Essays on Institutions and Development

by

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A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Economics)
in The University of Michigan
2020

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For my grandfather, Depin Xu

ACKNOWLEDGEMENTS

I am fortunate to have benefited from the advice of many outstanding researchers at Michigan. First and foremost, I am indebted to my advisors, Hoyt Bleakley, Ach Adhvaryu, Dean Yang, and Lauren Bergquist, for their continuous guidance and support.

I started meeting with Hoyt when I was working on my third-year paper. It did not take me long to be impressed by his intellect, his acuity, as well as his generosity with his time and support. Every time I went to his office, I left with new ideas, perspectives, and intellectual resources. He always thinks fast. In that way, he has pushed me to think fast (otherwise I would not have been able to stay on the same page with him). He truly cared about my well-being and treated me not just as a student, but also as a friend. He reached out to show genuine concern for my occasional predicaments, even when he had no obligations to do so. For that, I am forever grateful.

The most important lesson I learned from Ach is to think big. I started working with him in my second year. He kept killing my research ideas (softly) and encouraging me to pursue more novel and interesting subject matter. Part of me was motivated by the desire for ideas he couldn't kill. He took a chance on me and invited me to collaborate on an RCT in India, even though I had little idea what an RCT was at that time. He also taught me how to implement ideas, communicate findings, organize papers, and that one can even stay productive while managing three kids at the same time.

I met with Dean in my second-year Development class, where I was exposed to a variety of fascinating topics in Development Economics. The readings and discussions from his class have shaped my thinking and intuition in Economics. Dean supervised my first independent research project and taught me to use rigorous economic methods to address important real-world questions. I also learned GIS modeling skills while working for him as a research assistant, which turned out being instrumental to my dissertation project. He is efficient and genuine with his students. His suggestions are both brilliant and doable.

Lauren has been a role model to me since she joined Michigan. I am inspired by her attitude towards work and her dedication to teaching and research. She is responsible for her students and always responds to my requests promptly. My job market paper has benefited from her insight and critique. She was also an essential source of support when I was on the job market.

The Department of Economics and the Rackham graduate school provided generous financial support during my graduate studies. I am grateful for this and hope one day to be able to give back to the community. I would also like to thank Martha Bailey, John Bound, Charlie Brown, Fang Fang, Anant Nyshadham, Mike Mueller-Smith, Nichole Scholtz, Jeff Smith, Mel Stephens, Ran Tao, Ugo Troiano, Justin Wolfers, and many colleagues and friends for discussion and assistance.

I thank my parents and my sister for their unconditional support. Thanks to my fiancée, Huiling, for her love, company, and entertaining conversations about life and research. Huiling, you make me a better person, and I am fortunate to be able to share the rest of our lives together. Finally, thanks to my grandfather, who passed away many years ago. He taught me the value of education and the power of determination. I hope I have made him proud.

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ABSTRACT

This dissertation explores the importance of formal and informal institutions in determining human capital and economic development. Using detailed, georeferenced data from the Chinese primary land market, Chapter 1 provides evidence that policymakers are motivated by intrinsic preferences for their social group members, and that this in-group preference can, in turn, cause spatial misallocation of public resources and economic activities. Chapter 2 takes advantage of a civil service examination reform in Taiwan as natural experiment, and studies how merit-based recruitment of bureaucrats affects individuals' human capital investment. Chapter 3 evaluates the long-term impacts of China's land property reform on rural populations. It finds that the reform improved later-life health and economic outcomes for individuals who had been exposed early in life. It also finds that the reform reduced human capital investment for those exposed at critical school ages, making them less likely to receive education and more likely to remain in agriculture.

CHAPTER I

In-group Preferences and Land Misallocation in China

I collect georeferenced data on each parcel of land supplied by Chinese city governments for industrial activities, and spatially match them to river networks. Using variation induced by unique city leader appointment rules, I provide evidence that areas *upstream* of city leaders' hometowns receive less land for industrial activities than otherwise similar areas. However, areas *downstream* of leaders' hometowns do not exhibit this pattern. These findings are consistent with policymakers' social preferences for in-group members, suggesting that they are less willing to allocate industrial activities to areas where industrial pollution spills over to residents of their hometowns. Additional evidence suggests that this in-group preference leads to price distortion and lost surplus in the industrial land market.

1.1 Introduction

The efficient allocation of public resources is a key driver of economic development (Alesina et al., 1999). However, public resources are often misallocated and the sources of misallocation are not always apparent (Finan and Mazzocco, 2016; Padró i Miquel, 2007). Since politicians and bureaucrats are usually responsible for these allocation decisions, it is important to explore their incentives and preferences, so as to understand what has prevented them from making efficient resource allocations.

Standard economic models often assume politicians and bureaucrats to be self-interested. An extensive empirical literature has shown that policymakers' choices and behavior are highly driven by career concerns and rent-seeking incentives (Besley and Case, 1995; Besley and Burgess, 2002; Padró i Miquel, 2007; Burgess et al., 2012; Shleifer and Vishny, 1993; Olken and Pande, 2012). However, policymakers have concerns beyond self-interest. Originating from particular social groups (e.g. race, ethnicity, hometown), they may also be intrinsically motivated by their social group identities (Ashraf and Bandiera, 2018; Chen and Li, 2009; Bernhard et al., 2006; Fisman et al., 2019; Shayo and Zussman, 2011).[1] In this study, I present evidence that policymakers can indeed exhibit social preferences towards their own group members, and that the in-group preferences can cause spatial misallocation of resources and economic activities.

The context for this study is the Chinese industrial land market, in which city governments *sell* land to firms for industrial activities.[2] As the statutory owners and the only suppliers of land, city governments can determine the spatial allocation of industrial activities by deciding where in their jurisdictions to supply the land (State Council DRC, 2014). Since industrial activities in China are highly polluting, and industrial pollution produced in upstream areas is transported by rivers to downstream areas and thus harm downstream residents, I hypothesize that city leaders may be less willing to allocate industrial activities to areas upstream of their hometowns, should they care more about the welfare of their hometown residents. For that purpose, they may strategically supply less land for industrial activities to those areas.

To test this hypothesis, I collect detailed georeferenced data on each parcel of industrial land supplied by city governments, and spatially match it to Chinese rivers

[1]A recent review article by Ashraf and Bandiera (2018) summarizes the evidence for social preferences in the workplace. Chen and Li (2009) find laboratory evidence that individuals exhibit social preferences even for those who share group identities induced by random assignment.

[2]As discussed later in the paper, it is the land use rights that are for sale. The government still owns the land.

and city boundaries. My research design takes advantage of China's unique city leader appointment rules: city leaders are prohibited from serving in their home cities due to the "Rule of Avoidance", but are often appointed to jurisdictions proximate to their home cities due to the lack of connections to other provinces.[3] Therefore, in some leaders' jurisdictions, some river segments flow towards their home cities, while other river segments flow towards non-home cities. I focus on the areas surrounding those river segments, and employ an estimation strategy to compare industrial land supplied to areas upstream of leaders' hometowns (which I refer to as "**upstream areas**") to similar areas upstream of non-hometowns ("**control areas**"), before and after city leader turnover.[4] Conceptually, if a city leader exhibits preferences for the residents of her hometown, we would expect to see less industrial land supplied to upstream areas than to control areas while she is in office. Later, when the jurisdiction city is governed by a new leader who is not from any of the downstream cities, all areas will be treated equally and the previously observed differences are expected to disappear. Exploiting variation across areas within the same jurisdiction city, and time series variation induced by leader turnover, the estimation strategy is similar in spirit to a difference-in-difference strategy.

Using georeferenced data on the supply of industrial land from 2004–2015, and biographical data on city leaders in office during the same period, I find that areas upstream of city leaders' hometowns receive significantly less land for industrial activities, at both the extensive and intensive margins. In my preferred specifications, I find that being upstream of city leaders' hometowns reduces the probability of receiving any industrial land in a given year by 5.8 percentage points. The estimated effect is economically meaningful in magnitude, and corresponds to a 15 percent drop at the sample mean. Conditional on receiving land, the annual quantity of land supplied to upstream areas is also 25.5 percent lower. These results are robust to alternative measures of land supply, and to a hyperbolic sine transformation that captures changes at both the extensive and intensive margins. The findings are consistent with policymakers' social preferences for own-group members, suggesting that they are less inclined to allocate industrial activities to areas upstream of residents of their hometowns, so as to protect them from industrial pollution.

I provide additional evidence in support of the above interpretation. In the heterogeneity analysis, I show that the estimated effects on land supply depend on the

[3] Among the 927 city leaders appointed during the sample period, 65% had taken office in their home provinces.

[4] Throughout the paper, hometown is defined as home city (prefecture), which is the level at which the identifying variation is generated.

linguistic distance between leaders' home cities and other cities in their vicinity. Regions that speak unique languages or dialects tend to have been historically more isolated by geography (Michalopoulos, 2012). They have had less interaction with other regions, and have preserved more distinct cultural identities (Alesina et al., 2003; Falck et al., 2012; Guiso et al., 2009). Therefore, people in these regions may have stronger hometown identification. Consistent with prior studies that find individuals to be more biased towards their in-group counterparts when their group identities are salient (Shayo and Zussman, 2011; Fisman et al., 2019; Hjort, 2014), I find that city leaders tend to be more protective of their hometowns, and undersupply industrial land by a larger amount when their hometowns are linguistically distinct from nearby cities. In addition, the estimated effects are greater when leaders' hometowns are more populated, suggesting that leaders have stronger incentives to misallocate industrial activities when more residents of their hometowns are likely to be affected.[5]

Using water pollution data measured at monitor stations and an event-study approach, I demonstrate that industrial construction is indeed strongly associated with increased river pollution. To do so, I spatially match each monitor station to each industrial land parcel within its 20-kilometer radius.[6] Using information on land transaction years, I present evidence that industrial construction projects in upstream areas significantly increase the pollution readings at the monitor stations. I also find that industrial construction projects located downstream of the monitors have no statistically significant effect on pollution readings, suggesting that the estimated pollution effects of upstream construction are not driven by omitted confounders.

To probe the importance of alternative channels through which industrial activities may affect policymakers' hometowns, I conduct a placebo test with a different sample of areas. In the placebo sample, some of the areas are **downstream** of city leaders' hometowns, and others are downstream of non-hometowns. If the main results are indeed driven by policymakers' environmental concerns for their home residents, there should not be any similar effects for areas downstream of leaders'

[5]These findings also suggest that the effects are unlikely to be driven by the interests of a narrowly-defined, small group of families or friends. It bears mentioning that the economic benefits of reduced pollution are minimal, from the perspective of each individual. For instance, an average US household is only willing to pay $78 to improve rivers from fishable to swimmable (Sigman, 2005), an improvement that corresponds roughly to a one third decrease in major water pollutants (grade III to II). Furthermore, river pollution tends to have a minimal effect on housing values. A recent study by Keiser and Shapiro (2018) finds that a 4 percent increase in water quality in the US only leads to a 0.025 percent increase in housing values near the river.

[6]This is consistent with the main analysis, where the unit of observation, area, is defined based on a 20-kilometer radius around each river's exit point from the jurisdiction.

hometowns, since pollution produced in downstream areas does not affect upstream residents. By contrast, if city leaders allocate less industrial activities near their hometowns for economic concerns, for instance to reduce competition for firms in their hometowns, downstream areas should also receive less industrial land. The lack of any effects in the placebo sample is consistent with the dominant role of pollution concerns in driving the main results. An array of additional evidence has also been provided to address several alternative explanations. Although it may be impossible to fully rule out all other interpretations, the empirical findings, taken together, are most consistent with policymakers' intrinsic social preferences for residents of their hometowns.

Given the large effects of leaders' hometown preferences on the spatial allocation of industrial land, it is natural to ask how allocative efficiency and local economic welfare would be affected. To address this question, I examine the effect of hometown preferences on land prices, using the strategy described earlier. I find that the prices of newly-supplied industrial land are significantly higher in upstream areas than in otherwise similar areas. This effect is not driven by different land compositions or different sales methods. Rather, the higher prices in upstream areas are the result of less supply. These findings suggest that under the current allocation scheme, there are firms with higher willingness to pay for land in certain areas. A reallocation of land from low-price to high-price areas would, therefore, increase total market surplus, while holding the total supply and environmental costs constant. However, the city governments are willing to forgo the additional surplus due to leaders' hometown preferences. Using the estimated effects on both supply and price, I estimate the lost market surplus to be roughly 1.6 percent of the average land sale revenue for each area. Using data on nighttime light intensity, I also find that having been upstream of leaders' hometowns over previous years reduces that area's current economic activity.

This paper is related to the empirical work on in-group favoritism in resource allocation settings. Existing studies in this literature have provided evidence that politicians in power tend to distribute public goods in favor of their own-group members (Pande, 2003; Cruz et al., 2017; Burgess et al., 2015; Do et al., 2017; Dreher et al., 2019; Hoffman et al., 2017). In terms of interpretation, it could be that politicians distribute more resources to their own group members because they truly care about their welfare (Broockman, 2013; Do et al., 2017), or because they intend to exchange resources for votes, political support, or economic benefit (Cruz et al., 2017; Hodler and Raschky, 2014). The context of this study allows for a better distinction of social preferences from self-interest as the underlying mechanism. It focuses on a non-electoral regime and a context in which the in-group members are not governed by the politicians under consideration. Therefore, politicians should not be concerned about votes or political support. In addition, it focuses on policymakers' environmental instead of economic concerns, which allows me to better rule out rent-extracting explanations. Using data on linguistic distances, this work also provides new evidence that in-group preferences are stronger when group identity is more salient.

Moreover, standard models of in-group favoritism predict resource misallocation, to the extent that resources are not always allocated to those who value them the most if policymakers trade efficiency for additional utility gains from favoring in-group members (Fisman et al., 2017). As self-evident as it appears, due to the lack of measures for efficiency, there is little direct evidence of misallocation as a result of policymakers' competing preferences. The context of this study differs in that the resources (industrial land) are allocated through market mechanisms. The market-determined prices reveal individuals' willingness to pay for resources, which in turn allow me to provide evidence for, and measure the size of, misallocation costs.

Furthermore, causally identifying in-group preferences is challenging due to omitted variables and reverse causality (Fisman et al., 2017). For instance, the social groups of politicians in power might systematically differ from other groups. Social groups that are more likely to have politicians in office may have different socioeconomic characteristics and different resource demands. This confounds the interpretation of the empirical findings. Leveraging variations induced by unique and plausibly exogenous city leader appointments in China, this study confronts endogeneity problems and provides additional quasi-experimental evidence for in-group preferences among political elites.

This paper also speaks to a sizable body of research analyzing environmental externalities at jurisdiction borders. Studies of this topic have provided evidence that politicians tend to allocate more polluting activities near downstream borders so that the pollution costs will be relegated to downstream neighbors (Sigman, 2002, 2005; Lipscomb and Mobarak, 2016; Cai et al., 2016; Kahn et al., 2015). The present study shows that policymakers indeed consider water pollution externalities at jurisdictional borders when deciding on the spatial allocation of industrial activities. Moreover, policymakers are more likely to internalize pollution externalities when the pollution harms their in-group members. This paper also joins a small set of work that studies the causes and consequences of friction in China's primary land market (Cai et al., 2013; Chen and Kung, 2018; Wang et al., 2016; Fei, 2019). It adds to this line of inquiry by identifying policymakers' in-group preferences as an additional source of land misallocation.

This paper proceeds as follows. In the next section, I discuss the background of the Chinese land market, hometown identity, and city leader appointments. In Section 1.3, I outline a simple conceptual framework to guide the empirical analysis and quantify the deadweight loss. Section 1.4 describes the data and the variable construction. Section 1.5 reports the findings of the empirical analysis. Section 1.6 concludes.

1.2 Context

This section details the study context, focusing on key institutional features relevant to this paper's conceptual framework and identification strategy. It first describes China's land market and its importance to local governments as a source of revenue and as an instrument for interfering in local economies. It then documents the pollution costs of China's industrial development over recent decades, and environmental free riding at jurisdictional downstream borders. Finally, it discusses the role of hometown identity in shaping individual and social outcomes, as well as China's unique city leader appointment system.

1.2.1 Determining the Spatial Allocation of Economic Activity via the Land Supply

Land is a central input in industrial development and economic growth. Unlike in the US and many other economies where most land ownership is private, land in China is owned by the state or by the collectives. Under the Constitution of the People's Republic of China, it is the State Council (the Central government) that exercises ownership rights over urban land on behalf of the state. It also allocates land use rights to individuals and firms for private use.[7] In the 1980s, most land allocation was conducted through executive orders or other non-market mechanisms. A series of reforms that began in that decade, including an amendment to the Constitution in 1988, have promoted market-oriented land allocation.[8] Furthermore, the Land

[7]Rural land is collectively owned by rural households and is allocated within a village or a production team by village committees. In most cases, use rights to farmland are contracted to individual households with tenures of up to 30 years (Central Document No. 11 of 1993). However, the constitution and land laws also afford the government the power to acquire, convert, and supply rural land for industrial or other non-agricultural uses (State Council DRC, 2014).

[8]The amendment clarified the separation of land use rights from land ownership rights and legalized the transaction of land use rights through market mechanisms. Previously, Clause 4, Article 10 of the 1982 Constitution had prohibited any form of land sale or lease (Zhu, 2004).

Management Law of 1989 granted local governments the exclusive power to sell land (use rights) in their jurisdictions (State Council DRC, 2014).

The monopolistic power in land supply is crucial to local governance in China for two reasons. First, it provides an instrument for local governments to interfere in local economies. By determining where to supply the land, local governments directly affect the allocation of economic activity within their jurisdictions. Second, revenues generated from land sales constitute a significant share of local government income. This has especially been the case since the fiscal reform of 1994, which has deprived local governments of a large share of tax revenues (Tao et al., 2010). To compensate, the Central government has delegated administrative control rights over state-owned land to local governments, and classified land sale revenue as "extra-budgetary" revenue that does not need to be shared with the Central government. These institutional arrangements incentivize local governments to seek revenues from the land market, which spurred exponential growth in the urban land supply during the early 2000s. From 1999 to 2013, the annual quantity of land sold by local governments increased by a factor of 8.3, from 45,390 hectares to 374,804 hectares. Meanwhile, annual government revenue from land sales increased from 51.4 billion to 4.4 trillion Chinese *yuan*, a factor of 85. The average share of land sale revenue in total local government revenue was nearly 60 percent in 2009, and 70 percent in 2010 (Fang et al., 2016). Studies of Chinese political economy have found that government revenues are positively correlated with the career advancement of local political leaders. This is likely because higher government revenues signal higher aptitude for local political leaders, and allows them to transfer economic benefits to upper-level governments (Lü and Landry, 2014; Chen and Kung, 2016).

Ninety percent of the land parcels sold by local governments consist of former agricultural land in peri-urban areas (State Council DRC, 2014). After compensating farmers, local governments are allowed to convert the expropriated land into other use-types, either industrial, commercial, or residential. The compensation paid to farmers is low, only corresponding to a small fraction of their property's market value (Wang et al., 2016).[9] The inclination of local governments to oversupply led to a dramatic drop in China's total amount of farmland in the 1990s. In 1998, out of concerns for food security, the Central government set a policy target of protecting a minimum of 120 million hectares of farmland. To maintain that target, a land quota system

[9]According to the Land Management Law, the compensation must not exceed 30 times the average annual value of products generated from the agricultural land over the three years before the conversion.

was initiated in the same year to tie the hands of local governments. The quotas, which cap the amount of each type of land that can be supplied by local governments in a given period, are first assigned to each province by the Central government. The provincial governments in turn allocate these quotas to their prefectural cities (Yu and Shen, 2019). Given the benefits of land sales, it is not surprising that local governments exhaust their quotas (Wang et al., 2016; Xie, 2015). Sometimes local governments even conduct illegal land sales without the approval of the superior government bodies.

A land parcel can be sold through either private negotiation or public bidding. Before 2002, the vast majority of land was sold through private negotiations between local governments and buyers. The lack of transparency in the transaction process allowed local governments discretionary power to set prices, thus creating rent-seeking opportunities from land transactions. In order to combat corruption, in 2002, the Ministry of Land and Resources required land parcels that were zoned for commercial or residential purposes to be transacted through open and competitive bidding or auctions. In 2004, this requirement was expanded to land parcels for industrial uses. While local governments continued using private negotiation as a sales method, after 2004, the vast majority of land was sold through open and competitive bidding or auctions. From 2004–2015, over 80 percent of land sold by the government, in terms of area, was transacted through public auctions, corresponding to nearly 95 percent of the revenue generated from land sales.[10] Furthermore, local governments are required to disclose details about each land transaction on the Land Transaction Monitoring System website, including the size of the parcel, transaction value, sales method, location, use type, and buyer (firm) name, regardless of the method through which the transaction is conducted. This is the main source of the land data used in this paper.

Each land parcel can be sold for one of the following three use types — industrial, commercial, or residential. For the research purposes of this paper, I focus on the supply of industrial land. As is shown in Figure 1.2, the annual share of industrial land in the total land supply remained over 50 percent from 2003–2017. The use-type for each land parcel is determined and publicized on the Internet before the transaction,

[10] The prevailing competitive method of selling land is listing auction, which accounted for about 70 percent of the total amount of land transacted between 2004 and 2015. The other two competitive sales methods are invited bidding and English auction, which accounted for 2 percent and 8 percent of the total amount of land transacted during the same period, respectively. The listing auction is a "two-stage" auction where potential buyers first compete through bidding in a given time window (about 10 business days). If there is more than one active bidder at the end of the time window, they compete in a standard English auction (Cai et al., 2013).

along with other details on the land parcel. Buyers must use the purchased land in accordance with the specified use-type. For example, an industrial land parcel cannot be used for commercial or residential purposes.[11] Additionally, the contract must specify the projects (i.e. structures, facilities) for which the purchased land will be used. In order to prevent land speculation, firms and individual buyers are prohibited from transferring the use rights to other parties before the specified project is completed according to the contract (State Council DRC, 2014). The contract also specifies the length of leasehold for each land parcel. The tenure is usually 50 years for industrial land, 40 years for commercial land, and 70 years for residential land. These leaseholds are renewable upon expiration. There are few cases in which local governments can buy the land back before the expiration date.

The land laws do allow land transactions between private buyers, i.e. buyers who have purchased land from the government can sell it to other buyers for the remaining years of tenure. However, the secondary market remains thin due to government restrictions on private land transfers. For instance, as discussed earlier, the buyers have to specify the construction project for which the land parcel will be used, and they are prohibited from transferring the land before they have completed the project in accordance with the contract (State Council DRC, 2014). These restrictions have reduced the transferability of industrial land between private buyers, since industrial structures and fixtures sitting on the land have to be sold with the land.[12] According to data from the Land Monitoring System, during the sample period of 2004–2015, industrial land transactions on the secondary market were only 5.3 percent of those on the primary market.[13]

1.2.2 Inter-Jurisdictional Water Pollution Spillover

This paper focuses on land supplied for industrial activities. Industrial activities are the most significant source of water pollution in China, accounting for 40 percent of the total amount of wastewater discharged from 2004 to 2015.[14] Since industrial wastewater is more concentrated and toxic, the overall environmental damage caused

[11]In a few situations, with government approval, and after paying the difference between the original and the would-be price, buyers may change the use type of the land they have purchased (State Council DRC, 2014).

[12]See Land Management Law, Interim Provisions on the Transfers of Urban Land Use Rights, Urban Real Estate Management Law for more details on these restrictions.

[13]Residential land transactions are more frequent, accounting for 85 percent of all transactions in the secondary market. Secondary transactions of industrial land, in contrast, only account for 3 percent.

[14]China Statistical Yearbook on the Environment 2017.

by industrial activities exceeds that of agricultural and domestic sources (Cai et al., 2016; Ebenstein, 2012).

Given the polluting effects of industrial activities and China's rapid industrialization over the last three decades, it is not surprising that water pollution has become a pressing social issue. Industrial wastewater dumping by manufacturing firms has rendered the water of many rivers unfit for human consumption. According to a report by the World Bank in 2006, over 70 percent of China's monitored river segments had water categorized as undrinkable during that year (World Bank, 2006). At the same time, a large proportion of the country's population, including 115 million rural people, still rely on surface water as their main source of drinking water (Ebenstein, 2012). Therefore, industrial expansion in China has been associated with increased public health burdens, infant mortality, diarrhea, and cancers of the digestive tract (Ebenstein, 2012; He and Perloff, 2016). The media has frequently reported incidents of contaminated water from industrial activities leading to cancer outbreaks in rural villages.[15] It has been reported that the cancer rates in villages along the Huai River, a highly polluted waterway in Central China, are 50 percent higher than in the rest of the country (The Worldcrunch, 2017).[16] Total economic losses due to water pollution are estimated to be around 150 billion Chinese *yuan* per year (Cai et al., 2016).

The enforcement of pollution controls and environmental regulations is decentralized to local governments, who are aware of the huge costs of industrial pollution. However, since local governments rely on industrial firms to generate revenue, they lack the incentive to comply with regulatory efforts.[17] Rather, local governments are inclined to environmentally free ride on their downstream neighbors by allocating a disproportionate amount of industrial activity to their jurisdictions' downstream borders (Cai et al., 2013; Kahn et al., 2015). In this way, local governments can shirk the effects of water pollution to downstream jurisdictions. It has been shown that areas with little industrial activity of their own, but that are downstream of major industrial zones, also suffer from water quality degradation (Ebenstein, 2012; Economy, 2010). As a result, there have been a myriad of pollution-related disputes between upstream and downstream jurisdictions (Kahn et al., 2015).[18] These free-

[15] David McKenzie, "In China, 'cancer villages' a reality of life," CNN (May 29, 2013), https://www.cnn.com/china-cancer-villages/index; "China acknowledges cancer villages," BBC (February 22, 2013), https://www.bbc.com/news/china-cancer-villages.

[16] Julie Zaugg, "China's Polluted Rivers Yield Cancer Villages," Worldcrunch (August 23, 2017), https://www.worldcrunch.com/china-polluted-river.

[17] The enforcement of environmental regulations is delegated to the local Bureau of Environmental Protection.

[18] China is not alone in having inter-jurisdictional conflicts over water use and management. Dis-

riding problems have become even more frequent since the Central government has increased efforts to improve water quality and tied local officials' career advancement to pollution reduction and environmental performance (Cai et al., 2016; He et al., 2018). When environmental protection, including water quality, has been a criterion for assessing and promoting local officials, the officials have become more incentivized to conduct strategic allocation of polluting activities in order to free ride on neighboring jurisdictions.

In light of the above discussion, this paper focuses on the allocation of industrial activities at a city's most downstream borders. Because the spatial allocation of industrial land determines where in their jurisdiction industrial plants and facilities can be located, and who the industrial pollution will affect, there may be less land allocated to areas where pollution harms city leaders' hometowns.

1.2.3 Hometown Identity and China's Unique City Leader Assignment Process

Like many other countries, hometown identify is salient social group identity used for individual identification in China. In this paper, I define hometown as an individual's city (prefecture) of birth. Because China's Household Registration System (*hukou*) prohibited internal migration across cities until the late 1980s, an individual's birth city, in most cases, was also the place where she grew up, and the patrilineal hometown where her father and grandfather had lived their entire lives.[19]

In a diverse environment, individuals from the same regions are likely to congregate and bond. They speak common dialects, share similar norms and values, and feel psychologically proximate to each other (Wang, 2016). For instance, ethnic Chinese communities abroad often preserve their Chinese cultural identity centered on shared hometown roots (Douw et al., 1999). It has also been documented, both academically and anecdotally, that, individuals in high places tend to provide preferential treatment to those who share their hometowns. For instance, it has been shown that sharing hometown identity with superiors assists subordinates with career advancement in Chinese bureaucracy (Shih et al., 2012; Opper and Brehm, 2007), military (Wang, 2016), and academia (Fisman et al., 2018; Li, 2018). This evidence suggests

putes and conflicts are common in many other countries. See Lipscomb and Mobarak (2016) for examples.

[19]China's *hukou* system has a history of over 2000 years, during which it has been used to monitor population flows. Individuals are required to register at their place of birth. Their access to public goods, including eligibility for public jobs, education, and social security benefits, is tied to their places of registration. See Chan and Zhang (1999) and Kinnan et al. (2018) for details on China's *hukou* system.

that hometown identities are indeed meaningful to individuals, and play an important role in shaping individuals' behavior. This evidence may imply individuals' intrinsic social preferences for those with shared hometown identities. Preferential treatment can also be provided in exchange for bribes or future returns from trusted in-group members.

The leaders of local governments in China are appointed by upper-level government officials. Under China's single-party regime, there is little distinction between bureaucrats and politicians. Most government positions, especially those of leaders, must be filled by members of the Chinese Communist Party.[20] Leaders who are assigned to their hometowns may prioritize local interests over national interests (Xu et al., 2018). They are also more likely to be "captured" by local elites (Persson and Zhuravskaya, 2016). In order to reduce the opportunities for corruption and nepotism in Chinese politics and bureaucracy, in the late 1990s, the Central Committee of the Chinese Communist Party and the State Council introduced a so-called "rule of avoidance" policy to regulate the appointment of local leaders.[21] This policy prohibits native politicians and officials from filling the highest positions in city Party committees and city governments.[22]

Meanwhile, most city leaders begin their careers in their home provinces and remain there most of their careers. Due to the lack of connections to other provinces or the Central government, most city leaders are selected from, and appointed within, their home provinces. For instance, among the 927 city leaders appointed from 2004-2015 in my sample, 65 percent had been allocated within the their home provinces.[23] This has given rise to the 20 percent of leaders who happen to govern a jurisdiction city with river segments flowing towards their home cities. Allocation of industrial activities close to these rivers will increase pollution and deteriorate water quality for residents of leaders' hometowns, and thus negatively affect their welfare. Industrial activities allocated close to other river segments within the same jurisdiction, however, will not have such effects. China's rich river networks provide geological variation across different areas within the same city. In addition, each city leader is in office for

[20] This is similar to the Vietnamese context described in Do et al. (2017).

[21] The rule has been codified by the 2002 "Regulations on the Selection and Appointment of Leading Party and Government Cadres", and the 2006 "Interim Provisions on the Offices for Party Leaders and Government Cadres to Avoid".

[22] A similar rule was adopted in Imperial China, which prevented district magistrates from serving in their home districts (Ebrey and Smith, 2016; Xu et al., 2018).

[23] A small fraction of appointments (4.2 percent) violate the rule of avoidance, where city leaders are native to their jurisdictions. Many of these "defiers" come from ethnic minority cities (i.e., autonomous prefectures), which enjoy more discretion over political appointments. I return to this issue in Section 1.5.

only 4 years on average. The high city leader turnover induces time-series variation that allows comparison of the same area across years. Since the appointment is made by either the provincial or the Central government, and city leaders themselves have little discretionary power over where they are assigned. Therefore, self-selection concerns are minimal. I return to this point in Section 1.5 and provide evidence that city leader appointments are conditionally exogenous.

1.3 Conceptual Framework

Based on the institutional features discussed above, I outline a simple framework to conceptualize the potential effect of policymakers' hometown preferences on the supply and prices of industrial land in different areas of a jurisdiction city.

Consider a city that consists of $1 + N$ areas, where area 0 is upstream of the city leader's hometown (i.e. an upstream area), and areas $j = 1, 2, ..., N$ are upstream of non-hometowns (i.e. control areas) in a given year t. Each area is a distinct market for industrial land. In each year, new business opportunities emerge at random in each area j and determine the demand for land in that area.[24] The inverse demand function for industrial land in area j and year t is given by $P_{jt} = a_{jt} - Q_{jt}$, where a_{jt} is firms' highest willingness to pay for one unit of land in area j and year t, and follows a normal distribution $a_{jt} \sim \mathcal{N}(\overline{a}_t, \sigma_t^2)$. In other words, the model assumes that firms are willing to pay higher prices for land in some areas because they expect higher rates of return on industrial investment from those areas. In this regard, upstream areas are similar to control areas on average. Q_{jt} denotes the quantity of new land sold by the city government in area j and year t. Q_{jt} is weakly positive since the government can not retake the land in the short term.

The city government is the monopolistic supplier of land in the jurisdiction, and the leader is the decision maker who cares about both land sale revenues in her jurisdiction, and the welfare of residents in her hometown. The marginal (social) cost of land is constant and denoted by c, which could be the sum of financial and environmental costs associated with each unit of land developed. Increased industrial pollution for home residents leads to a loss of utility for the city leader. An additional unit of land supplied for industrial activities to the upstream area causes a disutility of μ for the leader. The leader chooses the quantity of land to be supplied to each area in each year to maximize the following objective function. The total quantity of land supplied in a given year is subject to a quota, L_t, set by the provincial or

[24]Land input is assumed to be necessary for capitalizing on new investment opportunities.

Central government.

$$\max_{\{Q_{jt}\}} \quad \sum_{j=0}^{N}(a_{jt} - Q_{jt}) \times Q_{jt} - \mu Q_{0t} - c\sum_{j=0}^{N} Q_{jt}$$
$$\text{s.t.} \qquad \sum_{j=0}^{N} Q_{jt} = L_t$$

This model is static for two reasons. First, the average term of a city leader is less than four years. Therefore, she is unlikely to consider the long-term consequences of land sales when making supply decisions.[25] Second, as discussed earlier, the secondary market for industrial land is thin, equaling only 5 percent of the transactions in the primary market. Thus, prices of the newly supplied land do not rely on past supplies. Hereafter, the subscript t is omitted for clarity. The Lagrange equation for the leader's maximization problem can be expressed as follows, where λ represents the shadow value of industrial land.

$$\mathcal{L} = \sum_{j=0}^{N}(a_j - Q_j - c)Q_j - \mu Q_0 - \lambda(L - \sum_{j=0}^{N} Q_j)$$

An immediate prediction of this model is that the supply of industrial land is equal across all areas when the leader is not from any downstream cities, i.e. no upstream areas ($\mu = 0$). This is because demand is downward sloping and an equal supply for similar areas will allow the government to sell to firms with the highest willingness to pay and thus maximize the revenue. When some areas become upstream of leaders' hometowns ($\mu > 0$), the model predicts a decrease in land supply to those areas and an increase in supply to other areas (control areas). In this case, leaders trade off revenue for additional utility from favoring people in their hometowns. Since the allocation of industrial activities to areas upstream of hometowns incurs utility costs to the leaders, they are less willing to do so. Thus, they strategically allocate industrial activities to other areas in their jurisdictions.

Due to these supply changes, land prices are expected to fall in upstream areas and rise in control areas, leading to an allocative inefficiency. Misallocation arises to the extent that land is not always allocated to those who value it the most. Since land prices are higher in some areas than others, there are firms who are willing to pay more for land in those locations. This is likely because the firms find it more

[25] Increased supply of industrial land will increase future tax revenue (Tao et al., 2010). However, adding tax revenues to the objective function does not qualitatively alter the predictions.

profitable to operate there. However, due to the undersupply, the firms have to forgo their preferred land, leading to a loss of productivity and/or profit. From the city government's perspective, this can create larger market surplus and generate larger revenues if it supplies more to high-priced areas and less to low-priced areas, while holding the total supply constant. However, the governments forgo additional market surplus so as to favor leaders' hometowns. In later analysis, I develop an estimation strategy to identify the supply and price differentials between upstream and control areas. These estimates, in turn, allow for the quantification of the lost surplus in the industrial land market due to leaders' hometown preferences.

1.4 Data

To evaluate the effects of hometown bias on the industrial land market and investigate potential mechanisms, I assemble a comprehensive dataset from a variety of sources. This dataset contains information on land supply, city leaders, river networks, water pollution, and geographic, climatic, and economic characteristics at granular levels.

Land Supply

I collected data on each parcel of land supplied in the primary market from 2004–2015 by scraping the Land Transaction Monitoring System website.[26] Local governments are required to publicize details about each land transaction they conduct. Figure 1.5 presents an example of the transaction record obtained from the system. The transaction records include information on the size of the parcel, transaction value, sales method (i.e. whether the land was sold through negotiation or public bidding), use-type, quality grade evaluated based on the landscape and access to infrastructure and markets, transaction date, name and industry of the buyer (firm), the construction project for which the purchased land will be used, and the start date and completion date for construction. Of note, the transaction records contain detailed text based addresses of the land parcels, which enables me to obtain their geographic coordinates. I geocode all land parcels using Baidu Map's Application Programming Interface, the Chinese equivalent of Google Maps.[27]

The original data contain 1.23 million land transaction records.[28] 33 percent of the

[26] http://www.landchina.com

[27] http://lbsyun.baidu.com/products/map

[28] 0.43 million land parcels are appropriated by the government for public, military, or other special uses. These parcels are not included in the sample.

parcels are sold for industrial projects, 48 percent for commercial use, and 19 percent for residential use. I first drop a small fraction of parcels (4.4 percent) for which the size, price, and/or location are missing. To account for abnormal values likely due to data entry error, I then trim the data by winsorizing the price and size at the .5th and 99.5th percentile, separately for each use-type.[29] The average price of industrial land is 199 *yuan* per square meter, lower than that of residential and commercial land (960 and 1,515 *yuan* per square meter, respectively). The average size of industrial land parcels is 3.6 hectares, larger than that of residential and commercial land parcels (1.4 and 1.5 hectares, respectively). 74.8 percent of the industrial land parcels are sold through public bidding. These parcels account for 79.8 percent of the supply in terms of size, and 86.9 percent in terms of transaction value.

River Networks

I spatially match the georeferenced data on land supply with data on Chinese rivers and city boundaries. Data on China's river network come from the Ministry of Water Resources. I focus on over 800 named rivers that cross prefectural cities' borders. Data on city boundaries are obtained from the China Data Center at the University of Michigan. In addition, elevation data are used to identify the direction of water flow. The original elevation data from the Shuttle Radar Topography Mission (SRTM) have a series of anomalies that may cause errors in hydrological applications (Lehner et al., 2008). Therefore, I use the hydrologically conditioned elevation data provided by the HydroSHEDS Project.[30] This elevation data is available at 3 arc-second resolution (approximately 90 meters).

Local governments are sensitized to water pollution spillover, and are inclined to allocate disproportionately more polluting activities near downstream borders to relegate the costs of pollution to their neighbors (Cai et al., 2013; Lipscomb and Mobarak, 2016; Kahn et al., 2015). For this reason, this study focuses on the allocation of industrial land among the most downstream areas within each city. I use GIS modelling to identify each river's exit point from a city, and define a semi-circle based on a 20-kilometer radius around each exit point. These areas are illustrated in Figure 1.1. By design, the defined areas are similar to each other in terms of size, and hydrological and geographical conditions. They constitute the units of analysis in this study. River segments shared by cities as boundaries are excluded.

I overlay the geocoded land parcels on city and river maps and only keep those

[29] The empirical results in this study are not sensitive to the trim, however.

[30] https://hydrosheds.cr.usgs.gov/

parcels that lie within the sample areas. There are a total of 95,170 parcels of industrial land located within the sample areas. For each area, I use the spatially matched land parcels to compute the total number, total size (in hectares), and average price of land supplied in a given year. These variables are the key outcomes of interest. Summary statistics on land outcomes, at both the parcel and area levels, are reported in Panels A and B of Table 1.1, respectively.

Biographical Data

Biographical data on city leaders (i.e. party secretaries) are manually collected from several sources. First, I compiled a list of city party secretaries in office during the sample period, based on provincial and city yearbooks, government websites, and publications of the provincial Organization Departments. I then collected biographical information on each party secretary through a combination of yearbooks and listings on Baidu Baike (China's Wikipedia). These sources are supplemented by name searches via Baidu (China's Google), and existing databases of Chinese political elites, including the China Vitae database maintained by the Carnegie Endowment for International Peace, and the Political Elites of the Communist Party of China database maintained by National Chengchi University in Taiwan. I collect information on each leader's basic demographics (gender, ethnicity, birth year, city of birth), educational background, and career trajectory. Wherever possible, this information is cross-checked.

In total, I collected biographical data on 1,306 leaders who had served in 305 prefectural cities from 1998–2015. Summary statistics for the sample of city leaders are reported in Panel C of Table 1.1. Females are underrepresented in Chinese city leadership; during the sample period, 96 percent of the city leaders were male. 91 percent were ethnic *Han*, and 60 percent had master's degrees or above. On average, they were first appointed as city leaders at the age of around 50, with about 26 years of tenure in the Communist Party. Each leader had served, on average, in 1.16 cities between 1998 and 2015. Each term in office lasted for about 4 years. For the main analysis, I use data on 977 city leaders in office between 2004 and 2015. Data from 1998–2013 are used for the analysis of nighttime light intensity.

Water Pollution

Data on water pollution measured at 499 monitoring stations are obtained from the China Environmental Yearbooks published between 2004 and 2010. The monitoring stations are located in 10 of China's major river basins, and they report 7 water pollutant indicators—chemical oxygen demand (COD), biochemical oxygen demand (BOD), ammonia nitrogen (NH), petroleum, phenol, mercury, and lead. Each of these water pollutants can travel reasonably far downstream and cause significant spillover effects (Kahn et al., 2015). The number of monitoring stations varied slightly from year to year in the data, resulting in an unbalanced panel of 3,391 individual observations. Summary statistics on water pollution readings are reported in Panel D of Table 1.1.

Nighttime Light Intensity

Data on nighttime light intensity comes from the Defense Meteorological Satellite Program (DMSP). These data are constructed as annual averages of daily satellite images of the earth taken between 20:30 and 22:00 local time. This data is available at a 30 arc-second resolution (approximately 1 km^2). The raw data on nighttime light intensity for each pixel ranges from 0 to 63. I use data from 1998–2013, and average over pixels within each defined area and year. Because the distribution of the average nighttime light intensity is right-skewed, and some observations have no reported nighttime light, I follow previous studies in using the logarithm of average nighttime light intensity plus .01 as a proxy for economic activity (Hodler and Raschky, 2014; Michalopoulos and Papaioannou, 2013, 2014).

Controls, Placebo Outcomes, and Mediator Variables

Climate Data. The georeferenced data on rainfall and temperature are taken from the standard Willmott and Matsuura series available at the University of Delaware. The raw data are measured at a 0.5 by 0.5 degree grid level (approximately 1 km^2), where the grid nodes are centered on the 0.25 degree. I merge the climate data to my sample areas by taking the average over grid points within each area. For each area, I then use the monthly data to calculate the annual averages and standard deviations of both rainfall and temperature.

Dialect Data. I use linguistic distances to measure the extent to which a leader's home city is culturally distinct from its nearby cities. Data on linguistic distances between two given Chinese cities are drawn from Liu et al. (2015). This measure ranges from 0 to 3, where 0 indicates that the two cities speak the same dialect. I calculate the linguistic distance between a city and its nearby cities by taking a simple average of the distances between that city and each of its nearby cities. Doing so ensures that no city has an average linguistic distance equal to 0.

Economic Data. Additional socioeconomic data, including data on population, GDP, and education are obtained from the Chinese Prefectural City Statistical Yearbooks and the 2000 wave of the Chinese Population Census. To account for the proliferation of the national highway system during the sample period (Cai et al., 2013), I calculate the distance between each sample area and the nearest highway using maps of national highways. To measure the stringency of environmental regulations, I collect data on emission fees paid by firms from the 2004 Annual Survey of Industrial Firms (ASIF). This survey also contains address information for sampled firms. I geocode these firms and match them to my sample areas following the procedures described earlier.

1.5 Empirical Analysis

1.5.1 Model Specification

I begin by examining how city leaders' hometown preferences affect industrial land allocation across different areas within their jurisdictions. As illustrated in Figure 1.1, my estimation strategy compares the quantity of industrial land supplied to areas upstream of city leaders' hometowns (upstream areas) with that supplied to areas within the same city but upstream of non-hometowns (control areas). Conceptually, if city leaders care about the environmental welfare and public health of people in their hometowns more than people elsewhere, there will be less industrial land supplied to upstream areas than to control areas. Due to city leader turnover, I can also compare the same area in years when it is upstream to years when it is control. The identification can be formalized by the following specification:

$$Y_{ijt} = \alpha + \beta \times \text{UpHome}_{ijt} + \gamma \times X_{ijt} + \lambda_i + \mu_{jt} + \epsilon_{ijt} \tag{1.1}$$

Y_{ijt} is the total supply of industrial land to area i of city j in year t. I explore the extensive and intensive margins to examine how leaders' hometown preferences affect the probability that an area will receive industrial land, and conditional on receiving land, how they affect the amount allocated. Therefore, Y_{ijt} can be a dummy equal to one if there is any supply, or the (log) total amount of supply, in area i and year t.

As detailed in Section 1.4, the unit of analysis, area, is defined based on a radius of 20 kilometers around each river's exit point from the jurisdiction city (Figure 1.1).

I focus on areas around rivers' exit points out of the city for two reasons. First, these areas are comparable in terms of geographical, hydrological, and other characteristics. For instance, by design, the defined areas are of similar size, are close to borders, and contain a river segment. The key difference is that some areas contain a river that flows downstream to city leaders' hometowns, while others contain a river that flows elsewhere – thus pollution produced there is less harmful to residents of the city leaders' hometowns. The comparison between geographically and hydrologically similar areas will attenuate concerns over varying trends across areas.

Second, a large body of research in environmental economics has found that water pollution increases as a river flows towards a jurisdiction's exit point, primarily because local governments allocate disproportionately more production and polluting activities near river's downstream exit point, so that the pollution costs will be relegated to downstream neighbors (Lipscomb and Mobarak, 2016; Cai et al., 2016; Kahn et al., 2015; Sigman, 2005).[31] These studies suggest that policymakers indeed consider water pollution externalities across downstream borders when making locational choices for polluting activities. My focus on the downstream border areas will therefore, facilitate testing for policymakers' environmental concerns for their hometowns, should there be any.

The key explanatory variable, UpHome_{ijt}, takes the value of 1 if area i is upstream of the hometown of the leader of city c in year t, and 0 otherwise. An area that is upstream of the city leader's hometown, i.e. an upstream area, is defined as an area that contains a river that flows to the leader's home city. In my main analysis, in order to maximize the number of "switchers" that experience changes in upstream indicators, I do not require the upstream area to be adjacent to the leader's hometown. However, the results are largely unaffected when an upstream area is required to be immediately upstream of leaders' hometowns (Table 1.24).

All regression models include area fixed effects, λ_i, to absorb any time-invariant determinants of land outcomes in each area. I further control for city $\times$ year fixed effects, μ_{jt}, to account for time-variant shocks common to all areas in the same city. With the inclusion of the city $\times$ year fixed effects, the identification will come exclusively from variation across areas within the same city and year. In a subset of

[31] Sigman (2005) shows that the decentralization of environmental authority to a US state is followed by 4 percent water quality degradation in the downstream state. Lipscomb and Mobarak (2016) provide evidence that county governments in Brazil allow more settlements to develop close to rivers in the downstream portions of counties. Cai et al. (2013) find that provincial governments in China allocate 20 percent more water polluting activities to the most downstream counties in response to the pollution reduction mandates imposed by the Central government in 2001.

specifications, I control for other time-varying characteristics at the area level, including weather shocks (averages of and standard deviations in rainfall and temperature), access to highways, and cumulative supply of land up to the previous year. Standard errors are all clustered at the area level. In additional analysis, I also show that the results are robust to alternative clustering strategies, including clustering at the city level and two-way clustering by city and year (Table 1.25).

Validity of Identification Strategy

The identifying assumption of the estimation strategy is that city leader assignment is conditionally exogenous. In particular, leaders who are assigned to jurisdictions upstream of their hometowns, i.e., those who manage river segments flowing towards their hometowns, should not systematically differ from the others. For instance, if those who are assigned upstream tend to have stronger hometown preferences (due to self-selection), the estimated effects will be overstated. Furthermore, if their hometowns are more populous, or have residents who are more vulnerable to water pollution, they will allocate less polluting activities to areas upstream of their hometowns, even if they care about each downstream resident equally.

The conditional exogeneity assumption is likely to be satisfied for several reasons. The appointments are determined by either the provincial or the Central government. This is a process during which city leaders have little discretion or bargaining power. Moreover, the appointment of a politician to a city also depends on a multitude of random factors, such as whether a position in that city is vacant, or whether the politician has network connections to other politicians in that city. Therefore, those who happened to be assigned to cities upstream of their hometowns may not be systematically different from those who are not. To validate this identifying assumption, I conduct balance checks on a large set of city leaders' personal and hometown characteristics measured in the year of appointment. The results are presented in Table 1.12. During the sample period of the main analysis, there were a total 927 appointments, of which 189 (20.3%) were placed upstream of their hometowns.[32] Each coefficient in the table represents the difference in a balance variable between these city leaders and the others. The estimated differences and corresponding p-values, with and without year of appointment fixed effects, are reported in Columns 1 and 2, respectively.

Overall, there is no evidence that city leader appointments hinge on the up- and down-stream relationships between leaders' jurisdictions and their hometowns. Those

[32] There are a total of 977 city party secretaries in my sample. Some of the appointments were made before the sample period.

who are placed upstream of their hometowns appear similar to those who are not, in terms of demographics (age, gender, ethnicity), education, political seniority (years of tenure in the Party, political rank, work experience). Their hometowns do not differ in terms of economic performance, population, or industrial structure.

1.5.2 Main Results

Table 1.3 presents the regression results from Equation 1.1. Column 1 explores the extensive margin to examine how leaders' hometown preferences affect the probability that an area will be allocated any industrial land. Consistent with the model predictions, the estimated coefficient suggests that compared to similar control areas of the city, areas upstream of leaders' hometowns are on average, 5.9 percentage points less likely to receive industrial land. This corresponds to a 15.5% change at the mean probability. In Column 2, I use the log total quantity of supply measured in hectares to explore the intensive margin effects of hometown preferences on land supply. Due to the log transformation, this variable is defined only for areas where there is a positive supply in a given year. The point estimates show that being upstream of the city leader's hometown decreases the quantity of industrial land supplied to an area by roughly 25.5 percent ($e^{-0.294} - 1$). Both coefficients are statistically significant at the 5% level.

In order to have a dependent variable that captures changes in land supply at both the extensive and intensive margins, I transform annual quantity of land supplied to each area using the inverse hyperbolic sine function. This allows me to include zeros for areas that do not receive any land.[33]. Given the significant and negative effects presented in Columns 1 and 2, it is unsurprising that the coefficient in Column 3 is large and has a negative sign, with a p-value below 0.01. The point estimate related to an alternative measure of supply – the total number of land parcels supplied to each area – is consistently negative and statistically different from zero (Column 4).[34]

Panel B of Table 1.3 presents corresponding estimates from Equation 1.1 after including an additional set of controls. To account for extreme weather shocks that may (directly or indirectly) have influenced local demand for land, I add the annual rainfall and temperature levels, as well as their monthly standard deviations, to each regression.[35] During the sample period, China experienced a drastic expansion in

[33] $\text{sine}^{-1}(y_{ijt}) = \ln(y_{ijt} + (1 + y_{ijt}^2)^{1/2})$

[34] The results are also robust when using levels (total size, total number of land parcels), instead of inverse hyperbolic sine transformation, as the dependent variables.

[35] For instance, weather shocks may affect agricultural income, which in turn, affects local demand for industrial goods. They could also affect labor costs and firm productivity due to changes in

transportation infrastructure. To account for the changing access to transportation infrastructure, I include log distance to the nearest highway as a control variable. The results reported in Panel B show that the former results are robust when these controls are included. All estimates are in the same direction, and are of size and significance similar to those of the former estimates. I then conduct several robustness checks to test the sensitivity of these main results to the changing sample and specification.

Alternative radii: In the main analysis, I focus on areas defined based on a 20-kilometer radius from each river's exit point out of the jurisdiction city. Though the results do not hinge on this choice, I do realize there is a trade-off in choosing the radius. Industrial activities allocated closer to rivers and downstream borders are likely to have larger spillover effects on, and be more harmful to downstream residents. Therefore, using a shorter radius and focusing on smaller areas near the downstream border may produce larger estimated effects. At the same time, since the main outcomes are aggregated at the area level, and smaller areas will contain less parcels of land, a shorter radius may increase sampling errors and reduce statistical power. In Table 1.4, I show that the main results are robust to radii between 15 and 25 kilometers. In unreported analyses, the statistical significance of the results declines beyond this range.

The exclusion of native city leaders: According to the Rule of Avoidance, city leaders (i.e. party secretaries), cannot be native politicians (Regulations on the Selection and Appointment of Leading Party and Government Cadres, 2002; Interim Provisions on the Offices for Party Leaders and Government Cadres to Avoid, 2006).[36] Nonetheless, 4.2% of the city leaders appointed during the sample period were natives of their jurisdictions.[37] Having these "defiers" in our sample should not affect the results, since the estimation strategy exploits the variation across areas within the same city. Under my definition, all areas of the city governed by a native leader are NOT upstream of leaders' hometowns. Since there is no reason to expect the native leader would treat these areas in systematically different ways, the estimated effects should not be biased. The regression results in Table 1.14 confirm this conjecture. Estimates remain largely unaffected after excluding these "defiers" from the sample.

rural-to-urban migration (Imbert et al., 2018).

[36] *Dangzheng Ganbu Xuanba Renyong Gongzuo Tiaoli* (2002); *Dangzheng Lingdao Ganbu Renzhi Huibi Zanxing Guiding* (2006).

[37] Many of these "defiers" are from ethnic minority cities (or autonomous prefectures), which enjoy more discretionary power over political appointments.

Allowing for dynamic effects: In the conceptual framework, I consider a static setting where the supply decision in the current period does not affect land demand in future periods. I make this assumption since city leaders in the sample, on average, remain in office for less than 4 years. Thus, they might not consider the potential long-term effects of the current land supply when making these decisions. However, from the identification point of view, the dynamic effects of current supply on future demand might bias the estimates. If less supply in the past increases demand in the current period since there is less competition, the estimates will be biased upward. Conversely, if existing industrial activities have positive spillover effects on new ones, less supply in the past will induce less demand in the current period, and the estimates will be biased downward. In Table 1.5, I account for the potential effect of past supply by adding a cubic polynomial in cumulative land supply to each area up to the previous year ($t-1$). After the inclusion of controls for past-supply, the estimated effects become slightly smaller in magnitude, but are sizable enough and remain statistically significant at conventional levels. This suggests that the overall effects of past supply are unlikely to significantly bias the estimates. In Table 1.15, I include two lags for the key explanatory variable, *UpHome*, to account for the plausible influence of past leaders on current policymaking. Neither the magnitude nor the significance of the estimated parameters change markedly.

1.5.3 Mechanism Analysis

I interpret the main results as evidence for in-group preferences among Chinese city leaders. The findings suggest that they can be motivated by environmental concerns for their hometowns when they are spatially allocating industrial activities. In the following analysis, I provide empirical evidence in support of this interpretation.

1.5.3.1 Do Industrial Activities Increase Water Pollution?

I first demonstrate that there are indeed strong water pollution spillover effects associated with industrial activities, using water pollution data measured at monitor stations. The monitor stations are located along major Chinese rivers and report water pollution readings for each year between 2004–2010. I spatially match each monitor station to each parcel of industrial land within 20 kilometers. For each land parcel, I have collected information on the year in which it was sold for industrial construction, and whether it is upstream or downstream of the linked monitoring station. I then estimate the following specification to identify the changes in water pollution following the land sales.[38]

$$\text{POL}_{lmt} = \beta_1 \text{UP}_{lm} \times \text{POST}_{lt} + \beta_2 \text{DOWN}_{lm} \times \text{POST}_{lt} + \mu_{lm} + \lambda_{(b)t} + \epsilon_{lmt} \quad (1.2)$$

The unit of observation is the land parcel-monitor-year. The dependent variable, POL_{lmt}, is water pollution measured in year t, at monitor station m which is linked to a nearby land parcel l. UP_{lm} is a dummy that equals 1 if land parcel l is upstream of its linked monitor station m. Conversely, DOWN_{lm} is a dummy that equals 1 if land parcel l is downstream of the linked monitor. POST_{lt} is a post-event dummy that takes the value 1 if land parcel l was sold in or before year t. The model controls for year fixed effects and land parcel-monitor pair fixed effects, which absorb the main effects of the UP_{lm} and DOWN_{lm} dummies. In order to account for different trends in water pollution across river basins, year fixed effects are allowed to vary by river basin.[39] Because rivers transport pollutants from upstream to downstream areas, I expect β_1 to be positive and β_2 to be zero. The standard errors are two-way clustered by monitor and land parcel.

Table 1.6 presents the regression results from Equation 1.2. In Column 1, I follow the literature in environmental economics and use the COD level (chemical oxygen demand) as the measure of water pollution. COD is the amount of oxygen required to break down the oxidizable chemicals and microorganisms contained in a unit amount of surface water. This is relevant for a variety of pollutants in industrial waste (He et al., 2018; Kahn et al., 2015; Zhang et al., 2018). A higher level of this measure indicates more water pollution. Consistent with my assumption, the estimates show a strong positive association between COD levels and the expansion of industrial activities in upstream areas. In particular, COD levels increased by an average of 0.5 mg/L following the sale of industrial land parcels within 20 kilometers upstream of the monitor station, corresponding to an 8.5 percent increase at the sample mean.

The estimated pollution spillovers are sizeable. The results indicate that every 10 additional industrial projects allocated upstream of the monitor station will increase pollution readings at the monitor station by 5 percent at the sample mean. Do et al. (2018) estimate that a 10 percent rise in river pollution increases infant mortality rate by 4 per 1,000 in India. Sigman (2005) estimates that a 4 percent decrease in

[38]This specification is similar to that in Currie et al. (2015), which estimates the effect of industrial plant openings on air quality.

[39]Environmental standards and the stringency of environmental regulations may vary across river basins. Some pollution control programs (e.g., the "Three Rivers and Three Lakes" regulatory program) have only been implemented in certain river basins. This may lead to varying water quality trends across basins (Wang et al., 2018).

water quality had an annual cost of $17 million (in 1983 dollars) in the U.S. Ebenstein (2012) finds that a 9.7 percent increase in digestive cancer death rates is the result of one-grade deterioration in water quality (on a six-grade scale) in China. Each of these estimates suggests that industrial development imposes immense economic and health effects on downstream residents.

Because industrial pollution produced downstream of monitor stations will not contribute to monitor readings, I find no statistically discernible effects for downstream construction projects. These results also suggest that the estimated pollution effects of upstream construction projects are not driven by omitted confounders. Table 1.17 reports the results of robustness checks for alternative pollutant measures, including BOD (biological oxygen demand), NH (ammonia nitrogen), petroleum, phenol, mercury, and lead. For ease of interpretation, these measures have been standardized by subtracting the mean and dividing by the standard deviation. Thus, each has a mean of 0 and a standard deviation of 1, and the estimated coefficients can be interpreted in terms of standard deviation units. Notably, the estimated coefficients for upstream construction are positive in all six models, though not all are statistically significant. Industrial development in upstream areas also appears to have a sizable effect (.055 SD) on BOD levels. Since BOD is different from COD, and captures primarily domestic pollutants from biological sources like sewage and plant and animal matter, the estimated coefficient is statistically less strong.[40] To address multiple testing problem, I follow Currie et al. (2015) to combine all seven pollutants into a single summary measure. This is done by computing the simple average of the seven (standardized) pollutants and standardizing the average to be mean 0 and standard deviation 1. Estimated coefficients for this summary pollution index are presented in Column 2 of Table 1.2. These estimates are consistent with those in Column 1, in terms of both direction and significance level.

In order to check the pre-trends and explore the dynamic pollution effects of industrial construction, Figure 1.3 presents the event-study coefficients and the 90% confidence intervals estimated from two separate regressions, for years before and after the land sales. Both specifications are augmented from Equation 1.2, by replacing the post-sale dummy POST_{lt} with a set of event time dummies. In this way, I allow the pollution effect of land sales to vary with event time and by its upstream/downstream relation to the monitoring station. I use the COD level and the summary pollution index as the dependent variables in Panels A and B, respectively. In both graphs, the estimated coefficients and error bars are colored red for upstream land sales and

[40] https://www.rmagreen.com/chemical-oxygen-demand-biological-oxygen-demand

blue for land sales in downstream areas. Because the pollution data from monitoring stations are only available for seven years (2004–2010), I group the event years into a small number of bins ($t \leq -3$, $-2 \leq t \leq -1$, $t = 0$, $1 \leq t \leq 2$, $t \geq 3$) to increase the statistical power. Event time $t = 0$ represents the year in which a land parcel is sold for industrial construction. The dummy for period $-2 \leq t \leq -1$ is omitted to make the effects relative to that period. Corresponding regression results are reported in Table 1.16.

Three features stand out in Figure 1.3. First, rivers with and without land sales in upstream areas do not vary in pollution increase, prior to the sales; the degree of pollution in the river more than three years before the sales is not statistically different from that of 1 or 2 years before. Second, upstream land sales are immediately followed by increased water pollution. The effect is small in the year of the land sale, and increases afterwards, suggesting a duration of time required to complete construction.[41] Third, land sales in downstream areas are not followed by changes in water pollution. All of the coefficients for downstream sales are small and statistically indifferent from zero. This implies that the estimates for the pollution effects of upstream sales are not confounded by time-variant shocks to each river. Taken together, the patterns observed in both panels of Figure 1.3 support the validity of the identification assumptions underlying specification 1.2; they are consistent with the results presented in Table 1.6. All findings from this exercise show that the allocation of industrial activities to an area is indeed associated with high pollution costs for residents downstream of that area.

1.5.3.2 The Salience of Hometown Identification

In this subsection, I examine whether the effect of hometown bias on industrial land supply varies with the salience of leaders' hometown identities. Existing studies have found that individuals are more biased towards their social group members when their social group identities are intensified by inter-group conflict (Fisman et al., 2019; Hjort, 2014; Shayo and Zussman, 2011). In this light, I also expect city leaders to exhibit stronger preferences for their hometowns when their hometown identification is stronger. Since inter-region conflicts are not relevant in the Chinese context, I turn to linguistic distances between leaders' home cities and nearby cities as the measure of hometown salience. Since cities that speak different dialects from nearby regions have been geographically more isolated throughout history and have had less

[41] As discussed in Section 1.2, each parcel of land is used for projects as specified in their respective contracts. The majority (85.3%) of the industrial projects in the data take 1–3 years to complete.

interaction (i.e. trade, migration, etc.) with other regions, people in these cities have preserved more distinct culture. Thus, they may identify with stronger regional identities (Alesina et al., 2003; Falck et al., 2012; Guiso et al., 2009; Michalopoulos, 2012).

To calculate the linguistic distance between a leader's home city and its nearby cities, I first compute the linguistic distance between the home city and each of its nearby cities using dialect data drawn from Liu et al. (2015). I then take a simple average of the linguistic distances across all nearby cities. For ease of interpretation, I standardize the average linguistic distance to have a mean of 0 and a standard deviation of 1. As discussed earlier, a longer average linguistic distance between a leader's home city and nearby cities plausibly indicates stronger home city identification.

To test for heterogeneity by identity salience, in Columns 1–3 of Table 1.7 I augment Equation 1.1 with the interaction between the average linguistic distance and the *UpHome* indicator. In this way, the effect of hometown preferences is allowed to vary by leaders' hometown identity salience. The coefficients for the interaction term are negative and statistically significant in all three models. These findings suggest that the previously estimated land supply differences between upstream and control areas are indeed driven by city leaders' hometown preferences. They also indicate that city leaders are more "protective" of their hometowns when they have preserved more distinct cultural identities.

In Columns 4–6, I interact the *UpHome* indicator with the log of the population density of leaders' hometowns. River pollution likely affects more people when a river cuts through a densely populated area. In that case, leaders may be more incentivized to allocate industrial activities away from upstream areas. Consistent with this hypothesis, the findings show larger supply differences between upstream and control areas when leaders' hometowns are more populated. These findings lend further support to the social preferences interpretation.

1.5.3.3 Competing Hypotheses

The above analyses have provided evidence that city leaders' environmental concerns for residents of their hometowns may influence their decisions on the spatial allocation of industrial activities within their jurisdictions. However, there may be alternative interpretations consistent with the results, given that industrial activities might have other externalities. For example, city leaders may allocate less land to industrial firms near their hometowns in order to avoid competition with hometown firms. To evaluate the extent to which the estimated hometown effects are driven by other externalities, I perform a placebo test to compare areas “downstream” of city leaders’ hometowns to those “downstream” of non-hometowns. The use of this sample will rule out the water pollution externality mechanism, since water pollution generated in downstream areas does not spill over to upstream areas. If it is the other externalities that are at work, we should expect to see similar effects as before. This idea is illustrated in Figure 1.4.

I estimate a specification analogous to Equation 1.1, where the explanatory variable is a dummy for being **downstream** of city leaders’ hometowns. The results are presented in Table 1.8. The estimated coefficients are much smaller in magnitude than before, and none of them are statistically significant. These results suggest that the previously estimated land supply differences between upstream and control areas are not simply driven by the geographical proximity of upstream areas to city leaders’ hometowns. Rather, it is being upstream of, and having water pollution spillover to, leaders’ hometowns that differentiates these areas.

In another test, I repeat the estimation of Equation 1.1 using the respective supplies of commercial and residential land as the dependent variables. Unlike industrial land sold for industrial construction, commercial and residential land is used for less polluting activities. Thus, city leaders may not be incentivized to misallocate commercial or residential land between upstream and control areas. Consistent with this hypothesis, Table 1.20, shows that there are no statistically distinguishable effects for non-industrial land.

Additionally, I provide evidence that leaders do not exhibit similar social preferences for non-home cities where they had previously worked. This evidence also points against an information mechanism as the driver of the main results. If leaders reduce pollution for their hometowns because they have better information about them, similar effects should be expected for areas upstream of the cities where they had previously worked, since they would have better information about them as well. However, the results in Table 1.23 do not support this hypothesis. Finally, using data on emission fees paid by industrial firms in 2004, I find no evidence that environmental regulations are enforced with varying stringency across areas.

1.5.4 Implications for Efficiency

Given the large effects of policymakers’ hometown preferences on the spatial allocation of industrial land within their jurisdictions, it is natural to ask how the allocative efficiency and local economic welfare may have been affected. Conceptually, if the entire jurisdiction city is an integrated market for land, and there is little market friction (i.e. a free flow of capital and labor, low transportation costs, etc.), the spatial allocation of industrial land should matter little for efficiency. In that scenario, different firms may have different land valuations, but the same firm is indifferent to land in different areas within the jurisdiction city, and thus values them equally. Land allocation is efficient to the extent that land will always go to the firms who value it the most. Holding the total supply constant, total market surplus is irrelevant to the spatial allocation of land.

However, in reality, there is variance across areas. Firms are likely to prefer land in certain areas over others. The manipulation of land supply across areas, due to policymakers' personal preferences, may force firms to forgo land they value more and instead pursue less valuable land, thus causing a loss of market surplus. In order to test this hypothesis, I turn to the data on land prices. First, I use the same specification as before to examine whether, and to what extent, average prices vary between upstream and control areas. Then, I check the robustness of the results to different samples and specifications. Using the estimated effects on land prices and former estimates for supply, I quantify the lost market surplus due to leaders' hometown preferences.

1.5.4.1 Effects on Land Prices

In Table 1.9, I estimate Equation 1.1 with average land prices as the dependent variables. Average land prices are defined as the average prices of newly-supplied land parcels in each area in each year. In Columns 1–3, I take a simple, unweighted average of prices across all parcels of land and gradually add controls for weather, infrastructure, and cumulative past supply. All estimates are positive and statistically significant at the 1% level, suggesting that land prices are higher in upstream areas than in otherwise similar control areas. In the remaining columns of Table 1.9, I use the weighted average price of newly-supplied land in each area and year as the dependent variable, where the price of each parcel is weighted by its own size. The point estimates are consistently positive and statistically significant at the 5% level. In terms of magnitude, the estimated coefficients indicate a 12-15% price differential between upstream and control areas.

I conduct a battery of robustness checks on the results. In Table 1.18, I exclude observations where the city is governed by a native politician, and find that the estimated coefficients are similar in magnitude and significance to those presented in Table 1.9. These results are also robust to areas defined by alternative radii (Table 1.19), and to standard errors clustered at alternative levels (Table 1.25). Since price data are available only if land transactions take place in a given area and period, in order to reduce the incidence of zero transactions and alleviate the selection problem, I also analyze the price data at the area × office term level. Instead of aggregating data by area and year, in Table 1.21, I aggregate both the outcome and the control variables of Equation 1.1 by area and the leader's office term. With the inclusion of area fixed effects and office term fixed effects, the estimates from this specification are similar to previous ones.

I interpret the higher land prices in upstream areas as being driven by less land supply to these areas. However, since land parcels may vary in size, quality, and other characteristics, one concern with this interpretation is that the estimated price differentials may be driven by different land parcel compositions supplied to different areas. A related concern is that local governments may adopt different methods to sell land located in different areas. As noted earlier, the government may sell land through either private negotiations or pubic auctions. Since the average sales price is lower when land is sold through private negotiations than through public auctions, one may be concerned that the higher prices in upstream areas are driven by more frequent sales through public auctions. In order to alleviate these concerns, I estimate a parcel-level specification in which the land parcel characteristics are included as controls. The specification is the following:

$$\text{Price}_{pijt} = \alpha + \beta \times \text{UpHome}_{ijt} + \gamma \times X_{pijt} + \lambda_i + \mu_{jt} + \epsilon_{pijt} \tag{1.3}$$

In this specification, each observation is a parcel of industrial land p, located in area i of city j, and sold in year t. The dependent variable is the log sale price of land parcel p. The key explanatory variable is an indicator that equals 1 if parcel p is located upstream of the leader's hometown in sales year, t. For each parcel of land in the sample, I have information on its size, quality grade, and the method through which it was sold. I gradually add these controls to the baseline specification and report the regression results in Table 1.10. Area fixed effects and city × year fixed effects are included in Columns 1 through 3.

In Column 1 of Table 1.10, I add the size of the land parcel to account for potential second-degree price discrimination in the land market, i.e., different unit prices for different units purchased. This does not qualitatively alter the estimated price effect; if anything, the estimated coefficient from this specification increases slightly in magnitude. In Column 2, I add dummies for whether a land parcel is sold through private negotiation, listing auction, invited bidding, or a standard English auction.

This is done to address the possibility that city leaders use different sales methods for land in different areas. To account for the effect of land quality on land prices, in the last column, I also include quality grade dummies for each land parcel as controls. The land quality is evaluated by the government before the transaction, based on factors such as the parcel's landscape, its access to markets, and surrounding infrastructure. 76 percent of the sample land parcels had been rated before their respective transactions. However, 24 percent were left ungraded. I include a set of dummies for each quality grade, as well as a dummy for ungraded land parcels. Once again, the results are comparable, both in terms of point estimates and statistical significance. The stability of these estimates also suggests that it is unlikely that the estimates are the result of omitted variables.

1.5.4.2 Lost Market Surplus

Thus far, the empirical findings have shown that areas upstream of city leaders' hometowns receive significantly less land for industrial activities. Meanwhile, the average prices of newly-supplied land are significantly higher in these areas. I interpret these findings as evidence of the spatial misallocation of industrial activities due to city leaders' hometown preferences. As discussed, if production location is immaterial to firms, and they value land in different areas equally, average land prices should be the same across areas. In that scenario, land parcels would always go to the firms that value them the most. Holding the total supply constant, the spatial allocation of land would have no effect on efficiency. However, the empirical findings show that firms do care about the locational of production, and that land prices significantly vary between upstream and control areas. Furthermore, the fact that land prices are higher in upstream areas suggests that there are firms with higher valuation of land in these areas under the current allocation scheme. This is likely because it is more profitable for these firms to locate their production there. However, due to the restricted supply u, firms are unable to acquire land in their preferred areas, and thus their productivity and/or profits suffer.

How extensive are these losses due to land misallocation? In theory, one could use production data to quantify the productivity gains if firms were allocated to their preferred locations instead of the actual locations. Since the production data for land buyers (firms) are unavailable, I quantify the misallocation costs of leaders' hometown preferences using lost surplus in the industrial land market. The idea is simple. From city governments' perspective, holding the total supply constant, reducing the supply to low-price areas and increasing the supply to high-price areas can expand the market surplus (and generate higher revenues from land sales). However, city governments forgo additional market surplus and government revenue, due to leaders' personal concerns for their hometowns. Thus, the amount of foregone surplus can be used to gauge the misallocation costs of leaders' hometown preferences.

As noted, the total amount of land that each city government can sell in a given year is determined by the Central and provincial governments through quotas. The quotas are binding, to the extent that they are smaller than the quantity that city governments would like to sell had they been unconstrained. Therefore, they are regularly exhausted by city governments (Wang et al., 2016; Xie, 2015). In this light, I consider the total supply in a given city and year to be fixed. Under the assumption that the marginal cost of land supply is constant, the spatial allocation of land only affects the buyer surplus, and not the total cost. Therefore, the welfare losses are just the additional buyer surplus that could have been generated from the given amount of land. By construction, the sample areas are similar in both size and geographical characteristics. Therefore, I further assume that all sample areas face the same linear demand curve. Under these assumptions, the lost market surplus due to leaders' hometown preferences can be expressed in terms of the estimated quantity and price differentials between upstream and control areas. Next, I draw estimates for land supply and price from preferred specifications (see Tables 1.3 and 1.9), and evaluate the deadweight loss at the mean price and supply. The calculation shows that land misallocation associated with each upstream area leads to a loss of surplus roughly equal to 1.55 million Chinese *yuan*, corresponding to 1.6 percent of the average land sale revenue from each area. These calculations are detailed in Table 1.26.

The estimated welfare costs of policymakers' hometown biases have a modest magnitude for the following reasons. First, city leaders face quantity constraints imposed by the provincial and Central governments. The quotas are far smaller than the total quantity that they wish to sell. Therefore, when land supply decreases in upstream areas, it increases in other areas. In other words, the decreased market surplus in upstream areas, due to hometown bias, is partially offset by increased surplus in other areas. In a context where city leaders' hands are untied, the deadweight loss would be greater. Second, the spatial allocation of industrial land affects leaders' in-group members through water pollution spillover. In alternative environments where politicians' policy decisions have more effect on their in-group members' welfare, the distortion effect may be even larger. Finally, to simplify the calculation, I assume the cost of pollution to be linear. This overlooks environmental losses when the cost of pollution is convex. Nonetheless, the unambiguous sign of the welfare change provides evidence that in-group preferences among policymakers have led to a net welfare loss. Given that fiscal revenue is positively correlated with city leaders' career advancement (Lü and Landry, 2014; Chen and Kung, 2016), the evidence also suggests that politicians are willing to sacrifice a certain amount of personal benefit in exchange for social benefits for in-group members.

1.5.5 Additional Results

In the final set of empirical analyses, I examine how city leaders' social preferences for their hometowns influence overall economic activity across areas. Because the undersupply of industrial land to upstream areas mechanically reduces the number of industrial firms, job opportunities, and local income, I expect the upstream areas to be less developed. The following specification is used to test this hypothesis:

$$\text{Light}_{ijt} = \alpha + \beta \times \text{Exposure}_{ijt} + \gamma \times X_{ijt} + \lambda_i + \mu_{jt} + \epsilon_{ijt} \tag{1.4}$$

Due to the lack of employment, income, and GDP data at granular levels, I follow previous studies in using nighttime light intensity as a measure of economic activity. The original measure of light intensity for each grid ranges from 0 to 63. I take a simple average of light intensity over all grids within each sample area and year. Because the distribution of the average nighttime light intensity is right-skewed, and about 25 percent of the area×year observations have average intensity equal to 0, I follow previous studies and use the logarithm of average nighttime light intensity plus .01 as the dependent variable (Hodler and Raschky, 2014; Michalopoulos and Papaioannou, 2013, 2014). As discussed in Section 1.2, the majority (85.3 percent) of the industrial projects in the data take 1–3 years to complete. In order to account for the lagging effect of land supply on local economic activity, I define a key explanatory variable, Exposure_{ijt}, as the number of years over the previous three years that an area is upstream of city leaders' hometowns. Once again, I control for area fixed effects, city by year fixed effects, and other controls as in Equation 1.1.

The point estimates reported in Table 1.11 show that, indeed, having been upstream of a leader's hometown over the previous three years is associated with lower levels of economic activity; an additional year of exposure decreases nighttime light intensity by about 3 percent. The estimated coefficient is statistically significant at the conventional 10 percent level. Furthermore, the point estimates tend to be greater in magnitude and statistically more significant when looking at areas defined by shorter radii. This indicates that policymakers have stronger incentive to misallocate industrial activities in areas closer to rivers flowing towards their hometowns. Since a non-trivial fraction of the observations have average light intensity equal to 0, in Table 1.22, I use a dummy for positive light intensity as the dependent variable. These estimates consistently show lower levels of economic development in upstream areas. An additional year of being upstream of a leader's hometown reduces the likelihood of having a detectable light intensity by 0.5 percent. Once again, the effect is more pronounced for areas defined by shorter radii. These findings provide additional evidence in support of this study's main hypothesis. They also suggest that policymakers' social preferences for their hometowns have exacerbated spatial economic inequality within their jurisdictions.

1.6 Conclusion

Given the importance of policymakers in determining economic performance and jurisdictional welfare (Jones and Olken, 2005), a central question to political economy and development is what motivates policymakers (Besley, 2005). The answer to this question helps unpack the black box of policymaking, and has implications for institutional designs aimed at improving government decision-making.

Politicians and bureaucrats care about both career advancement and economic benefits. This point has been underscored by influential studies in both economics and political science (Besley and Case, 1995; Besley and Burgess, 2002; Olken and Pande, 2012; Shleifer and Vishny, 1993). However, the question remains as to whether policymakers are motivated by intrinsic social preferences for others. Is there a preference for members of their own social groups? If so, how do in-group preferences among policymakers influence their policy decisions, and in turn, local welfare? The answers to these questions are less clear.

This study addresses these questions in the context of Chinese cities. It leverages the unique features of city leader appointment in China—city leaders are often assigned to jurisdictions proximate to their hometowns—and develops a strategy to compare industrial activities allocated to areas upstream of leaders' hometowns with similar areas upstream of non-hometowns. Using georeferenced data on each parcel of land supplied by city governments for industrial uses, and matched with Chinese rivers and city boundaries, this study finds that areas upstream of leaders' hometowns receive significantly less land for industrial activities. A variety of additional evidence suggests that this is a means to reduce water pollution spillover effects to residents of leaders' hometowns. The hometown bias among city leaders, however, is not cost free. The undersupply of land to upstream areas drives up land prices, diminishing the surplus that could have been generated had there been no hometown preferences. Furthermore, data on nighttime light intensity shows that, due to the undersupply of industrial land, upstream areas experience less development.

This study's findings provide evidence that policymakers are indeed motivated by social preferences towards in-group members. More importantly, they show that in-group preferences among policymakers can induce spatial misallocation of both resources and economic activity. Standard models of in-group bias predict resource misallocation, as decision makers trade off efficiency against additional utility gains from favoring their own-group members (Fisman et al., 2017). As self-evident as it appears, due to the lack of measures of efficiency, there is little direct evidence for this prediction. Focusing on a context where resources are allocated through market mechanisms, this study overcomes the difficulty by using the loss of market surplus to gauge misallocation. It finds that policymakers' in-group preferences indeed have negative welfare consequences in our context. However, this does not imply that political elites' in-group preferences are necessarily welfare-reducing. In other circumstances, in-group preferences can be utilized to align the interests of and to sustain the cooperation between government officials within the bureaucracy (Jiang, 2018). They could also be used to improve the well-being of underrepresented social groups (Pande, 2003). Each of these calls for considerations of social group identities and cautious calculations of their costs and benefits during the process of political selection and appointment.

1.7 Tables and Figures

Table 1.1: Summary Statistics

VARIABLES	Mean	S.D.	Obs.
Panel A: Parcel-level Land Outcomes			
Size (*ha*)	3.264	5.966	95,170
Price (*yuan* per m^2)	208.2	167.0	95,170
Auction	0.725	0.446	95,170
English Auction	0.028	0.165	95,170
Invited Bidding	0.005	0.069	95,170
Listing Auction	0.692	0.461	95,170
Quality Evaluated (0/1)	0.755	0.430	95,170
Quality Rating	5.227	3.827	71,815
Panel B: Area-level Outcomes			
Any Supply	0.379	0.485	17,351
Total area supplied, log	2.593	1.896	6,573
Total area supplied, IHS	1.281	1.948	17,351
Num. of parcels supplied, IHS	0.946	1.443	17,351
Log price, unweighted	4.901	0.729	6,573
Log price, weighted	4.889	0.743	6,573
UpHome (0/1)	0.081	0.273	17,351
Monthly rainfall, avg	75.19	40.125	17,351
Monthly temperature, avg	12.23	6.341	17,351
Monthly rainfall, sd	69.33	31.33	17,351
Monthly temperature, sd	9.474	2.851	17,351
Distance to nearest highway, log	3.208	1.418	17,351

Notes: Panel A presents summary statistics on 95,170 industrial land parcels in sample areas. Panel B presents summary statistics on key variables aggregated at the area-year level.

Table 1.2: Summary Statistics (*cont.*)

VARIABLES	Mean	S.D.	Obs.
Panel C: City Leader Characteristics			
Male	0.961	0.194	1,304
Han ethnicity	0.911	0.285	1,300
Age at first appointment	49.75	3.931	1,293
Years of party membership at first app.	26.25	5.079	1,161
Master degree	0.605	0.489	1,187
Num. of cities served	1.158	0.387	1,304
Avg. length of office term (years)	3.931	1.820	1,025
Panel D: Water Pollution Measures			
COD (mg/L)	6.731	10.95	3,391
BOD (mg/L)	5.609	14.26	3,391
NH (mg/L)	2.131	5.273	3,391
Petroleum (mg/L)	0.107	0.295	3,391
Phenol (mg/L)	0.006	0.031	3,391
Mercury (μg/L)	0.033	0.091	3,391
Lead (mg/L)	0.006	0.015	3,391

Notes: Panel C presents summary statistics on 1,306 city leaders who were in office from 1998 to 2015. Age and years of tenure in the party are measured in the year when the politician becomes a city leader. Number of cities served and the average length of office term in a city are measured during the sample period. The average length of office term is calculated based on finished office terms only. Panel D presents summary statistics on water pollution measures at the monitor-year level.

Table 1.3: Hometown Preferences and the Quantity of Industrial Land Supply

VARIABLES	Any Supply	log Supply	$sinh^{-1}$ Supply	$sinh^{-1}$ Parcels
	(1)	(2)	(3)	(4)
	Panel A: No additional controls			
UpHome	-0.059***	-0.294**	-0.227***	-0.170***
	(0.020)	(0.146)	(0.075)	(0.052)
	Panel B: with additional controls			
UpHome	-0.058***	-0.289**	-0.225***	-0.169***
	(0.020)	(0.145)	(0.075)	(0.052)
Area FE	✓	✓	✓	✓
City × Year FE	✓	✓	✓	✓
Observations	17,351	6,573	17,351	17,351
Mean of dep.	.379	2.593	1.281	.946

Notes: Depend variables in columns 1–4 are a dummy equal to one if an area receives any land supply, the log of total quantity of land (in hectare), the inverse hyperbolic sine of total quantity of land, and the inverse hyperbolic sine of total number of land parcels supplied in an area, respectively. Regression models in panel A control for area fixed effects and city × year fixed effects. Regression models in panel B additionally control for the annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.4: Robustness – Alternative Radii

VARIABLES	Any Supply	log Supply	$sinh^{-1}$ Supply	$sinh^{-1}$ Parcels
	(1)	(2)	(3)	(4)
	Panel A: 15-kilometer radius			
UpHome	-0.043**	-0.265	-0.177***	-0.121***
	(0.018)	(0.173)	(0.061)	(0.042)
Observations	17,351	5,016	17,351	17,351
R-squared	0.709	0.849	0.783	0.821
Mean of dep.	.289	2.299	.898	.657
	Panel B: 25-kilometer radius			
UpHome	-0.033*	-0.239**	-0.154*	-0.133**
	(0.020)	(0.118)	(0.080)	(0.054)
Observations	17,351	7,925	17,351	17,351
R-squared	0.748	0.846	0.839	0.870
Mean of dep.	.457	2.859	1.659	1.236

Notes: Depend variables in columns 1–4 are a dummy equal to one if an area receives any land supply, the log of total quantity of land (in hectare), the inverse hyperbolic sine of total quantity of land, and the inverse hyperbolic sine of total number of land parcels supplied in an area, respectively. The unit of observation, area, is defined by a 15-km radius from rivers' exit point in panel A and by a 25-km radius in panel B. All regressions control for area fixed effects, city × year fixed effects, annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.5: Robustness – Allowing for Lagged Effects

VARIABLES	Any Supply	log Supply	$sinh^{-1}$ Supply	$sinh^{-1}$ Parcels
	(1)	(2)	(3)	(4)
UpHome	-0.054***	-0.295**	-0.170**	-0.126***
	(0.020)	(0.143)	(0.067)	(0.045)
Observations	17,351	6,573	17,351	17,351
R-squared	0.733	0.851	0.825	0.858
Mean of dep.	.379	2.593	1.281	.946

Notes: Depend variables in columns 1–4 are a dummy equal to one if an area receives any land supply, the log of total quantity of land (in hectare), the inverse hyperbolic sine of total quantity of land, and the inverse hyperbolic sine of total number of land parcels supplied in an area, respectively. All regressions control for area fixed effects, city × year fixed effects, annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, log distance to the nearest highway, and a cubic polynomial in cumulative land supply up to the previous year. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.6: Effects of Industrial Activities on Water Pollution

VARIABLES	Chemical Oxygen Demand	Summary Pollution Index
	(1)	(2)
Up_Monitor × Post	.494**	.065**
	(.222)	(.032)
Down_Monitor × Post	-.051	-.005
	(.127)	(.019)
Observations	539,080	539,080
R-squared	.734	.795
Mean of dep.	5.716	0

Notes: Unit of observation is the parcel-monitor-year. Depend variables in columns 1–2 are the level of COD (Chemical Oxygen Demand), and a summary index that averages together all seven pollutants measured from monitoring stations, respectively. The summary index has been standardized to be mean 0 and standard deviation 1. All specifications control for monitor-land pair fixed effects and river basin by year fixed effects. Robust standard errors two-way clustered by monitoring station and land parcel appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.7: Heterogeneity Analysis

	Hometown Salience			Pop Density		
VARIABLES	Any Supply	$sinh^{-1}$ Supply	$sinh^{-1}$ Parcels	Any Supply	$sinh^{-1}$ Supply	$sinh^{-1}$ Parcels
	(1)	(2)	(3)	(4)	(5)	(6)
UpHome × log (dist.)	-0.055**	-0.155*	-0.093*			
	(0.024)	(0.083)	(0.054)			
UpHome × log (dens.)				-0.023	-0.126**	-0.090**
				(0.016)	(0.057)	(0.041)
UpHome	-0.064***	-0.252***	-0.171***	-0.059***	-0.226***	-0.170***
	(0.023)	(0.086)	(0.058)	(0.020)	(0.074)	(0.051)
Observations	15,066	15,066	15,066	17,349	17,349	17,349
R-squared	0.729	0.818	0.851	0.731	0.818	0.850
Mean	.385	1.297	.955	.379	1.281	.946

Notes: This table provides results from the heterogeneity analysis. Depend variables in Columns 1–3 (4–6) are a dummy equal to one if an area receives any land supply, the inverse hyperbolic sine of total quantity of land, and the inverse hyperbolic sine of total number of land parcels supplied in an area, respectively. The key variable of interest, UpHome, is interacted with log average linguistic distance between a leader's home city and other cities within the same province in Columns 1–3, and is interacted with log population density of the home city in Columns 4–6. Both linguistic distance and population density have been standardized to be mean 0 and standard deviation 1. All regressions control for area fixed effects, city × year fixed effects, annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.8: Placebo - Effects for Downstream Areas

VARIABLES	Any Supply	log Supply	$sinh^{-1}$ Supply	$sinh^{-1}$ Parcels
	(1)	(2)	(3)	(4)
DownHome	-0.000	-0.064	-0.001	0.015
	(0.020)	(0.173)	(0.075)	(0.054)
Observations	17,047	4,822	17,047	17,047
R-squared	0.698	0.840	0.771	0.808
Mean of dep.	.283	2.332	.887	.657

Notes: This table provides a placebo test using the sample of areas downstream of city leaders' hometown and non-hometowns. Depend variables in columns 1–4 are a dummy equal to one if an area receives any land supply, the log of total quantity of land (in hectare), the inverse hyperbolic sine of total quantity of land, and the inverse hyperbolic sine of total number of land parcels supplied in an area, respectively. All regressions control for area fixed effects, city × year fixed effects, annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.9: Hometown Preferences and Land Prices

VARIABLES	log (avg price)			log (weighted avg price)		
	(1)	(2)	(3)	(4)	(5)	(6)
UpHome	0.143***	0.144***	0.141***	0.111**	0.114**	0.111**
	(0.046)	(0.046)	(0.044)	(0.045)	(0.045)	(0.044)
Geographic controls		✓	✓		✓	✓
Past-supply controls			✓			✓
Observations	6,573	6,573	6,573	6,573	6,573	6,573
R-squared	0.880	0.880	0.881	0.878	0.878	0.878
Mean of dep.	4.901	4.901	4.901	4.889	4.889	4.889

Notes: The depend variables in columns 1–3 and 4–6 are the log of average land price and the log of size-weighted average land price in each area and year, respectively. Area fixed effects and city × year fixed effects are included in all regressions. Columns 2 and 4 further include annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway. Columns 3 and 6 additionally control for a cubic polynomial in cumulative land supply up to the previous year. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.10: Land Prices – Transaction Level Analysis

VARIABLES	log (Price)		
	(1)	(2)	(3)
UpHome	0.169*	0.179*	0.161*
	(0.101)	(0.096)	(0.096)
Observations	95,170	95,170	95,170
R-squared	0.631	0.655	0.657
Mean of dep.	4.995	4.995	4.995

Notes: Each observation is a parcel of land sold by city governments in the primary market. The depend variables the log of price (per hectare). Area fixed effects and city × year fixed effects are included in all regressions. Columns 1 controls for the log of parcel size. Column 2 adds dummies for sales method. Column 3 further include the quality of land parcel measured on a 20-point scale. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.11: Nighttime Light Intensity

	ln (light intensity)		
VARIABLES	10-km	15-km	20-km
	(1)	(2)	(3)
Exposure over previous years	-0.057**	-0.044**	-0.031*
	(0.025)	(0.020)	(0.018)
Observations	22,047	22,047	22,047
R-squared	0.941	0.955	0.964
Mean of dep.	-.930	-.621	-.407

Notes: Dependent variable is log(0.01 + avg. light intensity). The key explanatory variable is the number of years over the previous three years that an area is upstream of city leaders' hometowns. The unit of observation, area, is defined by a 10-km, 15-km, and 20-km radius from each river' exit point in columns 1–3, respectively. All regressions control for area fixed effects, city × year fixed effects, annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Figure 1.1: Illustration of Identification Strategy

Note: This figure illustrates the identification strategy. One river flows from the jurisdiction city A to its leader's home city. The other river flows from the jurisdiction to a non-home city. The black dots represent each river's exit point from the jurisdiction. The stars (triangles) represent industrial land parcels supplied within a radius of each exit point in the jurisdiction. The identification strategy compares quantity and price of industrial land supplied to areas upstream of the jurisdiction city leader's hometown vs. areas upstream of non-hometowns, during and after the current leader's term.

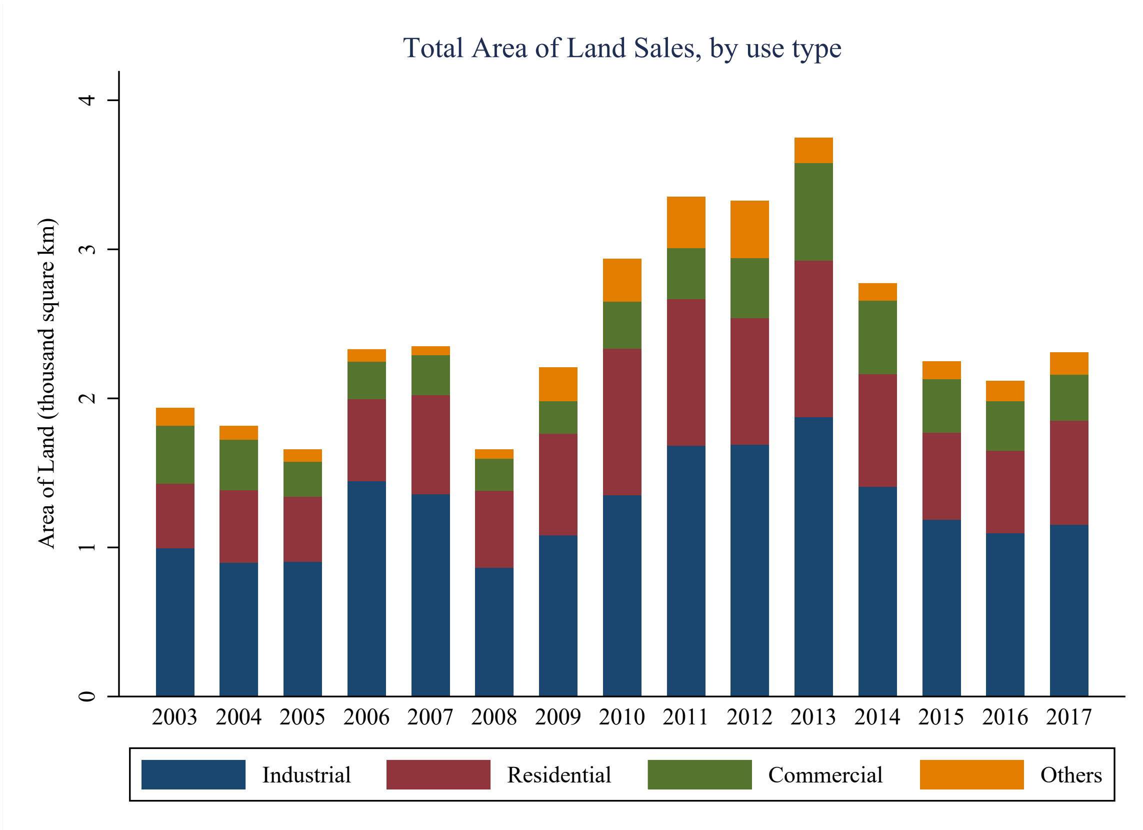
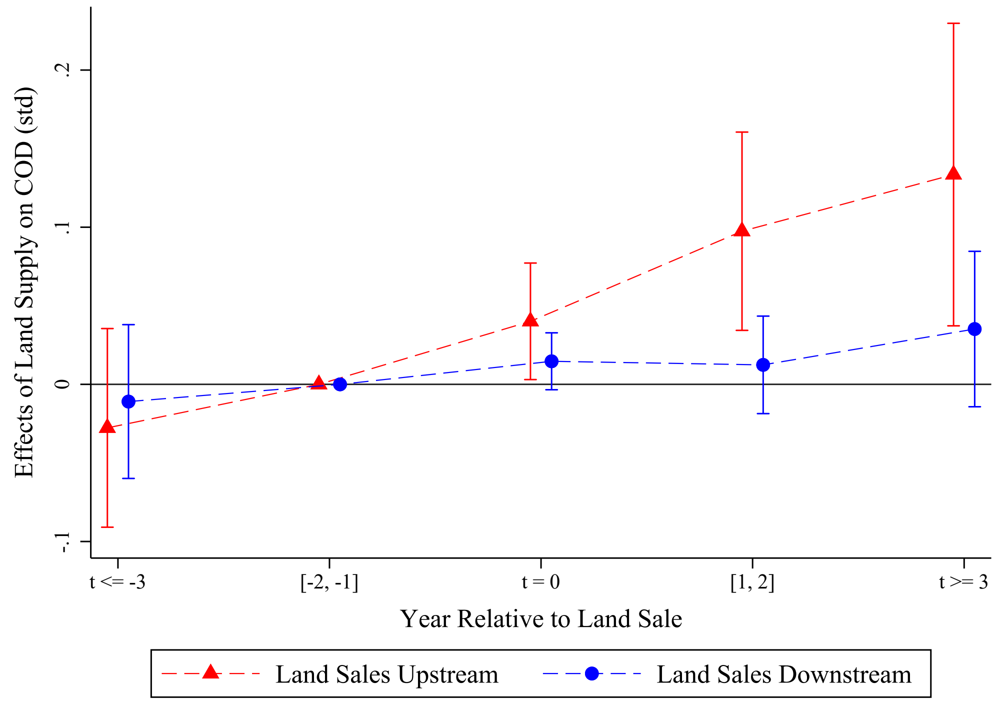


Figure 1.2: Annual Supply of Land, by use type

Source: China Statistical Yearbooks on Land and Resources; Land Transaction Monitoring System.

(a) COD (Chemical Oxygen Demand)



(b) Summary Pollution Index

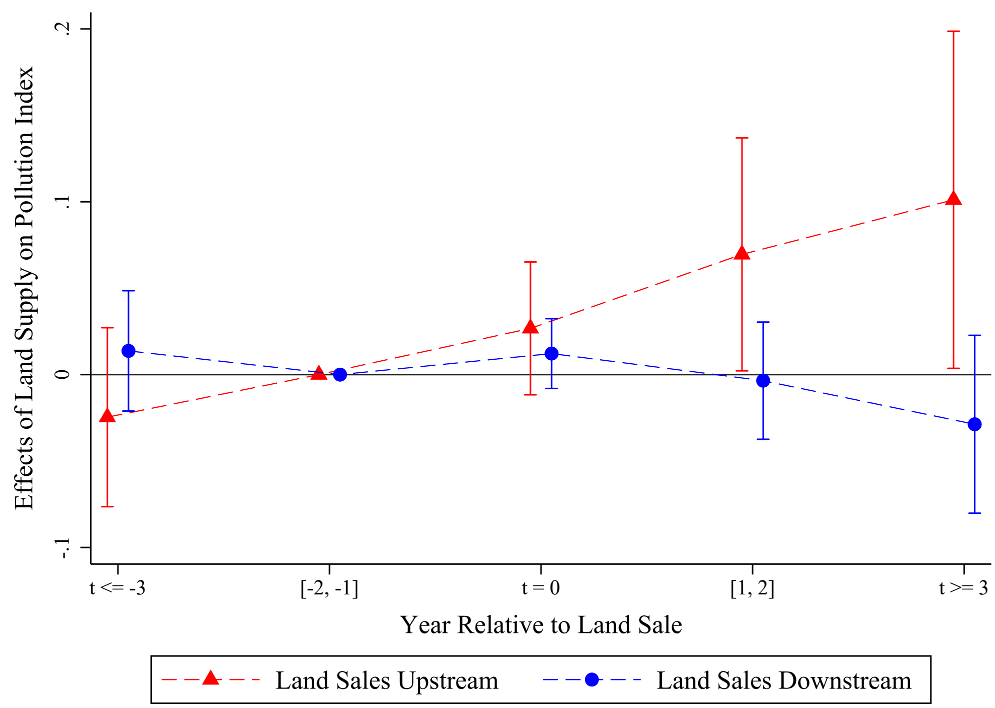


Figure 1.3: Effects of Industrial Activities on Water Pollution

Note: The above two figures plot coefficient estimates and 90% confidence intervals for the effects of industrial land supply on the level of COD and the summary pollution index, respectively. Both COD and the summary index have been standardized to be mean 0 and standard deviation 1.

1.8 Appendix: Additional Tables and Figures

Table 1.12: Balance Checks

VARIABLES	Difference	
	No Year of Appointment FE	With Year of Appointment FE
	(1)	(2)
	Panel A: Individual Characteristics	
Male	0.021	0.019
	(0.016)	(0.016)
Age	-0.319	-0.276
	(0.297)	(0.287)
Years of tenure in the party	-0.162	-0.090
	(0.436)	(0.425)
Master degree	0.010	0.023
	(0.041)	(0.041)
Political rank	-0.030	-0.021
	(0.029)	(0.028)
Years of experience as city head	-0.104	-0.064
	(0.184)	(0.186)
Any experience as city head	-0.012	-0.003
	(0.036)	(0.037)
Observations	927	927
Joint Significance Test (χ^2)	4.35	3.66
p-value	0.739	0.818

Notes: This table provides balance checks on individual characteristics between city leaders who are placed upstream of hometowns and those who are not. Each parameter is from a separate regression with outcomes indicated in the variables column. All balance variables are measured in the year of appointment. Estimated differences, with and without year of appointment dummies, are reported in columns 1 and 2, respectively. P-values calculated based on robust standard errors clustered at the city level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.13: Balance Checks (*cont.*)

VARIABLES	Difference No Year of Appointment FE (1)	 With Year of Appointment FE (2)
	Panel B: Hometown Characteristics	
GDP	0.833	0.991
	(0.694)	(0.691)
per capita GDP	-0.020	0.192
	(0.892)	(0.913)
Share in GDP, primary	-0.007	-0.008
	(0.009)	(0.009)
Share in GDP, secondary	-0.000	0.000
	(0.010)	(0.010)
Share in GDP, tertiary	0.008	0.008
	(0.007)	(0.007)
Pop density	3.588	7.673
	(28.38)	(29.40)
Rural poulation share	-0.001	-0.006
	(0.014)	(0.014)
Avg. years of schooling	0.011	0.026
	(0.096)	(0.096)
Observations	927	927
Joint Significance Test (χ^2)	4.45	4.38
p-value	0.814	0.821

Notes: This table provides balance checks on hometown characteristics between city leaders who are placed upstream of hometowns and those who are not. Each parameter is from a separate regression with outcomes indicated in the variables column. Data on population and average years of schooling are obtained from Population Census of 2000. Estimated differences, with and without year of appointment dummies, are reported in columns 1 and 2, respectively. P-values calculated based on robust standard errors clustered at the city level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.14: Robustness – Excluding Native City Leaders

VARIABLES	Any Supply	log Supply	$sinh^{-1}$ Supply	$sinh^{-1}$ Parcels
	(1)	(2)	(3)	(4)
UpHome	-0.058***	-0.259*	-0.219***	-0.173***
	(0.020)	(0.146)	(0.074)	(0.052)
Observations	16,625	6,333	16,625	16,625
R-squared	0.732	0.850	0.818	0.850
Mean of dep.	.381	2.570	1.280	.945

Notes: Depend variables in columns 1–4 are a dummy equal to one if an area receives any land supply, the log of total quantity of land (in hectare), the inverse hyperbolic sine of total quantity of land, and the inverse hyperbolic sine of total number of land parcels supplied in an area, respectively. This sample excludes city-years where the city leader is a native. All regressions control for area fixed effects, city × year fixed effects, annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.15: Robustness – Allowing for Lags of Key Explanatory Variable

VARIABLES	Any Supply	log Supply	$sinh^{-1}$ Supply	$sinh^{-1}$ Parcels
	(1)	(2)	(3)	(4)
UpHome	-0.061**	-0.301*	-0.169**	-0.128**
	(0.025)	(0.165)	(0.084)	(0.055)
Observations	14,413	6,324	14,413	14,413
R-squared	0.737	0.850	0.836	0.868
Mean of dep.	.439	2.619	1.493	1.098

Notes: Depend variables in columns 1–4 are a dummy equal to one if an area receives any land supply, the log of total quantity of land (in hectare), the inverse hyperbolic sine of total quantity of land, and the inverse hyperbolic sine of total number of land parcels supplied in an area, respectively. All regressions control for area fixed effects, city × year fixed effects, annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, log distance to the nearest highway, and two lags of the UpHome variable. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.16: Event-study Estimates for Water Pollution Effects

VARIABLES	COD	Summary Index
	(1)	(2)
Up_Monitor $\times$ $1[t \leq -3]$	-.223	-.025
	(.252)	(.026)
Up_Monitor $\times$ $1[t = 0]$	.311**	.027
	(.147)	(.020)
Up_Monitor $\times$ $1[1 \leq t \leq 2]$	.753***	.069**
	(.250)	(.034)
Up_Monitor $\times$ $1[t \geq 3]$	1.027***	.101**
	(.380)	(.050)
Down_Monitor $\times$ $1[t \leq -3]$	-.076	.014
	(.231)	(.021)
Down_Monitor $\times$ $1[t = 0]$	.110	.012
	(.085)	(.012)
Down_Monitor $\times$ $1[1 \leq t \leq 2]$	.088	-.004
	(.146)	(.021)
Down_Monitor $\times$ $1[t \geq 3]$	.260	-.029
	(.232)	(.031)
Observations	539,080	539,080
R-squared	.734	.795
Mean of dep.	5.716	0

Notes: Unit of observation is the parcel-monitor-year. Dependent variables in columns 1 and 2 are the level of COD (Chemical Oxygen Demand) and summary pollution index that averages together all seven pollutants measured at each monitoring station, respectively. The summary index has been standardized to be mean 0 and standard deviation 1. All regression specifications presented here control for monitor-land pair fixed effects and river basin by year fixed effects. Robust standard errors two-way clustered by monitoring station and land parcel appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.17: Effects of Industrial Activities on Water Pollution – Alternative Measures

VARIABLES	Std. BOD	Std. NH	Std. Petroleum	Std. Phenol	Std. Mercury	Std. Lead
	(1)	(2)	(3)	(4)	(5)	(6)
Up_Monitor × POST	.055	.008	.015	.038*	.036	.082**
	(.046)	(.026)	(.047)	(.022)	(.037)	(.037)
Down_Monitor × POST	-.035	.002	-.002	.007	.041	-.031
	(.030)	(.021)	(.022)	(.024)	(.026)	(.029)
Observations	539,080	539,080	539,080	539,080	539,080	539,080
R-squared	.686	.899	.667	.497	.472	.497
Mean of dep.	0	0	0	0	0	0

Notes: Depend variables in columns 1–6 are the levels of BOD (Biological Oxygen Demand), NH (Ammonia Nitrogen), Petroleum, Phenol, Mercury, and Lead, respectively. Each pollutant has been standardized to be mean 0 and standard deviation 1 by subtracting the pollutant-specific mean and dividing by the standard deviation. All regression specifications presented here control for monitor-land pair fixed effects and river basin by year fixed effects. Robust standard errors two-way clustered by monitoring station and land parcel appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.18: Land Prices – Excluding Native City Leaders

VARIABLES	log (avg price)			log (weighted avg price)		
	(1)	(2)	(3)	(4)	(5)	(6)
UpHome	0.143***	0.144***	0.142***	0.111**	0.113**	0.111**
	(0.046)	(0.046)	(0.044)	(0.045)	(0.045)	(0.044)
Observations	6,333	6,333	6,333	6,333	6,333	6,333
R-squared	0.886	0.886	0.887	0.883	0.883	0.884
Mean of dep.	4.901	4.901	4.901	4.888	4.888	4.888

Notes: The depend variables in columns 1–3 and 4–6 are the log of average land price and the log of size-weighted average land price in each area and year, respectively. This sample excludes city-years where the city leader is a native. Area fixed effects and city × year fixed effects are included in all regressions. Columns 2 and 4 further include annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway. Columns 3 and 6 additionally control for a cubic polynomial in cumulative land supply up to the previous year. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.19: Land Prices – Alternative Radii

VARIABLES	15-km radius		25-km radius	
	ln (avg price)	ln (weighted avg price)	ln (avg price)	ln (weighted avg price)
	(1)	(2)	(3)	(4)
UpHome	0.091*	0.070	0.142***	0.104**
	(0.054)	(0.060)	(0.044)	(0.043)
Observations	5,016	5,016	7,925	7,925
R-squared	0.879	0.876	0.877	0.872
Mean of dep.	4.933	4.923	4.868	4.854

Notes: Depend variables in columns 1 & 3 and columns 2 & 4 are the log of average land price and the log of size-weighted average land price in each area and year, respectively. The unit of observation, area, is defined by a 15-km radius from rivers' exit point in columns 1-2 and by a 25-km radius in columns 3-4. All regressions control for area fixed effects, city × year fixed effects, annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.20: Placebo – Supply of Commercial and Residential Land

VARIABLES	Any Supply	log Supply	$sinh^{-1}$ Supply	$sinh^{-1}$ Parcels
	(1)	(2)	(3)	(4)
	Panel A: Commercial Land			
UpHome	0.016	-0.050	-0.014	-0.013
	(0.019)	(0.049)	(0.039)	(0.079)
Observations	17,351	17,351	17,351	6,030
R-squared	0.677	0.738	0.779	0.844
Mean of dep.	.348	.616	.708	6.191
	Panel B: Residential Land			
UpHome	0.023	0.042	0.043	0.006
	(0.018)	(0.063)	(0.052)	(0.075)
Observations	17,351	17,351	17,351	6,730
R-squared	0.736	0.820	0.833	0.850
Mean of dep.	.388	.950	.982	6.330

Notes: Depend variables in columns 1–4 are a dummy equal to one if an area receives any land supply, the inverse hyperbolic sine of total quantity of land, the inverse hyperbolic sine of total number of land parcels, and the log of size-weighted average land price in each area and year, respectively. Results for commercial land and residential land are presented in Panel A and Panel B, respectively. All regressions control for area fixed effects, city × year fixed effects, annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.21: Land Prices – Office Term Level Analysis

VARIABLES	ln (avg price)			ln (weighted avg price)		
	(1)	(2)	(3)	(4)	(5)	(6)
UpHome	0.138**	0.142**	0.137**	0.094*	0.102*	0.098*
	(0.059)	(0.059)	(0.058)	(0.056)	(0.056)	(0.056)
Observations	2,805	2,805	2,805	2,805	2,805	2,805
R-squared	0.903	0.904	0.904	0.903	0.903	0.904
Mean of dep.	4.848	4.848	4.848	4.828	4.828	4.828

Notes: This table reports estimates from the area × office-term level regressions. The depend variables are average land prices within a given area and a leader's office term. In Columns 1–3 and 4–6 are the log of average land price and the log of size-weighted average land price, respectively. Area fixed effects and office term fixed effects are included in all regressions. Columns 2 and 4 further include average levels of rainfall and temperature, standard deviations of rainfall and temperature during each office term, and log distance to the nearest highway. Columns 3 and 6 additionally control for a cubic polynomial in cumulative land supply up to the previous office term. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.22: Nighttime Light Intensity – Alternative Measures

VARIABLES	1(light intensity > 0)		
	10-km	15-km	20-km
	(1)	(2)	(3)
Exposure over previous years	-0.017***	-0.009*	-0.005
	(0.006)	(0.005)	(0.004)
Observations	22,047	22,047	22,047
R-squared	0.818	0.810	0.807
Mean of dep.	.702	.787	.842

Notes: Dependent variable is a dummy equal to one if the average light intensity is positive in a given year. The key explanatory variable is the number of years over the previous three years that an area is upstream of city leaders' hometowns. The unit of observation, area, is defined by a 10-km, 15-km, and 20-km radius from each river' exit point in columns 1–3, respectively. All regressions control for area fixed effects, city $\times$ year fixed effects, annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.23: Robustness – Cities Worked Before

VARIABLES	Quantity				Price	
	Any Supply	log Supply	$sinh^{-1}$ Supply	$sinh^{-1}$ Parcels	log avg price	log weighted avg price
	(1)	(2)	(3)	(4)	(5)	(6)
UpHome	-0.058***	-0.307**	-0.227***	-0.174***	0.144***	0.108**
	(0.021)	(0.145)	(0.079)	(0.056)	(0.047)	(0.045)
UpWork	-0.003	0.087	0.008	0.015	-0.000	0.027
	(0.017)	(0.113)	(0.060)	(0.043)	(0.037)	(0.037)
Area FE	✓	✓	✓	✓	✓	✓
City-Year FE	✓	✓	✓	✓	✓	✓
Observations	17,351	6,573	17,351	17,351	6,573	6,573
R-squared	0.731	0.850	0.817	0.850	0.880	0.878
Mean of dep.	.379	2.593	1.281	.946	4.901	4.889

Notes: Depend variables in columns 1–6 are a dummy equal to one if an area receives any land supply, the log of total quantity of land (in hectare), the inverse hyperbolic sine of total quantity of land (in hectare), the inverse hyperbolic sine of total number of land parcels supplied in an area, the log of average land price, and the log of size-weighted average land price, respectively. UpWork is a dummy equal to one if an area is upstream of a city where the current leader has worked before. All regressions control for area fixed effects, city $\times$ year fixed effects, annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.24: Robustness – Alternative Definition

	Quantity				Price	
VARIABLES	Any Supply	log Supply	$sinh^{-1}$ Supply	$sinh^{-1}$ Parcels	log avg price	log weighted avg price
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: No additional controls						
UpHome	-0.062**	-0.304*	-0.217**	-0.178***	0.134***	0.108**
	(0.025)	(0.178)	(0.090)	(0.062)	(0.050)	(0.051)
Panel B: with additional controls						
UpHome	-0.061**	-0.297*	-0.217**	-0.177***	0.133***	0.109**
	(0.025)	(0.179)	(0.090)	(0.062)	(0.050)	(0.051)
Area FE	✓	✓	✓	✓	✓	✓
City-Year FE	✓	✓	✓	✓	✓	✓
Observations	17,351	6,573	17,351	17,351	6,573	6,573
Mean of dep.	.379	2.593	1.281	.946	4.901	4.889

Notes: UpHome is a dummy equal to one if an area is immediately upstream of city leaders' hometowns. Regressions in panel A control for area fixed effects and city × year fixed effects. Regressions in panel B additionally control for rainfall, temperature, and log distance to the nearest highway. Robust standard errors clustered at the area level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 1.25: Robustness – Alternative Clustering of Standard Errors

	Quantity				Price	
VARIABLES	Any Supply	log Supply	$sinh^{-1}$ Supply	$sinh^{-1}$ Parcels	log avg price	log weighted avg price
	(1)	(2)	(3)	(4)	(5)	(6)
UpHome	-0.058	-0.289	-0.225	-0.169	0.144	0.114
s.e. clustered by:						
city × year	(.018)***	(.125)**	(.061)***	(.041)***	(.037)***	(.040)***
city	(.022)***	(.137)**	(.090)**	(.063)***	(.049)***	(.046)**
city & year (2 way)	(.020)**	(.117)**	(.085)**	(.057)**	(.057)**	(.057)*
Observations	17,351	6,573	17,351	17,351	6,573	6,573
R-squared	0.731	0.850	0.817	0.850	0.880	0.878
Mean of dep.	.379	2.593	1.281	.946	4.901	4.889

Notes: This table reports the standard errors clustered at different levels for the main results. Depend variables in columns 1–6 are a dummy equal to one if an area receives any land supply, the log of total quantity of land (in hectare), the inverse hyperbolic sine of total quantity of land (in hectare), the inverse hyperbolic sine of total number of land parcels supplied in an area, the log of average land price, and the log of size-weighted average land price, respectively. All regressions control for area fixed effects, city × year fixed effects, the annual average levels of rainfall and temperature, standard deviations of rainfall and temperature, and log distance to the nearest highway (***p<0.01, **p<0.05, *p<0.1).

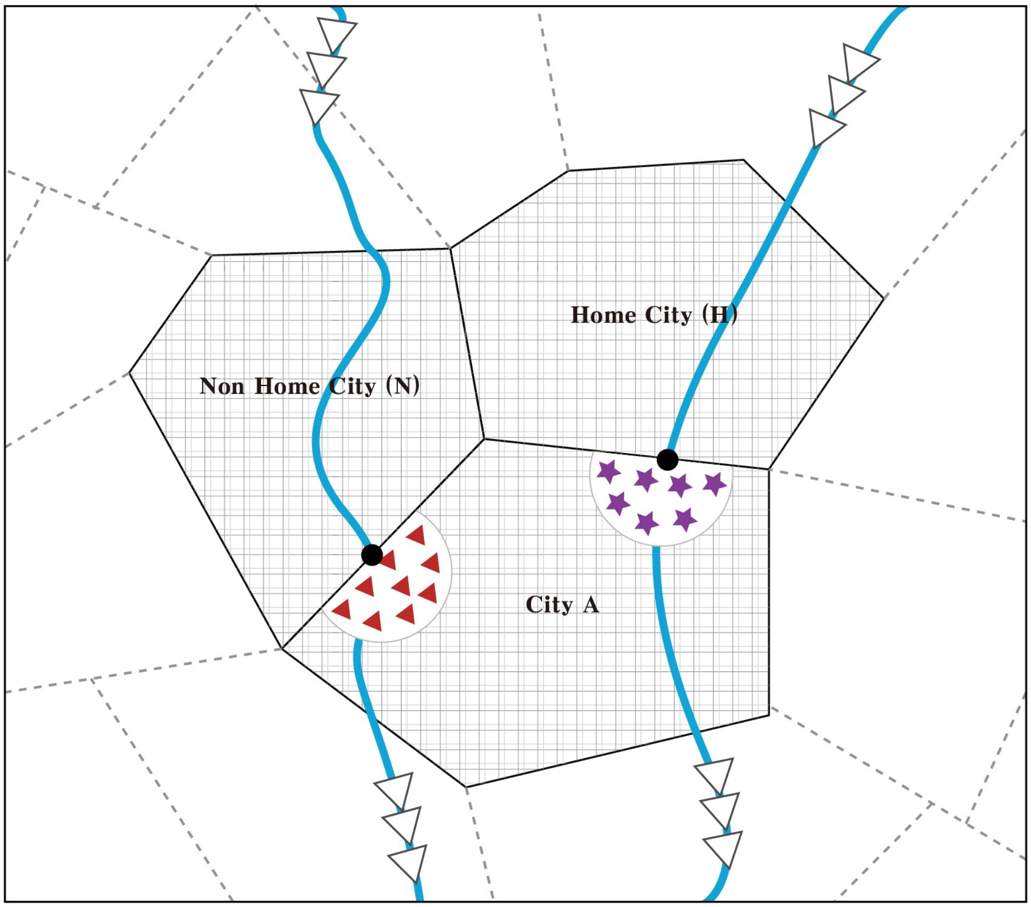


Figure 1.4: Illustration of Placebo Test

Note: This figure illustrates the idea of the placebo test. The black dots represent the points through which rivers enter the jurisdiction city A. The stars (triangles) represent industrial land parcels supplied within a radius of each entering point in the jurisdiction. The placebo test compares quantity and price of industrial land supplied to areas **downstream** of the jurisdiction city leader's hometown vs. areas **downstream** of non-hometowns, during and after the current leader's term.

Figure 1.5: An Example of Land Transaction Record

Source: The Land Transaction Monitoring System of China, http://www.landchina.com/.

1.9 Appendix: Additional Model Details

1.9.1 Predictions

The Lagrange equation for leader's maximization problem can be expressed as follows, where λ represents the shadow value of industrial land.

$$\mathcal{L} = \sum_{j=0}^{N}(a_j - Q_j - c)Q_j - \mu Q_0 - \lambda(L - \sum_{j=0}^{N} Q_j)$$

Solving the leader's first order conditions yields:

$$Q_0 = \begin{cases} 0 & \text{if } a_0 < \lambda + \mu + c \\ \dfrac{a_0 - \lambda - \mu - c}{2} & \text{if } a_0 \geq \lambda + \mu + c \end{cases} \qquad Q_{j\neq 0} = \begin{cases} 0 & \text{if } a_j < \lambda + c \\ \dfrac{a_j - \lambda - c}{2} & \text{if } a_j \geq \lambda + c \end{cases}$$

and

$$P_0 = \frac{a_0 + \lambda + \mu + c}{2}, \qquad P_{j\neq 0} = \frac{a_j + \lambda + c}{2}$$

Taking expectations of the quantities and prices yields the following predictions:

1. City leaders are less likely to sell land for industrial projects in upstream areas ($j = 0$). This can be seen from the following inequality

$$Pr\{Q_0 > 0\} = 1 - \Phi(\lambda + \mu + c) < Pr\{Q_{j\neq 0} > 0\} = 1 - \Phi(\lambda + c)$$

2. Conditional on the positive supply ($Q_j > 0$), the quantity of land supplied to upstream areas is less than other areas.

$$\begin{aligned} \mathbb{E}\left[Q_0 \mid Q_0 > 0\right] &= \mathbb{E}\left[\frac{a_0 - (\lambda + \mu + c)}{2} \mid a_0 > \lambda + \mu + c\right] \\ &< \mathbb{E}\left[Q_{j\neq 0} \mid Q_j > 0\right] = \mathbb{E}\left[\frac{a_j - (\lambda + c)}{2} \mid a_j > \lambda + c\right] \end{aligned}$$

3. Average land prices are higher in upstream areas.[42]

[42] The function $\mathbb{E}\left[X - \theta \mid X > \theta\right]$ decreases and $\mathbb{E}\left[X + \theta \mid X > \theta\right]$ increases in θ, when θ is positive and X follows the standard normal distribution.

$$\mathbb{E}\left[P_0 \mid Q_0 > 0\right] = \mathbb{E}\left[\frac{a_0 + (\lambda + \mu + c)}{2} \mid a_0 > \lambda + \mu + c\right]$$
$$> \mathbb{E}\left[P_{j\neq 0} \mid Q_j > 0\right] = \mathbb{E}\left[\frac{a_j + \lambda + c}{2} \mid a_j > \lambda + c\right]$$

1.9.2 Quantifying the Deadweight Loss

Leaders' hometown preferences lead to land misallocation to the extent that land is not always allocated to firms with the highest land valuation. City governments can increase land sale revenue and buyer surplus by increasing the supply to upstream areas and reducing the supply to other areas, holding the total supply constant. Figure 1.6 illustrates the lost market surplus in a simple case where land is misallocated between one upstream and one control area.

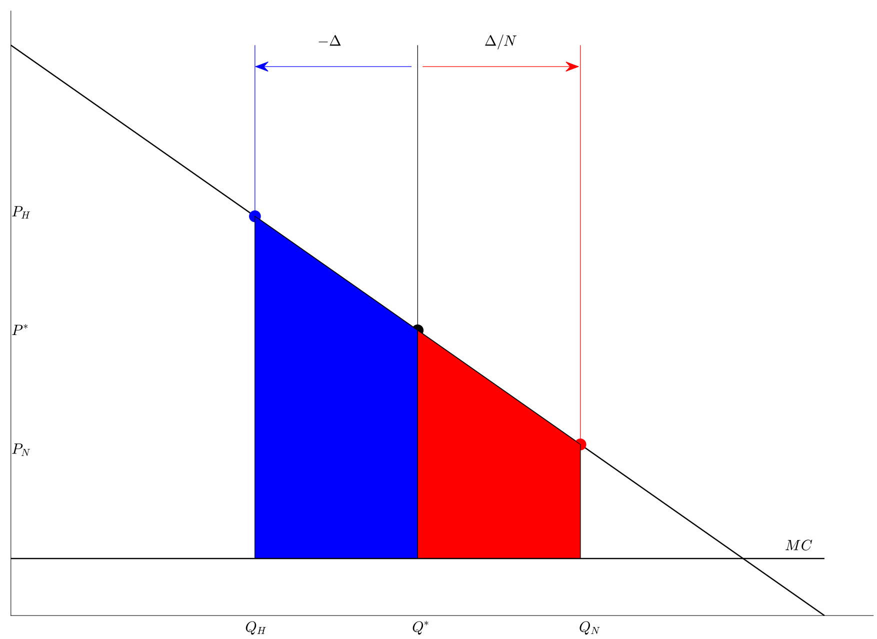


Figure 1.6: Changes in Market Surplus – Upstream vs. Control Area

Note: The blue region represents the *decrease* in surplus in an upstream area. The red region represents the *increase* in surplus in each of the other areas.

In the above figure, P^* and Q^* denote equilibrium price and quantity of land for each area when there are no hometown preferences. Total surplus is represented by the region below the demand curve and above the marginal cost, up to quantity Q^*. With hometown preferences, the supply to the upstream area shifts to the left by an amount denoted by Δ, and the equilibrium price and quantity become P_H and Q_H, respectively. Total surplus in the upstream area decreases by an amount represented by the blue region, which is equal to $\frac{1}{2}(P^* + P_H)\Delta$. Since there are N control areas, the expected quantity of supply will shift to the right by Δ/N. Equilibrium moves to (P_N, Q_N). Total surplus increases by an amount represented by the shaded red, which is equal to $\frac{1}{2}(P_N + P^*)\frac{\Delta}{N}$.

The net effect of hometown preferences on welfare is therefore given by

$$-\frac{1}{2}(P^* + P_H)\Delta + \frac{1}{2}(P_N + P^*)\frac{\Delta}{N} \times N = -\frac{1}{2}(P_H - P_N)\Delta$$

Note that the estimation strategy we leveraged in this paper allows us to causally estimate the price differential, $\Delta_P = P_H - P_N$, and the quantity differential $\Delta_Q = Q_H - Q_N$. Note also that the quantity differential between upstream areas and other areas, Δ_Q, is equal to $\Delta + \frac{\Delta}{N} = \frac{N+1}{N}\Delta$. Therefore, the average deadweight loss associated with each representative upstream area can be expressed by

$$DWL = -\frac{1}{2}\Delta_P \Delta_Q \frac{N}{N+1} \tag{1.5}$$

The following table shows the calculations for the deadweight loss. Evaluated at the mean price and quantity, the results show that the lost market surplus associated with each upstream area is roughly 1.55 million Chinese *yuan*. This loss corresponds to 1.6% of the average land sale revenue from each area.

This calculation does not consider the changes in environmental welfare for jurisdictional residents. This is because the total supply of industrial land in a given city and year is constant. Under the assumption that the costs of pollution are linear, the net change in environmental benefits should be zero since total amount of industrial activities is held constant through quotas. I adopt the linear pollution costs assumption since it is common in the environmental economics literature (Greenstone and Hanna, 2014; Lipscomb and Mobarak, 2016). If the cost of pollution is convex, the disparities in the spatial land allocation, caused by leaders' hometown preferences, will also lead to losses of environmental welfare. In that case, the estimated welfare losses will represent a lower bound.

Table 1.26: Quantifying the Lost Surplus

$dlogQ/dX$	-0.289**
$\Delta Q = Q_H - Q_N$ (ha)	-11.83**
$dlogP/dX$	0.143***
$\Delta P = P_H - P_N$ (RMB 1,000 per ha))	261.1***
DWL $= \frac{1}{2}(P_H - P_N)(Q_H - Q_N)\frac{N}{N+1}$ (Million RMB)	1.545**
DWL / PQ	0.016**

Notes: This table shows the calculations for the deadweight loss according to Equation 1.5. Estimates in Row 1 and Row 3 are drawn from Tables 1.3 and 1.9, respectively. The quantity and price differences between upstream and control areas are evaluated at their sample means. Standard errors for estimated quantity and price differences, and estimated deadweight loss, are constructed using the delta method (***p<0.01, **p<0.05, *p<0.1).

CHAPTER II

Education and the Meritocratic Recruitment of Bureaucrats

with Achyuta Adhvaryu

Do merit-based recruitment policies encourage individuals to climb socioeconomic ladders? We provide evidence from historical Taiwan, where elite bureaucrats were recruited through a civil service examination. Quotas for successful candidates were set based on the 1948 populations of individuals' native provinces in mainland China, resulting in highly preferential quotas for certain groups. This system was abruptly replaced in 1962 by a uniform admissions policy. We leverage this variation to study the impacts of differential access to elite bureaucratic posts. We find that the additional incentives created by preferential quotas increased human capital accumulation and resulted in better long-run economic outcomes.

2.1 Introduction

A well-functioning bureaucracy is vital for economic growth. Bureaucrats oversee the most essential responsibilities of the government – for example, the delivery of public goods and services, income redistribution, and regulatory enforcement (Acemoglu et al., 2011, 2015; Besley and Persson, 2009, 2010). The selection of bureaucrats is therefore critical to the success of these basic functions of the state (Rasul and Rogger, 2018; Rasul et al., 2018). Many countries have long histories of meritocratic recruitment of bureaucrats, often through the use of thresholds on years of schooling and scores on civil service examinations. China's system of exam-based recruitment, for example, was created more than 1,400 years ago under the Sui Dynasty.[1] It has served as the primary model for the civil service systems that later developed in India, Brazil, France, Germany, the United Kingdom, and the United States, among others.[2] There is general agreement that meritocratic recruitment is valuable to the state. The evidence for this fact has focused on one channel: merit-based recruitment enables the selection and promotion of candidates who are able to execute the responsibilities of the bureaucracy more honestly and efficiently (Weber, 1968; Evans and Rauch, 1999; Rauch, 1995; Besley, 2005).[3]

But there has always been another fundamental express purpose of meritocratic recruitment: to summon the aspirations – and ultimately to alter the choices – of common citizens to climb socioeconomic ladders. Indeed, this was one of the primary considerations in the design of the original Chinese civil service examination system (Ho, 1962; Hsu, 1949; Elman, 2000; Needham, 1969; Miyazaki, 1981).[4] Historians have argued that this system was one of the main drivers of the high levels of human capital observed in early modern China and East Asia (Rawski, 1979; Baten et al., 2010). Yet, despite its potentially central role in fostering upward social mobility,

[1]The civil service examination was institutionalized during the Song Dynasty (960–1276) and became the primary mechanism for the selection of bureaucrats under the Ming (1368–1644) and Qing (1644–1911) dynasties (Bai and Jia, 2016; Elman, 2000).

[2]The Chinese examination system existed in Japan, Korea, Ryukyu, as well as Vietnam (Liu, 2007). In later periods, France, Germany, and Great Britain adopted similar methods to select civil officials (Teng, 1943). Modeled after these previous adaptations, the United States established its own testing program for certain government jobs after 1883 (Huddleston and Boyer, 1996).

[3]In recent work, Serrato et al. (2019) argue to the contrary that perhaps despite meritocratic intentions, it may be hard for bureaucracies to monitor and reward performance well, leading to a persistent misreporting problem that attenuates the relationship between bureaucrat performance and promotion.

[4]In order to encourage young men to sit for the civil service exam, Emperor Zhenzong of the Song Dynasty – a well-known advocate of the system – penned a famous poem in which he asserts that "wealth and beauty can be gained through diligent study" of the Confucian Classics (Miyazaki, 1981).

no study to our knowledge has provided rigorous empirical evidence for the claim that the incentives created by merit-based recruitment actually cause human capital accumulation and improve economic outcomes.

This claim is not self-evident. The requirements for sitting a civil service exam are often highly stringent; this often effectively restricts access to the exam to the elite (Elman, 2000, 2009; Shiue, 2017). The exams themselves are also notoriously difficult – typically, a very small percentage of candidates who take the exam are actually selected to become bureaucrats.[5] These factors may on average dampen the incentive to invest in human capital. On the other hand, if the system does change human capital-related decision-making, the effects might be transformative in terms of creating better standards of living even for those who do not ultimately join the ranks of the bureaucracy.[6] In this way, public merit-based recruitment systems may have substantial spillovers onto productivity in the private sector.

We seek to answer these questions in the context of bureaucrat recruitment in Taiwan. The elite bureaucratic corps in Taiwan is recruited through a civil service exam. High school completion (or a diploma equivalent) is required in order to sit for the exam. Thus, the recruitment system creates an additional return to high school education that is implicitly a function of an individual's perceived probability of success in the exam (i.e., the probability of scoring above the cutoff and joining the elite civil service corps); the greater this probability, the higher the added return to high school completion.

Historically, success in the exam was determined by a quota system, which had its roots in the aftermath of the Chinese Civil War (Shiau, 2006; Hsu, 2013; Luoh, 2003). The Chinese Nationalist Party, or Kuomintang (KMT), which ruled mainland China in the early- to mid-20th century, was defeated by the Communist Party of China (CPC) and ousted from power in 1949, leading to the establishment of the People's Republic of China. Upon defeat, the KMT fled to the island of Taiwan, but continued to claim sole authority over all of China, despite territorial control of only Taiwan and a few small neighboring islands (Lin, 2011).

[5] For example, Bai and Jia (2016) estimate that at least 2 million men competed in each entry-level exam for about 30,000 government posts in the mid-Qing era. In 2016, over 1.1 million candidates competed in the Indian Civil Service Examination for just over a thousand posts. And 86,471 people signed up for the Taiwanese Civil Service Examination in 2019, out of which 5,811 candidates were selected.

[6] Even in Imperial China, where the civil service exam was essentially a test of knowledge of Confucian texts, failed candidates could apply the linguistic, literary, and other skills they had acquired by studying for the exam toward a variety of productive purposes; Elman (2000), for example, cites cases of failed candidates becoming physicians, pettifoggers, fiction writers, and teachers.

In part to lend legitimacy to its claim, the KMT continued allocating quotas for successful civil examination candidates based on the 1948 population levels of the candidates' (mainland) provinces of origin (i.e., before the "Great Retreat"). This method of quota allocation – along with a strictly enforced military blockade that prevented migration – resulted in substantial variation in the probability of exam success among residents of Taiwan. Since only those who resided in Taiwan could – and actually did – take the civil service exam, individuals whose native provinces had smaller fractions of the population migrate to Taiwan during the KMT's retreat from the mainland enjoyed less competition and larger *per capita* quota sizes. These test takers were therefore more likely to score above their province of origin's specific cutoff and consequently to be recruited into the elite bureaucracy.

This historical quota-based system was in place until 1962, when it was abruptly replaced with a nearly-uniform admissions system, in which the score cutoff for success was determined by the distribution of performance across all test takers regardless of province of origin (Luoh, 2003; Hsu, 2013; Chang, 2011).[7] This reform in effect equalized the formerly disparate probabilities of success, and thus erased the differential incentives to complete high school that previously existed across potential test takers. The reform was motivated by a public perception of unfairness, particularly in light of the extremely low *per capita* representation of native Taiwanese in the civil service (Hsu, 2013). There was no official government communication regarding this issue prior to the the policy change.

We leverage the pre-reform variation in *per capita* quota sizes, along with the sharp nature of the policy change to a nearly-uniform admissions system, to identify the human capital and longer-term economic consequences of changing the return to schooling. We hypothesize that if a higher chance of exam success encouraged individuals to invest more in education (and in particular in high school completion), we should expect that those whose native provinces enjoyed a larger number of *per capita* slots would be more likely to enroll in and complete high school before, but not after, the quota system reform of 1962.

The 1980 Taiwan Census contains detailed information on respondents' provinces of origin, which we use to assign quota sizes to individuals. We focus our analysis on children who were a few years younger or a few years older than 15 at the time of the reform, which is the age at which most students in Taiwan make the choice

[7]The admission is "nearly" uniform in the sense that when a province has no exam takers who score above the cutoff, the exam taker who obtains the closest score can be granted 10 points as a preferential treatment (out of a 100 points scale). This preferential treatment was provided to at most one candidate from each province (Civil Service Examinations Act, 1962).

of whether or not to enroll in high school.[8] We compare the impacts of quota size on outcomes for cohorts aged 10–14, who were poised to begin high school when the reform took place, with those of cohorts aged 16–20, who were older than the age of high school initiation at the time of the reform. Results are robust to the choice of a smaller window around age 15.

We find that exposure to a preferential *per capita* quota (and thus a higher probability of success in the civil service recruitment process) indeed changed individuals' incentives to invest in schooling. Our estimates suggest that a one standard deviation increase in the *per capita* quota size increased the likelihood of high school completion by about 1.4 percentage points for men, corresponding to a 3.9 percent change at the mean completion rate.[9] Results on secondary school completion and high school initiation are analogous to impacts on high school completion.[10] College initiation and completion also respond to preferential quotas: results show approximately the same magnitude impacts on these outcomes as estimated for high school initiation and completion. Impacts on human capital accumulation beyond high school completion, which was the minimum requirement for sitting the civil service exam, may be due to the fact that a college degree enabled entry into a higher tier of the civil service, generating additional incentives to initiate and complete college (Civil Service Examinations Act, 1948, 1952).[11]

We find that the lifecycle human capital impacts of preferential quotas are substantially larger for men from more advantaged families, as proxied by father's education level. These heterogeneous impacts emphasize that one key mechanism through which seemingly meritocratic admissions systems may nevertheless be discriminatory is through the barriers – information, credit constraints, etc. – that keep disadvantaged students from participating and succeeding in the examination process. These

[8] Average school enrollment in our sample drops 10 percentage points (21.5%) from the last year of secondary school to the first year of high school, indicating that this age is indeed a pivotal decision point for students with regard to continued investment in education.

[9] Though women were allowed to sit for the civil service exam, only about 4 percent of successful exam candidates in Taiwan at this time were women (Statistical Yearbook of Civil Service Examination, various years). The low equilibrium presence of women in the civil service suggests that women's incentives to invest in schooling likely did not vary much with quota size at this time. In addition, as will be detailed in Section 1.4, the accurate information on women's native provinces are not available since women were registered under their husbands' province of origin. For these reasons, we restrict our sample to men.

[10] 95 percent of students who initiate high school complete it in our sample; the more meaningful variation in decision-making is with respect to high school initiation.

[11] The experience of high school could also have decreased the utility cost of continuing schooling, or generated learning about costs and/or benefits that caused individuals to revise their human capital decisions.

findings echo results from work in the United States (Hastings and Weinstein, 2008; Dynarski and Scott-Clayton, 2006; Bettinger et al., 2012).

Finally, we study impacts on longer-term labor and marriage market outcomes. Preferential quotas increased the probability of salaried employment by 1.6 percentage points, and changed occupational income scores by 2.7 percent. The probability of marriage increased by approximately 4 percent. The age at marriage increased and number of children decreased, both in fairly small amounts. Men were more likely to marry women who had completed high school and who worked in the formal sector. These trends point to an overall increase in labor- and marriage-related outcomes as a result of the incentives created by the quota system.

In sum, the incentives created by preferential quotas caused substantial increases in human capital accumulation for exposed cohorts, as well as improved long-run economic outcomes. These changes are quite large compared to the change in the expected probability of passing the civil service exam induced by preferential quotas. Back of the envelope calculations show that the probability of exam success went from 0.65% before the reform to 0.08% afterward for the average migrant from mainland China. According to our estimates, this change in probability results in a 5.9 percentage point decline in the probability of high school initiation. This calculation suggests either that the value (inclusive of non-pecuniary benefits) of employment in the civil service is very high, or that human capital responses were perhaps irrationally large. This finding is consistent with other work on the often very large investment responses to changes in labor demand, particularly related to high skill sectors (Shrestha, 2016; Khanna, 2019; Oster and Steinberg, 2013).

We seek to make two main contributions with this work. This study is the first to our knowledge to assess whether meritocratic recruitment systems perform one of their basic express purposes: to raise the level of human capital and alter socioeconomic trajectories in the population. This channel is particularly important given the crucial role of human capital in the process of economic growth (Becker and Woessmann, 2009; Squicciarini and Voigtländer, 2015; Gennaioli et al., 2012). While this idea has been explored extensively in other academic areas, rigorous empirical evidence evaluating this claim has thus far been lacking (Baten et al., 2010; Shiue, 2017; Rawski, 1979). Our results complement previous work on the causes and consequences of bureaucrat selection mechanisms (Xu, 2017; Rauch, 1995; Rauch and Evans, 2000; Bai and Jia, 2016). We are also the first to provide evidence that the incentives created by public sector recruitment systems can generate positive externalities for private sector productivity.

Second, basic economic theory predicts that the optimal investment in human capital is determined in part by the return to schooling (Becker, 1967; Ben-Porath, 1967; Weiss, 1995). Simply put, the higher this return is, all else equal, the more individuals should tend to invest in education. As self-evident as this prediction appears, in a world rife with information frictions, credit market failures, and other distortions, it is not clear that individuals' education choices can and do indeed respond to changes in the return to schooling (Abramitzky and Lavy, 2014). This prediction has also proven elusive to test empirically, primarily because observable and exogenous cross-sectional variation in the return to schooling, as well as sharp changes in this return, are – with the exception of a few well-done studies – difficult to obtain (Abramitzky and Lavy, 2014; Shrestha, 2016; Jensen, 2010). Moreover, no study to our knowledge has credibly identified whether changing the return to schooling affects long-run economic outcomes in addition to human capital accumulation. Our study aims to fill these gaps in the literature.

The rest of the paper is organized as follows. Section 2.2 describes the historical and institutional context. Section 2.3 describes the data. Section 2.4 discusses our identification strategy and addresses threats to internal validity. Section 2.5 presents the main results and robustness checks. Finally, section 2.6 concludes.

2.2 Context

2.2.1 Civil Service Exams in Modern China

The roots of bureaucrat recruitment through civil service examinations in China date to the sixth century (Elman, 2000). The Chinese system is the first and perhaps the most well-known such institution in world; indeed, the systems in use in many countries today are modeled after the Chinese example. The current iteration of this system was established in the 1930s under the Chinese Nationalist Party led by Chiang Kai-shek (Hsu, 2013; Shiau, 2006). Article 85 of the Constitution of the Republic of China stipulates the following: "Government officials shall be selected through a system of open, competitive examination. No person shall be appointed to a public office unless he has successfully passed such an examination" (The Constitution of the Republic of China, 1946).[12]

The bureaucratic corps of the central government was recruited through a national civil service examination. There were two examination tiers – junior (*pukao*) and senior (*gaokao*). Successful candidates from both tiers were recruited into the Party

[12] Please refer to https://china.usc.edu/constitution-republic-china-1946 for further detail.

bureaucracy. Those who passed the senior exam were appointed to more elite posts (Civil Service Examinations Act, 1948). The number of successful candidates (i.e., the cutoff for admission into the bureaucracy) was governed by a constitutionally mandated quota system, which facilitated balanced *per capita* representation at the province level. For both the junior and senior examinations, quotas were allocated to 38 provinces based on their population size according to the most recent census (Hsu, 2013; Luoh, 2003; Chang, 2011). Figure 2.1 depicts the relationship between quota size and population (in millions) for the case of the 1948 census.[13] Each black dot represents a province; the line of best fit through these points (obtained via OLS) is drawn in grey. As is clear from this figure, the R^2 of population is nearly 1, and the slope of the line is also equal to 1, meaning that each additional million population per province generates one extra bureaucrat selected from that province. This figure also underscores the highly selective nature of the process – that is, very few ordinary citizens are successful in being recruited into the bureaucracy.

Exam success was associated with substantial social and economic benefits. First, it ensured a prestigious and stable government job, which was especially valuable during periods of high uncertainty in the labor market generated by wartime. Wages were substantially higher for government employees compared to work in the private sector (Liu and Liu, 1988). Additional benefits were also more generous, including free health care, free education for children, and additional allowances and pensions (Lin, 2012). In addition to these, being a government official in a one-party state likely provided access to hidden income and additional economic opportunities, not only to bureaucrats themselves but also to their families and social networks (Li et al., 2012; Fang et al., 2019; Do et al., 2017).[14] Finally, Confucian norms assert that the most talented members of society can contribute to the greatest degree by working for and being loyal to the government. In keeping with these norms, elite government positions *per se* are associated with substantial social value (prestige, honor, etc.) (Wang, 2013).

2.2.2 Chinese Civil War and the Great Retreat

The government of the Republic of China, led by Chiang Kai-shek and the KMT, had fought intermittently with Mao Zedong's CPC since 1927. The end of World War

[13] Quota sizes are the same for junior and senior examinations. Here we depict the quota size for one examination.

[14] Notably, in Imperial China, clans often pooled resources to support promising individuals from poor families to take the civil exam, with an expectation of favorable treatment in the case an individual succeeded in becoming a bureaucrat (Wang, 2013; Bai and Jia, 2016).

II and the defeat of Japan in the second Sino-Japanese War served to intensify this conflict, fueled in part by a proxy war between the Soviet Union, which supported the CPC with munitions and access to newly liberated territory, and the United States, which provided support to the KMT. The CPC's relative position grew rapidly during this immediate post-war period, leading to several successful military campaigns. By April 1949, the CPC had captured Nanjing, the KMT-led government's capital city. On October 1, 1949, Mao officially established the People's Republic of China, proclaiming the CPC's sovereign authority over mainland China. Chiang and the KMT government, along with over one million troops and civilians, were forced to flee to the island of Taiwan, in what is now known as the Great Retreat (Lin, 2011; Yap, 2018). The KMT-led government in exile (with help from the United States) immediately set up a military blockade in the Taiwan Strait, which was very strictly enforced until the mid-1970s. Among other things, this meant that there was virtually no immigration to or emigration from Taiwan during this period (Statistical Abstract of the R.O.C., 1974).[15]

From its seat in Taipei, the KMT continued to assert its claim as the sole legitimate government of mainland China, despite having territorial control of only Taiwan and several small neighboring islands. In a bid to signal this legitimacy, the government continued allocating civil exam quotas to each mainland province based on the province's population in 1948 (just before the Great Retreat). This meant that while the quota system "fairly" represented province populations prior to the Great Retreat, to the extent that the provinces were unequally represented in the population who had fled the mainland in 1949, the system allocated disproportionately favorable quotas to migrants from underrepresented provinces. Note that native province of family members of migrants was determined by the province of origin of the male head of household – i.e., children were registered under their father's province of origin, regardless of their own place of birth; women were registered under their husbands' province of origin. This practice continued until 1992, at which time individuals' province of origin stopped being recorded, in part due to the intention of the Taiwanese government to solidify a separate national identity (Wang, 2005).

Appendix Table 2.10 reports the number and proportion of people migrating from each mainland province to Taiwan. As is evident from the table, there is substantial variation in terms of migrant size and proportion across provinces. For instance, Fujian, a mainland province situated directly opposite the island of Taiwan, had the

[15] The total number of migrants between Taiwan and mainland China was under 5000 in almost every year from the mid-1950s through the 1970s (Francis, 2011).

largest fraction of the population migrate (17,734 per million). In contrast, Anhui, an inland province, sent 1,986 migrants per million population to Taiwan. The 23 provinces with the smallest fractions sent on average only 346 migrants per million population. Figure 2.2 demonstrates the fraction of migrants to Taiwan in 1949 originating from each mainland Chinese province, with darker shades indicating larger fractions. One takeaway from this figure is that while distance to Taiwan certainly played an important role in determining the intensity of Great Retreat migration, it does not entirely explain the observed pattern. Coastal areas proximate to Taiwan, which were areas occupied later by the CPC, sent more migrants to Taiwan. Northwest China, a CPC stronghold far from Taiwan, saw little migration during the Great Retreat. But inland areas of South China exhibit substantial variation in migrant shares.

In sum, individuals whose native provinces had smaller fractions of people who migrated to Taiwan enjoyed less competition and larger civil exam quotas per capita (Appendix Table 2.10). These individuals were therefore more likely to succeed in the exam and be recruited as government officials. Notably, native Taiwanese suffered the most from this unequal allocation system, as the military blockade ensured there were few Taiwanese migrating to mainland China.

Because the formal sectors of the economy were underdeveloped in Taiwan in this period, and because the national civil service examination provided an unusual opportunity to enter the elite bureaucrat class (which, as discussed earlier, proffered substantial pecuniary and non-pecuniary benefits), residents did indeed take part in the civil exam system.[16] From 1950 to 1970, a total of 221,067 residents registered for the examinations; 16,577 of these candidates succeeded, an average success rate of approximately 7.5% (Statistical Yearbook of Civil Service Examinations, various years).

A high school diploma or equivalent (*tong deng xue li*) was required in order to sit for the junior examination. High school graduates with three years of experience in the civil service and college graduates were additionally eligible to sit for the senior examination (Civil Service Examinations Act, the Republic of China (Taiwan), 1948, 1952). Of the 16,577 successful exam candidates between 1950 and 1970, 94 percent had a high school diploma and the remaining 6 percent had the test-based equivalent (Statistical Yearbook of Civil Service Examinations, various years). In our main

[16] According to the 1956 Population Census, only 51.7% of the working-age population (aged 15–64) had a job at the census time. Among those who had a job, only 21.2% worked in a formal private sector. Meanwhile, the government was a significant job provider; 15.5% of the workers worked in the public sector.

analysis, we focus on the decision to invest in a high school education, because this is the minimum requirement for taking (both the junior and the senior) civil service exam. Using the civil exam reform described in the next subsection, we test whether individuals who enjoyed larger *per capita* quota sizes are incentivized to complete higher school. While women were *de jure* eligible to take the civil service exam, *de facto* very few women actually did, particularly during the period we study (both in mainland China and Taiwan). Women accounted for about 4 percent of successful exam candidates in Taiwan during our sample period; this ratio was fairly stable until the Taiwanese Women's Movement in the early 1970s (Ku, 1988).

2.2.3 Civil Exam Reform of 1962

This quota-based admission system remained in place for more than a decade, until 1962, at which time the Taiwan government abruptly replaced it with a nearly uniform admissions system. The main motivation for this change was backlash caused by the unfair treatment of native Taiwanese, whose quotas were substantially smaller than those of migrants and their families. The score cutoff for admission into the civil service was, after 1962, set based on the performance of *all* exam takers, rather than set separately by native province (Hsu, 2013).

The one exception to this rule was that the cutoff score was lowered by ten points (out of a 100 points scale) for exam takers from native provinces from which no exam participant cleared the national cutoff. In addition, the lowered score cutoff could apply to at most one exam participant per province (Civil Service Examinations Act, 1962). Therefore, the amendment of the Civil Service Examinations Act in 1962 in effect equalized the formerly disparate probabilities of success, and thus erased the differential incentives to complete high school that previously existed across potential test takers.

The government did not disseminate any information about the new system prior to the policy change, and there was no other change to the civil exam process made concurrently with the change to uniform admissions. Taken together, these facts suggest that the change was unexpected and that studying decisions around this pivotal year can help us understand the impacts of the exam quotas on human capital investment and ensuing labor market outcomes.

2.3 Data

2.3.1 Data Sources and Variable Construction

Our main analysis combines province-level data on *per capita* quota size with the universe of data from the 1980 Taiwan Population Census, which is the earliest population census data available at the individual level in Taiwan. The Census was carried out by the Ministry of Interior in conjunction with the Population Census Office of the Executive Yuan of Taiwan. It covered all nationals and foreigners who had been living in Taiwan and its nearby islands (Kinmen and Matsu) for 6 months or more at midnight on a designated "census night," totaling 18,029,798 individuals. The 1980 Census is conducted by "personal interview," containing detailed information on gender, date of birth, place of residence, native Chinese province (*benji*), educational attainment (highest level attended and completed), marital status, age at marriage and number of children for married women, and employment status (industry and occupation).

We match this census data to data on quota size based on individuals' provinces of origin. Province of origin, or native province, is legally defined as the patrilineal home province of each respondent, which means that children's native provinces are inherited from their fathers, regardless of their own birth places (Household Registration Law, 1954).[17] For example, a resident whose father is a migrant from a mainland province would be registered under that mainland province as the native province, even if she were born in Taiwan. In contrast, women's (legally designated) native provinces could change throughout their life course (Luoh, 2003). For instance, women could legally change their native provinces to their husbands' native provinces after marriage.[18] Data from the 1980 census suggest that this practice was fairly common: 92.8% of married women reported the same native province as the male household head (though some of this may be assortative mating by province of origin, as well). Women could also revert back to their original native provinces after a divorce.[19]

Due to the lack of accurate information on women's native provinces and women's vastly underrepresented role in civil service during the sample period, in our main analysis we restrict our sample to men.[20] In order to minimize potential confounding

[17] Article 17, Household Registration Law of the R.O.C. (Taiwan), 1954.
[18] Article 17, Household Registration Law of the R.O.C. (Taiwan), 1954.
[19] Article 18, Household Registration Law of the R.O.C. (Taiwan), 1954.
[20] As noted earlier, women only accounted for 4 percent of the successful exam candidates from 1950–1970.

factors from later policy reforms,[21] we further restrict our attention to a relatively small window of birth cohorts, specifically, those aged 10–20 in the year of the reform (i.e. born between 1942 and 1952). These individuals are several years younger and older than the high school initiation age at the time of the reform. In addition, they are between 28 and 38 years old in the census year and thus have already completed education and have entered the labor market. These restrictions leave us with a sample size of 1,328,049.

One disadvantage of the 1980 Census is that it does not specify relationships (e.g. parent, spouse) between household members. In order to obtain controls for parental characteristics, we define a respondent's father to be the oldest male in the household who is 18–40 years older than that respondent. Under this definition, there are a total of 344,366 male respondents who are successfully linked to their fathers.[22] Finally, we drop 18 respondents for which the native province is missing and obtain a sample of 344,318 male respondents. This constitutes the primary sample used in empirical analysis.

The 1980 Taiwan Census also contains information on 3-digit occupations of working respondents. We use two IPUMS variables to proxy income by occupation, namely, the Occupational Status Score and Occupational Income Score. Both variables are average indicators by 3-digit occupational categories that were calibrated using data from the 1950 US Population Census. The occupational income score is the average by occupation of all reported labor earnings (Bleakley, 2007). The occupational status score is instead the average of "social standing" evaluated by respondents in a series of surveys for each occupation (Siegel, 1971).[23] Both variables are calculated and made available by IPUMS (Ruggles et al., 2019).

In order to study the effect of preferential quotas on marriage and other family outcomes, we define a (married) respondent's female partner to be a uniquely-matched married woman in the household who is 0–5 years younger or older than the respondent. A total of 162,736 male respondents in our sample are successfully matched to female partners in this manner. We then calculate the age at marriage and number of children for all matched male respondents.

Data on quotas allocated to each province are obtained from the *Statistical Year-*

[21] The extension of compulsory schooling from 6 to 9 years in 1968 (Chou et al., 2010), for instance.

[22] Using alternative age ranges yields similar results.

[23] Another commonly used income proxy is the Duncan Socioeconomic Index, which is a weighted average of earnings and education among males within each occupation (Duncan, 1961). We do not report results for this index since we already have separate measures for educational attainment. However, the results are similar when using the Duncan socioeconomic index; results available upon request.

book of Civil Service Examinations (*Kaoxuan Tongji Nianjian*), published by the Ministry of Examination of Taiwan in 1956. For descriptive purposes, we also collect data on the composition of exam candidates from this source for various years. Data on migrant populations from each mainland province are obtained from the *Report on the Population Census of Taiwan*, 1956 (*Zhonghua Minguo Hukou Pucha Baogaoshu*, 1956). The 1956 Census is the first population census conducted in Taiwan after the KMT government relocated to the island. We define *per capita* quota size to be the size of quotas allocated to each province divided by its population size in Taiwan in 1956.[24] To show that the quotas allocated to each province were indeed proportional to its population in 1948, we also collect data on provincial populations in 1948 from the *Yearbook of the Republic of China* (*Zhonghua Minguo Nianjian*), published in 1951 by the Ministry of Budget, Accounting and Statistics.[25]

2.3.2 Summary Statistics

Summary statistics on our variables of interest and principal controls for the primary sample (ages 10–20 at the time of the reform) are presented in Table 2.1. Respondents aged 6 years or above at the time of the census were asked a two-part question, in which respondents were asked to report the highest level of schooling they had attended, and whether or not they had completed that level of schooling. 48.1% of the sample had completed secondary school, but only 37.8% had ever initiated / attended high school. However, once high school was initiated, the vast majority of these students (95%) completed it. Together these statistics suggest that the most meaningful variation in this context is with respect to high school initiation. The college enrollment rate is 16.6%, and the rate of college graduation is 16%.

At the time of the census, sample respondents were aged between 28 and 38. 94.9% of the (male) sample were working, among whom 58.6% were working in a formal sectors position. The occupational income score ranges between 4 and 80, with an average of 23.1. The occupation status (prestige) score ranges between 12.4 and 81.5, with an average of 32.9. 74% of the sample have ever been married and the average age at marriage is about 25. Respondents had about 2 children. 21.8% of female partners had completed high school, and 26% were working in 1980.

8.8% of our sample are migrants or the descendants of migrants originating from mainland Chinese provinces. The fraction of migrants in this sample is lower than

[24]Our results are robust to the use of male population size, instead of total population size, as the denominator.

[25]The predecessor of the current Directorate-General of Budget, Accounting and Statistics of Taiwan.

that in the 1956 Census, where male migrants accounted for 12.5% of total male population. This is consistent with the fact that a disproportionate number of migrants are at prime ages. The statistics also show that there is considerable variation in *per capita* quota size across sample respondents; while the average quota is only about 56 per million, the standard deviation is roughly 1,164. The quota-induced variation in the probability of exam success, together with the variation induced by the 1962 exam reform, creates a unique opportunity to test whether better access to elite government posts actually incentivizes individuals to invest in education. We detail the estimation strategy and the empirical findings in the next section.

2.4 Identification Strategy

We identify impacts of preferential civil exam quotas by focusing on cohorts making the key decision to enter high school around the time of the 1962 reform. The intuition for our empirical strategy is that prior to the reform, students from native provinces that had particularly preferential exam quotas may have been differentially incentivized to pursue high school education compared to students whose native provinces had less preferential quotas, since high school completion (or a test-based equivalent) is required to sit for the exam. After the reform, this incentive would have been equalized given the change to a uniform score cutoff for admissions. We then make use of cohort differences based on the idea that the cohorts for which the change mattered the most in terms of high school initiation were those who were just finishing secondary school and deciding whether to enter high school. For slightly older cohorts, since that key time in which to decide whether to continue schooling had passed, the impact of the quota system change on schooling would be small. Since the prevailing age of high school initiation is 15 in Taiwan, in our main analysis we compare cohorts aged 10 to 14 at the time of the reform to cohorts aged 15 to 20 at the time of the reform.[26] Another advantage of this sample restriction is that all of the cohorts we analyze grew up on the island of Taiwan. Even migrants from the oldest cohort (those born in 1942) would have arrived in Taiwan at age 7. This equalizes the environment to a certain degree – all students were part of the same educational system, which mitigates potentially confounding effects of different socioeconomic conditions across native provinces.

As a robustness check, we also restrict attention to a smaller window of cohorts

[26] Compulsory schooling requires that children enroll in primary school by age 6. The majority of students thus complete secondary school at age 15.

aged 12 to 17 at the time of the reform, and results are essentially unchanged. This difference in differences strategy is summarized in the estimating equation below:

$$Y_{ipc} = \alpha + \beta \times \text{lnQuota}_p \times \text{Old}_c + \gamma \times X + \lambda_p + \mu_c + \epsilon_{ipc}$$

Here, Y_{ipc} represents either human capital investment (e.g., a dummy for high school initiation) or a labor market outcome for individual i, from native province p, born in year c. lnQuota_p is the log of *per capita* quota size for residents from province p; Old_c is an indicator for cohorts aged 15 or older in the year of the reform. Native province and (year-by-year) cohort fixed effects absorb the main effects of these two variables, and thus the coefficient β on the interaction term identifies the differential effects of preferential quotas. Standard errors are clustered at the province of origin level.

Also included (in X) are district of residence fixed effects, as well as father's education level and its interaction with Old_c. If provinces with smaller *per capita* quota sizes also had relatively lower educational attainment, their education levels might have grown at faster rates due to a catch-up effect, leading to an upward bias of the estimated effects of preferential quotas. Notably, native Taiwanese, who had the smallest per capita quota size, were less educated than the average migrant. To account for the potentially different trends based on initial education, we control for the fathers' years of schooling and allow it to have different effects for older and younger cohorts. District of residence fixed effects are further included in some specifications in order to account for regional variation in education resources.

Figure 2.3 provides some preliminary evidence in support of this identification strategy. In this figure, we use data from the 1980 Census of Taiwan to plot the proportion of each cohort between ages 12 and 20 who has ever attended (initiated) high school and who is currently attending high school (i.e., in 1980). Both patterns suggest that the majority of Taiwanese students initiate high school at the age of 15. Of those aged 14 in 1980, only 0.8% had ever attended high school. This proportion increases sharply to 35.7% for 15 year-olds, and then remains at roughly this number for cohorts aged 16 and older. The proportion of each cohort attending high school experiences a similar discrete jump at age 15. Additionally, the graph shows that the majority of high school students leave school at age 18. While 50% of those aged 17 are attending high school, this proportion drops to 26% for age 18 and further to 9.6% for age 19.

2.5 Results

We report results first on human capital investment, followed by labor market outcomes, and finally family- and partner-related outcomes.

2.5.1 High School Initiation

The decision to start high school is perhaps the most salient margin on which the quota change is hypothesized to have impacted schooling investment. Specifically, if the policy change to a uniform quota system did indeed affect human capital investment, this impact should have been largest for cohorts just under age 15 at the time of the change, given that 1) high school completion is required to sit for the exam, and 2) the evidence shown above on the timing of choices regarding initiating high school.

Table 2.3 reports impacts of preferential civil exam quotas on high school initiation. For ease of interpretation, in all tables reporting regression coefficients we have divided the log quota by its standard deviation, so that the coefficient can be interpreted in terms of standardized units. (Whenever the quota level is used instead of the log of the quota, this variable has also been standardized.) Across specifications, we find that a larger *per capita* quota significantly increases the probability of high school initiation. The estimated coefficients are statistically significant at the 1% level and remain stable in size across specifications (Columns 1–3). The estimates are also robust to the use of quota level instead of log quota (Columns 4–6) as the key explanatory variable. The estimated effects of preferential civil exam quotas on high school initiation are economically meaningful in magnitude. A one standard deviation increase in *per capita* quota size, which is about 1.2 slots per 1,000 population, leads to a 1.7–2.1 percentage point increase in the likelihood of high school initiation, corresponding to 4.5–5.5 percent of the sample mean.

To further understand the magnitude of this response, we translate the quota values into an approximate probability of exam success. We study the change (post- compared to pre-reform) in the average probability of exam success for migrants, who on average enjoyed highly preferential quotas prior to the reform. A back of the envelope calculation shows that the probability of exam success went from 0.65% before the reform to 0.08% afterward for the average migrant from mainland provinces.[27]

[27] We use data from 1956 to calculate the pre- and post-reform probabilities of exam success. We assume that all men aged 20–29 take the civil service exam. The pre-reform probability is calculated by diving the migrant quota size by the number of (potential) migrant candidates. The post-reform probability is the total quota size divided by the total number of potential candidates.

According to our estimates, this change in probability results in a 5.9 percentage point change in the probability of high school initiation (Appendix Table 2.11). This calculation suggests either that the value (inclusive of non-pecuniary benefits) of employment in the civil service is very high, or that human capital responses were perhaps irrationally large, as has been demonstrated in other settings (Shrestha, 2016; Khanna, 2019; Oster and Steinberg, 2013). To show the time path of these effects, we estimate a more flexible specification in which the *per capita* quota is interacted with (round year-specific) age dummies. The results are reported in Figure 2.4. This figure shows several important things. First, pre-trends are parallel, and the level difference in high school initiation prior to the reform (for older cohorts) is also approximately zero. These results help to validate the difference in differences strategy we employ. Second, impacts on high school initiation appear immediately and are fairly stable across younger cohorts (showing a slight but statistically undetectable increase in impact size from ages 14 to 10). This finding is consistent with the idea that the impact of changing the quota system on the decision to continue to high school should be internalized in the same way for all exposed cohorts.

We then conduct several robustness checks to test the sensitivity of these main results to the changing sample and specification. Table 2.4 replicates the analysis from Table 2.3 but using a smaller window of cohorts, ages 12–17. Impact estimates are slightly smaller (though not statistically so) than in the baseline analysis, but remain significant and stable in magnitude across all specifications. In Appendix Table 2.11, we replace the quota variable with a dummy for migrants from mainland provinces. We find that, the reform reduces the gap in high school initiation rate between migrants and native Taiwanese by about 12%.

Next we perform checks related to the potential for unobservables correlated with the size of province-level migrant populations (which is a determinant of the extent to which quotas were preferential). As described in Section 1.2, the fraction of migrants originating from mainland provinces was (among other things) a function of 1) geographical proximity to Taiwan, and 2) the order in which each province was occupied by the Communist Army. To control for the potentially differential trends among far-away and close-by provinces, in Panel A of Appendix Table 2.12, we add the straight-line distances between each province and Taiwan, interacted with the dummy for older cohorts as a control variable. Here, the distance variable is defined as zero for Taiwan. To control for the differences between provinces occupied early vs. late, in Panel B of Appendix Table 2.12, we add the order of occupation for each province, interacted with the dummy for older cohorts as a control variable in all regression models. (The order of occupation for Taiwan is obviously top-coded since it has never been occupied by the Communist Party government). The results of this analysis show that the estimated effects are similar to our baseline results both in size and precision.

As noted earlier, native Taiwanese suffered the most from the quota-based admission system and had the smallest per capital quota size for the civil service exams. Meanwhile, they might also be systematically different from migrants in other aspects. If their educational attainment follows a different trend than that of the migrants, the estimated effects of quota on educational attainment might be biased. To alleviate this concern, in Appendix Table 2.13, we exclude all native Taiwanese from our sample and estimate the main specifications using variation across mainland provinces only. We find that the estimated effects are largely unaffected. Under the baseline specification, the estimated coefficient is statistically significant at the 1% level, indicating that a one standard deviation increase in *per capita* quota size increases the likelihood of high school initiation by 1.6 percentage points. The estimates are robust to the use of quota level instead of the log quota as the explanatory variable, and remain statistically significant after we include the full set of controls. The findings suggest that the effect of preferential civil exam quotas on high school initiation are not fully driven by the differences between native Taiwanese and migrants from mainland provinces.

2.5.2 High School Completion

Next we study impacts of preferential quotas on high school completion. That is, we ask, to what extent did the increased rate of high school initiation as a result of preferential *per capita* quota lead to successful completion of high school (and thus actual eligibility for the civil service exam)? We estimate the same regression equation as before but with a dummy for high school completion (unconditional on having entered high school) as the dependent variable.

Results are reported in Table 2.5. We find that preferential quotas do indeed have a significant effect on high school completion – a 1.4 percentage point increase – and that this effect is nearly as large as the impact on high school initiation. Results are shown using a window of cohorts from 10 to 20 as well as from 12 to 17; results when using the 12 to 17 window are slightly smaller but still statistically significant. Overall the findings suggest that the increase in high school initiation generated by preferential quotas has been largely carried through to high school completion.

To study the time path of effects, we again plot coefficients on the interactions of (year-by-year) age dummies with the log of *per capita* quota size. These results are shown in Figure 2.5. Reassuringly, again we find stable pre-trends around 0 in older cohorts, and fairly rapid convergence (within two cohorts) to a stable quota effect for younger cohorts.

2.5.3 Lifecycle Human Capital Investment

Next we examine other human capital accumulation outcomes that may have been affected by the preferential quota system, namely, secondary school completion (for younger students who forecasted the relatively larger return to high school), college initiation, and college completion. The latter two outcomes were not requirements for sitting the civil service exam, but may have been affected by preferential quotas through several channels. First, a college degree enabled applicants to gain access to the "senior" (more elite) tier of the bureaucracy; thus preferential quotas could have created additional incentives to initiate and complete college education to gain access to this tier. Second, the experience of high school could have decreased the disutility of schooling such that continuing on to college would be less marginally costly. Third, initiating high school could have spurred more learning about the costs and/or benefits of schooling, which may have caused students to decide to revise their decisions about human capital accumulation. Through all of these channels it is possible that college education outcomes could have changed as a result of access to preferential quotas.

We report the results of these analyses – along with the previously reported impacts on high school initiation and completion – in Table 2.6.[28] We find that secondary school completion, college initiation, and college completion all respond to preferential quotas. The largest response is for completion of secondary school. College initiation and completion increase as well, each by about 1 percentage point (or about 5 percent at the mean) in response to a one standard deviation change in the log quota.

2.5.4 Heterogeneous Effects by Father's Education

We focus on one important dimension of heterogeneity in the impacts of preferential quotas: father's education level, a proxy for socioeconomic status. Ostensibly meritocratic admissions systems around the world have long been criticized for

[28]Throughout the remainder of the paper we report results using specification I (corresponding to columns 1 and 4 of Table 2.3); all results using specification III are reported in Appendix Table 2.14–2.16 and are robust to the inclusion of the additional controls used in this specification.

the barriers that prevent potentially high-achieving but disadvantaged students from gaining the same access as their more advantaged peers. For example, disadvantaged students might have less information about the exam's requirements or the probability of success; or greater credit or time constraints may prevent these students from preparing adequately for the exam. This would dampen the schooling response to changing quota incentives for disadvantaged students. Studying the extent to which this is true in the Taiwanese context offers insights into the meritocratic nature of bureaucrat recruitment in this case.

We construct a dummy for father's education level being less than the median, and interact this dummy with the difference in differences variables used in the baseline analysis (a dummy for age greater than 14 at the time of the reform, ln(Quota), and the interaction of these two). We show coefficient estimates for the double difference interaction and the triple interaction term (interaction with the dummy for below median father's education). Results are reported in Table 2.7. Across a variety of lifecycle schooling investment measures, we find muted responses (about one third smaller) for children of lower socioeconomic status, suggesting that meritocracy, as it is in many contexts, appears to have been imperfectly meritocratic in Taiwan.

2.5.5 Long Term Effects on Labor Market and Family Outcomes

Finally, we study impacts of preferential quotas on longer-term labor market and family outcomes. The hypothesis tested here is that the human capital investment generated by preferential quotas may have had returns in the labor and marriage market. We begin by looking at labor market outcomes in the census.

Results are reported in Table 2.8. Column 1 shows impacts on employment; we find a fairly precisely estimated 0 effect here, perhaps because there is little room for growth here – nearly 95 percent of the males in our sample are employed. Column 2 reports results on salaried (i.e., formal sector) employment, which accounts for about 60% of employment. Here we find a 1.6 percentage point increase (or 2.7 percent) as a result of preferential quotas. This is in part driven by differential occupational choice. In columns 3 and 4 we report results for two commonly used scores – Siegel's occupational status (or prestige) score (Siegel, 1971; Borjas, 1992) and occupational income score from IPUMS (Bleakley, 2007; Ruggles et al., 2019). These measures both improve as a result of preferential quotas: occupational status increases by about 2 percent, and occupational income increases by about 2.6 percent. Once again, the estimates are similar both in magnitude and significance level after the inclusion of additional controls, as shown in Appendix Table 2.15. This evidence suggests that there is indeed a labor market return to the additional schooling spurred by preferential quotas.

We then examine impacts of preferential quotas on marriage- and family-related outcomes. The idea here is that better education and labor market outcomes may have had returns in the marriage market, and may have influenced choices regarding marriage timing and fertility. Results of this analysis are reported in Table 2.9. Column 1 reports impacts on a dummy for whether the individual was ever (or is currently) married; we find that preferential quotas resulted in a 3.2 percentage point (or 4.3 percent at the mean) increase in the probability of marriage. Column 2 reports age at marriage. Here we find a precisely estimated positive impact – the coefficient is statistically significant and slightly less than 0.3 years; that is, the age at marriage was delayed on average by 3.6 months. Column 3 shows results for number of children. Once again, we find a precisely estimated impact: quotas decreased fertility by about 5 percent at the mean.

Column 4 reports results for female partners' high school completion rate. This is meant to shed light on a key question regarding marriage market impacts: do men with higher rates of high school and college completion, who have better labor market outcomes, match with better educated partners? The results suggest this is the case: wives' high school completion rate increases due to preferential quotas by 2 percentage points, a large increase of 9 percent at the mean.

Columns 5 and 6 investigate female partners' labor market outcomes, looking specifically at total employment and formal sector employment, respectively. Despite the fact that there is quite some room for growth in female employment in this context (the mean is 26.2 percent) we see small impact estimates precisely bound around 0. Conditional on having a job, we find that preferential civil exam quotas increased the likelihood of wives' formal employment by 3.4 percentage points (or 5.9 percent). Estimates in Column 7 and 8 echo this finding, suggesting a roughly 3 percent increase in wives' occupational status and occupation income due to preferential quotas. Overall, the findings point to positive assortative mating by social and economic status.

2.6 Conclusion

This study provides what is to our knowledge the first empirical answer to the question of whether meritocratic recruitment systems for bureaucrats accomplish one of their main intended purposes: to raise the level of human capital investment and ultimately the standard of living of the citizenry. We do this in the context of a unique natural experiment, in which the government of Taiwan allocated highly preferential quotas to migrants from mainland China in the decades following the Great Retreat, only to abruptly abandon this policy and replace it with a uniform quota system in the early 1960s. We use a difference in differences strategy that exploits the timing of the reform and differences in the preferential nature of the quotas by individuals' provinces of origin to estimate impacts on human capital levels and labor market and family outcomes.

The resulting changes in incentives for human capital accumulation (given that a high school degree was required to sit for the civil service exam) along with the high perceived benefits of joining the government bureaucracy generated a substantial human capital response to preferential quotas. Labor market outcomes, partner choice, and family outcomes respond as well. These impact estimates suggest that preferential quotas had substantial positive effects on standards of living, despite the fact that most candidates who compete for bureaucratic posts are unsuccessful.

In addition to demonstrating that meritocratic recruitment can indeed meaningfully affect human capital investment, the evidence presented here contains several lessons for policymaking. Preferential quotas are often used as a policy tool for affirmative action that allows for explicit targeting of disadvantaged groups based on observable characteristics. Our results suggest that this form of targeting, while it might be politically challenging (as the historical episode we analyze makes clear), is highly valuable for the targeted population in that the incentives generated by preferential quotas are internalized by the target group, and outcomes correlated with overall standards of living (labor force participation, income, etc.) respond positively.

2.7 Tables and Figures

Table 2.1: Summary Statistics

VARIABLES	Whole Sample {344,318} (1)	Exposed {99,014} (2)	Unexposed {245,304} (3)
		Educational Outcomes	
Secondary school completion	0.481	0.391	0.517
	(0.500)	(0.488)	(0.500)
High school initiation	0.378	0.287	0.414
	(0.485)	(0.452)	(0.493)
High school completion	0.359	0.272	0.394
	(0.480)	(0.445)	(0.489)
College initiation	0.166	0.117	0.186
	(0.372)	(0.322)	(0.389)
College completion	0.160	0.113	0.180
	(0.367)	(0.317)	(0.384)
		Labor Market Outcomes	
Currently working	0.949	0.957	0.946
	(0.220)	(0.203)	(0.226)
Salaried employment	0.586	0.516	0.615
	(0.492)	(0.500)	(0.487)
Occupational Income Score	23.10	21.81	23.63
	(10.71)	(10.82)	(10.61)
Occupational Status Score	32.866	31.47	33.44
	(12.97)	(12.99)	(12.92)

Notes: This table reports summary statistics for the sample of males aged 10–20 at the time of the reform, and for the exposed (aged 15-20) and unexposed exposed cohorts (aged 10-14) separately. Standard deviations appear in parentheses.

Table 2.2: Summary Statistics (*cont.*)

VARIABLES	Whole Sample {344,318} (1)	Exposed {99,014} (2)	Unexposed {245,304} (3)
		Family Outcomes	
Ever married	0.743	0.846	0.701
	(0.437)	(0.361)	(0.458)
Age at marriage	24.82	24.79	24.83
	(3.114)	(3.538)	(2.908)
Number of Children	2.132	2.877	1.805
	(1.611)	(1.640)	(1.484)
Wife has a high school degree	0.218	0.150	0.247
	(0.413)	(0.357)	(0.431)
Wife is currently working	0.262	0.289	0.250
	(0.440)	(0.453)	(0.433)
Wife is a salary earner	0.570	0.460	0.626
	(0.495)	(0.498)	(0.484)
		Explanatory Variables	
Quota per million	55.99	37.72	63.37
	(1,164)	(392.0)	(1,356)
log Quota per million	0.673	0.536	0.728
	(1.627)	(1.398)	(1.708)
Migrant status	0.088	0.064	0.097
	(0.283)	0.244	(0.296)
Father's years of schooling	5.223	4.415	5.549
	(4.219)	(4.127)	(4.212)

Notes: This table reports summary statistics for the sample of males aged 10–20 at the time of the reform, and for the exposed (aged 15-20) and unexposed exposed cohorts (aged 10-14) separately. Standard deviations appear in parentheses.

Table 2.3: Effects of Preferential Quotas on High School Initiation, ages 10–20

VARIABLES	High School Initiation					
	(1)	(2)	(3)	(4)	(5)	(6)
Age_{15} × ln(Quota)	.017***	.017***	.016***			
	(.002)	(.003)	(.003)			
Age_{15} × Quota				.021***	.017**	.017**
				(.006)	(.007)	(.007)
Observations	344,318	344,318	344,318	344,318	344,318	344,318
R-squared	.105	.226	.256	.105	.226	.256
Mean of dep.	.378	.378	.378	.378	.378	.378
Specification	I	II	III	I	II	III

Notes: The dependent variable is an indicator that takes the value 1 if an individual has ever attended (initiated) high school. Sample includes males aged 10–20 in the year of the reform 1962. Specification I controls for province of origin fixed effects and cohort fixed effects; specification II additionally controls for father's education and its interaction with an indicator for older cohorts; specification III further includes district of residence fixed effects. ln(Quota) and Quota are both standardized, such that the coefficients can be interpreted as impacts resulting from a one standard deviation change in *per capita* quota size. Robust standard errors clustered by province of origin appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 2.4: Effects of Preferential Quotas on High School Initiation, ages 12–17

VARIABLES	High School Initiation					
	(1)	(2)	(3)	(4)	(5)	(6)
Age_{15} × ln(Quota)	.010***	.012***	.011***			
	(.002)	(.003)	(.003)			
Age_{15} × Quota				.016***	.015**	.015**
				(.006)	(.006)	(.006)
Observations	170,201	170,201	170,201	170,201	170,201	170,201
R-squared	.097	.223	.255	.097	.223	.255
Mean of dep.	.361	.361	.361	.361	.361	.361
Specification	I	II	III	I	II	III

Notes: The dependent variable is an indicator that takes the value 1 if an individual has ever attended (initiated) high school. Sample includes males aged 12–17 in the year of the reform 1962. Specification I controls for province of origin fixed effects and cohort fixed effects; specification II additionally controls for father's education and its interaction with an indicator for older cohorts; specification III further includes district of residence fixed effects. ln(Quota) and Quota are both standardized, such that the coefficients can be interpreted as impacts resulting from a one standard deviation change in *per capita* quota size. Robust standard errors clustered by province of origin appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 2.5: Effects of Preferential Quotas on High School Completion

VARIABLES	High School Completion					
	Cohorts 10-20			Cohorts 12-17		
	(1)	(2)	(3)	(4)	(5)	(6)
$Age_{15} \times \ln(Quota)$	.014***	.015***	.014***	.007***	.009***	.009***
	(.002)	(.003)	(.003)	(.002)	(.002)	(.002)
Observations	344,318	344,318	344,318	170,201	170,201	170,201
R-squared	0.103	0.223	0.251	0.097	0.220	0.249
Mean of dep.	.359	.359	.359	.343	.343	.343
Specification	I	II	III	I	II	III

Notes: The dependent variable is an indicator that takes the value 1 if an individual has completed high school. Sample includes males aged 10–20 in the year of the reform for columns 1–3 and those aged 12–17 for columns 4-6, respectively. Specification I controls for province of origin fixed effects and cohort fixed effects; specification II additionally controls for father's education and its interaction with an indicator for older cohorts; specification III further includes district of residence fixed effects. ln(Quota) is standardized such that the coefficients can be interpreted as impacts resulting from a one standard deviation change in *per capita* quota size. Robust standard errors clustered by province of origin appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 2.6: Effects of Preferential Quotas on Schooling Decisions over the Life Cycle

VARIABLES	Secondary Completion	HS Initiation	HS Completion	College Initiation	College Completion
	(1)	(2)	(3)	(4)	(5)
Age_{15} × ln(Quota)	.022***	.017***	.014***	.009***	.008***
	(.002)	(.003)	(.003)	(.003)	(.003)
Observations	344,318	344,318	344,318	344,318	344,318
R-squared	.081	.105	.103	.103	.098
Mean of dep.	.481	.378	.359	.166	.160
Specification	I	I	I	I	I

Notes: Dependent variables in Columns 1–5 are an indicator for secondary school completion, high school initiation, high school completion, college initiation, and college completion, respectively. Sample includes males aged 10–20 in the year of the reform. All regression models presented in this table control for province of origin fixed effects and cohort fixed effects. ln(Quota) is standardized such that the coefficients can be interpreted as impacts resulting from a one standard deviation change in *per capita* quota size. Robust standard errors clustered at the native province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 2.7: Heterogeneous Effects by Father's Education

VARIABLES	Secondary Completion	HS Initiation	HS Completion	College Initiation	College Completion
	(1)	(2)	(3)	(4)	(5)
$\text{Age}_{15} \times \ln(\text{Quota})$	.017***	.015***	.013***	.011***	.009***
	(.001)	(.002)	(.002)	(.002)	(.002)
... $\times$ Below	-.003***	-.005***	-.005***	-.006***	-.006***
	(.0002)	(.0001)	(.0001)	(.0006)	(.0006)
Observations	344,318	344,318	344,318	344,318	344,318
R-squared	.150	.167	.162	.132	.125
Mean of dep.	.481	.378	.359	.166	.160
Specification	I	I	I	I	I

Notes: Dependent variables in Columns 1–5 are an indicator for secondary school completion, high school initiation, high school completion, college initiation, and college completion, respectively. Sample includes males aged 10–20 in the year of the reform 1962. All regression models presented in this table control for province of origin fixed effects, cohort fixed effects, and father's education. Robust standard errors clustered at the native province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 2.8: Effects of Preferential Quotas on Labor Market Outcomes

VARIABLES	Employed	Formal Employment	Occupation Status Score	Occupation Income Score
	(1)	(2)	(3)	(4)
$Age_{15} \times \ln(Quota)$	.002	.016***	.666***	.608***
	(.002)	(.002)	(.076)	(.066)
Observations	344,318	326,793	320,097	320,097
R-squared	.017	.036	.036	.034
Mean of dep.	.949	.586	32.87	23.10
Specification	I	I	I	I

Notes: Dependent variables in Columns 1–4 are an indicator for being employed at the census time, an indicator that equals one if an individual is working for pay in the formal sector, Siegel's occupational prestige score, and occupational income score constructed by the IPUMS, respectively. Sample includes males aged 10–20 in the year of the reform, 1962. All regression models presented in this table control for province of origin fixed effects and cohort fixed effects. Robust standard errors clustered at the native province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 2.9: Effects of Preferential Quotas on Family Outcomes

VARIABLES	Ever Married	Age at Marriage	Num of Children	Partner HS Diploma
	(1)	(2)	(3)	(4)
$Age_{15} \times \ln(Quota)$	.032***	.291***	-.111***	.020***
	(.002)	(.015)	(.009)	(.002)
Observations	344,318	162,732	162,732	162,732
R-squared	.061	.031	.151	.095
Mean of dep.	.743	24.82	2.132	.218
Specification	I	I	I	I
VARIABLES	**Partner Employed**	**Partner Formal emp.**	**Partner Occ. Status**	**Partner Occ. Income**
	(5)	(6)	(7)	(8)
$Age_{15} \times \ln(Quota)$	-.007*	.034***	1.037***	.600***
	(.004)	(.003)	(.140)	(.087)
Observations	162,732	42,595	42,413	42,413
R-squared	.016	.083	.061	.043
Mean of dep.	.262	.570	34.11	20.51
Specification	I	I	I	I

Notes: Dependent variables in Columns 1–8 are an indicator for being ever-married, age at marriage (conditional on being married), number of children at the census time, an indicator for whether female partner has completed high school, an indicator for female partner's employment status, and an indicator for whether female partner is working for pay in a formal sector (conditional on working), female partner's occupational status score, and female partner's occupational income score, respectively. Sample includes males aged 10–20 in the year of the reform 1962. All regression models presented in this table control for province of origin fixed effects and cohort fixed effects. Robust standard errors clustered at the native province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

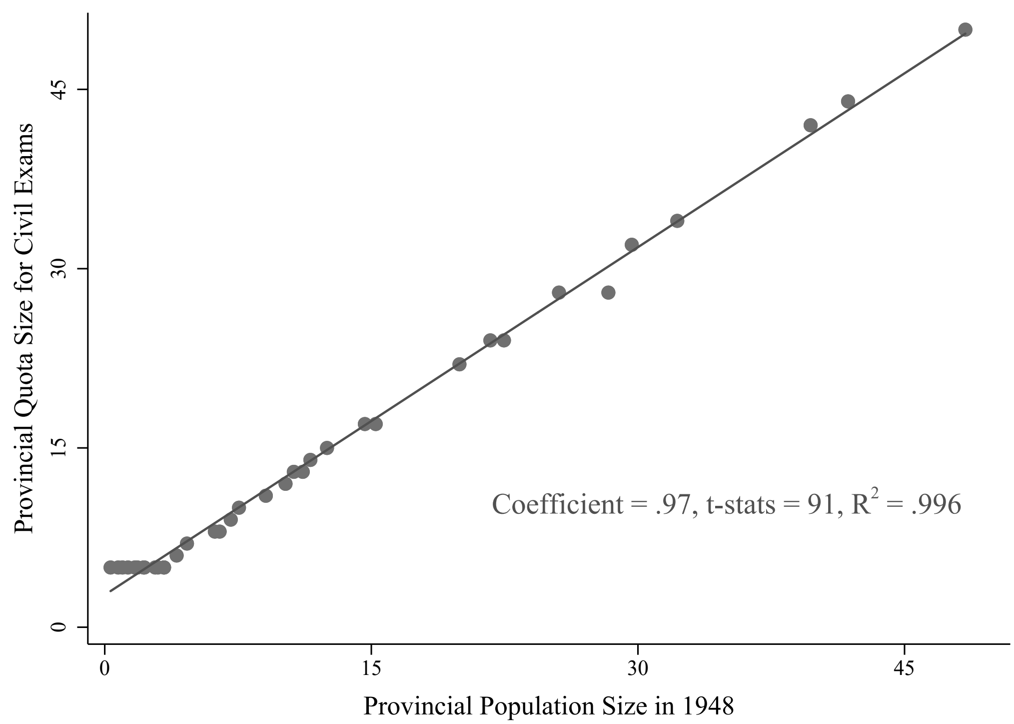


Figure 2.1: Population Size (in millions) and Civil Exam Quota Size by Province

Note: This figure shows the scatter plot and linear fit of provincial quota size and population in millions in 1948.

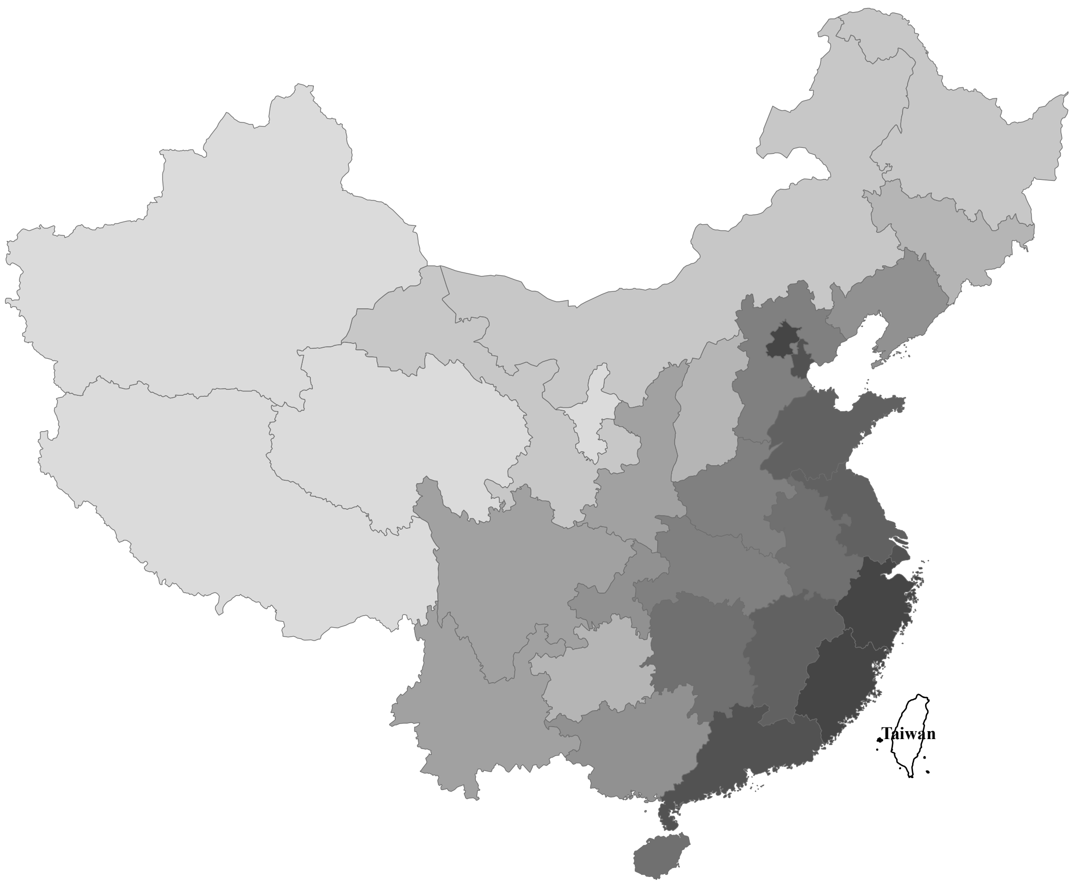


Figure 2.2: Fraction of People Migrating from Each Mainland Province to Taiwan

Note: This figure demonstrates the fraction of migrants to Taiwan originating from each mainland Chinese province. Darker shades indicate larger fractions of the population migrating from a mainland province.

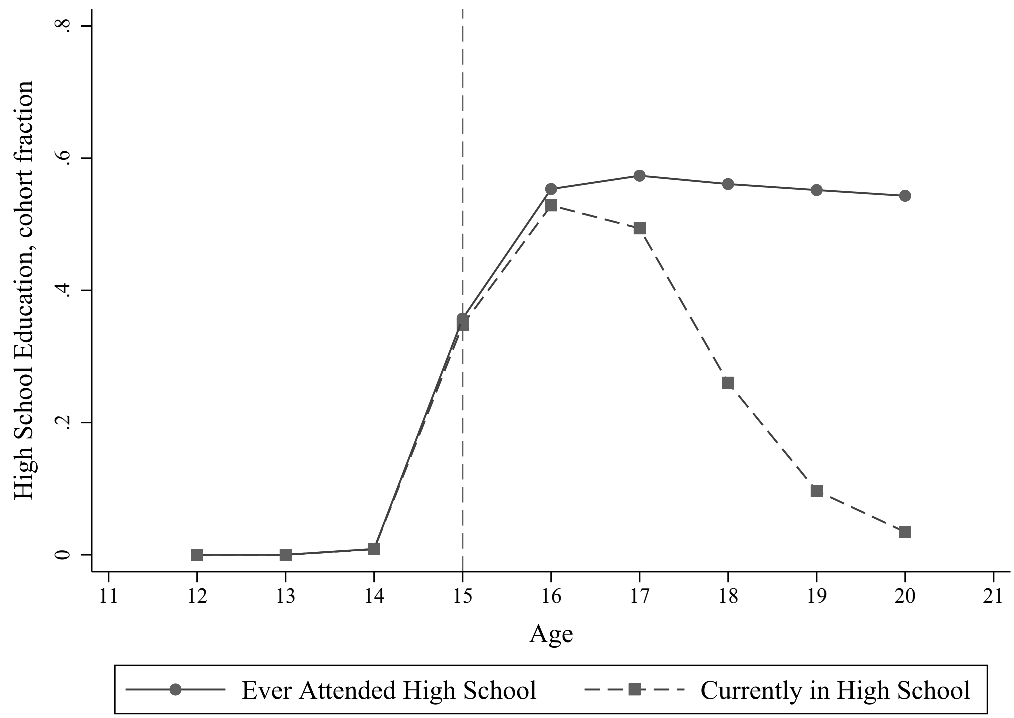


Figure 2.3: Fraction of High School Attendance by Age

Note: This figure uses the 1980 Taiwan Population Census to plot the proportion of each cohort between ages 12 and 20 who has ever attended high school (in solid line) and who is currently attending high school (in dashed line).

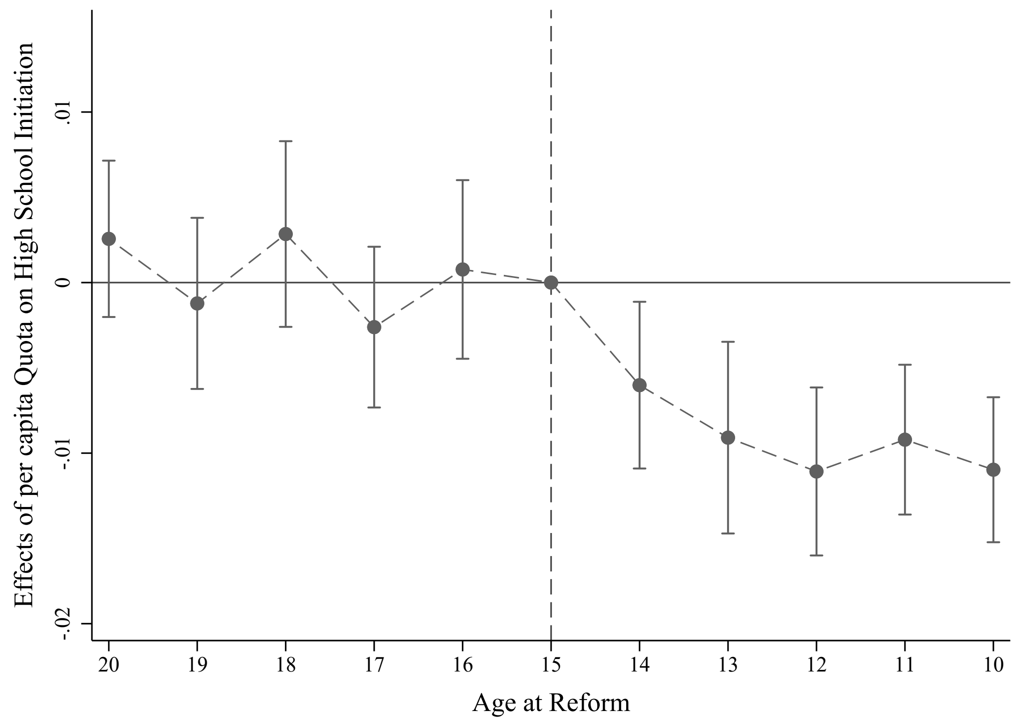


Figure 2.4: Effects of Preferential Quotas on High School Initiation

Note: This figure plots coefficient estimates and 95% confidence intervals for the log of *per capita* quota interacted with cohort dummies. Ages in the year of the reform are indicated on the x-axis. The dependent variable is an indicator for high school initiation. The estimating equation controls for province of origin fixed effects, cohort fixed effects, district of residence fixed effects, father's years of schooling and its interaction with an indicator for older cohorts. Standard errors are clustered by province of origin.

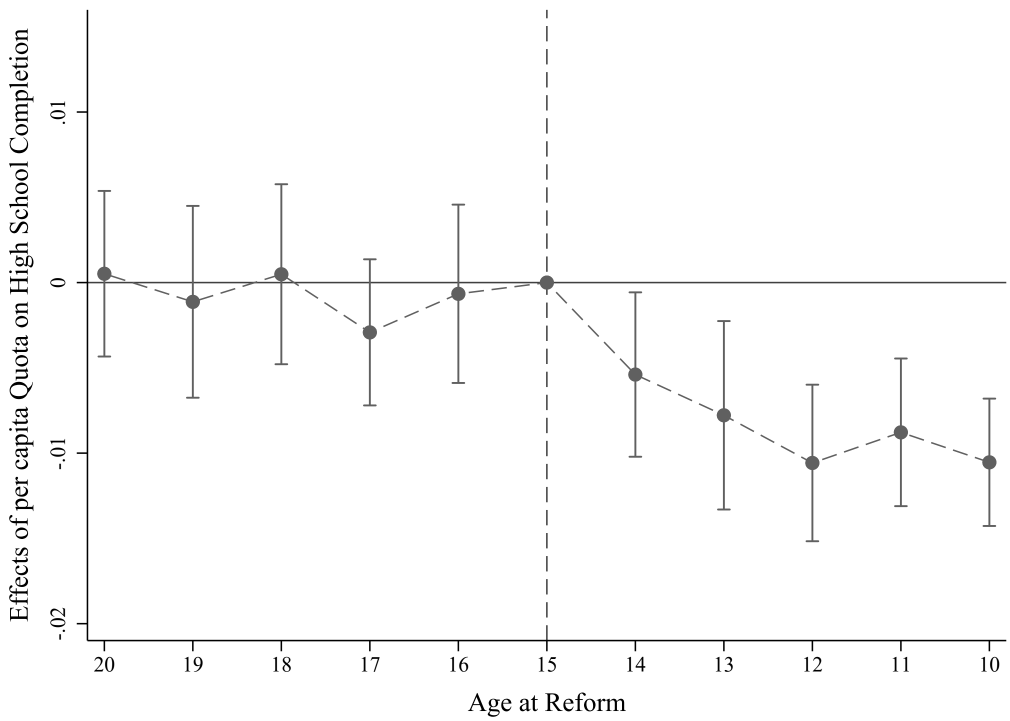


Figure 2.5: Effects of Preferential Quotas on High School Completion

Note: This figure plots coefficient estimates and 95% confidence intervals for the log of *per capita* quota interacted with cohort dummies. Ages in the year of the reform are indicated on the x-axis. The dependent variable is an indicator for high school completion. The estimating equation controls for province of origin fixed effects, cohort fixed effects, district of residence fixed effects, father's years of schooling and its interaction with an indicator for older cohorts. Standard errors are clustered by province of origin.

2.8 Appendix: Additional Results

Table 2.10: The Proportion of Migrants and *per capita* Quota Size

Province	Population in 1948 (*million*)	Number of Migrants in Taiwan	Migrants (*per million*)	Quota size (*per million*)	
				pre-reform	post-reform
Fujian	11.14	197,611	17,734	65.8	59.9
Zhejiang	19.96	114,950	5,759	191.4	59.9
Guangdong	28.34	93,635	3,304	299.0	59.9
Jiangsu	41.82	124,611	2,979	353.1	59.9
Jiangxi	12.51	30,814	2,464	486.8	59.9
Shandong	39.72	95,917	2,415	437.9	59.9
Hunan	25.56	54,268	2,123	516.0	59.9
Anhui	22.46	44,616	1,986	537.9	59.9
Hubei	21.70	37,851	1,745	634.1	59.9
Hebei	32.21	49,319	1,531	689.4	59.9
Henan	29.65	41,768	1,409	766.1	59.9
Liaoning	11.57	14,096	1,218	993.2	59.9
Guangxi	14.64	11,631	795	1,461.6	59.9
Sichuan	48.42	37,436	773	1,335.6	59.9
Others	97.42	33,669	346	4,577.8	59.9
Taiwan				1.2	59.9
Migrants				559.8	59.9

Notes: This table reports the proportion of people migrating from mainland provinces to Taiwan, and per capita quota sizes they enjoyed before and after the reform. The proportion of migrants in total population is defined as $M_{j,1956}/P_{j,1948}$, where $M_{j,1956}$ is the number of people from province j who resided in Taiwan in 1956 and $P_{j,1948}$ is the total population of province j in 1948. Pre-reform *per capita* quota size is defined as $Q_j/M_{j,1956}$, where Q_j is the quota size allocated to province j. Post-reform *per capita* quota size is the sum of quota size over all provinces divided by total population residing in Taiwan in 1956.

Table 2.11: Effects of Migrant Status on High School Initiation

VARIABLES	High School Initiation		
	(1)	(2)	(3)
$\text{Age}_{15} \times 1(\text{Migrant})$	.059***	.059***	.056***
	(.009)	(.012)	(.011)
Observations	344,318	344,318	344,318
R-squared	.105	.226	.256
Mean of dep.	.378	.378	.378
Specification	I	II	III

Notes: The dependent variable is an indicator that takes the value 1 if an individual has ever attended (initiated) high school. Sample includes males aged 10–20 in the year of the reform 1962. Specification I controls for province of origin fixed effects and cohort fixed effects; specification II additionally controls for father's education and its interaction with an indicator for older cohorts; specification III further includes district of residence fixed effects. 1(Migrant) is an dummy that equals one if an individual's province of origin is a mainland Chinese province. Robust standard errors clustered by province of origin appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 2.12: Robustness – Effects of Preferential Quotas on High School Initiation

VARIABLES	High School Initiation		
	(1)	(2)	(3)
	Panel A: Controlling for Geographical Distances		
$\text{Age}_{15} \times \ln(\text{Quota})$	.017***	.018***	.016***
	(.005)	(.005)	(.005)
	Panel B: Controlling for the Order of Occupation		
$\text{Age}_{15} \times \ln(\text{Quota})$	.013**	.016***	.016***
	(.005)	(.005)	(.004)
Observations	344,318	344,318	344,318
Mean of dep.	.378	.378	.378
Specification	I	II	III

Notes: The dependent variable is an indicator that takes the value 1 if an individual has ever attended (initiated) high school. Sample includes males aged 10–20 in the year of the reform 1962. Specification I controls for province of origin fixed effects and cohort fixed effects; specification II additionally controls for father's education and its interaction with an indicator for older cohorts; specification III further includes district of residence fixed effects. All models in Panel A control for the straight-line distances from each province to Taiwan interacted with an indicator for older cohorts, where the distance for Taiwan is defined as zero. All models in Panel B control for the order in which each province was occupied by the Communist Army interacted with an indicator for older cohorts, where the order of occupation for Taiwan is top-coded. ln(Quota) and Quota are both standardized, such that the coefficients can be interpreted as impacts resulting from a one standard deviation change in *per capita* quota size. Robust standard errors clustered by province of origin appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 2.13: Effects of Preferential Quotas on High School Initiation – migrants only

VARIABLES	High School Initiation			
	(1)	(2)	(3)	(4)
Age_{15} × ln(Quota)	.016***	.006*		
	(.003)	(.003)		
Age_{15} × Quota			.019**	.010**
			(.007)	(.005)
Observations	30,163	30,125	30,163	30,125
R-squared	.036	.206	.036	.206
Mean of dep.	.843	.843	.843	.843
Specification	I	III	I	III

Notes: The dependent variable is an indicator that takes the value 1 if an individual has ever attended (initiated) high school. Sample includes male migrants aged 10–20 in the year of the reform 1962. Specification I controls for province of origin fixed effects and cohort fixed effects; specification III additionally controls for father's education and its interaction with an indicator for older cohorts, and district of residence fixed effects. ln(Quota) and Quota are both standardized, such that the coefficients can be interpreted as impacts resulting from a one standard deviation change in *per capita* quota size. Robust standard errors clustered by province of origin appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 2.14: Effects of Preferential Quotas on Schooling Decisions over the Life Cycle

VARIABLES	Secondary Completion	HS Initiation	HS Completion	College Initiation	College Completion
	(1)	(2)	(3)	(4)	(5)
			Panel A: Main Effects		
$Age_{15} \times \ln(Quota)$	.014***	.016***	.014***	.018***	.017***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
			Panel B: Heterogeneous Effects		
$Age_{15} \times \ln(Quota)$	.011***	.012***	.009***	.015***	.015***
	(.002)	(.002)	(.002)	(.004)	(.004)
... $\times$ Below	-.003***	-.003***	-.003***	.002	.002
	(.001)	(.001)	(.001)	(.002)	(.002)
Observations	344,318	344,318	344,318	344,318	344,318
Mean of dep.	.481	.378	.359	.166	.160
Specification	III	III	III	III	III

Notes: Dependent variables in Columns 1–5 are an indicator for secondary school completion, high school initiation, high school completion, college initiation, and college completion, respectively. Sample includes males aged 10–20 in the year of the reform 1962. All regression models presented in this table control for province of origin fixed effects, cohort fixed effects, district of residence fixed effects, father's years of schooling and its interaction with an indicator for older cohorts. ln(Quota) is standardized such that the coefficients can be interpreted as impacts resulting from a one standard deviation change in *per capita* quota size. Robust standard errors clustered at the native province level in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 2.15: Robustness – Effects of Preferential Quotas on Labor Market Outcomes

VARIABLES	Employed	Formal Employment	Occupation Status Score	Occupation Income Score
	(1)	(2)	(3)	(4)
$Age_{15} \times \ln(Quota)$	-.0002	.014***	.447***	.390***
	(.0014)	(.002)	(.080)	(.066)
Observations	344,318	326,793	320,097	320,097
R-squared	.028	.088	.142	.163
Mean of dep.	.949	.586	32.87	23.10
Spec	III	III	III	III

Notes: Dependent variables in Columns 1–4 are an indicator for being employed at the census time, an indicator that equals one if an individual is working for pay in the formal sector, Siegel's occupational prestige score, and occupational income score constructed by the IPUMS, respectively. Sample includes males aged 10–20 in the year of the reform, 1962. All regression models presented in this table control for province of origin fixed effects, cohort fixed effects, district of residence fixed effects, father's years of schooling and its interaction with an indicator for older cohorts. Robust standard errors clustered at the native province level in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 2.16: Robustness – Effects of Preferential Quotas on Family Outcomes

VARIABLES	Ever Married	Age at Marriage	Num of Children	Partner HS Diploma
	(1)	(2)	(3)	(4)
$Age_{15} \times \ln(Quota)$	.016***	.179***	-.079***	.022***
	(.003)	(.017)	(.007)	(.003)
Observations	344,318	162,732	162,732	162,732
R-squared	.061	.031	.151	.095
Mean of dep.	.743	24.82	2.132	.218
Specification	III	III	III	III

VARIABLES	Partner Employed	Partner Formal emp.	Partner Occ. Status	Partner Occ. Income
	(5)	(6)	(7)	(8)
$Age_{15} \times \ln(Quota)$	-.005	.024***	.537***	.228***
	(.004)	(.003)	(.129)	(.077)
Observations	162,732	42,595	42,413	42,413
R-squared	.016	.083	.061	.043
Mean of dep.	.262	.570	34.11	20.51
Specification	III	III	III	III

Notes: Dependent variables in Columns 1–8 are an indicator for being ever-married, age at marriage (conditional on being married), number of children at the census time, an indicator for whether female partner has completed high school, an indicator for female partner's employment status, and an indicator for whether female partner is working for pay in a formal sector (conditional on working), female partner's occupational status score, and female partner's occupational income score, respectively. Sample includes males aged 10–20 in the year of the reform 1962. All regression models presented in this table control for province of origin fixed effects, cohort fixed effects, district of residence fixed effects, father's years of schooling and its interaction with an indicator for older cohorts. Robust standard errors clustered at the native province level in parentheses (***p<0.01, **p<0.05, *p<0.1).

CHAPTER III

The Long-Term Health and Economic Consequences of Improved Property Rights

This chapter studies the long-term health and economic consequences of China's *Household Responsibility System* (HRS) reform—a property reform that assigned collectively owned farmland to individual households with secure tenures and boosted labor productivity among rural populations. Using regional variation in reform timing, I provide evidence that the reform *improved* later-life health, education, and labor market outcomes for individuals who had been exposed early in life. However, it *reduced* human capital investment for those exposed at critical school ages, making them less likely to receive education and more likely to remain in agriculture.

3.1 Introduction

Property rights are an important component of institutional structure, and a key determinant of economic development (Acemoglu and Johnson, 2005; Besley and Ghatak, 2010). Over recent decades, property rights reform has been increasingly used by governments in the developing world to fight poverty and improve individuals' long-term welfare (De Soto, 2003; Deininger et al., 2003). A large body of work has provided important evidence that these reforms are generally associated with improved contemporaneous outcomes pertaining to economic growth, labor supply, investment, and nutrition (Banerjee et al., 2002; Besley, 1995; Do and Iyer, 2008; Field, 2007; Galiani and Schargrodsky, 2010). However, there is little empirical evidence of how property reforms affect the long-term outcomes of their target populations.

This study addresses China's prominent *Household Responsibility System* (HRS) reform—a property reform that has assigned collectively-owned farmland to individual households with secure tenures. Prior to the reform, private land rights had been abolished for decades, and Chinese farmers were forced to work in production teams composed of 20–30 households. Due to difficulty monitoring the efforts of each member, agricultural output was distributed across team members based on work hours, rather than marginal labor input. This led to widespread free-rider problems, low work incentives, and stagnant agricultural productivity. HRS reform, which granted land use rights to individual households and granted them rights to residual income from farm activities, solved the incentive problem and stimulated labor productivity and income among rural households (Lin, 1992; McMillan et al., 1989). In this paper, I evaluate the long-term impact of HRS reform on the well-being of the rural population. In particular, I ask: how does early-life and childhood exposure to reform affect individuals' health, education, social and economic outcomes in adulthood?

I first limit the attention to individuals who were exposed to the reform very early in life, and explore how early-life exposure to the reform affects individuals' adult outcomes. The focus on early-life influence is motivated by the growing body of literature linking early-life circumstances to later-life outcomes (Adhvaryu et al., 2019; Maccini and Yang, 2009; Almond, 2006; Isen et al., 2017; Hoynes et al., 2016). The theory of fetal origins posits that initial health endowments are crucial to the formation of health and human capital during each subsequent life period (Grossman, 1972; Barker, 1998). Therefore, shocks and interventions at early-life stages can have immense, lasting effects that persist into adulthood (Almond and Currie, 2011; Heckman, 2006, 2007). On the eve of China's HRS reform, when the private economy and labor migration were still prohibited, land was the only source of income for most rural households. HRS reform revolutionized incentives, productivity, and many other aspects of their lives. It may also have had a long-term effect on the target population, especially those who had been exposed during the early and "critical" life periods.

I examine this hypothesis using a nationally representative household survey from China. I restrict the sample to respondents who were born during the reform period (1978–1984), and explore the effect of birth-year reform exposure on their adult outcomes. Conceptually, increased household income induced by HRS reform afforded individuals better nutrition during gestation and infancy. This may have translated into improved adult outcomes. However, the reform could have also been disruptive, as it increased the opportunity costs of parents' time, and might have induced them to switch out of child care and into productive activities (Miller and Urdinola, 2010). In the latter case, the reform may have inhibited infant health, and consequently, adult outcomes. A *priori*, which effect dominates is ambiguous, and constitutes an important empirical question of this paper.

In addition to the early-life effects, I also explore the effect of HRS reform for those exposed at older and critical school ages. The extensive literature on the theory of fetal origins finds that nutritional shocks and interventions at older ages have a muted effect on health endowment. However, one may expect the reform to affect these children through other mechanisms, such as human capital investment. This focus is motivated by recent studies that have found outsized effects of income shocks on children's educational attainment (Atkin, 2016; Carrillo, 2019; Shah and Steinberg, 2017). In my context, HRS reform permanently increased labor productivity and income for the agricultural sector. It can increase human capital investment in children through the income effect (increased household resources). Yet, it can also reduce investment through a substitution effect (increased opportunity costs of schooling and reduced returns to education). If the latter effect dominates, parents who had previously been on the margins of enrolling their children in school may find it less attractive to do so, and may instead choose to keep their children on the farm, leading to a decrease in human capital investment. To test this hypothesis, I turn to a second sample of individuals who had been exposed to reform at school-entry age (age 6), a key transition point at which parents make the choice of whether or not to enroll their children in formal education. I then examine how exposure to the reform at this age has affected individuals' education and other outcomes over the long term.

This study's research design takes advantage of the substantial variation in reform timing and pace across Chinese provinces. Employing a difference-in-difference specification, I provide evidence that birth-year reform exposure significantly improves individuals' health, educational attainment, and labor market performance, as measured around age 30. My findings suggest that a 10 percentage point increase in reform adoption rate in the birth year increases an individual's total years of schooling by about 0.1 years, as well as their height by 0.45 centimeters. In addition, individuals exposed to the reform in their birth years tend to achieve higher health scores, have more prestigious jobs, earn more, and are more likely to have left agriculture and obtained an urban *hukou* (i.e. register as an urban resident).[1] These results are robust to the inclusion of additional controls and reform exposure in adjacent years before and after birth. Placebo tests based on a sample of respondents born in urban areas show a lack of any effects, lending credibility to the identification strategy used in this study. Additional findings suggest that the estimated effects are not driven by endogenous fertility responses or selective child survival.

Moreover, I provide evidence that reform exposure at school entry age significantly reduces individuals' likelihood of receiving formal education. As the HRS adoption rate at age 6 increases by 10 percentage points, the likelihood of primary education decreases by roughly 1 percentage point; total years of schooling decreases by an average of 0.14 years. As a consequence, individuals are 1.7 percentage points more likely to remain in agriculture and register as rural residents. These findings align with recent studies which find that income shocks during children' school-going years can largely affect their schooling choices and long-term achievement (Shah and Steinberg, 2017; Carrillo, 2019; Atkin, 2016). Consistent with the "fetal origins" hypothesis that stresses early-life circumstances in the formation of health, I find no statistically distinguishable effects of school-age exposure on individuals' adult health.

This study speaks to a large body of work examining the consequences of improved property rights on target populations. Much of this literature has studied the effects of property reforms and titling programs on important contemporaneous outcomes such as investment (Besley, 1995; Do and Iyer, 2008; Galiani and Schargrodsky, 2010), economic performance (Alston et al., 1996; Banerjee et al., 2002; Goldstein and Udry, 2008; Hornbeck, 2010; Montero, 2018), labor supply and allocation (Chari et al., 2017; De Janvry et al., 2015; Field, 2007; Wang, 2012). This work complements these studies

[1]China's *hukou* is intended to monitor population flows. People are required to register in their county of birth, either as rural or urban residents. An individual's *hukou* determines his or her eligibility for jobs, schooling, housing, and other social security benefits. Prior to the first *hukou* reform in the late 1980s, few people were allowed to migrate from rural to urban areas or from one county to another. See Chan and Zhang (1999) and Kinnan et al. (2018) for more details on the Chinese *hukou* system.

by examining the effect of property reform on individuals' long-term well-being (as measured by a large set of health, educational, social, and economic outcomes). It also provides new evidence that the direction of these effects can depend on individuals' age of exposure.

This work is also related to the growing literature on fetal origins. Much work in this area has focused on rare and adverse shocks early in life, and how they have hurt individuals' later-life outcomes (Almond, 2006; Almond et al., 2010; Behrman and Rosenzweig, 2004; Chen and Zhou, 2007). Several recent studies have examined the long-term effect of positive, policy-driven treatments in the US context, such as Medicaid expansions (Brown et al., 2019), environmental regulation (Isen et al., 2017), and social security programs (Hoynes et al., 2016). I add to this line of work by providing quasi-experimental evidence from policy-driven shocks in a development context.

Finally, standard models of optimal human capital investment predict that increased opportunity costs of schooling and decreased returns to education are associated with less investment in human capital (Becker, 1967; Ben-Porath, 1967; Weiss, 1995). This paper joins a small set of recent studies that provide rigorous evidence in support of this claim (Black et al., 2005; Charles et al., 2015; Carrillo, 2019; Shah and Steinberg, 2017). Unlike previous work that focuses on temporary income shocks induced by weather or commodity price fluctuations, this study addresses permanent productivity shocks induced by a prominent institutional reform. This focus is likely to yield stronger effects, since forward-looking individuals might not adjust long-term education decisions in response to temporary shocks (Becker, 1967; Eckstein and Wolpin, 1999).

This paper proceeds as follows. The next section discusses the background of HRS reform, focusing on institutional details relevant to the research design. Section 3.3 describes the data. Sections 3.4 and 3.5 discuss the empirical strategy, and present the primary results for the early-life and school-age samples, respectively. Section 3.6 concludes.

3.2 HRS Reform

The Chinese Communist Party (CCP) rose to power by defeating the Nationalist Party (Kuomintang) in the Chinese Civil War in 1949. At that time, China had a poor agrarian economy with over 85% of its population living on small plots of land in rural areas. In order to transform China into an industrialized economy, the CCP collectivized agriculture. This was accomplished under the leadership of CCP Chairman Mao Zedong in the early 1950s. Mao believed that collectivization would transform Chinese agriculture from small household farming into large-scale mechanized production. This, in turn, would boost agricultural productivity so that more output could be extracted from the rural sector for industrialization (Li and Yang, 2005).[2] With this belief, he urged rural China to adopt a collectivized farming system. Under the collectivized system, private property rights to land and assets were abolished. Markets for private transactions were also banned. Farmers were forced to pool their equipment and work in production teams consisting of 20–30 households (Lin, 1992).

Since it was difficult and costly to monitor each team member's labor efforts, the distribution of output between farmers within a production team was tied to their work hours, rather than to marginal labor input. In practice, workers were accredited with "work points" for their daily tasks. At the end of a year, and after deductions for state taxes and public welfare spending, the net team income was distributed according to the work points that each person had accumulated during the year.[3] Because team members were not paid based on their efforts, they tended to free ride, and lacked the incentive to work. As a result, when agricultural collectivization was completed in 1958, total factor productivity (TFP) in agriculture dropped by about 30 percent, and the productivity stagnated at low levels throughout the collectivization period (as demonstrated in Figure 3.1). Because of the dramatic decline in grain output and other radical policies implemented by the Central government, a famine struck China between 1958 and 1962, killing over 30 million people (Lin, 1990; Yang, 1996).[4]

After Mao's demise, China reconsidered its agricultural policies and sought agricultural reform by implementing the so-called *Household Responsibility System* (HRS). Under the HRS, previously collectively-owned farmland was assigned to individual households, based on the total number of household members. Secure tenure was also provided, initially for 15 years; later extended to 30 years (Lin, 1992). While land ownership remained with the collective, land use rights and residual rights to output from land were granted to individual households. This institutional change

[2]For example, at that time, grain was procured from rural households to feed urban workers and for export in exchange for industrial equipment. Millions of rural laborers were mobilized for the construction of dams, roads, and other infrastructure (Li and Yang, 2005).

[3]See Lin (1988) for more on management under the collectivized system.

[4]It is known as the Great Leap Famine, one of the worst humanitarian disasters in recorded history.

solved the incentive problem and boosted labor productivity and agricultural output (Lin, 1992; McMillan et al., 1989). Figure 3.2 provides evidence for this claim. This figure presents scatterplots of per capita grain output for each province and year against reform adoption rates using data from 1978 to 1984, conditional on province fixed effects, year fixed effects, and province-specific linear trends. As is clear from the figure, there is a strong and positive association between grain output and HRS adoption. Point estimates presented in Appendix Table 3.9 indicate that a full adoption of the new system (i.e. an increase from 0 to 100 percent) leads to an increase of 195 kg in per capita grain output, a 45 percent increase at the sample mean.

Unlike many other reforms initiated by the government, the HRS was first implemented by a small group of farmers in a small village in Anhui province. After a drought in 1978, they secretly experimented with contracting land, agricultural equipment, and output quotas to individual households, unbeknownst to the Central government (Lin, 1987). A year later, these farmers' yields far surpassed those of others in the same region. Inspired by their success, production teams in both this and other regions switched to the new system, with the blessing of local governments. Anecdotal evidence suggests that regions that suffered more famine deaths from 1958 to 1962 were more resistant to the collective system, and tended to adopt the new system earlier (Yang, 1996).[5] For instance, Sichuan, Anhui, and Shandong, which suffered the most from the 1958–1962 famine, were the three earliest reform adopters. In addition, regions stricken by bad weather demanded the reform more urgently and were also early adopters (Lin, 1987; Zhang, 2012). On the supply side, opponents of the HRS initially questioned the new system on the grounds that it violated "socialist principles." To minimize the political risk that would occur should the reform fail, China's Central leaders did not immediately recognize the HRS. Instead, they experimented with the system and gave more support to regions distant from the capital (Lin, 1987). I evaluate these claims and address potential threats to internal validity in Section 3.4.

Ultimately, the HRS was successful and popular. By late 1981, when the Central government officially recognized the HRS, 45.1 percent of China's production teams had already adopted the system. However, as demonstrated in Figure 3.3, the adoption rate varied substantially across provinces. By the end of 1981, over 60 percent of the production teams in early adopters such as Anhui and Sichuan had adopted the new system. However, late adopters such as Jilin and Heilongjiang had less than

[5] Appendix Figure 3.4 demonstrates the variation in famine severity (measured by cumulative famine mortality rate) across provinces.

10 percent. The remaining production teams gradually adopted the HRS, and by the end of 1984, over 99 percent of the production teams in China had switched to the new system.

HRS reform transformed China's land property rights from a communal system to an individualized tenure system. The reform, widely recognized as the engine behind China's rapid agricultural growth, is considered China's most far-reaching economic reform to date. Furthermore, the substantial variation in the timing and pace of reform across regions provides the opportunity to evaluate its long-term effect on rural populations.

3.3 Data

My primary analysis combines data on HRS adoption rate at the provincial level with a nationally representative household survey, the *Chinese Family Panel Study* (CFPS). Through a multistage probability sampling procedure, CFPS includes interviews of a total of 14,798 sampled households and all adults living in these households. It includes 36,000 observations, with participants born in 31 Mainland China provinces. This study's analysis utilizes the baseline wave of the CFPS conducted in 2010. This wave contains details about each respondent's demographic characteristics, educational attainment, family background, work experience, and health status, among other information. Individuals born in rural areas were retained for the main analysis because the causal factor of interest, the HRS reform, was only implemented in rural areas. Thus, it should have more pronounced effects on the rural population.[6] To explore different mechanisms through which the reform may have affected individuals' long-term outcomes, I focus on two samples.[7] The first consists of respondents who were born during the reform period (1978–1984), which has a size of 2,629. The second consists of respondents who turned the age of primary school entry (age 6) during the same period, which has a size of 3,440. All respondents were between 26 and 38 years old at the time of the survey, and thus had already completed their formal education and entered the labor market. Throughout, I refer to the first sample as the "early-life" sample and the second as the "school-age"

[6] Because CFPS does not ask *hukou* status at birth, I define individuals born in rural areas as those who held a rural *hukou* at age 3. This should not affect our results because over 98% of the respondents in the sample still lived in their birthplaces at age 3. The sample of respondents born in urban areas is used for placebo tests.

[7] I conduct separate regressions for each sample instead of pooling them, since the reform lasted for a short period and there is not enough variation to estimate a pooled specification with many cohorts.

sample. The primary outcomes of interest are educational attainment, height, health score, employment, earnings, occupational status score, urban residency, and other socioeconomic outcomes, all measured in the survey year.

Although the rich survey data allow analysis of an array of important outcomes at the individual level, the sample size is small, compared to prior work that uses data from population censuses or administrative earnings records (Almond, 2006; Isen et al., 2017; Bleakley, 2007; Atkin, 2016). This may limit the statistical power and lower the estimation precision. However, this concern is minimal since most of my estimates are statistically significant at conventional levels. In the regression analysis, the sampling weights are also used to make the estimates more representative of the population.

The HRS adoption rate is measured by the proportion of production teams in a province that had adopted the HRS by the end of a year. Data for 1978–1982 are compiled by Chung (2000) and those for 1983–1984 are drawn from the *Compendium on Agricultural Collectivization since the Founding of the People's Republic*. To test for confounding factors that might inflate the results, I supplement the analysis with data from the 1990 wave of the Chinese Population Census, which are made available by Integrated Public Use Microdata Series (IPUMS). In robustness checks, I include rainfall and other time-varying characteristics as additional controls. The rainfall data are available at a 0.5 by 0.5 degree grid level (approximately 1 km^2), and are taken from the standard Willmott and Matsuura series available at *climate.geog.udel.edu/ climate/*. I merge the rainfall data to the birth provinces by taking the average over grid points within each province. Data on provincial characteristics are obtained from *A Compilation of Historical Statistical Data of Provinces, Autonomous Regions, and Municipalities*, 1949–1989 and *Statistical Compendium on Sixty Years of New China*, 1949–2009, which are compiled by the State Statistical Bureau. Summary statistics on outcomes of interest and principal controls are reported in Table 3.1.

3.4 The Effects of Reform Exposure Early in Life

3.4.1 Estimation Strategy

To evaluate the effects of birth-year reform exposure on later-life outcomes, I estimate the following specification for the early-life sample:

$$Y_{ipc} = \alpha + \beta \times \text{Adoption}_{pc} + \gamma \times X + \mu_p + \delta_c + \epsilon_{ipc}. \tag{3.1}$$

Y_{ipc} is a measure of educational, health, or socioeconomic outcome for individual i, born in province p in year c. The key explanatory variable, Adoption_{pc}, is the HRS adoption rate in birth province p and year c. In all regression models, I control for province of birth fixed effects μ_p to absorb any time-invariant determinants of long-term outcomes for individuals born in a particular province. I also include the birth year (cohort) fixed effects δ_c to control for time-varying determinants of long-run outcomes common to the same cohort. The key parameter of interest, β, estimates the effect of a one-unit increase in the HRS adoption rate in an individual's birth province and birth year on their adult outcomes measured in the survey year, holding constant other variables.

The validity of the estimation strategy depends on the conditional exogeneity of the reform timing. As discussed in Section 1.2, anecdotal evidence has suggested that the reform tended to be adopted earlier by regions that suffered more severe famine from 1958 to 1962, and regions that were more distant from Beijing (Yang, 1996; Lin, 1987; Zhang, 2012). If the same characteristics are associated with differential trends in the outcome variables, the key parameter of interest, β, might be biased. To evaluate the extent to which the reform timing is predicted by these factors, I regress the year in which each province adopted the reform on each pre-reform provincial characteristic separately. The results presented in Appendix Table 3.10 show minimal correlation between reform timing and most of the factors, including level of development, total population, rural population share, remoteness, and access to medical resources. The only exception is historical famine mortality. Consistent with previous findings, provinces that suffered more famine deaths in the late 1950s and early 1960s do appear to be early adopters. In a regression that includes each of these variables as regressors, rural population share and geographic distance also become statistically significant. The pre-reform characteristics all together only account for 32 percent of the variation in reform timing, suggesting that there are considerable idiosyncratic differences that can be utilized for the estimation. To address endogeneity concerns, I follow Hoynes et al. (2016) and control for the interactions between observable determinants of reform timing and linear cohort trends. Because regions stricken by bad weather tended to adopt the reform earlier (Lin, 1987), and because early-life exposure to negative weather shocks adversely affects adult outcomes (Maccini and Yang, 2009), I also control for the level of rainfall in each individual's birth province and birth year. In order to account for changes in health-care resources, I include the number of physicians per capita as an additional control variable.

In a later specification, I interact the main treatment variable, HRS_{pc}, with a dummy for male respondents, in order to explore heterogeneity by gender. As a robustness check, I follow Maccini and Yang (2009), and control for reform exposure in years before and after birth (ages -2 through 2), to account for potential confounding effects from adjacent years. To shed more light on the validity of the research design, I conduct a placebo test with a sample of respondents who had been born in urban areas, and thus, were unlikely to have been affected by the reform. Standard errors are clustered at the birth-province level to account for common error distribution within provinces over time.

Although some regions adopted the reform earlier than others, all production teams had switched to the new system by the end of the reform. In particular, the reform adoption rate rose monotonically from 0 in 1978 to 100 percent for all provinces by the end of year 1984.[8] This means that provinces that adopted the HRS earlier should have experienced slower adoption rate growth in later years. In the above specification, any unobserved or omitted confounder must follow the same reversed pattern if it is to confound the estimates (Fujiwara, 2015). For example, in the early-adoption provinces, any omitted variables that confound the estimates must first grow faster, and subsequently grow slower. This is unlikely, given the short HRS reform period. It also bears mentioning that two other reforms were implemented during the same period: a price reform that raised state procurement prices for major crops, and a market-oriented reform that canceled mandatory production plans (Lin, 1992). Since these reforms were implemented uniformly across provinces with little variation in timing, their effects would have been absorbed by the cohort fixed effects, and therefore should not confound the estimates.

3.4.2 Main Results

I begin by documenting the effects of birth-year reform exposure on individuals' educational attainment. Table 3.2 presents the estimates of Equation 3.1 with educational outcomes as the dependent variable. These results indicate a statistically significant and economically meaningful effect of HRS reform in the birth year on individuals' long-term educational attainment. In Columns 1–3, I use an indicator for high school completion as the outcome variable and find that a higher reform adoption rate in an individual's year and province of birth is associated with a higher likelihood of high school completion.[9] This relationship is robust to the inclusion of trends in

[8] The national HRS adoption rate had surpassed 99 percent by the end of 1984.

[9] China enacted its first compulsory schooling law in 1986. I focus on high school education for this sample because by the time most of them had reached school-entry age, primary and secondary

provincial characteristics and additional time-varying controls. The point estimates are close in magnitude and significant at the 5% level, suggesting that an increase in reform adoption rate of 10 percentage points at birth will lead to an increase in high school completion rate of 1.8 percentage points. In Columns 4 through 6 where total years of schooling is the outcome variable, I find that individuals who had been exposed to reform in their birth years also completed more years of schooling. The point estimates indicate that a 10 percentage point increase in the adoption rate leads to 0.1 more years of schooling. Once again, the estimated coefficients are statistically different from zero at conventional levels and are comparable in size across specifications. The stability of these estimates also suggests that it is unlikely that the estimates are the results of omitted variables.

Next, I estimate Equation 3.1 with three health outcomes as the dependent variables and report the estimates in Table 3.3. I first present results for adult heights, which are measured in centimeters and are commonly used as a proxy for health in the literature (Bleakley, 2010). Columns 1 and 2 present the regression results with and without controls, respectively. The point estimates are both positive and statistically different from zero, indicating that a 10 percentage point increase in the adoption rate at birth increases adult height by roughly 0.45 centimeters. In Columns 3 and 4, I use the health score evaluated by enumerators on a 1–7 scale as the outcome variable. I use enumerator-evaluated rather than self-reported health status because the latter largely depends on education, cognitive skills, and access to information and medical examinations (Lange, 2011). Each of these can be endogenously determined by the reform in early life, and therefore can confound the interpretations.[10] Estimates related to health score are positive and become statistically more significant and larger in magnitude after controlling for trends and time varying characteristics. For ease of interpretation, I construct a dummy variable that equals 1 if an individual's health score is above the sample average; the results are reported in Columns 5 and 6. The estimates are both positive and statistically significant at the 1% level; a 10 percentage point increase in reform adoption rate in an individual's birth year and birth province makes them 3 percentage points more likely to have an above-average health score.

Table 3.4 presents the regression results from Equation 3.1 with a series of social and economic outcomes as the dependent variables. The results show evidence that

education had already become compulsory.

[10] In additional analyses not reported, I find no statistically discernible effects of HRS reform on self-reported health status.

birth-year exposure to reform makes individuals more likely to leave rural areas and register as urban residents. Columns 1 and 2 show that as the birth-year adoption rate increases by 10 percentage points, individuals are 1.4 percentage points more likely to switch from a rural to an urban *hukou*. Additionally, I find that a higher birth-year adoption rate is associated with higher income (Columns 3–4). In Appendix Table 3.11, I provide evidence that this income differential is not driven by selective labor force participation; the reform does not seem to have significantly affected employment status among the target population. Instead, the income effect is likely driven by the fact that individuals who had been exposed to reform at birth have more prestigious jobs. The results in Columns 5–6 of Table 3.4 support this claim. They suggest that a 10 percentage point increase in birth-year adoption rate increases the ISEI occupational score (*International Social Economic Index of Occupational Status*) by 0.77 points, corresponding to 2.1 percent of the sample mean. Lastly, Columns 7 and 8 present the estimated effects for individuals' political status, as measured by a dummy for affiliation with the ruling Chinese Communist Party (CCP). CCP membership is not only a symbol of political status; it also provides extra access to various economic resources and opportunities in China (Li et al., 2008). Estimates related to this variable are positive and significant at the 1% level, suggesting that birth-year reform exposure significantly raised individuals' economic and political status.

3.4.3 Additional Results

Next, I conduct several robustness checks to test the sensitivity of the main results to changes in the specification. Because the reform adoption rate can be serially correlated over time, one concern is that the coefficient for birth-year reform exposure may pick up effects from adjacent years. To alleviate this concern, I follow Maccini and Yang (2009) and include the adoption rates from age -2 (two years prior to the birth year) to age 2 (two years after the birth year) as explanatory variables. Results are presented in Table 3.5. They show that the estimates for birth-year reform exposure are largely unaffected by the control of reform exposure in adjacent years. For 6 out of the 7 outcome variables, the estimated effects remain statistically significant and similar in magnitude. Only the coefficient for urban residency becomes marginally insignificant. However, that effect remains similar in size to the previous ones. Point estimates for adjacent years, by contrast, are either statistically indifferent from 0 or have inconsistent signs. These findings lend credibility to the identification strategy, and point to the crucial role of early-life circumstances in the determination of individuals' long-term well-being.

HRS reform may also shift gender preference or resource sharing within rural households (Almond et al., 2019). For example, the reform may disproportionately increase men's productivity more than women's. Moreover, since land is distributed and adjusted based on the number of household members, and girls will leave their parents' homes after marriage, the reform may increase the value of a boy relative to a girl. If parents allocated disproportionately more resources to boys as a result of the reform, the effects could be more pronounced for male respondents. Appendix Table 3.12 explores heterogeneity by gender. I interact the birth-year HRS adoption rate with a dummy for male respondents, but do not find statistically different effects for most outcomes. The only exception is height, where I find a stronger effect for men than for women.

Appendix Table 3.13 presents the results of a placebo test using a sample of respondents who were born during the same period, but in urban areas. Because the reform was only implemented in rural areas, the placebo sample should not have been directly affected. Consistent with this hypothesis, all point estimates related to the placebo test are statistically insignificant and have inconsistent signs across outcome variables. These findings corroborate the estimation strategy, suggesting that the main results are not driven by province-level confounders.

The results thus far show that HRS reform in the birth year significantly improves individuals' adult outcomes along important dimensions. These results are consistent with the fetal origins hypothesis, and indicate that the positive effects of increased household resources during gestation and infancy dominate any negative effects of reform disruptions. In terms of magnitude, the estimated effects are comparable to those of prior studies. My results suggest that full adoption of HRS reform in the birth year is associated with roughly 1 more year of schooling, a 4.5 centimeter increase in height, and a 14 percentage point increase in the likelihood of obtaining urban residency. Chen and Zhou (2007) find that exposure to the Chinese Great Leap Famine in infancy reduces adult height by 3.03 centimeters among survivors. Because famine survivors are likely those who were less affected or were healthier at birth, their estimates serve as a lower bound of the average population effect. Maccini and Yang (2009) estimate that a positive (20 percent higher) rainfall shock in the birth year raised total years of schooling by 0.22 and height by 0.57 centimeters among women in rural Indonesia. My estimates are larger than theirs because of the focus on a dramatic institutional reform that boosted productivity and income, as opposed to more moderate variations.

3.5 The Effects of Reform Exposure at School-Entry Age

3.5.1 Conceptual Framework

In this section, I investigate how reform exposure at school-entry age affects individuals' long-term education and other outcomes. For research design purposes, I focus on an important extensive margin—whether an individual has any formal education. There is meaningful variation with respect to this variable since primary education was not compulsory during the sample period, and 22 percent of the individuals in my second sample are illiterate. Below, I outline a simple conceptual framework to motivate the empirical analysis.

Consider a child who is about to enroll in primary school and start formal education (age 6). The child's parents decide whether to enroll her in school based on the expected lifetime income and schooling costs. Children differ in academic ability θ_i, which follows a uniform distribution over the interval $[0, 1]$. Each child incurs psychological costs of schooling, given by $\kappa(1-\theta_i)$. Tuition and school fees are represented by F. After reaching legal working age J, individuals with formal education have the probability ρ of finding an urban job and receiving labor market income of ω_t. Meanwhile, those without formal education have to stay on the farm and earn an income of υ_t ($\omega_t > \upsilon_t$). This is a realistic assumption, since rural-to-urban migration was prohibited during the reform period, and education was the only way for rural children to access employment opportunities in urban sectors (Kinnan et al., 2018; Pan, 2017). If the child does not go to school, child labor gets a payoff of χ_t. Let the discount factor be r and the retirement age be T. Parents will enroll their children in school if, and only if, the education premium exceeds the schooling costs (inclusive of opportunity costs).

$$\sum_{t=J}^{T} e^{-r(t-6)}\rho(\omega_t - \upsilon_t) \geq F + \kappa(1-\theta_i) + \sum_{t=6}^{J} e^{-r(t-6)}\chi_t$$

The likelihood that a child will receive formal education is then determined by the ability threshold, and is given by $\frac{\Pi_i}{\kappa}$, where $\Pi_i = \sum_{t=J}^{T} e^{-r(t-6)}\rho(\omega_t - \upsilon_t) - \sum_{t=6}^{J} e^{-r(t-6)}\chi_t - F$.

HRS reform could have reduced the likelihood of formal education for two reasons. First, it could have permanently increased the (expected) agricultural income, υ_t. This would have depressed the relative attractiveness of skilled jobs in urban sectors and the expected returns to education, from the perspective of rural households.

Second, the reform may also have increased the value of child labor, χ_t, which in turn, would have raised the opportunity costs of schooling. If these effects had dominated any countervailing income effects, we should see children as less likely to receive formal education after the reform.[11]

3.5.2 Specification

To examine the effect of reform on education and other outcomes for individuals exposed at critical school ages, I turn to the second sample of respondents—those who turned 6 during the reform period. In theory, one could estimate the following equation:

$$Y_{ijpc} = \alpha + \beta \times \text{Reform}_{j,c+6} + \gamma \times X + \mu_{jp} + \delta_c + \epsilon_{ijpc}.$$

Y_{ijpc} is a dummy that equals 1 if individual i, born in production team j of province p in year c, has ever received formal education. The key explanatory variable, $\text{Reform}_{j,c+6}$, takes the value 1 if the individual's production team j had adopted the reform when she turned 6. The model controls for production team fixed effects, birth year (cohort) fixed effects, and a set of covariates at various levels. Note that once the new system is adopted in a production team, it continues operating indefinitely. Therefore, individuals with exposure at age 6 are also exposed at older ages.

This strategy compares education choices between cohorts who turned 6 after the reform and slightly older cohorts in the same production teams who turned 6 beforehand. I focus on age 6 because it is the prevailing age for Chinese children to begin primary school and start formal education (Population Census Report, 1990). The idea is that if the reform discourages human capital investment, those who turned 6 after the reform should be less likely to begin school and receive formal education. However, for slightly older cohorts who turned 6 before the reform, the key time for schooling decision-making had passed. Therefore, many of them had already started primary education, and thus the effect of reform on the education dummy would have been small. The coefficient of interest, β, is thus expected to be negative.

Because reform adoption data are not available at the production-team level, the indicator variable for reform exposure at age 6, $\text{Reform}_{j,c+6}$, is proxied by its aggregate counterpart (Bleakley, 2007). Specifically, I replace the reform dummy, $\text{Reform}_{j,c+6}$, with a continuous variable, $\text{Adoption}_{p,c+6}$, which is the share of production teams in

[11] Charles et al. (2015) use detailed labor market data to distinguish between the two mechanisms. I am not able to conduct similar analysis since China was a planned economy during the reform period, and wages and prices were set by the government instead of the market.

each province that had adopted HRS when an individual turned 6. This measures the *likelihood* that an individual was exposed to reform at age 6. The following specification estimates the intent-to-treat (ITT) effects.

$$Y_{ipc} = \alpha + \beta \times \text{Adoption}_{p,c+6} + \gamma \times X + \mu_p + \delta_c + \epsilon_{ipc}. \tag{3.2}$$

The dependent variable, Y_{ipc}, is an indicator for formal education, which takes the value 1 if individual i, born in province p and year c, has ever received primary education. The HRS adoption rate and the control variables are defined as in Equation 3.1, except that they are now measured at age 6. To account for changing educational resources, I include the number of primary school teachers at age 6 as an additional control. Since internal validity concerns have been discussed in the previous section, I now shift to the main results and robustness checks.

3.5.3 Results

Table 3.6 presents the estimated effects of reform on educational outcomes from Equation 3.2. In Columns 1–3, the outcome variable is an indicator for formal education, which takes the value 1 if an individual has primary education or above. Without additional controls, the coefficient presented in Column 1 shows a significant and negative effect of reform exposure at age 6 on this variable. As the reform adoption rate increases by 10 percentage points, individuals become 1.1 percentage points less likely to have ever enrolled in school. In Columns 2 and 3, where a linear cohort trend interacted with provincial characteristics and time-varying controls are added to alleviate endogeneity concerns, this effect remains similar in size and becomes statistically more significant. The estimates are also robust to alternative measures of education investment. In Columns 4–6, where completed years of schooling is the dependent variable, I find consistently negative coefficients. The effects are statistically significant when linear cohort trends or time-varying characteristics are added as controls. Without these controls, the coefficient is marginally insignificant. However, the estimates are fairly stable in size across specifications, suggesting that a 10 percentage point increase in reform adoption rate at age 6 reduces total years of schooling by about 0.14.

The above findings echo recent studies which find that substitution effects dominate income in determining human capital investment. Atkin (2016) estimates that for every additional 25 manufacturing jobs, one student is induced to drop out of school at grade 9. Shah and Steinberg (2017) provide evidence that a drought in the previous year decreases children's math scores by 1.5 percent. Carrillo (2019) finds that higher coffee prices would decrease school attendance and increase child labor among coffee growing areas. The estimated education effects in this paper appear large in magnitude, compared to the previous work. This is probably because this study addresses a permanent institutional change, while previous work has focused on temporary shocks.

These findings also shed light on the validity of the identification strategy. They indicate that the main results are unlikely driven by omitted variables or differential trends in economic development across regions. If this were the case, omitted variables or trends would have biased the estimates for the two samples in the same direction. However, the estimated effects of reform exposure at birth and at school-entry age have opposite signs, which points against many confounding factors and alternative interpretations.

In Table 3.7, I re-estimate Equation 3.2 with the set of health outcomes as dependent variables. The theory of fetal origins hypothesizes that post-infancy circumstances have minor effects on health endowments since they do not occur during the critical period for "programming". Therefore, shocks and interventions later in life tend to play a limited role in shaping individuals' long-term health. Consistent with this hypothesis, I find muted effects of reform exposure at school entry age on adult health status. These estimates are fairly small in magnitude and statistically indistinguishable from zero for all specifications.

Table 3.8 provides evidence that exposure to reform at school-entry age translates into lower socioeconomic status in adulthood. In Columns 1 and 2, a 10 percentage point increase in the reform adoption rate reduces the likelihood that a rural child switches to urban residency by roughly 1.7 percentage points. Consistent with this, the results in Columns 5–6 suggest that individuals would also be more likely to work jobs with less prestige; the ISEI occupational status score drops by 0.65, corresponding to 2 percent of the sample mean. Due to the lack of statistical power, I do not find precisely estimated effects on income. Rather, the point estimates for income are small, and are sensitive to the inclusion of different sets of controls.

I then conduct several robustness checks for the above results. To account for potentially confounding effects of exposure at adjacent ages, in Appendix Table 3.14, I include reform adoption rates from ages 5 to 8 as additional controls; the estimates for age 6 remain similar both in terms of size and significance level. Appendix Table 3.15 shows that the negative effect of reform exposure on education, urban residency, and occupation scores do not exhibit significant differences by gender; coefficients on the interaction terms are always small and statistically insignificant. In addition, the placebo tests based on the sample of respondents who were born during the same period, but in urban areas, reveals no any statistically discernible effects. Once again, it suggests that the main results are not driven by unobserved confounders at the provincial level. The results of the placebo test are reported in Appendix Table 3.16.

Finally, the results do not appear to be driven by endogenous fertility responses or selective child survival. The reform may have changed parents' fertility choices. If they had chosen to have more children as a result of the reform, a larger family size would have reduced the resources available for each child. This could have, in turn, decreased educational investment in school-age children. Similarly, if the reform had reduced infant mortality, family size would also have increased and human capital investment in each child would have fallen. If either were the case, the same negative effects should hold for both the early-life and the school-age samples. However, the opposite effects documented contradict that possibility and are inconsistent with both the changing fertility and the mortality hypotheses. Nonetheless, I provide empirical evidence in Appendix Table 3.17 that the reform did not significantly affect birth cohort size.

3.6 Conclusion

In recent decades, programs designed to improve property rights have been effective at targeting the poor and have lifted many of them out of poverty. The Chinese *Household Responsibility System* (HRS) reform is one of the most successful efforts of this kind in the developing world. It has transformed China's land property rights from a communal system into an individualized tenure system. It has also stimulated labor productivity and rural household incomes. Although much is known about how this reform has contributed to poverty reduction and China's economic growth, less is known about how it has affected the well-being of targeted populations over the long term.

This study evaluates the long-term effects of HRS reform on individuals' health, education, and labor market outcomes. I find that the positive effects of higher income are not overwhelmed by any negative consequences from being born in a potentially disruptive reform period. The empirical evidence suggests that HRS reform significantly improved later-life outcomes for those exposed to the reform very early in life. Individuals who had birth-year reform exposure tended to be taller, healthier, complete more years of schooling, have stronger labor market performance, and achieve higher political status in adulthood. However, the findings also show that the reform significantly reduced human capital investment in those exposed at critical school ages. Individuals exposed to the reform at age 6 were less likely to have received formal education and were more likely to have stayed in rural areas over the long term.

From a research perspective, this study's findings corroborate the fetal origins hypothesis and standard economic models of optimal human capital investment. From a policy perspective, this study informs neglected long-run benefits of property reform, as well as its unintended consequences on the distribution of schooling. In most agricultural economies where land is the major source of subsistence and income, improved land property rights may disproportionately increase income in the low-skill, agricultural sector. This likely increases opportunity costs of schooling and depresses returns to education, leading to decreased human capital investment in children. Likewise, many anti-poverty workfare programs in the developing world provide low-skill job opportunities to the poor and may have similar effects.[12] Given the significance of human capital in the process of economic growth, a comprehensive understanding of these programs' costs and benefits is crucial for policymakers, so as to balance multiple objectives.

[12]For instance, Shah and Steinberg (2019) find that India's NREGS decreases children's school enrollment and academic performance.

3.7 Tables and Figures

Table 3.1: Summary Statistics

VARIABLES	Mean	s.d.	Obs.
Early Life Sample:			
Male	0.464	0.499	2,629
HRS Adoption Rate in Birth Year, %	46.37	43.39	2,572
High School Completion	0.255	0.436	2,627
Years of Schooling	8.347	4.257	2,627
Height, *cm*	164.8	7.594	2,602
Health Score, 1–7	5.618	1.086	2,625
Urban residency	0.195	0.396	2,627
Monthly Income, *yuan*	1,087	1,827	2,605
ISEI Occupational Score	35.96	15.46	1,607
Communist Party Member	0.039	0.193	2,629
Precipitation in Birth Year, log	4.180	0.549	2,629
Physicians per ten thousand in Birth Year	2.225	0.930	2,629
School-age Sample:			
Male	0.465	0.499	3,440
HRS Adoption Rate at Age 6, %	43.44	43.27	3,400
Primary Education or above	0.784	0.411	3,440
Years of Schooling	7.061	4.477	3,440
Height, *cm*	163.8	7.601	3,392
Health Score, 1–7	5.394	1.164	3,439
Urban residency	0.188	0.390	3,438
Monthly Income, *yuan*	998.5	1,589	3,414
ISEI Occupational Score	33.09	14.47	2,149
Communist Party Member	0.053	0.224	3,440
Precipitation at Age 6, log	4.167	0.54	3,440
Primary School Teachers per 10,000 at Age 6	3.772	1.513	3,440

Notes: This table reports summary statistics on outcome variables and principal controls. The early-life sample are respondents born during the reform period. The school-age sample are those who turned 6 during the same period (born between 1972 and 1978).

Table 3.2: Effects of Birth-year Reform Exposure on Educational Attainment

VARIABLES	High School			Years of Schooling		
	(1)	(2)	(3)	(4)	(5)	(6)
Birth Year HRS (/10)	.019**	.018**	.018**	.113***	.112***	.115**
	(.007)	(.007)	(.007)	(.039)	(.040)	(.042)
Mean of dep. var.	.245	.245	.245	8.261	8.261	8.261
Trends	N	Y	Y	N	Y	Y
Time-varying controls	N	N	Y	N	N	Y
Observations	2,570	2,570	2,570	2,570	2,570	2,570
R-squared	.104	.106	.107	.144	.146	.146

Notes: The dependent variable is an indicator for high school completion in Columns 1–3, and is total years of schooling in Columns 4–6. All regressions control for birth province and birth year fixed effects. Columns 2 and 4 also include birth year linear trend interacted with pre-reform provincial characteristics (distance to Beijing, cumulative famine mortality, rural population share, and per capita grain output in 1978). Columns 3 and 6 additionally control for the log of rainfall, and the number of doctors per capita in the birth year. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 3.3: Effects of Birth-year Reform Exposure on Health

VARIABLES	Height		Health Score		Above Average	
	(1)	(2)	(3)	(4)	(5)	(6)
Birth Year HRS (/10)	.450**	.488**	.044**	.050***	.030***	.033***
	(.218)	(.209)	(.016)	(.018)	(.011)	(.011)
Mean of dep. var.	164.8	164.8	5.608	5.608	.602	.602
Trends and controls	N	Y	N	Y	N	Y
Observations	2,545	2,545	2,568	2,568	2,568	2,568
R-squared	.079	.085	.060	.063	.044	.045

Notes: The dependent variable is height measured in centimeters in Columns 1–2, health score evaluated by the enumerators on a 1-7 scale in Columns 3-4, and is an indicator for whether the health score is above average in Columns 5-6. All regressions control for birth province and birth year fixed effects. Additionally, even columns control for birth year linear trend interacted with pre-reform provincial characteristics (distance to Beijing, cumulative famine mortality, rural population share, and per capita grain output in 1978), the log of rainfall, and the number of doctors per capita in the birth year. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 3.4: Effects of Birth-year Reform Exposure on Socioeconomic Status

VARIABLES	Urban Residency		Monthly Income	
	(1)	(2)	(3)	(4)
Birth Year HRS (/10)	.017**	.014*	59.31*	61.19*
	(.008)	(.008)	(31.61)	(32.20)
Mean of dep. var.	.186	.186	1,062	1,062
Trends and controls	N	Y	N	Y
Observations	2,570	2,570	2,548	2,548
R-squared	.068	.071	.088	.089

VARIABLES	Occupational Score		Party Membership	
	(5)	(6)	(7)	(8)
Birth Year HRS (/10)	.772**	.770**	.010***	.011***
	(.317)	(.330)	(.003)	(.004)
Mean of dep. var.	35.70	35.70	.039	.039
Trends and controls	N	Y	N	Y
Observations	1,563	1,563	2,572	2,572
R-squared	.096	.101	.034	.036

Notes: The dependent variable is an indicator for urban *hukou* in Columns 1–2, monthly income in Columns 3–4, ISEI occupational score in Columns 5–6, and is an indicator for affiliation with the Chinese Communist Party in Columns 7–8. All regressions control for birth province and birth year fixed effects. Additionally, even columns control for birth year linear trend interacted with pre-reform provincial characteristics (distance to Beijing, cumulative famine mortality, rural population share, and per capita grain output in 1978), the log of rainfall, and the number of doctors per capita in the birth year. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 3.5: Effects of Reform Exposure in Years Before and After Birth

VARIABLES	Years of Schooling	Height	Health Score	Urban Residency	Monthly Income	Occ. Score	Party Member
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year -2	.190*	.288	.010	.015	21.94	.807	.008
	(.096)	(.192)	(.023)	(.010)	(29.46)	(.547)	(.007)
Year -1	.022	-.193	.011	.020**	-55.45	-.110	.009
	(.087)	(.228)	(.017)	(.009)	(57.08)	(.690)	(.007)
Birth year	.169**	.468**	.056**	.015	97.80**	1.171***	.009*
	(.068)	(.215)	(.025)	(.010)	(45.95)	(.394)	(.005)
Year 1	-.025	.364	-.032	.010	-49.09*	-1.477***	.002
	(.065)	(.232)	(.021)	(.011)	(26.99)	(.407)	(.004)
Year 2	.027	-.198	-.003	.015*	11.70	.080	-.003
	(.079)	(.201)	(.026)	(.008)	(36.92)	(.686)	(.005)
Mean of dep.	8.167	164.7	5.593	.173	1,029	35.32	.038
Trends & Ctrl's	Y	Y	Y	Y	Y	Y	Y
Observations	2,508	2,484	2,506	2,508	2,487	1,518	2,510
R-squared	.141	.085	.061	.064	.085	.108	.027

Notes: The dependent variables in Columns 1–7 are total years of schooling, height measured in centimeters, health score evaluated by the enumerators on a 1-7 scale, an indicator for urban *hukou*, monthly income, ISEI occupational score, and a dummy for affiliation with the ruling Chinese Communist Party, respectively. All regressions include the full set of control variables. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses.

Table 3.6: Effects of Reform Exposure at Age 6 on Educational Attainment

VARIABLES	Formal Education			Years of Schooling		
	(1)	(2)	(3)	(4)	(5)	(6)
HRS at Age 6 (/10)	-.011*	-.012**	-.011**	-.137	-.141*	-.142*
	(.006)	(.005)	(.005)	(.083)	(.082)	(.081)
Mean of dep. var.	.782	.782	.782	7.019	7.019	7.019
Trends	N	Y	Y	N	Y	Y
Time-varying controls	N	N	Y	N	N	Y
Observations	3,400	3,400	3,400	3,400	3,400	3,400
R-squared	.151	.152	.152	.160	.161	.161

Notes: The dependent variable is an indicator for any formal education in Columns 1–3, which takes the value 1 if an individual has ever received primary education, and is total years of schooling in Columns 4–6. All regressions control for birth province and birth year fixed effects. Columns 2 and 4 also control for birth year linear trend interacted with pre-reform provincial characteristics (distance to Beijing, cumulative famine mortality, rural population share, and per capita grain output in 1978). Columns 3 and 6 additionally control for the log of rainfall, and the number of primary school teachers per capita at age 6. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 3.7: Effects of Reform Exposure at Age 6 on Adult Health

VARIABLES	Height		Health Score		Above Average	
	(1)	(2)	(3)	(4)	(5)	(6)
HRS at Age 6 (/10)	-.006	-.019	-.009	-.009	-.006	-.006
	(.078)	(.075)	(.023)	(.024)	(.008)	(.008)
Mean of dep. var.	163.8	163.8	5.388	5.388	.524	.524
Trends and controls	N	Y	N	Y	N	Y
Observations	3,352	3,352	3,399	3,399	3,399	3,399
R-squared	.090	.092	.073	.074	.050	.053

Notes: The dependent variable is height measured in centimeters in Columns 1–2, health score evaluated by the enumerators on a 1-7 scale in Columns 3-4, and is an indicator for whether the health score is above average in Columns 5-6. All regressions control for birth province and birth year fixed effects. Additionally, even columns control for birth year linear trend interacted with pre-reform provincial characteristics (distance to Beijing, cumulative famine mortality, rural population share, and per capita grain output in 1978), the log of rainfall, and the number of primary school teachers per capita at age 6. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses (***$p<0.01$, **$p<0.05$, *$p<0.1$).

Table 3.8: Effects of Reform Exposure at Age 6 on Socioeconomic Status

VARIABLES	Urban Residency		Monthly Income	
	(1)	(2)	(3)	(4)
HRS at Age 6 (/10)	-.018***	-.017***	3.409	-.815
	(.005)	(.005)	(25.99)	(25.52)
Outcome mean	.182	.182	979.5	979.5
Trends and controls	N	Y	N	Y
Observations	3,398	3,398	3,374	3,374
R-squared	.095	.098	.098	.100

VARIABLES	Occupational Score		Party Membership	
	(5)	(6)	(7)	(8)
HRS at Age 6 (/10)	-.617*	-.659*	.005	.005
	(.343)	(.339)	(.003)	(.003)
Outcome mean	32.97	32.97	.052	.052
Trends and controls	N	Y	N	Y
Observations	2,119	2,119	3,400	3,400
R-squared	.109	.113	.029	.031

Notes: The dependent variable is an indicator for urban *hukou* in Columns 1–2, monthly income in Columns 3–4, ISEI occupational score in Columns 5–6, and is an indicator for affiliation with the Chinese Communist Party in Columns 7–8. All regressions control for birth province and birth year fixed effects. Additionally, even columns control for birth year linear trend interacted with pre-reform provincial characteristics (distance to Beijing, cumulative famine mortality, rural population share, and per capita grain output in 1978), the log of rainfall, and the number of primary school teachers per capita at age 6. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Figure 3.1: Total Factor Productivity (TFP) in Chinese Agriculture, 1952–1988

Notes: This figure plots the TFP index in Chinese agriculture by year. The TFP in the base year (1952) is normalized to 100. Data are drawn from Lin (1990).

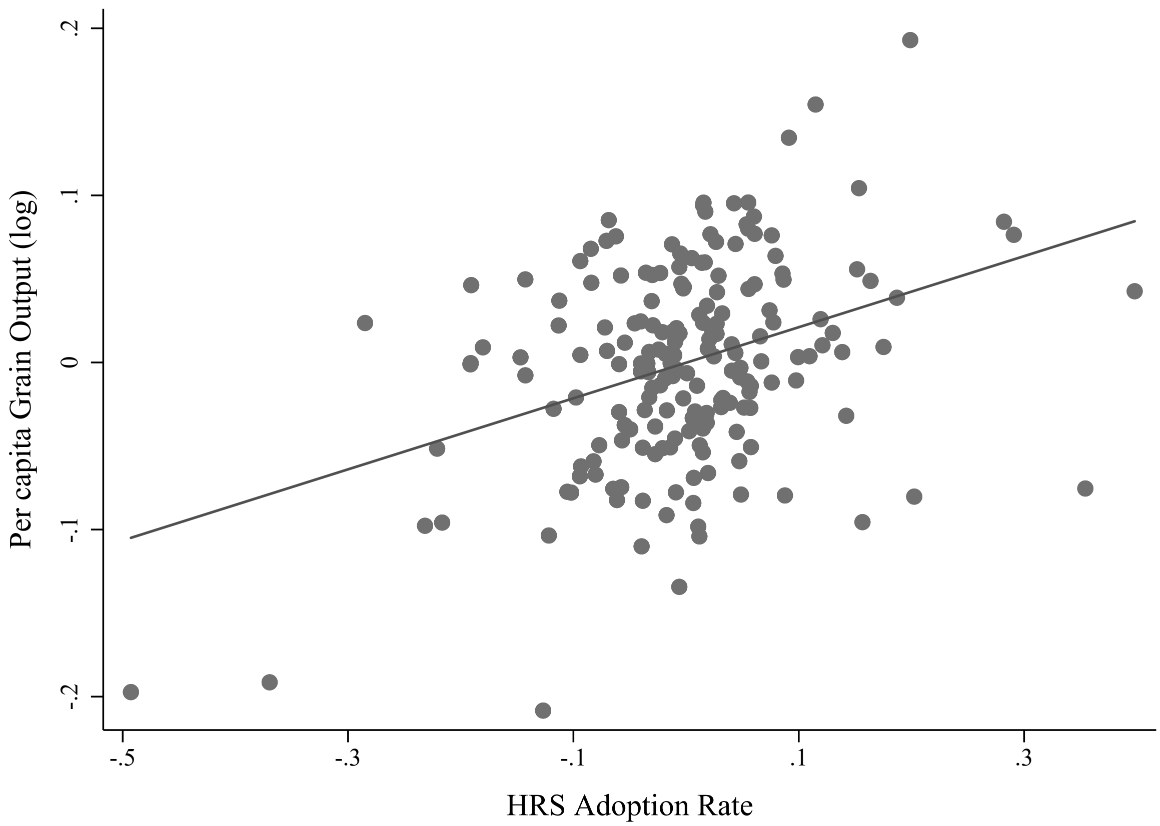


Figure 3.2: HRS Reform and *per capita* Grain Output

Notes: This figure plots residuals from regressions of the per capita grain output and the HRS adoption rate. The regressions include province fixed effects, year fixed effects, and province-specific time trends. Corresponding estimates are reported in Column 4 of Appendix Table 3.9.

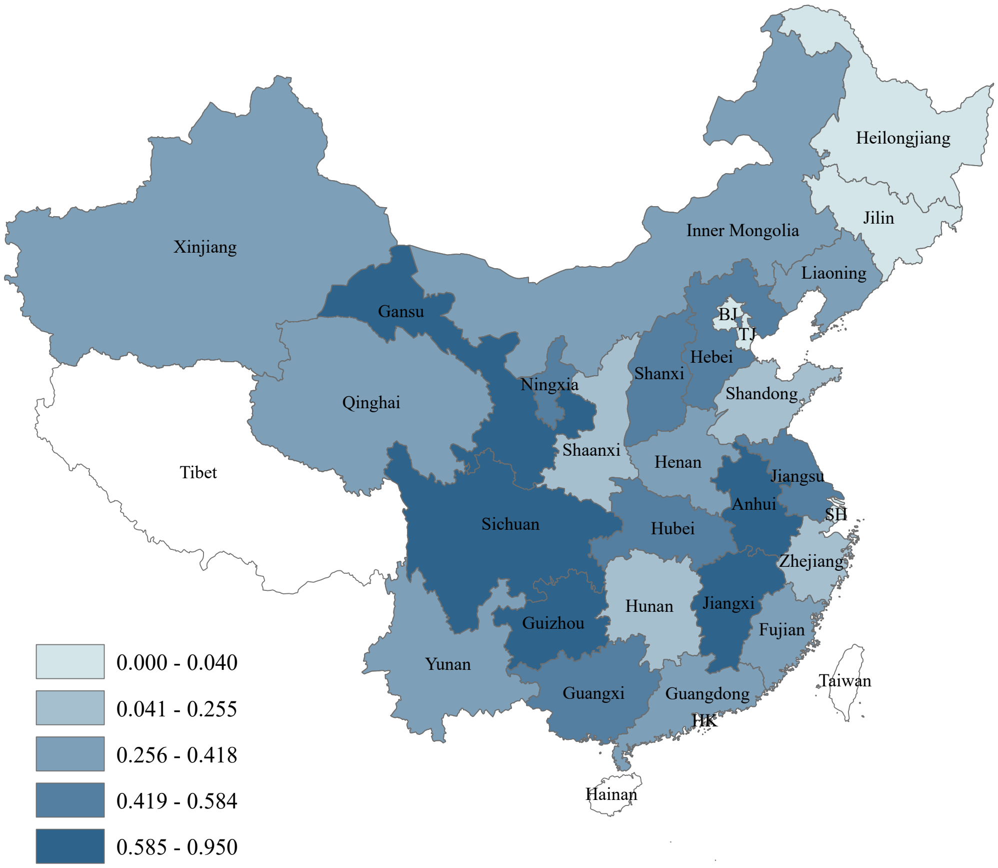


Figure 3.3: HRS Adoption Rate across Chinese Provinces in 1981

Notes: This figure demonstrates the variation in HRS adoption rate across Chinese provinces in 1981. The adoption rate is defined as the fraction of production teams that had adopted HRS by the end of each year.

3.8 Appendix: Additional Results

Table 3.9: Effects of HRS Reform on *per capita* Grain Output

VARIABLES	per capita Grain Output		Log per capita Grain Output	
	(1)	(2)	(3)	(4)
HRS Adoption Rate	174.9**	195.5**	.217***	.213**
	(68.45)	(94.35)	(.075)	(.090)
Mean of dep. var.	437.1	437.1	6.037	6.037
Province-Specific Trends	N	Y	N	Y
Observations	190	190	190	190
R-squared	.841	.943	.931	.964

Notes: The unit of observation is province by year. The sample period is 1978–1984. Columns 1 and 2 use per capita grain output as the dependent variable. Columns 3 and 4 use the log of per capita grain output as the dependent variable. Columns 1 and 3 control for province and year fixed effects. Columns 2 and 4 also include province-specific linear time trends. Robust standard errors clustered at the province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 3.10: Province-level Predictors of Reform Timing

VARIABLES	Start Year of Reform							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log GDP	-.011							-4.808
	(.091)							(21.79)
log per capita GDP		.091						3.943
		(.128)						(21.79)
log Population			-.046					4.962
			(.079)					(21.81)
Rural pop share				-.958				-4.187***
				(.724)				(1.445)
Distance to Beijing					.023			.103*
					(.024)			(.056)
Total famine deaths						-.005*		-.006**
						(.002)		(.003)
log per capita Physicians							.125	.238
							(.083)	(.143)
R-squared	.000	.009	.006	.061	.004	.078	.025	.318

Notes: This table reports point-estimates and robust standard errors from cross-province regressions of reform timing on selected 1978 provincial characteristics. The dependent variable is the year in which each province started adopting the HRS. The distance variable is defined as zero for Beijing (***p<0.01, **p<0.05, *p<0.1).

Table 3.11: Effects of Birth-year Reform Exposure on Employment

VARIABLES	Employment Status		
	(1)	(2)	(3)
Birth Year HRS (/10)	-.012	-.014	-.013
	(.011)	(.011)	(.011)
Mean of dep. var.	.648	.648	.648
Trends	N	Y	Y
Time-varying controls	N	N	Y
Observations	2,499	2,499	2,499
R-squared	.046	.047	.048

Notes: The dependent variable is an indicator that equals 1 if respondents are currently employed. All regressions control for birth province and birth year fixed effects. Column 2 also includes birth year linear trend interacted with pre-reform provincial characteristics (distance to Beijing, cumulative famine mortality, rural population share, and per capita grain output in 1978). Column 3 additionally controls for the log of rainfall and the number of doctors per capita in the birth year. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 3.12: Effects of Birth-year Reform Exposure, by gender

VARIABLES	Years of Schooling	Height	Health Score	Urban Residency	Monthly Income	Occ. Score	Party Member
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Birth Year HRS (/10)	.098*	.148	.052***	.014*	36.17	.818**	.012***
	(.056)	(.149)	(.018)	(.007)	(29.84)	(.366)	(.004)
HRS × Male	.002	.110**	-.010	-.001	6.710	-.070	-.004
	(.055)	(.042)	(.009)	(.006)	(21.14)	(.240)	(.002)
Mean of dep.	8.261	164.8	5.608	.186	1,062	35.70	.039
Trends & Ctrl's	Y	Y	Y	Y	Y	Y	Y
Observations	2,570	2,545	2,568	2,570	2,548	1,563	2,572
R-squared	.152	.600	.065	.072	.152	.101	.045

Notes: The dependent variables in Columns 1–7 are total years of schooling, height measured in centimeters, health score evaluated by the enumerators on a 1-7 scale, an indicator for urban *hukou*, monthly income, ISEI occupational score, and a dummy for affiliation with the ruling Chinese Communist Party, respectively. All regressions control for birth province and birth year fixed effects, birth year linear trend interacted with pre-reform provincial characteristics (distance to Beijing, cumulative famine mortality, rural population share, and per capita grain output in 1978), a dummy for male respondents, the log of rainfall, and the number of doctors per capita in the birth year. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 3.13: Placebo – Effects of Birth-year Reform Exposure on the Urban Born

VARIABLES	Years of Schooling	Height	Health Score	Urban Residency	Monthly Income	Occ. Score	Party Member
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Birth Year HRS (/10)	-.245	.026	-.081	-.004	-84.05	1.317	-.029
	(.167)	(.282)	(.070)	(.003)	(78.48)	(1.060)	(.019)
Mean of dep.	12.88	168.0	5.986	.986	2,071	49.37	.106
Trends & Ctrl's	Y	Y	Y	Y	Y	Y	Y
Observations	510	510	510	510	510	363	510
R-squared	.235	.160	.167	.061	.188	.200	.159

Notes: The dependent variables in Columns 1–7 are total years of schooling, height measured in centimeters, health score evaluated by the enumerators on a 1-7 scale, an indicator for urban *hukou*, monthly income, ISEI occupational score, and a dummy for affiliation with the ruling Chinese Communist Party, respectively. All regressions control for birth province and birth year fixed effects, birth year linear trend interacted with pre-reform provincial characteristics (distance to Beijing, cumulative famine mortality, rural population share, and per capita grain output in 1978), the log of rainfall, and the number of doctors per capita in the birth year. The sample includes respondents born during the reform period in urban areas. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses (***$p<0.01$, **$p<0.05$, *$p<0.1$).

Table 3.14: Effects of Reform Exposure at Age 6 – controlling for adjacent years

VARIABLES	Formal Education	Years of Schooling	Height	Health Score	Urban Residency	Monthly Income	Occ. Score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HRS at Age 6 (/10)	-.015**	-.152*	-.003	-.003	-.014**	-7.058	-.737
	(.006)	(.085)	(.127)	(.022)	(.006)	(32.88)	(.534)
Mean of dep.	.779	6.975	163.8	5.381	.177	955.4	32.72
Trend & Ctrl's	Y	Y	Y	Y	Y	Y	Y
Observations	3,357	3,357	3,309	3,356	3,355	3,331	2,085
R-squared	.154	.159	.093	.074	.098	.084	.099

Notes: The dependent variables in Columns 1–7 are an indicator for formal education, total years of schooling, height measured in centimeters, health score evaluated by the enumerators on a 1-7 scale, an indicator for urban *hukou*, monthly income, and ISEI occupational score, respectively. All regressions control for birth province and birth year fixed effects, birth year linear trend interacted with pre-reform provincial characteristics (distance to Beijing, cumulative famine mortality, rural population share, and per capita grain output in 1978), the log of rainfall and the number of primary school teachers per capita at age 6, and reform adoption rates through ages 5 to 8. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses (***$p<0.01$, **$p<0.05$, *$p<0.1$).

Table 3.15: Effects of Reform Exposure at Age 6, by gender

VARIABLES	Formal Education	Years of Schooling	Height	Health Score	Urban Residency	Monthly Income	Occ. Score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HRS at Age 6 (/10)	-0.011**	-0.139*	0.010	-0.011	-0.018***	1.592	-0.581*
	(0.005)	(0.077)	(0.074)	(0.026)	(0.006)	(21.971)	(0.316)
HRS * Male	0.001	-0.002	-0.039	0.005	0.001	-0.828	-0.099
	(0.005)	(0.044)	(0.047)	(0.012)	(0.003)	(10.487)	(0.156)
Mean of dep.	.782	7.019	163.8	5.388	.182	979.5	32.97
Trends & Ctrl's	Y	Y	Y	Y	Y	Y	Y
Observations	3,400	3,400	3,352	3,399	3,398	3,374	2,119
R-squared	.164	.177	.551	.080	.098	.193	.117

Notes: The dependent variables in Columns 1–7 are an indicator for formal education, total years of schooling, height measured in centimeters, health score evaluated by the enumerators on a 1-7 scale, an indicator for urban *hukou*, monthly income, and ISEI occupational score, respectively. All regressions control for birth province and birth year fixed effects, birth year linear trend interacted with pre-reform provincial characteristics (distance to Beijing, cumulative famine mortality, rural population share, and per capita grain output in 1978), a dummy for male respondents, the log of rainfall, and the number of primary school teachers per capita at age 6. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses (***$p<0.01$, **$p<0.05$, *$p<0.1$).

Table 3.16: Effects of Reform Exposure at Age 6 on the Urban Born (*Placebo Test*)

VARIABLES	Formal Education	Years of Schooling	Height	Health Score	Urban Residency	Monthly Income	Occ. Score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HRS at Age 6 (/10)	.005	.132	-.277	.001	.007	10.72	.301
	(.005)	(.103)	(.343)	(.024)	(.006)	(117.3)	(.487)
Mean of dep.	.983	11.86	166.9	5.748	.983	2,956	46.04
Trends & Ctrl's	Y	Y	Y	Y	Y	Y	Y
Observations	536	536	536	536	536	536	359
R-squared	.100	.177	.170	.194	.079	.121	.135

Notes: The dependent variables in Columns 1–7 are an indicator for formal education, total years of schooling, height measured in centimeters, health score evaluated by the enumerators on a 1-7 scale, an indicator for urban *hukou*, monthly income, and ISEI occupational score, respectively. All regressions control for birth province and birth year fixed effects, birth year linear trend interacted with pre-reform provincial characteristics (distance to Beijing, cumulative famine mortality, rural population share, and per capita grain output in 1978), the log of rainfall, and the number of primary school teachers per capita at age 6. The sample includes respondents born between 1972 and 1978 in urban areas. Estimates are weighted using survey weights. Robust standard errors clustered at the birth province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table 3.17: Effects of HRS Reform on Birth Cohort Size (*Selection Checks*)

	Log Birth Cohort Size			Log Rural Birth Cohort Size		
VARIABLES	year t	year t+1	year t-1	year t	year t+1	year t-1
	(1)	(2)	(3)	(4)	(5)	(6)
Birth-year HRS /10	-.000	.001	-.004	.007	.010	-.004
	(.007)	(.004)	(.009)	(.008)	(.006)	(.009)
Mean of dep.	8.581	8.581	8.581	8.333	8.333	8.333
Observations	190	190	190	190	190	190
R-squared	.989	.990	.988	.988	.989	.990

Notes: The unit of observation is province by year. The sample period is 1978–1984. The dependent variables in Columns 1–3 are the log of birth cohort size in the current year (t), following year (t+1), and previous year (t-1), respectively. Corresponding rural birth cohort sizes are used as dependent variables in Columns 4-6, respectively. All regressions control for province and year fixed effects. Robust standard errors clustered at the province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

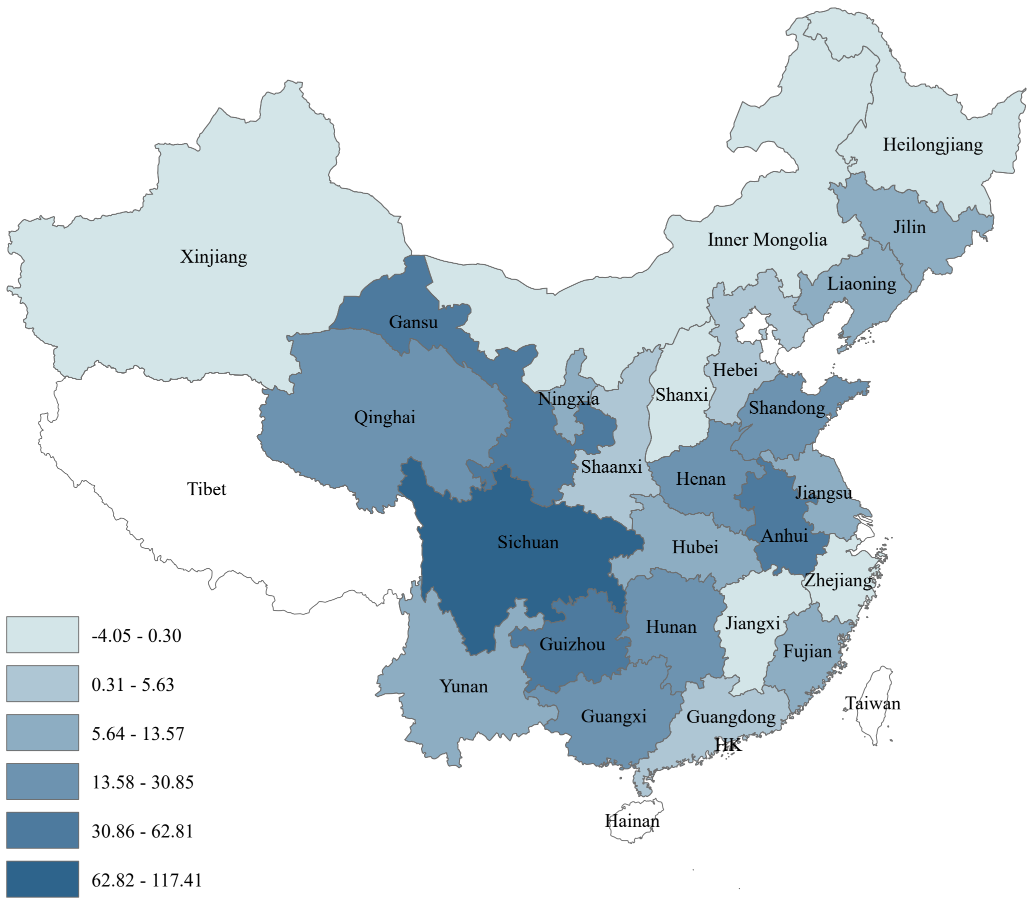


Figure 3.4: Cumulative Famine Mortality during China's Great Leap Famine

Notes: This figure demonstrates the cumulative famine deaths in each province from 1958 to 1962. Famine mortality is defined as the difference in mortality between famine and non-famine years (1955-1957).

3.9 Appendix: Selection Checks

The main analysis shows that HRS reform has a positive effect on various adult outcomes for individuals exposed early in life; it has a negative effect on educational attainment for those exposed at critical school ages. I interpret these results as evidence consistent with the fetal origins theory and standard models of optimal human capital investment, respectively. In this section, I evaluate whether the interpretations will be threatened by the selective child mortality or endogenous fertility responses. The following equation is similar to that used in Miller and Urdinola (2010):

$$\text{Birth}_{pt} = \alpha + \beta \times \text{Adoption}_{pt} + \gamma \times X + \mu_p + \delta_t + \epsilon_{pt}.$$

The dependent variable, Birth_{pt}, is the log of the birth cohort size in province p and year t, constructed from the 1990 wave of the Chinese population census. Adoption_{pt} again is the reform adoption rate in that province and year. I use data from 1978 to 1984 and control for province and year fixed effects for all regression models.

Column 1 of Table 3.17 presents the basic results. Column 2 uses birth cohort size in the leading year $t+1$ as the dependent variable to account for the fact that most fertility decisions are made roughly one year before the children are born. The birth cohort size in the previous year $t-1$ is used in Column 3. The rationale for using birth cohort size from the previous year is that if reform in the current year t increases the infant mortality rate, especially among children aged 1 or below, there should be a smaller cohort size for the previous year $t-1$ (Miller and Urdinola, 2010). Columns 4 through 6 restrict the analysis to individuals born in rural areas and report the analogous regression results. Overall, the results indicate that HRS reform has no discernible effect on birth cohort size. This suggests endogenous fertility responses or selective child survival are unlikely to bias the main results.

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