of railroad access because the average revenue will increase. However, if there is an income effect, it also increases labor in the industrial sector and consequently increases the number of firms. Therefore, if this effect is sufficiently strong, $M$ will also increase.

How does railroad access affect structural change? I can show the following by using Proposition 1.

**Proposition 3.** *1. $L_y$ is strictly increasing in $\tau$.*

*2. $p_x/p_y$ is strictly increasing in $\tau$.*

*Proof.* I only have to solve the price of agricultural goods and labor allocation in the two sectors. Agricultural wages are determined by the labor market condition in the agricultural sector. Like wage income, they can get $p_y \theta A L_y^{\theta-1}$ per labor. Total profit in the agricultural sector is $p_y A L_y^{\theta} - p_y L_y \theta A L_y^{\theta-1}$. Therefore, by engaging in the agricultural sector, workers can get $p_y A L_y^{\theta}$ as their aggregated income. Note that under competitive labor market conditions, workers can obtain the same income if they engage in the industrial sector, meaning that $p_y A L_y^{\theta-1}$ must be one. Given this, $L_y$ is pinned down by the following market-clearing condition for agricultural goods:

$$
\begin{aligned}
& p_y A L_y^{\theta} = (1-\beta) L (1 + \bar{x} p_x) \\
\implies & L_y - (1-\beta) L - (1-\beta) \bar{x} p_x = 0
\end{aligned}
$$

This is strictly increasing in $L_y$ ($p_x$ is not a function of $L_y$, because $E_x = E\beta - p_x(1-\beta)\bar{x}$ and $p_x$), and strictly decreasing in $\tau$ (by increasing $p_x$). Therefore, $L_y$ is increasing in $\tau$.

To see the second part, I consider the following equation:

$$
\frac{p_x}{p_y} = \frac{1-\beta}{\beta} \frac{y}{x + \bar{x}}
$$

$\tau$ increases $y$ through an increase in $L_x$ and decreases $x$ through an increase in $P_x$ because $x = E/P_x - (1-\beta)\bar{x}$ ($E$ do not change by $\tau$).

□

### 1.A.2 Data Description

#### Railroad-related Data Sources

***Nihon Kokuyu Tetsudo Hyakunenshi (The History of Japan National Railroad)*** This is an official record of national railroad services published in 1972 by the Japan National Railway. It covers all national railroad lines constructed before 1945 and the private lines nationalized in 1906, and contains information such as the opening year of each section. Therefore, I can identify the opening year of the sections in the shapefile above using this source.[79]

***Nihon Tetsudoshi (History of Japanese Railroad)*** This is also a publication of Japanese railroad history published by the Ministry of Railroads in 1921. This document covers the lines not included in the *Nihon Kokuyu Tetsudo Hyakunenshi* and private lines that were not nationalized in 1906 or built after 1906 since this document has less readability than the *Nihon Kokuyu Tetsudo Hyakunenshi*.

***Nihon no Kaido Handbook (Handbook of Japanese Traditional Roads)*** Although there are no official records about traditional roads, this book collects local information and describes the routes of traditional roads with the names of the towns that the traditional roads passed through. I made straight lines to connect these towns to cities to represent the traditional road network. Straight lines will not be the same as traditional roads, but I do not think that this will change the county-level variation or results because towns listed are fairly close to each other. The main roads, *Gokaido*, are also identified as the five roads specified by the Tokugawa Shogunate.[80]

#### Factory-Related Sources

***Noshomu Tokeihyo (Agriculture, Commerce and Manufacturing Statistics)*** I digitize these official statistics for the factory-related variables. The Ministry of Agriculture and Commerce published these data, which cover the entire nation. This is the most comprehensive series of industrial statistics covering Japan in the 1880s and 1890s. It has a list of factories with addresses and the horsepower of each type of engine (water or steam) as well as the number of workers. Unfortunately, the names in some cases were not recorded properly (there are some cases, such as "silk factory"). Hence, I construct county-level panel data for the total number and horsepower of engines, and total number of factories

[79] A limited number of lines closed by 1950, and thus I used a straight line to locations that the line connected.

[80] They are *Tokai-Do* and *Nakasen-Do* from Tokyo to Kyoto, *Oshu-Dochu* from Tokyo to Shirakawa, *Koshu-Kaido* from Tokyo to Shimosuwa, and *Nikko-Kaido* from Tokyo to Nikko.

and workers for each type of power source. The document also has product information, and thus I construct industry-level outcomes for further analysis. Only the publications in 1885, 1888, 1889, 1890, and 1891 have the list; for 1885, many prefectures could not report their results to the central government, and thus I use only data for 1888–1891 for the analysis. I also digitize the number and total days of expositions held in each prefecture in 1886–1900 almost annually from this publication.

***Kojo Tsuran (Factory Catalog)*** This series is published after 1902 and has the same list of factories as above. I use its publication in 1902 and 1919, which is after the factory list in *Noshomu Tokeihyo* became unavailable.[81][82] The format is consistent with *Noshomu Tokeihyo*, which is sufficient to construct the same variables related to steam engines at the industry and county levels.

***Huken Tokeisho (Prefectural Statistics)*** These series of prefecture-level statistics were published by prefecture governments. They contain a factory list, as in the publications above, and sometimes have more precise information such as revenue or share capital. This is useful to check the positive relationship between technology and productivity or firm size, which I presume throughout this study. However, this publication has many missing volumes in addition to inconsistent data formats across time and prefectures, especially after the central government repealed its publication order. Prefecture governments were free to publish at will thereafter. Thus, I do not use these factory data for the main results and keep them only to examine the relationship between steam power and labor productivity or firm size at the factory level. Again, it was impossible to match factory names to construct panel data.

### Population-Related Data Sources

***Nihon Teikoku Minseki Koguchi Hyo (Table of Registered Population)*** These county-level population statistics were published almost annually from 1879 to 1898 and between 1903 and 1908 under a different title, (*Nihon Teikoku Seitai Jinko Tokei*).[83] I use a dig-

[81]Again, I greatly appreciate Yutaka Arimoto, Kaori Ito, Asuka Imaizumi, Tetsuji Okazaki, Kentaro Nakajima, and Tomohiro Machikita, who digitized these catalogs with support from the JSPS KAKENHI Grant-in-Aid for Scientific Research (B) 21330064.

[82]This data set covers factories with more than nine workers. Because *Noshomu Tokeihyo* covers all factories using water or steam power, these series do not have consistent data. However, the results do not change when excluding factories with fewer than 10 workers from the *Noshomu Tokeihyo* sample because these factories are negligible under aggregation.

[83]There were two types of registered population, *Honseki* and *Genju*, which requires an explanation of the Japanese registry system to illustrate the difference. Suppose you are born in a family county X; your name will be registered in your house account (*Koseki*) in county X. *Honseki* counts the population based on this information. Now, if you move from county X to county Y, you must submit a document to the

Table 1.A.1: List of Excluded Counties

| Reason | Counties (By the name in 1885) |
|---|---|
| Domestic Colonies | All counties in Hokkaido and Okinawa |
| Isolated Islands | All counties in Tsushima-island, Nagasaki, and Oki-island in Shimane |
| Destinations | All wards in Tokyo city, Toshima and Higashi-Tama counties in Tokyo, Nakakambara county and Nigata ward in Nigata, Kiku county in Fukuoka, and Akamagaseki county in Yamaguchi |
| Foreign Ports and Outliers | All wards in Osaka city in Osaka, Kobe ward in Hyogo, and Yokohama ward in Kanagawa |

itized version for 1879, 1880, 1882, and 1885 from the Historical Regional Statistical Data,[84] and newly digitized the data in the other years, 1888–1891 and 1903.

***Population Census*** In 1920, the government conducted its first census, which I digitized to obtain population data at the county level.[85] Additionally, this document reports the number of workers by sector, allowing me to construct the share of workers in each sector in 1920.

***Huken Tokeisho (Prefectural Statistics)*** I use this series to obtain the number of agricultural workers around 1885. Because some prefectures did not publish in this year, I use 1884 or 1886 if available. Combined with the population in *Nihon Teikoku Minseki Koguchi Hyo*, I calculate the share of the population working in the agricultural sector around 1885 and use it as a control for the regression analysis to see the impact on industrialization.

### 1.A.3 Excluded Counties

Some counties are excluded from all the analysis for various reasons (see Table 1.A.1). After excluding these, 487 counties remain in the main data set.

county offices, which will then account for this movement to calculate the total population currently living in each county, which is the *Genju* population. For this reason, I choose *Genju* as the main variable, though for 1879, 1880, and 1882, there are no available data for *Genju* and I therefore use *Honseki* in these years only for the falsification test without connecting them with *Genju* data after 1885.

[84] http://giswin.geo.tsukuba.ac.jp/teacher/murayama/datalist_e.htm

[85] I regard this census population variable as equivalent to the *Genju* population variable, since both attempt to measure the residential population.

Table 1.A.2: County Characteristics and Timing of Railroad Access Gain

| | OLS | | | | | |
|---|---|---|---|---|---|---|
| | Log of Pop. | Distance. to | | | Traditional Road | |
| | Density in 1885 | Destinations | Tokyo or Osaka | Coast | Main | All |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Railroad Access in 1890-1894 | -0.0284 | 24.92 | 91.94*** | -0.204*** | -0.139+ | 0.0256 |
| | (0.134) | (19.20) | (19.72) | (0.0405) | (0.0756) | (0.0500) |
| Railroad Access in 1895-1899 | 0.0122 | 20.95 | 78.92*** | -0.267*** | -0.366*** | -0.0462 |
| | (0.138) | (20.66) | (20.06) | (0.0378) | (0.0635) | (0.0571) |
| Railroad Access in 1900-1904 | -0.468** | 16.84 | 61.76** | -0.0729 | -0.161+ | 0.0559 |
| | (0.146) | (23.25) | (21.63) | (0.0468) | (0.0852) | (0.0528) |
| Railroad Access in 1905-1909 | -0.802*** | 73.84* | 131.5** | -0.188** | -0.306** | 0.115** |
| | (0.167) | (36.63) | (42.84) | (0.0646) | (0.0932) | (0.0411) |
| Railroad Access after 1910 | -0.703*** | 53.68** | 116.4*** | -0.226*** | -0.337*** | -0.157** |
| | (0.119) | (17.20) | (15.51) | (0.0380) | (0.0642) | (0.0533) |
| Constant | 5.327*** | 171.0*** | 124.8*** | 0.320*** | 0.377*** | 0.885*** |
| | (0.106) | (14.31) | (8.819) | (0.0365) | (0.0624) | (0.0411) |
| N | 487 | 487 | 487 | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The Robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The base line is a county that got railroad access before 1890.

### 1.A.4 Descriptive Statistics of Factory-Level Data in 1902

Table 1.A.3 shows the descriptive statistics of the factory-level data in 1902 with industry-level breakdowns. Of the 3361 factories, 2097 adopted steam power. This adoption rate is higher for the mining industry and lower for the domestic industry. The average steam power and number of workers at the steam-powered factory level varies. The mining industry factories are larger than the textile and domestic industries.

### 1.A.5 Regression Analysis on the First Stage

I confirmed the relationship in Figure 1.16 by a regression analysis, using the following specification:

$$
\begin{aligned}
&\textit{Railroad Access}_{it} = \textit{Distance from the Path}_i * \textit{Year FE} + \textit{Local Geographical Controls}_i * \textit{Year FE} \\
&(+\textit{Local Other Controls}_i * \textit{Year FE}) \\
&+\textit{Year FE} + \textit{County FE} + e_{it}
\end{aligned}
$$

where *Railroad Access*$_{it}$ is a binary variable, which takes one if county $i$ has railroad access in year $t$, *Distance from the Path*$_i$ is the distance between the cost-minimizing path and county $i$, and *Local Geographical Controls*$_i$ are the time-invariant controls in county $i$, such as the sum of slopes, log of area, log of flat area, and the distance from the

Table 1.A.3: Descriptive Statistics: Factory-Level Data in 1902

| | Factories | Steam-Powered Factories | – Horsepower | – Workers |
|---|---|---|---|---|
| Mining | | | | |
| mean | | .7254902 | 165.46 | 234.4414 |
| sd | | .4477325 | 278.8256 | 736.0175 |
| sum | 153 | 111 | 18035.14 | 26023 |
| Texitle | | | | |
| mean | | .6239808 | 17.50445 | 96.80092 |
| sd | | .4845011 | 79.73636 | 213.9992 |
| sum | 2085 | 1301 | 20882.81 | 125938 |
| Brewing and Printing | | | | |
| mean | | .3636364 | 18.22857 | 100.1389 |
| sd | | .4834938 | 29.10082 | 145.3673 |
| sum | 99 | 36 | 638 | 3605 |
| Other | | | | |
| mean | | .5991254 | 60.88207 | 63.72506 |
| sd | | .4904333 | 314.2329 | 124.1667 |
| sum | 686 | 411 | 24657.24 | 26191 |
| Total | | | | |
| mean | | .614952 | 36.86176 | 97.77138 |
| sd | | .4866872 | 183.136 | 263.2247 |
| sum | 3023 | 1859 | 64213.19 | 181757 |

The mining industry includes the mining, metal, and explosive materials (such as oil production) industries. The textile industry includes the cotton-spinning, weaving, and yarn-making industries.

coast. Table 1.A.4 shows the results. Column (1) is the basic specification and column (2) has the additional non-pure geographical controls (interacted with *Year FE*), *Local other Controls*$_i$, which include the population density in 1885[86] and the existence of traditional roads. The result shows that the instruments are working as expected such that the *Distance from the Path*$_i$ has significantly negative coefficients. The magnitude of the coefficients shows that until 1898, there is monotonic growth in railroad access in counties close to the path. In 1902 and 1903, prioritized counties continue to have advantage in railroad access through the backlash that had already started.

### 1.A.6 Other Factory-related Outcomes and Prefecture-level Control and Clustering

Table 1.A.7 shows the results for the other factory-related variables. See the main text for the interpretation. Table 1.A.8 shows the result with prefecture-level control and clustering corresponding to Table 1.A.7. The point estimates do not change dramatically from Table 1.A.7, and the effect on steam power and the share of people working in steam-powered factories are still significant. The effect on population becomes insignificant, suggesting a high correlation in the error term of the population within prefectures. However, when I analyze the heterogeneous impact analysis by using the same specification as in Table 1.10 with prefecture-level control and clustering, I find a significant heterogeneous effect on population growth between 1885 and 1920, although I do not discuss this in this study for brevity. Overall, I think that prefecture-level control and clustering do not affect the implications of the main results.

### 1.A.7 Additional Specification for the Cost Function

In the main result, I simply assume the following cost function when passing mesh *i*:

$$Cost_i = Ave.Slope_i \tag{1.9}$$

However, for the robustness check, I tried additional specifications, which are

$$Cost_i = 1 + Ave.Slope_i \tag{1.10}$$

and

$$Cost_i = Ave.Slope_i * \mathbf{1}[Ave.Slope_i > 25\ per\ mill] \tag{1.11}$$

[86] 1885 is not the year before construction started, and thus there may be concern about bad control. However, most construction started after 1885, and the main results do not change by including this variable.

Table 1.A.4: IV and Railroad Expansion

| | (1) | (2) |
|---|---|---|
| Distance from Path * 1889 | 0.0223 | 0.0163 |
| | (0.0192) | (0.0193) |
| Distance from Path * 1890 | -0.116*** | -0.0865** |
| | (0.0335) | (0.0290) |
| Distance from Path * 1891 | -0.203*** | -0.179*** |
| | (0.0392) | (0.0366) |
| Distance from Path * 1892 | -0.308*** | -0.288*** |
| | (0.0477) | (0.0467) |
| Distance from Path * 1893 | -0.312*** | -0.293*** |
| | (0.0478) | (0.0469) |
| Distance from Path * 1894 | -0.301*** | -0.281*** |
| | (0.0476) | (0.0471) |
| Distance from Path * 1895 | -0.351*** | -0.336*** |
| | (0.0500) | (0.0503) |
| Distance from Path * 1896 | -0.363*** | -0.349*** |
| | (0.0504) | (0.0512) |
| Distance from Path * 1897 | -0.336*** | -0.324*** |
| | (0.0505) | (0.0515) |
| Distance from Path * 1898 | -0.353*** | -0.352*** |
| | (0.0539) | (0.0541) |
| Distance from Path * 1899 | -0.282*** | -0.287*** |
| | (0.0609) | (0.0600) |
| Distance from Path * 1900 | -0.252*** | -0.261*** |
| | (0.0596) | (0.0594) |
| Distance from Path * 1901 | -0.257*** | -0.262*** |
| | (0.0601) | (0.0599) |
| Distance from Path * 1902 | -0.229*** | -0.233*** |
| | (0.0608) | (0.0597) |
| Distance from Path * 1903 | -0.175** | -0.175** |
| | (0.0614) | (0.0602) |
| County FE and Year FE | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes |
| Log Area * Year FE | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes |
| Initial Population Density * Year FE | No | Yes |
| (Main) Traditional Road * Year FE | No | Yes |
| N | 7792 | 7792 |
| N County | 487 | 487 |

*N* represents the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. I use data from 1888 to 1903. *Distance from Path$_i$* is the Euclidean distance (100 km) from the path to county $i$.

Table 1.A.5: Main Result using Cross Sectional Variation: Effect of Railroad Access in 1892 on Technology Adoption in 1902

| | IV | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *First Stage: Railroad Access Dummy in 1892* | | | | |
| Distance from Path (100km) | -0.477*** | -0.436*** | -0.494*** | -0.454*** |
| | (0.0593) | (0.0579) | (0.0591) | (0.0577) |
| *Panel A: Steam Power [HP]* | | | | |
| Railroad (1892) | 220.2** | 225.8** | 180.9** | 179.5** |
| | (69.04) | (76.45) | (57.45) | (61.58) |
| *Panel B: Number of Steam Engines* | | | | |
| Railroad (1892) | 9.696** | 9.241* | 7.975** | 7.254* |
| | (3.487) | (3.902) | (2.922) | (3.176) |
| Sum of Slope | Yes | Yes | Yes | Yes |
| Log Flat Area | Yes | Yes | Yes | Yes |
| Log Area | Yes | Yes | Yes | Yes |
| Distance to Coast | Yes | Yes | Yes | Yes |
| Initial Population Density | No | Yes | No | Yes |
| (Main) Traditional Road | No | Yes | No | Yes |
| Dist. to Destinations | No | No | Yes | Yes |
| First KP-Stat | 63.86 | 55.67 | 68.88 | 60.77 |
| N | 487 | 487 | 487 | 487 |

*N* shows the sample size. Robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use the data in 1902.

Table 1.A.6: Result without IV: Effect of Railroads, 1888–1902

| | DID | | | |
|---|---|---|---|---|
| | Steam Power [HP] | | Steam Engines | |
| | (1) | (2) | (3) | (4) |
| Railroad (-1892) | 48.63 | 41.30 | 1.942 | 1.274 |
| | (52.42) | (57.28) | (1.748) | (1.812) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | No | Yes | No | Yes |
| (Main) Traditional Road * Year FE | No | Yes | No | Yes |
| N | 974 | 974 | 974 | 974 |
| N County | 487 | 487 | 487 | 487 |

*N* represents the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. I use data in 1888 and 1902.

Equation (1.10) is similar to the functional form considered in Faber (2014). By adding a constant term, not only the slope, but also the moving cost in a flat area is now considered, and consequently, the slope is considered to be less important than in the main specification.

Equation (1.11) uses an indicator function to drop the slope information below 25 per mill. The motivation is that the Railroad Ministry set 25 per mill as the maximum slope for mountainous areas (Nihon Kokuyu Tetsudo, 1969) given the capacity of locomotives. By having specifications, the cost-minimizing path could avoid steeper areas more than the main specification.

Newly calculated paths and paths using the main specification are depicted in Figure 1.A.3. They are similar, but there is one line where the new specifications consistently diverge from the main specification. In this area, the new cost-minimizing paths are along the coastline, which was not chosen as the main line in reality. This is because when they constructed the mainline to the north, there was already a line from Tokyo to Takasaki, which was originally constructed from Tokyo to the west. This route was abolished because the route to the west from Takasaki was too steep as explained in the text, but they extended the line from Takasaki to reach northern area, which is why the line in inland areas, and not the coastline, was chosen.

The result when using these alternative cost functions is in Table 1.A.9. Overall, the results in Panels B and C with the alternative IVs show the effect on steam power and

Table 1.A.7: Other Outcomes: Effect of Railroads, 1888–1902

| | DID-IV | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A: Number of Steam-Powered Factories* | | | | |
| Railroad (-1892) | 4.557* | 2.589 | 5.150* | 3.328 |
| | (2.220) | (2.134) | (2.335) | (2.216) |
| *Panel B: Number of Water-or-Steam-Powered Factories* | | | | |
| Railroad (-1892) | -0.428 | -3.249 | -0.0876 | -2.728 |
| | (4.830) | (5.113) | (4.841) | (5.091) |
| *Panel C: Workers in Steam-Powered Factory / Population* | | | | |
| Railroad (-1892) | 0.00767** | 0.00616* | 0.00826** | 0.00678* |
| | (0.00284) | (0.00270) | (0.00297) | (0.00281) |
| *Panel D: Workers in Factory / Population* | | | | |
| Railroad (-1892) | 0.00214 | 0.000623 | 0.00301 | 0.00162 |
| | (0.00397) | (0.00424) | (0.00401) | (0.00428) |
| *Panel E: Log Population* | | | | |
| Railroad (-1892) | 0.0748+ | 0.0627 | 0.0383 | 0.0213 |
| | (0.0385) | (0.0402) | (0.0381) | (0.0397) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | No | Yes | No | Yes |
| (Main) Traditional Road * Year FE | No | Yes | No | Yes |
| Dist. to Destinations * Year FE | No | No | Yes | Yes |
| First KP-stat | 41.52 | 38.15 | 40.91 | 36.48 |
| N | 956 | 956 | 956 | 956 |
| N County | 487 | 487 | 487 | 487 |

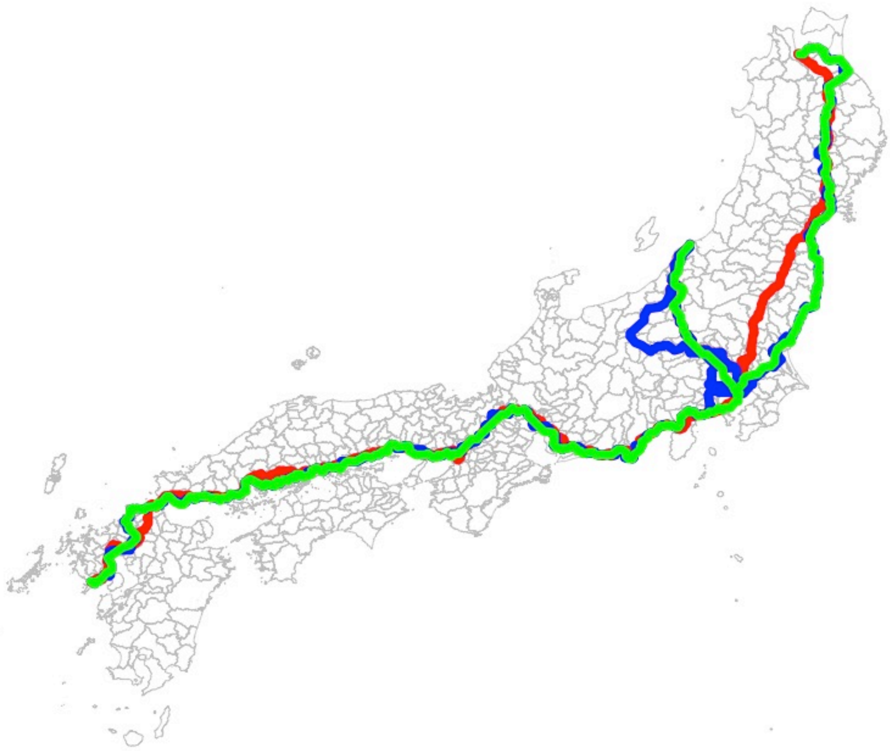
*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888 and 1902.

Table 1.A.8: Robustness Check with Prefecture-level Control and Clustering: Effect of Railroads, 1888–1902

| | DID-IV | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A: Steam Power [HP]* | | | | |
| Railroad (-1892) | 288.1* | 295.2+ | 253.8* | 255.3* |
| | (145.2) | (154.8) | (107.8) | (110.8) |
| *Panel B: Number of Steam Engines* | | | | |
| Railroad (-1892) | 20.86* | 20.24* | 19.06* | 18.16* |
| | (9.557) | (9.725) | (8.582) | (8.408) |
| *Panel C: Number of Steam-Powered Factories* | | | | |
| Railroad (-1892) | 3.946 | 1.873 | 4.759 | 2.777 |
| | (2.676) | (2.550) | (3.058) | (2.763) |
| *Panel D: Number of Water-or-Steam-Powered Factories* | | | | |
| Railroad (-1892) | -0.957 | -3.736 | -0.443 | -3.183 |
| | (7.900) | (7.765) | (7.957) | (7.791) |
| *Panel E: Workers in Steam-Powered Factory / Population* | | | | |
| Railroad (-1892) | 0.00706** | 0.00527* | 0.00790** | 0.00618** |
| | (0.00257) | (0.00240) | (0.00263) | (0.00233) |
| *Panel F: Workers in Factory / Population* | | | | |
| Railroad (-1892) | 0.00175 | 0.000130 | 0.00273 | 0.00120 |
| | (0.00502) | (0.00553) | (0.00506) | (0.00554) |
| *Panel G: Log Population* | | | | |
| Railroad (-1892) | 0.0712 | 0.0531 | 0.0373 | 0.0152 |
| | (0.0742) | (0.0736) | (0.0750) | (0.0750) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | No | Yes | No | Yes |
| (Main) Traditional Road * Year FE | No | Yes | No | Yes |
| Dist. to Destinations | No | No | Yes | Yes |
| Prefecture Geography * Year FE | Yes | Yes | Yes | Yes |
| First KP-stat | 12.11 | 12.19 | 12.98 | 12.70 |
| N | 956 | 956 | 956 | 956 |
| N County | 487 | 487 | 487 | 487 |

Prefecture-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888 and 1902. *Prefecture Geography* includes the prefecture-level average slope and share of the flat area.

Figure 1.A.3: IV with Other Specifications



engines. The results do not change qualitatively. I also see the robustness of Table 1.9 and Table 1.10 by using these alternative cost specifications, and I find similar results, although I omit the results for brevity.

The effect on steam power, which is the main outcome of this study, is lower in Panels B and C than in Panel A. To determine the reason, I compare the industry-specific effects in Table 1.A.9. The results show that the difference in the effect on steam power between the main result and the result with alternative IVs arises from the effect on the mining industry. The difference in the effect on total steam power is roughly 70–80 and the difference in the effect on the mining industry's steam power is 60–70. This is because the location of the mining industry is sensitive to the location of natural resources and subtle differences in the specification for the IV.

### 1.A.8 Omitting Subsample

For further robustness checks, I drop some subsamples that may affect my main results. I compare the result in column (1) of Table 1.4 with the results obtained by dropping four regions, Shikoku Island, the western part of Honshu Island, Gifu prefecture, and the counties that had already gained railroad access in 1885. The reasons for this selection are as follows. First, Shikoku Island does not have any destinations as shown in Figure

Table 1.A.9: Robustness Checks on the Main Results during 1888–1902: IV with the Alternative Cost Function

| | Steam Power [HP] | Steam Engines |
|---|---|---|
| | (1) | (2) |
| *Panel A: Main Specification* | | |
| Railroad (-1892) | 299.0** | 21.16*** |
| | (102.0) | (6.015) |
| *Panel B: Adding Constant Term* | | |
| Railroad (-1892) | 228.0** | 15.15*** |
| | (79.90) | (4.600) |
| *Panel C: Ignoring* < 25 *per mill* | | |
| Railroad (-1892) | 219.3** | 10.87** |
| | (74.80) | (3.856) |
| County FE and Year FE | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes |
| Log Area * Year FE | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes |
| First KP-stat for Panel A | 41.52 | 41.52 |
| First KP-stat for Panel B | 56.48 | 56.48 |
| First KP-stat for Panel C | 50.29 | 50.29 |
| N | 974 | 974 |
| N County | 487 | 487 |

*N* represents the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888 and 1902. Panel A uses the main IV specification in equation (1.9), which are just replications of the results. Panels B and C use an alternative specification for the IV, equations (1.10) and (1.11).

Table 1.A.10: Robustness Checks on the Industry-specific Effects during 1888–1902: IV with the Alternative Cost Function

| | DID-IV | | | |
|---|---|---|---|---|
| | Steam Power [HP] | | | |
| | Total | Domestic | Mine | Textile |
| | (1) | (2) | (3) | (4) |
| *Panel A: Main Specification* | | | | |
| Railroad (-1892) | 299.0** | 5.024+ | 184.9* | 34.15 |
| | (102.0) | (2.606) | (80.73) | (35.29) |
| *Panel B: Adding Constant Term* | | | | |
| Railroad (-1892) | 228.0** | 4.368+ | 124.3* | 41.84 |
| | (79.90) | (2.247) | (58.99) | (31.82) |
| *Panel C: Ignoring < 25 per mill* | | | | |
| Railroad (-1892) | 219.3** | 4.146* | 115.6* | 50.09+ |
| | (74.80) | (2.081) | (55.50) | (26.37) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| First KP-stat for Panel A | 41.52 | 41.52 | 41.52 | 41.52 |
| First KP-stat for Panel B | 50.29 | 50.29 | 50.29 | 50.29 |
| First KP-statfor Panel C | 56.48 | 56.48 | 56.48 | 56.48 |
| N | 974 | 974 | 974 | 974 |
| N County | 487 | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888 and 1902. Panel A uses the main IV specification in equation (1.9), which are just replications of the results. Panels B and C use an alternative specification for the IV, equations (1.10) and (1.11).

Table 1.A.11: Robustness Checks on the Main Results during 1888–1902: Main Result without the Neighboring Counties of Destinations

| | DID-IV | | | |
|---|---|---|---|---|
| | Steam Power [HP] | | Steam Engines | |
| Excluded Neighbors of Destinations | 15 km | 30 km | 15 km | 30 km |
| | (1) | (2) | (3) | (4) |
| Railroad (-1892) | 258.8** | 226.0** | 17.99** | 16.98** |
| | (90.39) | (84.56) | (5.777) | (5.704) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | Yes | Yes | Yes | Yes |
| (Main) Traditional Road * Year FE | Yes | Yes | Yes | Yes |
| Distance to Destinations * Year FE | Yes | Yes | Yes | Yes |
| First KP-stat | 36.09 | 35.98 | 36.09 | 35.98 |
| N | 964 | 934 | 964 | 934 |
| N County | 482 | 467 | 482 | 467 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888 and 1902. In columns (1) and (3), I exclude counties within 15 km from the destination counties. In columns (2) and (4), I exclude counties within 30 km from the destination counties.

Table 1.A.12: Omitting Subsample

| | DID-IV | | | | | | |
|---|---|---|---|---|---|---|---|
| | Steam Power [HP] | | | | | | |
| | No Omit | Shikoku | West | Gihu | Rail in 1888 | Steam in 1888 | Tokyo –Osaka |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Railroad (-1892) | 304.3** | 300.7** | 436.8* | 306.0** | 267.5** | 168.6+ | 403.1* |
| | (110.6) | (115.1) | (170.9) | (109.4) | (99.14) | (98.38) | (168.5) |
| County FE and Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| (Main) Traditional Road * Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| First KP-Stat | 37.65 | 33.98 | 27.87 | 38.31 | 42.26 | 30.31 | 25.03 |
| N | 974 | 906 | 770 | 948 | 856 | 786 | 786 |
| N County | 487 | 453 | 385 | 474 | 428 | 393 | 393 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888 and 1902. Columns (2), (3) and (4) omit Shikoku Island, the prefectures between Kyoto and Shimonoseki, and Gifu prefecture respectively. Column (5) omits the counties that had railroad access in 1888 and column (6) omits the counties that had steam power in 1888. Column (7) omits the prefectures on the road from Tokyo to Osaka, and Osaka to Kobe, namely, Tokyo, Kanagawa, Shizuoka, Aichi, Gifu, Shiga, Kyoto, Osaka, and Hyogo.

1.15. I drop the western part of Honshu Island, because the original statement by *Nihon Tetsudo* did not include Shimonoseki as its destination, and only noted Kyoto as a destination. As I explained in the main text, Shimonoseki was a natural selection as a final destination since it is the gateway to Kyushu Island, but the inclusion may cause some bias in the main results. I exclude all the prefectures on the possible routes from Kyoto to Shimonoseki. Gifu prefecture is a concern in terms of a consistent unit of measurement throughout my sample period, because in this prefecture there were many changes in counties at the village level, thereby rendering it difficult to construct a truly consistent unit of measurement. Finally, for further robustness checks, I exclude counties that had already gained railroad access in 1888 or counties that had no steam power in 1888. Table 1.A.12 shows the results. Each column shows the result omitting the subsamples, but the results are fairly robust. Only the result without the western part of Honshu Island shows different estimates, but this would be because this area is mostly Sanyo-San'in region where railroad did not decrease trade cost dramatically. Further, observe the significant effect in column (6) and (7), which implies that the effect in the main result is partly coming from the adoption in counties without any steam power in 1888 and county not on the Tokyo to Osaka traditional road (Tokaido).

### 1.A.9 Long-term Effects

As shown in Figure 1.1, after the 1900s, a less prioritized county far from the cost-minimizing path started to get railroad access. By 1920, the advantage of the prioritized county had almost disappeared. Now, I analyze the following reduced-form model to see the time series pattern between the distance to the path and outcome:

$$\begin{aligned} & Outcome_{it} = Distance\ from\ the\ Path_i * Year\ FE \\ & + Local\ Geographical\ Controls_i * Year\ FE + Year\ FE + County\ FE + u_{it} \quad , \end{aligned} \tag{1.12}$$

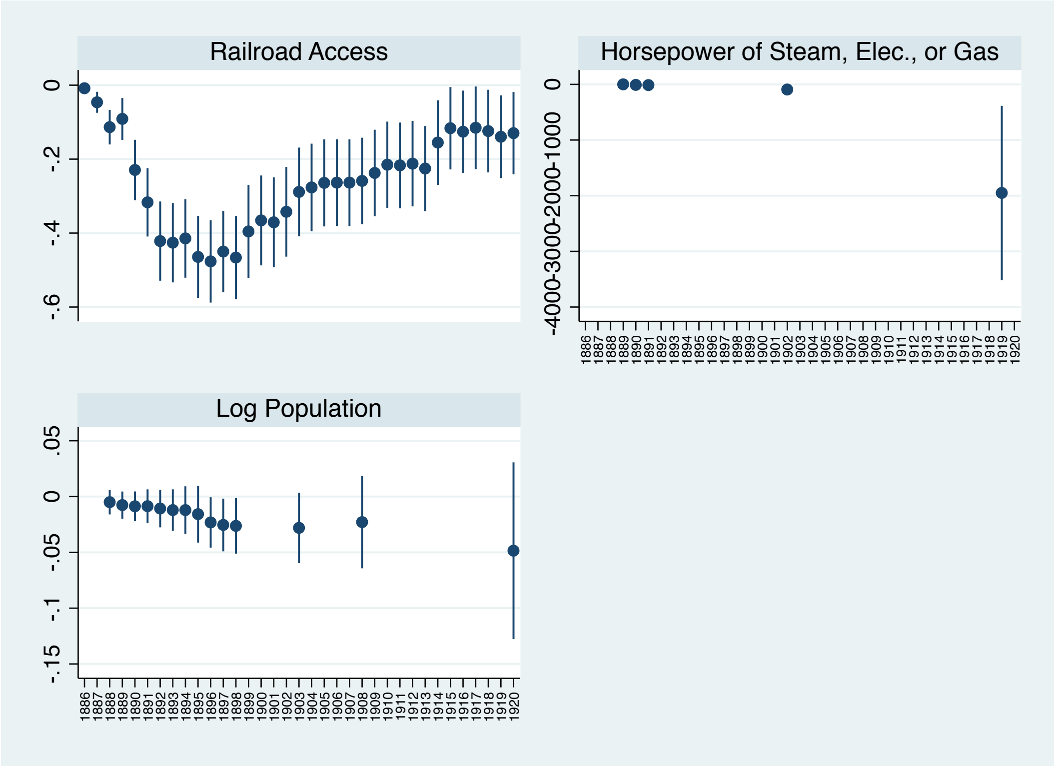
where $Outcome_{it}$ is the log of the population and steam power for 1902; and steam, electricity, or gas power for 1920 because by 1920 electricity and gas had become popular as advanced technologies. The coefficients for $Distance\ from\ the\ Path_{it} * Year\ FE$ are of main interest; if there is no persistent effect, then the coefficients of railroad access and outcomes will show a similar pattern. However, if there is persistence, then even after the coefficients of railroad access become zero, the coefficients for outcomes will not become zero or may even diverge if the positive feedback effect is sufficiently strong.

To show the whole pattern, I rely on a graphical presentation. Figure 1.A.4 shows the results, and each panel represents the coefficients of $Distance\ from\ the\ Path_i * Year\ FE$ for each outcome. First, the catching up in railroad access started around 1898, when the coefficient started to go back to zero. In line with this railroad effect, until 1902, less prioritized counties had lower horsepower and a lower population as shown in the previous regression results.

However, they fail to catch up in engine power as shown in the graph. Moreover, the gap widened even though less prioritized counties did catch up in railroad access by 1920. We can see this relationship by using data from 1888 and 1919 or 1920; see the regression analysis in Table 1.A.25. However, identifying the exact mechanism is beyond the scope of this study. This result has an implication for policymakers: the decision of where to build railroads *first* may create a large gap in the long term. Redding et al. (2010) and Bleakley and Lin (2012) also find that the initial advantage in transportation lasts for the long term through sunk costs and/or path dependence. For the population, early adopters still have an advantage, although its SE has increased and is insignificant.[87] This high SE could be because of the higher variance in population growth, which is often observed during urbanization.

[87] Berger and Enflo (2017) find long term impact on the population in a similar setting in Sweden. They find no difference in physical capital and thus explain this effect by path dependency.

Figure 1.A.4: Reduced-form Effects in the Long Term




## 1.A.10 Comparison with Tang (2014)

Note that the specification in Table 1.A.14 has an implication to compare the results in Tang (2014), which uses a prefecture-level treatment variable with a prefecture-level data and estimates the impact of railroad access by a simple DID analysis. Unlike Tang (2014), I find significant effect of prefecture-level railroad access to the outcome. When I use a simple DID without any controls as in Tang (2014) in Table 1.A.15, I did not find significant impacts of railroads on steam power adoption. Hence, the difference in the main result partially stems from the different identification strategies, though the outcomes and time windows are not exactly the same between the two papers (Tang (2014) sees firm capital or count from 1883 to 1893).

Table 1.A.13: Displacement Effect? Analysis by using Different Treatment Variables

| | Steam Power [HP] | | | Steam Engines | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Railroad (-1892) | 299.0** | | | 21.16*** | | |
| | (102.0) | | | (6.015) | | |
| Railroad (-1892) within 50 km | | 275.3** | | | 19.48** | |
| | | (96.73) | | | (6.016) | |
| Neighbor's Railroad within 50 km | | | 57.10 | | | -6.043 |
| | | | (198.6) | | | (5.840) |
| County FE and Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| First KP-stat | 41.52 | 37.44 | 12.21 | 41.52 | 37.44 | 12.21 |
| N | 974 | 974 | 974 | 974 | 974 | 974 |
| N County | 487 | 487 | 487 | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. I use data in 1892 and 1902. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. *Railroad (-1892) within 50 km*$_{it}$ takes one if *Railroad (-1892)*$_{it}$ takes one or *Railroad (-1892)*$_{jt}$ of any of neighboring counties within 50 km takes one. This is instrumented by *Distance to the Path*$_i *$ *Year FE*$_t$ as in the main specification. Of the counties that switch on *Railroad (-1892) within 50 km*$_{it}$ between 1888 and 1902, 29 percent do so because of their neighbor's access. *Neighbor's Railroad within 50 km*$_{it}$ takes one if *Railroad (-1892)*$_{it}$ takes zero but *Railroad (-1892) within 50 km*$_{it}$ takes one. This is instrumented by *Adjacent to the Path*$_i *$ *Year FE*$_t$, where *Adjacent to the Path*$_i$ takes one if *Distance to the Path*$_i$ is from 20 km to 50 km.

Table 1.A.14: Displacement Effect? Analysis by using Prefecture-level Treatment Variable

| | DID-IV | | | |
|---|---|---|---|---|
| | Steam Power [HP] | | Steam Engines | |
| Prefecture-level Railroad Access (-1892) | 331.3* | 317.5* | 18.56+ | 16.91+ |
| | (152.8) | (143.4) | (9.726) | (9.064) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | No | Yes | No | Yes |
| (Main) Traditional Road * Year FE | No | Yes | No | Yes |
| First KP-stat | 5.974 | 6.298 | 5.974 | 6.298 |
| N | 974 | 974 | 974 | 974 |
| N County | 487 | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The prefecture-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. I use data in 1888 and 1902. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument

Table 1.A.15: Simple DID Analysis using Prefecture-level Treatment as in Tang (2014)

| | DID | | | |
|---|---|---|---|---|
| | 1889-1892 | | 1889-1902 | |
| | Steam Power | Steam Engines | Steam Power | Steam Engines |
| | (1) | (2) | (3) | (4) |
| Prefecture-level Railroad Access (-1892) | 105.2 | -0.0582 | 23.44 | 1.698+ |
| | (79.29) | (3.226) | (17.97) | (0.926) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| N | 974 | 974 | 974 | 974 |
| N County | 487 | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The prefecture-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. I use data in 1888 and 1891 in column (1) and (2) and at 1888 and 1902 in column (3) and (4).

Table 1.A.16: Displacement Effect? Analysis by using Shikoku Island

| Compared with counties # km from the Path: | 20 km - 50 km | | > 50 km | |
|---|---|---|---|---|
| | Power | Engines | Power | Engines |
| | (1) | (2) | (3) | (4) |
| Shikoku Island * 1902 | -28.97 | -1.587 | 45.88 | -0.817 |
| | (50.53) | (1.076) | (31.28) | (1.001) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| N | 274 | 274 | 242 | 242 |
| N County | 137 | 137 | 121 | 121 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. I use data in 1892 and 1902. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. In columns (1) and (2), I compared counties in Shikoku Island with counties in other islands 20–50 km far from the path. In columns (3) and (4), I compare counties in Shikoku Island with counties in other islands more than 50 km far from the path.

Table 1.A.17: Short-term Effects of Railroads, 1888–1891

| | DID | DID-IV | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: Steam Power [HP]* | | | | | |
| Railroad (-1892) | 8.062 | 63.07$^{*}$ | 55.55$^{+}$ | 53.86$^{*}$ | 43.18$^{+}$ |
| | (6.809) | (26.40) | (29.76) | (23.66) | (25.80) |
| *Panel B: Log Population* | | | | | |
| Railroad (-1892) | 0.00458$^{+}$ | 0.0112 | 0.00646 | 0.00467 | -0.00109 |
| | (0.00235) | (0.0118) | (0.0129) | (0.0116) | (0.0126) |
| County FE and Year FE | Yes | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | No | No | Yes | No | Yes |
| (Main) Traditional Road * Year FE | No | No | Yes | No | Yes |
| Dist. to Destinations | No | No | No | Yes | Yes |
| First KP-stat | | 13.34 | 12.69 | 13.84 | 12.69 |
| N | 1948 | 1948 | 1948 | 1948 | 1948 |
| N County | 487 | 487 | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888, 1889, 1890, and 1891. Because of the low F-values in the first stage, I estimate the same model with LIML or use the linear trend to be interacted with the distance to the path as an alternative instrument to increase the F-values as well, but the results do not change qualitatively.

Table 1.A.18: Heterogeneous Effect of Railroads on the Log of Population, 1888–1891

| | Log Population | | | |
|---|---|---|---|---|
| | DID | DID-IV | | |
| | (1) | (2) | (3) | (4) |
| Railroad Access | 0.00588 | 0.00181 | 0.000848 | -0.00400 |
| | (0.0115) | (0.0131) | (0.0113) | (0.0129) |
| Railroad * Initial Log Pop Density | -0.000447 | -0.000947 | 0.00361 | 0.00282 |
| | (0.0138) | (0.0139) | (0.0137) | (0.0139) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | Yes | Yes | Yes | Yes |
| (Main) Traditional Road * Year FE | No | Yes | No | Yes |
| Dist. to Destinations | No | No | Yes | Yes |
| First KP-stat | 6.022 | 7.525 | 7.707 | 7.423 |
| N | 1948 | 1948 | 1948 | 1948 |
| N County | 487 | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888, 1889, 1890, and 1891. Because of the low F-value in the first stage, I estimate the same model with LIML or used the linear trend to be interacted with the distance to the path as an alternative instrument to increase the F-values as well, but the results do not change qualitatively.

Table 1.A.19: Coal Price Channel?

| | Steam Power (Non-Mining) [HP] | | | |
|---|---|---|---|---|
| | 1888-1891 | | 1888-1902 | |
| | (1) | (2) | (3) | (4) |
| Railroad (-1892) | 47.97+ | 27.76 | 92.83 | 70.14 |
| | (24.93) | (27.23) | (59.05) | (60.07) |
| Railroad * Distance to Coal | -2.623 | 15.38 | 33.61 | 116.9 |
| | (14.55) | (24.15) | (38.89) | (90.06) |
| Railroad * Initial Population Density | | 43.55 | | 213.5 |
| | | (35.93) | | (165.6) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | Yes | Yes | Yes | Yes |
| (Main) Traditional Road * Year FE | Yes | Yes | Yes | Yes |
| Dist. to coal * Year FE | Yes | Yes | Yes | Yes |
| First KP-stat | 5.165 | 3.130 | 17.90 | 5.276 |
| N | 1948 | 1948 | 974 | 974 |
| N County | 487 | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. The dependent variable is steam power, excluding the mining industry.

Table 1.A.20: Information Channel? The Effect of Railroad on Expositions

| | N of Expos | | Total Days of Expos | |
|---|---|---|---|---|
| | OLS | IV | OLS | IV |
| | (1) | (2) | (3) | (4) |
| Railroad County Prop (-1892) | 2.671 | 6.261 | 57.80 | 69.46 |
| | (5.483) | (11.97) | (51.74) | (89.99) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | Yes | Yes | Yes | Yes |
| Coastal * Year FE | Yes | Yes | Yes | Yes |
| (Main) Old Road * Year FE | Yes | Yes | Yes | Yes |
| First KP-stat | | 17.77 | | 17.77 |
| N | 504 | 504 | 504 | 504 |
| N Pref | 36 | 36 | 36 | 36 |

*N* shows the sample size and *N Pref* shows the number of prefectures. Prefecture-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use the data from 1886 to 1900, except 1888, 1890, and 1895. I exclude Tokyo, Osaka, Kyoto, Kanagawa, and Hyogo as outliers, but the qualitative results do not change by excluding them. *N of Expo* is the number of expositions and *Total Days of Expo* is the total days the expositions were open, taken from *Noshomu Tokeihyo*.

Table 1.A.21: Industry-specific Effects: The Effect of Railroads on Steam Power, 1888–1902

| | Domestic | Mining | Textile |
|---|---|---|---|
| | (1) | (2) | (3) |
| Railroad (-1892) | $5.148^{+}$ | $215.9^{*}$ | 17.20 |
| | (2.822) | (91.59) | (36.84) |
| (Normalized coefficient) | $0.771^{+}$ | $1.011^{*}$ | 0.190 |
| County FE and Year FE | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes |
| (Main) Traditional Road * Year FE | Yes | Yes | Yes |
| Initial Population Density * Year FE | Yes | Yes | Yes |
| First KP Stat | 38.15 | 38.15 | 38.15 |
| N | 974 | 974 | 974 |
| N County | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888 and 1902.

Table 1.A.22: Robustness Check: Main Result without Telephone-connected Counties

| | DID-IV | | | |
|---|---|---|---|---|
| | Steam Power [HP] | | Steam Engines | |
| | (1) | (2) | (3) | (4) |
| Railroad (-1892) | 255.7** | 267.8* | 21.34*** | 20.95** |
| | (97.01) | (106.8) | (6.134) | (6.677) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | No | Yes | No | Yes |
| (Main) Traditional Road * Year FE | No | Yes | No | Yes |
| First KP-stat | 41.25 | 36.91 | 41.25 | 36.91 |
| N | 960 | 960 | 960 | 960 |
| N County | 480 | 480 | 480 | 480 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888 and 1902. I exclude telephone-connected counties as of 1902 from the sample (i.e., those counties where Nagoya, Fukuoka, Kuwana, Sendai, Kumamoto, Hiroshima, and Kanazawa cities are located).

Table 1.A.23: Robustness Check: Main Result without Counties where State-owned Factories were Located

| | DID-IV | | | |
|---|---|---|---|---|
| | Steam Power [HP] | | Steam Engines | |
| | (1) | (2) | (3) | (4) |
| Railroad (-1892) | 295.0** | 278.6** | 20.53*** | 19.46** |
| | (102.9) | (106.0) | (6.090) | (6.541) |
| County FE and Year FE | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | No | Yes | No | Yes |
| (Main) Traditional Road * Year FE | No | Yes | No | Yes |
| First KP-stat | 39.36 | 35.37 | 39.36 | 35.37 |
| N | 956 | 956 | 956 | 956 |
| N County | 478 | 478 | 478 | 478 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888 and 1902. I exclude counties with state-owned factories.

Table 1.A.24: Robustness Check: Interest Rate and Instrument

| | 1888 | | 1888-1902 | |
|---|---|---|---|---|
| | Low | High | Low | High |
| | (1) | (2) | (3) | (4) |
| Distance to the Path | -0.0720 | -0.249 | | |
| | (0.472) | (0.947) | | |
| Railroad County Prop (-1892) | | | -1.699 | -2.113 |
| | | | (1.212) | (2.680) |
| County FE and Year FE | No | No | Yes | Yes |
| Sum of Slope (* Year FE) | Yes | Yes | Yes | Yes |
| Log Flat Area (* Year FE) | Yes | Yes | Yes | Yes |
| Log Area (* Year FE) | Yes | Yes | Yes | Yes |
| Initial Population Density (* Year FE) | Yes | Yes | Yes | Yes |
| Coastal (* Year FE) | Yes | Yes | Yes | Yes |
| (Main) Old Road (* Year FE) | Yes | Yes | Yes | Yes |
| First KP-stat | | | 13.52 | 13.52 |
| N | 36 | 36 | 539 | 539 |
| N County | | | 36 | 36 |

Source: Institute for Monetary and Economics Studies, Bank of Japan. Low and High mean the lowest and highest. The original data are monthly, and thus I take the average for each year. *N* shows the sample size and *N County* shows the number of counties. The robust SEs are in parentheses in columns (1) and (2) and county-level cluster robust SEs are in parentheses in columns (3) and (4). + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888 in columns (1) and (2) and 1888–1902 in columns (3) and (4). I exclude counties with state-owned factories.

Table 1.A.25: Long-Term Effect of Railroads during 1888–1919 or 1888–1920

| | DID | DID-IV | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: Steam Power [HP]* | | | | | |
| Railroad (-1892) | -56.54 | 6329.9* | 5252.1** | 5417.6** | 4305.2* |
| | (1100.3) | (2582.7) | (1881.5) | (2077.3) | (2016.3) |
| *Panel B: Log Population* | | | | | |
| Railroad (-1892) | 0.0689* | 0.141 | 0.102 | 0.0621 | 0.0138 |
| | (0.0333) | (0.114) | (0.122) | (0.113) | (0.120) |
| County FE and Year FE | Yes | Yes | Yes | Yes | Yes |
| Sum of Slope * Year FE | Yes | Yes | Yes | Yes | Yes |
| Log Flat Area * Year FE | Yes | Yes | Yes | Yes | Yes |
| Log Area * Year FE | Yes | Yes | Yes | Yes | Yes |
| Distance to Coast * Year FE | Yes | Yes | Yes | Yes | Yes |
| Initial Population Density * Year FE | No | No | Yes | No | Yes |
| (Main) Traditional Road * Year FE | No | No | Yes | No | Yes |
| Dist. to Destinations * Year FE | No | No | No | Yes | Yes |
| First KP-stat | | 41.52 | 38.15 | 40.91 | 36.48 |
| N | 974 | 974 | 974 | 974 | 974 |
| N County | 487 | 487 | 487 | 487 | 487 |

*N* shows the sample size and *N County* shows the number of counties. The county-level cluster robust SEs are in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *First KP-stat* shows the Kleibergen–Paap statistics to test for a weak instrument. I use data in 1888 and 1920 for steam power, and 1888 and 1919 for the population.

# Chapter 2

# Time Horizon of Government and Public Goods Investment: Evidence from Japan

## 2.1 Introduction

States play an important role in providing fundamental foundations for economic development such as peace, laws, and infrastructure. Therefore, it is difficult to imagine economic activity without states, but the timing and pace of development varies across regions. For example, in Japan, the first centralized political power, *yamato* is estimated to have emerged in the third century, which is more than hundreds of years after the Shang dynasty in China ruled their domains. Other regions, Mesoamerica, Mesopotamia, Egypt, and Indus also had a political organization in B.C. (Service, 1975). Regarding the pace of development, Western Europe experienced the development of states after the 17th century (Tilly, ed, 1975) while certain states, such as Syria, fail to function effectively and are faced with chaotic situations even in the 21st century. Therefore, the origin and development of states is one of the most important subjects in social sciences.

There are several theories to explain the origin of states, and Olson (1993, 2000) consider a longer time horizon as one of the key factors in the emergence of states. To understand its mechanism, assume there are autocrats without any conflict within their organization such as the principal-agent problem. These autocrats can exploit a portion of local economic output to consume and invest in local public goods or other state capacities, for example, property rights. Then, the autocrats who expect to govern their domain longer will have a stronger incentive to invest in local public goods or state capacities because doing so will yield higher economic returns and consumption in the long run. However, there are a limited number of empirical studies to examine this longer time horizon effect because it is hard to collect statistical data when a state is emerging or developing.

In this study, I examine the effect in the context of the Edo period (1615 to 1868) in Japan. The Edo period has several unique features; thus, it is a suitable context for the study of the longer time horizon effect. First, during the Edo period, local lords had administrative and judicial autonomy and were free from central power, that is, the Tokyo government. However, the Tokyo government could influence local governments by ordering the transfer of local lords (*tenpou*) along with all their officers (*samurai*). This unique variant of feudalism, a mixture of decentralized economics and centralized politics, provides an appropriate context to test for the longer time horizon effect.[1] Second, the transfers suddenly ended as a result of an executive leader (*shogun*) in the Tokyo government, which I use to examine the said effect. Following the death of the leader in 1651, the Tokyo government ceased most of the transfers, reducing transfer risks. This was true for local lords who had an insider identity, supported Tokugawa in the 1600 war, and who

[1]To the best of my knowledge, European feudalism had no such central power.

had a higher transfer risk than other local lords prior to the regime change. Because both the death and identity are plausibly exogenous to economic conditions, I can identify the effect of tenure risks using a difference-in-differences (DID) analysis.[2] Third, ethnic or religious conflicts between local lords were not a significant issue in Japan[3], and local lords did not know which local lords would be transferred to their administrative area (or constituency) to govern on their behalf. Therefore, I ignore the conflicts that cause a turnover effect, as discussed in Burgess et al. (2015), to focus on the time horizon effect.

I digitized several data sources to conduct the analysis. Information on local governments, such as the identity or date of transfer, is taken from a Japanese history dictionary, Nagahara, ed (1999), which summarizes the records maintained by the Tokyo government. Local governments' domains are identified through an investigation conducted by the Tokyo government on land ownership in the 1640s. Izumi (2008) compiled scattered documents of the investigations and discussed land ownership by village. For the outcome variable, I digitized Doboku-Gakkai (1994), which lists public works, such as irrigation systems, along with the name of the county and the era in which the project was initiated[4] based on documents dated 1912. Then, I constructed era and county-level data for the number of initiated projects.

Using data for 1615 to 1700, I find that the number of counties governed by insiders significantly increased the number of initiated projects financed by the local government following 1651. The effect is statistically and economically significant given the small number of projects during the period. Further, this result does not substantially change after controlling for factors such as county-specific trends, local geographies, or the size of local governments and is robust in the Poisson or negative binomial regressions. On the other hand, I find no effect on the number of initiated projects supported by those outside of the local governments implying that the main result cannot be explained by a local economic boom. The regime change might induce other policy shifts, which could affect the interpretation of the reduced-form effect; however, I find no systematic change in other policies such as in-kind transfers between the Tokyo government and local governments. Similarly, the death of an executive leader may cause other political consequences such as a political race to be appointed to officers in the Tokyo government although, I also find no effect of a leader's death in 1681 on the outcome when transfer risk did not change. Moreover, the main effect survives after controlling the area produced the highest-ranked

[2]For the impact of leaders' deaths, see Fisman (2001), Jones and Olken (2005), Jones and Olken (2009), and Besley et al. (2012)

[3]There was one major religious conflict, *Shimabara no Ran*, an uprising by the Christian people in the Nagasaki region to pursue their freedom of religion. However, this did not cause conflict among local lords and ended in the 1620s. Thus, I believe it does not affect the key narrative of this study.

[4]Japan adopted their own periodization system and the era changed quite often (for religious reasons) as I will discuss later.

officers, which reflects such career concern effect. Further, military investments by local lords remain unaffected, which is inconsistent with the explanation that peace promotes investment reallocation from military to economic infrastructure. I also address issues related to measurement errors in the number of projects and land ownership, and additional analyses suggest that the main result is not influenced by measurement errors.

Although these results imply a transfer risk effect on local pubic goods investment, the effect might comprise a longer time horizon effect and other effects, such as experience and income shock effects. Local governments who stayed longer in the local domain are likely to have more information or recover from a temporal income shock during a regime change. If these effects, excluding the longer time horizon effect, explain the main result, the effect of a regime change will be greater for these governments. However, there is no supportive evidence for heterogeneous impacts.

This study is related to several literatures. First, this study is related to studies that analyze the relationship between political tenure and local public goods provision or economic performance such as Li and Zhou (2005), Wright (2008), Bó and Rossi (2011), and Smart and Sturm (2013). These scholars examine bureaucrats in China, developing countries with dictatorships, and politicians in Argentina or the United States. In contrast, the present analysis investigates local governments within the same country to gain a greater understanding of the origin of states.[5] A within-country analysis elucidates the effect by comparing governments against the same institutional background.[6]

Second, this study is linked with limited empirical literature on the origin of states, such as Gennaioli and Voth (2015) and Raul Sanchez de la Sierra (2014, 2015), and my main contribution is the application of the historical context to study a longer time horizon effect with a DID estimator. Because the contexts are different, we also have different interpretations for the results. For example, Raul Sanchez de la Sierra (2014, 2015) analyze the behavior of bandits in the modern Eastern Congo and reveal that a shock to mineral demand led to the initial stage of state formation and the threat of military action by the central government against bandits' worsening behavior. On the other hand, my study analyzes the development of states rather than the emergence of states because, by 1615, local governments had already monopolized violence and taxation in their territories. Additionally, military threats are a different type of risk that could emerge from transfers. For instance, a government may increase military investments to fight against an opponent in the case of war and decrease public goods provisions, thus elevating the risk of destruction and discouraging public goods investments.[7]

[5]Politicians and bureaucrats with longer tenure have other related channels such as moral hazard and re-election motives.

[6]For example, ethnic conflicts may amplify the longer time horizon effect on public goods provision, as found in Burgess et al. (2015)

[7]Raul Sanchez de la Sierra (2015) denotes that this effect would be minimal because bandits had no

Third, this paper is connected to the literature discussing the effect of a longer horizon in various fields. For example, some papers analyze the impact of longer tenure of CEOs on their organization and payment structure (Bertrand and Mullainathan, 2001; Jenter and Kanaan, 2015) or the impact of strong security for farmers (Besley, 1995; Jacoby et al., 2002; Banerjee and Iyer, 2005; Fetzer and Marden, 2016) and slum-dwellers (Field, 2005; Field and Torero, 2006) on their investments while this study looks at different agents, autocrats.

Finally, this work is related to the empirical literature analyzing relationships between pre-modern institutions and economic developments or cultures. For example, Acemoglu et al. (2001), Tabellini (2010), Dell (2010), Michalopoulos and Papaioannou (2013), Dell et al. (2015), and Lowes et al. (2017) explore the long-term impact of pre-modern institutions on economies, cultures, and norms. My study complements this literature by investigating shorter term relationships between pre-modern institutions and historical economic variables as in De Long and Shleifer (1993) and Chaney (2013). Some Japanese economic history literature argues that the longer time horizon of the local domain was crucial to economic development following the *Sengoku* period (1467 to 1590) (Flath, 2005). The authors discuss this subject from a time-series perspective, and the key result of this study adds evidence to this viewpoint.

The remainder of the paper is organized as follows. Section 2.2 describes the conceptual framework used to examine the longer time horizon effect and discusses other potential effects associated with transfer risks. Section 2.3 explains the institutional background, data, and descriptive statistics. Section 2.4 presents the main result and robustness checks and other potential interpretations of the main result. Section 2.5 concludes.

## 2.2 Conceptual Framework

In this section, I elaborate on the longer time horizon effect. I predict that a higher transfer risk will reduce investments in local public goods and, then, explain the interpretation of the effect in the present context.

Consider a two-period model with an autocratic government that earns tax revenue from a fixed proportion of output in the private sector in its domain. For simplicity, assume that the private sector has inelastic labor supply and no means of investment or savings and, thus, the private sector consumes output after tax. Output in the private sector is a function of the level of local public goods, such as an irrigation system or dams. The government can choose the consumption and investment level for public goods in the initial period contingent on their revenue. If the government invests, the private sector

chance of winning against the central government.

will have a higher output in the subsequent period and the government will have a higher tax revenue. In the second period, the government consumes all the tax revenue.

The key factor of this framework is that the government faces the risk of transfer in the second period. Without loss of generality, assume that the government will have zero utility if a transfer occurs in the second period. Then, a higher transfer risk means a higher discount rate, which reduces investment. This is the longer time horizon effect.

First, this is similar to the effect of property rights on investment (Besley, 1995; Besley and Ghatak, 2010). Both exploitation risk for individuals and transfer risk for the government function in a similar manner. Second, this framework assumes that utility in the event of a transfer is invariant to public goods investment. This assumption is suitable in the present context as I will explain in the next section.

The outlined framework predicts that lower transfer risks will increase investment in local public goods. I want to interpret this as the longer time horizon effect, but there are other possible ways to interpret the transfer risk effect. Thus, I abstract from potential conflicts with other governments, which may affect the investment.[8] For example, the government knows that if their land is taken over by their enemy in the event of a transfer, then the enemy will benefit from the investments in the local domain. In addition, I abstract from the effect of experience and income shock. For example, over time, the government might accommodate information to improve yields on investments.[9] Further, immediately after the transfer, the government may be credit constrained because of their cost of moving. I test these stories by conducting a heterogeneous impact analysis later in the study.

## 2.3 Background and Data

In this section, I briefly explain the historical context prior to the Edo period and the emergence of political identity and describe the political and financial structure of the local government during the Edo period and the regime change in 1651, to exploit this regime change to estimate the longer time horizon effect. Then, I describe the agricultural investment in the period and why transfer will discourage it. The data description comes after that.

[8] See Besley and Persson (2011) and Burgess et al. (2015) as related literature.

[9] Political economy literature suggests that a longer tenure may result in increased corruption through collusive relationships with local agents (Campante et al., 2009; Coviello and Gagliarducci, 2016). However, I do not consider local public investment corruption in this paper because it will not benefit a particular type of agent in the private sector.

### 2.3.1 Background: Political Identity, Government Structure, and Transfer Policy

From the mid-15th century to the late 17th century, the *Sengoku* period, there were conflicts between local lords and no powerful central government to control them.[10] Local lords (*daimyo*) are viewed as typical "stationary bandits" (Olson, 1993) since they monopolized violence in their territory and collected tax. Following two attempts to establish a central authority by *Oda Nobunaga* and *Toyotomi Hideyoshi*, in 1600, *Tokugawa Ieyasu* won the biggest conflict (The Battle of *Sekigahara*) and established the Tokyo government (*Edo Bakuhu*) in 1603.

The conflict of 1600 separated local lords into those who were pro and anti-Tokugawa, in other words, insiders (*fudai*) or outsiders, (*tozama*). The identity was formally recognized In 1615, which is generally recognized as the start of the Edo period, *Tokugawa Ieyasu* seized dominant power by defeating the *Toyotomi* family at the Siege of Osaka. Following this victory, *Tokugawa Ieyasu* and his successors controlled approximately 300 local lords. The Tokyo government treated outsiders and insiders differently throughout the Edo period. For example, the Tokyo government appointed some insiders as high rank officers in the Tokyo government keeping their position as local lords. The Tokyo government also allocated anterooms in the castle in Tokyo for local lords based on their identity and size of their domain. Though a few local loads applied for changing status from outsider to insider aspiring high status of insiders or positions as officers in the Tokyo government and it was accepted in a few cases, basically the status was fixed at the clan level throughout the Edo period.

There is no clear historical evidence that the local lords who supported Tokugawa in the 1600 war were from more or less developed regions. Some lords were originally vassals of Tokugawa and, naturally, supported Tokugawa. Blood relationships were another determinant. Whether local lords played a military or bureaucratic role under *Toyotomi*'s hegemony is believed to have been a factor (Honda, 2010).[11] The effect of this non-random assignment on the main result is an empirical question that can be assessed by including controls such as local geographies or the size of government.[12]

The Edo period is characterized as a unique variant of the feudal system, that is, a mix of decentralization and centralization. Local lords could autonomously control eco-

[10]This section is based on basic historical facts and several books (Somada, 2011; Fujino, 1975, 2002).

[11]There was tension between these groups concerning the post-war process of the 1592 to 1598 invasion of Korea. Eventually, the militarist group supported Tokugawa in the war of 1600.

[12]Because certain insiders were originally militarist groups, there were concerns regarding the effect of different preferences on the main result. However, time-invariant preferences did not affect the key objective of this study because increased public investments by insiders following the policy change in 1651 cannot be explained. In addition, I will test the importance of this concern using data on the building and repair of castles.

nomic policy; specifically, they had their own courts to tackle disputes in their domain and the right to issue paper money. Hereinafter, I treat the local lords and their vassals as governors and officers of local governments and refer to such organizations as "local governments." The central government possessed its own vassals and domains in the nation (*Tenryo*). The central government collected land tax from these domains by using their vassals. There was no inter-governmental monetary transfer although there were in-kind transfers for military construction work, which will be discussed later. Thus, the Tokyo government was not as powerful as modern central governments or the Meiji central government following the end of the Edo period in 1868, which pursued centralization and modernization to avoid colonization by Western countries.

However, the Tokyo government had noteworthy similarities to present-day central governments in that it had the political authority to affect the tenure of local governments. The policy focus of this study is the Tokyo government's ability to relocate local governments, which is a symbol of their hegemony and the predecessor of the Tokyo government, *Toyotomi Hideyoshi* also had the power to conduct. With this policy, local governments were at the risk of being moved to a new location, which affected investments in local public goods given that the local governments would have to give up their land. The transfer entailed local lords, their vassals, and their families, as shown in Figure 2.1. Due to this harsh policy, some anecdotes show that local lords born in the Edo period perceived their domain as the land the Tokyo government asked them to keep, not their own land. A local lord born in 1609, *Ikeda Mitsumasa* wrote in 1651 to his officers that we are taking care of the land of the Tokyo government. Similarly, another local lord born in 1556, *Todo Takatora*, wrote a similar thing as his will in 1630 (Ozawa, 2003).

The purpose underlying the transfer was not explicitly recorded in official documents, but it has long been considered that the military played a role, particularly in the early 17th century. The Tokyo government attempted to place its supporters, insiders, in militarily important locations to monitor their past enemies, outsiders.[13][14] The Tokyo government accounted for their talent and age; for example, if a military important domain had a new, young local lord following the death of the previous lord, the Tokyo government would send senior insiders to the domain. In this case, the transfer would be with insiders. The data suggest an identical pattern for most transfers. Of the 151 transfer cases between 1615 and 1651, 94 were transfers of an insider to another insider's domain, 23 were transfers of an outsider to another outsider's domain, and the remainder were transfers

[13]Another category is *Shimpan*, who are close relatives of the Tokugawa clan. Since *Shimpan* were not affected by the policy change, as were the outsiders, I compare insiders with non-insiders (or outsiders and close relatives).

[14]In 1622, *Nobuyuki Sanada*, a local lord, mentioned in his letter to his vassal, *Ideura Tshushima-no-Kami*, that he was transferred to Matsushiro under the orders of the Tokyo government because of military reasons.

exclusively between an insider and outsider. Locational preferences, moving costs, and the ambiguity of the true productivity of the new territory made transfers a tedious and unfavorable process for local governments.

In 1651, the transfer risk decreased after a regime change. The third executive leader (*shogun*), *Tokugawa Iemitsu*, the grandson of the first executive leader, died at the age of 48, which was officially announced to local lords a day after the death. The late leader's son, *Tokugawa Ietsuna*, who at the time was only 11 years old, was designated to take his place. The young leader was supported by his cabinet members to avoid political confusion because of his lack of experience. The fear among the cabinet members presented the opportunity to put political pressure on the Tokyo government and protest Tokyo's severe local government policies. Immediately after the death of the third executive leader, several ex-officers whose governments were forfeited by the Tokyo government attempted a coup d'etat (*Keian* Uprising), which failed. Together with another failed coup d'etat (*Jouou* Uprising) in 1652, the cabinet members felt threatened and revised their harsh policies for local governments.

As mentioned, the key policy focus in this study is the reduced transfer risk in 1651. Transfers became rare, and this had a greater effect on insiders because they had been subject to a higher transfer risk before 1651. In addition, the Tokyo government allowed local lords to adopt a son prior to their death to avoid a forfeit, albeit with limited restrictions. Therefore, the death of the third executive leader was interpreted as the biggest turning point for the Tokyo government from a military (*Budan*) to a more bureaucratic government (*Bunchi*). Although only the change in the forfeit policy was officially announced, local lords will have expected the same for the transfer policy because both reflected the harshness of the Tokyo government against local lords. Some anecdotes show that local lords after 1651 perceived low transfer risk; A insider lord, *Naito* house, experienced two transfers in 1622 and 1747. At the transfer in 1747, a letter was sent from their office in Tokyo to their local domain, saying that transfer did not happen for more than 100 years and it surprises their office in Tokyo (Hibi, 2011). The dairy also documented the process of the unsuccessful coup in 1651 (Kanzaki, 1988). A Confucian scholar, *Ogyu Sorai* published his work, *Seidan*, in 1726-27. He commented that "[...] around the time of *Shimabara* conflict in 1638, transfer risk was higher for insiders and it was unfair. [...] Now, there are not so many differences between insiders and non-insiders." Moreover, *Uesugi Harunori* born in 1751 and wrote his will that we received our land from our ancestors. Ozawa (2003) compares this will with other wills above written by local lords in the early Edo period and denotes that the change in the policies by the Tokyo government affected local lords' philosophy as rulers.

I will exploit this regime change to identify the effects of a longer time horizon using

DID analysis. For cross-sectional variation, I use the political identity of local governments irrespective of insiders because they were at a higher transfer risk before 1651. For time-series variation, I use the period before and after 1651, when the rate of transfer risk particularly dropped for insiders. As I will show later in this study, I find a significant effect of tenure on the number of new public works.

To interpret the DID result by using regime change as the tenure risk effect, I assume (i) a parallel trend and (ii) the death of a leader does not affect other policies whose influence on local lords varies by identity. To address the parallel trend assumption, I examine whether the result is robust when adding controls such as a unit-level linear trend. For the other policy channels, I explicitly examine for evidence of a change in the policies in 1651. I find that the main result is robust to unit-level trends, and other policies were stable before and after 1651.

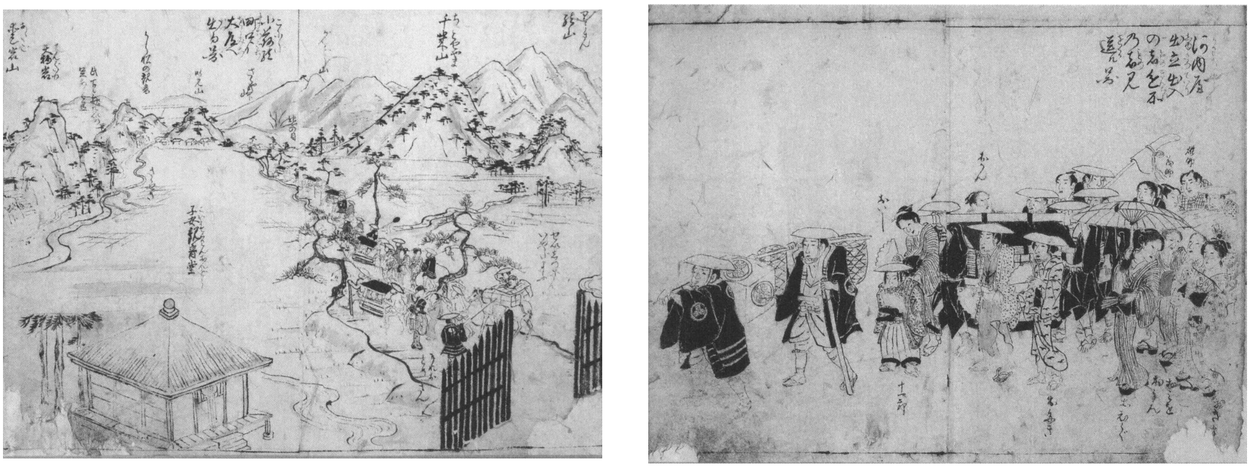
### 2.3.2 Background: Agricultural Investment

The decentralized economic structure encouraged local governments to increase their own tax revenue by agricultural investment. There are many records about agricultural projects such as developing new agricultural land, digging of irrigation canals and dams, land reclamation by drainage, financed by local lords. For example, an outsider's government in Okayama cultivated new fields in the domain and developed about 300 ha in the 17th century. Their main domain, Bizen province, increased its tax base by about 30 percent from the 1590s to 1728 (Doboku-Gakkai, 1994). Though the practice of the projects varies over the local governments, the local government typically bear the cost of equipment and force or hire farmers to work and after the completion of the project they sell the land to farmers with several years of tax exemption (*Kuwashita Nenki*) (Kikuchi, 1958). In the case of *Oki Shinden*, the biggest project in Okayama's domain, it cost about 20,000 *goku*, which is around 6 percent of total output in their domain.[15] The boom of investment was not limited to Okayama's domain; From 1598 to 1873, cultivated land increased from 2 million ha to 3.3 million ha in the whole nation.

Transfer will discourage such agricultural investment by local governments. Upon transfer, they were likely to receive almost the same productivity-adjusted amount of land, but local governments had to give up their returns on investment from the local domain; if the transfer occurred during an investment project, the project would not be accounted for. Even if a project was finished at the time of the transfer, the productivity gain was not considered because land productivity was calculated based on the 1590 land investigation. A collection of local laws and records about administration during the Edo period (*Jikata Ochibo Shu*) denotes that "newly cultivated land will not be accounted as de jure tax base

[15] The de-jure size of their domain was 315,000 *goku*

Figure 2.1: Okunigae Emaki (Transfer Narrative)



Owned by Yamada Family in Ashikaga City

at transfer, so it is not good for local lords." and by quoting this, Kikuchi (1958) has a view that transfer risk must have hindered investments by local lords.

### 2.3.3 Data Sources

I digitize three data sets to obtain information on transfers, land ownership, and the number of public works.

Before explaining each data source, I explain the unit of observation, which is more complicated than other data sets because of its historical context. I have two types of observation units, statistical and political borders. Statistical borders assume a hierarchical structure, that is, state, province, county, and village in descending order. These statistical borders were set under a centralized structure in 701 and were used as administrative borders; however, following the collapse of the central power in the 15th century, the administrative borders became statistical. Data using statistical boarders take the form "Village A was governed by lord B" or "A new public goods project started in County C in 1637." Political borders were largely determined by the civil wars before the Edo period and, as a result, often do not overlap statistical borders. Data using political borders are measured for each domain of the local governments; for example, taking the form "Lord D was appointed in Domain E in 1617 to 1620."

***Iwanami Nihonshi Jinten (Iwanami Dictionary of Japanese History)*** Nagahara, ed (1999) is a dictionary of Japanese history and a summarized version of diaries and records maintained by the Tokyo government, such as *Ryueihinamiki* in Figure 2.2. The dictionary is composed of a list of transfers along with the details of the local lord (e.g., name and identity) at the domain level and the year in which the transfer occurred. Using this list, I construct domain-year panel data for the transfer and identity.

***Kinsei Zenki Gosondaka to Ryoshu no Kiso Kenkyu (Research on Lords and their Domains in Early Pre-Modern Japan)*** This book (Izumi, 2008) is a collection of the records of land investigations (*Kenchi*) conducted by the Tokyo government in the 1640s.[16] The Tokyo government conducted several national land investigations to compile data on local lords' ownership of land and its productivity. Using the names of local lords for each village, I calculate the share of domains at the county level to connect domain-level transfer data with county-level outcome data.[17]

***Meiji Izen Nihon Dobokushi (History of Civil Engineering Before Meiji Era)*** (Doboku-Gakkai, 1994) is a collection of recorded public works prior to 1868 and edited by the Japanese Academy of Civil Engineering. The collection covers agricultural infrastructure, such as dams and irrigation systems, along with their name, location and county, bearer of costs, and the era in which the public works were initiated. The era changed 11 times during 1615 to 1700. I construct panel data of the number of new projects at the county and era level based on the financial supporter.[18][19] Unfortunately, the size or cost of projects were seldom recorded. To the best of my knowledge, there are no other county-level data that could be used as an outcome, such as production or population.

Using these three data sets, I construct domain-year data for transfers and political identities and county-era panel data with public projects for the main analysis. Because one county had several villages and there was possibly more than one domain per county, transfer events and political identities are measured as an average using the share of owners. For robustness checks, I examine whether the main result is attributed to transfer risks using other data types such as the number of in-kind transfers between the local governments and Tokyo government. I will return to this point after the discussion on the main results.

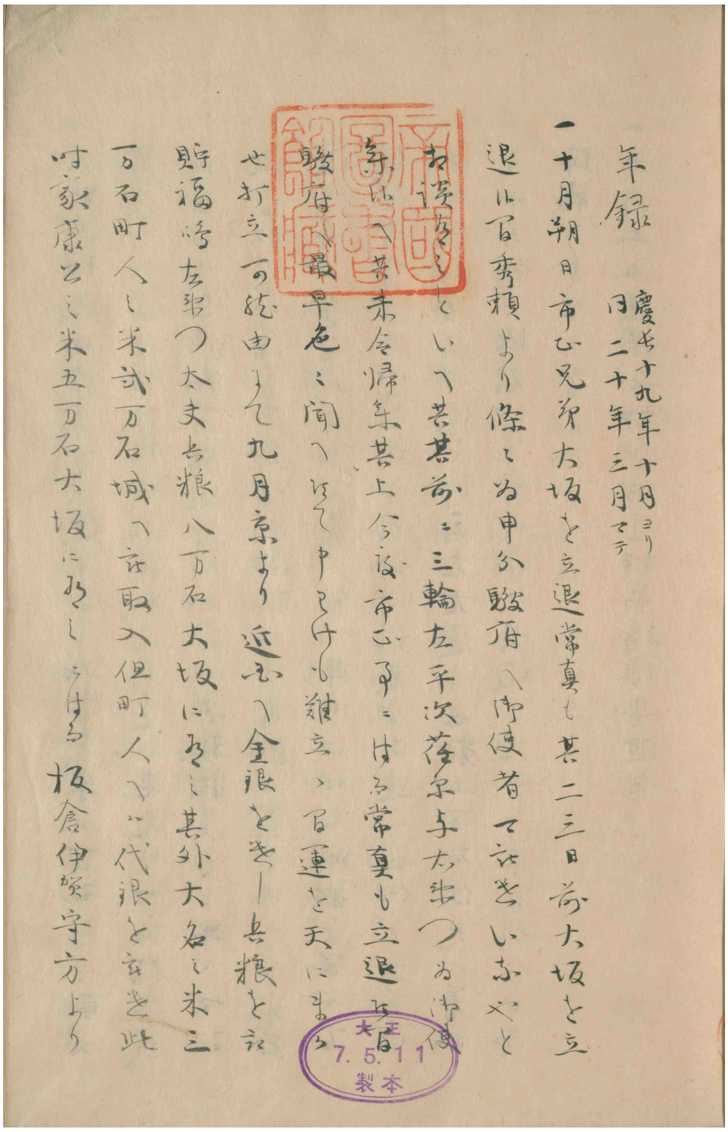
[16] To compensate for the missing data for the investigations by the Tokyo government, I use the data for land investigations by the local governments.

[17] In certain instances, more than one name is listed for a village and, thus, I reference the first instance to determine the main owner.

[18] The panel data also describe other infrastructural development but do not record the start year and investor. Given this and the dominant role of agriculture during the Edo period, I focus on agricultural investments.

[19] In the book, there are implicit groups with different styles of explanations: "normal" and "supplemental" works. Normal works, typically, have more information including financial sources, whereas supplemental works are only composed of location and timing information and are listed at the end of each section (sections are allocated for each prefecture). In this study, I reference both normal and supplemental works.

Figure 2.2: Page from *Ryueihinamiki*



Source: National Archive Digital Collection URL: http://dl.ndl.go.jp/info:ndljp/pid/2607434/3

Table 2.1: Descriptive Statistics: Political Variables

| | mean | sd |
|---|---|---|
| Transfer | .0191527 | .1370649 |
| Forfeit by Any Reason | .0052143 | .0720237 |
| Forfeit by No-Successor | .0019554 | .0441775 |
| Insider | .441464 | .4965742 |
| Observations (Domain * Year) | 19945 | |

Data are sourced from Nagahara, ed (1999). The data period is from 1615 to 1700, and the data are at the domain and year level. There are approximately 230 domains on average. Max and Min are valued at one and zero for all four variables.

### 2.3.4 Descriptive Statistics

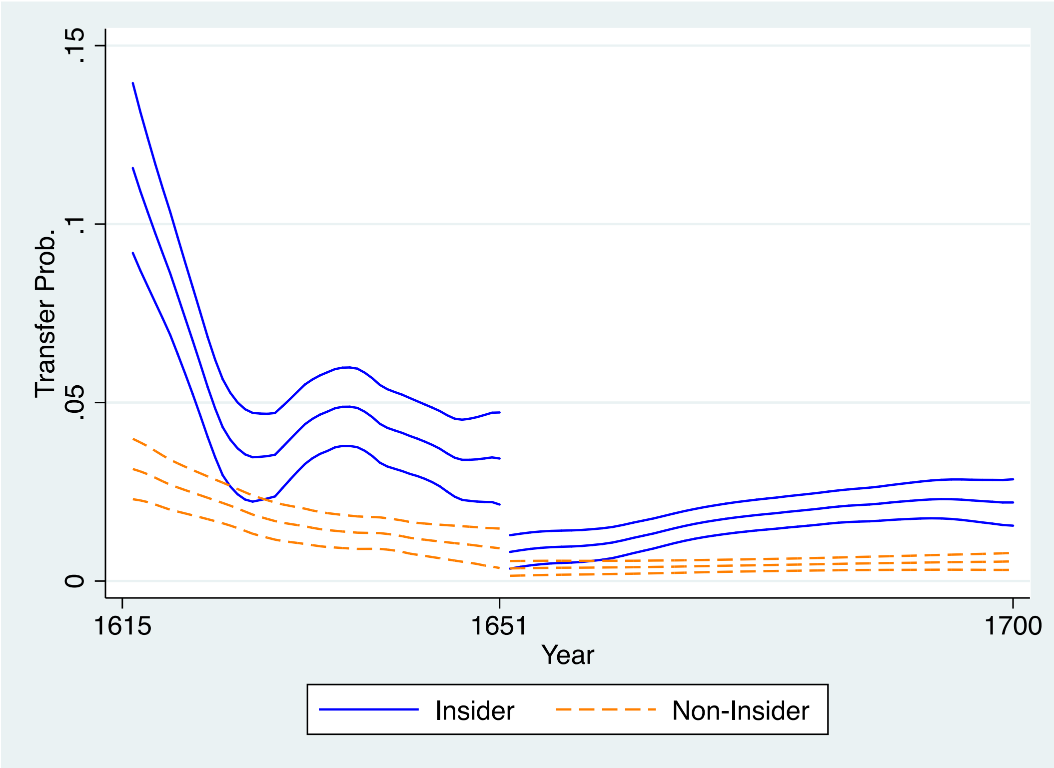
In this subsection, I present descriptive statistics and conduct a pre-analysis. First, I discuss the frequency of transfers and forfeits and the share of political identity at the domain level in Table 2.1. Each variable is a dummy variable that takes the value of one in the event of a transfer, forfeit, forfeit by no successor, or an insider-governed domain. On average, local lords were subject to a two per cent risk of transfer per year, which is considerably higher than that of a forfeit (0.5 per cent) or forfeit due to no successor (0.2 per cent). Over a 30-year period, this translates to a 46 per cent risk. The table also reports that less than half of the domains belonged to insiders (44 per cent).

To explain the shock of the death in 1651, I present a graph for transfer at the year and domain level. Using the same data as in Table 2.1, Figure 2.3 depicts the average frequency of transfers per year and political identity obtained using a non-parametric estimation. The figure shows that insiders were at higher risk before 1651 and, in 1651, there was a declining trend, and this drop was greater for insiders[20]. This pattern is confirmed by a regression analysis in Table 2.2. In column (1), I run a simple ordinary least squares (OLS), where I use transfer risk on the left-hand side and an insider dummy, post-1651 dummy, and their interaction terms on the right-hand side. I add year and domain fixed effects in columns (2) and (3), and the results suggest that the insider domain was subject to a higher transfer risk before 1651 although this risk dropped in 1651, particularly for insiders.

I present a similar graph and regression analysis for forfeit in Figure 2.4 and Table 2.3. I find no clear systematic pattern, and the results are not robust in the regression analysis. Table 2.4 shows that the regression results and interaction term are significantly negative in the regression with year and domain fixed effects. It is difficult to explain the

[20]Similarly, I show the raw data in Figure 2.A.1.

Figure 2.3: Transfer Risk



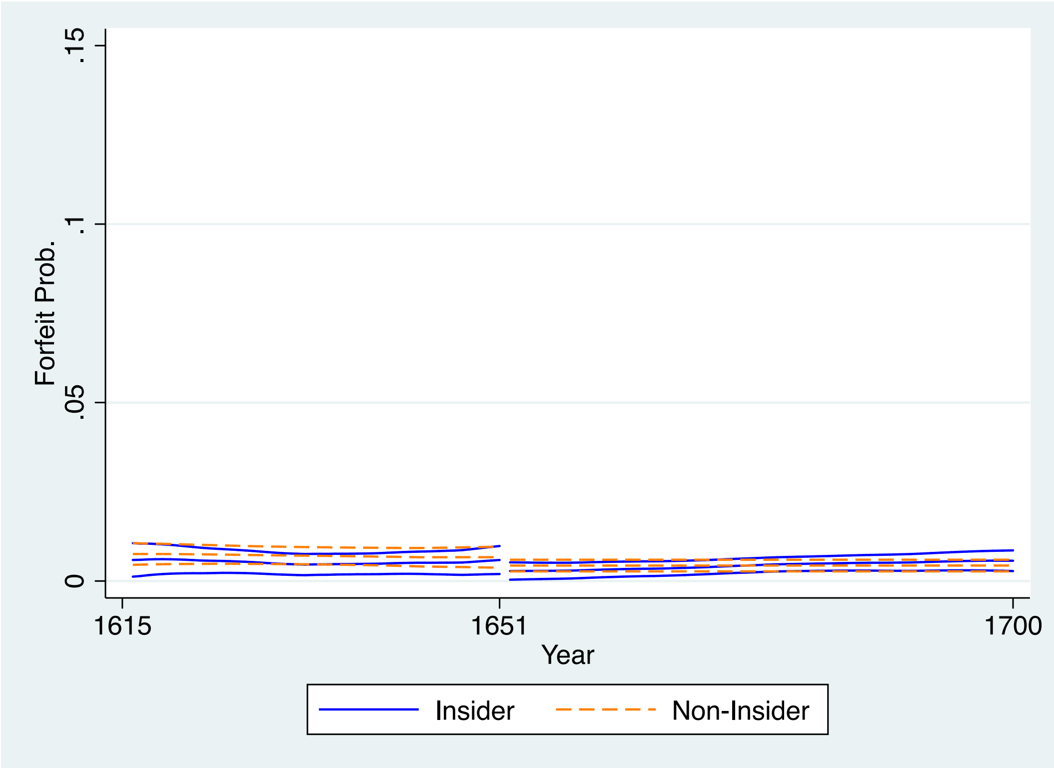

The band shows 95% CI.

Table 2.2: Regression Analysis: Decline in Transfer

| | Transfer | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Insider | 0.0347*** | 0.0365*** | |
| | (6.61) | (7.29) | |
| After 1651 | -0.0134*** | | |
| | (-6.28) | | |
| Insider*After 1651 | -0.0218*** | -0.0239*** | -0.0209*** |
| | (-4.04) | (-4.59) | (-4.43) |
| Year FE | No | Yes | Yes |
| Domain FE | No | No | Yes |
| Observations (Domain * Year) | 19945 | 19945 | 19945 |

Standard errors in parentheses are domain-level cluster robust. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$
Data are sourced from Nagahara, ed (1999). The data period is from 1615 to 1700. Data are at the domain and year level.

Figure 2.4: Forfeit Risk




The band shows 95% CI.

Table 2.3: Regression Analysis: Forfeit

| | Forfeit | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Insider | -0.00204 | -0.00192 | |
| | (-1.13) | (-1.06) | |
| After 1651 | -0.00299* | | |
| | (-2.08) | | |
| Insider * After 1651 | 0.00200 | 0.00181 | -0.00714** |
| | (0.98) | (0.88) | (-3.13) |
| Year FE | No | Yes | Yes |
| Domain FE | No | No | Yes |
| Observations (Domain * Year) | 19945 | 19945 | 19945 |

Standard errors in parenthesis are domain-level cluster robust. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$
Data are sourced from Nagahara, ed (1999). The data period is from 1615 to 1700. Data are at the domain and year level.

Table 2.4: Descriptive Statistics: Public Works

| | mean | sd |
|---|---|---|
| New Project | .0263789 | .1813391 |
| New Project by Local Governments | .0101918 | .1081158 |
| Observations (County * Era) | 5004 | |

Data are sourced from Doboku-Gakkai (1994). The data period is from 1615 to 1700. The data are at the county and era level. There were 417 counties and 12 eras in the data set.

cause of this result because the forfeit policy was not particularly severe for insiders, and the policy change allowing an adopted son was uniformly applied. However, even if the death reduced the forfeit risk for insiders as column (3) suggests, the main interpretation of the reduced-form analysis remains robust because a forfeit is analogous to a transfer, and both changes in transfer and forfeit risk would exert an affect in the same direction.

Next, I present the descriptive statistics for public works. Table 2.4 shows the number of new projects and new projects supported by local governments. I find that the number of new projects is low. At the county level, only 0.026 projects were initiated per era, which lasted seven years on average. This is partly because of the geographically small size of counties. Table 2.4 also shows that roughly 40 per cent of the projects were financed by local governments, which implies that the remaining 60 per cent were not financed by local governments. Bearers were typically wealthy community leaders or exemplary farmers indicating the relatively strong power of the local community. I use projects that have not been financed by local governments as a placebo outcome assuming no externality from public works financed by local governments. Figure 2.5 shows a time-series trend of the average number of public works by local governments per year and that this number increased during the study period.

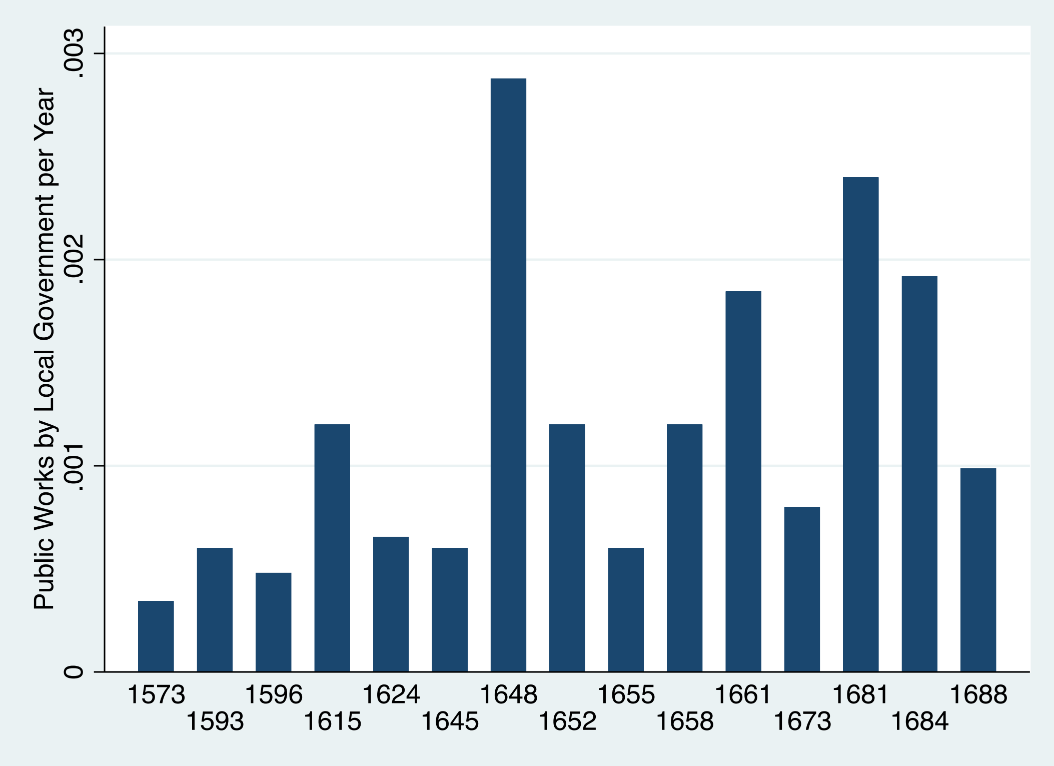
For data on land investigations, I show the average share of insiders' land. The measure of insider's land is denoted by

$$Insider_i = \frac{\sum_{v \in V(i)} Insider_v}{\sum_{v \in V(i)} 1}, \tag{2.1}$$

where $i$ is the county; $V(i)$ is the set of villages in county $i$; and $insider_v$ is the average of the dummy variable for 1615 to 1651, which takes the value of one if village $v$ is owned by an insider's government. I take an average of the time because, for example, certain domains had lords with different identities in the case of a new local lord following a forfeit.

Figure 2.6 depicts the distribution of $Insider_i$, and approximately 60 per cent of the

Figure 2.5: Time Series Trend of Public Works by Local Governments

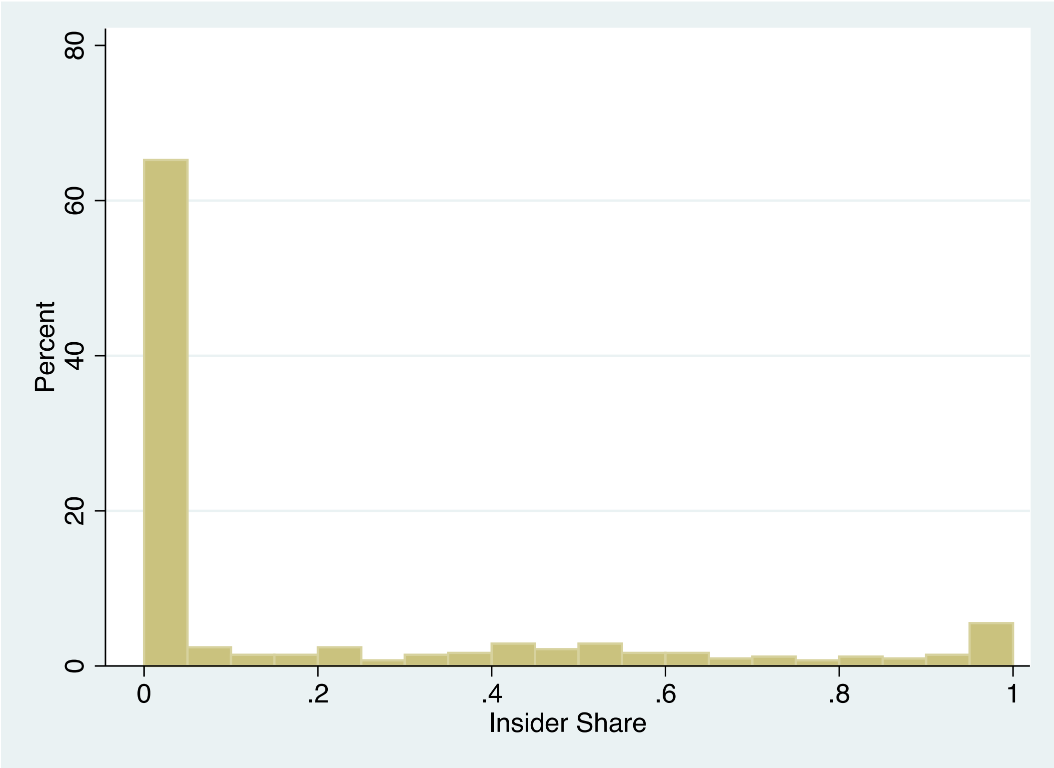



counties have no insider. The average of $Insider_i$ is 0.19, which is lower than the share in the number of domains. This is because insiders' domains are smaller and there are non-government owners. Non-government owners (neither insider nor non-insider) comprise the Tokyo government and religious groups owning temples and shrines. These owners were not affected by the change in transfer policy and are considered non-insiders when calculating insiders' share.

To demonstrate the geographical pattern, I construct a state-level average of an insider's share as shown in Figure 2.7. We see that insiders are mainly in the Eastern or Northern areas, which is expected because insiders were supporters of Tokugawa, who was based in Tokyo. Nishikai state held the smallest share of 6.8 per cent and insiders were spread across the country. Because different states may have varying economic time-variant conditions, I control for state-level fixed effects and their interactions with the post-1651 dummy to examine whether this spatial imbalance affects my main result.

Note that the data do not cover the entire nation. First, in the map, I focus on the three main islands of Japan by excluding territories (*Hokkaido* and *Okinawa*) formally recognized after the 19th century and isolated islands. In addition, nine provinces were excluded because of missing data or the inability to conduct a time-consistent unit of observation due to splits and mergers. The data set was composed of 62 provinces. The full list of provinces is presented in the Appendix, Table 2.A.1.

Figure 2.6: Distribution of Insiders' Share at the County Level




## 2.4 Results

### 2.4.1 Main Results

First, I estimate the reduced-form effect of the regime change on the number of newly initiated projects. Accordingly, I estimated the following equation:

$$\text{outcome}_{ijt} = \alpha \text{Insider}_{ij} * \textit{After 1651}_t + \text{Era FE} + \text{County FE} + \epsilon_{ijt}, \quad (2.2)$$

where $i$ is a county, $j$ is a province, and $t$ is an era. Since this is a simple DID, I assume parallel trends to interpret it as causal. However, there are possibly other factors that correlate with political identity and have time-variant effects on the outcome. I conduct robustness checks by controlling for a county-wise linear trend and the interaction terms of the post-1651 dummy with state-level fixed effects.

Table 2.5 presents the results. Column (1) shows that the number of new projects financed by local governments was affected by the regime change. The coefficient is 0.025 indicating that if you compare a county fully governed by insiders with one fully governed by non-insiders, the number of projects will increase by 0.022 per era. This is an economically significant effect because, on average, there are only 0.01 projects per era during the sample period. The result is robust even when including a county-level

Figure 2.7: State-level Average Insiders' Share

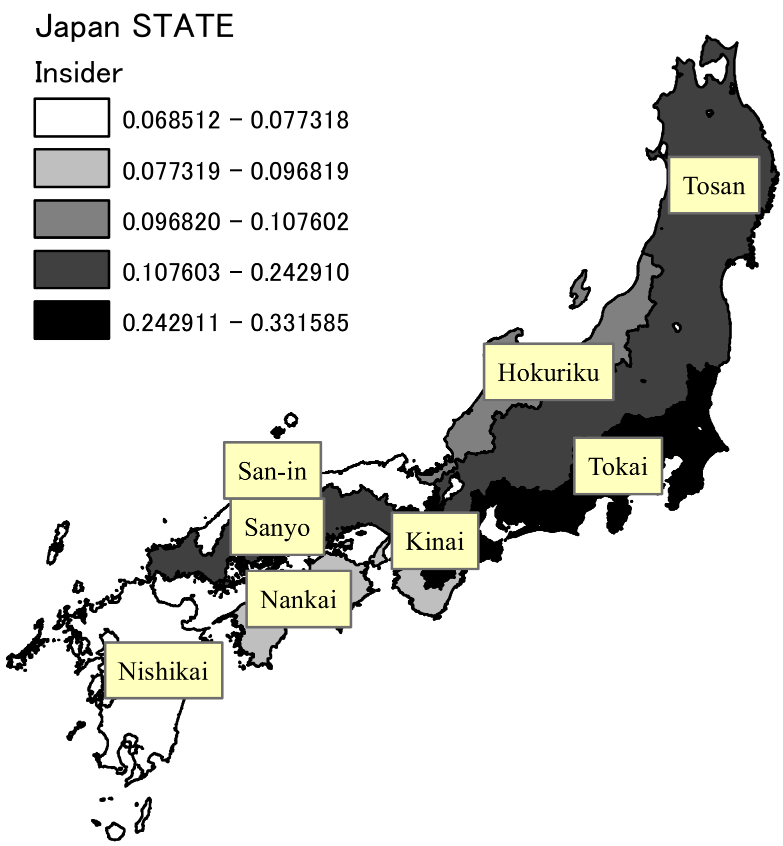


Table 2.5: Main Result: DID Analysis on Public Works Supported by Local Governments

| | Public Works by Local Governments | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Insider * After 1651 | 0.0217** | 0.0174* | 0.0172+ |
| | (0.00735) | (0.00862) | (0.00894) |
| Era FE and County FE | Yes | Yes | Yes |
| County-level Trend | No | Yes | Yes |
| State FE * After 1651 | No | No | Yes |
| Observations (County * Era) | 5004 | 5004 | 5004 |

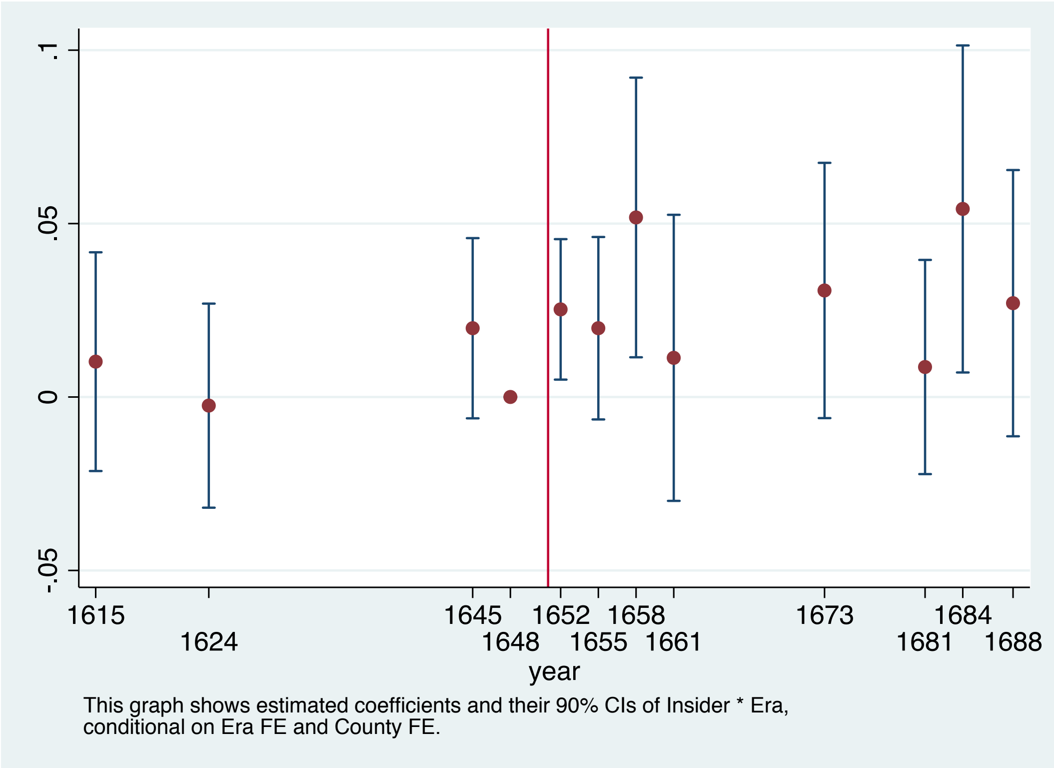
Standard errors in parentheses are 61-province-level cluster robust.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$
I use a fixed-effects estimator. The data are at the county and era level. As for the data points, the start year for each era is 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, and 1688.

linear trend and state-level fixed effects that interact with the post-1651 dummy variable, as shown in columns (2) and (3), although these specifications have lower p-values. I also present the graphical DID analysis conducted using the era-wise coefficient of $\text{Insider}_{ij}$ in Figure 2.8[21]. The pattern shows that before 1651, there are no clear systematic differences and, post-1651, the coefficient becomes significantly positive in certain eras. The effect fluctuates across eras because, in our data, public goods are durable and, thus, once a county has received an investment, no further investments are made in the subsequent era. In the analysis, I choose 1615 to 1700 as the time window so that there are a sufficient number of observations to include a county-level trend. However, as Figure 2.8 shows, the effect does not originate from the edges of the time window.

Political identity is not randomly assigned and may affect the main result. To assess this, I added three key controls to the main specification, ruggedness and local government size. I employ ruggedness because in the 17th century, there was technological progress in civil engineering that enabled the development of paddy fields in flat areas (Kikuchi, 1958), and this may affect the outcome. I also control the rice suitability index, which would be more direct to local investment opportunity. However, we have no prior on the relationship between political identity and the geographical variables. I add local government size because insiders were originally Tokugawa's vassals and tended to have lower tax revenue than outsiders, although it remains unclear why the impact of local

[21] I also show the level of each era fixed effects and the sum of era-level fixed effects and era-wise coefficient of $\text{Insider}_{ij}$ in Figure 2.A.2.

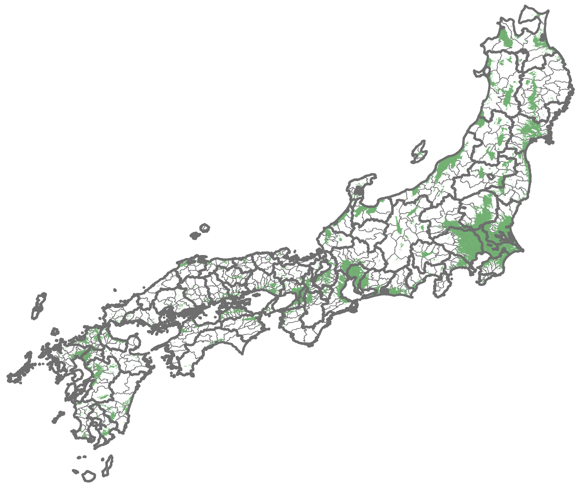
Figure 2.8: Graphical Representation of the Main Results using DID Analysis




government size on public goods investment varied by regime.

Given the above concerns, I added three controls, $FlatAreaProportion_{ij} * After1651_t$, $RiceSuitability_{ij} * After1651_t$ and $logGovernmentSize_{ij} * After1651_t$. $FlatAreaProportion_{ij}$ is the county-level proportion of a flat area defined by area that is less than a two-degree gradient. The measure is constructed using 1 km × 1 km mesh-level information of slope available at the National Land Numerical Information Download Service. To determine whether an area is flat or steep, I denote two degrees as the threshold. Figure 2.9 shows the area with a slope of less than two degrees and, roughly speaking, this corresponds to plains and basins. $RiceSuitability_{ij}$ is the average rice suitability using gravity irrigation and intermediate inputs provided by FAO-GAEZ database. To calculate $logGovernmentSize_{ij}$, I use the average size of local governments in 1651, as shown in Nagahara, ed (1999), and construct a county-wise variable using the government's share in the county. In Table 2.A.2, I show the correlations between these variables and the share of insiders at the cross sectional level. In column (1) to (3), I find that the government size is negatively correlated with insider share, but the flat land share and rice suitability index do not show the significant correlation. In column (4), flat land proportion becomes significantly positive suggesting that insiders were in relatively flatter area, which might be the frontier for agricultural investments at that period as explained above. This might affect the main result and therefore we have to control this variable for the robustness checks.

Figure 2.9: Distribution of Flat Area



Source: National Land Numerical Information Download Service. The green area denotes that the average slope is less than two degrees.

The results are presented in Table 2.6, and the main results are in column (1) in Table 2.5. In columns (2) to (4), I control for the above-mentioned variables and find that the coefficient increased. In addition, the log of government size has a significantly positive effect and because the insider's domain was smaller than that of the outsider's, the coefficient *increased* by adding the control. Overall, the non-random assignment does not have an upward bias in the main result and, thus, the positive reduced-form effect cannot stem from this bias.[22]

### 2.4.2 Robustness Checks: Placebo and Other Changes Between Regimes

Although the main result was robust to additional controls, in this subsection, I conduct a placebo test and address other potential channels that could explain the reasons underpinning the impact of the regime change.

As a placebo test, I use the number of public works initiated by those outside the local government such as rich farmers. I assume that projects supported by local governments do not affect those not supported by them. For example, if an insider's region experienced

[22] I also quantify the effect of a transfer risk by conducting a 2SLS with county-level average transfer probability in each regime, 1615 to 1651 and 1652 to 1700, as treatment. I employ the sets of controls in Table 2.6. In column (1) of Table 2.A.3, I find that an additional one percentage point of transfer risk reduces public works per era to 0.036 on average, and the point estimates are larger when additional controls are present in Table 2.6.

Table 2.6: Robustness Check: DID with Geographical or Political Controls

| | Public Works by Local Governments | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Insider * After 1651 | 0.0217** | 0.0237** | 0.0261** | 0.0301** |
| | (0.00735) | (0.00729) | (0.00911) | (0.00927) |
| Flat Land Share * After 1651 | | -0.0217+ | | -0.0253+ |
| | | (0.0130) | | (0.0137) |
| Rice Suitability * After 1651 | | 0.0419 | | 0.0274 |
| | | (0.0537) | | (0.0482) |
| Log Government Size * After 1651 | | | 0.00304 | 0.00413* |
| | | | (0.00192) | (0.00204) |
| Era FE and County FE | Yes | Yes | Yes | Yes |
| Observations (County * Era) | 5004 | 4992 | 5004 | 4992 |

Standard errors in parentheses are 61-province-level cluster robust.

$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$

I use a fixed-effects estimator. The data are at the county and era level. As for data points, the start year for each era is 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, and 1688.

*Flat Land Share* is defined as (demeaned) *Area with less than two degrees/Area* as per the 1 km × 1 km mesh-level data from the National Land Numerical Information Download Service (`http://nlftp.mlit.go.jp/ksj-e/index.html`). Since data are missing for one county (isolated island), the number of observations decreased.

*Rice Suitability* is the average rice suitability index FAO-GAEZ ver 3.0 provides. I choose the following parameters: Crop:Wetland rice, Water Supply:Gravity irrigation, Input Level:Intermediate, Time Period:1961-1990.

*Log Government Size* is defined by the log of the weighted average of log Government Size in 1651 (in ten thousands of *kokudaka*, which is a proxy of land productivity), as adopted from Nagahara, ed (1999). Because the size of the Tokyo government is not listed, I assign the popular estimate of 800 for villages under the Tokyo government. I also assign the value of zero for villages under religious groups.

Table 2.7: Placebo Test: DID Analysis of Public Works not Supported by Local Governments

| | Public Works Not by Local Governments | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Insider * After 1651 | 0.00268 | -0.0172 | -0.0155 |
| | (0.0252) | (0.0380) | (0.0405) |
| Era FE and County FE | Yes | Yes | Yes |
| County-level Trend | No | Yes | Yes |
| State FE * After 1651 | No | No | Yes |
| Observations (County * Era) | 5004 | 5004 | 5004 |

Standard errors in parentheses are 61-province-level cluster robust.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$
I use a fixed-effects estimator. The data are at the county and era level. As for data points, the start year for each era is 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, and 1688.

an economic boom post-1651 for reasons other than a transfer and this influenced the main result, we will see a positive effect on the number of public works initiated by those outside the local government. Table 2.7 shows the result using the same specification as Table 2.5 and that there is no such pattern. Therefore, it seems unlikely that the main result can be attributed to an economic boom in the insider's region post-1651. [23]

Thus far, we have seen the effect of a regime change on the number of initiated projects and interpreted it as the effect of tenure risk. However, to interpret identity effects as tenure effect, we need an exclusion restriction: the assumption that the death of an executive leader does not cause another policy change, particularly in a disproportional manner for different identities.

The other four policies, which the Tokyo government could impose to intervene in local governments, other than transfer policy do not explain the reduced-form effect. First, in 1615, the Tokyo government set up a general law (*Buke Shohatto*) for local lords specifying ritual rules and military rules that prohibited, for example, the construction of new castles. This law was uniformly applied for insiders and non-insiders and, as per the current sample period, was revised in 1635, 1663, and 1683. However, the 1663 and 1683

[23] Since initiating public works is a rare event, as shown in Table 2.4, I also conduct Poisson and negative binomial regressions as robustness checks. Table 2.A.4 and 2.A.5 present the results. As in the linear regression results, I find that an insider's regime change increases the number of public works by local governments but not that of public works by non-local governments.

revisions do not affect the incentive to make a public goods investment, and the revision was uniformly applied regardless of political identity. Thus, they cannot explain the main result.[24] Second, the alternate attendance system (*sankin kotai*) does not explain the reduced-form effect. In the system, local lords had to stay in Edo for several months and leave their wives and children behind as hostages upon return to their local regions. This policy has been customary since the beginning of the Edo period and was formalized in 1635 when the general law was revised. It may discourage investment because of transportation cost, but there was no policy change to this effect in 1651, and the 1635 formalization was particularly severe for insiders[25] indicating that the main result was not driven by the policy.

Third, the Tokyo government could order the local government to incur the cost of public works, but the number of orders did not change between pre and post-1651. This was executed as an in-kind transfer whereby local governments would provide labor to construct or repair castles or temples. In a limited number of cases, the Tokyo government financially supported the local governments in building castles. There was no explicit written rule for these in-kind transfers; thus, I check the actual data of these in-kind transfers to identify a related policy change in 1651. Yoshizumi (1968) listed orders by the Tokyo government for the local governments along with information such as start year and the name of lords for each project. These orders were not financially supported by the Tokyo government. Using this list, I constructed a year and domain-level panel data set composed of the number of new orders for 1615 to 1700. Figure 2.A.3 depicts the identity-wise average number of new orders per year. We see no systematic change in 1651, and I confirm this conclusion (Table 2.A.6) using a simple DID specification and one with a domain-level linear trend. Therefore, this channel is unlikely to explain the reduced-form effect.

Fourth, the Tokyo government supported local governments in building or repairing military infrastructure, but the data suggests that this does not affect the main result. Kato (1969) listed the castles that were built or re-built by local governments with support from the Tokyo government. According to the list, from 1615 to 1700, there were only five cases of assistance from the Tokyo government. Prior to the regime change, there were four cases, of which three were for insiders. Following the regime change, there was one assistance case, and this was for an insider. The regime change did not affect the allocation of support between identities, and the amount of assistance was small. Thus,

[24] In 1635, the Tokyo government prohibited Christianity and, in 1663, explicitly prohibited suicide following their master's death. In 1683, the Tokyo government allowed local lords to take a son-in-law just before their death, although this was already permitted de facto post-1651.

[25] Prior to the formalization, there was no such expectation from insiders; however, post-1635, it became mandatory.

the regime change should not affect the interpretation of the main result.

The death of the executive leader may have affected another two political situations for local lords, but the data do not support either of them. First, because only insiders could obtain a position in the Tokyo government such as a cabinet member [26], the death of the executive leader may have caused a political race among insiders.[27] This has a positive or negative effect on the level of public goods investment. If public goods investments signify talent, local lords would likely invest greater efforts to attain a higher position. However, a focus on the political race and not local governance may have decreased local lords' chances of a higher position.

We can check for the plausibility of this explanation by referencing another death of the executive leader during the same approximate period. In 1681, the fourth executive leader died and *Tokugawa Tsunayoshi* became the fifth executive leader at the age of 34. Unlike the 1651 case, there was no change in transfer risk in 1681.[28] In Table 2.5, I perform a similar DID analysis and add the interaction term, $Insider_{ij} * After\ 1681_t$, which captures the death effect excluding the transfer risk effect. Table 2.A.7 shows the results: all specifications are insignificant, and point estimates are negative.

The other way to check this possibility is to directly control the interaction term of "after 1651" and the number of cabinet members (*Roju*) produced by the county between 1651 and 1700. Because transfers occurred at the same time with or before appointments to the cabinet to call insiders to Tokyo area, I use the domain that the lord owned before the transfer as the domain produced cabinet members. Adding this control to the Table 2.5, I show the results in Table 2.A.8. The cabinet member effect shows mostly insignificant positive signs, but the main effect remains significant in the all columns. Therefore, both results do not support the view that the main result is explained by the political race for promotion.

Second, local governments' attitudes toward military expenditure were possibly affected by the regime change, and this may have driven our main result. For example, prior to 1651, the military expenditures to prepare for the possible onset of a war were greater for insiders than for outsiders. Military tensions may have eased as a result of a regime change, and insiders might have started investing their revenue in public goods. This concern should be seriously considered given that prior to 1600, certain insiders were militarist groups under Toyotomi's hegemony. I examine this possibility using a

[26] There are several positions for insiders other than cabinet members (*Roju*): *Kyoto Shoshidai*, *Jisha Bugyo*, *Wakadoshiyori*, *Sojaban*, *Osaka Jodai*, and *Osaka Joban*. Note that even if an insider is appointed to one of these positions, the insider continues to be a local lord. *Roju* was the highest position.

[27] Since appointment to these positions does not entail a payment, positive motivation included indirect revenue through strong or intrinsic political power.

[28] This is clearly shown in Figure 2.3. I also confirm this by conducting a regression, which for simplicity, is not mentioned here.

list of castle construction and repair work, as summarized in Kato (1969). Castles were a major military investment at the time, although such investment was prohibited by the general rule de jour. Thus, I can analyze how military investments differed between political identities and regimes. There were a total of 13 castle construction or repair works between 1615 and 1700.[29]

I conducted a regression analysis using the following specification:

$$Castle_{it} = \alpha_0 Insider_{it} + \alpha_1 Insider_{it} * After\ 1651_t + Year\ FE_t, \tag{2.3}$$

where $i$ is the domain, $t$ the year, and $Castle_{it}$ is the number of construction or repair works initiated in domain $i$ at time $t$. I add domain-level fixed effects and insider-specific trends as robustness checks. Table 2.A.9 shows the results and no systematic pattern between identities and regimes. The point estimate of the interaction term is positive implying that with a longer time horizon, other types of local public investments, such as castles, also increase. In addition, $\alpha_0$ is insignificantly negative, which is inconsistent with the above-mentioned narration because certain insiders were originally militarist groups and, thus, prioritized military expenditure, particularly prior to the regime change. This cannot be explained with a reduced-form effect.

Taken together, these two results suggest that the reduced-form effect of regime changes (Table 2.5) is not driven by changes in policy such as intra-governmental transfers or the local government's attitude toward the military or military expenditure.

### 2.4.3 Robustness Checks: Data Quality

Although we interpreted the reduced-form effect as the effect of a transfer risk, there are certain complications emanating from the fact that the public works data were compiled after 1868 using documents on pre-1868 projects. First, this suggests that small-scale projects are unaccounted for and, thus, the result should be interpreted as the effect of the number of projects, which is high enough to be recorded. Second, it is possible that transfers affected the probability of public works being recorded because local government documents may have been lost during transfers. If so, the number of projects is not subject to the reduced-form effect but to the probability of projects being recorded, which is problematic.

Since it is impossible to separate this hypothesis from the key narrative of this study using the same data period, I use data on transfers for 1701 to 1868, which is outside the sample period used in the main regression conducted to check whether the total number of transfers is not correlated with the number of initiated projects from 1615 to 1700. If

[29] I excluded few because the start year was prior to the establishment of the corresponding domain.

this concern is legitimate, the total number of transfers post-1700 should be negatively correlated with the number of initiated projects from 1615 to 1700 because the domains will lose the documents for projects from 1615 to 1700 even in the case of transfers after 1700.

Table 2.A.10 presents the result. I include $\mathit{Insider}_i$ because it should correlate with the number of transfers post-1700. In addition, I include ($\mathit{Insider}_i * \mathit{After\ 1651}_t$) and state or province-level fixed effects in certain specifications to eradicate a bias or increase precision. I find no significant coefficient for $\mathit{Transfer\ 1700\text{-}1868}_i$ in any specification, and the point estimates of coefficients for $\mathit{Transfer\ 1700\text{-}1868}_i$ are small (the mean of $\mathit{Transfer\ 1700\text{-}1868}_i$ is .5606723). Therefore, I conclude that this concern does not drive the present study's main result.

In addition, we cannot track the number of insiders over the years because the land investigation data are limited to a single time point. In general, domain boundaries are stable, although detached areas are an exception. Detached areas were commonly used as adjustments when a transfer occurred between local lords and have varying productivity size. Thus, the owners of counties with a detached area were more likely to be time-variant. Since transfers generally occurred within the domain's identity, the county's identity was unlikely to change. As a result, I do not believe that time-variant owners affect the main result. Furthermore, the measurement error has an attenuation bias if it is classical and, thus, the estimated positive coefficient is a lower bound. Nevertheless, to be rigorous, I control for the share of detached area, defining the variable as $\mathit{Main\ Area}_v$, which takes the value of one if the village is owned by a domain whose main castle was in the same province and zero if not or the owner is not from the same province. Similarly, I define the county-level counterpart $\mathit{Main\ Area}_i = \frac{\sum_{v \in V(i)} \mathit{Main\ Area}_v}{\sum_{v \in V(i)} 1}$ and, then, control for its interaction term and $\mathit{After\ 1651}_t$.

The results are presented in Table 2.A.11. Columns (1) and (2) correspond to columns (1) and (2) in Table 2.5. Here, we still find that the reduced-form and estimated effects are greater in Table 2.5. Note that $\mathit{Main\ Area}_i * \mathit{After\ 1651}_t$ has negative signs in columns (1) and (2) suggesting that a higher number of insiders own detached areas, which is natural because detached areas were used for transfers. In columns (3) and (4), I conducted a similar analysis as in columns (1) and (2) of Table 2.A.7 to analyze the impact of the executive leader's death in 1681, which is more likely to be affected by time-variant owners; however, the results do not substantially change. I conclude that time-variant owners' problems do not affect the main result.

### 2.4.4 Time Horizon and Other Explanations

The study reveals the effect of a transfer risk on the number of projects supported by local governments; however, certain concerns remain concerning the interpretation. In other words, there may be other explanations, for example, a shorter tenure might hinder local governments for the suitability of projects in local domains, which, in turn, could decrease the number of projects. Thus, I add a variable that captures the length of tenure up to 1651; for example, if the government entered the domain in 1640, it takes the value of 11 and, if it has been the same since 1615, it takes the value of 36. Using this information, I construct the weighted average of experience $Experience_i$ using the share of local government. Assuming local knowledge is important, domains with greater experience should be able to further increase the number of projects after attaining greater stability as a result of the policy shock. In the econometric model, I add $Insider_i * After\ 1651_t * Experience_i$, which should have a positive coefficient to be consistent with the channel. I demean $Experience_i$ to make the results comparable with the other results. Table 2.A.12 shows the results with similar specifications as those in Table 2.5. In certain specifications, I include $After\ 1651_t * Experience_i$ as robustness checks. The results do not show a significant sign for interaction terms and, therefore, this evidence is inconsistent with the experience viewpoint.

This also rules out the possibility that the cost of transfer works has a temporal negative income shock, and this could be the main channel to decrease public goods investment. Here, if cost plays a key role, counties with a shorter experience would have a budget problem and not increase public goods investment following the regime change, as the experience narrative suggests. Overall, the other alternative channels do not explain the main result. Therefore, I conclude that the main mechanism of the transfer effect is the reduced probability of relocating, not the loss of experience or cost of transfer.

## 2.5 Conclusions

In this study, I use a newly digitized data set and unique context to examine whether a longer time horizon for a government promotes local public provisions. The DID estimates reveal a substantial positive effect. In addition, I conducted robustness checks to confirm the reduced-form effect and a subsequent analysis to investigate if the reduced-form effect can be interpreted as a longer time horizon effect, which might be a part of a tenure effect or other political channel. I also address measurement issues regarding data set quality. All results support the view that the reduced-form effect emerges from a longer time horizon effect.

This study is not free from limitations. For example, it is impossible to obtain a domain's financial data in the present context because, at the time, small domains did not have accounting capabilities. This is certainly not the case for present-day studies on Eastern Congo that use various measures, such as taxation and violence, for stationary bandits. Further, land ownership data do not track changes in boundaries although this problem does not affect the results of this study. These data availability issues are common in such historical contexts and despite this, the study demonstrates the importance of a longer time horizon.

# Appendix

## 2.A Appendix

Table 2.A.1: Data Coverage

| | |
|---|---|
| Isolated Islands | *Oki*, *Sado*, *Iki*, *Tsushima*, and *Ryukyu* |
| Excluded Provinces for Various Reasons | *Wakasa*, *Yamashiro*, *Satsuma*, *Kamihusa*, *Simohusa*, *Hitachi*, *Shinano*, and *Simotsuke* |
| Covered Provinces | *Awa*, *Aki*, *Awa*, *Iga*, *Ise*, *Izu*, *Iyo*, *Imba*, *Ugo*, *Uzen*, *Echigo*, *Echizen*, *Echu*, *Totomi*, *Simotsuke*, *Kaga*, *Kawachi*, *Iwami*, *Iwashiro*, *Kii*, *Oumi*, *Kai*, *Mikawa*, *Sanuki*, *Shima*, *Suo*, *Izumo*, *Suruga*, *Kozuke*, *Setsu*, *Sagami*,*Osumi*, *Yamato*, *Tajima*, *Tango*, *Tanba*, *Awaji*, *Tikugo*, *Tikuzen*, *Nagato*, *Tosa*, *Hyuga*, *Noto*, *Harima*, *Hoki*, *Iwaki*, *Higo*, *Hizen*, *Hida*, *Bingo*, *Bizen*, *Bichu*, *Owari*, *Mimasaka*, *Mino*, *Musashi*, *Bungo*, *Buzen*, *Mutsu*, *Rikuzen*, *Rikuchu*, and *Izumi* |

Figure 2.A.1: Transfer Risk for Each Identity in Raw Data

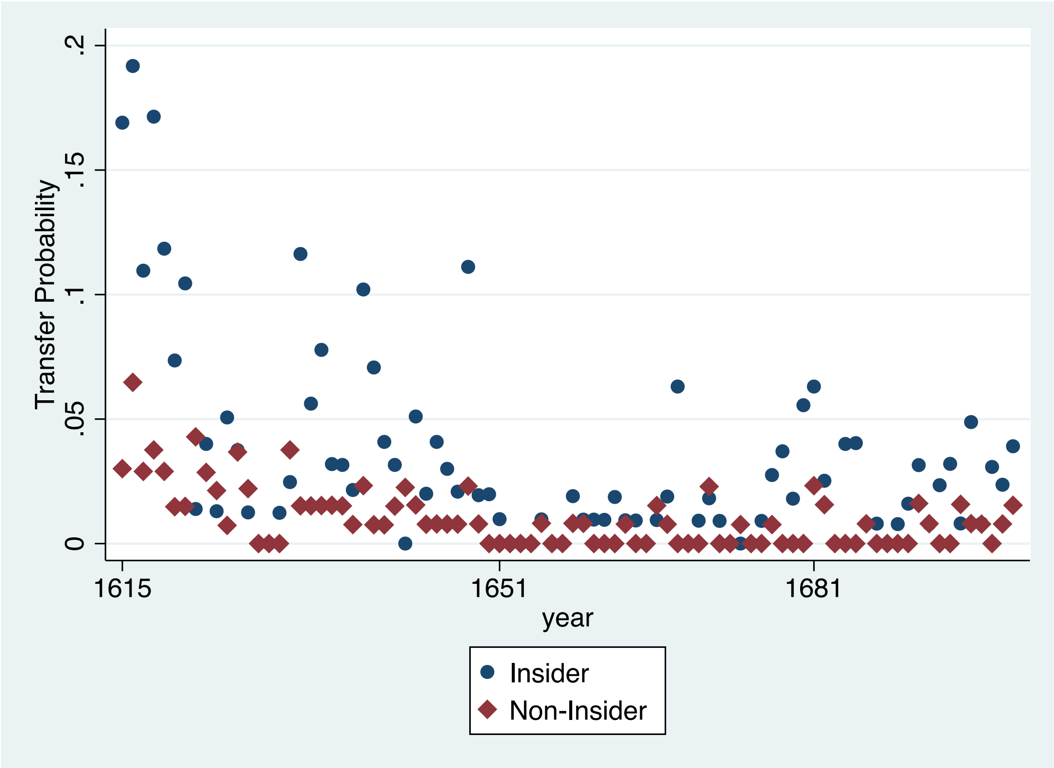



Figure 2.A.2: Graphical Representation of the Main Results using DID Analysis

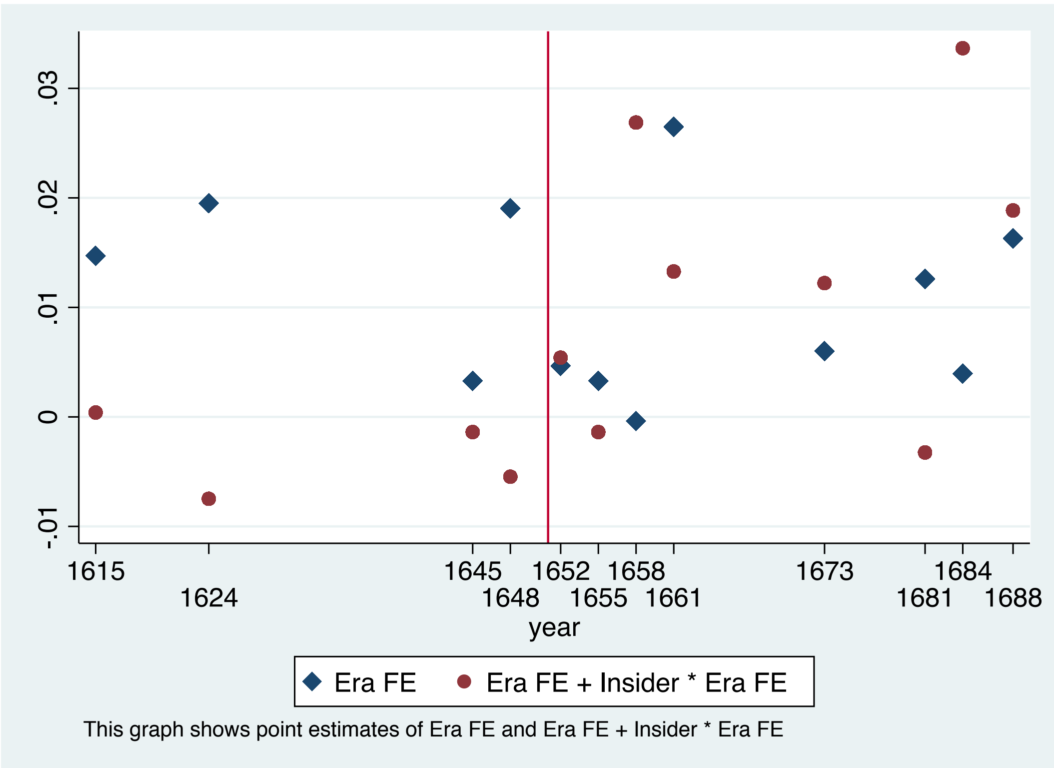


Table 2.A.2: Robustness Check: Correlation with Geographical or Political Controls

| | Insiders | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Log Government Size | -0.0698*** | | | -0.0789*** |
| | (0.0156) | | | (0.0156) |
| Flat Area Prop | | 0.149 | | 0.242* |
| | | (0.0967) | | (0.0994) |
| Rice Suitability | | | 0.424 | -0.0514 |
| | | | (0.405) | (0.385) |
| Constant | Yes | Yes | Yes | Yes |
| Observations | 417 | 416 | 416 | 416 |

Standard errors in parentheses are 61-province-level cluster robust.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$
I use a OLS estimator. The data are at the county level.
*Flat Land Share* is defined as (demeaned) *Area with less than two degrees/Area* as per the 1 km × 1 km mesh-level data from the National Land Numerical Information Download Service (`http://nlftp.mlit.go.jp/ksj-e/index.html`). Since data are missing for one county (isolated island), the number of observations decreased.
*Rice Suitability* is the average rice suitability index FAO-GAEZ ver 3.0 provides. I chose the following parameters. Crop: Wetland rice, Water Supply: Gravity irrigation, Input Level: Intermediate, Time Period: 1961-1990.
*Log Government Size* is defined by the log of the weighted average of log Government Size in 1651 (in ten thousands of *kokudaka*, which is a proxy of land productivity), as adopted from Nagahara, ed (1999). Because the size of the Tokyo government is not listed, I assign the popular estimate of 800 for villages under the Tokyo government. I also assign the value of zero for villages under religious groups.

Table 2.A.3: DID-IV Analysis to Quantify Transfer Risk Effect

| | Public Works by Local Governments | | | |
| --- | --- | --- | --- | --- |
| | (1) | (2) | (3) | (4) |
| Transfer Risk in Each Regime | -0.0362* | -0.0390* | -0.0438* | -0.0495* |
| | (0.0176) | (0.0183) | (0.0217) | (0.0234) |
| Flat Land Share * After 1651 | | -0.0172 | | -0.0197 |
| | | (0.0141) | | (0.0150) |
| Rice Suitability * After 1651 | | 0.0255 | | 0.00664 |
| | | (0.0609) | | (0.0626) |
| Log Government Size * After 1651 | | | 0.00316 | 0.00411 |
| | | | (0.00239) | (0.00257) |
| Era FE and County FE | Yes | Yes | Yes | Yes |
| Observations (County * Era) | 5004 | 4992 | 5004 | 4992 |

Standard errors in parentheses are 61-province-level cluster robust.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$
*Flat Land Share* is defined as (demeaned) *Area with less than two degrees/Area* as per 1 km × 1 km mesh-level data available at National Land Numerical Information Download Service (`http://nlftp.mlit.go.jp/ksj-e/index.html`). The number of observations is lower because data are missing for one county (isolated island).
*Rice Suitability* is the average rice suitability index FAO-GAEZ ver 3.0 provides. I choose the following parameters: Crop:Wetland rice, Water Supply:Gravity irrigation, Input Level:Intermediate, Time Period:1961-1990.
*Log Government Size* is defined by the weighted average of log Government Size in 1651 (in ten thousand *kokudaka*), as taken by Nagahara, ed (1999). Since the size of the Tokyo government is not listed, I assign the popular estimate of 800 for villages under the Tokyo government. I also assign the value of zero for villages under religious groups.
I use fixed-effects estimators. As for data points, the start year of each era is 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, and 1688.
I use Insider * Post-1651 as an instrument. *Transfer Risk in Each Regime* is the average transfer risk (per cent) at the domain level during 1615 to 1651 if the observation is prior to 1651 or 1651 to 1700 otherwise.

Table 2.A.4: Robustness Check: Main Result for Poisson Regression

| | Public Works | | | | | |
|---|---|---|---|---|---|---|
| | by Local Governments | | | Not by Local Governments | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Insider * After 1651 | $4.031^{**}$ | $4.038^{+}$ | $4.109^{*}$ | 0.0358 | -0.574 | -0.310 |
| | (2.62) | (1.94) | (2.05) | (0.06) | (-0.54) | (-0.26) |
| Era FE and County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Insider Share * Trend | No | Yes | Yes | No | Yes | Yes |
| State-level Trend | No | No | Yes | No | No | Yes |
| State FE * After 1651 | No | No | Yes | No | No | Yes |
| Observations (County * Era) | 480 | 480 | 480 | 984 | 984 | 984 |

Standard errors in parenthesis are county level cluster robust.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$
I conduct a Poisson regression. As for data points, the start year for each era is 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, and 1688.
Note that many observations are excluded owing to zero outcomes.

Table 2.A.5: Robustness Check: Main Result for Negative Binomial Regression

| | Public Works | | | | | |
|---|---|---|---|---|---|---|
| | by Local Governments | | | Not by Local Governments | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Insider * After 1651 | $3.590^{+}$ | $4.327^{+}$ | $4.912^{+}$ | -0.695 | -0.544 | -0.520 |
| | (1.74) | (1.89) | (1.81) | (-1.29) | (-0.97) | (-0.92) |
| Era FE and County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Insider * Trend | No | Yes | Yes | No | Yes | Yes |
| State-level Trend | No | No | Yes | No | No | Yes |
| Observations (County * Era) | 480 | 480 | 480 | 984 | 984 | 984 |

Standard errors are in parentheses.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$
I conduct a negative binomial regression. As for data points, the start year for each era is 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, and 1688.
Note that many observations are excluded because of zero outcomes.

Figure 2.A.3: Average Number of Orders from Tokyo Government to Local Governments by Identity

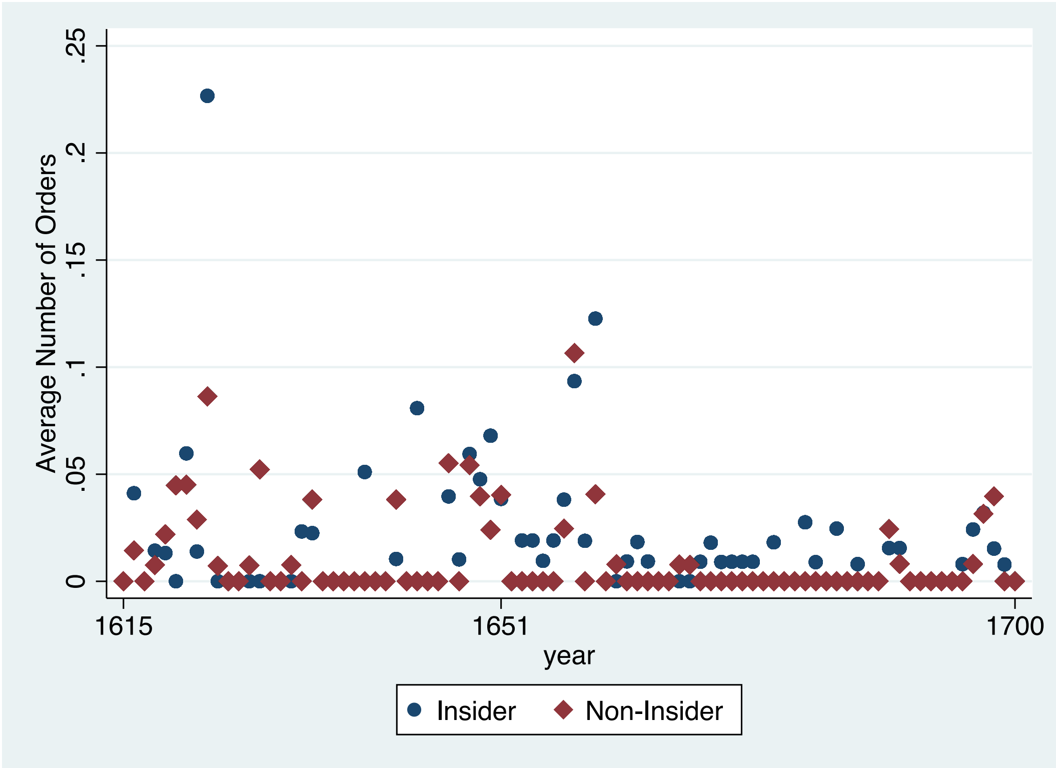



Table 2.A.6: Robustness Check: Importance of Tokyo Government's Orders

| | Orders from the Tokyo Government | |
|---|---|---|
| | (1) | (2) |
| Insider * After 1651 | -0.0216 | -0.0218 |
| | (-0.72) | (-0.71) |
| Domain FE and Year FE | Yes | Yes |
| Domain-level Trend | No | Yes |
| Observations (Domain * Year) | 19459 | 19459 |

Standard errors in parentheses are province-level cluster robust. Standard errors are domain-level cluster robust.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$,and $^{***}$ $p < 0.001$
I use a fixed-effects estimator. The data period is from 1615 to 1700. The data are at the domain and year level.

Table 2.A.7: Robustness Check: Possibility of a Political Race

| | Public Works by Local Governments | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Insider * After 1651 | $0.0209^{**}$ | 0.0168 | 0.0166 |
| | (0.00721) | (0.0106) | (0.0105) |
| Insider * After 1681 | 0.00220 | -0.00156 | -0.00156 |
| | (0.00959) | (0.0156) | (0.0156) |
| Era FE and County FE | Yes | Yes | Yes |
| County-level Trend | No | Yes | Yes |
| State FE * After 1651 | No | No | Yes |
| Observations (County * Era) | 5004 | 5004 | 5004 |

Standard errors in parentheses are 61-province-level cluster robust.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$
I use a fixed-effects estimator. The data are at the county and era level. For data points, the start year for each era is 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, and 1688.

Table 2.A.8: Robustness Check: Career Concern

| | Public Works by Local Governments | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Insider * After 1651 | $0.0210^{**}$ | $0.0147^{+}$ | $0.0157^{+}$ |
| | (0.00748) | (0.00839) | (0.00900) |
| Cabinet Member * After 1651 | 0.0142 | $0.0530^{+}$ | 0.0418 |
| | (0.0165) | (0.0310) | (0.0291) |
| Era FE and County FE | Yes | Yes | Yes |
| County Trend | No | Yes | Yes |
| State FE * After | No | No | Yes |
| Observations (County * Era) | 5004 | 5004 | 5004 |

Standard errors in parentheses are 61-province-level cluster robust.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$
I use a fixed-effect estimator. The data are at the county and era level. As for data points, the start year for each era is 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, and 1688. *Cabinet Member* is defined as the weight average of the number of cabinet member produced by domains at the county level.

Table 2.A.9: Robustness Check: Changes in Military Incentives

| | Castle | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Insider | -0.00136* | | |
| | (0.000667) | | |
| Insider * After 1651 | 0.00154* | 0.000292 | 0.00111 |
| | (0.000691) | (0.000877) | (0.000692) |
| Year FE | Yes | Yes | Yes |
| Domain FE | No | Yes | Yes |
| Insider * Trend | No | No | Yes |
| Observations (Domain * Year) | 19929 | 19929 | 19929 |

Standard errors in parentheses are domain-level cluster robust.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$
The data period is 1615 to 1700, and the data are at the domain and year level.

Table 2.A.10: Robustness Check: Effect of Transfers on Recording

| | Public Works by Local Governments | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Transfer 1700-1868 | -0.00142 | -0.00141 | -0.00134 |
| | (0.00100) | (0.000980) | (0.00113) |
| Insider | -0.0176** | -0.0164* | -0.0111 |
| | (0.00584) | (0.00622) | (0.00680) |
| Insider * After 1651 | 0.0224** | 0.0224** | 0.0224** |
| | (0.00772) | (0.00772) | (0.00777) |
| Era FE | Yes | Yes | Yes |
| State FE | No | Yes | No |
| Province FE | No | No | Yes |
| Observations (County * Era) | 4680 | 4680 | 4680 |

Standard errors in parentheses are 61-province-level cluster robust.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$
I conduct an OLS. The data are at the county and era level. For the data points, the start year for each era is 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, and 1688.
"Transfer 1700–1868" denotes the average number of transfers from 1700 to 1868.

Table 2.A.11: Robustness Check: Controlling for Detached Area

| | Public Works by Local Governments | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Insider * After 1651 | 0.0233** | 0.0176+ | 0.0221** | 0.0169 |
| | (0.00799) | (0.00943) | (0.00771) | (0.0116) |
| Main Land * After 1651 | -0.0105 | -0.00119 | -0.00831 | -0.000832 |
| | (0.00686) | (0.0102) | (0.00667) | (0.0113) |
| Insider * After 1681 | | | 0.00306 | -0.00170 |
| | | | (0.00998) | (0.0161) |
| Main Land * After 1681 | | | -0.00590 | 0.000951 |
| | | | (0.00996) | (0.0133) |
| Era FE and County FE | Yes | Yes | Yes | Yes |
| County-level Trend | No | Yes | No | Yes |
| Observations (County * Era) | 5004 | 5004 | 5004 | 5004 |

Standard errors in parentheses are 61-province-level cluster robust.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$
I used a fixed-effects estimator. The data are at the county and era level. For data points, the start year of each era is 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, and 1688.

Table 2.A.12: Other Channels?: Role of Experience

| | Public Works by Local Governments | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Insider * After 1651 | 0.0212** | 0.0204** | 0.0162+ | 0.0155 |
| | (0.00747) | (0.00670) | (0.00888) | (0.00950) |
| Insider * Experience * After 1651 | -0.000190 | -0.000138 | -0.000462 | -0.000415 |
| | (0.000282) | (0.000432) | (0.000735) | (0.000812) |
| Experience * After 1651 | | -0.0000429 | | -0.0000394 |
| | | (0.000273) | | (0.000314) |
| Era FE and County FE | Yes | Yes | Yes | Yes |
| County-level Trend | No | No | Yes | Yes |
| Observations (County * Era) | 5004 | 5004 | 5004 | 5004 |

Standard errors in parentheses are 61-province-level cluster robust.
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, and $^{***}$ $p < 0.001$
I use a fixed-effect estimator. The data are at the county and era level. As for data points, the start year for each era is 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, and 1688.
*Experience* is defined as the weight average of the number of years from the last transfer of domains before 1651 at the county level.

# Chapter 3

# Measuring Economic and Political Development in Pre-modern and Early Modern Japan

## 3.1 Introduction

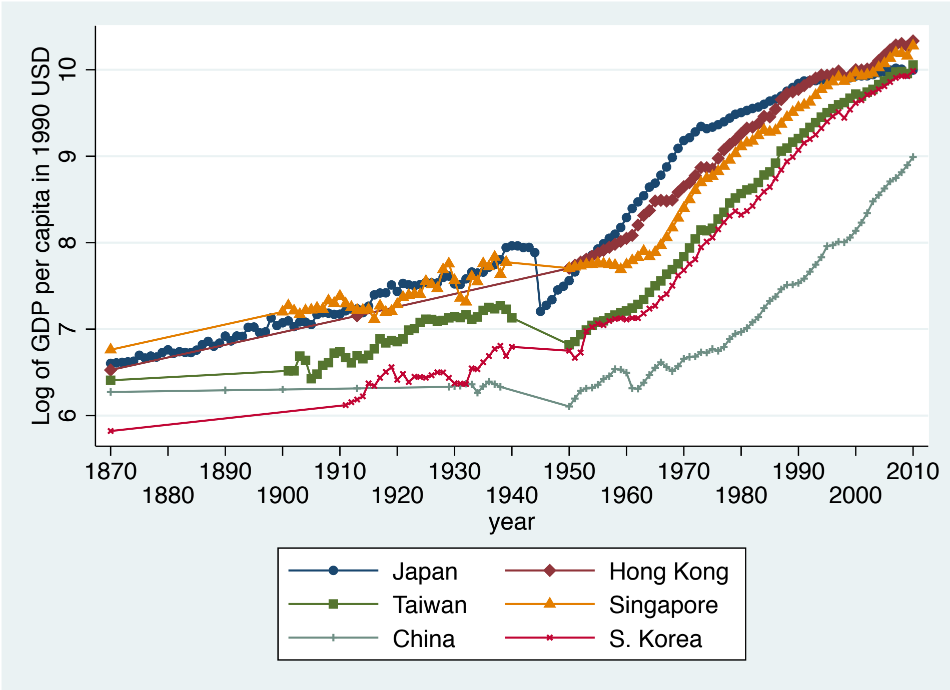
East Asian countries experienced different economic paths, despite their geographical proximity. As we saw in Chapter 1, Japan started to take-off in the late 19th century, which is generally considered as the earliest industrialization in East Asia. As shown in Figure 3.1, from the late 19th century to 1945, the growth of GDP per capita in Japan was similar with those in the two city-states of Hong Kong and Singapore[1] and higher than those in other regional neighbors. In the 1950s and 60s, Japan began progressing again after the destruction during WWII, as did their neighbors, during the "East Asian Miracle" of the 1960s–1990s (World Bank, 1993) and China's take-off in the 1990s. From the 2000s to 2010s, convergence within the region seemed to occur.

Their political and institutional paths vary as well. China was the superpower in the ancient era and influenced the culture and institutions of the region. For example, Japan imitated the Chinese administrative system in 610, maintained cultural interaction by sending missions from 600 to 894, and defeated the invasion of the Yuan dynasty in the 13th century. Hong Kong became Chinese territory during the Qing dynasty in (221–206 BC). The Korean Peninsula was also culturally influenced and occasionally under China's control. However, in the 19th century, the region experienced a big transition from a Chinese order to a Western order. After Britain defeated China in the 1842 Opium War, China had to cede Hong Kong to Britain. Taiwan was ceded to Japan from China after the Sino-Japanese War in 1894. Korea, which was a subject state under the tributary system of Chinese dynasties, signed an annexation treaty with Japan in 1910. During WWII, the Japanese Army occupied part of China, Hong Kong, and other countries in Southeast Asia such as Singapore. After WWII, the Cold War had a huge effect on the area. China had a civil war, resulting in the China-Taiwan divide between the Chinese Communist Party and Chinese Nationalist Party. Korea split into North and South under the occupation of the Soviet Army and US Army, respectively. Both gained independence in 1950, but North Korea became a country under a totalitarian dictatorship, which is still in power today, while South Korea experienced a military dictatorship and became democratic afterward. Hong Kong was a British colony until the handover to China in 1997, though the Sino-British Joint Declaration in 1984 set a "One Country, Two Systems" policy for Hong Kong to keep its capitalist economic system, which will expire in 2047.

A new empirical literature is emerging that revisits this history, particularly the initial stage of development to understand the different experiences of economic development in these countries. Using historical data sets, they try to identify the causal impacts of

[1]One potential reason why Japan advanced more after 1945 is its abundant labor resources in rural areas. Hayashi and Prescott (2008) argues that the removal of cultural barriers to migrate from rural to urban areas boosted structural change and economic growth.

Figure 3.1: Economic Growth of East Asian Countries 1870–2010




Source: The Maddison-Project, 2013 version.

policies and institutions or discuss its macro-economic importance as in Yoo and Steckel (2010), Bai (2014), Sng and Moriguchi (2014), Marden (2015), Dell et al. (2015) and Lane (2016).

In this chapter, I will summarize Japanese economic history and its data, and discuss the future research direction. I first briefly explain the background of Japanese history and data availability in the Edo and Meiji periods, including data I did not use in the previous chapters. The data description focuses on disaggregated data at the county level, which is a useful means to exploit regional variation for empirical analysis. The other chapters in this thesis or other studies use some of the data sets, though some data sets were not used in existing economics research as far as I am aware. In the conclusion, I discuss potential research directions.

The remainder of the paper proceeds as follows. Section 3.2 explains the historical context.[2] Sections 3.3 and 3.4 explain the available data and the descriptive statistics for the Edo and Meiji periods, respectively, and Section 3.5 concludes with the future research direction.

[2]See Brown (2008) and Sugiyama (2012) for additional readings.

## 3.2 Background

From the 12th century to 1868, the warrior class played the leading role in Japanese politics. Among the warrior classes, the most powerful houses established Tokyo government, a central power. From 1185 to 1333, the *Minamoto* family was the executive leader (*Shogun*) in the central power called *Kamakura bakufu*. After their collapse, *Asikaga* family took over until 1573 (*Muromachi bakufu*). The *Shogun* was heredity. However, after the *Ohnin* War in 1467, the *Muromachi bakufu* lost its political power, resulting in consecutive civil wars between the warrior houses (local lord). This political chaos lasted about 100 years and *Oda Nobunaga* was the first to successfully gain central power after 1467. As an iconic event, he purged the lame duck *Shogun* of the *Muromachi bakufu* from Kyoto in 1567. However, he failed to established a stable political system due to his death in an uprising at *Honno-ji*, and his successor *Toyotomi Hideyoshi* failed to create stability. The battle of *Sekigahara* in 1600 occurred in this context. *Tokugawa Ieyasu*, the leader of the East, won the battle to take over as *Toyotomi Hideyoshi*'s successor and he established the Tokyo government.

As discussed in Chapter 2, The Tokyo government was a feudal system politically, and therefore highly fiscally decentralized.[3] The local lords and *Tokugawa* houses had their own territories and collected land tax. They had their own courts to solve local conflicts, soldiers and officers (vassals), and could issue notes. However, the Tokyo government could control the local lord's tenure. Formally, the local lords were allotted land by the Tokyo government, who could appropriate this land. This occurred often at the beginning of Edo period as part of the post-civil war process. It also happened when the local lords did not have sons, meaning that they lacked a successor, or when the local lords violated the laws set by the Tokyo government (*Buke Shohatto*). The Tokyo government could transfer the local lords to other domains. In that case, all local lords and his vassals and their families had to move to a new domain. The Tokyo government used this policy for military purposes to allocate local lords to reduce the risk of conflict or call some local lords to a domain close to Edo when they appoint the local lords to multiple positions in the Tokyo government. Additionally, the local lords had to spend alternate years between Edo and his domain, leaving their wives and heirs in Edo as hostages (the alternate attendance, or *sankin kotai*). These two policies were with full compliance until the end of the Edo period.

The local lord's political situation differed by political identity. Putting aside *Tokugawa*'s relatives, the local lords had two identities: *tozama* (outsiders) and *fudai* (insiders) depending on whether they supported *Tokugawa* during the war in 1600. The former

[3]See Fujino (1975, 2002); Kato (1969); Somada (2011) for more a precise background on the political system.

supported *Tokugawa*, who used them to monitor outsiders to avoid further conflicts after the war. The Tokyo government also cross-appointed some local lords as cabinet members in the Tokyo government, such as *roju*, which were almost exclusively insiders until the 1860s. The identity did not change over the Edo period aside from some exceptional cases such as blood relationships with insiders.

Edo (the former name of Tokyo) started to grow in this era. The city was mostly marsh in the past, but *Tokugawa Ieyasu*'s political success changed the area into a big city. The Tokyo government specified five main routes from Edo where they put horse stations and tolls to hasten and control the flow of people and goods. The local lords used this to go to their offices in Edo for the alternate attendance, attracting migrants from surrounding rural areas and resulting in the growth of Edo. The population was approximately one million in the early 18th century, about twice as large as the population of London or Paris at that time.[4] The huge population was partly sustained by a water system, which was at least as good as in the West (Hanley, 1997). The alternate attendance system also facilitated the cultural flow between Edo and the other regions (Vaporis, 1997).

Osaka grew as well and became the trade center for rice and other goods. Though Osaka was a battlefield of the war in 1615, *Tokugawa* put Osaka under their direct control due to its geographical potential as a domestic trade hub for marine transportation. Its rice market, *Dojima Kome Kaisho*, traded rice from across the nation and started the world's first futures exchange in 1710.[5] In rural areas, various actors such as the Tokyo government, local lord, village community, or powerful merchants from urban cities invested in agricultural development projects such as irrigation and reclamation works. Several factors, including technological progress, geographical characteristics, and political factors affected these investments.[6]

The Tokyo government had restrictive trade policies related to foreign countries, especially Western countries. Over most periods, they allowed trade only with the Netherlands and China in *Nagasaki*, Korea in *Tsushima*, and the Ainu people in *Matsumae*. Culturally, they prohibited Christianity in 1613 and purged missionaries from the country. Foreign traders were not allowed permitted beyond the foreign settlement in Nagasaki. This changed in the 1850s after political pressure from Western countries and the conclusion of unequal treaties with them.[7]

The Edo period was politically stable, but the foreign pressure led to the Meiji Restoration. The fact that China, the superpower in the region, was defeated in 1840 by Britain

[4] Half of the population belonged to the samurai class (Sugiyama, 2012), meaning that the population was hugely gender-biased.

[5] See Takatsuki (2012).

[6] See Kikuchi (1958, 1986) and Chapter 2 of this thesis

[7] Bernhofen and Brown (2004) uses this natural experiment to test the Heckshser-Ohlin theorem.

changed the Japanese view on the East and West. The unequal treaties concluded in 1850s triggered anti-foreign sentiment toward Western countries. After the conflict of local lords, *Satsuma* and *Choshu*, with Britain and France in 1863–1864, they thought that the best strategy to challenge Western colonization was to establish a powerful centralized government and adopt Western technologies. After several civil wars with the opponents, they successfully established a new government in 1868, the Meiji Restoration.

The Meiji government implemented several reforms to centralize and modernize the politics. They ended all feudal power by taking the land and people (*Hanseki Hokan*) in 1869 and replacing local lords with prefectures, local governments (*Haihan Chiken*), in 1871. Afterward, the local government chiefs were appointed from the central government, who typically sent people from other areas rather than appointing locals (Muehlhoff, 2013). The appointees had connections with politicians in the central government, but in 1899, the central government introduced an exam system to avoid patronage. On the other hand, the important positions in the central government were mainly occupied by the *Satsuma*, *Choshu*, and their allies (Meiji oligarchy). They also reformed the land tax system in 1873 (*Chiso Kaisei*) to unify the tax rate and establish individual property rights. Land tax remained the most important revenue source and accounted for more than half of tax revenue in the 1880s–1890s.

At the same time, they sent their politicians and bureaucrats to Western countries, hired Western specialists, and adopted advanced technology for the administration, infrastructure, military, and economy. About 50 students accompanied the Iwakura mission that visited the United States and Europe in 1871–1873. In 1872, 214 foreign specialists were working in the central government. These human and financial resources from the central government led to many achievements, such as the development of laws, modern state-owned factories, post offices, telegraphs, and railroads.

In terms of religion, the Meiji Restoration restored imperial rule. The imperial house and its aristocrats held the reins of government in the ancient era. After the 12th century, they lost their tax base and political power, but they held their authority based on their religious power. For example, they appointed the most powerful warrior house to a special position to authorize them. Therefore, besides the Tokyo government, the imperial house was the only authority potentially able to sustain a powerful central government during the nation-building period.

Education was another key element for nation building and modernization.[8] Before the Meiji period, schools (*hanko*) existed for samurai's sons, which were financed by the local lords. They typically taught reading, writing, and Confucianism. Private schools also existed, which typically taught reading, writing, and arithmetic to the merchant class.

[8]See Ministry of Education (1981) for its history.

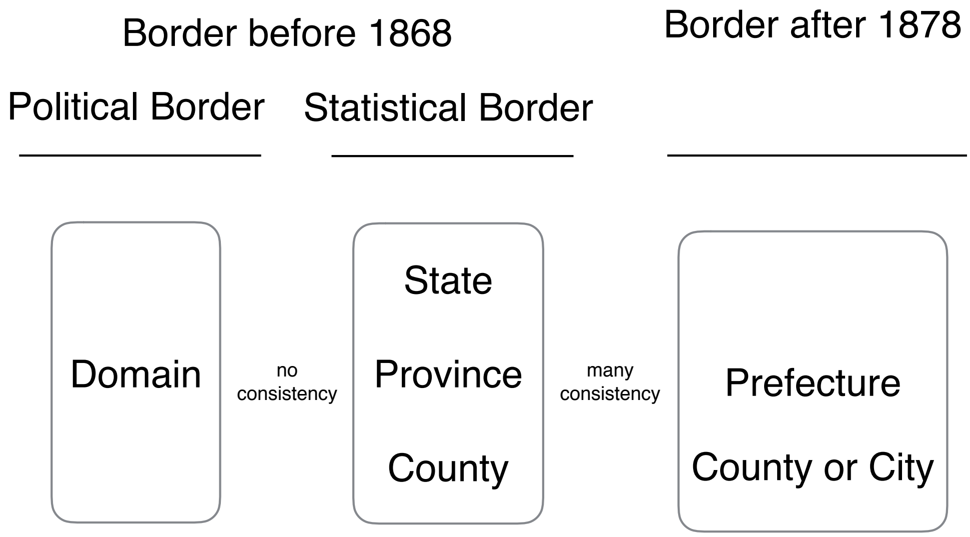
However, these were not under a uniform regulation by the central government, and were not open to everyone. After the Meiji Restoration, the government set up a uniform education system in 1872 and revised it in 1886. In 1886, primary education, often seen as a key to education in developing countries, became mandatory. In 1890, the government required local municipalities to set up at least one primary school in principle. In addition, the Emperor signed the Imperial Rescript on Education (*kyoiku ni kansuru chokugo*) to state the objective of education based on an emperor-centric philosophy. Students had to memorize the text and classrooms had the Emperor's portrait as a key nation building practice.

The Meiji period saw a drastic change in the industrial sector, which adopted factory production and Western technologies such as steam power. For example, the government set up the *tomioka* silk mill in 1872, which had three hundred basins powered by steam engines (Kiyokawa, 1995; Kobayashi, 1977), to demonstrate Western technology to the private sector. Though these state-owned factories were not performing well and were disposed of around the 1890s, the private sector, such as the Osaka Cotton Spinning Company established in 1882, also started to adopt Western technologies. On the other hand, important agricultural technologies, such as high-yield varieties of rice and cocoon or agricultural machines were adopted after the 1900s (Kiyokawa, 1995).

Though development in the non-agricultural sector occurred in large firms that adopted Western-style production, other studies also emphasize development in small enterprises. Nakamura, ed (1997); Tanimoto (2006); Matsumoto (2006) call these enterprises as the traditional sector and it constantly grew in terms of sectoral labor share from the 1880s to 1935 by absorbing the increased labor force from the population growth in the agricultural sector. The traditional sector consists of manufacturing, services, and commerce sectors, but the latter two were the main driver of this growth.

In line with the industrialization, Japanese imperialism began growing in the Meiji period. Japan obtained Taiwan in 1895 from China after the Sino-Japanese War, the southern half of Sakhalin and southern Manchuria in 1905 from Russia after the Russo-Japanese War, and made Korea a protectorate in 1905 and colonized it in 1910. This expansion would be motivated by several factors, but two differed from the motivations behind Western colonization: maintaining independence under threat of Western imperialism and demands to revise unequal treaties giving extraterritorial rights to Western countries and depriving Japan of its autonomy to impose tariffs (Beasley, 1987; Kimura, 1995). After two wars with China and Russia, in 1895, Japan nullified Western countries' extraterritorial rights, and in 1911, Japan regained its tariff autonomy.

Figure 3.2: The Unit of Observations in Different Time Periods




## 3.3 Data: Edo Period (1615–1868)

Unfortunately, we do not have as much statistical data for the Edo period as we might expect. This is because the Tokyo government did not have the state capacity to collect and keep data, and its decentralized system hindered data collection across the nation in a uniform format. Additionally, parts of the Tokyo government's documents were lost due to a fire caused by the Great Kanto Earthquake in 1923. However, there are some available data sets covering the nation.

Before explaining the data, I will introduce the unit of observation. As discussed in Chapter 1, there are two types of borders: political and statistical borders. As shown in the left side in Figure 3.2, statistical borders have state, province, and county from the top, and are not consistent with the political borders that determine local lords' domains.

In this section, I summarize the data during the Edo period. It covers land investigation data, population data, administrative records to capture political events and other retrospective surveys on public works. Their descriptive statistics follows.

### 3.3.1 Data List

#### Land Investigation

*Tokugawa* or the Tokyo government conducted several land investigations (*kenchi*) across Japan to record the lord's name and the land productivity at the village level. Land productivity measure was called *kokudaka*. The Tokyo government investigated the amount of *kokudaka* held by the local to determine their obligation. For example, it determined

Table 3.1: Available Data on Land Investigations

| Year | Unit | Available Variable | Source |
|---|---|---|---|
| 1600s | Province (*Kuni*) | Land productivity | Ono (1987) |
| 1640s | Village (*Mura*) | Land productivity and lord's name | Izumi (2008) |
| 1680s | County (*Gun*) | Land productivity | Ono (1987) |
| 1830s | County (*Gun*) | Land productivity and lord's name | Kinsei Rekishi Siryo Kenkyukai (2010) |
| 1870s | Village (*Gun*) | Land productivity and lord's name | National Museum of Japanese History (1990) |

the minimum number of *samurai* the local lords should have. Several land investigations occurred during the Edo period and the transition to the Meiji period. Among them, data for the land investigations in the 1600s, 1640s, 1680s, 1830s, and 1860s are available in several forms (original, copies, etc.). In the 1640s, not all documents were kept, but copies of the local lords or similar land investigations at the local lords level complements the missing parts. Table 3.1 summarizes the unit of observation and available information.[9]

It is tempting to use the land productivity measure to analyze economic growth during the period, but it would be misleading. Actually, the land productivity measure was not updated well in the 17th and 18th centuries. This means that land productivity measure do not change according to the productivity growth. This will be because the main purpose of land investigation was to know the owner of land and local lords and local villages had incentives to under-report their land productivity to avoid heavy duties.[10][11]

### Population

The Tokyo government started studying the population in 1721 and conducted 22 investigations until 1846. The unit of observation is the province level, and often contains a gender breakdown. Home Ministry (1994) provides about half of the results. There are more disaggregated data in some regions using records kept at community level used in demography such as *Shumon Ninbetsu Aratame Cho* (宗門人別改帳), but I am not aware of data at this level of disaggregation covering the whole nation during the Edo period.

### Administrative Records

The Tokyo government maintained diaries to record policies such as laws, punishments, and appointments to the local lord. The most important source is the *Ryuei Hinamiki* (柳営日次記), and the original images are accessible in National Archives of Japan Digital

[9] I use Japanese characters in the reference list when I think it is useful in searching for sources.

[10] In some cases, the record contains data for the increase in land productivity separately, recorded as "new filed" (*shinden*) , for each village. However, this is not uniformly conducted

[11] Land productivity data after 1868 is more reliable in this sense.

Archive. In related information, the Tokyo government made family trees of local lords. The *Kan-ei Shokakei Zuden* (寛永諸家系図伝) was published in 1643, and was continued in the *Kansei Choushusho Kahu* (寛政重修諸家譜) published in 1812. These also cover other important people such as vassals in the Tokyo government and doctors, but the *Tokugawa* family was not covered by the *Kansei Choushusho Kahu*, so the *Tokugawa Shoke Keihu* (徳川諸家系譜) complements it. In addition, we can use other publications, *Danka Hu* (断家譜) and Keizu Sanyo (系図纂要) published in 1809 and the late 1850s, respectively, to find the family trees of all local lords during the Edo period.

These are more than a thousand volumes of handwritten records from that time, and I am not aware of any handy digitized version. However, Nagahara, ed (1999) provides a summary of the local lords' history such as their size of domain and years of transfer and forfeit, which is useful to understand political events for the local lord.

#### Other Data

There are several other documents published after the Edo period. Because the land investigation data above does not adequately capture economic growth or investments, I list use two complementary works.

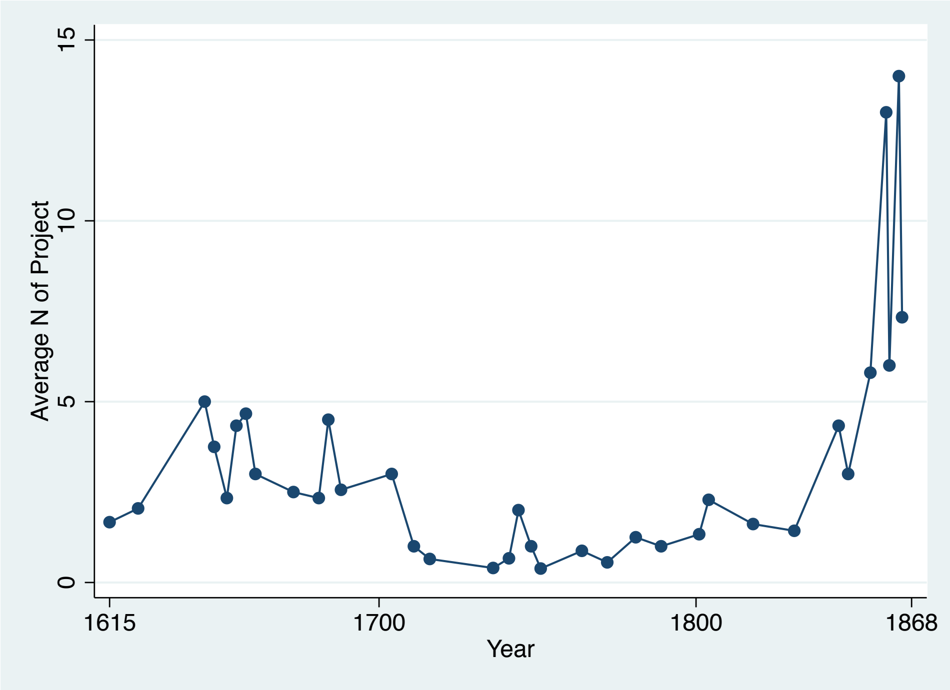
The first, Norinsho, ed (1927) published by the Ministry of Agriculture is an investigation of agricultural investment during the Edo period. They collected local documents, such as from local lords houses or rich farmers, and described each project with the timing, actors, and size, though precise information is sometimes missing. The second, Doboku-Gakkai (1994) published in 1936 by the Japanese Civil Engineering Academy, describes the history of civil engineering, including agricultural and other projects such as waterworks or bridges. Its second chapter describes agricultural projects similarly to Norinsho, ed (1927) by referring to it and additional sources. We can identify the era and county of each project.[12]

### 3.3.2 Descriptive Statistics

To capture economic growth during the Edo period, I provide descriptive statistics of agricultural projects in Figure 3.3. Figure 3.3 shows the average number of new public agricultural projects, such as irrigation, in each era. We can see that there were many projects in the 17th century and the end of Edo period, while the number of project became smaller from the middle of the 18th century to the middle of 19th century as discussed in Kikuchi (1958). In the 17th century, more than 2.5 projects started per year, while it became almost zero in the 18th century.

[12] Japanese eras changed frequently enough to see its time series pattern. During the Edo period, the

Figure 3.3: Number of Public Projects during the Edo Period




Source: Doboku-Gakkai (1994)

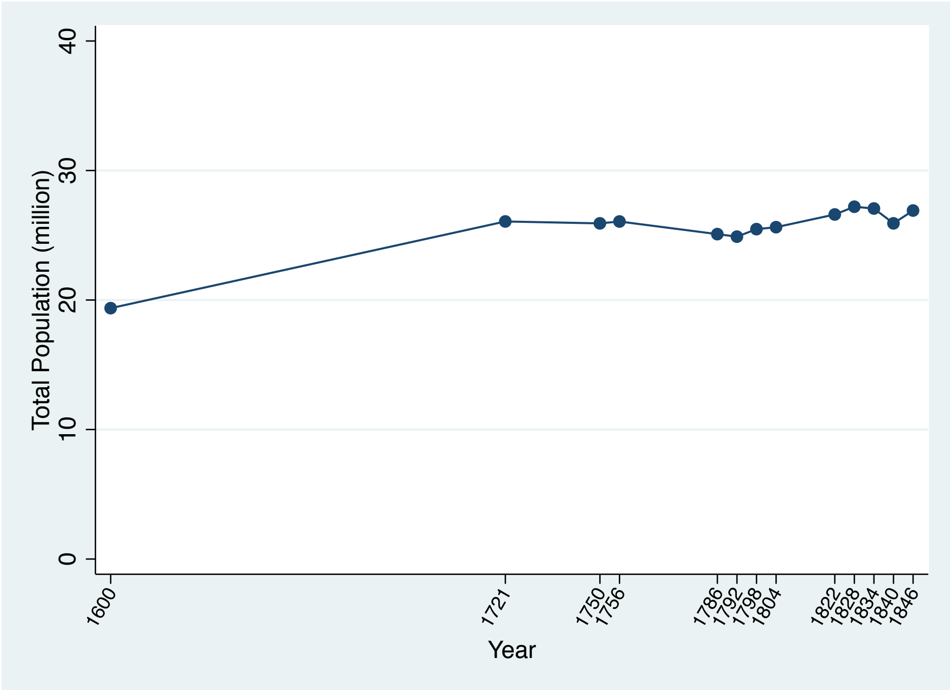
In Figure 3.4, I show the population growth from 1600 to 1847. Because there is no original data for 1600, I use estimates from Kinsei Rekishi Siryo Kenkyukai (2010). The figure is consistent with the pattern in Figure 3.3. From 1600 to 1721, the population grow by 34 percent, from 19 million to 26 million, and from 1721 to 1846, we do not see population growth in the long term, though it fluctuates in the short term. This matches the increase in agricultural projects in the 17th century and almost no projects in 18th century Figure 3.3, so the agricultural productivity is a key determinant during this period.

I now summarize the data related to politics during the Edo period. In Figure 3.5, I show the number of local lords during this period. The solid line shows the total number of local lords, which was increasing in 1615–1625 (post-war period after the battle of *Sekigahara*) and 1680–1700 (the first twenty years of rule by the fourth *shogun*, *Tokugawa Tsunayoshi*), and fairly constant in other periods. After 1700, there were 260 local lord. The pattern differs for insiders and outsiders. The insiders (dashed line) shows a similar pattern as the total, while outsiders (dash-dot line) shows the opposite pattern. This implies that the Tokyo government appropriated from outsiders and new insiders, typically *Tokugawa*'s vassals or brothers of insider local lords who emerged to take over the land.

In Figure 3.6 and Figure 3.7, I show the total amount and average of land produc-

average length of an era was 7.2 years.

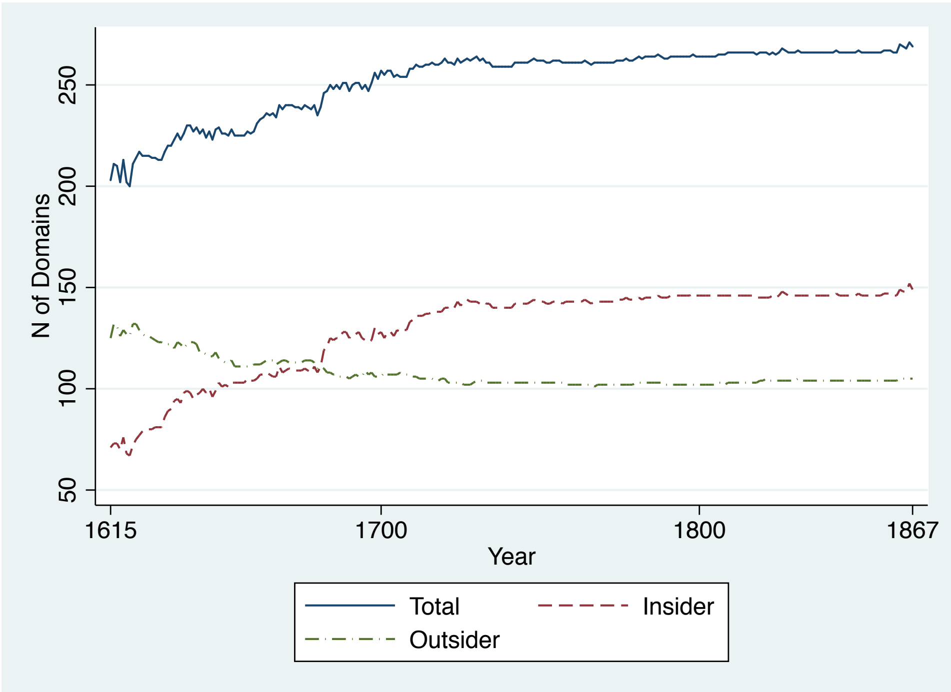
Figure 3.4: Population Growth, 1600–1846




Source: Kinsei Rekishi Siryo Kenkyukai (2010) for 1600 and Home Ministry (1994) for the rest. The number in 1600 is an estimate in Fujino (2008). This figure does not include *samurai* and aristocrats, unlike the investigation after the Meiji period.

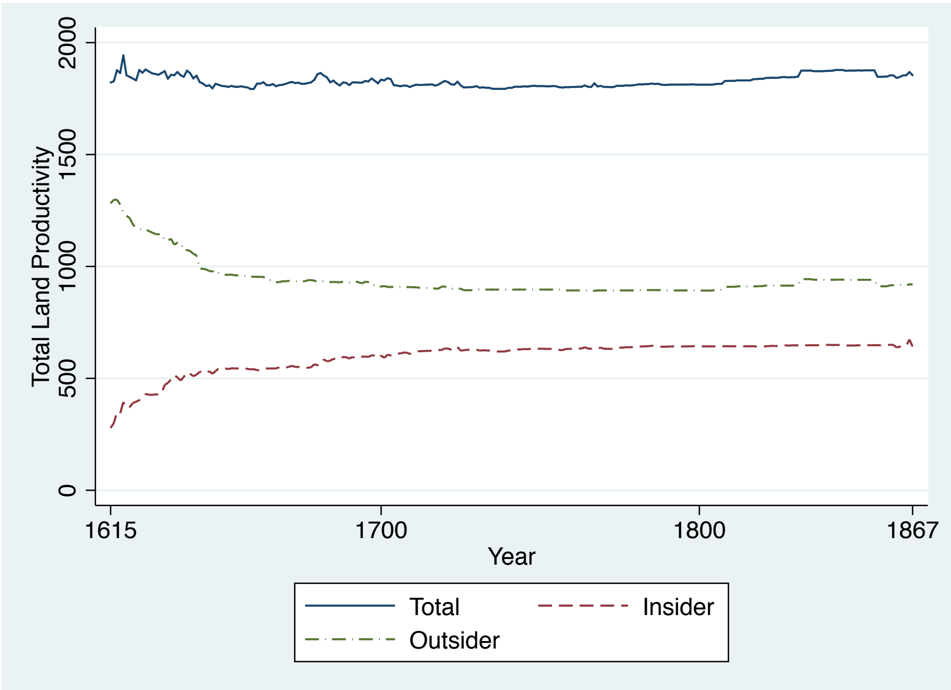
tivity and the amount of land owned by local lord. Similar to Figure 3.5, it shows the reallocation of land from outsiders to insiders in 1615–1625 and 1680–1700. In terms of the average, both insiders and outsiders show decline in the 17th century, except in 1615–1617. These patterns mean that the Tokyo government appropriated from powerful outsiders and re-distributed their land to one or more insiders in the 17th century. The figures also show almost no appropriations in the 18th and 19th centuries, suggesting political stability for local lord.

Figure 3.5: Number of Domains in the Edo period



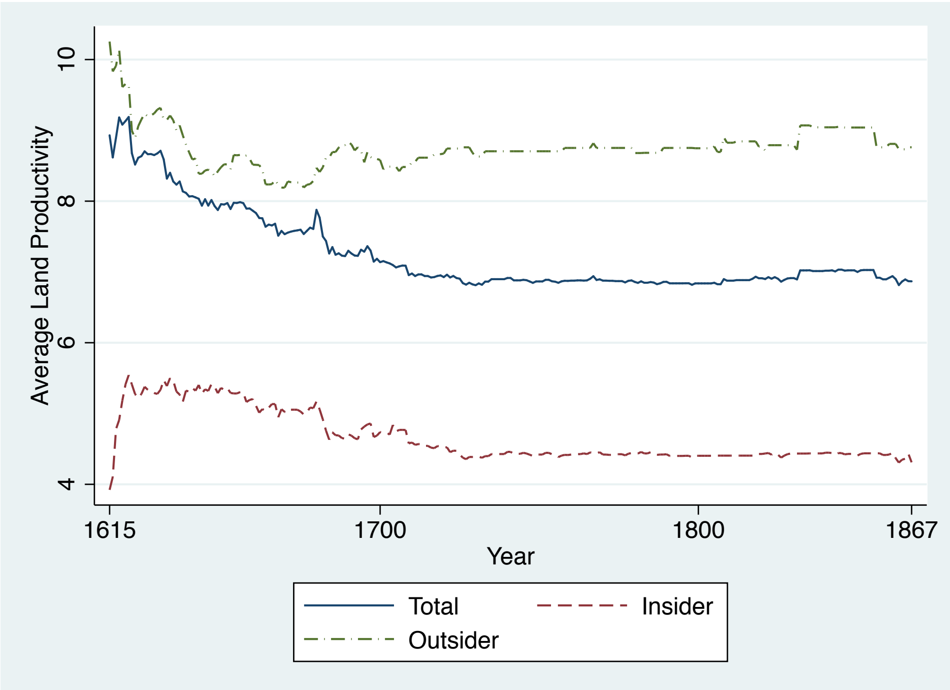

Source: Nagahara, ed (1999)

Figure 3.6: Total Land Productivity held by Local Lords in the Edo period




Source: Nagahara, ed (1999)

Figure 3.7: Average Land Productivity held by Local Lords in the Edo period




Source: Nagahara, ed (1999)

## 3.4 Data: After the Meiji Period (1868–1912)

I briefly explain the unit of observation before discussing the data list because there are some difficulties in constructing panel data, especially using data from the 1870s. As explained above, before 1868, the most common statistical unit was the county (*gun*) or province (*kuni*). After 1868, however, the Meiji set prefecture as the upper division of administration. At *Haihan Chiken* in 1871, the Meiji government replaced the formal feudal domains with prefectures. The boundaries are mostly the same as the domain boundaries, which were fragmented and do not match the provincial boundaries. After the mergers and splits in the prefectures in the early 1870s, the prefecture boundaries became similar to the traditional provincial boundaries in 1876, which is almost consistent with today's prefecture borders.

At the lower division, the government introduced a new local administration system for the lower division of administration in 1871 (*Daiku Shoku Sei*), which ignored county boundaries and placed local municipalities instead of traditional local communities. The local community opposed it and at the end the Meiji government, repealed the new system in 1878. The traditional county boundaries came into force again at the lower division, though there were simple splits or mergers afterwards.[13] Therefore, we can connect the Edo and Meiji data sets, except for that from 1871 to 1878 if the data set is at the county or prefecture-province level as shown in Figure 3.2.

After the Meiji Restoration, the new government began collecting statistical data in the 1860s to capture regional economic and military potential. The following data list will cover the publications about population, trade, and other ministry related statistics at the central government, publications by prefecture governments and colonial governments. It is a list of important data sets with disaggregated information rather than a complete list of data sets published in each period.

### 3.4.1 Data List

#### Population

The Meiji government administration had two definitions of population: the legally domiciled (*honseki*) population and the resident (*genju*) population. The former was the legal domiciled registration with class and family information, while the latter recorded current residential registration. People at birth were registered in his or her family account with their home addresses, which the legally domiciled accounts for, and when they migrate out to another town, then they have to register with the new town, which the residential

[13] Gifu prefecture was the exception. There were many villages that changed counties.

population accounts for. However, it is not clear whether the system worked perfectly, and when the whole family migrated out, their the legally domiciled might be moved to the new location. We will not be able to use the difference between the legally domiciled and residential population as net migration.

The Ministry of Home had a population statistics series covering 1872–1898, 1903, 1908, 1913, and 1918 (Home Ministry, 1877–1920). All data were contained in Home Ministry (1994). County-level data is available after 1879, and residential population information is available after 1884. the legally domiciled population comes with a breakdown at the social-class level: peerage (*kazoku*), former samurai (*shizoku*), and commoner (*heimin*). More precise statistics such as deaths and births became available for this series after 1880. The first census was conducted in 1920. Notably, this is the first investigation of the county-wise sectoral allocation of workers in a national publication. After this, they conducted the census every five years.

### Trade

After two treaties in the 1850s, the five ports of Yokohama, Nagasaki, Kobe, Niigata, and Hakodate became treaty ports. In 1889, nine other ports are specified as "special export ports" to allow exports of several specified goods with Japanese ships. In 1896–1897, these and another port are specified as "special trade ports" to allow imports. In 1899, they became treaty ports that permitted foreign ships to trade. The Meiji government was interested in foreign trade and trade statistics for 1866 and thereafter, such as in *Kaku Minato Yushutsu Buppin Kinryotaika Hyo* (n.d.) and Ministry of Finance (1882-1928a). They report trade statistics at the port- or partner-level.[14]

### Ministry Statistics Series

In 1885, the Meiji government set up ministries for each administrative field, such as education, foreign affairs, agriculture, commerce and industry, and war. Most had statistical yearbooks and provide data about the economy and society or their administration. I list publication series with the breakdown at the prefecture level below.

The Ministry of Agriculture and Commerce had a statistical yearbook that reported agricultural and industrial goods output and other related information at the prefecture level (Ministry of Agriculture and Commerce, 1888–1923). Moreover, they listed factories with details such as type of product or address in some publications in the 1880s and 1890s, so we can construct county-wise panel data. The list was published as *Kojo Tsuran*

[14] Note that Western consular reports are also available to double-check. For example, see "Commercial Reports by Her Majesty's Consuls in Japan" for Great Britain.

separately after 1902.[15][16]

The Ministry of Education has a statistical yearbook, initially published in 1873. We can use these to track the development of education at the prefecture level, though the format varies across the period (Ministry of Education, 1873–1911). It includes the number of schools, teachers, and pupils, and the financial status of schools, such as revenue, expenditure, and assets.

The Ministry of War had several interesting statistical tables. Their statistical yearbook, *Rikugunsho Tokei Nenpo*, published annually after 1887 (Ministry of War, 1887–1939) shows the results of provincial or prefecture level drafts and other aggregate figures on military expenditure and assets such as military horses. Additionally, they kept secret military statistics (Ministry of War, 1875–1911).[17] This series is in the public domain now and most recorded potential military resources such as population, the number of specialists such as doctors or craftsman, and the number of horses or ships at the county or village level.[18]

The Home Ministry had their own statistical yearbook as well from 1886 (Home Ministry, 1886–1944). It reported statistics about the police, local parliament, population, weather, and jails at the prefecture level. Because the Home Ministry controlled prefectural governments' budgets through their public projects, it reported local public finance, but it is more precisely reported in the Ministry of Finance's (Ministry of Finance, 1876-1928b) statistical yearbooks. The yearbooks over the periods are summarized in Toyo Keizai Shimpo Sha (1975).

The Postal Bureau recorded its statistics in their yearbook Postal Bureau (1872–1942). The figures in the yearbook are not disaggregated, but the Bureau kept the administrative records for post offices, which contain the types, addresses, and opening years at the post office level, useful for keeping track of the expansion of the post office network. The images of these records are available in Narumi Co. (2008).

### Local Publications

The publications described above were published at the national level, but there were statistical series published at the prefecture level. The most important and comprehensive ones are the *Huken Tokeisho* (府県統計書), published at the prefecture level. This publication started in 1884 under orders from the Home Ministry. The order was repealed in 1893, but prefectures maintained its publication. The advantage of this series is that they show county-level figures in most of the tables, from which we can analyze more

[15] See Hosoya (1976–1980); Matsuda et al. (1990a,b) for more precise information.

[16] Braguinsky et al. (2015) used these lists.

[17] Some are available in digitized format in Murayama (n.d.).

[18] As far as I know, no studies use this data in Economics exists as of yet.

disaggregated data than that in national publications. They cover various topics, such as population, agricultural and industrial outputs, land usage, education, prices, public finance, and police.[19]

There is, however, a disadvantage in this series. Because publication was local, the format and content were not uniform across prefectures and time, especially after the order from the Home Ministry repealed. Additionally, the prefecture did not publish the series in some years, so it is impossible to balance panel data throughout the period. Table 3.2 shows the publications available in 1884–1906 from the Regional and Industrial Statistics Archives, the most comprehensive archives covering this series, though I show it only every two years for brevity. We can that there are always some publications missing, at a rate of around 10–20 percent. There is little systematic pattern in the missing publications. The available data also differs by year and prefecture. For example, most publications in 1888 recorded the number of bridges, but only about the half of the publications in 1908 recorded this. Therefore, it is almost impossible to construct unbalanced panel data for all prefectures.

### Colonial Publications

Though this chapter focuses on mainland Japan, I also briefly discuss other data sets in Japanese foreign colonies. After Japan obtained foreign colonies, they implemented various surveys for their governance. For example, in Taiwan, the colonial government implemented the first population census in 1905, 15 years before the first population census in mainland Japan. Though there are many publication,[20] the statistical yearbook is the most comprehensive one covering a similar set of variables as *Huken Tokeisho* from 1897 to 1944 (Office of Governor-General of Taiwan, 1899–1944). The colonial government in Korea also had a similar statistical yearbook from 1907 (Office of Governor-General of Korea, 1907–1944). Their unit of observation were mostly at the higher division level, but the key variables, such as population, were reported at the county-level.[21] Additionally, factory lists with their county name, number of Japanese and local workers are reported in their very earlier periods.[22]

[19] See Matsuda (1980) for more description.

[20] Nihon Keizai Tokei Bunken Center in Institution of Economic Research, Hitotsubashi University (1985) and Nihon Keizai Tokei Bunken Center in Institution of Economic Research, Hitotsubashi University (1994) provide the list of all publications for Taiwan and Korea.

[21] ho-ri-wan for Taiwan and *gun* for Korea

[22] Aggregated statistics replaced them afterwards.

Table 3.2: Availability of *Huken Tokeisho* 1884–1906

| Prefecture | 84 | 86 | 88 | 90 | 92 | 94 | 96 | 98 | 00 | 02 | 04 | 06 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Aomori | | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ | ✓ |
| Iwate | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ |
| Miyagi | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Akita | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ |
| Yamagata | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Hukushima | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Ibaraki | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Tochigi | ✓ | ✓ | ✓ | ✓ | ✓ | | | | | | | |
| Gunma | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Saitama | ✓ | | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Chiba | | | | ✓ | | | | ✓ | ✓ | ✓ | ✓ | ✓ |
| Tokyo | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Kanagawa | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Niigata | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ |
| Toyama | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Ishikawa | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Hukui | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ | | ✓ | ✓ | ✓ | ✓ |
| Yamanashi | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Nagano | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Gifu | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | | ✓ | ✓ | ✓ | ✓ |
| Shizuoka | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Aichi | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Mie | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ |
| Shiga | ✓ | ✓ | ✓ | ✓ | ✓ | | | ✓ | ✓ | ✓ | ✓ | ✓ |
| Kyoto | | ✓ | ✓ | ✓ | ✓ | | | | ✓ | | ✓ | ✓ |
| Osaka | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Hyogo | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Nara | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Wakayama | ✓ | | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Tottori | ✓ | ✓ | ✓ | ✓ | | | | | ✓ | ✓ | ✓ | ✓ |
| Shimane | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Okayama | ✓ | ✓ | | | | | | ✓ | ✓ | ✓ | ✓ | ✓ |
| Hiroshima | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | | ✓ | ✓ |
| Yamaguchi | | | | | | | ✓ | | ✓ | ✓ | ✓ | ✓ |
| Tokushima | | | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Kagawa | | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Ehime | ✓ | ✓ | | | | ✓ | | | ✓ | ✓ | ✓ | ✓ |
| Kochi | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ |
| Hukuoka | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Saga | ✓ | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Nagasaki | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | | ✓ | ✓ | | |
| Kumamoto | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Oita | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | | ✓ | ✓ |
| Miyazaki | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | | ✓ | ✓ | ✓ | ✓ |
| Kagoshima | | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Total Available Prefectures | 35 | 37 | 39 | 42 | 40 | 36 | 34 | 36 | 37 | 41 | 43 | 43 |

✓means that at least one volume is available.

### 3.4.2 Descriptive Statistics

Because there are many data sets for this period, I focus on population as a major indicator of economic growth, trade volume as openness, and tax revenue and education and as indicators of public sector growth. I will cover structural changes or technology adoption as other indicators of economic growth in Chapter 3 as the key outcomes.

#### Population

Figure 3.8 shows population growth during 1872–1920 based on the legally domiciled population, except the first census data in 1920. The figure clearly shows the population growth during the period. It is not straightforward to match data for the pre-Edo period in subsection 3.3.1 since the figures in the Edo period do not count *samurai* and aristocrats. The data in 1888, however, recorded the total legally domiciled population for these classes (1.98 million). Adding this number to the population in 1846, it becomes 28.9 million. Because it will be the upper bound assuming a positive growth rate for these classes, it is fair to say that either population growth started somewhere between 1846 and 1872 or the record in the Edo period underreported the whole population.

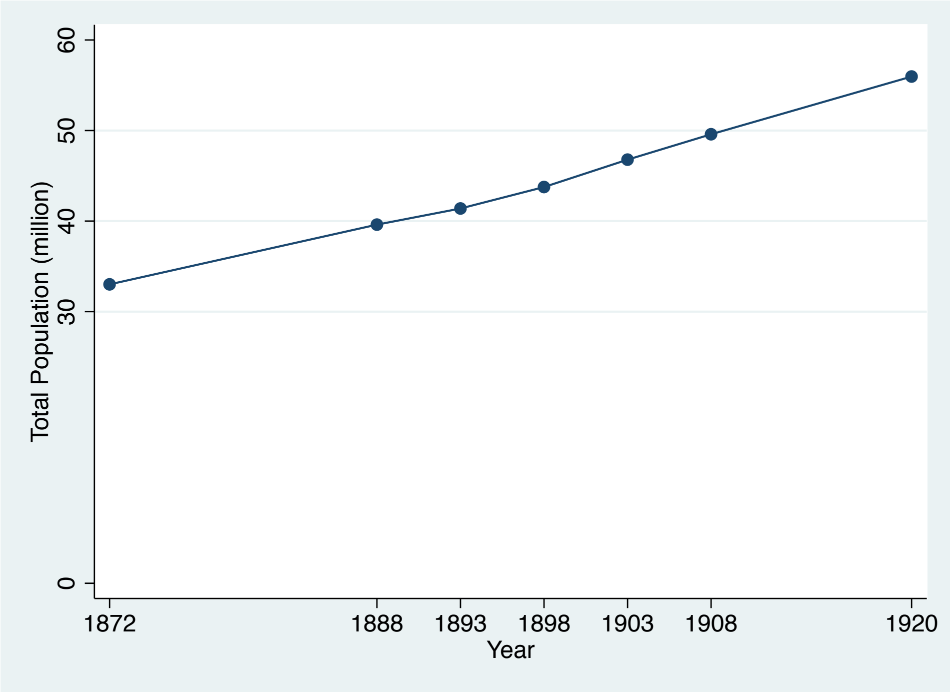
Urbanization is another aspect of modern economic growth as investigated in Chapter 1. I show this by plotting kernel density estimations for the log of population density at the county level in 1885, 1903, and 1920. Figure 3.9 tells us that the population density was moving to the right throughout the periods, and in 1920, we see a thick distribution around 7 to 8, suggesting the growth of cities. To investigate the urbanization process, I plot the relationship between the population growth rate and initial log of population density in Figure 3.10. The left panel shows that the relationship is negative in 1885-1903, suggesting mean reversion in population dynamics. In this context, bigger cities cannot grow as much as smaller cities. On the other hand, the relationship in the right panel is flatter than in the left panel, suggesting a constant growth rate, which enables bigger cites to grow. [23]

#### Trade

I plot the time series pattern of aggregate and port-level trade volume in Figure 3.11 and Figure 3.12 for 1882–1912, adjusted to 1882 price levels. In Figure 3.11, we see the increase in both export and import trade volumes as GDP increases. Figure 3.12 shows the breakdown of port-level exports for four major ports. The figure shows different patterns between ports. Yokohama and Kobe increased their trade from the 1880s, while Osaka grew after the 1890s, and Nagasaki was stagnant throughout the period. This was because

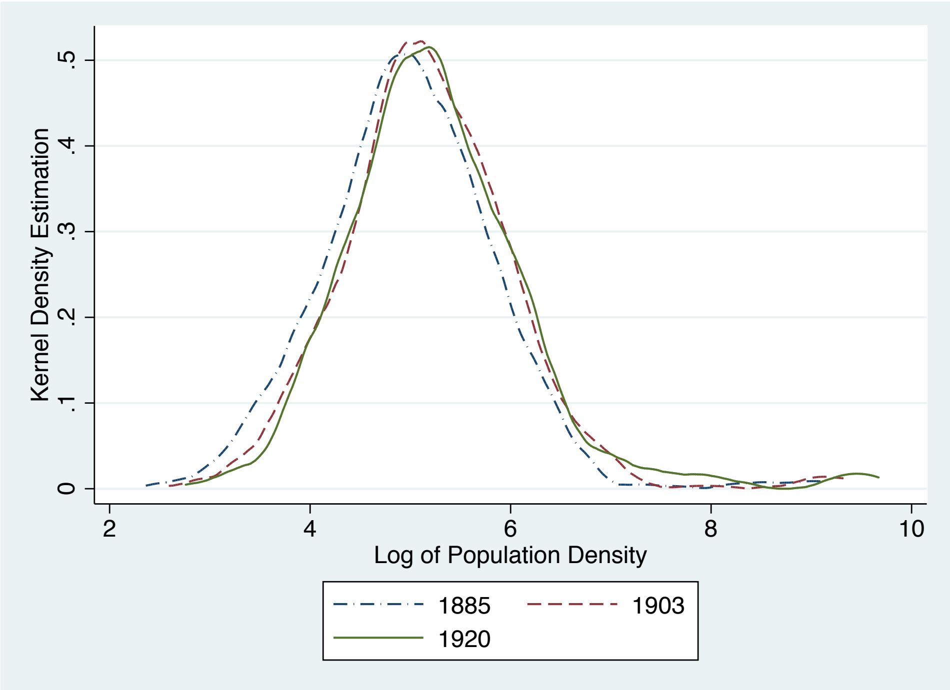
[23] We call this Gibrat law.

Figure 3.8: Population Growth, 1872–1920



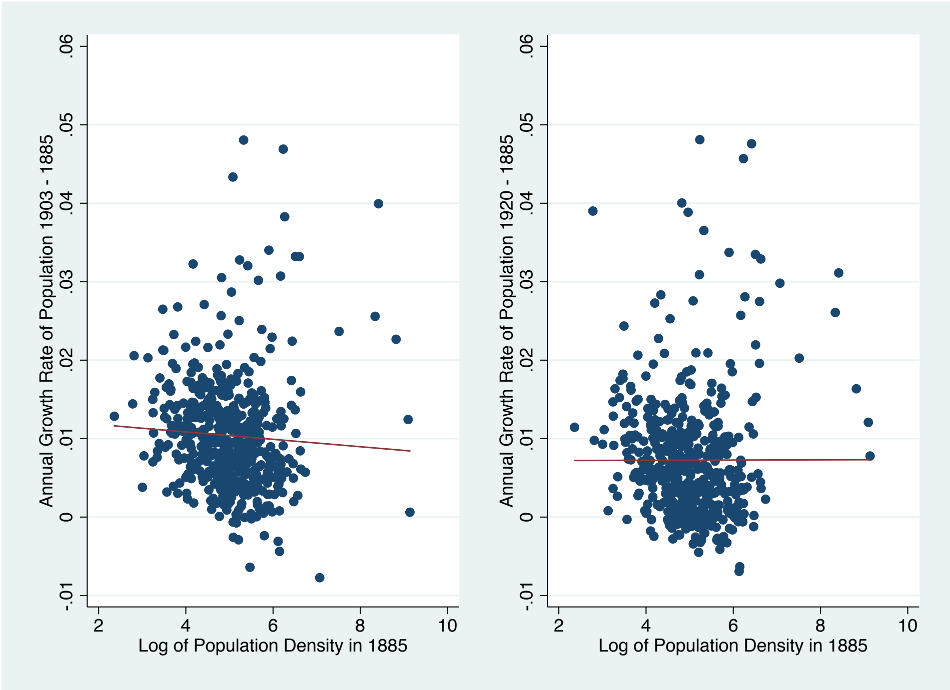

Source: Home Ministry (1877–1920). This number includes *samurai* and aristocrats, unlike the investigation before the Meiji period.

Figure 3.9: Log of Population Density, 1885–1920




Source: Home Ministry (1877–1920). I excluded *hokkaido*, *okinawa*, and other small isolated islands.

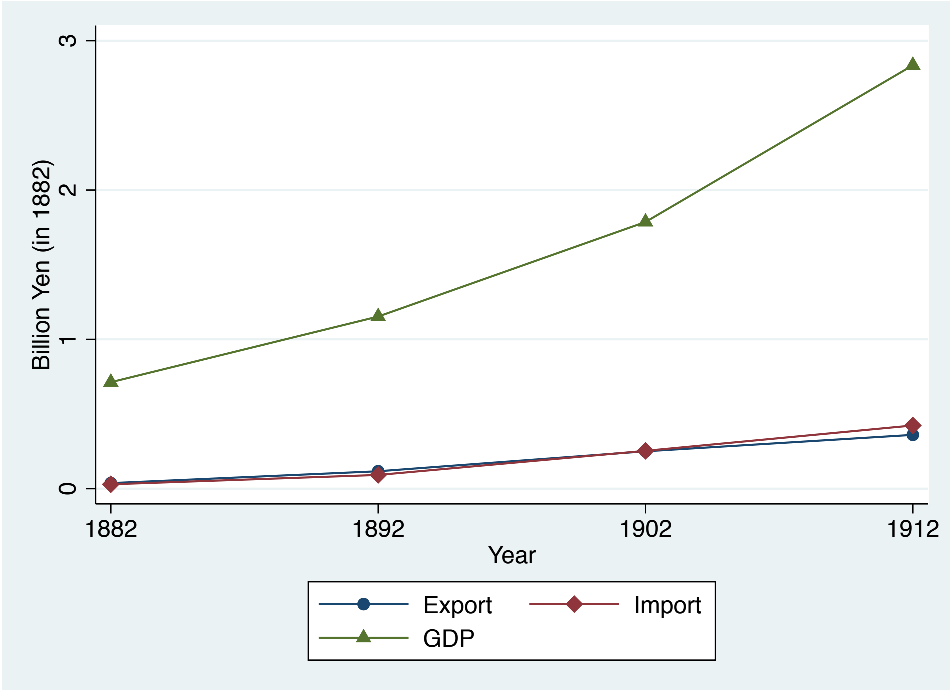
Figure 3.10: Gibrat Law: 1885–1903 and 1885–1920




Source: Home Ministry (1877–1920). I excluded *hokkaido*, *okinawa*, other small isolated islands, and a few outliers for its visibility.

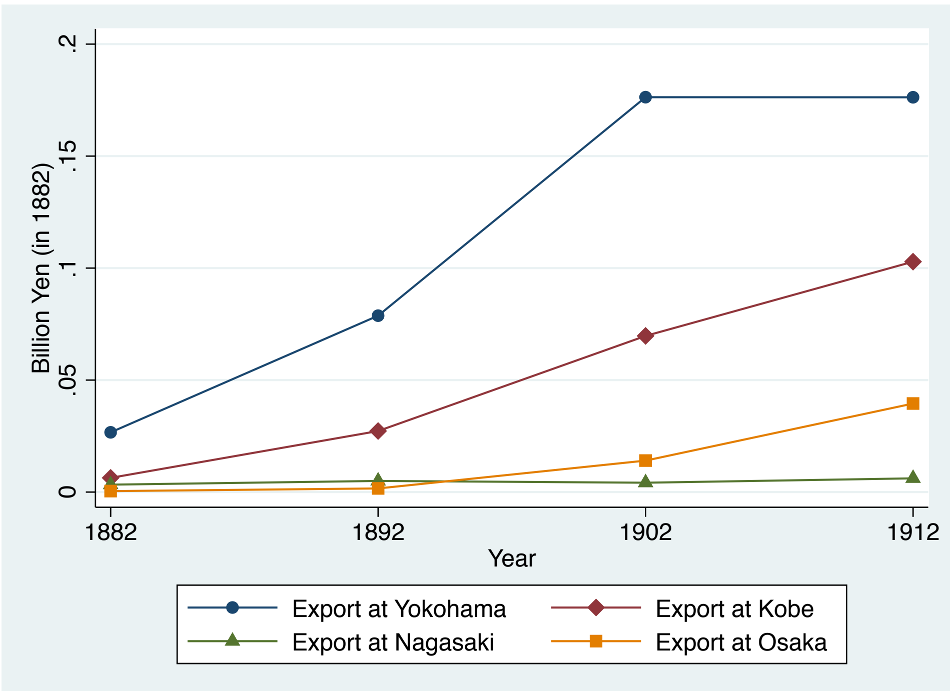
Yokohama and Kobe are located in the Kanto plain and the Osaka plain, where Tokyo and Osaka are located as I show in Figure 3.13. Osaka port is located in the Osaka plain as well, but the port was not suitable for big ships due to its shallow water and narrow width. In 1897, Osaka invested in the port and expanded its capacity, which was the main cause of growth after the 1890s. Nagasaki did not grow, unlike the other three ports, because Nagasaki is not located in a plain as Kobe or Osaka are. It was close to coal production areas in Northern Kyushu island, but Moji, not Nagasaki, was used for its export due to the shorter distance from the mines. In 1902, exports from Moji were twice the exports from Nagasaki, and about 34 percent were for goods from the mining sector, while is only about 12 percent in Nagasaki. Figure 3.14 shows the growth in trade with major trade partners. All three countries, the United States, Great Britain, and China, increased both imports and exports. The goods composition slightly varied over the time. With the United States and Great Britain, Japan imported industrial goods such as machines, chemicals, and textiles, and exported raw silk. With China, Japan exported cotton textiles and imported raw cotton.

Figure 3.11: Trade Volume and GDP Growth, 1882–1912




Source: Umemura et al. (1965) for GDP and deflator, and Ministry of Finance (1882-1928a) for export and import.

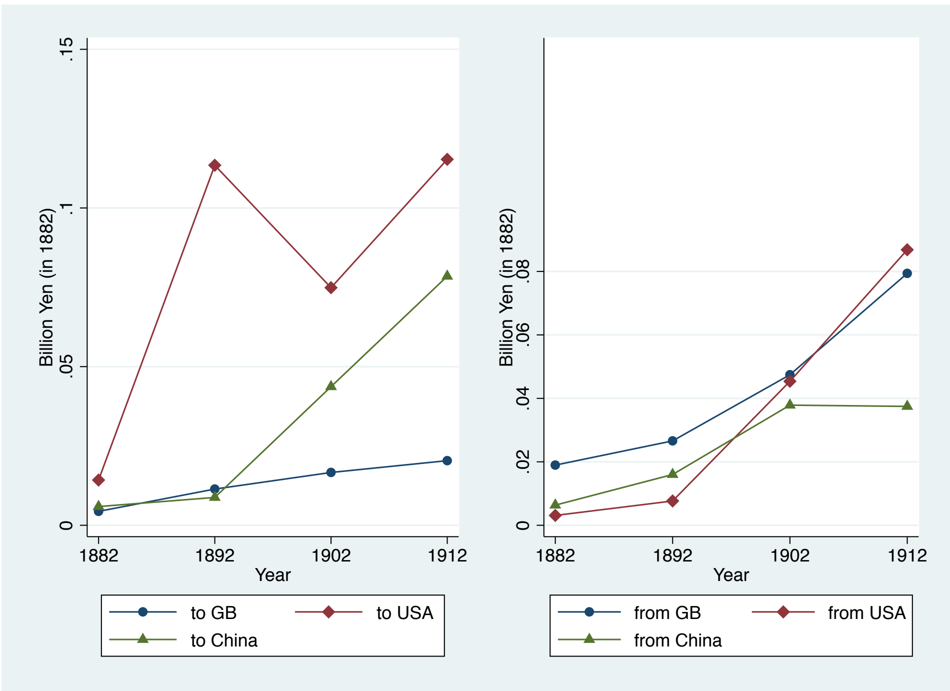
Figure 3.12: Exports at Four Major Ports, 1882–1912




Source: Ministry of Finance (1882-1928a) for exports and imports.

Figure 3.13: Port Locations


Figure 3.14: Trade Partners in 1882–1912




Source: Ministry of Finance (1882-1928a).

### Tax Revenue

Figure 3.15 shows the growth in tax revenue and the annual breakdown. As the figure shows, income tax was introduced in 1887, just a few decades after its introduction in the U.S., Germany, and Italy. It reached the same level as duty tax, which suggests development in fiscal capacity (Besley and Persson, 2013, 2014). We also see that tax revenue was increasing, especially after the Russo-Japanese War in 1904. This was because war expenditures in Russo-Japanese War were about 9 times larger than in the Sino-Japanese War in 1894, and Japan could not obtain reparations in the Russo-Japanese War, unlike after the Sino-Japanese War. The proportion of tax revenue of GDP was higher both after 1904 and in the 1870s–1880s, which is considered to be an effect of the civil war in 1877 and other tax reforms, such as introduction of tobacco and liquor taxes.

Figure 3.16 shows the proportion of each revenue source in total tax revenue. This figure highlights that the share of land tax is quite high, which decreased over time, but remained at over 20 percent in 1910. This is much higher than 2.6 percent, the share in developing countries of wealth and property taxes in 1989 (Burgess and Stern, 1993). Similarly, the shares of duty taxes wsd significantly smaller than its share in developing countries for 1989. Overall, these figures suggest that fiscal capacity in these periods were higher than for more recent data from developing countries.

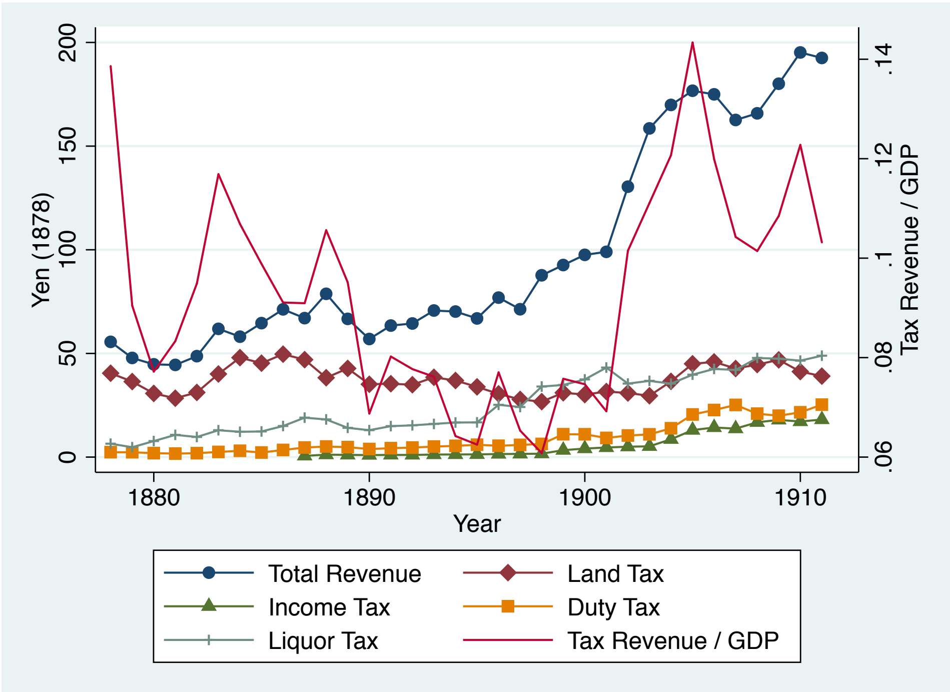
One potential reason for the high proportion of land tax is the traditional institution. Land tax was the dominant tax revenue source throughout the Edo period.[24] Local lords collected tax at the village level (*murauke* system) and typically delegated tax collection work to local village leaders (*shoya*, *nanushi*, or *kimoiri*). Tax duty was under joint liability and it would have strengthened the bonds of the village community, which was mostly unchanged between the Edo period and early Meiji period. The Meiji government used the village community to avoid conflict throughout the land tax reform (Kimura, 2010).

### Education

In 1886, primary education became mandatory, and after 1890, local municipalities were required to have at least one primary school. In 1900, public primary education became free. To see the impact of these policies on schooling rates, in Figure 3.17, I show a box plot of prefecture-level schooling in 1885, 1895, and 1902 using the data from Ministry of Education (1873–1911). I define schooling rate as the number of children attending or graduating from primary schools (*jinjo shogakkou*) divided by the total number of school-aged children. We can see that the schooling rate increased and its variance de-

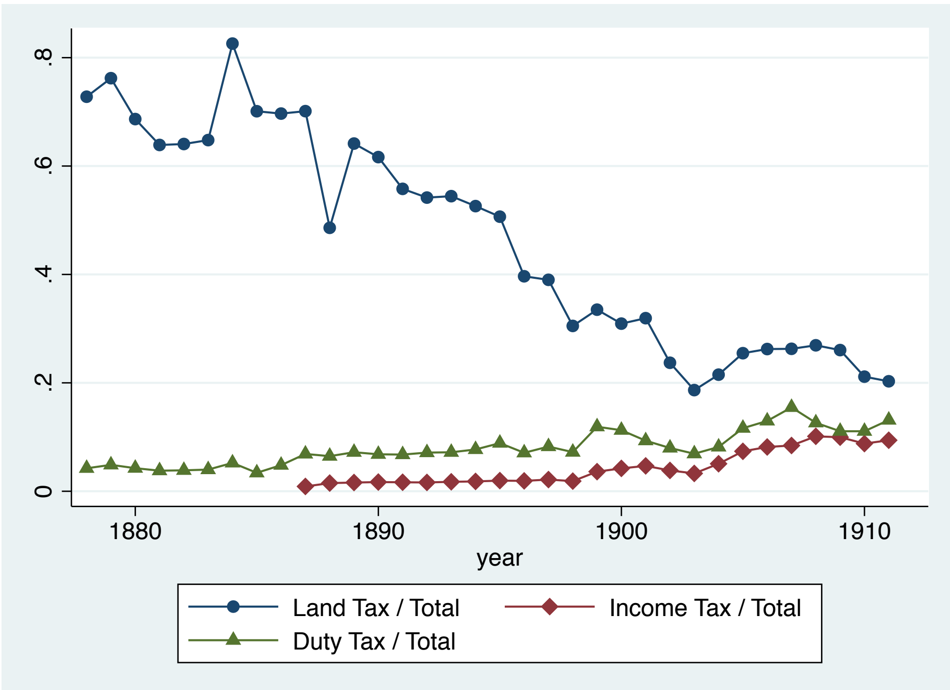
[24]In the Tokyo government domain, tax collection was based on a proportion of output that officials assessed annually in the (*kemi* system). In the early 18th century, they decreased the frequency of investigations to every five or ten years, which resulted in quasi-fixed tax amount (*jomen* system).

Figure 3.15: Tax Revenue, 1878–1912



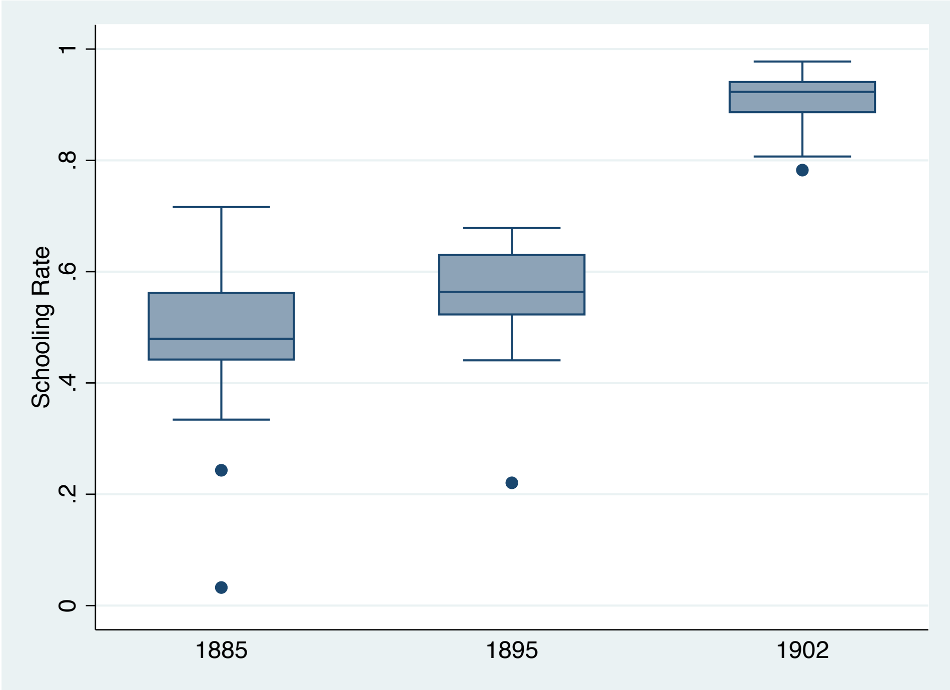

Source: Toyo Keizai Shimpo Sha (1975).

Figure 3.16: Share of Tax Sources, 1878–1912




Source: Toyo Keizai Shimpo Sha (1975).

Figure 3.17: Prefecture-level Primary Schooling Rate 1885–1902




Source: Ministry of Education (1873–1911). Primary schooling rate is the number of children attending or who graduated from primary schools (*jinjo shogakkou*) divided by total school-aged children.

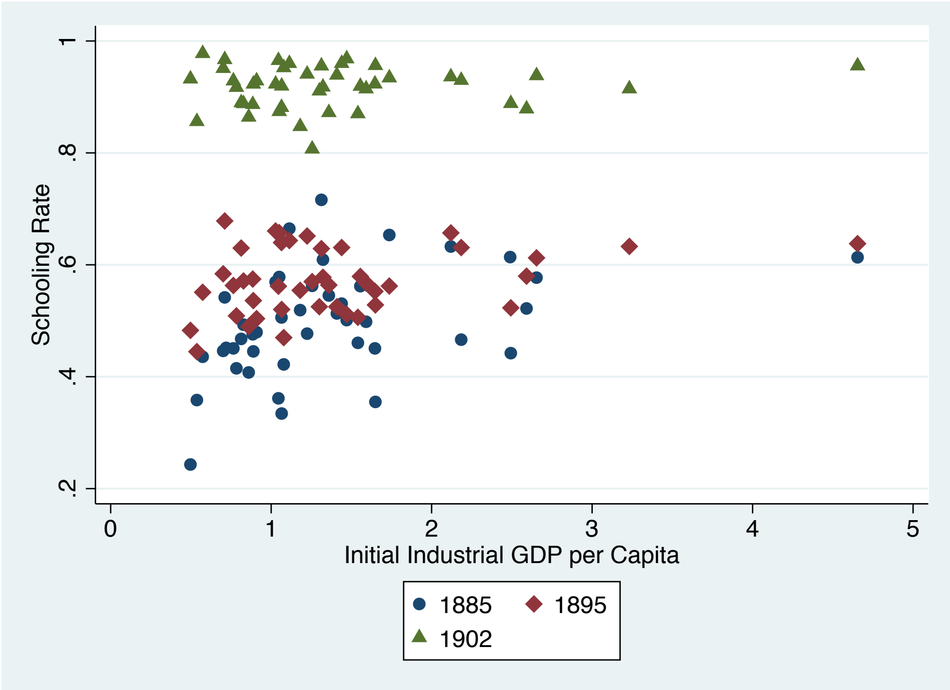
creased over time, suggesting an association between free education and the order to local municipalities and the alleviation of regional inequality in access to education. To investigate this point further, I show the correlation between schooling rate and industrial GDP per capita in the initial period. Figure 3.18 shows the positive correlation in 1885, but this weakens over time until it disappears in 1902. This suggests that the policy changes over the period are associated with the increase in the schooling rate, particularly in less industrialized prefectures, where the demand for education will be lower.[25]

## 3.5 Conclusion

Japan experienced a transition from the pre-modern to early modern period from the 17th to 19th centuries, which provide potentially useful means to address important empirical questions. The Edo period was characterized by its unique mix of feudalism with a central power and a well-known restrictive trade policy. After the Meiji Restoration, the

[25]It is not obvious that this is a policy effect from this data. The income effect is not the reason because I do not find a correlation between the schooling rate and initial agricultural GDP per capita. However, the growth rate in industrial GDP is higher in the less industrialized region between 1874 and 1890, which might increase the demand for education. In this setting, it will be hard to separate the policy effect from other demand side effects, such as a higher demand for education due to industrial growth, which is out of the scope for this chapter.

Figure 3.18: Primary Schooling Rate and Initial Industrial GDP per capita 1885–1902




Source: Ministry of Education (1873–1911), Home Ministry (1994), Institute of Economic Research, Hitotsubashi University (n.d.) and primary schooling rate is defined as in Figure 3.17. Industrial GDP is for 1874 (valued at 1874 Yen) and population (residential) is for 1883.

government pursued centralization and westernization in the public sector. They increased investment in public infrastructure, such as railroads, and changed local governance from feudalism to a bureaucracy. The industrial sector grew during this period by adopting Western technologies.

At the same time, there are rich data for both the Edo and Meiji periods. During the Edo period, the Tokyo government kept records on administration, land investigations, and public projects. The Meiji government began collecting statistical data systematically using their ministries, which covers most of the important statistics, such as population, at least at the prefecture level.

I propose two research directions here. First, it would be interesting to understand the mechanism of fiscal capacity in pre-modern Japan.[26] For example, feudal lords with more frequent transfers or smaller governments may have weak political power and rely on village leaders to enforce the land tax collection. This may result an increase in village capacity. Further, the land tax collection with joint liability at the village level might also affect the culture and norms in the long run.

It is hard to capture village capacity in the Edo period, but year of post office openings

[26]It was higher than pre-modern Chinese fiscal capacity (Sng and Moriguchi, 2014) and heavily based on land tax, as we saw in the previous section.

in the early Meiji period is a good proxy. The Meiji government asked village communities to manage local post offices to establish the national network. In the late Meiji period, village leaders managed about 90 percent of the post offices. Therefore, villages that opened their post offices earlier would have a stronger capacity in the Edo period.

Second, the colonial policies in Korea, Taiwan, and China and their long-term impact should be investigated further.[27] For example, how the Japanese government reacted to the Korean independence movement after WWI would be important. Like the process of democratization due to the threat of revolution (Acemoglu and Robinson, 2001), the Japanese government conceded and changed their policies in Korea to more conciliatory ones. For instance, they provided more primary schools in rural areas (Nagata, 2005). Whether the Japanese colonial government targeted certain areas for public goods provision, such as area with more elites who could organize further independence movements, would be a potential research question.

[27] See Kohli (1994); Booth and Deng (2016) discussing the legacy of the Japanese colonial government in Korea.

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