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Essays on urban and environmental economics in developing countries

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Declaration

I certify that the thesis I have presented for examination for the MPhil/PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I declare that my thesis consists of approximately 55,000 words.

Statement of conjoint work

I certify that Chapter 1 of this thesis is co-authored with J. Vernon Henderson and Wei Cai. I contributed 50% of the work. Chapter 4 of this thesis is co-authored with Pascal Jaupart and Nathalie Picarelli. I contributed 50% of the work.

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Abstract

My thesis is comprised of essays that study urban and environmental economic topics in developing countries. Three of the four essays study causal drivers behind the phenomenal urbanization and local economic growth in China. Its rapid growth in the recent decades provides an illustrative case for understanding how the spatial distribution of economic activities is affected by policies regulating factors of production. The fourth essay extends to another developing country, Tanzania, where the challenges posed by climate change faced by populations agglomerating in fast growing urban centers are substantial. This thesis strives to contribute to current research with my understanding of the contexts, utilization of new yet publicly available data, and novel methodology.

The first chapter, "Political favoritism in China's capital markets and its effect on city sizes", examines political favoritism of cities and the effect of that favoritism on city sizes. To study favoritism we focus on capital markets, where defining favoritism is more clear-cut and not confounded with issues of city scale economies. Efficiency in capital markets requires equalized marginal returns to capital across cities, regardless of size. We estimate the city-by-city variation in the prices of capital across cities in China from 1998 to 2007. It shows how the prices facing the highest order political units and overall cross-city price dispersion change with changes in national policy and leadership. Next, the effect of capital market favoritism on city growth after the national relaxation of migration restrictions in the early 2000's is investigated. We develop a simple model to show that those cities facing a lower price of capital respond with larger population increases over the next decade, with the change labor mobility. The elasticity of the city growth rate with respect to the price of capital is estimated to be - 0.07 in the OLS approach and -0.12 in the IV approach.

The second chapter, "Early Chinese development zones: first-mover advantage and persistency", studies the heterogeneous effects of China's special economic zone program by their level of government support and timing of designation. Using a difference-in-differences (DID) approach, I observe that the early national develop-

ment zones in China have substantially greater and persistent success in attracting FDI compared to national zones established later, or those at the provincial level. Early national zones persistently attract higher levels of FDI inflows, attract more internal migration and are of significantly larger city sizes. To investigate whether the persistent success of early national zones is driven by their first-mover advantage or their unobservable high growth potential, I use their stronger ties to overseas Chinese investors in past waves of political instability as instrumental variable. The IV estimates are comparable to DID, suggesting the success of early national zones relative to newer and provincial zones can be attributed to their first-mover advantage. This conclusion also suggest that the large positive impacts found in China in the existing literature of evaluating place-based policies can potentially be driven by a small group of first-movers.

In the third chapter, "Air pollution, regulations, and labor mobility in China", I study the local economic impacts of pollution regulation in China at the time when migration costs fall. On the one hand, environmental regulations impose costs on firms, which tend to reduce local employment. On the other hand, lower pollution levels are an appealing amenity that attracts human capital to the region, possibly providing a boost to economic activity. The overall net effect of these two opposing forces is ambiguous. To investigate this, I study how local economies in China between 2000 and 2010 are affected by two significant reforms in environmental regulations and internal migration. Following the environmental reform, Chinese prefectures face new national air quality standards whose enforcement intensity can be proxied by their existing air quality at the time of the policy introduction. Meanwhile, the migration reform reduces migration costs and allows workers to relocate based on their preferences for air quality across prefectures. To formalize how air quality regulation affects local employment and city sizes by skill types following the two reforms, I first develop a spatial equilibrium model to guide the empirical analysis. To address the non-random spatial distribution of local air quality, I construct a novel instrumental variable of power plant suitability to capture a prefecture's likelihood to pollute heavily. Thermal power plants are major contributors to China's emissions, while electricity distribution and pricing are centralized. Therefore, locations with comparable economic characteristics may differ substantially in their air pollution levels simply because that some host thermal power plants and some do not. The estimation results show that air pollution regulations have an overall adverse impact on local manufacturing employment, with modest reallocation from heavy to non-polluting industries locally. There is little reallocation across space of low-skilled workers, whose employment prospects are more vulnerable under pollution regulation. However, the population of high-skilled workers in heavily polluted prefectures declines, showing their strong preference for air quality as migration costs fall.

The last chapter, "Cholera in times of floods: weather shocks and health in Dar es Salaam", takes a slightly different perspective on urban and environmental issues in developing countries. We examine the challenges faced by urban population in Tanzania as the result of growing urban density and increasing extreme weather occurrences. Urban residents in developing countries have become more vulnerable to health shocks due to poor sanitation and infrastructure. This paper is the first to empirically measure the relationship between weather and health shocks in the urban context of a developing country. Using unique high-frequency datasets of weekly cholera cases and accumulated precipitation for wards in Dar es Salaam, we find robust evidence that extreme rainfall has a significant positive impact on weekly cholera incidences. The effect is larger in wards that are more prone to flooding, have higher shares of informal housing and unpaved roads. We identify limited spatial spillovers. Time-dynamic effects suggest cumulated rainfall increases cholera occurrence immediately and with a lag of up to 5 weeks.

Contents

List of Figures

List of Tables

Chapter 1

Political favoritism in China's capital markets and its effect on city sizes

1.1 Introduction

Policy bias towards politically favored cities in developing countries is a major policy issue (e.g., Ades and Glaeser, 1995; Davis and Henderson 2003) in general as well as in specific countries such as China (World Bank 2014). Simple theoretical models suggest that cities which are favored in national capital markets, in export or import markets or with enhanced transport infrastructure, will attain larger sizes than non-favored cities, increasing inequality in size distributions (Henderson, 1988; Ades and Glaeser, 1995; Duranton, 2007). The empirical work to date has focused on special cases such as favoritism of national capitals (e.g., Ades and Glaeser, 1999; Davis and Henderson 2003) or favoritism of a national leader's birthplace (Holder and Raschky, 2015). For national capital's the idea is either that national leaders favor the place they live and their relatives may work or that they garner key political support from the national capital population. Favoritism of one type or another will shift up returns to living or producing in favored cities, potentially drawing in workers and firms, with the advantages of favoritism being dissipated by increased disamenities from increased populations. This literature suggests that if migration is unrestrained, that can lead to gross over-population of favored cities and the specter of poor living conditions such as congestion and urban slums. Alternatively, countries can try to restrict in-migration to favored cities, as is China in the 1990's and even continuing today.

In these analyses, a better articulation, let alone a political economy model of different aspects of favoritism is lacking, beyond just favoritism or not of the national capital. And empirical evidence on political favoritism is limited and indirect. In the literature which focuses on national capitals in Ades and Glaeser (1995) and David and Henderson (2003), one must worry that capital cities are, for example, often located in prime geographies and have transport links which make the capital the main hub of the country (as opposed to some other city), in potentially an efficient hub and spoke system. Both factors draw in high numbers of people themselves and separately identifying the effect of political bias on size is a challenge. A second challenge is to identify degrees of bias. For example, evidence that per capita public expenditures is higher in a capital city is not evidence of bias per se; higher per capita expenditures in larger cities may be efficient if there are greater relative benefits of such expenditures. Or evidence that capital to labor ratios are higher in larger capital cities is not necessarily evidence of bias, since nominal labor costs are higher.

Keeping in mind the above challenges, we utilize the specific institutional and political setting in China to quantify political favoritism directly, and examine its causal relationship to city sizes comprehensively and carefully. While the context is particular, it will inform us about analysis of other contexts. We first articulate a basis for widespread favoritism in China where it is not just favoritism of one or two political cities versus all others, but shifting favoritism of a wide variety of cities at the expense of the rest, based on the specific and shifting national and local leadership structures, and incentives inherent in that. Second, we use data on Chinese capital markets to study political favoritism. This has two key advantages. In general, defining bias in capital markets is more clear-cut and not confounded with issues of city scale, unlike for analysis of differential per capita public expenditures or capital to labor ratios. Efficiency in capital markets requires equalized marginal returns to capital across cities, regardless of size. The second advantage in looking at capital markets in China is that they can be the key mechanism of political favoritism: banks are still de facto state owned. The state intervenes in capital markets in response to policy initiatives and political pressures, to potentially favor different types of firms and cities.

The final step is to look at the effects of bias on city sizes. There is a cross-section, or long run equilibrium model, where cities with lower costs of capital, ceteris paribus will be larger. While we will examine such a model, estimation is challenged by all the unobservables which might drive differences in city sizes and might be related to the degree of bias, either incidentally or by design. However again, China provides a context where we can construct an empirical framework where inferring the effects of bias is better grounded. We examine the effects of bias on population growth from 2000 to 2010, or how more favored cities grow differentially faster. Looking at growth in a specific context arguably allows us to difference out other fundamentals which affect city sizes. China's internal migration policies provide variation helpful to identification of effects. Prior to 2000, migration in China was legally and sharply limited (Chan, 2010), curtailing the ability of migrants to move to locations with better wages and job opportunities (Au and Henderson, 2006a and 2006b). Around 2000, legal constraints on migration[1] disappear, although migration still faces impediments. This context allows us to argue that there is a regime shift with all cities facing more elastic supply curves of population after 2000, in terms of drawing upon the national reserve of under-employed rural populations. We develop a simple model to show that those favored cities facing a lower price of capital then are likely to respond with larger population increases over the next decade, with the change in supply elasticity.

What do we find? By analyzing firm level data in China from 1998 to 2007, we find that, depending on the year, one or two of the four highest level administrative unit cities in China experience a 20-40% lower price of capital than others. The magnitude of differential in price is similar to the differential in favor of state over private owned firms near the end of our time period. For these four cities, we find that the degree of bias varies overtime in predictable ways as national leadership changes, consistent with our conceptual framework. We then turn to our main analysis which looks at how the price of capital differs city-by-city for the whole set of cities and how these differentials seem to be driven by changing political forces across China as they affect local areas. Finally, we turn to the effects of these differential prices on city sizes and growth rates. The effects on city sizes in a cross-section framework are large. But even in the growth framework where identification is better defended we find an elasticity on growth of about -0.124. A one standard deviation in decrease in the price of capital would increase a city's growth rate from 2000-2010 by 4% given an average growth rate of 5%.

These findings face a variety of identification issues that we try to resolve. A city's cost of capital may be driven by non-political factors and capital prices are not randomly allocated across cities. We construct relevant measures to account for varying local economic culture, differences in historical total factor productivity

[1] For examples, these constraints include: taxes and fees on migration, job quotas, needing official permissions from both the sending and receiving places.

(TFP), national programs promoting investment in specific sectors in which a city has historical employment and built-up comparative advantage, and the like, to shore up our evidence. For city growth analysis, in uncovering causal effects, many other factors may simultaneously affect the cost of capital facing a city and its population growth, including other forms of favoritism such as local infrastructure investments funded from the outside and the assignment of politically favored leaders to faster growing cities. Another issue for this growth analysis involves separating out the positive effects on growth of lower costs of capital from the negative effects of capital market inefficiencies such as allowing inefficient firms to be propped up and to remain in business. We make serious attempts in circumventing these identification issues by use of specific controls and with two different instrument variables. However, most candidates for an instrument for capital market favoritism are inherently likely to have direct effects on city growth, if only through affecting other forms of favoritism which might also affect city growth. We interpret our instrumental variable estimations cautiously, discussing their validity and limitations.

The rest of the paper is organized as the following. Section 1.2 discusses a conceptual framework for local favoritism in capital markets in China and reviews the existing literature and Chinese context. In section 1.3, models and general econometric specifications are respectively developed for measuring political favoritism in the capital market and its link to city growth. Section 1.4 details data sources and examines how the descriptive patterns further motivate this paper. Results on capital market bias and its effect on city growth are separately presented and discussed in Section 1.5. Lastly, section 1.6 concludes.

1.2 Conceptualizing urban political bias in China

1.2.1 Conceptualizing favoritism

Based in part on work by Li (2001, 2002, 2005, 2013), Li (2005), and in Lim, Porter, Romer and Spence (2011), how should we think about spatial favoritism in China's capital markets? As we will discuss in more detail below the Communist Party has various well defined factions. People at the top of a faction are patrons to those below within that faction. At the city level, local leaders are evaluated on the basis of economic performance, meaning literally local GDP growth during their tenure as a local leader. The patrons of a local leader want to make conditions conducive to economic growth in that city, so that leader will get a good evaluation. A simple lever is intervention in local capital markets to ease credit for producers in that city. Next we will discuss mechanisms for how capital is allocated across cities, discussing, in particular, how provincial leaders influence the allocation of capital across cities, noting that provincial leaders also appoint local leaders. The expectation is that provincial leaders appoint people they favor and that they in turn favor the cities of their appointees. This is simply patronage politics. Of course these provincial leaders expect a pay-back: backing within the Party by these local officials as these leaders later seek higher office within the Party.

While this is a general framework we note also that there is an administrative hierarchy among cities in China: provincial level cities (our four visible cities), provincial capitals and other prefectures. By place in the hierarchy, cities have different formal degrees of autonomy, different fiscal responsibilities and resources, as well as potentially differential favoritism in the state influenced capital allocation process. Leaders of provincial level cities are like provincial governors and are appointed by national leaders. These leaders are relatively high up in the political hierarchy; and as such we might expect that leaders of higher level cities are overall favored in national politics and capital market allocations. However, as we will explained later, given the specific factions within the party, national leaders can have allegiance to particular provincial level cities and can favor those over others.

How is it possible for political leaders to distort capital market allocations? To answer that, we examine the literature on the evolution of China's banking system. The Appendix gives more detail but here we note key items. Despite China's economic reforms since 1978 that successfully freed up most output market and induced widespread growth, reforms in factor markets for labor, capital, and land have been slow and incomplete. Banks in China today remain de facto state owned. There have been extensive reforms over the last 20 years designed to put banks more on a market basis and minimize the extent of non-performing loans. However, the Committee of the Chinese Communist Party retains the power to appoint the boards of directors and senior management of banks. The state's interest is not communicated through shareholder's meetings but via the firm-level Communist Party Committee. The Party Committee is not telling banks to act in the interest of shareholders, but in accordance with "stability", "lawfulness", and national "macroeconomic measures". Individuals appointed to bank senior management posts are personnel with standing in the Communist Party hierarchy (Howson, 2010) and move between government and state bank corporate functions. As such, it is difficult for state owned banks to operate independently while facing pressure from different levels of government. In terms of spatial bias, Liu (2007) notes that after the establishment of China's commercial banking system, bank lending concentrated not just on China's state-owned enterprises, but also major cities. One issue is that commercial banks in China have cautiously retrenched credit-extending authority from their local branches (Liu, 2007). Bank branches below provincial level are limited in their autonomy to extend credit to new clients and new investment projects. Branches in cities are allocated funds for loans with stated priorities, and such allocations may reflect the political influence and connections of local leaders to provincial and antional leaders, as they attempt to garner credit for enterprises in their cities. In addition there are national industrial priorities announced once every few years, aimed at expansion of particular industries through better access to credit (State Council, 1989; 1997; 2000; 2005). To the extent such announced priorities actually affect capital allocations, cities with an initial larger base of favored industries may gain. However, interviews we conducted also suggest there is simply a lot of idiosyncratic variation in local practices both in interest rate manipulation and charges and in default provisions. Corruption in the disbursement of loans is analyzed in Nan and Meng (2009).

As the major banks in China are unable to function freely, bias measured in the capital market captures signals of political favoritism. The bias in China's capital market by firm type is well documented. For example, Dollar and Wei (2007) show much lower marginal products of capital in the state compared to private sector in China for 2002-2004, consistent with the notion that private firms have less political influence than state owned firms in capital markets. On the spatial side, early research on China noted the differential access to capital markets as reflected in higher returns to [shortage of] capital in the rural versus urban sector, or higher returns to rural town and village enterprises compared to state owned enterprises (Jefferson and Singhe, 1999; Au and Henderson, 2006a). In this paper we expand these analyses to look more in-depth at spatial biases.

1.2.2 Other relevant literature

There is a literature on estimating the cost of capital. We utilize the traditional average revenue product [ARP] methodology as in Dollar and Wei (2007), which differs from the recent work on China's overall factor market distortions. Starting with Hsieh and Klenow (2009), and extended by Gao (2013) and then by Song and Wu (2013), these papers develop methodologies appropriate to identifying welfare losses from overall factor distortions. Our objective, however, is to quantify specific differentials in the price of capital faced by firms in different locations. Furthermore, we note that some of the simplifying assumptions in the basic Hsieh and Klenow (2009) or Gao (2013) approach are unpalatable in an urban framework. In particular is the assumption of a single market clearing wage. From standard work on systems of cities (Henderson, 1974; Roback, 1982; Duranton and Puga, 2004; Albouy, 2009b), while returns to capital are equalized across space under a free capital market, there are large cross-city differences in nominal wages and corresponding values of labor marginal products under national free mobility of labor which equalizes real wages across cities. Because city sizes, industrial compositions, and costs-of-living differ across cities in a first best equilibrium, nominal wages and marginal products of labor differ also. In developed countries such differences between a big and small city may be 60% or more (Albouy, 2009a, Combes, Duranton and Gobillon, 2012) for both nominal wages and cost-of-living, but in developing countries they can be much more (Henderson, 2002). To do a full analysis of distortions would require quality adjusted wage data for a large sample of Chinese cities. Also Hsieh and Klenow (2009) assume firm constant returns to scale and a single nominal wage clearing all labor markets in a country under free mobility equilibrium. Constant returns to scale has the uncomfortable issue that, with costless trade, all production in an industry in a country should occur solely in the one most efficient firm in that country.

Apart from the literature on favoritism of major political cities, there is a literature on the uneven distribution of city sizes and their differential growth rates within countries. For example, Duranton and Puga (2013) note that the population of US metropolitan areas range from 0.5 to over 18 million, and that their growth rates from 2000 to 2010 have a standard deviation as large as the mean. And in China more dramatically, the distribution of this figure has a standard deviation double its mean[2]. The relative differences have led to modeling and empirical efforts to explain why there are wide size differences, with explanations based on varying agglomeration economies and specialization, producer and consumer amenities, spatial frictions and the like (e.g., Henderson, 1974; Roback, 1982; Duranton and Puga, 2013; Behrens and Robert-Nicole, 2015). Less is known about the role of institutional factors in affecting size distributions, such as democratization and federalism, although there is suggestive empirical work that federalism and democratization both lead to reduced dispersion, or greater "equality" in spatial allocations (Arzaghi and Henderson, 2005; Fetzer and Shanghavi, 2015). Work on political favoritism especially in less than democratic regimes suggests that politically important cities in countries under dictatorship enjoy substantially better amenities than their counterparts in the hinterlands (Ades and Glaeser, 1995) leading to increased urban primacy. Country-specific evidence on favoritism of the largest political cities is also documented for Indonesia in Henderson and Kuncoro (1996); and, in Albouy (2009a, 2012), there is an analysis of bias in national taxation structures against bigger cities in the US and Canada. This paper will look in a more comprehensive fashion at differential bais across the entire set of cities in a country. A challenge to the literature remains how to incorporate politics and bias into models looking at the differential allocation of resources across cities and the size distribution of cities.

Finally there is a large literature on migration restrictions in China. One strand motivates the empirical approach in looking at the effect on capital cost reductions on city growth, where a key element is the easing of migration restrictions after

[2]Authors' calculation.

2000 (e.g., Chan, 2010; Cai, 2006). Second is a literature which suggests that the easing of such restrictions will lead to increased growth of what are implicitly favored cities. While existing theory (e.g., Duranton, 2007) predicts over-population of favored cities in general under free mobility, Au and Henderson (2006a and 2006b) demonstrate how formal migration restrictions muted the growth of cities in China in the 1990's. Scholars in China then argued that the State understood that such easing would induce greater population growth of the biggest and political cities (Cai, 2006) and that localities would respond by trying to discourage such in-migration, as in Brazil in the 1980's (Feler and Henderson, 2011).

1.3 Models

1.3.1 Quantifying capital market biases

In this subsection we first specify how to estimate the degree of China's capital market favoritism city-by-city. Our objective is to quantify specific differentials in the price of capital faced by firms in different locations. Specific estimates call for the average revenue product [ARP] approach, just as in Song and Wu (2013) when they look at how the costs of capital vary across certain firm attributes, or as in Dollar and Wei (2007) when they look at favoritism by firm type.

The firm produces output with inputs of labor and capital. Output (value added) is taxed by a VAT. In the framework, labor costs are assumed to vary by location and there can be non-optimization in the choice of labor. Output markets are assumed to be relatively free, but the degree of local competition will differ across industries and locations. We allow for non-constant returns to scale.

Firm i in industry j in location s has the optimization problem:

$$\max_{l,k} \; p_{js}^{*} A_{ijs} x_{ij}(l_i, k_i) - w_s^{*} l_i - r_{ijs} k_i$$

where p_{js}^{*} is output price to the firm net of taxes; and depends on industry, location, and competition. A_{ijs} is a Hicks neutral productivity shifter, representing inherent firm efficiency and local scale externalities; l_i, k_i are firm specific firm inputs of labor and capital; w_s^{*} is compensation cost per worker, which varies in equilibrium by location; and r_{ijs} is the price of capital specific to the firm. Optimizing with respect to capital usage yields a first order condition:

$$p^*_{js}(1-\frac{1}{\eta_{js}})A_{ijs}\frac{\partial x_j(l_i,k_i)}{\partial k_i}=r_{ijs}\quad \begin{matrix} > \\ \\ < \end{matrix}\quad \bar{r}$$

where $\bar{r}$ is the true market cost of capital and η_{js} is the elasticity of demand facing a firm in industry j in location s. If $p^*_{js}(1-\frac{1}{\eta_{js}})A_{ijs}\frac{\partial x_j(l_i,k_i)}{\partial k_i}=r_{ijs}\ >\bar{r}$ that implies either the firm faces a higher than market cost of capital or the firm faces a binding quantity constraint that raises its marginal revenue product (and shadow price of capital) above the market return. If $p^*_{js}(1-\frac{1}{\eta_{js}})A_{ijs}\frac{\partial x_j(l_i,k_i)}{\partial k_i}=r_{ijs}\ <\bar{r}$, the firm is getting an effective subsidy in capital markets.

The literature typically approximates the production function as being log-linear, so $x=Ak^{\alpha}l^{\phi}$. Then the first order condition for the use of capital becomes:

$$p^*_{js}(1-\frac{1}{\eta_{js}})\alpha_j x_j\ \ /\ \ k_i=r_{ijs} \tag{1.1}$$

$p^*_{js}x_j$ is measured as value added net of taxes. Taking logs the estimating equation is:

$$\ln(\frac{p^*_{js}x_j}{k_i})=\ln r_{ijs}-\ln(1-\frac{1}{\eta_{js}})-\ln\alpha_j+\varepsilon_{ijs} \tag{1.2}$$

While capital prices are specified to vary by each firm, in practice we have price of capital varying by the typical firm of type i, in industry j and in location s. $\ln r_{ijs}$, relative to a base (e.g., private firms in textiles on the coast in regular prefecture cities) is captured by a set of firm, industry, and location type dummies. We note that the figures discussed earlier assume $\eta_{js}\to 0$, $\alpha_j=\alpha$, $\forall j$. These industry and elasticity terms will now be present and represented by controls to be discussed in section 1.5.1.

Note if equations 1.1 and 1.2 hold exactly, for implementation, issues of selection on for example A's where perhaps better firms go to better locations does not matter per se. The critical assumption is that all firms adjust capital usage until the marginal revenue product which is proportional to the average revenue product equals the price (or shadow price) of capital they face. For the same price of capital, demand elasticity, and capital intensity, firms with higher A's simply expand capital usage until they have the same $p^*_{js}x_j/k_i$, as firms with lower A's, so A is not a right hand side variable. This fact depends critically on the log linear specification. If for example we have a constant elasticity of substitution production function where $x = [al^\rho + bk^\rho]^{\frac{1}{\rho}}$, $\ln(p^*_{js}x_j/k_i) = \frac{1}{1-\rho}\ln r_{ijs} + \frac{1}{1-\rho}\ln[(1-\frac{1}{\eta_{js}})^{-1}(p^*_{js}A_{ijs})^{-\rho}/\ \ \alpha_j]$. Then unobserved A's appear on the right hand side [RHS] and that creates problems in estimation if, say, the allocation of capital costs across firms is related (positively or negatively) to firm efficiency. Second if production is constant elasticity of substitution [CES] the coefficient we estimate is $\frac{1}{1-\rho}\ln r_{ijs}\ =\sigma\ln r_{ijs}$.This says for a given change in the price of capital, responses rise with the elasticity of substitution, σ , in production as would be expected.

The error term in equation 1.2 is for a firm in a city and industry, capturing for example optimization and measurement error. As noted, if the specification is exact there are no identification problems in estimation of capital price differentials, even if capital prices are not randomly allocated across cities. However, two sorts of problems may arise. First is that variables may not fully capture what they are intended to represent. For example, within an industry, α_j's may vary. Older firms with more Soviet technological influence and engineering from the past may favor more capital intensive technologies and younger or foreign direct investment influenced firms may favor lower capital intensive technologies. If capital prices are slanted towards one group or another, that will bias estimates of capital prices by firm type. The second type of problem relates to whether or not technology is log-linear. If not, the unobserved A's can be a RHS variable. If lower capital price are slanted towards cities with higher A's that could result in an under-estimate of capital market bias towards these cities. This has two implications. First our estimates of capital prices themselves could misrepresent the extent of bias. In the results section we will experiment with some controls based on historical TFP by city and firm type to try to control for A's. The second implication is that, when we turn to city growth, systematic biases could be related to factors that affect city growth. While we use a static model of capital allocation, in a dynamic world with durable capital, expectations and risk play a role. Cities that are favored may be viewed as lower risk or may have expectations of higher future growth, both of which would reduce the current returns demanded on investments. These identification problems will be analyzed below in the paper.

1.3.2 Capital market favoritism and city growth

Now, we turn to modeling the effect of capital market favoritism on city size and growth. For a simple and standard city growth framework we use the Roback (1982) model, modified to incorporate the standard systems of cities model from Henderson (1974), as articulated in handbook chapters (Duranton and Puga, 2004; Behrens and Robert-Nicole, 2015; Desmet and Henderson, 2015). On the production side we utilize a log-linear production technology, consistent with the previous specification of estimation of capital market biases. In that specification we assume firms use capital (k) and labor (l) to produce output x, where $x = Ak^{\alpha}l^{\phi}$. Here to simply discussion, we assume firm constant returns to scale and perfect competition. Given these assumptions we set the corresponding unit cost function equal to output price, or $A_s^{-1}cr_s^{\alpha}w_s^{1-\alpha} = p_s$, where for city s, A_s incorporates urban scale economies and city production amenities, c is a collection of parameters, and w and r are respectively the prices of labor and capital.

If we use the usual urban scale economy formulation, L_s^{ε}, where L_s is effective labor in the city to be defined below, then we can rearrange to get:

$$w_s = C_s r_s^{\frac{-\alpha}{1-\alpha}} L_s^{\frac{\varepsilon}{1-\alpha}}$$

where C_s contains c, and the price and any amenity terms.

While this gives us urban wages it does not given us urban real income, or utility. For that we turn to the standard urban model where workers commute in mono-centric city from residences to the city center. Following Duranton and Puga (2004), workers live in a city where they must commute to work in the city center. Each worker is endowed with 1 unit of labor and commuting reduces time spent working at a rate of $4t$ per unit distance commuted. Those living far from the city center spend less on land rents to compensate for their higher commuting costs, or lost labor earnings. City land rents are redistributed to urban workers. Per worker net income, after commuting and land rents are paid and land rent income is redistributed, is $y = w(1 - tN)$ where N is city population[3]. City effective total labor supply net of time spent commuting, L, is $L = N(1 - tN)$. Substituting into $y = w(1 - tN)$ for w from above and for L gives:

$$y_s = Cr_s^{\frac{-\alpha}{1-\alpha}} N_s^{\frac{\varepsilon}{1-\alpha}} (1 - tN_s)^{1+\frac{\varepsilon}{1-\alpha}} \tag{1.3}$$

The economics literature has a standard empirical framework for evaluating the effect of capital price differentials on city sizes, the Roback model. In the standard Roback model there is prefect mobility of labor so everyone nationally earns $\bar{y}$ (relevant to the time period). One might then argue that cities are endowed with differential costs of capital in China in a world of perfect mobility. That then results in differential changes in N_s such that $y_s = \bar{y}$. A city with a higher costs of capital will be smaller than an otherwise similar city (same C_s), or:

$$\frac{d \log N}{d \log r} = \frac{\alpha/(1-\alpha)}{z} < 0, \qquad Z \equiv (1 - tN)^{-1}[\varepsilon/(1-\alpha)(1 - 2tN) - tN] < 0$$

which specifies a relationship between city size and price. Z is signed by imposing stability of city size. In the model just presented, ceteris paribus, y rises, peaks, and then declines as N increases. Stability in the Roback model requires the city be on the downward sloping part of the city size-real income relationship. Note even in this simple framework the implied coefficient of a regression of city size on price of

[3]Following Duranton and Puga (2004), in a linear city, where each worker is endowed with 1 unit of time and working time is $1 - 4tu$ where u is distance from the city center and $4t$ unit commuting costs, it is easy to derive expressions for city labor force L as a function of population N (by integrating over the two halves of the city each of length $N/2$), for the city rent gradient (equating rent plus commuting costs for a person at u with that of a person at the city edge where rents are 0, so they are equally well off in equilibrium) and for total rents. These have forms respectively: $L = N(1 - tN)$; $R(u) = wt(2N - 4u)$; total rents$= wtN^2$; where w is the wage rate. A person living at the city edge and paying zero rent earns in net $w(1 - 2tN_U$, with the diseconomy arising from increasing commuting distances reducing time available to work. After getting a share in urban rent income their net income is $y = w(1 - tN)$.

capital represents complex function of parameters and size. That makes it difficult to learn about specific parameter values. Inspection would suggest effects should be increasing in capital's share in production, α, as would be expected. We will estimate such an equation. However such estimation faces severe missing variables problems: all natural amenity and historical political infrastructure differences (here captured by differentials in A) which might influence sizes and be related to prices of capital.

A standard way to try to deal with problems of missing variables bias in estimation of levels equations is to first difference out the A's. The Chinese context presents a compelling context to help with identification in a growth context. Labor mobility in China prior to 2000 was sharply limited as explained above. A key change is relaxation of at least formal migration restrictions right around 2000, inducing a surge in migration and facing cities with more elastic labor supplies. So our approach is to argue that differentials in capital costs appear before 2000 and persist. The change inducing differential growth is cities' ability overall to attract migrants with an increase in the elasticity of labor supply from the countryside facing all cities. We next argue that cities with lower prices of capital have a larger increase in population with the change in supply elasticity, than cities with higher prices of capital.

To show this we assume the supply curve facing the city in y, N space is $\gamma_s N_s^\delta$ where $\delta = d\log y / d\log N$ is the inverse supply elasticity. We now look at the effect of an increase in δ, or decrease in the supply elasticity of city population. Equating $\gamma_s N_s^\delta$ to y_s in equation 1.3, taking logs:

$$\log\gamma + \delta\log N = \log C - \frac{\alpha}{1-\alpha}\log r + \frac{\varepsilon}{1-\alpha}\log N + (1 + \frac{\varepsilon}{1-\alpha})\log(1 - tN) \tag{1.4}$$

and differentiating $\log N$ with respect to δ, we have:

$$\frac{d \log N}{d\delta} = \frac{\log N}{Z - \delta} < 0, \qquad Z \equiv (1 - tN)^{-1}[\varepsilon/(1-\alpha)(1-2tN) - tN] < 0 \qquad (1.5)$$

where stability would require that $Z - \delta < 0$. So an increase in the supply elasticity or decrease in δ increases city population as must be the case. The issue is to show that a city facing a lower cost of capital has a larger increase in N, or that:

$$\frac{d(d \log N/d\delta)}{d \log N} \frac{d \log N}{d \log r} < 0 \qquad (1.6)$$

This states that, if the cost of capital to a city is higher, the response in city population increase as the supply elasticity changes is reduced. From equation 1.4 we can show that $\frac{d \log N}{d \log r} = \frac{\alpha}{1-\alpha}(Z - \delta)^{-1} < 0$, given $Z - \delta < 0$. Then we need to sign $\frac{d(d \log N/d\delta)}{d \log N}$ by differentiating equation 1.5. This yields $\frac{d(d \log N/d\delta)}{d \log N} = \frac{Nt \log N(1+\frac{\varepsilon}{1-\alpha})+(1-tN)^2(Z-\delta)}{(Z-\delta)^2(1-tN)^2}$. This is not unambiguous since the second term in the numerator is negative, even though the first contains a city size term. One can pick accepted values in the literature on ε (0.02-0.08 in Behrens et al), α (0.25-0.35) and values of t such that real incomes peak at city sizes from anywhere from 100,000 to 10,000,000, to make a numerical evaluation. The bottom line will be as long as the starting value of δ is not too large, the expression will be positive, with a benchmark being Roback where $\delta = 0$. Our results will be consistent with the current signing of equation 1.6.

In estimation, given the equilibrium size condition is non-linear and involves functional form simplifications/approximations, we use a simple growth formulation where:

$$\log N_{st+1} - \log N_{st} = \alpha + b_0 \log r_{st} + X_{st} b_1 + e_{st+1}, \; b_0 < 0. \qquad (1.7)$$

N_{st} is the population for city s at time t; r is our measurement of capital prices from the previous section; X is the set of other characteristics that may affect growth. The sign of b_0 is based on equation 1.6. Estimation of equation 1.7 faces obvious challenges such as unobservables correlated with the price of capital which in themselves affect city growth. Our approach to identification is detailed in the later relevant section.

1.4 Data and descriptives

In this paper, we confine the analysis to the 283 prefectures in the provinces of the Han part of China, where the spatial unit defining local market areas is the prefecture, which we have been labeling as a city. The data we use are from the survey of medium and large size industrial firms in each year from 1998 to 2007, the last year the relevant economic data are available. Industry is manufacturing plus utilities. The survey is designed to cover all state owned firms plus all other firms with over 5 million RMB in annual sales. So our sample covers most of industrial output[4]. We trim the samples as is common with the China data, where with typos and mis-reporting there can be significant outliers. Our trimming is modest. We start by removing a tiny fraction of observations which are clearly flawed: output, wages, fixed assets and material inputs are less than or equal to zero; fringe benefits are negative, or current annual depreciation exceeds accumulated depreciation. We then order observations by the ratio of value added to net assets, the dependent variable. We remove the top 2% of observations in each year and we remove at least the bottom 2%. At the bottom we remove all observations with negative value added (rounding up to the nearest integer). In 2007 this is 2%. The fraction rises over time as we go backwards, with the most being 6% in 1998. Tables will report specific numbers for each year.

For establishing capital market bias patterns across groups, we first define city-region and firm types. As noted above for city types in China there is an administrative hierarchy: provincial level cities, provincial capitals and other prefectures. We also distinguish regions (west, central, coast/east) based on policy initiatives, such as those to promote development of the West. Firm types are defined by share

[4]Some firms have sales less than 5m RMB, but in 2007 that is only 2.2% of firms in the data. What are we missing in this data set? From the 2008 Economic Census, 75.3% of industrial firms have under 5M RMB in sales, but they account for only 5.8% of total industrial sales nationally. Nevertheless, to the extent there is even greater discrimination against smaller firms we are potentially understating effects of discrimination.

of paid-in capital rather than legal status per se. The hierarchy of types we identify are (1) wholly state owned firms; (2) majority state owned firms, where 50% or more of paid in capital is from the state; (3) wholly collective owned firms; (4) majority collective owned firms, where 50% or more of paid in capital is from the collective, (unless the state owns 50%); (5) wholly private owned firms; (6) majority private owned firms, where 50% or more of paid in capital is private (unless the state or collective owns 50%); (7) wholly foreign owned firms; and (8) majority foreign owned firms, where 50% or more of paid in capital is foreign owned (unless the state, collective, or private owns 50%)[5]. Table 1 shows the dramatic change in industrial structure for the 10 years, 1998-2007. In 1998 only 19% of firms are wholly private firms, while in 2007 that has risen to 72%. Correspondingly there is a truly dramatic drop in the relative number of firms in the state owned sector, but a more modest drop in the state owned sector's share of value added. These reflect state policy focused on limiting the state owned sector size, with the state sector focused on strategic key industries, with typically large plant sizes.

In gauging bias in terms of cost of capital, people typically look at two types of data. First is quantity data. Quantity data suggest that state-owned enterprises are favored in capital markets: in 2011, state enterprises only contributed 26.2% to national industrial output but still represented 43.9% of total debt; the corresponding numbers for private firms were 29.9% and 17.8% (China Statistical Yearbook, 2012). For cities, the City Statistical Yearbooks tell us that in fixed assets of industrial enterprises per capita from 2002-2007 in provincial level cities was almost double that in ordinary prefecture level cities despite the greater relative presence of manufacturing in the latter types of cities. However, the problem with quantity data is that quantities may differ for reasons other than favoritism: differential total factor productivity; varying efficiency by types of firms; disparate sub-industry composition of firms in different types of cities; and better economic fundamentals in some city types than others.

[5]Note we break the small number of ties (0.34% of total firms) where ownership is equally split (50/50) in a hierarchical fashion based on a modest presumption about level of political influence (highest to state, then collective, then domestic private, then foreign). We drop the tiny percent (0.56% in 2007) of firms where there is no majority control. The issues with this firm hierarchy have been well discussed in the literature and it is not our focus (specifically to capital markets there is Dollar and Wei, 2007).

Thus, the more compelling raw data evidence is to look at the distributions of a simple measure that is proportional to the private marginal product of capital under certain assumptions – perfect competition, equal capital intensity across industries, and log-linear production functions. As discussed in the previous section, that measure is the ratio of after-tax value added to net assets[6] (as defined in equation 1.2). Before getting into the econometric details, we first present here descriptive patterns of this key variable to establish the existence of large scale capital cost variations across firm types and cities in China. The descriptives support two central points. The first is more well known: favoritism by firm type. Figure 1 for 2007 compares the (log) marginal returns of firms which are wholly or majority state owned to those that are wholly privately owned. The distribution for private firms is distinctly shifted to the right, indicating that they face either or both higher costs of capital or restricted access to capital markets (and thus a higher shadow price of capital).

The second point is that, controlling for firm type, certain cities per se are favored in capital markets. While the approach when analyzing city growth across China will be general, in the first part of the paper, we focus on known political cities and regions, compared to more ordinary cities. Figures 2 and 3 examine an aspect of this spatial dimension. Here the differentials are a little more modest but still compelling. Figure 2 compares the returns for all wholly private firms in non-provincial level cities nationally with (1) all firms in the 3 east coast provincial level cities (Beijing, Tianjin and Shanghai) and (2) just wholly private firms in the same 3 east coast provincial level cities. The latter two distributions overlap suggesting that within provincial level cities, by 2007, all firms may be treated fairly equally,

[6] A discussion is due for the controversial measure of capital stock in the existing literature. Papers advocate for a perpetual inventory based measure (see Brandt, van Biesebroeck, and Zhang, 2012; or Song and Wu, 2013), using accepted measures of economic depreciation and investment price indices applied to investment flows, with a base of some form of first recorded book value. The problem is that this requires linking firms across years using ID's (that may be incorrectly recorded), without interruption and missing values of investment series. These problems can generate significant losses in sample, with then various fix-ups (Brandt, Tombe, and Zhu, 2013). An alternative is to use either gross book value or net book value based on accounting depreciation and ignoring investment price increases. The tradeoff is that, the longer the series for a firm the worse is the problem between an economic measure of capital stock and an accounting one (which uses different depreciation rates and no indexing for price changes). However, our context is unusual. In the 10 years covered by our sample, the total number of firms increases by 130%. In one critical margin, private firms, the number increased by 780% over the 10 years. About 50% of firms in 2007 have been in the data set for less than 5 years (Brandt, Tombe, and Zhu, 2013). Therefore, we use net book value to avoid loss of sample which means we rely on accounting depreciation rather than an economist's estimate of depreciation and we do ignore price changes for investment although such changes in the mid-2000's are modest (Brandt, Tombe, and Zhu, 2013). We think this is a reasonable way to proceed, but do robustness checks such as controlling for firm age later on.

at least as suggested by the raw data. However the returns for private firms in non-provincial cities nationally are more concentrated and shifted right, compared to firms in provincial level cities, suggesting political cities are favored in capital markets. Figure 3 isolates an example and directly compares returns to privately owned firms in each of the three east coast provincial level cities, Tianjin, Beijing and Shanghai, with the supposedly freer wheeling, more (non-state) capitalistic cities in the south-east, in particular Guangzhou and Shenzhen. The ordering is very suggestive. The worst distributions in terms of favoritism are for Beijing and its twin, Tianjin, the centers of national political influence certainly in 2007. Then there is Shanghai, followed by Guangzhou. Guangzhou as a provincial capital still has a distribution distinctly more concentrated and shifted to the right of those for Beijing, Tianjin and arguably Shanghai. But Shenzhen dominates all these political cities with a distribution distinctly to the right of all the others, reflecting its distinctly higher cost of capital.

1.5 Results

1.5.1 Estimating capital market favoritism

Regression specifications

As established in the previous sections, we estimate differences in the returns to capital by city, accounting for differences in inherent firm and/or location productivity and in the degree of competition in different markets across locations and industries. Recall that in equation 1.2, $\ln(\frac{p^*_{js}x_j}{k_i}) = \ln r_{ijs} - \ln(1-\frac{1}{\eta_{js}}) - \ln\alpha_j + \varepsilon_{ijs}$, the $\ln r_{ijs}$ aspect is captured by fixed effects variously representing firm type, city or region type and individual cities, depending on the specification and issue we are discussing. In equation 1.2, industry fixed effects control for differences in α_j. To control for differences in demand elasticities, we include the number of firms in the industry the observation is in for that prefecture (where the degree of local competition affects the local value of η_{js}). To account for other differences in demand elasticities, for each city-industry a firm is in, we control for measures of overall market scale and access, in particular distance to the coast and GDP within 150 kms of the prefecture city center. Furthermore, to avoid feedback effects, the base industry firm counts are from the 1995 industrial census and the income measures are for 1990 GDP. Results are robust to other dating choices for these controls (such as for 2007 estimation, 2004 Census firm counts and 2000 GDP).

In the following subsections, we present the estimation on capital market favoritism as they pertain to Chinese policies and give suggestive evidence of political influence. We will first focus on the 2007 cross-section pattern to establish the methodology. Then we turn to all ten years of results, which we relate to political events, so as to argue the link between capital market prices and political influence.

Illustrative results on city type and regional favoritism in 2007

Table 2 presents the cross-sectional results for 2007. The first two columns look at the effect on average returns of being a particular type of firm, in a particular type of location. We represent the hierarchy of firm types in each column and a simple characterization of spatial differences, based on city-region types, not individual cities. For these, the base case covers private firms in east coast ordinary prefecture level cities. Column 1 controls only for industry fixed effects, while all other columns reflect the inclusion of city-industry characteristics affecting local demand elasticities. Many of the results are similar but our preferred specification is in column 2.

In column 2, for firm types, relative to wholly private owned firms, wholly and majority state owned firms have about 45-50% lower marginal products and effective prices of capital. Foreign firms have about 25% lower returns; this may reflect their overall easier access to (international) capital than domestic private firms. In the smaller collective sector, wholly owned have modestly higher costs and majority owned modestly lower costs. Finally, compared to wholly owned private firms, majority owned private firms also face lower effective prices of capital. In the Appendix Table A2, we look at these majority owned categories for collectives, foreign and private. If the minority stakeholder is the state, compared to not the state, returns are typically 20 percentage points lower. That is, the role of the state ownership in having better access to capital markets extends beyond majority and wholly state owned firms to ones where the state is a minority stakeholder.

Next, turning to our main focus, spatial biases, in this section, we examine how the price of capital in the four provincial level cities, in provincial capitals as a group, and by west and east region differs relative to ordinary prefecture level cities on the coastal region. Below when we look across years we will build an evolving political story around this. For regions, we ask if the policy of developing the hinterlands, especially the West has any bite in capital markets. We see in column 2 in fact overall the price of capital in the West and Middle regions is higher than on the coast in 2007, despite the "develop the West" rhetoric. When we group together provincial capitals, perhaps surprisingly, *on average* compared to ordinary prefecture cities in the Coast, we find no significant differences. Finally we look at each of the 4 provincial level cities at the top of the hierarchy on their own. For provincial level cities, the twins, Beijing and Tianjin in 2007 have favored access with distinctly lower returns: 19% for Beijing and 30% for Tianjin in column 2. On the other hand Chongqing and Shanghai operate the same as ordinary prefecture level cities on the coast. Below, we will see that these patterns differ over time, as national leadership changes.

In columns 3-6 we conduct robustness checks. First in column 3 we replace the measure of capital stock, net book value, by gross book value. While qualitatively results are the same, using gross book value tends to inflate the degree of bias for, for example, the state owned sector with its older firms (with long series of non-depreciated gross capital numbers). In column 4 we give LAD estimates for column 2 to check that measures based on the median as opposed to average firms are similar. The coefficients in the two columns are remarkably similar. In column 5, we add to column 2 a control for firm age. That has no effect on spatial biases but affects magnitudes for the older state owned firms.

Finally in column 6 we explore the bias issue from missing covariates in a non-log-linear specification, in particular city (and industry) specific A's. We have 1995 industrial census data (where we do not observe paid in capital by firm type). For 1995 we calculate TFP for each firm as $\ln(netVA) - 0.30 \ln k + 0.70 \ln l$. We then regress TFP on industry and city fixed effects, as well as elasticity controls on 1995 numbers of firms in the own industry in the city, ln distance to the coast and 1990 GDP within 150 kms from the city. Then we insert these measures of industry and city TFP into the column 2 specification. The TFP variables themselves have large positive but at best weakly significant effects. Second, they have no effect on other the covariates in this table. This is not to say we do not have an omitted variables problem, but this specification suggests missing A's may not be a key issue.

Margin at which to measure bias

In Table 3 we turn to several issues, which are critical to final choices on how to measure city type or city-by-city biases. Column 1 of Table 3 repeats the base result from column 2, Table 2. The first issue is if we want to represent city bias, do we control for firm type or look just at private firms? Column 2 presents results for just private firms; results on the city-type and regions are very similar as for all firms in column 1. In column 3, we look at all firms again, but remove firm type controls. The idea is that a city faces two sources of bias which we may want to combine. First, all industries are favored by some percent; and, second, a city may have a firm type composition where it has more favored types of firms, such as state owned firms. The combined effect is captured in column 3. Since cities grow both by new firm entry and expansion of existing firms of all types, it seems this overall measure may be more relevant. However, again the city type and region results are very similar to column 1.

Finally in Table 3 in columns 4-6 we turn to the actual measures considered for the growth analysis in Section 3. For these we remove the region and provincial capital variables, and have a fixed effect for every city with Shenzhen being the base city but we just report for the 4 provincial level cities. In column 4 we show results where we control for firm type. In column 5, we remove firm type controls and in column 6 we show for just private firms. We have three key findings. First in column 4 relative to column 1, we get almost identical firm type effects. Second and mostly critically, in columns 4-6 with a full set of individual city fixed effects, where the base city is Shenzhen, results on cities are not really comparable with those in columns 1-3. All we can say is that all provincial level cities are favored relative to

Shenzhen.

The third result is that if we take the 3 sets of city fixed effects in columns 4-6 the three pairwise correlations are all over 0.9775. This suggests that variants of the basic formulation give very similar results. We are generally going to rely in what follows on the results for the full sample without firm type controls, although we will report some results when we add back in firm type controls.

Ten-year patterns and national politics

Are these region, provincial capital, and provincial level city patterns the same over time? We will argue that we expect them to change to some degree with national leadership. The Communist Party has factions and a hierarchy, although a precise description is elusive and there is inter-mingling. Key in the hierarchy are Princelings, who are the descendants of senior communist officials historically in the People's Republic of China (Li, 2013). For factions two stand out, the "Shanghai" branch (*Shanghai bang*) and the Communist Youth League, or what we will call "Beijing" branch (*tuanpai*). Until early 2003, Jiang Zemin was in office and represented the Shanghai branch of the party. As such he may have favored Shanghai relative to other cities. But he was also a reformer and committed to privatization (Li, 2001). When Hu Jintao was selected for office in late 2002 (officially to take effect in early 2003) he represented more the Beijing branch of the party and in subsequent years pulled back on the full thrust of privatization reforms (Li, 2002; Li, 2005). These national changes affected policy, which might be represented in the city-type and region fixed effects, as well as firm type effects.

What are the patterns over time? In Table 4 we repeat the Table 2 column 2 formulation for all years. For firm type variables, we see the massive advantage of state owned firms erodes almost monotonically with time, starting at a 118% discount on the cost of capital and falling to 49%. Collectives experience little change and little advantage or disadvantage over time. The advantage of foreign firms also declines. That could be because capital markets in China improve, so that the overall advantage of access to international capital markets declines. Or it could be that that state promoted (subsidized) foreign firms more in the early years, but less or not at all later on.

Of central interest to this paper are the regional effects. There are some particular patterns in the data of interest, which coincide with changes in national leadership. We do not want to make too much of this, but they do motivate the idea that politics play a significant role in the capital market. In Table 4 and coefficients graphed in Figure 4, relative to ordinary prefecture cities in the east, cities in the West have lower costs of capital under the Jiang Zemin regime but under Hu Jintao that reverses with the West facing higher costs of capital, whatever the rhetoric may have been.

More telling, under the Jiang Zemin regime, Shanghai faced lower costs and Beijing higher costs of capital. That differential disappears by 2003/04 as the Party regime switches more to a Beijing orientation with Hu Jintao coming to office. By 2006/07 under Hu Jintao, the pattern has reversed with Beijing facing lower costs of capital and its twin, Tianjin, being very heavily favored from 2004 on. Under neither regime do provincial capitals as a group experience advantages or disadvantages. The graph in Figure 4 suggests some degree of convergence in these differentials by spatial units towards zero in 2003 then strong divergence after that. Is that a more general pattern in the data: narrowing of spatial differences and then divergence?

To see if the data suggests a more general pattern of convergence and divergence, we turn to the analysis of individual city fixed effects as in column 4 of Table 3. We re-estimate the Table 4 specification, for each year with firm type variables and region and provincial capital indicators removed, but having a complete set of city fixed effects, where Shenzhen is the base case in each year. Changes in the mean are not relevant since that is about Shenzhen versus other cities in China. To look at changes in dispersion we examine the time pattern of the coefficients of variation for these year-by-year city fixed effect coefficients. Figure 5 plots these. The coefficient of variation is lowest at the end of the Jiang Zemin era in 2002. After 2004 and especially as we move into 2006 and 2007 the coefficient escalates, consistent with overall increase in dispersion of capital prices across cities, and retrenchment from aspects of reform in capital markets.

Correspondingly in Table 5 we examine the year-by-year pairwise correlations in these annual vectors of city fixed effects. Two items emerge. On the relevant diagonal, the sequential year correlation is lowest in 03-04 and 04-05, in the early years of Hu Jintao's leadership. Second fixed effects in say 1998 (but any year up to 2002) remain highly correlated with those for subsequent years up until about 2004 when that correlation drops noticeably in years subsequent to 2004. That suggests a change in patterns of city capital market favoritism, timed with the change in national leadership. Again, these are suggestive results, indicating that capital markets in China are politicized and certainly different cities face evolving prices of capital as individual city fortunes evolve over time.

1.5.2 How does capital market favoritism affect growth?

Regression specifications

In this section, we turn to a central theme of the city-urban bias literature: cities which experience favoritism will have larger populations. To try to show this here, we look at the effect of the change in population supply elasticities to cities after 2000, to show that cities with lower prices of capital experience larger population increases. Recall from section 1.3, we propose to estimate a standard city growth equation 1.7:

$$\log N_{st+1} - \log N_{st} = \alpha + b_0 \log r_{st} + X_{st} b_1 + e_{st+1}$$

with expected $b_0 < 0$. Similarly, we do the parallel analysis for the 2010 (log) level of city sizes to shed light on the long-run equilibrium effect:

$$\log N_{s,2010} = \alpha\prime + b\prime_0 \log r_{st} + X_{st} b\prime_1 + e\prime_{st+1} \tag{1.8}$$

with expected $b\prime_0 < 0$.

First, we need to deal with the issue of how to characterize capital market favoritism as it affects city population growth from 2000 to 2010. One idea that relates to identification is to go with pre-period measures of favoritism (i.e., 1998-1999), as occurring before shocks to city growth during the 2000 - 2010 time period (such as later changes in political leadership and contemporaneous favoritism). Note unlike Table 4 where each year is treated separately, to reduce noise we pool two adjacent years to try to capture pre-growth period favoritism. We use a 98-99 measure of city fixed effects, which will be uncorrelated with later shocks affecting growth from 2000 to 2010. However rather than a pre-period measure, we might want to try instead to capture the effects after 2002 of the changes in capital market favoritism that followed changes in national leadership. For this we would want to know the later, say 06-07, level of favoritism. We show a variety of OLS specifications, experimenting with different measures of favoritism, before turning to instrumental variables work.

The challenges to arguing that we identify the causal effects of capital market favoritism on city growth are twofold. First is the possibility of selection bias – it could be that favored cities are ones with more growth potential to begin with – that is favoritism is not randomly sprinkled across cities.In examining prefecture population growth from 2000 to 2010, to try to mitigate the issue of non-random favoritism we control for pre-period growth trends (in an era of strong migration restrictions, 1982-1990). Second, even if favoritism was spatially randomized, favoritism of cities may be favored in dimensions other than capital markets. To try to separate out capital market favoritism from what other aspects the estimated fixed effects might represent, we add relevant controls for which we have measures. There are two main concerns: other types of favoritism and aspects of capital market efficiencies which may be correlated with our measures of capital market favoritism. Other forms of favoritism would be public sector allocations and the one key is infrastructure investment. We might expect that if a city if favored in "private" capital markets it might be favored with subsidized public infrastructure investments. For that, we control for the key item: allocations of major highways to cities in 1999. From Baum-Snow, Brandt, Henderson, Turner and Zhang (2014) we extract a count of major highway rays going out from the city center.

For aspects of capital market (in)efficiencies hindering growth we have two concerns. First is that a lower local price of capital for all firms overall in a city could also be correlated with a local culture of enhanced market reforms and privatization, building on a long standing notion that cities dominated by SOE's suffered from slower reforms, inhibiting growth (Naughton, 2007; see also Au and Henderson, 2006a,b). Second and in contrast, a lower price of capital could be associated with the propping up of local firms who are inefficient, detracting from city growth potential. To deal with these two concerns we utilize two measures from the 1995 Census of Industry. In 1995 we don't have data on types of firms other than by title; from that we extract the share in employment of LTD firms which are supposed to be limited liability private firms in 1995, as a measure of the local culture of reform and enthusiasm for privatization in a city. To deal with aspects of capital market bias which may be correlated with gross inefficiencies in the operation of local markets, for 1995 we control for the share of industrial employment by firms with revenues which do not cover operating costs, or specifically value added is less than labor costs.

After all of these considerations and adjustment to the specification, identification is still a concern. For example, even pre-period measures of capital prices may be correlated with unobserved persistent conditions which affect city growth directly, like aspects of local business culture which persist over time that we don't capture fully with our controls. The use of 06-07 measures of favoritism is even more challenging. Increased favoritism after 2002 may be correlated with other shocks which stimulate city growth. And there may be other dimensions to city favoritism correlated with capital prices which we don't fully capture with our controls. Ideally, in this non-experimental context there would be an instrument which was correlated with capital market favoritism per se, but not with city growth potential or other forms of favoritism. We return to this issue after presenting the OLS results.

OLS Results

Table 6 presents OLS results for city population growth from 2000 to 2010. For prices of capital we use the individual city fixed effect coefficients from specifications where we pool 2 years of data and generally do not control for firm type. Standard errors are bootstrapped. We start with pre-period estimates of city fixed effects as the key covariate. The first 3 columns show 2010 - 1990 growth regressed on a set of pre-period covariates. In column 1 the only covariate is the city fixed effects from

98-99 representing the city-by-city cost of capital (in logs). The effect is insignificant and has a sign that is positive, as opposed to the expected negative sign. Recall cities with higher costs of capital are expected to have smaller population increases after the relaxation of many migration restrictions. Column 2 adds in controls which may be directly correlated with the cost of capital, but have their own effects. In particular, the share of employment by firms which revenues are not covering basic operating costs would detract from growth and be negatively correlated with the cost of capital. Thus adding these controls turns the positive coefficient to the expected negative sign but it is still insignificant. In column 3, we add in other basic controls for growth which could be incidentally (or otherwise) correlated with the cost of capital. These include 1982 population and 82-90 growth of prefecture population as controls both for size and for early growth trends, distance to the coast which represents poorer access to export markets and 1999 road rays which represents greater infrastructure investments in the city. Controlling for these factors makes the cost of capital coefficient more negative and significant at the 1% level. In column 3, a 1% decrease in the cost of capital is associated with an increase in city size of 0.07%. Column 4 repeats column 3 with a measure of 98-99 city fixed effects from a specification which adds in firm type controls; the coefficient is modestly enhanced relative to column 3.

Column 5 presents results for city capital prices measured in 06-07 with strong negative effects of similar magnitude as column 3. This could be the most relevant column. For growth from 2000-2010, it would be the price of capital after the national political regime switch in 2002/03 which should mostly drive growth and a mid-period (for 2003-2010) estimate should capture the average price differential across cities as these start to widen after 2003 (Figure 5). The elasticity in column 5 is a little larger than column 3, at 0.08%. Column 6 controls for both 98-99 and 06-07 capital prices. Both significantly deter growth, with marginal effects being somewhat higher for the later price. These OLS results are suggestive of the effects on growth of capital market favoritism, but there are clearly many sources of bias, in addition to the downward bias of measurement error.

Identification: challenges and solutions

In addressing the endogeneity concerns in the OLS specification discussed above, our hope was to find a suitable instrument for individual city level bias in capital prices. We thought of two potential solutions. First, we turned to national policies supposedly governing differential capital allocations across types of industries and then deriving a city specific version of those based on cities' historical industrial composition, a Bartik (2002) - Card (2001) type instrument. For the second candidate, we turned to political variables, measuring changes in local leadership and their relationship to national factions of the party following the national leadership change in early 2003.

For the Bartik-Card type instrument, once every few years, China's State Council lists industries which development is to be favored by policy, the key to which should be access to capital markets. For 2000 and 2005 we coded the favored industries at the 4 digit level, with a "1" for favored and "0" for not listed. We calculated for both the 1995 census and the 1998 survey of industries the share of each 4-digit industry in 1990 prefecture manufacturing employment (from the 1990 Population Census). We multiplied the share by the indicator of favored or not and summed across the 4-digit industries, to create an index of city likely capital market favoritism from national policy for 2000 and 2005.

In Table 7 we present what might have been a first stage of an IV regression with this index of national favoritism. The 06-07 fixed effects are regressed on the index in 2005 for each city for the two sets of Bartik weights. With regression controls from Table 6 or without controls, this index is unrelated to our measures of capital market prices. Recognizing that many favored industries have been on the books for many years, we looked at just 35 newly favored ones added between 2000 and 2005; again there was no relationship to capital market prices. We also worried that our controls for 2-digit industry effects in the basic city fixed effects regression was limiting variation in the data, and we re-estimated city fixed effects without these controls. But these new fixed effects again are uncorrelated with national industry favoritism indices relevant to each city. All this suggests that whatever national industrial policies are on the books, this has no effect on individual city capital market prices in the later 2000's. While disappointing to an identification strategy, the non-result hints that political forces drive price differentials, not policies announced by national planners.

We then looked at observable political events at the local level. A newly appointed local leader with good patronage ties may bring in favorable capital market conditions. We know when local prefecture leaders (party secretary) change office and thought that newly appointed ones in the late 2002 to end of 2004 time frame might be more favored leaders under certain conditions. There were also an unusual number of changes in early 2003: 57 in the first 4 months where usually that would be about 20. We do not know the affiliation of these party secretaries but we know the affiliation of the provincial governors who appointed them. We know if those governors are Princelings, Communist Youth League ("Beijing" branch), "Shanghai affiliated", no known affiliation, or ambiguous affiliation, from records and a data set assembled by Qinghua Zhang of Peking University. Being a new party secretary appointed by one of these types of provincial leaders actually gives us a first stage for predicting capital market prices for 06-07, as reported in column 1 of Table 8. Relative to no new appointment (about 50% of the cases), a new appointment that is the most politically connected in the party hierarchy, a "Princeling", brings a low price, while "fence-sitting" (ambiguous affiliation) brings the highest. This first stage gives a second stage, reported in column 2 of Table 8. The capital market price is significant and now even more negative with an elasticity of -0.12. If this is the true elasticity, it implies that a one standard deviation decrease in the price of capital would increase city growth by 4% relative to an average growth overall from 2000-2010 of 5%.

The first stage results are consistent with our idea that politics play a big role in capital market distortions. The problem is that politics may not only drive capital market prices but other forms of local favoritism. New dynamic local leaders appointed from late 2002 to 2004 with better connections to the national new leadership may have brought in other dimensions of favoritism. All this suggests that the use of political instruments (in the literature for all kinds of local conditions) are suspect and must be viewed with caution. At best the -0.12 estimate relative to -0.08 in Table 6 is correcting for measurement error; at worst it is biased estimate picking up other favoritism factors besides capital market favoritism. Then what we have is a reduced form estimate of the overall effect of bais, not just capital market bias.

How levels results differ

We now turn to more typical types of estimates in the favoritism literature, which uses a cross-section approach. In Table 9 we show results of estimating a levels equation as in equation 1.8, where the LHS variable is the log of prefecture population in 2010. Such a cross-section estimation may be worrisome for potentially larger bias than the growth estimation, because fixed city characteristics which affect size are differenced out in the latter. Nonetheless, this estimation sheds light on the long-term outcome of capital market favoritism on city sizes with implied full adjustment, while the growth estimation is only for 2000-2010. Column 1 of Table 9 show the results for the formulation corresponding to Table 6 column 3, and column 2 shows the second-stage IV result corresponding to Table 8 column 2. The set of control variables is kept identical. Essentially, we are looking at 2010 population conditioning on 1982 population, 82-90 population growth and other city characteristics as a way of controlling for unobserved city amenity differentials (Duranton and Turner, 2012). Now we see larger impacts of differences in capital costs on city size as might be expected. For the IV results, a 1% reduction in the price of capital reduces city size in 2010 by 0.19%, whereas the growth results suggest a 0.12% reduction in growth.

Spillovers and heterogeneity

What about potential spillovers and heterogeneity? For spillovers, neighbors' prices of capital may affect an own city in two ways. First, lower neighbor prices which spur their development may result in increased demand for own city products, in a market access context. On the other hand, lower neighbor prices of capital may make them better able to compete for local resources (e.g., regional labor) and hurt the own city. To the Table 6 column 3 base case, if we add in the average of all contiguous neighbors' prices controlling for own and neighbor characteristics (other dimensions on which neighbors compete), we get a small (absolute value less than 0.01) negative coefficient for this spatially weighted neighboring price of capital. In a column 5 or 6 formulation, the coefficient on contemporaneous (i.e., pooled 2006/07) prices is negative and significant[7]. All in all, the signing of the coefficient on contiguous neighbor's average capital price suggest that higher neighbor prices hurt the own city, presumably via the aforementioned channel of reducing their demand for own city products.

We examine heterogeneity in two dimensions. First, we asked if capital intensity as measured by capital share in value added or as the capital to labor ratio either increased from 2000 to 2007, or was higher in 2007 (as a level "long run" effect) for lower priced cities. The use of capital relative to labor is based on a clear price effect, and is thus the simplest measure. (Share measure responses depend on functional form in production.) In all of our tested formulations, an increase in the price of capital reduces capital intensity, but results differ in significance. In general, the results for capital as a share of value added are strongest; but, for the key IV results, we get strong significant effects for capital to labor ratios as well. For the growth in capital intensity formulation using Table 6 covariates, the OLS coefficient in the share version is significant at the 10% level, but insignificant for the capital to labor ratio. We then did an IV version where the 2006/07 price variable is instrumented for as in Table 8. There the coefficients on the levels of capital intensity as either the capital to labor ratio or share in value added are negative and significant with a good first stage. For example, for a coefficient of -0.552 (and standard error of 0.275), a one standard deviation decrease in the price variable leads to an increase in the capital to labor ratio of 0.187 from a mean of 5.17. The parallel IV results for the growth formulation have the correct signs but are statistically weaker. We conclude that indeed higher prices of capital reduce capital intensity, passing a basic test of what one might expect.

Additionally, we asked if cities with higher capital intensities have stronger price

[7] We have no way to do an IV version of this with any strength as in Table 8.

effects on size as might be expected from the model. We measure capital intensity variously as total industrial survey capital in 1998, 1999, or the average of both per industrial worker for corresponding years, per total employment in 2000, and as a share of value added. In OLS versions corresponding to Table 6, capital intensity itself generally has a positive and sometimes significant effect on growth, but the interaction with the price of capital generates a tiny positive and insignificant effect, where a negative effect is expected. We also tried an IV version as in Table 8 where we use 2007 capital intensity measures instrumented with historical intensity measures (with the price variable also instrumented for as in Table 8). There the interaction terms are negative as expected but insignificant; and the first stage regressions are very weak. Trying to capture an interaction term with shifting capital intensities over time seems to be more than we can do with our data.

Thinking about welfare cost implications

In this paper, what we have estimated is a micro econometric relationship which describes, from the equilibrium in China, how the growth or size of a single city would change as response to a change in the price of capital. To evaluate welfare impacts requires two considerations. For the single city, an increase in size in principle moves it further down its average real income curve as graphed against city size so the city is more over-sized and operating at a less efficient scale. If the real income curve is relatively flat this loss in efficiency might be quite small; but there is the factor of giant over-sized political cities with teeming slums and the work in Henderson (2003) showing considerable growth losses induced by excess primacy, with a more nuanced approach now available in Castells-Quintana (2015). Secodn, however, the welfare inference issue is much bigger. In a deeper analysis moving beyond a partial equilibrium look at one city, in a general equilibrium context such as in Hsieh and Klenow (2009), we would be trying to quantify the effects on real national income of misallocation of capital across cities on a national scale. But that involves a general equilibrium model where there are cities and efficient nominal wage differentials between bigger and smaller cities in equilibrium. And for China this would be in the context of limited labor mobility. While our ability to carry out such analysis has improved (Desmet, Nagy, and Rossi-Hansberg, 2015), we are still some distance from combined micro-econometric and structural estimation that would allow a proper evaluation in such complex contexts. But it would be fundamental to evaluating welfare costs.

1.6 Conclusion

This paper provides evidence on political favoritism in China's capital market and quantifies the differentials by firm, city, and region types. We also document the shifting climate in city and regional favoritism over time in light of national and local leadership changes. Lastly, we make a serious attempt to identify the causal relationship between capital market favoritism and city population growth in China. Our basic findings are as follows.

Firm type affects the cost of capital. Compared to private firms, wholly or majority state owned firms earn a much lower return on capital consistently across space. The inferred value of marginal product of capital is over 50% lower for state owned firms in 2007. This is an improvement over the 117% lower returns for state owned firms in 1998. By city and region type, the provincial level political cities and regions experience changing costs of capital related to changing national policies and leadership. In the early years under Jiang Zemin, Shanghai and the West were favored relative to Beijing and Tianjin. By 2006-2007 under Hu Jintao, this has reversed. Dispersion in capital market prices across cities drops to a low in 2002 and then escalates under Hu Jintao, representing a reintroduction of willingness to intervene in capital markets.

We then test the effects of a lower price of capital on the growth of cities after the national relaxation in migration restrictions in the period around 2000. Controlling for other factors likely to be correlated with the costs of capital which also affect city growth, we find strong and suggestive evidence that lower costs of capital result in larger increases in city size. This finding is further supported and its magnitude strengthened in the better defended causal context with local political faction affiliation as the instrumental variable. Additionally, we also examined the spatial dimension of this relationship between capital prices and city sizes by considering neighboring cities.

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Figures

Figure 1.1: 2007 Differential Returns. Ownership Comparison at National Level State owned or majority controlled firms versus wholly private firms
Distribution of after tax value added divided by net asset value

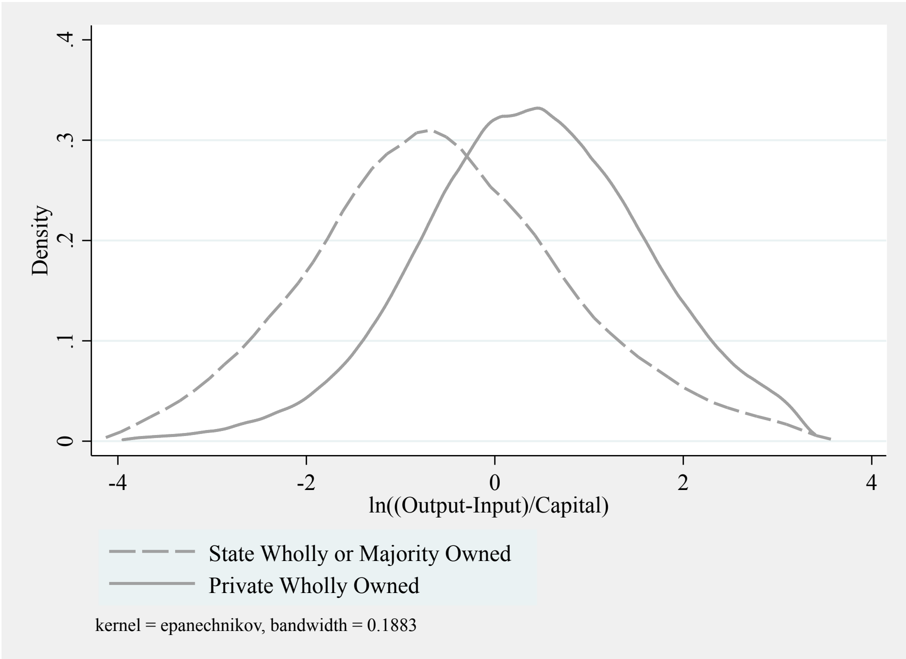


Figure 1.2: 2007 Differential Returns. All Private Firms Nationally in Non-Provincial Level Cities versus Private and also All Firms in East Coast Provincial Level Cities
Distribution of after tax value added divided by net asset value

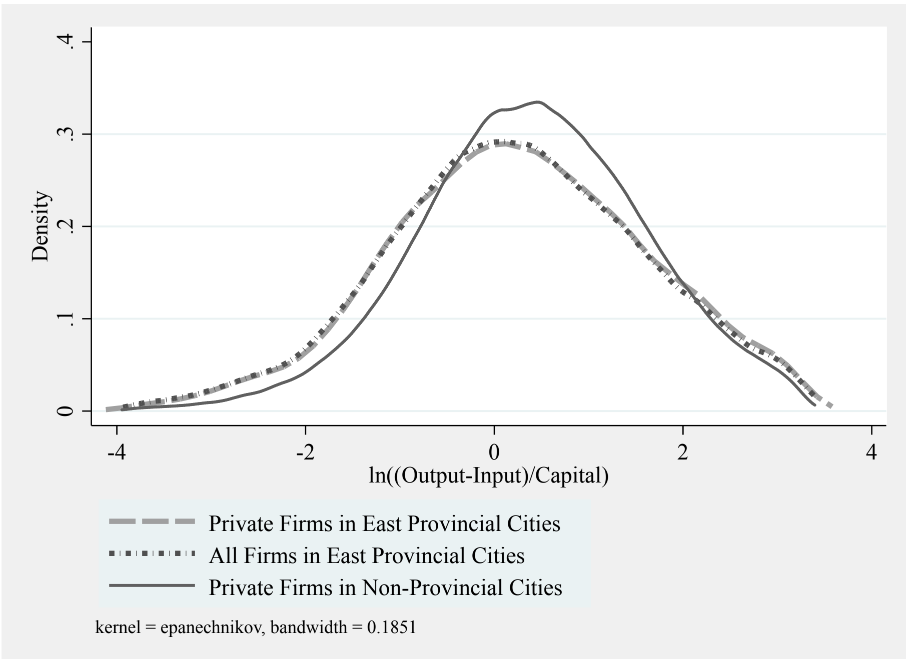


Figure 1.3: Differential Return. Wholly Private Firms by Major City. Prefecture Level East Coast Cities versus South Coast Free Wheeling Cities
Distribution of after tax value added divided by net asset value

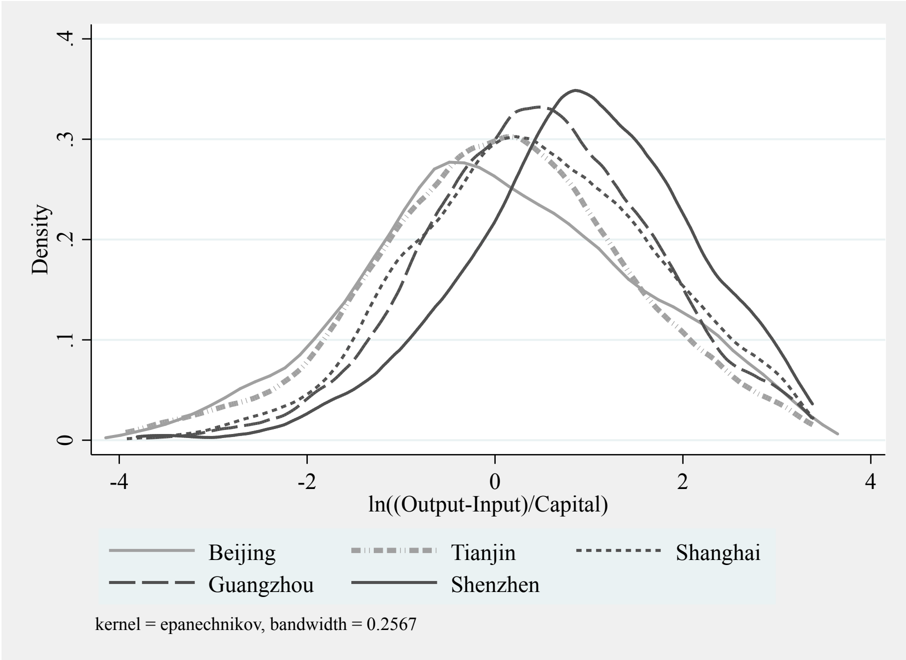


Figure 1.4: Overtime changes in place based bias coefficients.

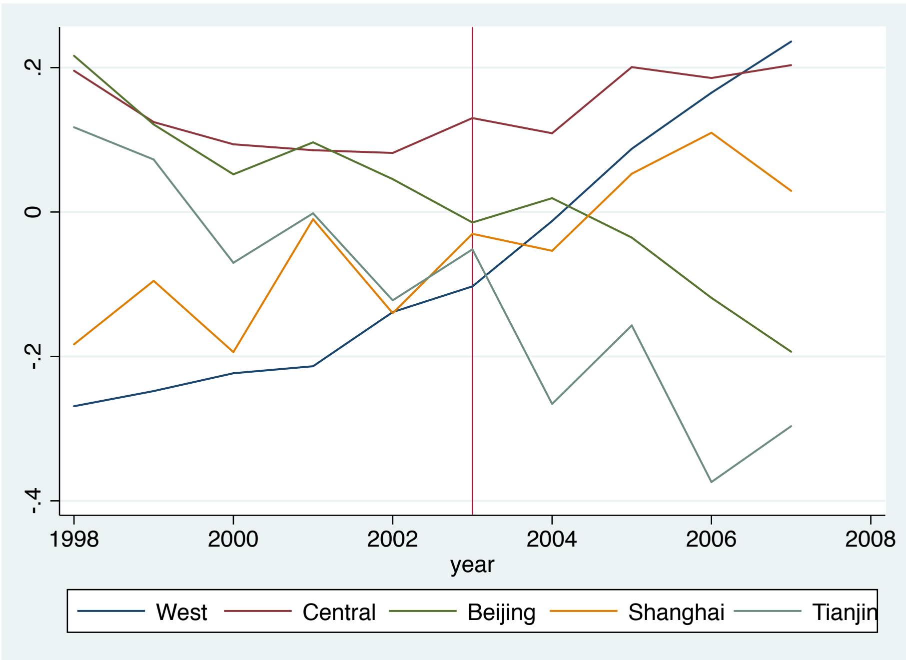
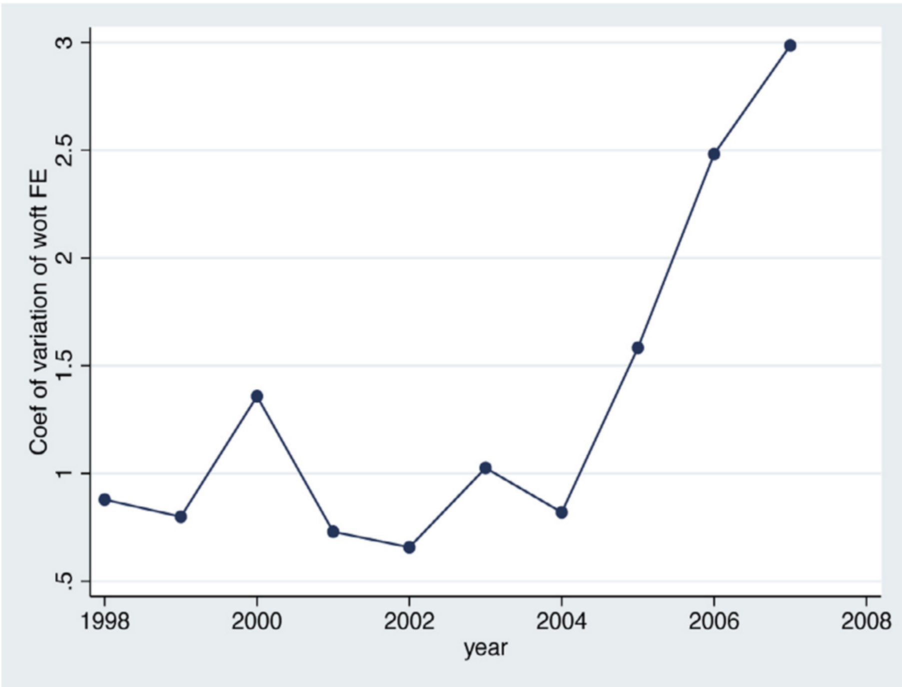


Figure 1.5: Dispersion of city fixed effects over time (no firm type controls)



Tables

Table 1.1: Composition of ownership (by paid-in-capital) 2007 versus 1998

Estimating sample:	Share of firm count		Share of value added	
	1998	2007	1998	2007
Majority collective	9.14%	0.94%	6.74%	1.22%
Wholly collective	21.30%	3.51%	11.00%	2.77%
Majority foreign	4.30%	2.45%	6.95%	4.81%
Wholly foreign	6.29%	12.42%	7.26%	16.12%
Majority private	9.13%	5.36%	11.09%	10.78%
Wholly private	18.98%	72.06%	16.45%	48.46%
Majority state	5.43%	0.71%	11.15%	4.23%
Wholly state	25.44%	2.55%	29.35%	11.62%
Any state ownership	34.46%	4.15%	47.33%	19.90%

Table 1.2: Base results for 2007

	Base (1)	Full covariates (2)	Gross book k (3)	LAD (4)	Add firm age (5)	Add TFP covar. (6)
West	0.0377	0.2361***	0.1803**	0.2630***	0.2413***	0.2463***
	(0.0583)	(0.0707)	(0.0781)	(0.0131)	(0.0703)	(0.0695)
Central	0.0672	0.2034***	0.2501***	0.2361***	0.2039***	0.1978***
	(0.0506)	(0.0573)	(0.0606)	(0.0096)	(0.0573)	(0.0576)
Provincial capital	-0.0074	0.0216	-0.0455	0.0272***	0.024	0.0168
	(0.0812)	(0.0883)	(0.0886)	(0.0085)	(0.0886)	(0.0852)
Beijing	-0.2405***	-0.1935***	-0.2961***	-0.1850***	-0.1910***	-0.1691**
	(0.0390)	(0.0711)	(0.0707)	(0.0222)	(0.0712)	(0.0748)
Shanghai	0.0432	0.0293	-0.0579	0.0766***	0.0301	0.0469
	(0.0396)	(0.1310)	(0.1322)	(0.0211)	(0.1311)	(0.1355)
Chongqing	0.0119	0.0067	-0.0249	0.0189	0.0074	0.0623
	(0.0506)	(0.0942)	(0.0989)	(0.0316)	(0.0939)	(0.1002)
Tianjin	-0.3015***	-0.2966***	-0.3309***	-0.2854***	-0.2961***	-0.2765***
	(0.0379)	(0.0784)	(0.0772)	(0.0229)	(0.0786)	(0.0820)
Wholly state	-0.4614***	-0.4491***	-0.6271***	-0.4253***	-0.3793***	-0.4486***
	(0.0305)	(0.0293)	(0.0302)	(0.0182)	(0.0332)	(0.0292)
Majority state	-0.4978***	-0.5004***	-0.6839***	-0.5094***	-0.4672***	-0.4988***
	(0.0491)	(0.0470)	(0.0441)	(0.0313)	(0.0486)	(0.0469)
Wholly collective	0.1160***	0.1161***	-0.0733**	0.1487***	0.1459***	0.1183***
	(0.0270)	(0.0252)	(0.0294)	(0.0145)	(0.0247)	(0.0251)
Majority collective	-0.1145***	-0.1302***	-0.3167***	-0.1291***	-0.1044***	-0.1269***
	(0.0390)	(0.0331)	(0.0356)	(0.0272)	(0.0323)	(0.0336)
Wholly foreign	-0.2385***	-0.2777***	-0.3833***	-0.2963***	-0.2785***	-0.2762***
	(0.0334)	(0.0374)	(0.0418)	(0.0086)	(0.0372)	(0.0375)
Majority foreign	-0.2014***	-0.2307***	-0.3917***	-0.2514***	-0.2272***	-0.2297***
	(0.0263)	(0.0262)	(0.0335)	(0.0171)	(0.0263)	(0.0265)
Majority private	-0.2138***	-0.2268***	-0.3144***	-0.2268***	-0.2182***	-0.2276***
	(0.0209)	(0.0203)	(0.0203)	(0.0118)	(0.0203)	(0.0203)
Firm age					-0.0041***	
					(0.0009)	
1995 ind. TFP FE					0.7069	
						(0.8064)
1995 city TFP FE						0.1408*
						(0.0823)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Elasticity controls	No	Yes	Yes	Yes	Yes	Yes
N	312351	312351	312142	312351	312351	312351
R-squared	0.0767	0.0836	0.1028		0.0844	0.0843

This table shows various specifications to look at the cross-sectional average return differentials by firm type and locational hierarchy in 2007. Column 1 accounts for industry fixed effects, while columns 2-6 include city-industry fixed effects reflecting local demand elasticities. Column 2 is our preferred specification.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.3: Variants of the basic specification and individual city fixed effects

	Main spec	Private firms	Without firm type	ind city FE	Without firm types	private firms
	(1)	(2)	(3)	(4)	(5)	(6)
West	0.2361***	0.2778***	0.2117***			
	(0.0707)	(0.0769)	(0.0722)			
Central	0.2034***	0.2169***	0.1947***			
	(0.0573)	(0.0623)	(0.0584)			
Provincial capital	0.0216	0.0209	0.0053			
	(0.0883)	(0.1013)	(0.0847)			
Beijing	-0.1935***	-0.2567***	-0.2183***	-0.6341***	-0.5185***	-0.7796***
	(0.0711)	(0.0758)	(0.0748)	(0.0399)	(0.0410)	(0.0447)
Shanghai	0.0293	0.0289	-0.0135	-0.4698***	-0.4305***	-0.5872***
	(0.1310)	(0.1425)	(0.1350)	(0.0492)	(0.0507)	(0.0553)
Chongqing	0.0067	-0.0155	0.0044	-0.2701***	-0.1177*	-0.3201***
	(0.0942)	(0.0985)	(0.0974)	(0.0682)	(0.0677)	(0.0744)
Tianjin	-0.2966***	-0.3490***	-0.3457***	-0.7185***	-0.6543***	-0.8629***
	(0.0784)	(0.0848)	(0.0820)	(0.0382)	(0.0397)	(0.0421)
Wholly state	-0.4491***			-0.4222***		
	(0.0293)			(0.0261)		
Majority state	-0.5004***			-0.4735***		
	(0.0470)			(0.0452)		
Wholly collective	0.1161***			0.1069***		
	(0.0252)			(0.0173)		
Majority collective	-0.1302***			-0.1113***		
	(0.0331)			(0.0286)		
Wholly foreign	-0.2777***			-0.2973***		
	(0.0374)			(0.0300)		
Majority foreign	-0.2307***			-0.2196***		
	(0.0262)			(0.0225)		
Majority private	-0.2268***			-0.1795***		
	(0.0203)			(0.0182)		
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Elasticity controls	Yes	Yes	Yes	Yes	Yes	Yes
City fixed effects	No	No	No	Yes	Yes	Yes
N	312351	225086	312351	312351	312351	225086
R-squared	0.0836	0.062	0.0738	0.1308	0.122	0.1202

This table presents a variant of specifications to establish how we measure bias in the capital market. Column 1 is a repetition of our preferred specification (column 2 of Table 2) for comparison. Column 2 constrains this specification to just private firms. Column 3 repeats column 1 without firm type controls. Column 4 uses individual city instead of political hierarchy fixed effects. Columns 5 and 6 are parallels of columns 3 and 2 for column 4.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.4: All firm sample for all years

	1998 (1)	1999 (2)	2000 (3)	2001 (4)	2002 (5)	2003 (6)	2004 (7)	2005 (8)	2006 (9)	2007 (10)
west	-0.2690***	-0.2480***	-0.2233***	-0.2136***	-0.1386**	-0.1030*	-0.0125	0.0876	0.1652**	0.2361***
	(0.0541)	(0.0520)	(0.0511)	(0.0528)	(0.0629)	(0.0557)	(0.0576)	(0.0656)	(0.0686)	(0.0707)
middle	0.1957***	0.1246**	0.0938	0.0856	0.0818	0.1301**	0.1091**	0.2007***	0.1856***	0.2034***
	(0.0604)	(0.0581)	(0.0578)	(0.0580)	(0.0548)	(0.0556)	(0.0453)	(0.0502)	(0.0537)	(0.0573)
Provincial capital	0.0131	0.0512	0.0244	0.051	0.06	0.0633	0.0426	0.033	0.0334	0.0216
	(0.0741)	(0.0629)	(0.0586)	(0.0646)	(0.0636)	(0.0670)	(0.0677)	(0.0717)	(0.0843)	(0.0883)
Beijing	0.2164***	0.1213***	0.0522	0.0965**	0.0455	-0.0145	0.0192	-0.0354	-0.1189*	-0.1935***
	(0.0399)	(0.0397)	(0.0417)	(0.0457)	(0.0518)	(0.0483)	(0.0509)	(0.0662)	(0.0653)	(0.0711)
Shanghai	-0.1833***	-0.0953	-0.1940***	-0.01	-0.1401*	-0.0302	-0.0537	0.053	0.1098	0.0293
	(0.0539)	(0.0582)	(0.0627)	(0.0670)	(0.0812)	(0.0827)	(0.0908)	(0.1210)	(0.1211)	(0.1310)
Chongqing	0.0391	0.0607	-0.0914*	0.0954	-0.0053	0.1324**	0.0622	0.0014	-0.024	0.0067
	(0.0610)	(0.0519)	(0.0552)	(0.0599)	(0.0702)	(0.0673)	(0.0732)	(0.0907)	(0.0946)	(0.0942)
Tianjin	0.1174***	0.0727*	-0.0703*	-0.0018	-0.1223**	-0.0517	-0.2658***	-0.1571**	-0.3740***	-0.2966***
	(0.0384)	(0.0392)	(0.0408)	(0.0441)	(0.0506)	(0.0489)	(0.0557)	(0.0710)	(0.0714)	(0.0784)
Wholly state	-1.0980***	-1.0703***	-1.0617***	-1.0326***	-0.9285***	-0.8263***	-0.7430***	-0.6177***	-0.5993***	-0.4491***
	(0.0547)	(0.0492)	(0.0416)	(0.0410)	(0.0366)	(0.0334)	(0.0338)	(0.0356)	(0.0362)	(0.0293)
Majority state	-0.8786***	-0.8548***	-0.8357***	-0.8494***	-0.7936***	-0.7245***	-0.5960***	-0.5665***	-0.5568***	-0.5004***
	(0.0486)	(0.0439)	(0.0426)	(0.0430)	(0.0431)	(0.0364)	(0.0373)	(0.0380)	(0.0576)	(0.0470)
Wholly collective	0.1371***	0.1153***	0.1026***	0.0894***	0.0768***	0.0838***	0.1205***	0.1280***	0.1028***	0.1161***
	(0.0362)	(0.0315)	(0.0303)	(0.0296)	(0.0265)	(0.0275)	(0.0262)	(0.0249)	(0.0277)	(0.0252)
Majority collecitve	-0.0585*	-0.0668**	-0.0754**	-0.1009***	-0.0720**	-0.0729**	-0.1032***	-0.0604*	-0.0768**	-0.1302***
	(0.0333)	(0.0321)	(0.0318)	(0.0306)	(0.0298)	(0.0316)	(0.0298)	(0.0316)	(0.0316)	(0.0331)
Wholly foreign	-0.3475***	-0.3300***	-0.3171***	-0.3736***	-0.2936***	-0.3183***	-0.3321***	-0.2927***	-0.2739***	-0.2777***
	(0.0535)	(0.0479)	(0.0497)	(0.0448)	(0.0470)	(0.0459)	(0.0510)	(0.0380)	(0.0433)	(0.0374)
Majority foreign	-0.5005***	-0.4344***	-0.4036***	-0.4270***	-0.3573***	-0.3138***	-0.2849***	-0.2718***	-0.2367***	-0.2307***
	(0.0416)	(0.0393)	(0.0346)	(0.0329)	(0.0306)	(0.0325)	(0.0262)	(0.0267)	(0.0281)	(0.0262)
Majority private	-0.1426***	-0.1635***	-0.1864***	-0.2004***	-0.1760***	-0.1816***	-0.1831***	-0.1850***	-0.2048***	-0.2268***
	(0.0253)	(0.0253)	(0.0182)	(0.0195)	(0.0190)	(0.0184)	(0.0172)	(0.0178)	(0.0186)	(0.0203)
N	134717	134443	137569	146658	156751	175404	247443	246302	276888	312351
Percentage bottom trim	6%	5%	4%	3%	4%	3%	3%	3%	2%	2%
R-squared	0.1967	0.1933	0.1875	0.1809	0.1621	0.148	0.1168	0.1054	0.1017	0.0836

This table repeats our preferred specification (column 2 of Table 2) for the full time span of our data, i.e., 1998-2007. This table shows us how bias in the capital markets varies over the 10-year span, to which we relate the changes in national leadership.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.5: Year pairwise correlations in annual city fixed effects (Table 4 specification)

	1998	1999	0	2001	2002	2003	2004	2005	2006	2007
1998	1									
1999	0.927	1								
2000	0.884	0.947	1							
2001	0.859	0.911	0.946	1						
2002	0.814	0.862	0.902	0.921	1					
2003	0.779	0.804	0.851	0.895	0.896	1				
2004	0.648	0.665	0.699	0.746	0.795	0.854	1			
2005	0.462	0.475	0.498	0.539	0.614	0.714	0.843	1		
2006	0.336	0.352	0.393	0.442	0.507	0.635	0.747	0.901	1	
2007	0.23	0.223	0.253	0.314	0.378	0.522	0.652	0.83	0.924	1

Table 1.6: How does varying city-level capital market favoritism relate to city growth, OLS

	(1)	(2)	(3)	(4)	(5)	(6)
City fixed effects without firm type controls, 1998-1999	0.0263	-0.0289	-0.0688***			-0.0468**
	(0.0202)	(0.0218)	(0.0248)			(0.0219)
City fixed effects with firm type controls, 1998-1999				-0.0823***		
				(0.0259)		
City fixed effects without firm type controls, 2006-2007					-0.0801***	-0.0657***
					(0.0172)	(0.0174)
1995 share of emp by firms whose Y-M-labor costs <0		-0.1231**	-0.1639***	-0.1454***	-0.0818*	-0.1386**
		(0.0555)	(0.0582)	(0.0485)	(0.0441)	(0.0577)
1995 share of emp by LTD firms		0.4575***	0.3223***	0.3132***	0.3283***	0.3339***
		(0.0770)	(0.0896)	(0.0874)	(0.0776)	(0.0767)
Log pref Pop in 1982			-0.0262**	-0.0270**	-0.0238**	-0.0213*
			(0.0117)	(0.0114)	(0.0115)	(0.0119)
Population change from 1982 to 1990			-0.049	-0.0415	-0.0493	-0.0375
			(0.2074)	(0.2045)	(0.2274)	(0.3048)
ln(Distance to coast)			-0.0248***	-0.0241***	-0.0149***	-0.0215***
			(0.0056)	(0.0055)	(0.0039)	(0.0054)
1999 all road rays			0.0107***	0.0114***	0.0101***	0.0097**
			(0.0041)	(0.0042)	(0.0038)	(0.0041)
Constant	0.0652***	0.0123	0.5124***	0.5088***	0.4325**	0.4195**
	(0.0149)	(0.0188)	(0.1876)	(0.1888)	(0.1840)	(0.2057)
N	284	283	283	283	283	283
R-squared	0.0109	0.1912	0.2824	0.286	0.2999	0.3151

Bootstrapped standard errors in parentheses. This table presents the OLS regression results on relating capital market favoritism to city population growth. Various city-level measures of capital market bias are tested with or without variables measuring other city characteristics that may simultaneously affect growth.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.7: Capital market favoritism (06-07 city fixed effects) on city growth, Historical employment composition of favored industries as IV, first-stage.

	(1)	(2)
Index of favored manufacturing industries in 2005 with 1995 weights	0.0051	
	(0.015)	
Index of favored manufacturing industries in 2005 with 1998 weights		0.0098
		(0.012)
Other controls: same as Column 3 Table 6	Yes	Yes
N	283	283
R-squared	0.022	0.022

* p < 0.10, ** p < 0.05, *** p < 0.01.

Table 1.8: Capital market favoritism (06-07 city fixed effects) on city growth, Political faction as IV

	First stage (1)	Second stage (2)
Political faction: Chinese Youth League	0.1198	
	(0.0793)	
Political faction: ambiguous	0.3230***	
	(0.0948)	
Political faction: none	0.0232	
	(0.0544)	
Political faction: prince	-0.2315***	
	(0.0566)	
Political faction: Shanghai	0.0855	
	(0.0566)	
1995 share of emp by firms whose Y-M-labor costs < 0	-0.1093	-0.0830*
	(0.1806)	(0.0445)
1995 share of emp by LTD firms	0.1382	0.3388***
	(0.2211)	(0.0821)
ln(prefecture population in 1990)	0.2689	-0.0367
	(0.1664)	(0.1619)
ln(prefecture population in 1982)	-0.18	0.0174
	(0.1567)	(0.1561)
ln(Distance to coast)	-0.0098	-0.0148***
	(0.0145)	(0.0038)
1999 all road rays	-0.0219*	0.0092**
	(0.0118)	(0.0042)
City fixed effects 06/07, no firm type controls		-0.1237**
		(0.0535)
Constant	-1.3730***	0.3593
	(0.5101)	(0.2242)
N	283	283
R-squared	0.1363	0.2838
KP F stat		8.8139

* p < 0.10, ** p < 0.05, *** p < 0.01.

Chapter 1 Appendix

Data definitions

China Enterprise Database 1998-2009 covers all state-owned enterprises in China and all non-state enterprises with annual sales revenue more than 5 million Yuan. Most of the variables come from three basic accounting tables for Chinese enterprises, namely the Balance Sheet, the Income Statement and the Cash Flow Statement, although many of them are not available in all years. The rest variables include total number of employees, firm paid-in capital, firm registered ownership and opening years etc.

The main variables we use in this paper include output in current price, total input, net fixed asset, gross fixed asset, paid in capital, wage, welfare payable and industry code at four-digit level[8]. Value added net of tax is calculated as output in current price minus input. Input includes both direct inputs that turn into products and indirect input consumed by the administrative and sales branches of a firm. Input is not available in 2008 and 2009; hence our investigation on capital efficiency can only go as far as 2007. Welfare payable includes employee medical expenses and the cost of providing other amenities. Welfare payable is one part of fringe benefit as the later also includes employee housing fund and social insurance. However, welfare payable is the only variable available in all years between 1998 and 2007. Thus we use it as a proxy to fringe benefit. Paid in capital variables show the amount of equity owned by the state, the collective, legal persons, individuals, Taiwan Hong Kong and Macau investors and foreign investors. Unlike firm registered ownership, paid in capital variables are updated every year.

Control variables include city types, region types, count of firms in own industry own prefecture in 2004 (for 2007 regressions) and city characteristics in 2000. Provinces in the east include Liaoning, Hebei, Shandong, Jiangsu, Zhejiang, Fujian, Beijing, Tianjin, Shanghai, Guangdong and Hainan. Provinces in the west include Xinjiang, Gansu, Shaanxi, Ningxia, Sichuan, Chongqing, Guizhou, Yunnan, Guangxi, Xizang and Qinghai. We later confine our analysis to Han Chinese area. Provincial level cities are the four cities that enjoy the same level of administrative ranking as provinces, namely Beijing, Tianjin, Shanghai and Chongqing. City (prefecture) characteristics in 2000 include total population, the share of agriculture in total employment, the share of manufacturing in total employment, the share of migrants in total population and the share of high school and above graduates in the labor force (age group of 19 to 55), from the 2000 Population Census.

We drop irregular observations that have negative values in output, input, wage, net fixed asset or whose current year depreciation is smaller than accumulated depreciation. We also drop firms that don't have a majority ownership. Furthermore, for all 2007 regressions, we trim the bottom and top 2% of *(output-input)/net fixed asset*

[8] In the current work we only use industrial classification at two-digit level. Industrial code in all years has been converted to the standard 2002 Industrial Classification for National Economic Activities (GB/T4754-2002).

and *(output-input-wage-fringe benefit)/net fixed asset* to get rid of extreme values, as explained in the text. Table (A-1) shows the statistics of the main variables.

Table A-1. Descriptive Statistics (2007 industry variables)

Variable	Mean	SD	Min	Max
Value added	29872.8057	374969.044	2	140503104
Capital (net of tax)	41205.2114	719104.01	3	156000000
Capital (gross)	55820.1147	1032540.9	0	221000000
Wholly state share	0.0255	0.1576	0	1
Wholly collective share	0.0351	0.184	0	1
Wholly private share	0.7206	0.4487	0	1
Wholly foreign share	0.1242	0.3298	0	1
Majority state share	0.0071	0.0841	0	1
Majority collective share	0.0094	0.0963	0	1
Majority private share	0.0536	0.2253	0	1
Majority foreign share	0.0245	0.1547	0	1
count: coal mining	3.3063	32.5454	0	784
count: petro and natural gas mining	0.0011	0.1118	0	50
count: black metal mining	0.2756	4.1039	0	98
count: colored metal mining	0.2867	5.2009	0	200
count: non-metal mining	0.5962	7.333	0	215
count: mining supportive activities	0.0001	0.0239	0	7
count: agro-processing	8.3754	42.8876	0	674
count: food manufacturing	2.0749	21.3042	0	477
count: beverage manufacturing	1.0084	14.266	0	803
count: tobacco manufacturing	0.0012	0.0833	0	14
count: textile manufacturing	32.8311	139.0384	0	1296
count: apparel manufacturing	12.0786	78.5493	0	1104
count: leather/fur manufacturing	3.6454	33.1225	0	535
count: wood/bamboo manufacturing	2.5786	24.3463	0	477
count: furniture manufacturing	0.7616	9.0445	0	225
count: paper manufacturing	2.3982	18.6764	0	333
count: printing	2.6605	31.0576	0	655
count: stationary/sports/entertainment	1.0272	13.4918	0	381
count: petro product	0.1587	2.7302	0	96
count: chemical product	14.3677	75.0318	0	872
count: medicine	0.8856	10.3418	0	287
count: chemical fiber	0.1145	2.0243	0	60
count: rubber product	0.6343	8.4559	0	216
count: plastic product	11.0065	68.3546	0	797
count: nonmetal mineral product	21.7019	93.9588	0	1365
count: black metal refine and process	1.2075	12.1957	0	339
count: color metal refine and process	0.7565	7.887	0	197
count: metal product	16.8047	106.1789	0	1336
count: equipment - general purpose	31.1282	148.4186	0	1429
count: equipment - special purpose	8.1898	56.7691	0	794
count: equipment - transport	10.887	76.2625	0	880
count: electrical machinery	17.6089	106.7582	0	1376
count: telecom/computer/electronics	6.8543	48.7862	0	654
count: instrument and office machinery	1.8282	22.9009	0	557
count: crafts and miscellaneous	2.6663	23.1239	0	422
count: electric/heat production and supply	0.9776	13.0193	0	502
count: gas production and supply	0.0048	0.159	0	9
count: water production and supply	0.1324	2.5741	0	139
ln(Distance to coast)	4.1868	1.8535	-5.3784	7.3961
firm age	8.2651	9.4892	0	1807
population growth, 2000-2010	0.0505	0.1139	-0.5921	0.5423
1995 share of emp by firms whose Y-M-labor costs <0	0.2342	0.1192	0.036	0.7104

Continued on next page

Table A-1. Continued from previous page

Variable	Mean	SD	Min	Max
1995 share of emp by LTD firms	0.1117	0.1138	0.0011	0.691
ln(population in 1990)	14.9283	0.6757	11.6081	17.1782
ln(population in 1982)	14.7974	0.7074	11.3022	17.1103
1999 all road rays	2.8561	1.6962	0	8

Table A2: Baseline with majority ownership split out by state participation or not

Majority collective with some state ownership	-0.3620***
	(0.0837)
Majority collective with no state ownership	-0.1120***
	(0.0347)
Majority foreign with some state ownership	-0.4440***
	(0.0639)
Majority foreign with no state ownership	-0.2059***
	(0.0249)
Majority private with some state ownership	-0.4331***
	(0.0344)
Majority private with no state ownership	-0.2018***
	(0.0203)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The evolution of domestic capital markets and the banking sector in China

The banking sector in China controls 70% of assets in the entire financial system, and the four large state-owned banks are the central pillars of the banking sector. Together they hold 45-55% of banking sector assets in China (Wang, 2010; China Bank Regulatory Commission, 2012). In order to understand capital market issues, we discuss the banking sector's evolution and current practices that lead to differential treatment toward firms of different ownership types and firms in different cities. We will also briefly review the limited alternatives for private enterprises in seeking credit outside the state controlled banking sector.

Banking sector reform

In 1978 as China embarked on its economic transformation, the People's Bank of China (PBC) and rural credit cooperatives were the only financial institutions in China (Chen, 2010). In urban areas, enterprises were owned by government or collective bodies and investment decisions were made by the state. The PBC had no independent lending policies and primarily functioned to collect deposits (Chen, 2010). The first banking reform was to remove deposit-taking functions from the PBC and establish four state-owned banks[9] to take over specific lines of business, and then later to designate the PBC as the central bank, supervising all other state-owned banks. The four state-owned banks were established as policy banks, so the lion's share of lending went to finance projects that were sanctioned by planning committees and regional governments (Green, 2010). In the mid-1990s, three policy banks[10] were established to take over policy loans from the big four banks and to free them for more market-oriented businesses. However in this time period, the four state-owned banks did not progress to develop independent risk assessment mechanisms, since they were merely generally underwriters of projects that had been pre-determined by the state. By 1999, the four state-owned banks had inherited and accumulated large proportions non-performing loans (NPL), which crippled their ability to develop market-oriented commercial business. At this point in preparation for later public listing and strategic investment and minority shareholding by foreign banks to add technical know-how and modern corporate governance (Green, 2010), the state removed RMB1.3939 trillion worth of NPLs from the balance sheets of the four banks, handing the NPLs over to specially established Asset Management Companies[11] (AMCs). Since then, the risk situation of the four banks has improved significantly (Wang, 2010), but political influence remains strong with two forms of discrimination of interest here.

Sources of discrimination by firm ownership type

Despite the on-going reform effort, private enterprises still face strongly discriminatory access to the formal financial system. According to an All China Federation of Industry and Commerce report (2010), it is very hard for private enterprises to get

[9] The Agriculture Bank of China took over lending business for the agricultural sector; the China Construction Bank for infrastructure finance; the Bank of China for foreign exchange management and the Industrial and Commercial Bank of China for commercial banking

[10] These were the China Development Bank (CDB), the Agricultural Development Bank (ADB) and the Export-Import Bank of China (China Exim Bank))

[11] The Asset Management Companies are Huarong, Changcheng, Dongfang and Xinda. They were set up specifically to attend to the task of disposing of the NPLs of the state-owned banks (Wang, 2010).

mid-term or long-term loans. Wei (2012) estimates that only 20% of private firms in Wenzhou (one of the centers of informal financing) can get loans from the formal system, with the rest relying solely on underground financing. And earlier we noted the disproportionate share of industrial enterprise debt held by state enterprises even in 2011. Why is there discrimination by firm type?

Howson (2010) notes that, despite years of reform of state-owned banks, the Committee of the Chinese Communist Party retains the power to appoint the boards of directors and senior management of banks. The state's interest is not communicated through shareholder's meetings but via the firm-level Communist Party Committee. The Party Committee is not asking banks to act in the interest of shareholders, but in accordance with "stability" , "lawfulness" , and national "macroeconomic measures" . Individuals appointed to bank senior management posts are personnel who have standing in the Communist Party hierarchy (Howson, 2010) and move between government and state bank corporate functions. As such, it is difficult for state owned banks to operate independently while facing pressure from different levels of government. Private enterprises have little leverage in this power politics. It creates an environment where private sector financial needs will be sidelined when state-owned enterprises and local governments are looking for money.

Feyzioglu, Porter and Takats (2009) further argue that China's existing interest rate control structure with a large gap between the ceiling on the deposit rate and the floor on the bank lending rate[12] reduces the incentive for China's state-owned banks to improve efficiency and risk assessment. In exercising credit control and in deciding which firms get how much credit, since politically and well-connected enterprises have better access and connections to bank officers and the bottom line is not profit maximization, credit control creates an environment where loan officers are more likely to channel funds to politically favored undertakings (Feyzioglu, Porter and Takats, 2009). Moreover the whole structure tends to breed corruption in the banking sector (Nan and Meng, 2009).[13]

[12] As for June 2012, the Yearly Current Deposit Baseline Rate was 0.35% , while the Yearly 6-month Loan Baseline Rate was 5.6% (the People's Bank of China, 2012).

[13] All this is over and above the incentive to focus on bigger loans to spread the fixed costs in making a loan. The costs include information costs and supervision costs. Information costs include resources invested on researching a firm's creditability and the costs of Non-Performing Loans due to insufficient information before lending. Supervision costs include the costs of supervising both lending enterprises and bank loan officers (Yin, Weng and Liang, 2008).

Finally, the standard for evaluating credit ratings is determined by the headquarters in Beijing of the banks in China; each bank imposes a unified standard on all branches across the country. Enterprises that do not reach a certain credit rating standard do not qualify for loans (Chen, 2010). However, many standards were set up according to the circumstances of the largest firms in major cities. Smaller enterprises often lack the resources to meet the formal standards for preparation of accounting and auditing documents, even though they may provide higher growth potential than large state-owned firms.

Discrimination over space

The first aspect of discrimination across space concerns the urban versus rural sector. Chen (2010) argues that the commercialization of state-owned banks led to the fall of rural financing for rural, town and village enterprises (TVE's). The "Law on Commercial banks of the People's Republic of China" passed in 1995 set up stringent requirements on collaterals and guarantees. State-owned commercial banks can extend banking credit only on the basis of clearly defined assets (Chen, 2010), usually land and buildings (Cousin 2006)[14]. The Collateral Law forbids rural land from being mortgaged, so rural enterprises and households cannot collateralize their most important asset to get credit from the banking sector (Chen, 2010). Beyond the evolving TVE sector, more generally, county-level branches of state-owned commercial banks do not have the power to issue loans (Chen, 2010). Since state enterprises disproportionally concentrate in big cities, private enterprises in smaller cities and rural enterprises suffer relative credit shortage.

In the paper we focus initially on discrimination across cities of different political significance, but then generalize to all cities. We already observed the imbalance in bank loans going to entities in provincial capitals and provincial level cities, relative to their contribution to GDP. Liu (2007) notes that after the establishment of China's commercial banking system, bank lending concentrated not just on China's state-owned enterprises, but also major cities. One issue is that commercial banks in China have cautiously retrenched credit-extending authority from their lo-

[14] Movable assets back only 4% of commercial loans; inventory and receivables cannot be collateralized under Chinese Security Law (WB-PBOC Report, 2006).

cal branches (Liu, 2007). Bank branches below provincial level are limited in their ability to extend credit to new clients and new investment projects. In general one would expect that firms in provincial level cities and provincial capitals have a closer relationship to provincial level branches where most of the credit extending power rests, than their counterparts in cities lower in the political hierarchy. Later we will argue that issues of favoritism are more widespread and may for example be related to the political influence and connections of local party secretaries and provincial leader attempting to garner credit for enterprises in their cities, or to national programs aimed at expansion of particular industries (through better access to credit), in which some cities are more specialized.

Alternatives for private firms

The private sector in China depends on informal financing on different degrees in different places to meet their investment needs (Tsai 2002, Allen, Qian and Qian 2005, Linton 2006). Farrell et al. (2006) notes that alternative financial sources could represent up to a quarter of total bank deposits in China; and Tsai (2002) claims that at least one quarter of all financial transactions are done through the informal system. Unlike formal financial intermediaries, informal lenders require no or very lenient collateral, charge very high interest rate, and reply on alternative enforcement measures that usually involve reputation or coercion (Allen, Qian and Qian, 2008). Ayyagari, Demirguc-Kunt & Maksimovic (2010) argue that Chinese private enterprises which have access to bank loans grow faster than similar firms without bank financing. They argue that the monitoring and enforcement mechanisms from the informal sector are ill equipped to scale up and serve the higher end of the market. Hence informal financing is not a sufficient substitute for formal financing, and it is increasingly the case as the size of Chinese economy grows. Firms, especially private firms, face sub-optimal growth potential when they are denied access to the formal financial system. To the extent this lack of credit varies across cities that affects city growth potential.

In principle, private firms could turn to equity markets. However in China, the banking sector dwarfs its equity market and bond market, in terms of both market capitalization and total value traded (Ayyagari, Demirguc-Kunt & Maksimovic, 2010; Naughton, 2007). The equity market is a vehicle for state-owned enterprise semi-privatization rather than a level playing field for all firms to raise capital. Furthermore, the process of capital market listing is largely controlled by the government (Wang, Xu and Zhu, 2004). Durnev et al. (2004) argues that China has one of the worst performing equity markets in the world. Highly synchronous returns in the market are the consequences of weak property rights, corporate opacity and rent seeking. China's corporate bond market is undeveloped too, crippled by excessive government regulation, the lack of institutional investors and credit rating agencies to set the price accurately (Ayyagari, Demirguc-Kunt & Maksimovic, 2010).

Chapter 2

Early Chinese development zones: first-mover advantage and persistency

2.1 Introduction

Establishing development zones[1] is one of the most prominent examples of place-based policies and certainly the most popular tool among policy makers across the world. Such policy instrument aims at attracting firms and businesses (with a frequent focus on foreign direct investment) to designated locations by offering tax holidays, providing fiscal support, infrastructures, and sometimes regulatory concessions. The latest statistics from the World Association of Investment Promotion Agencies report 249 member agencies from 157 countries[2]. These common policies are expensive to implement, hence the relevant outcomes of their targeted areas require careful evaluation.

The efficacy of such place-based policies is heatedly debated, as challenges in identification often impede researchers from deriving unbiased conclusions from standard cost-benefit analysis. (e.g., Greenstone et al. (2010); Head and Ries (1996); Moretti (2014); Wei (1993)). Such programs are set out to spark local economies by spending large amount of tax expenditure, where decision makers have the incentive to choose locations that are most likely to success from such government support. In some cases, they are timed specifically with the hope to rescue economic downturns. What exactly goes on behind these selection processes is rarely fully available to researchers, hindering our ability to identify the most suitable control group for estimating causal treatment effects of place-based policies.

In the case of China, its development zone program has been accredited to have played a crucial role in boosting its post-reform economy, as one of the first and major set of policies carried out by the Chinese reformist leadership after taking office in 1978[3]. Led by Deng Xiaoping, these policies were "learning-through-experiment" measures aimed at revitalizing the economy, which at the time was in much distress. Today, China has more than 219 such zones at the national level, and approximately 1350 at provincial level (Ministry of Commerce of the PRC, 2016; Wang, 2013),

[1] Development Zones, Special Economic Zones, or sometimes Enterprise Zones, are industrial park type of areas within a location that enjoy a set of policy packages (e.g., low tax rates, subsidies) to attract foreign investment and foster export production (mostly in developing economies), or to rejuvenate local business and growth (more often in developed countries) (Moretti, 2014).

[2] http://www.waipa.org/why.htm. Accessed in August 2014.

[3] Another one of the major first breakthroughs is the well-known "Household Responsibility System", which greatly boosted agricultural productivity by providing the right incentives (Naughton, 2007).

spanning over 270 prefectures. According to the Ministry of Commerce of the PRC (2014) Announcement No.54, national development zones in 2013 accounted for 12.1%, 9.9%, and 18.7% of China's annual national GDP, tax revenue, and import and export volumes.

Despite how the media has celebrated its expansive scale, efficacies in promoting local economy and fostering healthy urbanization, research on China's development zone program face the same set of aforementioned identification challenges. Drawing consistent and unbiased inference from this program will enhance our understanding of similar place-based policies. This will continue to play an important role in China's economy today, as well as being modeled after and implemented in many other countries (Boyenge, 2007; Farole and Akinci, 2011). While a handful of persuasive evidence for evaluating placed-based policies exists for various programs in the United States (Busso et al., 2013; Greenstone et al., 2010; Kline and Moretti, 2014), that of development zone programs in developing economies (and most notably in China given its unique context) is more debatable due to less convincing identification strategies.

Regarding the challenges stated above, this paper complements the existing literature from the following aspects. Firstly, I highlight the heterogeneous treatment effects on attracting foreign direct investment (FDI) by zones governed at the national versus provincial levels, and their timing of establishment between 1980 to 2006. Given the drastic changes in China's political and economic climates during this time period, early versus late hosting prefectures of the development zone program faced completely different economic conditions. Specifically, prefectures hosting early (prior to 1990) national development zones were the only places to legally receive FDI inflow then, and this advantage does not hold for later hosts as this restriction was lifted in 1991. Along with the differential subsidy and supervision levels by national versus provincial governments, prefectures having received this treatment varying by timing and support level are hardly comparable. Hence in this paper, I firstly present both descriptive plots and a differences in differences panel analysis to show that prefectures hosting early (pre-1990) national development zones attract substantially more FDI inflows and more new foreign firms than their late or provincial counterparts since designation, and that these differential effects persist.

Next, I devise an original instrumental variable to predict the designation of the group of early national development zones, in order to recover the causal effect of hosting these zones on FDI inflow and their subsequent urbanization patterns. The construction of this instrumental variable utilizes the fact that the group of early national development zones is designated with the emphasis to attract overseas Chinese investors (Vogel, 2011). Therefore, when a prefecture has strong ties with overseas Chinese investors, its probability of becoming host to an early national development zone is higher, all else being equal. Throughout numerous historical incidences and political movements prior to China's economic reform in late 1970s, waves of out-migration from mainland China occurred for reasons uncorrelated to the local economic conditions and potential of their sending places. This variation thus exogenously predicts a location's later likelihood of being chosen as an early national development zone. Thus, the connection between a Chinese prefecture and overseas regions with potential investors of Chinese descendant is modeled through shares of historical Chinese immigrants in these regions by their place of origin in mainland China.

The 2SLS estimates from this set of instrumental variables, having controlled for a comprehensive set of other locational economic characteristics and geography, show that prefectures hosting early national development zones persistently receive higher FDI inflows and attract more new foreign firms. As a result, these early national development zone hosts also attract more internal migrant workers. This happened firstly within the urban sector in the 1990s due to China's strict migration restriction. Later in the 2000s with policy relaxations, migration occurred from rural to urban sectors and intensified China's urbanization process, consistent with existing literature (e.g., Au and Henderson, 2006a). In terms of potential mechanisms behind this persistency, I find that prefectures hosting early national development zones have more productive firms, for either their head-start in growth and hence local agglomeration advantages or firm sorting, or likely both.

On the exclusion restriction front of instrumental variable strategies, one may worry about a prefecture's stronger ties with overseas Chinese in and of itself attract more FDI from this group of investors. In order to investigate this concern, I show that among early national development zones, FDI did not dis-proportionally flow into places with stronger connection to overseas Chinese[4]. While the Chinese central government's locational choices of early national development zone hosts are largely determined by each prefecture's closeness to overseas Chinese, as evident in the first stage results, this sentimental tie alone does not attract foreign investors. Rather, the hosting prefectures of early national development zones had the first-mover advantage in attracting FDI as they were the only legal FDI-receiving places and faced nearly no competition. This contrast between policy-making and economic motivation is of itself interesting, and further mitigates the concern of it violating exclusion restriction.

The rest of the paper is organized as the following. Section 3.2 briefly overviews the institutional context of China's development zone program, the existing literature and challenges, as well as the historic relevance to the construction of IV and China's internal migration policies. Section 2.3 carries out an analysis parallel the existing literature, which distinguishes heterogeneous effects by groups of development zones. Section 2.4 estimates the impact of early national development zone establishment on FDI and urbanization patterns via instrumental variable approach. Lastly, Section 3.6 concludes.

2.2 Background

2.2.1 Research context: types and timing of development zones

In 1980, four cities were first established as hosts of Special Economic Zones (SEZ's). The term "special economic zone" is a misnomer for what should have been translated as "development zone", yet adopted in the existing literature until today. SEZ is effectively a type and hence subset of development zones. For a jurisdiction to host a development zone, it entitles the local management committee to autonomously set up an industrial park type area, within whose boundary various

[4]Note that however this group has a small sample size of 16.

policy benefits and liberal economic environment are endowed. Common benefits for firms locating inside these zones include tax deductions, customs duty exemptions, discounted land-use fees, and so on (Alder et al., 2013; Wang, 2013). During China's early reform – a time of capital- and technology-deprivation – founding development zones was largely motivated to attract foreign direct investments. Over the following three decades, many more development zones of various types were gradually designated and built up both along the coastal cities and across major prefectures in China, as a result of the widely believed success of the first waves of pilot areas.

As previously mentioned, paramount contributions to economic growth made by national development zones have been consistently documented by official sources. But in contrast, such statistics by provincial zones are not systematically available, as they are designated and managed by individual provincial governments as opposed to the central government. Regardless, the perceived success of development zones across the board continues to incentivize experiments on new (types of) zones. For instance, the current central leadership of Li Keqiang launched the Pilot Free Trade Zone in Pudong, Shanghai in 2013 to test further liberalization on international trade and currency. The growth-promoting efficacies of development zones is also widely supported by academic research, a discussion that I elaborate in the next section. In this paper, however, I argue for the need to distinguish heterogeneous effects of these development zones by their level of governing, as well as by early versus late waves of zone establishment. The distinctions between these groups of development zones are closely related to the reform and political contexts (Xu, 2011). The difference of support level, managing governance, and motivations behind zone designation (especially for the provincial ones) could all affect their efficacies varyingly, yet systematically within-group, which has been largely absent from existing literature (except Alder et al., 2013). In this paper, I distinguish hosts of national from provincial development zones. National development zones are supported by the central government, either since its designation or had a later promotion from provincial level. The provincial development zones are only supported by the provincial government and never became national during the period of study (1970-2006). Furthermore, the central government could also shut down any level of development zones when the proliferation of lower level development zones was deemed unnecessary and ill-managed. One major round of "tidying up" started in late 2003, and reduced the number of development zones from 6,866 to 1,568 in two-year's time (Ministry of Commerce of the PRC, 2013a).

More importantly, I further categorize national development zones into three waves according to the official definition. The necessity of this distinction is also motivated by the fact that the timing of a development zone designation is closely related to the local economic conditions and climate, which evolved rapidly and substantially since China's market reform in and around the 80's, rendering a homogeneous treatment effect or assumption of random timing unsuitable. Specifically, the national development zone designation waves were of the following official timeline. Between 1980 and 1989, the first wave of national development zones was established as "experiments". This policy was pushed forward by China's leader at the time, Deng Xiaoping, and was temporarily interrupted between 1989 and 1990 due to political instability (e.g., Tiananmen incident). The second wave of national development zone establishments resumed in 1991 and continued onto 1990, which was known as the "growing" period. During this time, China also started to open up its economy nationally with relevant laws put in place to protect and regulate foreign investment, making it possible for all Chinese cities to legally receive FDI (Chen, 2011). And the last wave, the "stable development" group, in my sample was between 2000-2006[5] (Ministry of Commerce of the PRC, 2012, 2013b; Zhou, 2012). Since these waves of national development zones were established with different economic goals, the selection of them as well as the overall political and legal climates they face were also different from one another. Therefore in this paper, I put particular emphasis on the potential heterogeneity in treatment effects by waves of national development zones, as well as distinctively apart from the provincial level ones. As my analysis in the section 2.3 shows, the first wave of national development zones, which I call the early NDZ group, is the only group showing efficacies in attracting FDI. The geography of early national, late national, and provincial development zones is shown in Figure 2.1.

[5]This group is, by the official definition, those established between 2000 to 2008.

2.2.2 Relevant literature and ongoing challenges

In the existing research of place-based policies, much of the early effort is dedicated to studying their effectiveness. Such policies are common tools for stimulating local economy, with the goal to either start it up or break it free from recession. They are set out to first generate economic growth, which will then be ideally followed up with self-reinforcing agglomeration forces that both sustain and compound growth. On the other hand, place-based policies are financed by tax revenues and costly to implement, while the cost-benefit analysis do not always offer consistent verdicts (e.g., Einio and Overman, 2015; Head and Ries, 1996; Neumark and Simpson, 2014; Wei, 1993). The main challenge of any such policy evaluation lies in many sources of confounding factors. The information used in the decision-making of which place to receive such of a policy, either to pick the ones more likely to succeed or help lagging regions, is often not fully disclosed to researchers. Hence, any simple event study by treating place-based policies as stand-alone exogenous shocks would be subject to selection bias. Additionally, place-based policies are frequently accompanied by infrastructure investment, labor, land market regulations and so on, which are supplemental policies that tend to induce bias to key point estimator in the case of data unavailability (Zeng, 2010). To circumvent these selection and omitted variable bias, recent research focuses on finding the most convincing control group with differences in differences (DD) strategy and panel data – most notably are Greenstone et al. (2010), Busso et al. (2013), and Kline and Moretti (2014), in order to estimate unbiased and consistent economic effects of these place-based policies.

Across the existing literature of all place-based policies, China's development zone program is where the successful evidence concentrates (Moretti, 2014). The early work on China's development zone policies show substantial growth differentials between treated versus non-treated prefectures, however is shy of convincing counterfactuals despite the consistently positive findings (e.g., Cheng and Kwan, 2000; Demurger et al., 2002; Head and Ries, 1996; Jones et al., 2003; Wei, 1993). More comprehensive data availability has enabled recent studies to improve upon identification and expand the sample period for evaluation. For example, Wang (2013) and Alder et al. (2013) confirm the growth-generating effects (in FDI, export, GDP growth, etc.) on prefectures with development zone treatment under the DD and panel setting. Schminke and Biesebroeck (2013) analyze firm productivity and export behavior changes after they locate to development zones. Cheng (2014) further refines the unit of analysis to counties, and finds that the development zone program shift labor across sectors from agriculture to manufacturing, but not across regions within manufacturing.

Despite such comprehensive scrutiny of China's development zone program, identification in this context still falls short for two reasons. Firstly, all of the above mentioned research relies heavily on the DD strategy in absence of a nearly identical control group, where the assumption validity requiring random timing of treatment is easily challenged. In other words, development zones established by different levels of governments and at different times are treated as on "parallel trends". Meanwhile, development zones supported by national versus provincial governments are arguably set up to serve different agendas and receive policy benefits at varying scales. Moreover, development zones established right after China's economic reform in 1978 and before it opened up fully to foreign direct investment in the early 90's, face different market conditions than those set up afterward[6]. Secondly, all but one of the recent evaluations of China's DZ programs carried out their analysis at the prefecture level covering a long period of time – which means that most prefectures in sample would have received at least one or more opportunities of hosting a development zone. This would lead to ambiguous interpretations of key point estimates, where prefectures receiving treatment late might face entirely different conditions than their early counterparts. With unreliable identification, potential modifiable areal unit problem could also exacerbate (Briant et al., 2010; Openshaw and Taylor, 1979). In this paper, I construct a novel instrumental variable to provide an alternative causal identification approach to this question, which addresses both of the above challenges. This will allow me to complement the existing literature by

[6]Another minor issue in DD and panel designs is that they are generally subject to potential caveats of downward bias and leaving out useful information, as discussed in detail in Bertrand et al. (2004) and Baum-Snow and Ferreira (2014).

examining the impact of early designation of national development zones on FDI attraction over time, as well as the program's role in influencing China's urbanization patterns between 1990 and 2010.

Even if place-based policies have worked in starting up the local economy, does its positive effect last? This is the natural follow-up question that has strong theoretical underpinning (e.g., Glaeser et al., 1992; Henderson, 1974; Henderson et al., 1995; Krugman, 1991), but not yet fully addressed empirically. Conceptually, successful local economies would further attract and host productive firms, share infrastructure, well-matched labors, and foster knowledge and information spillovers. With such dynamic agglomeration forces, such of an economy could self-sustain growth in the context of place-based policies when the subsidy is later withdrawn. In their notable empirical work, Kline and Moretti (2014) show that the regional development program of the Tennessee Valley Authority led to large employment gains in both agriculture and manufacturing, while only the latter persisted after the federal subsidy ended. A similar persistent trend is also observed in China's development zone program under the DID framework (e.g., Wang, 2013). This paper complements this research question with evidence from the IV approach, showing a positive and long-lasting positive impact of China's early national development zone policy in attracting FDI inflows and internal labor movement. Along with evidence of more productive firms locating in hosting prefectures of early national development zones, this paper empirically supports the existence of persistent agglomeration economies of this policy.

2.2.3 Mainland Chinese emigration and early national development zone designation

To further complement existing literature, I provide an instrumental variable approach to identify the effects of early national development zone establishment on attracting foreign direct investment and its subsequent urbanization patterns. The construction and validity of this instrumental variable relies on the historical and institutional contexts of early waves of Chinese emigration, which is depicted in this section.

In the first few years following China's market reform, the majority of its inward FDI came from Hong Kong and Taiwan (Huaxia Jingwei News, 2007a,b; Xinhua News, 2008; Zhang, 2005). This is a result of many political and economical reasons combined. Both Hong Kong and Taiwan are geographically close to mainland China. They share very similar languages and culture ties, as Hong Kong and Taiwan are populated with Chinese descendant throughout hundreds of years of historical migration waves. Such proximities offer a strong bond and low fixed cost for potential overseas Chinese investors to be more likely to invest in China. Javorcik et al. (2011) further conceptualize that strong ethnic network also facilitates information flows and serve as contract enforcing mechanism. As a result, strong ethnic ties indicate more FDI inflows to migrants' places of origin, other factors being equal. In the Chinese case, these channels are arguably more important with its cultural emphasis on networks and connections (Zhang, 2005).

In addition to theoretical and empirical supports in the literature, the connection between ethnic ties and FDI also motivated the policy makers' locational decisions for designating the early development zones. When Deng Xiaoping, Chairman of China during this era, was selecting the first sites to host this special economic zone experiment, he particularly looked for places that are more attractive to overseas Chinese investors, on top of their underlying economic characteristics and potential. Deng's persistent emphasis on targeting overseas Chinese originated from the fact that most of the receiving places of Chinese emigrants at this time were far ahead than mainland China in their economic conditions[7], and were more likely to invest in mainland China than other investors with no ethnic or nostalgic ties. Hence, a prefecture's tie to regions with potential overseas Chinese investors such as Hong Kong and Taiwan was an important consideration in Deng's selection of national development zones locations (Vogel, 2011).

Building on this well-supported framework, I measure a Chinese prefecture's historical share of outgoing emigrants to Taiwan in order to proxy for its tie with Taiwan. This measure relies on the existing ethnic demographic composition of Taiwanese residents prior to 1970 (a "stock" measure), as well as a similar measure for

[7]For instance, the four East Asian tigers, Hong Kong, Singapore, South Korea, and Taiwan have had rapid annual economic growth between the early 1960s to 1990s.

the most recent wave of emigrants from mainland China to Taiwan (a "flow" measure). The idea is that the "flow" measure may capture additional variation resting on the fact that the most recent wave of emigrants would have much stronger ties to their places of origin. Unfortunately, I am unable to do the same for Hong Kong due to limited data availability and variation. Less worryingly, however, various news sources report that FDI by Taiwanese investors in the early stage of China's market reform (1980-1990) has consistently been under-reported for political reasons. The tense cross-strait relations between mainland China and Taiwan caused much of Taiwanese investments to re-route through Hong Kong before entering mainland China, despite cross-strait economic cooperation such as this was encouraged by both Chinese and Taiwanese governments at the time. As a result, early FDI coming from Taiwan has been systematically under-reported (People's Daily Online, 2002; Xinhua News, 1979, 1981). This in turn mitigates the concern of not having the same IV measures for Hong Kong.

In short, the greater a prefecture's historical share of outgoing emigrants to Taiwan, holding everything else equal, the more likely it would be chosen as one of the first hosting places of national development zone. In order for this to be a valid instrument, the motivations for the early emigrants to leave their hometowns in mainland China need to be uncorrelated to other local characteristics that later contributed to their designation as national development zones and economic growth. This condition is validated by the history of mainland Chinese emigrants. Very early waves of emigration from mainland China to Taiwan prior to the 20th century, which arguably contributes to the majority of variations in the "stock" measurement, occurred as results of war (Skeldon, 1996, 2002; Wong, 2011). Given the long gap in time between these migration waves and China's unstable modern history until the late 1970s, it is unlikely that prefecture differentials in economic conditions persisted.

As for the "flow" measurement, the most recent wave of migration from mainland China to Taiwan was a result of *KuoMinTang* (KMT) retreat, when the party ruling China for the first half of the twentieth century lost the civil war to the Chinese Communist Party (CCP). Throughout the 1940s, over 1.3 million KMT officials, other elites and their families relocated to Taiwan. Entrepreneurs intending to flee from a Communist system started to transfer funds, equipment and stocks of raw materials abroad. The fare from Shanghai to Taiwan was 80% more expensive by air and 60% by sea in 1949, and were still in great shortage(Lin, 2011; Mengin, 2015). While the exact data in ethnic composition of this wave of emigrants by prefecture is unavailable, it is modeled via the share of KMT army generals and vice generals by their hometown multiplying the distance to the nearest of the five prefectures with air/seaports still under KMT control during this time of retreat (namely, Shanghai, Chongqing, Guangzhou, Qingdao, and Chengdu). Hence, the proposed proxy measures the varying difficulty of retreating to Taiwan from different prefectures. The higher the KMT general share of a given province indicates higher chance for the resident (otherwise similar in other provinces) from this province to gain access to airplanes and ferries through same-province networks of family and friends[8]. Multiplying distance to the nearest of the five main KMT ports then captures the within-province differentials in travel cost for getting to the port. The economic conditions under KMT ruling may not necessarily be positively correlated to that under CCP ruling, as the two parties do not share the same political favoritism in geography. However, they could potentially be negatively correlated. For example, a prefecture with strong KMT ties may intentionally be less favored under CCP ruling. This raises a potential threat to the exclusion restriction of IV design.

2.2.4 Urbanization in China under the *hukou* system

In order to examine how early national development zones affect the urbanization patterns in its hosting prefectures via FDI, I provide a necessary overview of the relevant context – namely China's unique *hukou* system in this section. China's *hukou* system works similar to an internal passport system. When a Chinese citizen is born, he or she inherits a local citizenship tied to his or her mother's *hukou* place of residence (as specific as to neighborhood subdistrict, *jiedao*). This citizenship entitles the individual to either a rural or an urban status, which is a direct inheritance of whether or not one's ancestors are "peasants". This dichotomy of legal status,

[8]The *lao xiang* network – network of people from the same hometown – is especially potent among immigrants from developing countries (Munshi, 2003)

along with locational differentials, determine one's rights to housing, schooling, job opportunities, health care, and even "grain ratios" prior to 1990s (Chan, 1994).

As one's legal rights are so firmly tied with his or her *hukou* location and status, China's internal migration is strongly restricted through the *hukou* system. Prior to 2000, changing one's *hukou* status or to move across locations legally is very difficult. The rare channels are through college education or job relocation within the government or large state owned enterprises, with high friction and monetary cost. People could otherwise move illegally as "unregistered" migrant or legally but temporarily, and in both cases, they are entitled to zero or little local public provision at the receiving place (Au and Henderson, 2006b). Therefore, such inflexibility discourages anyone from moving.

However, China's market reform that started in the late 1970s and the subsequent inflows of foreign direct investment (FDI) created labor demand for manufacturing workers. This motivated surplus labor to move both within the urban sector and more so from rural to urban sectors even under strict *hukou* regulations. Starting in 2000, substantial relaxation (but not elimination) over *hukou* restrictions had further opened the door for internal migration (Chan, 2013). Existing literature comprehensively studies effects of the *hukou* system in labor mobility and city sizes (e.g., Au and Henderson, 2006a,b; Bosker et al., 2012). This paper is the first one to provide empirical evidence on this curious topic in light of the regional policy instrument of designation and establishing development zones.

2.2.5 Data

The scope of this paper covers all 286 majority-Han Chinese prefectures (colored areas in Figure 2.1) . Han China is where most economic activities take place, containing about 85% of China's population. The rest of China (gray area in Figure 2.1) is left out mainly because of data restrictions. As they are mostly autonomous regions, this exclusion is not concerning for the purpose of this paper. The relevant time span varies by the outcome variable examined, and is noted in each respective specification[9]. Prefecture definitions over time are standardized to the 2005

[9]Some variables are available from 1970 to 2006 while others 1982 to 2010.

boundary following Baum-Snow et al. (2015).

Data comes from several sources. Firstly, I collect information on both national and provincial development zones from official government websites such as the Ministry of Commerce and the Ministry of Land and Resources of the People's Republic of China[10]. The location, level of supporting government and designating year of a development zone are used to define types of development zones, as discussed in Section 2.2.1 and their geography visualized in Figure 2.1.

In order to examine the effect of development zones on attracting foreign firms, I use two measures. One of them is the direct measure of annual FDI inflows from 1996 to 2010 reported by the official Chinese statistical office. These figures are available at the county level from China Data Online, which I then aggregated up to construct prefecture measures[11].

However, the earliest FDI inflow measure starts in 1996, which is more than 15 years later than the first national development zone designation. In order to better capture the immediate post-treatment effects, I alternatively compute the total number of new foreign firms births annually from the Medium and Large Industrial Firm Surveys, 1998-2007. Foreign firms are identified as those with more than 50% foreign paid-in capital. As firms' open years are reported in the survey, I can track birth years back to as early as 1970. The total number of foreign firm births at the prefecture level from 1970 to 1998 is computed in the following way. Firstly from the 1998 survey, prefecture births of foreign firms are calculated for 1970-1998. Then the same annual figure for 1999 to 2007 are filled in from each of the corresponding survey. This conservative computation avoids double counting as the same firms may be surveyed multiple times during 1998-2007, while attempting to identify repeated firms from this series of surveys faces many challenges (Brandt et al., 2014).

Next, to examine urbanization patterns of prefectures that are hosts to early national development zones, I gather data on population and various migration measures from 1982, 1990, 2000, and 2010 national population censuses. Again,

[10]Accessible at: http://www.mofcom.gov.cn/ and http://www.mlr.gov.cn/.

[11]Since its market reform, China went through intensive urbanization that inevitably involved frequent boundary changes. By aggregating all figures up from the smallest geographic unit possible, I reduce potential bias introduced via measurement errors from inconsistent boundaries.

data at the county level is aggregated up to the prefecture level for minimizing measurement errors from inconsistent boundary change.

Lastly, the two instrumental variables proxy for ties between mainland Chinese prefectures and Taiwan, as introduced in section 2.2.3 and detailed in 3.4.2, are the distribution of Taiwanese residents by their original domicile in mainland China and the hometown of all Kuomintang (KMT) generals and vice generals. The distribution of Taiwanese by their original domicile is recorded in 1956 for all residents settled in Taiwan in and after 1945 (post civil war in China). This data is publicly available in paper form at the Taiwan National Library. The hometown record of army generals are those served between 1912 and 1993, compiled by historian Ren (2007). Next, distance from each prefecture to Taiwan and its nearest *Kuomintang* retreat port are computed in ArcGIS. Both distance measures are to proxy for the costs of traveling between one's hometown and Taiwan, either directly or through one of the KMT retreat ports. Distance is computed as the shortest distance between two polygons (from the border of a mainland Chinese sending city to the border of Taiwan).

2.3 Heterogeneous time trends

In this section, I present descriptive time trends of FDI attraction by different types of development zones. The descriptive patterns showcased in this section, although not causal, provides further evidence that the **early national** development zones enjoyed the early-mover advantage of this policy by persistently attracting more new foreign firms than late national and provincial development zones. In other words, drastic heterogeneous effects exist across types of development zones. This heterogeneity in turn motivates the question on how hosting early national development zones shapes the urbanization patterns of their hosting prefectures, as laid out in Section 2.4.

2.3.1 Time trends by types of development zones

As previously discussed, one would expect there to be differential effects by zone type in this context. National zones are expected to perform superior to provincial ones because they receive more and better support. The early national development zones, having been established in the first decade of China's market reform, were the only legal places to receive foreign investment and hence faced little competition. In comparison, their later counterparts were established when the entire country has opened up for foreign trade.

Figures 2.2 and 2.3 provide descriptive evidence for this. Both figures are Kernel density plots of logged total prefecture-level FDI inflows by zone types (early national, late national, and provincial). Each curve is the smoothed distribution of pooled five-year sum of annual census figures of committed FDI inflow to the group of prefectures hosting a particular zone type[12]. As these figures clearly show, prefectures hosting the early national development zones are substantially ahead of the rest of the development zone types. However, two factors could be at play here (assume for a moment that all treated zones are on average comparable, a causality issue I bring back to discussion later). One is that the early national development zones are more effective at attracting FDI than late national and provincial development zones. The other explanation could be the length of time since zone establishment as the policy benefits to attract FDI may take time to set in. For example, in Figure 2.2, the early national development zones would have been designated for at least 10 years while the late national and provincial zones were just around 4 years into their establishment. Regardless of which factor or both, the first-mover advantage enjoyed by the early national development zones is sizable and its persistency is unambiguous.

Next to make these types of development zones more comparable, I employ the differences in differences (DD) strategy to estimate this heterogeneous effect by development zone types on new foreign firm births before and after zone designation. The births of new foreign firm, as a measure on the extensive margin of foreign

[12]Prefecture figures are aggregated up from the county figures to minimize measurement errors introduced by administrative boundary change over time.

investment, can be traced back to as early as 1970, and hence will inform us with regards to the pre-trends and immediate effects of zone designation. The specification is the following:

$$\text{newFF}_{pt} = \text{Year}_t + \text{Prefecture}_p + \sum_{k \in K} \theta_k (\text{type k DZ post treatment trend})_{pt} + \varepsilon_{pt} \tag{2.1}$$

The dependent variable is the share of new foreign firm births over all new firms in a given prefecture, p, and a year, t, where $t \in [1970, 2007]$, the sample coverage. On the right hand side, the panel nature of this dataset allows for differencing out the prefecture- and year-specific characteristics with fixed effects (Year_t and Prefecture_p). θ's capture the annualized post treatment time trend for each of the k types of development zones: $k \in K = \{$early national development zones, late national development zones, provincial development zones $\}$. respectively. (type k DZ post treatment trend) is the product of post treatment dummy (switches on for all post treatment periods) and the number of years since treatment. Hence, these post trend variables take value 0 prior to the prefecture's treatment of development zone designation, and take the value of number of years since its treatment post-designation. In other words, each estimated θ is the average annual effects for the specific type of development zone, while the reference group is all prefectures prior to zone designation. The sample window is 7 years before and 7 years after designation. Lastly, the disturbance term is clustered at county level to address potential heteroskedasticity and serial correlation.

This specification is applied firstly to the entire sample of 286 Han prefectures. The estimation results are shown in column 1 of Table 2.1. Prefectures hosting the early national development zones are the only group showing positive and significant trends after designation: an annual average of 3 more foreign firm births than before. Whereas those hosting late national and provincial development zones show (small and) negative or no statistically significant effects. Secondly, equation (2.1) specification is applied to the sample of 273 prefectures that ever received a development zone designation by 2006, and the results are shown in column 2. The overall pattern is very similar to column 1 with the first wave of NDZ's showing statistically significant and positive estimate.

To visualize these time trends on a year-to-year basis, I plot the average annual births of new foreign firms for each development zone type in Figure 2.4, as estimated by the specification below:

$$\text{newFF}_{pt} \;=\; \text{Year}_t + \text{Prefecture}_p + \sum_{k \in K, n \in \{[-7,7] \setminus 0\} \cap \mathbb{Z}} \delta_{k,n} D_{pt}^{k,n} + \varepsilon_{pt} \qquad (2.2)$$

where everything except for δ's follow exactly the same as Equation 2.1. $D_{pt}^{k,n}$ is a binary variable equaling to 1 if in year t the prefecture p is n years to/from the year it became host to k type development zone, and 0 otherwise. Hence, δ's are the average annual births of new foreign firms for each development zone type relative to the year of their designation, while this number for the year of designation is normalized to 0. These point estimates are plotted in Figure 2.4[13]. It reveals the familiar pattern corresponding to the regression results. The early national development zone hosts on average attract 3 more new foreign firms than before its designation, which in this plot translates to about 20 firms after 7 years. The late national development zones worked in the first 4 years and this effect starts to fade out. However, we need to be cautious about the fact that part of this diluted effect could be due to their late designation. For instance, a late national development zone designated in 2003 would only have 4 years of post-treatment data since the analysis ends in 2007. Hence, we do not have sufficient evidence to conclude on the persistency of late national development zone designation. Lastly, the provincial development zones experience a small drop right after treatment and maintain a flat trend over time. The three sets of evidence above all point to a consistent pattern that the early national development zones are substantially more effective and its impact longer-lasting than the late national and provincial development zones. Analysis in this section shows that the scale and timing of policy implementation matter for its effectiveness. However, it is worth noting that no causal claim is being drawn

[13]Please note that these parameters are estimated after demeaned with prefecture and year fixed effects.

here, since the requirement of random timing of designation is not likely to be met in this context. Unbiased and consistent estimate of these DID setup require that these development zones follow the same trend as their pre-treatment selves in absence of treatment, where any trend breaks would come from being designated as development zones. As exhibited in Figure 2.4, the three types of development zones do not follow a parallel trend prior to treatment. Instead of any causal claims, this section focuses on providing evidence for the heterogeneity in development zone effects – that the FDI-attracting effect is seen on the early designated national development zones for potential explanations discussed earlier. The next section will address how the designation of early national development zone affects its hosting prefectures urbanization patterns.

2.4 The effects of early national development zone designation on urban growth

As learned from Section 2.3, the effectiveness of attracting FDI on the first wave of national development zones is substantially stronger and more persistent than the late national and provincial development zones. However, the above estimates are likely biased. Ideally for an unbiased estimate of such a policy, one would need to utilize exogenous variation that influences the locational decision of treatment but is uncorrelated with the economic potentials of such a location. Hence, I construct an original instrumental variable to assess the local average treatment effect of early national development zone designation, firstly on FDI and then on how it subsequently influenced the urbanization patterns of its hosting prefectures.

2.4.1 Predicting the first wave of national development zone designation

IV construction

As discussed in Section 2.2.3, the share of historical emigrants from a Chinese prefecture to Taiwan (in stock and flow terms) predicts its probability of being chosen as host to an early national development zone. The motives behind these historical emigration waves are largely uncorrelated to local economic potentials.

However, there are locational features that could affect its likelihood to host an early national development zone and its future economic performance. I try my best to control for these potential endogenous conditions. A prefecture's distance to coast is an example, as coastal area could be sending out more emigrants and favored by foreign investors simultaneously for its geography. As first formulated by Sachs et al. (1999) and widely applicable to the economic geography literature, distance to coast is one of the dominating factors for explaining economic growth differentials across countries. Meanwhile, residents from coastal regions, who face shorter distance to coast as well as to Taiwan, are also more likely to emigrate given cheaper transport cost, especially during an era when incurring transportation cost could be substantial. Therefore, the distance to coast of a location is systematically correlated to both its share of emigrants and economic potentials. I address this challenge by first incorporating distance into the instrumental variable construction following Boustan (2010) and later separately control for it:

$$\text{IV-TW}_p = \text{ShImTW}_j \cdot \frac{1}{\text{DTW}_p} \tag{2.3}$$

where p, j are subscripts for prefecture and provincial level variations respectively. IV-TW is the instrumental variable at the prefecture level that captures the tie between a Chinese prefecture and Taiwan through the "stock" of its population demographic breakdown by original domicile in the 1956 Taiwanese Population Census. It is the product of two components: the share of its Chinese immigrants by their mainland Chinese province of original domicile (ShImTW) and the inverse distance from each Chinese prefecture to Taiwan (DTW^{-1})[14]. The first component measures the then existing tie between a Chinese province and Taiwan, while the second component takes a form of distance measure to proxy for the out-migration transportation cost variation. This instrumental variable construction therefore models a mainland Chinese prefecture's likelihood of being chosen to host national development zone in the early stage of market reform by capturing its non-economic tie to Taiwan, and proxied by its share of emigrants to Taiwan prior to the Chinese Civil

[14] Distance is computed as the nearest distances between the borders of two polygons.

War. Additionally, a similar measure is applied to proxy for the most recent "flow" of emigration from mainland China to Taiwan as a result of the Chinese Civil War:

$$\text{IV-KMT}_p = \text{ShGenKMT}_j \cdot \frac{1}{\text{DPort}_p} \tag{2.4}$$

where ShGenKMT_j is the share of KMT generals whose hometown is province j, proxying for the tie between a province and the party. Similarly, the cost to travel to one of the main KMT retreat ports[15] is modeled by the shortest distance from a given prefecture to one of the nearest ports, computed similarly as above. Therefore, the interaction of the two variables gives this instrumental variable proxying for higher emigration flows to Taiwan in the 1940s from prefectures with stronger KMT ties.

First stage specification and results

With the above instrumental variables, I predict the designation of the first wave of national development zones with the following specification:

$$\begin{aligned} \text{earlyNDZ}_p =& \beta_1(\text{IV-TW}_p) + \beta_2(\text{IV-KMT}_p) + \gamma(\text{DCoast}_p^{-1}) \\ &+ (\mathbf{X}_p^{-1})\omega + \text{KMT Port}_p + \varepsilon_p \end{aligned} \tag{2.5}$$

where the dependent variable is a binary indicator for early NDZ designation at the prefecture level. It is predicted by the instruments following equations 2.3 and 2.4 while controlling for prefecture inverse distance to coast (DCoast_p^{-1}), and other prefecture-level characteristics, $\mathbf{X}_p^{-1}$, depending on specification. Prefecture inverse distance to coast is included to mitigate the worry that it is correlated to both larger share of emigration pre-reform and better economic performance post-reform, as the former determines travel cost for emigrants and the latter could be a dominating factor for explaining economic growth differentials across regions (Sachs et al., 1999). $\mathbf{X}_p$ are measures on prefecture-level economic characteristics with data availability, which include 1980 measures of road and rail rays to proxy for local infrastructure conditions as in Baum-Snow et al. (2015), and 1982 population to proxy for initial

[15] Recall that these ports are in Shanghai, Chongqing, Guangzhou, Qingdao, and Chengdu.

market size. Lastly, KMT retreat port fixed effects are included to take out any permanent connections between the port and KMT. The sample is the full set of 286 Han Chinese prefectures[16].

Table 2.2 presents variations of the first-stage regression outputs. The first two columns predict early national development zone designation with and without other prefecture-level economic characteristics. Both instrumental variables have strong predictive power, with the most recent flow of Chinese emigrants showing smaller point estimates but substantially stronger statistical power than the historical stock of emigrants. On average, having sent more immigrants to Taiwan by 1950s increases a prefecture's likelihood of hosting an early national development zone by 20%, whereas the most recent tie-strengthening post Civil War flow increases it by 7%. It is worth noting that by including controls on economic conditions in column 2, point estimates on the instrument variables stay consistent. This implies that the IV's are not biased by these measurable economic potentials. Next, columns 3 and 4 run the predictions instead on designations of late national development zones (i.e., after 1990). No positive predictive power is shown here, consistent with the previous reasoning that the instrumental variables should and are only applicable to the early treated prefectures supported at the national level.

2.4.2 Identifying the effect of early national development zone designation on foreign investment and urbanization patterns

With predicted linear probability of early national development zone designation, we can then estimate the unbiased effect of such treatment on post-treatment economic measures via two-stage least squares (2SLS) estimator following the general specification:

$$\begin{aligned} \ln(\mathrm{y}_{p,t}) =& \kappa(\widehat{\mathrm{earlyNDZ}}_p) + \eta(\mathrm{DCoast}_p^{-1}) + (\mathbf{X}_p^{-1})\rho \\ &+ \psi(\mathrm{y}_{p,t-n}) + \mathrm{KMT\ Port}_p + u_p \end{aligned} \tag{2.6}$$

[16]Instead of the full sample, same analysis are run by restricting it to the group of 273 prefectures ever received any development zone designation between 1978 and 2006, as well as the sample of prefectures in coastal provinces only. Both sets of results show consistent patterns .

where the dependent variable y represents various outcome measures (logged) at the prefecture level at time t. Additionally, the same measure of y lagged by n years is included on the right-hand-side when available, to control for pre-conditions[17]. The rest of the equation remain the same as previously stated. Equations 2.5 and 2.6 combined hence gives unbiased estimates of the treatment effect of early national development zone designation on hosting prefectures' FDI attraction over time and its resulting urbanization patterns.

Effects on FDI inflow, 1996-2010

Firstly, this analysis is applied to the measure of FDI from 1996 to 2010 at a five-year interval with $\ln(\text{FDI}_{p,1996-2000})$, $\ln(\text{FDI}_{p,2001-2005})$, and $\ln(\text{FDI}_{p,2006-2010})$ as the dependent variables. I pooled these annual measures every five years for two main reasons. The first is that data availability for this time series is not perfectly balanced, and pooling could mitigate potential missing variable concern. Secondly, the main pattern is readily present and easily seen when annual measures are aggregated up to five years. Since FDI measures prior to 1996 are not available, I use instead the count of total foreign firms born between 1970 to 1995 in the 1998 industrial survey to proxy for the pre-condition). The results are shown in Table 2.3 for each five-year intervals with OLS and IV estimations tabulated side by side.

Across the first row, we see that being able to host one of the first national development zones allows a prefecture to persistently attract substantially more FDI inflows during the period of 1996-2010. Specifically, these prefectures are estimated to attract approximately 381% higher total FDI inflows over the five-year interval of 1996-2000 (cumulative), and this lowers slightly to 328% and 338% times more between 2001 to 2005 and 2006 to 2010 respectively. Translating these figures into annualized terms, early national development zone hosts on average attract 31% more FDI than the rest of Han China annually from 1996-2000. This first mover advantage persists at an average annual rate of 27% more throughout the first decade of the 21st century. These estimates seem large in scale, but not unreasonable given the context. Essentially, these early national development zones were the only legal

[17]This lagged variable, although not shown in equation 2.5, is also included in the first-stage when 2SLS analysis is performed.

receiving places throughout the 1980s for any FDI, as China was only slowly opening up and improving its legal regulations over foreign investment starting in the 1990s. These early NDZ hosts thus faced much less competition for FDI inflows than they later did from 2001-2010. As China joined the World Trade Organization in 2001 and sparked another surge of annual FDI inflow (Walmsley et al., 2006), these early movers remarkably maintained their advantage[18].

Next, we see that IV results are consistently larger than their OLS counterparts across all time periods. OLS estimates showing downward bias suggest that the first wave of national development zones are not all comprised of places that are more likely to "win". Instead of looking for places that are originally more likely to be successful in attracting FDI, the central government could have allocated some of this beneficial policy to politically important (rather than economically sound) cities, of which such political favoritism is known to exist (Chen et al., 2015). This negative selection is consistent with what we observed in the first stage results where measured economic potentials in the early 1980's do not predict early NDZ designation. Moreover, this pattern of strong and positive treatment effect of the early national development zones is consistent with the previous DD estimates in Section 2.3. Although the IV analysis here is of direct FDI inflow measures whereas the DD design focused on new foreign firm births, evidence from both the intensive and extensive margins are in line. Lastly, we turn to the set of covariates in this analysis. Everything else being equal, having better rail infrastructure, large market size, and stronger pre-existing ties to foreign firms are positively associated with attracting more FDI inflows. However, these do not imply causality and are hence not of focus[19].

Effects on own urbanization patterns

As prefectures that host early national development zones consistently attract more foreign investment, how does the effectiveness of this policy subsequently impact the urban patterns of these prefectures? Along with greater FDI inflows, these

[18]However, I caution the literal interpretation of these coefficients as the sample of early NDZs is small (17 out of 286 prefectures).

[19]I had looked into analyzing neighbors' treatment effects with the same set of instruments with spatial weight, but the first stage does not have enough power

hosting prefectures also enjoy other benefits from this policy bundle (e.g., more infrastructure building or earlier improvements in the legal system) that could fundamentally affect their city growth trajectory and in particular labor market. This section looks into the average treatment effect of hosting early national development zone on city size differentials, as well as by more detailed demographic group.

Tables 2.5 through 2.7 are set out to test if prefectures hosting early national development zones are larger in terms of various types of population measures. Table 2.5 firstly shows that early national development zone hosting prefectures do not differ in total population sizes to the other prefectures in any of the decennial census measures, although OLS results in 2000 and 2010 would lead to different conclusions.

To examine this further, Tables 2.6 and 2.7 explore how the internal movements of different types of labor are affected by this policy varyingly. As discussed in section 2.2.4, while internal migration was strictly limited prior to 2000, holders of urban *hukou* always faced relative ease for relocation in comparison to their rural counterparts (Chan, 2010, 2013). In these tables, each of the in-migration outcome measure is defined as inflows of the specific type of migrants from the rest of China in the previous five years leading up to the census year. Specifically, urban migrants are those who hold non-agricultural *hukou*. Similarly, rural migrants are those who hold agricultural *hukou* in their sending place. Working-age migrants are then those aged between 18 and 55, irrespective of their *hukou* type. Since these migration variables are only available in 1990, 2000, and 2005, a pre-trend measure (i.e., $y_{p,t-n}$) for the 1990 regressions is absent. However, this does not challenge the comparability across regressions for the three census years because internal migration was practically nonexistent prior to 1990 due to very strict *hukou* regulations[20].

Table 2.6 looks at the effect of hosting early national development zone on attracting urban and rural migrants respectively. Nationally, urban *hukou* holders showed strong movement to prefectures hosting early national DZ's by 1990. This effect diminishes in the 2000 and 2005 censuses. Columns 4-6 show a reverse pattern when rural migrants are examined, with larger scales in 2000 and 2005 and less

[20]Some special cases existed but happened only infrequently (Au and Henderson, 2006b).

movements in 1990. Next, table 2.7 looks at specifically population of migrants of working-age. They show very similar patterns to that of rural migrants.

These urbanization trends suggest the following interactions between hosting early national development zones and changes in *hukou* regulation. Firstly, hosting early NDZs bring in higher FDI and more foreign firms. These capital and firms were mostly concentrated in the export-processing industries (Zhang, 2005; Zhang and Song, 2001; Zhang and Felmingham, 2001), which then generated demand for manufacturing labor that guided internal migration flows. Given the strict *hukou* restrictions prior to 2000, this surge in demand for labor was only met by migrants with urban *hukou* by 1990, and later in 2000 and 2005 by migrants with rural *hukou*. However, as internal migrants account for a small share of overall population (average share is 10% in 2010), I do not find the same effect on city size. These quantitative trends confirm the existing qualitative and descriptive findings in the literature, most notably by Chan (2012) and Chan (2013).

The underlying mechanisms behind persistency

With the above analysis, we can conclude that the hosting prefectures of early national development zones are given the head-start by this policy, benefiting from the status of being the only legal recipients of FDI at the time. Along with other benefit packages that come with the development zone program such as infrastructure investment, these prefectures built up agglomeration economies that allowed them to have persistent advantage over the rest of Han China in attracting FDI and grew bigger in size. Although I am unable to pinpoint which particular channels of agglomeration (e.g., labor pooling, sharing of infrastructure or intermediate suppliers, knowledge spillover, etc.) are at play or matter more relative to one another, I explore these with my best effort in this section.

Firstly, the investigation is done at the firm level to examine any productivity differentials between firms in early development zone hosting prefectures and those in the rest of Han China. The specification is of the following:

$$\begin{aligned}\ln(\text{output})_{ipt} =& \psi_1(\ln(\text{capital})_{ipt}) + \psi_2(\ln(\text{employment})_{ipt}) + \text{industry}_i + \text{ownership}_i \\ &+ \text{year}_t + \lambda(\text{TFP1995}_p) + \kappa(\widehat{\text{earlyNDZ}_p}) + \eta(\text{DCoast}_p^{-1}) \\ &+ (\mathbf{X}_p^{-1})\rho + \text{KMT Port}_p + u_{ipt} \end{aligned} \tag{2.7}$$

where i is the subscript for firm level variation. The dependent variable is the log of value added output. Firms are assumed to follow standard Cobb-Douglas production function where capital and labor have differential impacts on output, where 2-digit industry, firm ownership type, as well as year fixed effects are included. Furthermore, I control for 1995 prefecture level TFP measures to control for the pre-trend. This prefecture TFP measure is derived from 1995 industrial census data, same as in Chen et al. (2015). Firstly the firm-level TFP is calculated as $\ln(\text{net VA}) - 0.30 \ln k + 0.70 \ln l$. Next, firm-level TFP is regressed on industry and city fixed effects, as well as elasticity controls on 1995 numbers of firms in the own industry in the city, ln distance to the coast and 1990 GDP within 150 kms from the city. Then the estimated city fixed effects are used as city-level TFP for Equation 2.8. The rest of the Equation 2.8 follows exactly Equation 2.6 where treatment is predicted similarly to Equation 2.5, but at the firm level with the above list of covariates.

Since the dataset is cross-sectional annual medium and large firm surveys from 1998 to 2007, I am unable to follow firms over time during the key treatment period (namely, from 1980 to 1990 when the early national development zones are established). Hence, I am unable to tease out firm sorting from local agglomeration forces. Instead, we should think of this as the total factor productivity differential between firms located in prefectures hosting early national development zones versus the rest of Han China. The results are shown in Table 2.8. The first column follows exactly the specification of Equation 2.8, where the second column has an additional control to account for the number of years that the firm has been located in an early national development zone prefecture at the time of survey. It is evident that firms in the treatment prefectures are at least 20% more productive than the rest. Also as expected, state-owned firms are substantially less productive than the reference group, which include the private firms. Lastly, cities that are more productive in 1995, have better rail connections and higher populations in the early 1980s also have more productive firms. However as discussed, my analysis in this section could only provide suggestive rather than causal evidence. It is clear that productivity differentials exist between treatment and control cities, but nothing about firm sorting versus agglomeration can be identified.

Another potential underlying mechanism behind the observed persistency of early national development zones is that, potentially the early Taiwanese investors settle into these zones and build up local networks that further attract more Taiwanese investors in the following years. Here, we turn to analysis that may shed light on this potential channel. For this, I collect data on yearly Taiwanese FDI flows by Chinese provinces from 1991 to 2010, as well as the migration pattern of foreign born and settlers in China. However, data is again limited as the variation is only at the provincial rather than prefecture level. Thus, I aggregate all other measures to provincial level to perform the following IV-estimate:

$$\ln(\mathrm{y})_{jt} = \kappa(\widehat{\mathrm{earlyNDZ}_j}) + \eta(\mathrm{DCoast}_j^{-1}) + (\mathbf{X}_j^{-1})\rho + \mathrm{Year}_t + u_{jt} \tag{2.8}$$

where j again denotes provincial variation. The dependent variables are the log-levels of Taiwan investment flows to Chinese provinces from 1991 to 2010 from the Investment Commission of Taiwan Ministry of Economic Affairs; number of foreign born settlers with Chinese citizenship in Chinese provinces; and lastly number of foreign born settlers without Chinese citizenship, both from the 2000 National Census. Thus for Taiwanese investment inflows, I can further include year fixed effects, while for the external immigration measures there is no time variation. The rest of the variables follow exactly from Equation 2.6 but all (first-stage included) at the provincial level.

The results are presented in 2.9. Column 1 shows that provinces that host early national development zones continues to attract substantially more FDI from Taiwan over time. The last two columns showing external migration patterns are very weak analysis with limited cross sectional variation across 23 provinces, where 9 of them being early national development zone hosts. Hence, nothing can be concluded from them. This exercise shows that indeed Taiwanese FDI has strong persistency flowing to places that initially attracted Taiwanese FDI and hence built up network and local knowledge that later reinforce further investment.

2.5 Conclusion

In this paper, I first highlight the heterogeneous effects of China's development zone program by their level of government support and timing of designation. I show that early and national development zones have substantially greater success in attracting FDI, with long-lasting impact over time. This observation face similar identification challenges as much of the existing research on this topic, which motivates the next set of analysis.

Secondly, I provide an instrumental variable approach to identify the causal effects of early national development zone designation on its local economy in attracting FDI and the subsequent urbanization patterns. Hosting a national development zone prior to 1990 allows the prefecture to attract substantially more inward FDI, migrants from within the urban sector prior to *hukou* relaxation, and migrants from rural to urban sectors afterward.

Despite the above complementaries to the existing literature, this paper is limited in the following ways. Firstly, the key treatment of interest – designation of early national development zones – is of a relatively small scale in comparison to the other levels and timing of development zone treatment. As a result, the point estimates may not be precise. Secondly and more importantly, despite the novel contribution of an instrumental variable strategy in the research of place-based policies that mostly rely on difference-in-difference and panel settings, the estimated effects come short in its external validity.

Reference

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Figures

Figure 2.1: Geography of DZ-hosting prefectures by type, 1980-2006

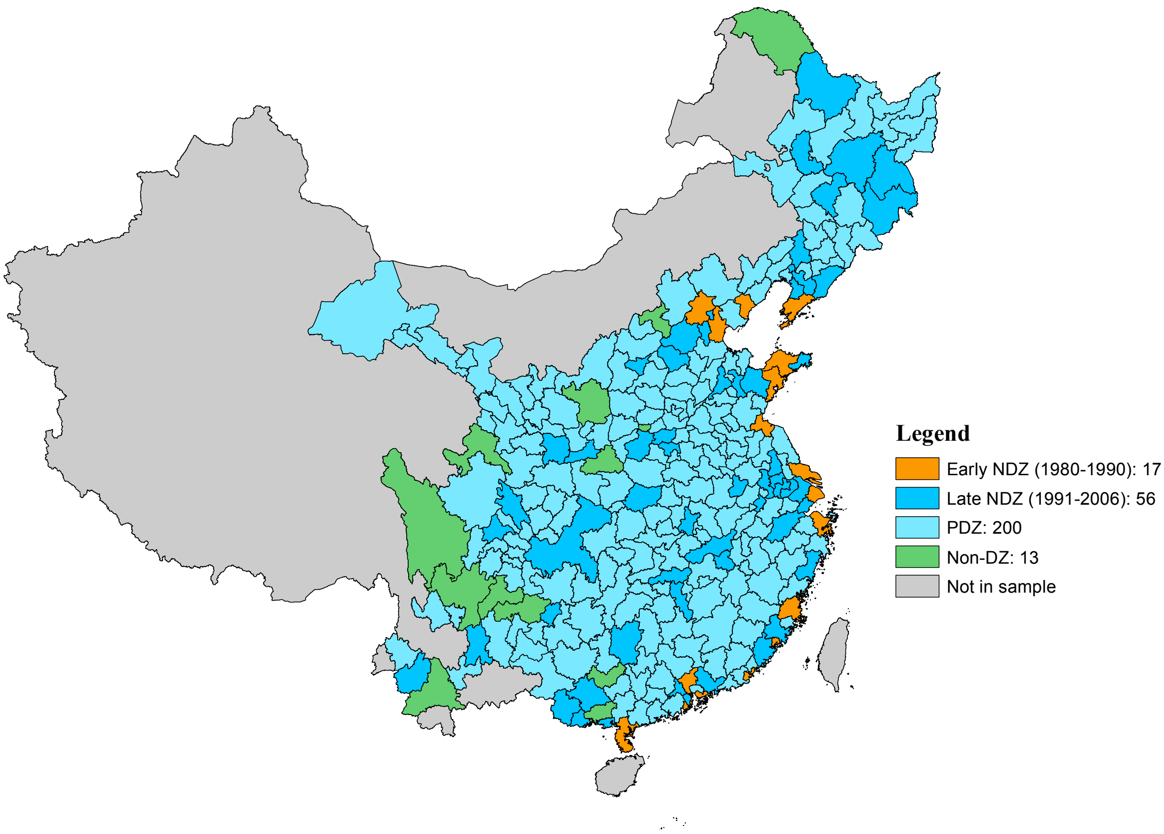


Figure 2.2: Total prefecture FDI inflows by type of zones, 1996-2000

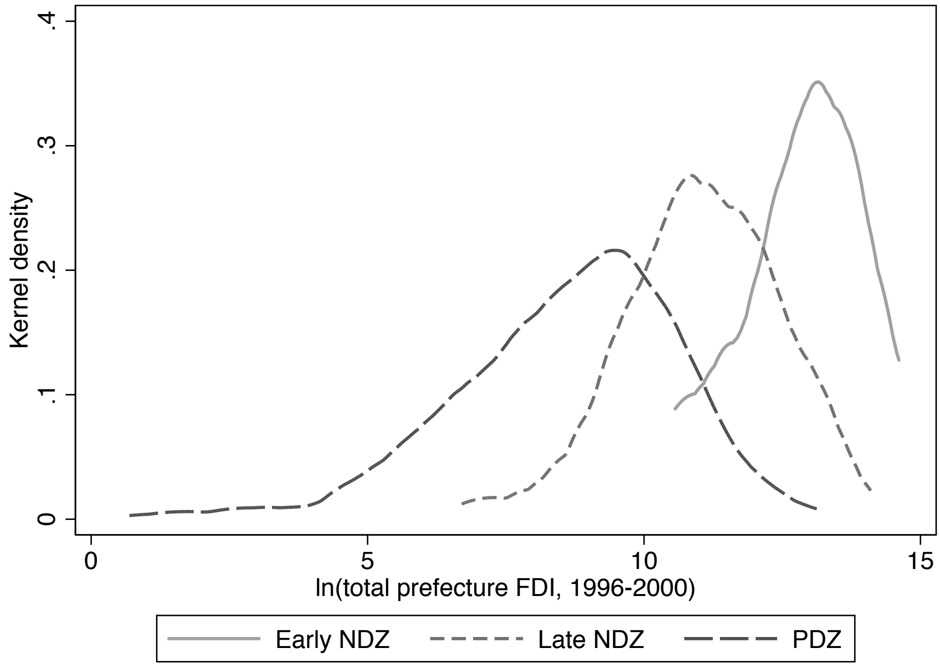


Figure 2.3: Total prefecture FDI inflows by type of zones, 2001-2005

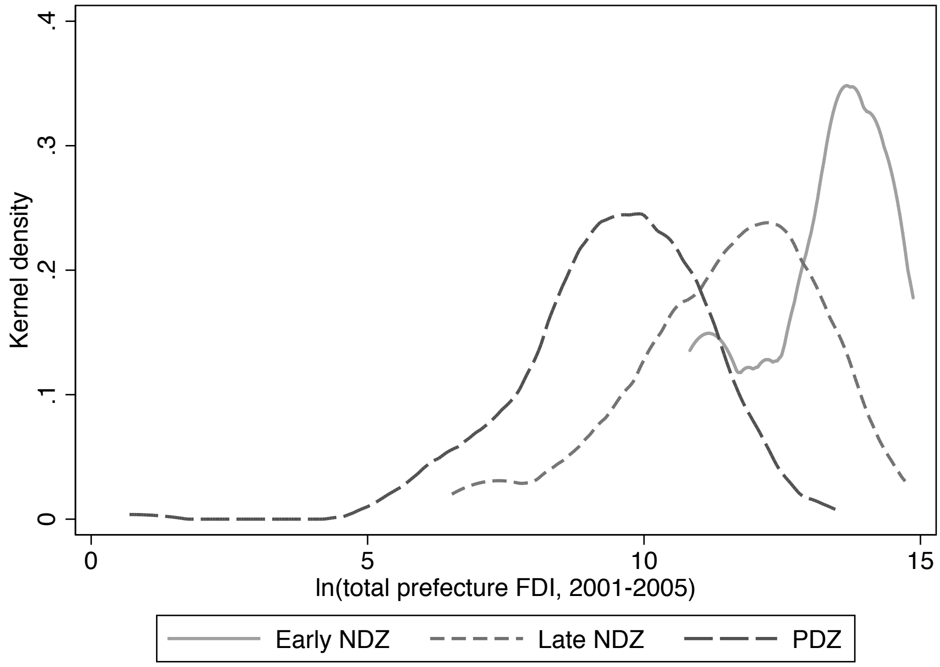
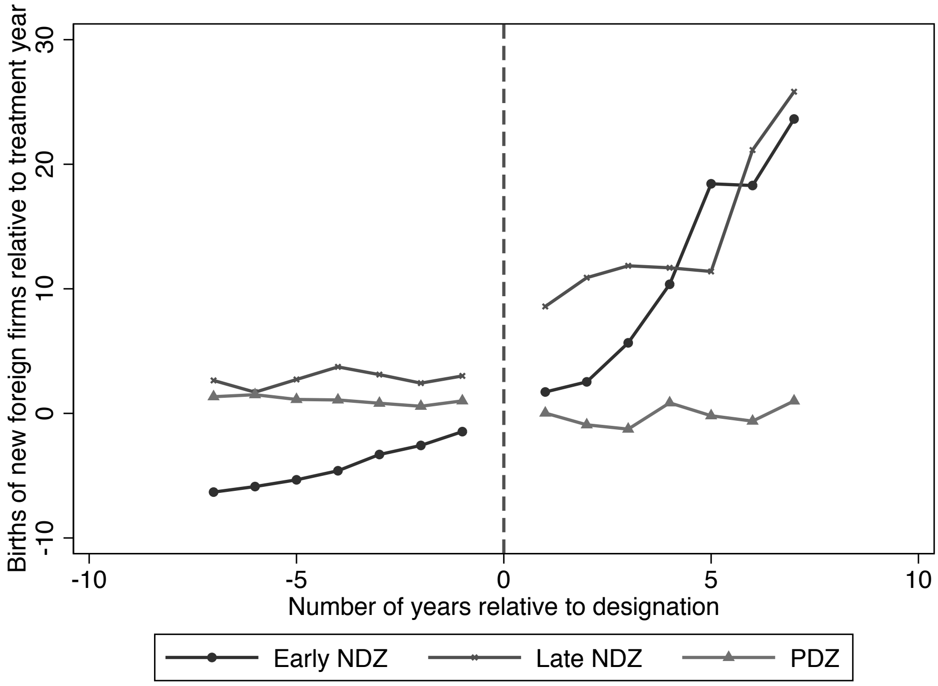


Figure 2.4: Annual births of foreign firms before and after development zone designation for early national, late national, and provincial DZ's



Tables

Table 2.1: Heterogeneous effects by types of development zone designation

	All prefectures: (1)	All treated prefectures: (2)
Post **early national** development zone trend	0.013***	0.012***
	(0.004)	(0.004)
Post **late national** development zone trend	0.000	-0.001
	(0.001)	(0.001)
Post **provincial** development zone (only) trend	-0.002*	-0.005***
	(0.001)	(0.001)
Constant	0.151***	0.165***
	(0.032)	(0.028)
N	4334	3877
R^2	0.630	0.681
Year and prefecture fixed effects	Yes	Yes

* p<0.1, ** p<0.05, *** p<0.01.
Prefecture-clustered standard errors in parenthesis.
Note that this table is not directly comparable to figure 2.4. The dependent variable here is the share of new foreign firms over total new firms, while the one in figure 2.4 is the number of new foreign firms. Furthermore, this table reports the time-weighted annualized average.

Table 2.2: IV: first stage

	Predicting early NDZ:		Predicting late NDZ:	
	(1)	(2)	(3)	(4)
IV-TW	0.204***	0.189**	0.094	0.133
	(0.071)	(0.073)	(0.142)	(0.139)
IV-KMT	0.071***	0.071***	-0.016***	-0.031***
	(0.007)	(0.008)	(0.004)	(0.008)
Inverse distance to coast	0.000	0.000	0.004***	0.004***
	(0.001)	(0.001)	(0.000)	(0.000)
Inverse distance to the nearest major river		-0.034**		0.099
		(0.014)		(0.064)
1980 road rays		-0.010		0.020
		(0.007)		(0.018)
1980 rail rays		0.001		0.041**
		(0.008)		(0.019)
ln(1982 population)		0.012		0.057
		(0.034)		(0.034)
ln(total raining days in a year)		-0.083*		0.058
		(0.047)		(0.076)
ln(range of elevation)		0.001		-0.060*
		(0.007)		(0.034)
Mean surface roughness		-0.000		0.000
		(0.000)		(0.000)
Mean daily temperature		0.006*		-0.003
		(0.003)		(0.006)
Constant	0.038***	0.187	0.192***	-0.591
	(0.012)	(0.414)	(0.024)	(0.593)
N	286	286	286	286
R^2	0.28	0.30	0.05	0.13
KMT port FE	Yes	Yes	Yes	Yes

* p<0.1, ** p<0.05, *** p<0.01.
Robust standard errors in parenthesis.

Table 2.3: OLS versus IV estimates: effects on FDI inflows

	1996-2000		2001-2005		2006-2010	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Early national development zone	1.779***	2.700**	1.447***	1.792*	1.273***	2.074***
	(0.497)	(1.291)	(0.512)	(0.980)	(0.449)	(0.536)
Inverse distance to coast	0.008***	0.008***	0.000	-0.000	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Inverse distance to the nearest major river	0.236	0.265	-0.022	-0.013	0.180	0.201
	(0.188)	(0.188)	(0.348)	(0.339)	(0.217)	(0.213)
1980 road rays	-0.115	-0.100	-0.047	-0.041	-0.014	-0.001
	(0.081)	(0.081)	(0.065)	(0.065)	(0.065)	(0.064)
1980 rail rays	0.228**	0.233***	0.101	0.102	0.234***	0.238***
	(0.092)	(0.090)	(0.077)	(0.075)	(0.077)	(0.075)
ln(1982 population)	1.030***	1.003***	0.946***	0.937***	0.545***	0.524***
	(0.177)	(0.173)	(0.157)	(0.156)	(0.128)	(0.127)
ln(total raining days in a year)	-0.678	-0.584	-0.316	-0.283	-0.801**	-0.721**
	(0.419)	(0.425)	(0.423)	(0.405)	(0.340)	(0.329)
ln(range of elevation)	0.012	0.020	-0.119	-0.116	-0.131	-0.124
	(0.138)	(0.135)	(0.132)	(0.129)	(0.127)	(0.124)
Mean surface roughness	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean daily temperature	0.150***	0.146***	0.138***	0.137***	0.120***	0.117***
	(0.031)	(0.031)	(0.023)	(0.023)	(0.022)	(0.022)
Counts of new foreign firms in 1995	0.022***	0.020***	0.020***	0.019***	0.017***	0.015***
	(0.007)	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)
Constant	-4.833	-4.947	-3.380	-3.423	5.608**	5.491**
	(3.309)	(3.184)	(3.302)	(3.180)	(2.686)	(2.614)
N	265	265	277	277	272	272
KP F stat		59.15		58.38		56.63
KMT port FE	Yes	Yes	Yes	Yes	Yes	Yes

* p<0.1, ** p<0.05, *** p<0.01.
Robust standard errors in parenthesis.

Table 2.4: FDI does not dis-proportionally go to places with strong Taiwanese ties

	Total number of foreign firms		ln(Foreign Direct Investment)	
	3 years post designation	5 years post designation	1996	1996-2000 total
	(1)	(2)	(3)	(4)
IV-TW	0.176	6.947	0.064	0.020
	(6.323)	(23.755)	(1.804)	(2.342)
IV-KMT	2.569	13.146	0.597	0.509
	(4.724)	(17.031)	(0.781)	(1.033)
Inverse distance to coast	0.443	2.049	-0.045	-0.029
	(0.556)	(2.016)	(0.128)	(0.166)
Inverse distance to the nearest major river	-35.287	-271.840	-0.521	1.191
	(53.467)	(192.826)	(9.547)	(12.561)
1980 road rays	3.429	29.414	0.158	0.060
	(9.850)	(37.125)	(2.856)	(3.709)
1980 rail rays	3.620	6.345	-1.056	-0.774
	(7.947)	(30.740)	(3.000)	(3.871)
ln(1982 population)	-0.503	-9.415	0.205	0.098
	(5.594)	(20.612)	(1.246)	(1.632)
ln(total raining days in a year)	-10.378	-73.154	-1.967	-1.420
	(24.352)	(89.165)	(6.196)	(8.033)
ln(range of elevation)	1.540	-8.263	3.101	2.898
	(18.498)	(69.018)	(4.573)	(5.976)
Mean surface roughness	0.000	0.004	0.000	0.000
	(0.001)	(0.006)	(0.001)	(0.001)
Mean daily temperature	1.182	6.647	0.135	0.113
	(1.730)	(6.020)	(0.190)	(0.251)
Constant	12.759	324.791	-5.568	-3.407
	(218.977)	(837.423)	(68.319)	(88.723)
N	17	17	17	17
R^2	0.88	0.91	0.90	0.83
KMT port FE	Yes	Yes	Yes	Yes

* p<0.1, ** p<0.05, *** p<0.01.
Robust standard errors in parenthesis.

Table 2.5: Effects on city sizes

	1990		2000		2010	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Early national development zone	0.135	0.027	0.338**	0.200	0.471**	0.351
	(0.088)	(0.043)	(0.164)	(0.159)	(0.182)	(0.317)
Inverse distance to coast	0.000	0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Inverse distance to the nearest major river	-0.003	-0.007	-0.004	-0.008	0.002	-0.002
	(0.007)	(0.008)	(0.018)	(0.019)	(0.026)	(0.028)
1980 road rays	0.012**	0.011**	0.025**	0.024**	0.026**	0.024**
	(0.006)	(0.005)	(0.011)	(0.010)	(0.013)	(0.012)
1980 rail rays	0.009**	0.009**	0.015**	0.014**	0.021**	0.020**
	(0.004)	(0.004)	(0.007)	(0.007)	(0.010)	(0.009)
ln(1982 population)	0.921***	0.923***	0.837***	0.840***	0.800***	0.802***
	(0.028)	(0.028)	(0.051)	(0.050)	(0.055)	(0.054)
ln(total raining days in a year)	-0.017	-0.027	-0.054	-0.066*	-0.111**	-0.121**
	(0.021)	(0.018)	(0.038)	(0.034)	(0.048)	(0.050)
ln(range of elevation)	0.010	0.009	0.022*	0.020*	0.019	0.017
	(0.007)	(0.006)	(0.012)	(0.011)	(0.017)	(0.015)
Mean surface roughness	-0.000**	-0.000**	-0.000**	-0.000**	-0.000*	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean daily temperature	0.006***	0.007***	0.015***	0.016***	0.021***	0.022***
	(0.002)	(0.002)	(0.004)	(0.005)	(0.005)	(0.006)
Constant	1.200***	1.228***	2.479***	2.515***	3.278***	3.309***
	(0.297)	(0.324)	(0.537)	(0.577)	(0.612)	(0.645)
N	286	286	286	286	286	286
KP F stat		49.42		49.42		49.42
KMT port FE	Yes	Yes	Yes	Yes	Yes	Yes

* p<0.1, ** p<0.05, *** p<0.01.
Robust standard errors in parenthesis.

Table 2.6: Effects on internal migration

	Urban citizens			Rural citizens		
	1990	2000	2005	1990	2000	2005
	(1)	(2)	(3)	(4)	(5)	(6)
Early national development zone	1.568**	0.942	0.857	0.813	1.746***	1.814***
	(0.687)	(0.632)	(0.680)	(0.804)	(0.558)	(0.656)
Inverse distance to coast	0.009***	-0.001	-0.003***	0.008***	-0.005***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Inverse distance to the nearest major river	-0.020	0.129	0.167	0.037	0.158	0.188
	(0.138)	(0.093)	(0.103)	(0.149)	(0.112)	(0.120)
1980 road rays	0.028	0.033	0.038	-0.022	0.037	0.047
	(0.052)	(0.033)	(0.040)	(0.051)	(0.043)	(0.047)
1980 rail rays	0.127**	0.081**	0.152***	0.156***	0.009	0.029
	(0.051)	(0.035)	(0.043)	(0.054)	(0.040)	(0.044)
ln(1982 population)	0.701***	0.203*	0.183	0.347**	0.131	0.101
	(0.140)	(0.114)	(0.116)	(0.141)	(0.099)	(0.106)
ln(total raining days in a year)	0.087	0.106	0.026	0.331	0.056	0.380*
	(0.260)	(0.155)	(0.187)	(0.251)	(0.186)	(0.206)
ln(range of elevation)	-0.015	0.033	0.023	-0.055	-0.024	-0.059
	(0.087)	(0.053)	(0.070)	(0.082)	(0.062)	(0.072)
Mean surface roughness	-0.000	-0.000	-0.000*	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean daily temperature	-0.028*	0.025*	0.031**	-0.004	0.043***	0.031**
	(0.017)	(0.014)	(0.014)	(0.020)	(0.015)	(0.015)
ln(1990 in-migration, urban)		0.381***	0.466***			
		(0.068)	(0.074)			
ln(1990 in-migration, rural)					0.531***	0.547***
					(0.063)	(0.066)
Constant	-1.823	2.033	2.677	3.847*	2.246	1.830
	(2.261)	(1.434)	(1.695)	(2.116)	(1.560)	(1.617)
N	283	283	282	285	285	285
KP F stat	49.16	75.21	74.58	49.52	74.93	74.93
KMT port FE	Yes	Yes	Yes	Yes	Yes	Yes

* p<0.1, ** p<0.05, *** p<0.01.
Robust SE in parenthesis.

Table 2.7: Effects on working-age internal migration

	1990		2000		2005	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Early national development zone	1.445***	1.005	1.101***	1.656***	1.126***	1.538**
	(0.360)	(0.822)	(0.250)	(0.600)	(0.275)	(0.674)
Inverse distance to coast	0.008***	0.008***	-0.005***	-0.004***	-0.006***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Inverse distance to the nearest major river	0.033	0.019	0.145	0.162	0.154	0.167
	(0.147)	(0.145)	(0.108)	(0.108)	(0.119)	(0.119)
1980 road rays	0.011	0.006	0.024	0.031	0.034	0.039
	(0.053)	(0.052)	(0.041)	(0.041)	(0.044)	(0.044)
1980 rail rays	0.136**	0.134**	0.026	0.031	0.057	0.061
	(0.057)	(0.055)	(0.039)	(0.038)	(0.044)	(0.043)
ln(1982 population)	0.436***	0.443***	0.119	0.123	0.106	0.109
	(0.150)	(0.151)	(0.101)	(0.096)	(0.106)	(0.102)
ln(total raining days in a year)	0.409	0.370	-0.013	0.044	0.222	0.264
	(0.268)	(0.270)	(0.173)	(0.174)	(0.196)	(0.196)
ln(range of elevation)	-0.021	-0.026	-0.031	-0.025	-0.056	-0.052
	(0.088)	(0.085)	(0.061)	(0.059)	(0.072)	(0.071)
Mean surface roughness	-0.000	-0.000	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean daily temperature	-0.011	-0.008	0.044***	0.039***	0.035**	0.032**
	(0.019)	(0.020)	(0.013)	(0.014)	(0.014)	(0.015)
ln(1990 in-migration, working-age)			0.548***	0.521***	0.582***	0.561***
			(0.059)	(0.063)	(0.061)	(0.066)
Constant	1.902	2.015	2.778*	2.697*	2.344	2.284
	(2.478)	(2.481)	(1.534)	(1.473)	(1.618)	(1.559)
N	286	286	286	286	286	286
KP F stat		49.42		76.99		76.99
KMT port FE	Yes	Yes	Yes	Yes	Yes	Yes

* p<0.1, ** p<0.05, *** p<0.01.
Robust standard errors in parenthesis.

Table 2.8: Total factor productivity differentials between firms in and out of early national development zone hosting prefectures

	(1)	(2)
Early national development zone	0.166***	0.260**
	(0.050)	(0.111)
ln(capital)	0.377***	0.376***
	(0.007)	(0.007)
ln(employment)	0.454***	0.454***
	(0.011)	(0.011)
Collective owned firms	-0.003	-0.008
	(0.017)	(0.017)
Foreign owned firms	0.026	0.028
	(0.030)	(0.030)
State owned firms	-0.708***	-0.713***
	(0.023)	(0.023)
City level TFP in 1995	0.231***	0.231***
	(0.065)	(0.064)
Inverse distance to coast	0.001	0.001
	(0.002)	(0.002)
Inverse distance to the nearest major river	-0.033	-0.033
	(0.037)	(0.038)
1980 road rays	0.009	0.009
	(0.012)	(0.012)
1980 rail rays	0.012	0.012
	(0.013)	(0.013)
ln(1982 population)	-0.001	-0.008
	(0.036)	(0.037)
ln(total raining days in a year)	-0.194**	-0.186**
	(0.081)	(0.082)
ln(range of elevation)	-0.023*	-0.022*
	(0.013)	(0.013)
Mean surface roughness	-0.001***	-0.001***
	(0.000)	(0.000)
Mean daily temperature	0.013**	0.012**
	(0.006)	(0.006)
Number of years having located in early NDZ		-0.007
		(0.007)
Constant	4.411***	4.492***
	(0.643)	(0.655)
N	1954312	1954312
KP F stat	43.24	15.96
Industry, ownership, year, and port fixed effects	Yes	Yes

* p<0.1, ** p<0.05, *** p<0.01.
Robust standard errors in parenthesis.

Table 2.9: Early national development zone effects on later Taiwanese FDI inflows and external migration patterns

dependent variable:	ln(Taiwan FDI)	ln(Foreign born citizens)	ln(Foreign born residents)
	(1)	(2)	(3)
Early national development zone	3.208***	1.935**	0.907
	(0.231)	(0.791)	(0.610)
Inverse distance to coast	-0.005***	0.018***	-0.001
	(0.002)	(0.006)	(0.005)
Inverse distance to the nearest major river	0.153***	-0.327**	-0.045
	(0.038)	(0.135)	(0.104)
1980 road rays	0.018	-0.038	0.031
	(0.015)	(0.052)	(0.040)
1980 rail rays	0.002	-0.071	-0.051
	(0.018)	(0.064)	(0.049)
ln(1982 population)	-0.055***	-0.058	-0.010
	(0.014)	(0.050)	(0.039)
ln(total raining days in a year)	0.143***	0.167*	0.080
	(0.029)	(0.100)	(0.077)
ln(range of elevation)	-0.002	0.084	-0.041
	(0.022)	(0.078)	(0.060)
Mean surface roughness	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
Mean daily temperature	0.009***	-0.011	-0.001
	(0.002)	(0.007)	(0.006)
Constant	10.547***	4.632***	3.805***
	(0.342)	(0.813)	(0.627)
N	429	23	23
KP F stat	149.76	4.44	4.44
Year fixed effects	Yes	No	No

* p<0.1, ** p<0.05, *** p<0.01.
Robust standard errors in parenthesis.

Chapter 3

Air pollution, regulations, and labor mobility in China

3.1 Introduction

Environmental regulations are becoming more prevalent in developing countries as they begin to tackle pollution. In order to understand the local economic impacts of such regulations, two opposing forces are of consideration. On the one hand, regulations increase the cost of production and lower productivity of polluting firms (Greenstone et al., 2012; Roback, 1982; Shapiro and Walker, 2017). This could hurt local economic activities and labor market (Becker and Henderson, 2000; Greenstone, 2002; Walker, 2013). On the other hand, good air quality is an amenity that attracts human capital and likely benefits the local economy (Chay and Greenstone, 2003; Chen et al., 2017a; Currie and Neidell, 2005; Glaeser et al., 2001; Heblich et al., 2016; Lin, 2018; Schlenker and Walker, 2016). While the existing literature provides sound evidence on how these two forces respectively affect the local economy, our knowledge of the net effects of these two opposing forces is limited.

In this paper, I study the effects of pollution regulation on China's local economies between 2000 and 2010 by considering the above two opposing forces and their net effects. China is one of the largest coal consumers in the world and findings in this context will provide valuable insights for other emerging economies. Policy implications for developing countries may be different to developed countries as the former face significantly greater levels of pollution and challenges to regulation enforcement, due to their institutional contentions (Greenstone and Hanna, 2014; Oliva, 2015). We also have little evidence of how amenities such as air quality is valued in developing countries (Freeman et al., 2017; Zheng et al., 2011; Zheng and Kahn, 2017). Lastly, avoidance behavior such as migrating away from high pollution areas may be more costly in the developing world for institutional reasons or lack of transport infrastructure and information, which could exacerbate adverse regional shocks (Morten and Oliveira, 2016).

China undergoes two unique policy reforms in its environmental and migration regulations at comparatively the same time. This enables the possibility of learning both the productivity and amenity effects of environmental regulation on its local economies. In around 1998, China implements the Air Pollution Prevention and Control Law (APPCL), marking the state's first serious effort to regulate pollution. The state council sets out a national air quality standard in APPCL for all prefectures to achieve. Prefectures with worse air quality at the time of implementation would face more stringent regulation in the following years. While intensity of pollution regulation is often difficult to measure (e.g., Greenstone and Hanna, 2014), in this case a prefecture's existing level of SO_2 pollution serves as a reasonable proxy for the regulation intensity it will face (Shi and Xu, 2018). This assumption is also supported by statistical evidence. Shortly after, in 2000, China's labor mobility restrictions (*hukou* system), which previously curtail internal migration, begins to relax (Au and Henderson, 2006a). This reform essentially reduces migration costs, making it possible for people to move to prefectures where they are better off. Changes in city sizes and local employment between 2000 and 2010, therefore, reveal both the potential productivity effect of APPCL and the amenity effect of different air quality across space.

To formalize the above discussion, I lay out a Rosen-Roback style spatial equilibrium model based on Moretti (2011) and Allcott and Keniston (2014). Under this framework, effects on the local economy are measured in terms of changes in prefecture population and employment. The implementation of APPCL is captured by reduction in productivity. Reform in labor mobility that reduces migration costs, modeled as an increase in eligible social benefits Tombe and Zhu (2017), allows population and employment changes across space to occur from initially sharply restricted starting points. I focus on heterogeneous workers of two skill types: high and low-skilled workers employed by different sectors who have skill-specific valuation of amenity and migration costs[1]. As APPCL stringency positively correlates with existing air pollution levels, it affects local population and employment when migration costs are lowered via two channels in the Roback terminology. The first channel relates to variation in air pollution itself. Workers are more likely to sort to places with better amenity (e.g., cleaner air) in light of reduction of migration costs. This

[1]While both heterogeneous tastes for amenity and migration costs by skill are backed by existing literature and actual policy, the assumption that high- and low-skilled workers have no overlap in sector is extreme. However, relaxing this assumption will not change the main messages from the model. Adhering to it allows me to keep the model simple and straightforward. In fact, less than 5% of the workforce in the industrial sector is documented to have college degrees and above.

channel is more salient for high-skilled workers, because of their relatively higher valuation of air quality and consistently lower migration cost. The second is through the productivity channel. APPCL imposes higher costs on SO_2-polluting firms, and more so on those in heavier polluted prefectures. Prefectures with higher pollution levels at the start of APPCL, relative to their less polluted counterparts, receive a negative productivity shock. This lowers local employment, particularly in the polluting sector. The resulting adverse labor demand shock worsens the employment outlook for low-skilled workers locally. Depending on the relative strengths of their migration costs and idiosyncratic preferences for home location, low-skilled workers may subsequently reallocate to the non-polluting sector in the same prefecture, other prefectures less affected by APPCL, or remain unemployed.

By empirically testing the above discussion, the main identification challenge I face is the non-random distribution of local pollution levels. A prefecture's air quality is frequently correlated with its level of economic activity (Chay and Greenstone, 2003). Heavily polluted places tend to be more developed or industrialized, and may have endogenously distributed industry compositions. Moreover, development and particularly urbanization can lead to further heavier air pollution (Zheng and Kahn, 2013), raising concerns of reverse causality. Therefore, comparing prefectures with non-randomly distributed air pollution levels and regulation intensity would lead to bias.

I extend the literature by constructing a novel instrument of power plant suitability to capture a location's likelihood to pollute heavily. Thermal power plants contribute to over 40% of China's SO_2 emissions (Lu et al., 2010), while electricity distribution and pricing are centralized. Two prefectures with comparable economic characteristics may differ substantially in their ambient SO_2 levels simply due to one hosting a thermal power plant and the other does not. However, the actual power plant locations may still be endogenous for reasons such as political nepotism. Therefore I construct a measure of a location's suitability for hosting thermal power plants by borrowing from engineering literature. This suitability index is a nonlinear transformation of a given location's characteristics, including measures of its elevation, slope, access to water, distance to rail and so on. The nonlinear transformation of these factors is determined from the engineering considerations based on their relative importance in power plant construction. In order to compute this index, I draw together a large set of remote sensing data that jointly produce spatial accuracy of nine by nine square kilometers.

The suitability index strongly predicts the actual thermal power plant locations, through which its predictive power of local ambient SO_2 levels is established. Notably, the exogeneity of this instrument comes from the nonlinear transformation and weights applied to each factor of its composition, rather than the factors themselves. This functional form determines the importance of each factor for building thermal power plants solely from engineering considerations, while some of the factors themselves may still be endogenous to the economic outcomes of a location. Therefore in the analysis, I take several measures to ensure the validity of using this suitability measure as an instrumental variable. The first step I take is to further control for three sets of variables. The first is a set of local economic characteristics including existing transport infrastructures, share of agricultural employment, etc. They control for key initial differences across prefectures. The second set includes other amenity measures that are not factors in the computation of power plant suitability, such as annual average temperature and wind speed. The last set of variables are components of the suitability index that could potentially have economic influences themselves. For example, having access to water is crucial to building thermal power plants for cooling. As proximity to waterways is commonly considered as a contributor to local economic growth, I include distance to large rivers and coasts as additional controls in the analysis. The inclusion of these three sets of variables minimizes threats to the instrument for meeting the criterion of exclusion restriction.

I conduct falsification and sensitivity tests conditional on the above covariates. The set of falsification tests show that prefectures with higher suitability values do not have pre-existing growth advantage in terms of prefecture sizes, high-skilled workers, or manufacturing and overall employment. Analysis using an alternative computation of the instrumental variable, where only relatively non-economic factors are included, conforms with baseline estimates. Moreover, key results are also robust to using the classic air pollution instrumental variable, thermal inversion (e.g., Arceo et al., 2016; Fu et al., 2017; Jans et al., 2014; Sager, 2016), which is subject to different spatial variation from power plant suitability. Further sensitivity tests, from dropping samples in the bottom quartile of SO_2 pollution distribution to additionally controlling for pre-regulation industrial compositions, all show consistent results aligning with the baseline.

The main findings of this paper are the following. Prefectures with 10% higher pollution see their high-skilled population decline by 1% between 2000 and 2010 as migration costs fall. The migration reform enables high-skilled workers to sort more easily across space based on their preferences, in particular air quality differences. On the other hand, a 10% higher local pollution, implying more stringent regulation under APPCL, causes local secondary industry employment to decline by 1.6%. While the secondary sector employs mostly low-skilled workers, this reduction in employment however does not translate into reduction in its population. As informed by the model, it is highly likely that migration costs for low-skilled workers remain relatively high even after the migration reform. As the high-skilled workers account for less than 10% of the overall population for an average prefecture, declining high-skilled population in these prefectures does not lead to substantially different city sizes.

To further investigate the regulatory impact on employment, I turn to industrial firm survey data which allows for detailed analysis by firm ownership type and SO_2 polluting intensity. Estimation results show that the employment reduction effect of APPCL concentrates in the heavy SO_2-polluting industries. Meanwhile, employment in local non-polluting industries increases slightly, but not enough to offset the overall negative impact on the secondary sector. A further break down by ownership shows that the state-owned sector bears the brunt of regulatory impacts. This heterogeneity by ownership shows non-uniform regulatory stringency across the distribution of firm size.

Findings in this paper connect to the literature on the impact of environmental regulations on local economies. This paper provides developing country evidence, where there is a growing demand for environmental regulations under different political contexts. Previous studies on APPCL have focused on its adverse effects on firms' export production (e.g., Hering and Poncet, 2014; Shi and Xu, 2018), whereas here I consider its impact on local employment. In comparison to environmental regulation effects in developed countries, the adverse labor market shock shown in this paper is smaller in magnitude (e.g., Becker and Henderson, 2000; Greenstone, 2002). This is perhaps related to various contextual differences in developing countries and especially quality of institution, as discussed in Greenstone and Hanna (2014). In China during this period of study, APPCL implementation did not alter prefectures' relative ranking in pollution severity. Whereas for example, the impact on air quality of the Clean Air Act in the US is immediate and substantial (Auffhammer et al., 2009; Henderson, 1996). This ineffectiveness of environmental regulations in developing countries partially explains the smaller adverse impact estimated in this paper.

With the unique setup of China's mobility reform, I am able to consider the overall impact of regulatory shocks on local economies when migration costs fall[2]. The lack of reallocation across space from low-skilled workers whose labor market outcomes are adversely affected by environmental regulations, implies that one or more of the following are true: they still face high mobility costs; they have strong preferences for their hometown; they have low valuation for air quality. Existing findings in the literature lean toward the explanation of high mobility costs (Au and Henderson, 2006b; Tombe and Zhu, 2017). With limited employment reallocation from polluting to non-polluting industries locally, lack of mobility across space further aggravates the adverse labor demand shock stemming from environmental regulations. This contributes to welfare loss additional to the loss of agglomeration productivity benefits as the *hukou* system restricts Chinese city sizes (Au and Henderson, 2006a).

On the other end of the skill distribution, high-skilled workers whose employment situation is largely unaffected by pollution regulations are able to move to where they are better off once migration costs reduce. This is consistent with the existing literature that high-skilled workers value amenity such as air quality, in both

[2]The recent work on this topic in China has focused solely on the amenity channel of how air quality differences affect migration choices after *hukou* reform (e.g., Chen et al., 2017a; Freeman et al., 2017; Zheng et al., 2011).

developed and developing countries alike (Chay and Greenstone, 2003; Chen et al., 2017a; Currie and Neidell, 2005; Glaeser et al., 2001; Heblich et al., 2016; Lin, 2018; Schlenker and Walker, 2016).

The rest of the paper is organized as follows. The next section describes the details of the two policy reforms, APPCL and *hukou*, in light of the existing literature. Section 3 presents a simple theoretical framework to outline the factors and channels at work. Sections 4 discusses the research design and data while Section 5 present estimation results and discussion. Section 6 concludes.

3.2 Background

3.2.1 SO_2 and APPCL

The air pollutant of focus in this paper is sulfur dioxide (SO_2). SO_2 is the major by-product of burning fossil fuels such as coal by power plants and related industrial facilities. High levels of SO_2 in the air is harmful to the human respiratory system. It is also a primary precursor of acid rain that damages agricultural land, forests, buildings, and overall ecosystem. When interacted with other compounds in the atmosphere, it contributes to the formation of haze and particulate matter (US Environmental Protection Agency, 2017a,b). SO_2 pollution in China is a serious issue because around 80% of its electricity is generated by coal on an annual basis prior to 2010 (EIA, 2013).

Given the severity of SO_2 pollution and its adverse health impacts, China's first serious environmental regulation, Air Pollution Prevention and Control Law (APPCL), targets SO_2 reduction and focuses on the usage of coal. Coal mines of high sulfur content are to be shut down and new mines can only be commissioned for low-sulfur coal. Main coal users including coal-fired power plants and factories in heavy SO_2 polluting industries are required to burn coal with low sulfur content, upgrade boilers and kilns, and treat effluent gas. Those with inefficient technology are shut down. Such policy instruments added input and operation costs to related industries. World Bank (2003) shows that high-sulfur coal is 50% cheaper than low-sulfur coal. Upgrading equipment and treating pollutant can also add costs. For example, Becker and Henderson (2000) estimate such increase in average cost is as high as 17% from the Clean Air Act in the US. In addition to the above requirement, emission charges are also levied on extremely heavy polluters, and in 2003 generalized to all discharges of pollutants (Goulder, 2005). The above details of APPCL show that it is reasonable to conceptualize its implementation as an adverse productivity shock to firms.

As part of China's 9th Five Year Plan (1996-2000), APPCL sets out a national air quality standard for all prefectures to achieve. APPCL implementation formally starts in 1998, meaning more polluted prefectures at the time will face more stringent regulations going forward. This allows me to use prefecture's existing level of SO 2 pollution as a reasonable proxy for the regulation intensity it will face, similar to that in Shi and Xu (2018). While the above regulation details apply to all prefectures across China, the State Council further designates 175 prefectures known as the "Two Control Zones (TCZ's)"[3] for direct supervision. These TCZ prefectures contributed to around 60% of SO_2 emission in China at the time (Hao et al., 2001).

3.2.2 SO_2 emission and pollution trends

While the efficacy of APPCL in improving SO_2 pollution is beyond the scope of this paper[4], a few facts about its trends following APPCL implementation are important for identifying its effects on the local economy. Firstly, national SO_2 emissions in China decreases following APPCL implementation, although its time trend fluctuates a lot. In figure 3.1, yearly time trends of SO_2 emission levels for China, US, and EU 33 are shown with emission numbers normalized to their 1990 values within each group of countries. The two brief declining periods in China's SO_2 emissions starting in 1998 and 2007 both follow the timing of APPCL regulations: 1998 is the beginning of APPCL implementation, and 2007 is two years after the State Council announces amendments to APPCL[5]. To highlight the contextual

[3]The "two" in TCZ means high in SO_2 pollution or acid rain frequency. These TCZ prefectures are identified based on their SO_2 emission records, annual and daily average ambient SO_2 concentrations at the time.

[4]For multi-disciplinary evidence, please refer to Li et al. (2010); Ma and Takeuchi (2017); Schreifels et al. (2012); Tan et al. (2017); Xu (2011)

[5]The detailed amendments are the following. All existing regulatory terms were strengthened. Most emission reduction targets termed as "expected" in the 1998 APPCL amendments became "compulsory", and their compliance linked directly to local political officers' promotion perspective (China Central Committee, 2006). SO_2 emission targets were set more specific for each province with emphasis on power sector and manufacturing firms in heavily polluting industries. Pollution levy tripled to 1.26 RMB/kg for SO_2 (Cao et al., 2009). Additionally, all coal-fired

differences in emission levels and regulations between emerging economies and developed countries, I compare China's national SO_2 time trend to those of the US and the EU 33 countries in figure 3.1. The latter two are both on steady decline between 1990 and 2010, when they are subject to respective air pollution regulations[6]. Therefore, it is important to keep in mind the degree of SO_2 pollution and the efficacy of regulation are different in this context for emerging economies and may not be directly comparable to evidence based in developed countries.

To further explore the effect of APPCL on SO_2 pollution levels, I turn to time trends at the prefecture level. Figures 3.2 and 3.3 take a closer look at prefecture ambient SO_2 levels between 1998 and 2010 by their Two Control Zone status. Figure 3.2 shows kernel density plot of ambient SO_2 levels by TCZ status in 1998 and 2010. The observation is that all prefectures become more polluted over this period, and the relatively more polluted in 1998 remain so in 2010. To closer examine any differences in trends by TCZ status, figure 3.3 is a convergence plot of SO_2 change between 1998 and 2010 based on their initial pollution levels. Both groups show greater decline in the change of SO_2 levels for prefectures that were more polluted within the groups. In particular, TCZ prefectures show small but significant decline in their change of SO_2 levels, potentially due to the regulation.

Lastly, figure 3.4 shows the relative decline in the change of SO_2 levels in TCZ prefectures shown in figure 3.3 is not enough to alter the ranking of prefectures based on their relative pollution levels in both 1998 and 2010. The rate at which almost all prefectures become more polluted is not too different to change how polluted they are relative to each other. Therefore throughout this paper, I focus on 1998 SO_2 pollution levels, rather than the change between 1998 and 2010, for that a prefecture's relative ranking of pollution levels is what matters in APPCL implementation stringency or people's migration choices.

power plants are required to install the Flue-Gas Desulfurization (FGD) system – SO_2-scrubbing equipment – and keep its operating rate above 90% of the time to avoid potential fines US Environmental Protection Agency and China State Environmental Protection Administration (2007).

[6]The US and EU 33 countries have reduced over 60% SO_2 emission between 1990 and 2010.

3.2.3 The *Hukou* system and its impact on labor mobility

Another key piece of policy change in around 1998 was China's reform of its *hukou* restrictions that relaxed internal labor mobility. The *hukou* system works similarly to an internal passport system. When a Chinese citizen is born, he or she inherits a local citizenship tied to his or her mother's *hukou* place of residence (as specific as to neighborhood subdistrict, *jiedao*). This citizenship entitles the individual to either a rural or an urban status, which is a direct inheritance of whether or not one's ancestors are "peasants". This dichotomy of legal status, along with locational differentials, determine one's rights to housing, schooling, job opportunities, health care, and even "grain ratios" prior to 1990s (Chan, 1994).

As one's legal rights are so strictly tied with his or her *hukou* location and status, China's internal migration is strongly restricted through the *hukou* system. Prior to 2000, changing one's *hukou* status or to move across locations legally is very difficult. The rare channels are through job assignments after university education, or job relocation within the government and large state owned enterprises, or marriage, etc. Each means comes with high friction and monetary cost. People could otherwise move illegally as "unregistered" migrant or legally but temporarily, and in both cases they are entitled to zero or little local public provision at the receiving place (Au and Henderson, 2006b). Therefore, such inflexibility hugely disincentives anyone from moving and local labor supply is largely inelastic.

The gradual *hukou* reform beginning in around 2000 substantially relaxes (but does not eliminate) *hukou* restrictions, and internal migration grows in scale. This relaxation allows people to relocate with legal rights to local employment and housing. However, access to other social benefits varies by skill. Those with university degree and above can relocate their *hukou* with relative ease. This means full access to the local social benefits. However, access to social benefits is limited for low-skilled workers, e.g., health care, schooling for their children, etc. (Chan, 2013). There is also spatial heterogeneity that prefectures with higher political hierarchy such as Beijing and Shanghai have greater restrictions even after *hukou* reform.

Hence in this paper, *hukou* reform is treated as a skill biased reduction in migration cost from 1998 to 2010. The amenity channel through which the variation in air quality is then more salient for high skilled workers because of their lower mobility cost and higher valuation of amenity. Whereas for low-skilled workers whose valuation of amenity is potentially lower, *hukou* reform makes it possible to identify whether they sort across places when their employment outlook is negatively affected by APPCL.

3.2.4 Reform of state-owned enterprises

Another key piece of background information is on the state-owned enterprises (SOEs). As China begins its market reform in the late 1970s, reform over SOEs also starts and its timing briefly overlaps APPCL implementation. Background information on the state-owned sector is hence important for understanding the heterogeneous effects of APPCL implementation by ownership types, if any.

The reform of SOEs began soon after China's market reform, but it was not until the mid-1990's that serious reinforcement actions had taken place. Measures such as financial support, layoffs, buy-out and action against corporate insolvency were implemented to help SOEs become profitable (OECD, 2009). Existing work documents large scale of privatization of SOEs across all firm sizes and industries between 1997 and 2001 (Fang, 2002; Jefferson and Su, 2006). Rawski (2002) estimates 6 out of 44 million state industrial workers loses their jobs during this time.

During my sample period, data shows that the privatization of state owned firms continued throughout 1998 to 2007. State's share of firm count reduced from 35% to 4% while private's grew from 29% to 77% between 1998 and 2007. Meanwhile, state's share of value-added shrank from 47% to 20% and private's increased from 28% to 59%. The reversing trends of their economic importance are crucial for interpreting estimation results.

3.3 Theoretical framework

The simple theoretical model presented here aims to formalize the relationship between prefecture-level ambient SO_2 pollution and local growth prospects in the context of China's policy changes on environmental regulation and labor mobility. It follows largely the works of Moretti (2011) and Allcott and Keniston (2014). There are two prefectures, $c \in \{a, b\}$. Workers have skill types either high or low $k \in \{h, l\}$. Each prefecture has two labor-intensive production sectors, $j \in \{m, s\}$. m represents the non-SO_2 polluting sector and s for SO_2-polluting sector. Both sectors employ low-skilled workers (at skill level l). Additionally, there is a skill-intensive sector f, employing workers of skill level h. All three goods are internationally traded and have exogenously determined prices P_m, P_s, and $P_f = 1$.

There are two time periods. $t = 1$ is the initial state, when both prefectures are the same in every aspect except for their air quality, A_{c1}. Suppose that prefecture a has worse ambient SO_2 pollution, so $A_{a1} < A_{b1}$. This difference is assumed to be exogenous here and will be instrumented for in the empirical exercise. There is no regulation over air pollution in $t = 1$. Labor mobility is greatly restricted under *hukou* regulation so in this period the spatial equilibrium is curtailed by extremely high skill biased migration costs.

In the second time period, $t = 2$, regulation over *hukou* is relaxed and migration cost is lowered for everyone, although its skill biased property remains. Meanwhile, environmental regulation is implemented. The SO_2-polluting sector s faces higher costs, captured by decrease in productivity. In particular, the SO_2-polluting sector in prefecture a is regulated more stringently as it has higher initial ambient SO_2 level. Prefecture-level productivity is hence lowered more in a than b.

3.3.1 Production

Demand for low-skilled workers

Each sector $j \in \{m, s\}$ has a composite firm employing N^l_{jc} low-skilled workers and earning revenue $R^l_{jc} = X^l_{jc}(N^l_{jc})^{1-\gamma}$, where $\gamma \in (0, 1)$. X^l_{jc} is revenue productivity[7], and change in X^l_{sc} will be used to capture cost of APPCL regulation in the polluting sector. Low-skilled workers have homogeneous productivity and hence wage level is equalized across the two sectors m, s within a prefecture. Firms in a given labor-intensive sector have profit equation $\Pi^l_{jc} = R^l_{jc} - W^l_c N^l_{jc}$. Assuming

[7]Revenue productivity by definition is $X^l_{jc} = P_j \Omega^l_{jc}$, price of the product times and physical productivity.

that workers are paid their marginal productivity, firm profit maximization gives the wage equation: $W^l_c = (1-\gamma)X^l_{jc}(N^l_{jc})^{-\gamma}$. Rearranging it obtains demand of low-skilled workers by a typical firm in j and prefecture c: $N^l_{jc} = (\frac{(1-\gamma)X^l_{jc}}{W^l_c})^{\frac{1}{\gamma}}$. Aggregating across m, s to obtain prefecture-level labor demand $N^l_c = N^l_{mc} + N^l_{sc}$, which is in detail:

$$N^l_c = (\frac{(1-\gamma)}{W^l_c})^{\frac{1}{\gamma}}(X^l_{mc}{}^{\frac{1}{\gamma}} + X^l_{sc}{}^{\frac{1}{\gamma}})$$

Taking logs (denoted with corresponding lower cases):

$$n^l_c = \frac{1}{\gamma}\ln(1-\gamma) - \frac{1}{\gamma}w^l_c + \widetilde{x}_c, \text{ where } \widetilde{x}_c \equiv \ln(X^l_{mc}{}^{\frac{1}{\gamma}} + X^l_{sc}{}^{\frac{1}{\gamma}})$$

Demand for low-skilled workers in prefecture a relative to prefecture b is then:

$$n^l_a - n^l_b = -\frac{1}{\gamma}(w^l_a - w^l_b) + (\widetilde{x}_a - \widetilde{x}_b) \tag{3.1}$$

Relative demand of low-skilled workers in prefecture a will be higher if its relative wage for low-skilled workers is lower and their relative productivity is higher.

Demand for high-skilled workers

For simplicity, the skill-intensive sector f is similar to m, s except for the skill type. The composite firm employs N^h_{fc} high-skilled workers and earning revenue $R^h_{fc} = X^h_{fc}(N^h_{fc})^{1-\gamma}$. Therefore, the relative labor demand for high-skilled workers in prefecture a is:

$$n^h_a - n^h_b = -\frac{1}{\gamma}(w^h_a - w^h_b) + \frac{1}{\gamma}(x^h_{fa} - x^h_{fb}) \tag{3.2}$$

3.3.2 Housing market

Following Moretti (2011), I assume that the number of housing units in prefecture c equals to the number of workers with housing supply elasticity of g: $p_{hc} = g\ln(N^l_c + N^h_c) + g_0$. $1/g$ is thus the price elasticity of housing supply. Although the local labor markets are segregated by skill, the housing market is not. The housing price difference between a and b is:

$$p_{ha} - p_{hb} = g \ln(\frac{N_a^l + N_a^h}{N_b^l + N_b^h}) = g(n_a - n_b) \tag{3.3}$$

The implication is straightforward that the relative housing price of a prefecture will be higher as more people move in.

3.3.3 Consumers and workers

Individuals decide where to live and how much to consume by supplying 1 unit of labor. A representative individual consumes $C_{im}, C_{is}, C_{if}, C_{ih}$ units of labor-intensive polluting, non-polluting, and skill-intensive goods, as well as housing. Their utility also depends on amenity, A_c, for which in this paper I focus on variation of local SO_2 level. I also incorporate the idiosyncratic taste term E_{ic} from Moretti (2011) to have imperfect labor mobility throughout. Both A_c and E_{ic} are skill-specific. Specifically, high-skilled workers value air quality more than low-skilled workers.

In order to incorporate migration costs that realistically describe the effect of *hukou*, I introduce a wage multiplier term D_c^k to consumer budget constraint. $D_c^k \geq 1$ represents the difference in local social benefits one receives at their *hukou* prefecture c relative to the non-*hukou* city. This is similar to that in Tombe and Zhu (2017) but instead of a flow measure I model it as sunk cost. In period 1, D_c^k decreases to 1 if an individual migrates to their non-*hukou* prefecture and forgo all the social benefits they get at their *hukou* prefecture. This term is again skill-biased to capture the fact that migration costs imposed by *hukou* have always been lower for high-skilled workers (i.e., $D_{ct}^l > D_{ct}^h$). In $t = 2$, *hukou* reform reduces migration costs for both skill types so that $D_{c2}^k < D_{c1}^k$.

Individuals in each skill type maximize their Cobb-Douglas utility:

$$U_{ic}^k = C_{ih}^{\alpha} C_{is}^{\beta} C_{im}^{\lambda} C_{if}^{1-\alpha-\beta-\lambda} A_c^k E_{ic}^k$$

subject to $W_c^k D_c^k - P_m C_{im} - P_s C_{is} - P_f C_{if} - P_{hc} C_{ih} \geq 0$, with $\alpha, \beta, \lambda, 1-\alpha-\beta \in (0, 1)$.

The indirect utility is then $u_{ic}^k = w_c^k + d_c^k - \alpha p_{hc} - \beta p_s - \lambda p_m + a_c^k + e_{ic}^k$ plus some constant. Individual with *hukou* in prefecture a is indifferent between prefectures a and b if $u^k_{ia} = u^k_{ib}$, which is equivalent to:

$$w^k_a - w^k_b = \alpha(p_{ha} - p_{hb}) - (a^k_a - a^k_b) - (e^k_{ia} - e^k_{ib}) - d^k_a$$

By assuming that e^k is distributed type I extreme value with scale parameter $(s^k)^2$ where $s^k \in (0, \infty)$, we have $e^k_{ia} - e^k_{ib} = -s^k(n^k_a - n^k_b)$. As defined in Moretti (2011), s characterizes the degree of labor mobility based on preference. Larger s means that workers have stronger attachment to their current location and are therefore less likely to migrate. Further combining with equation 3.3, we obtain the relative labor supply in prefecture a:

$$n^k_a - n^k_b = \frac{1}{s^k}[(w^k_a - w^k_b) - \alpha g(n_a - n_b) + (a^k_a - a^k_b) + d^k_a] \tag{3.4}$$

For both skill types, the relative labor supply in prefecture a will be higher if its relative wage is higher, housing cost is lower, and air quality is better. Additionally, better social benefits that workers receive in their *hukou* prefecture d^k_c is the opportunity cost to migrate. Lastly, these effects are scaled by skill-specific preference s^k.

3.3.4 Equilibrium conditions

Spatial equilibrium under *hukou* restriction

In the labor market equilibrium, there is equalization within skill type between the prefecture-level labor demand and supply. Combining equations 3.1, 3.2 and 3.4, we obtain population differences between a and b by skill type in period 1:

$$\text{low-skilled: } (n^l_a - n^l_b)_t = \frac{1}{\gamma + s^l}[\gamma(\widetilde{x}_a - \widetilde{x}_b)_t - \alpha g(n_a - n_b)_t + (a^l_a - a^l_b)_t + d^l_{at}] \tag{3.5}$$

$$\text{high-skilled: } (n^h_a - n^h_b)_t = \frac{1}{\gamma + s^h}[(x^h_{fa} - x^h_{fb}) - \alpha g(n_a - n_b)_t + (a^h_a - a^h_b)_t + d^h_{at}] \tag{3.6}$$

As standard Roback models would suggest, the population difference for both skill types is determined by differences in city-level productivity (positively), housing cost (negatively), and amenity (positively).

Furthermore, equations 3.5 and 3.6 above highlight two key features. Firstly, high-skilled workers value air quality more than low-skilled workers by assumption. Thus the same difference in air quality will result in stronger effects on population differences in high-skilled workers than low-skilled workers, all else being equal. Namely, $(a_a^h - a_b^h)_t > (a_a^l - a_b^l)_t$ since prefecture a is assumed to have higher SO_2 pollution to begin with. Secondly and uniquely to the Chinese context, these standard effects driving population differences across prefectures are muted under the *hukou* system due to the large discrepancy in social benefits between *hukou* and non-*hukou* locations that an individual is entitled to, d_{ct}^k. The scale of this migration cost keeps the actual relative population sizes of both skills in prefecture a artificially higher than they otherwise would be if $d_{ct}^k = 0$.

Employment in a given labor-intensive sector

Lastly, we are also interested in employment in different sectors. From firm profit maximizing FOC, relative sectoral labor demand is $n_{ja}^l - n_{jb}^l = -\frac{1}{\gamma}(w_a^l - w_b^l) + \frac{1}{\gamma}(x_{ja}^l - x_{jb}^l)$. Substituting in relative equilibrium wage difference for low-skilled workers, we can re-write it as:

$$n_{ja}^l - n_{jb}^l = \frac{1}{\gamma}(x_{ja}^l - x_{jb}^l) - \frac{1}{\gamma + s^l}[s^l(\widetilde{x}_a - \widetilde{x}_b) + \alpha g(n_a - n_b) - (a_a^l - a_b^l) - d_a^l] \tag{3.7}$$

Employment in a given sector j in prefecture a relative to b is therefore determined by two main parts. Firstly, its within prefecture sectoral productivity affects it positively. The second term of the above equation is comprised of prefecture-level characteristics. The relative strengths of these two forces determine the signing of relative employment in a given labor-intensive industry.

APPCL implementation, air quality, and *hukou* reform

Going from $t = 1$ to $t = 2$, two things happen. Firstly, the implementation of APPCL adds costs to sector s, reducing its productivity (Becker and Henderson, 2000; Greenstone, 2002; Henderson, 1996; Shapiro and Walker, 2017). My analysis based on a limited sample of firm panel data in the first part of Appendix also confirms this productivity reducing impact of APPCL. In addition, this effect is dis-proportionally stronger in prefecture a than in prefecture b. This is because prefecture a has higher initial ambient SO_2 level and under more stringent regulation. As a result, $(\widetilde{x}_a - \widetilde{x}_b)_1 = 0 > (\widetilde{x}_a - \widetilde{x}_b)_2$.

While existing literature on the US and EU countries show that air pollution regulations could largely improve air quality, it is less straightforward in the case of China with APPCL implementation as discussed in details in section 3.2.2. Recall that figure 3.9 shows that the most polluted group of prefectures in 1998 experience a small 2% decline in ambient SO_2 in the decade following APPCL implementation, while the rest of prefectures have no significant convergence trends. Furthermore, figure 3.10 shows that the relative ranking of prefectures in terms of air quality hardly changed during this time. In other words, the more polluted prefectures in 1998 remained relatively more polluted in 2010. When air quality enters people's migration choices, a prefecture's relative rather than absolute pollution level is what matters. This translates into $(a^k_a - a^k_b)_1 = (a^k_a - a^k_b)_2$.

Lastly, *hukou* reform relaxes mobility for both skill groups, causing the gap of social benefits one receives between two locations to narrow, $d^k_{c2} < d^k_{c1}$. The gap between high- and low-skilled workers persists, i.e., $d^h_{ct} < d^l_{ct}$. These assumptions are consistent with the actual reform policies documented in section 3.2.3. As a result, *hukou* reform allows the relative differences in productivity and amenity channels to manifest more in $t = 2$. However, census data show that a decade after *hukou* reform, the average prefecture level internal migrants share of population is only about 10% in 2010 (table 3.14).

Relative changes over time

The discussion above allows us to now write out relative population in prefecture a between the two time periods for low-skilled workers:

$$\Delta n^l_a - \Delta n^l_b = \frac{1}{s^l + \gamma}[\gamma\Delta(\widetilde{x}_a - \widetilde{x}_b) - \alpha g \Delta(n_a - n_b) + \Delta d^l_a], \tag{3.8}$$

for high-skilled workers:

$$\Delta n_a^h - \Delta n_b^h = \frac{1}{s^h + \gamma}[-\alpha g \Delta(n_a - n_b) + \Delta d_a^h], \tag{3.9}$$

and lastly for low-skilled workers in non-polluting sector:

$$\Delta n_{ma}^l - \Delta n_{mb}^l = \frac{1}{s^l + \gamma}[-s^l \Delta(\widetilde{x}_a - \widetilde{x}_b) - \alpha g \Delta(n_a - n_b) + \Delta d_a^l]. \tag{3.10}$$

The relative change that applies to populations of both skill types is the reduction of migration cost by *hukou* reform, $\Delta d_a^k < 0$. It allows workers to sort between prefectures based on the initial differences, which is sharply curtailed due to high initial migration cost. The effects of productivity and amenity differences between the two prefectures manifest stronger, particularly the difference in air quality for high-skilled workers because of $a_c^h > a_c^l$.

3.3.5 Effects of air pollution regulations in light of *hukou* relaxation

I now consider predicted effects of APPCL regulation during the period of *hukou* reform – that is, a decrease in prefecture a's polluting sector productivity, when migration costs also decline.

Effects on low-skilled workers

As discussed in section 3.3.4, the implementation of APPCL means that $\Delta(\widetilde{x}_a - \widetilde{x}_b) < 0$ and has a direct negative impact on the productivity of polluting sector, which reduces demand for low-skilled workers in this sector (equation 3.1). Air pollution regulation decreases local employment of low-skilled workers in the polluting sector.

How the negative productivity effect of APPCL on polluting sector translates to changes in local population of low-skilled workers further depends on local housing prices and migration costs (equation 3.8). The signing of housing price change is a direct result of overall population change, which in turn is determined by changes in the populations of low- and high-skilled workers and more importantly their respective share.

The next component is the decrease in migration costs as a result of *hukou* reform. $\Delta d_a^l < 0$ means that potential migration in $t = 1$ curtailed by high migration costs then, if any, will partially manifest through reduction of migration costs in $t = 2$. Lastly, these three terms above are scaled by s^l and γ.

Equation 3.10 complements the picture. Part of the workers whose jobs are affected by APPCL will adjust to the non-polluting sector.

In summary, APPCL implementation reduces polluting sector employment in prefecture a. Its impact on local population of low-skilled workers is moderated by potentially lower housing price and sector adjustment to the non-polluting sector, but also intensified by lower migration costs. Its precise signing will be informed by the empirical work.

Effects on high-skilled workers

Local employment prospects for high-skilled workers are not affected by air pollution regulation by assumption. As explained in section 3.3.4, APPCL did not improve the relative ranking of air quality across prefectures by $t = 2$. Therefore, changes in population of high-skilled workers come through the decrease in migration costs, and moderated by changes in housing prices.

Equations 3.9 and 3.6 jointly imply that reduction in migration costs will decrease high-skilled population in prefecture a, mainly driven by its worse air quality. *Hukou* reform allows this preference to manifest in $t = 2$.

Effect on overall population

The effect on overall population depends on how the population of high- and low-skilled workers are affected, as well as their respective shares. It is also relevant in the interpretation of results because local housing price is a function of its population. I revisit this matter in the results section.

3.3.6 Two- to many-prefectures

The discussion so far is based on the two-prefecture scenario, with two otherwise identical prefectures differ in air pollution levels. There are 286 Han Chinese prefectures in total. Hence to generalize the effect of air pollution regulation in light of mobility relaxation, I consider the expectations of prefectures with high versus low pollution levels. Let $A_c \in \{1, 0\}$ denote prefecture c's initial SO_2 pollution levels, with 1 for high pollution level and 0 for low. It captures the only difference between prefectures a and b, which is a proxy for APPCL regulation stringency and a direct measure of air quality. Let Y_c denote outcomes of interest at the prefecture level (e.g., population of high-skilled workers) . ΔY_c is then change in the outcome between $t = 1$ and $t = 2$, such as $\Delta n_a^k - \Delta n_b^k$. The outcome variables in 3.8 and 3.9 in the many prefectures case are therefore generalized to the difference in average change in outcomes between the high and low air pollution groups of prefectures.

$$\mathbb{E}[\Delta Y_c | A_c = 1] - \mathbb{E}[\Delta Y_c | A_c = 0] \tag{3.11}$$

Extending assumptions for equations 3.8 to 3.10, equation 3.11 (i.e., observed difference in outcomes) gives the average treatment effect of air pollution regulation if the group of low air pollution prefectures are valid counterfactuals for the group of high air pollution.

However in reality, these assumptions are easily violated. Equation 3.11 in fact can be written as: $\mathbb{E}[\Delta Y_c | A_c = 1] - \mathbb{E}[\Delta Y_c | A_c = 0] = \mathbb{E}[\Delta Y_{1c} | A_c = 1] - \mathbb{E}[\Delta Y_{0c} | A_c = 1] + \mathbb{E}[\Delta Y_{0c} | A_c = 1] - \mathbb{E}[\Delta Y_{0c} | A_c = 0]$, where $\mathbb{E}[\Delta Y_{0c} | A_c = 1] - \mathbb{E}[\Delta Y_{0c} | A_c = 0]$ is the selection bias that would arise if the group of high pollution cities is directly compared with the group of low pollution prefectures.

In the following section, I discuss my empirical strategy to tackle this challenge in detail.

3.4 Research design and data

To illustrate the empirical challenges of estimating equation 3.11, consider the following OLS specification depicting prefecture-level impacts of APPCL enforcement during mobility reform:

$$\ln(y_{i,2010}) - \ln(y_{i,2000}) = \xi + \rho \ln(\mathrm{SO}_2)_{i,1998} + \phi Z_i^{-1} + \epsilon_i. \tag{3.12}$$

where i denotes a prefecture. y is the outcome of interest, for example population sizes of high-skilled workers or low-skilled workers. Decadal difference in log of outcome variables, consistent with the setup in equations 3.8 to 3.11, cares for time-invariant prefecture characteristics. SO_2 is the prefecture-level ambient SO_2 in 1998, capturing pollution levels in the first period and serving as a proxy for APPCL regulatory intensity that a prefecture i faces as well as a proxy for its air quality[8]. It is analogous to A_c in equation 3.11 but as a continuous variable.

To best control for observable differences across prefectures, I include a vector of prefecture-level characteristics, Z_i. I use contemporaneous changes where possible, and time invariant measures as best proxies. This includes change in high-skilled population between 1990 and 2000 (prior to APPCL regulation and *hukou* reform), contemporaneous changes in transportation network (rail and road), regional dummies and prefecture political status dummies (provincial city or provincial capital), distances to coast and major rivers, and 1995 share of employment in polluting industries. These variables account for pre-existing and contemporaneous factors that tend to lead to differences in prefecture level productivity. Z also includes climatic characteristics that may affect local air pollution levels: average temperature, wind speed, and precipitation.

While Z_i's allow prefectures of varying pollution levels to be as comparable as possible in terms of observed differences, unobserved characteristics could still pose challenges for identification. Variation in ambient SO_2 across prefectures is far from randomly distributed. For example, places that are more economically active tend to pollute more in aggregate (Chay and Greenstone, 2003), and thus are more likely to face stricter environmental regulation. Prefectures under more stringent APPCL regulation due to their high levels of SO_2 pollution could differ in characteristics unknown to researchers, such as institution quality that influence both pollution level and economic prospects of prefectures.

[8]See sections 3.2.2 and 3.3.4 for discussions on why level rather than log change of SO_2 level is used.

3.4.1 Power plant suitability as IV

To address these endogeneity concerns, my research design uses power plant suitability to instrument for its hosting prefecture's level of SO_2 pollution. The suitability measure is based on the engineering literature and it measures how suitable a given location is for hosting thermal power plants subject to construction cost, safety, and feasibility considerations.

This instrumental variable works well in the Chinese context based on several useful attributes of coal-fired power plants. The first is their paramount role in China's energy generation and coal consumption. Currently, over 70% of China's electricity production is still from coal sources, according to the International Energy Agency[9]. Secondly, around 50% of China's coal consumption is used by coal-fired power plants, in 2000 and 2007 consistently. Meanwhile, industrial production accounts for another 40% and the rest is in domestic use or transportation (Lu et al., 2010). As the main contributor to SO_2 emission, coal-fired power plants are hence the key target for China's SO_2 regulatory policies since late 1990s. Lastly, China has separated its power generation and supply networks in 2002. Electricity is since then produced locally, but distributed centrally by regional offices. Therefore, firms do not have the incentive to locate near thermal power plants for accessing cheap electricity[10].

However, these attributes of thermal power plants are not sufficient as there could still be unobservable factors correlated to actual plant locations and economic prospects of their hosting cities. Actual power plant locations could be driven by political favoritism. It is possible that a coal-fired power plant was strategically placed to favor the local politician for his or her future career or nepotism motivations. These places and their incumbent firms can both be affected by such unobserved factors. Therefore in this paper, I take a step back and consider the selection process of power plant hosting sites. I consult the engineering literature to construct an index of coal-fired power plant suitability as the instrumental variable

[9]Data sources: IEA statistics: China, People's Republic of: Electricity and Heat. Data accessed on August 16, 2017. The percentage of electricity generated by coal in 1990, 2000, 2010, and 2014 are: 71%, 78%, 77%, and 73% respectively.

[10]They may still locate near thermal power plants for indirect reasons like access to coal source or transportation networks. I address this particular concern in section 3.4.2.

for a prefecture's ambient SO_2 level in 1998.

3.4.2 Construction of the IV

Measuring a location's suitability for hosting power plants is both well-researched in the engineering literature (Barda et al., 1990; Choudhary and Shankar, 2012; Pohekar and Ramachandran, 2004) and widely applied in practice (e.g., the state of Wisconsin has its own *Common Power Plant Siting Criteria* set by its Public Service Commission). The literature and practical guidelines generally agree on factors to be considered in the suitability measure such as topography, land use, water bodies, fuel supply, and so on. The transformation of factors to suitability index follows the general form:

$$S_j = \sum w_k f_k(k_j) \tag{3.13}$$

where suitability for hosting thermal power plant at location j is computed as weighted sum of rescaled value of factor k. Both the scaling function f and weights w are factor-specific.

The exact formula I employ comes from an Iranian government commissioned site selection project by Zoej et al. (2005). Table 3.1 lists the detailed weights for each factor and their rescaling categories. For example, elevation of the location has a weight of 0.06. Its exact values are reclassified to four scores, the higher the more suitable. When the location has elevation between 0-1km, it's given a reclassified score of 10; between 1-1.4km it's an 8; 1.4-1.8km gets a 4; above 1.8 is a 0. So the total contribution of elevation to the suitability index of this location is 0.06 times its reclassified value. This suitability index is therefore an aggregate of all these information, stipulating whether a given location is suitable for building coal-fired power plants and how costly it will be from an engineering perspective.

In order to compute this suitability index for China, I collected the finest and publicly available remote sensing data for all factors listed in Table 3.1. Appendix Table 3.17 details information on data sources. Since the suitability index is a weighted aggregate of all layers of satellite data, the spatial resolution of the final product is that of the coarsest layer – $0.08° \times 0.08°$ in this case. In other words, suitability index is computed for every 9×9 km^2 of China.

To assess relationships between power plant suitability, actual location, and pollution, further spatial data is collected. Prefecture ambient SO_2 density data are derived from NASA satellite imageries (details in Table 3.17). They provide better and more consistent coverage than ground measures, while both measures are shown to share consistent trends (Chen et al., 2017a). Actual power plant locations are collected from non-profit coal monitoring website SourceWatch Coal Issues[11]. These are geo-located based on official addresses and checked against Google Map Satellite imageries.

Figure 3.5a maps spatial distribution of the suitability index across Han China in blue, with gray lines tracing out prefecture boundaries. The darker the blue indicates greater suitability for hosting coal-fired power plants. Additionally, Figure 3.5b maps the distribution of ambient SO_2 density across China, with darker pixel indicating more severe SO_2 pollution in its area. Both parts of Figure 3.5 show the precise location of coal-fired power plants built by 1998. Visually, there is clear spatial overlapping patterns between power plant suitability and their actual locations, as well as the latter and SO_2 pollution.

3.4.3 Identifying assumptions and supporting evidence

The link between this suitability index and local ambient SO_2 density is established through that it predicts coal-fired power plant locations, which are the biggest contributor to SO_2 emission in China throughout the study period. Because the analysis is carried out at the prefecture level, I compute the prefecture-level mean of suitability as the instrument. The rationale is that while the suitability of an individual pixel might be difficult to detect for engineers and planners back in the 90s, prefecture-level comparison in terms of their respective average suitability would have been more readily available.

As the suitability index captures a location's probability, rather than actuality, of

[11] Accessed in June 2017.

hosting coal-fired power plants, it is more likely to meet the exclusion restriction than actual power plant locations. Furthermore, to address the concern over constituting factors of suitability having direct impact on local economy, these factors are time invariant and first differencing should mitigate this concern. Nonetheless, I include all factors of this index linearly in the regression. The channel through which this instrumental variable works is effectively the set of weights and rescaling function from the engineering literature. It is a valid instrument under the assumption that the non-linear functional form of this index is orthogonal to treatment.

In the rest of this section, I formalize the estimation equations and provide supporting evidence for the above assumptions.

First stage linkages

The first stage is formulated as follows:

$$\ln \mathrm{SO}_{2i,1998} = \alpha + \beta \mathrm{Suitability}_i + \gamma \mathrm{X}_i^{-1} + \varepsilon_i, \tag{3.14}$$

where suitability at the prefecture-level is used to predict 1998 variation in ambient SO_2 density level. Everything else follows from equation 3.12. The linear forms of constituting components of suitability index are additionally included in X_i. They are: log average elevation, log number of raw energy fields (gas, oil, and coal), dummy if near volcanoes, air fields, or earthquake spots, and urban area gas supply coverage.

The prefecture-level economic variables are kindly shared by Baum-Snow et al. (2017) from their work. The climatic measures are prefecture-level zonal computations from various NASA projects as documented in Table 3.17. The geographic covariates are constituting factors of suitability index and are hence from the same sources and computed as prefecture-level zonal averages. The only exception is urban area gas supply coverage. While a relatively coarse map is used for its components in suitability computation, its non-geocoded prefecture level measure in the 1999 China Urban Yearbook provides better quality for the regression.

To show that it is indeed through the likelihood of hosting coal-fired power plants that the suitability index predicts local ambient SO_2, tables 3.2 to 3.5 statistically establish the various linkages between power plant hosting, ambient SO_2 levels, suitability, and Two Control Zone status at the prefecture level. Firstly, table 3.2 shows that prefectures hosting thermal power plants by 1998 pollute more, with and without controlling for prefecture economic and climatic characteristics. Prefectures hosting thermal power plants and those with higher generating capacity by 1998 have higher ambient SO_2 in 1998 (columns 1-3), as well as higher extreme NO_2 levels in 2005[12] (columns 4-6). NO_2 is another major pollutant from coal-fired power generation. Results of similar patterns between SO_2 and NO_2 further help show that the variation in ambient pollution comes from hosting thermal power plants.

Next, Table 3.3 establishes the link between suitability index and thermal power plant hosting status. Columns 1-4 show that the higher the mean or top 25% suitability of a prefecture, the more likely it would have been hosting a thermal power plant by 1998, as well as one with higher generating capacity[13]. Columns 5 and 6 show a lack of such relationship in prefectures that started to host thermal power plants between 1998 and 2010. By 2010, 204 out of 286 prefectures in the sample are hosting power plants. This lack of association is partially because the suitability index in use is computed with information collected prior to 2000. New information related to power plants' suitability afterward could affect site selection from then on. Suitability index computed with information prior to 2000 is mainly relevant for power plants built by 1998.

Lastly, Table 3.4 provides support for using 1998 ambient SO_2 levels as valid proxy for regulation stringency. Prefectures with higher ambient SO_2 levels in 1998 are more likely to be listed as TCZ prefectures, which received stricter regulation under APPCL amendments. This relationship remains robust across specifications where different sets of covariates are included.

These linkages are summarized in Table 3.5, where the first-stage results are presented. Both the mean and top quartile of city-level suitability index are strongly predictive of ambient SO_2 in 1998, as well as for NO_2 levels in 2005 as a robustness check.

[12]2005 is the earliest year that NO_2 measures are available with NASA.
[13]I also tested the maximum and second maximum suitability value of a prefecture. The relationships are the same.

2SLS

Next, predicted SO_2 is used to estimate causal effects on the dependent variable:

$$\ln(y_{i,2010}) - \ln(y_{i,2000}) = \eta + \kappa \ln \widehat{\mathrm{SO}}_{2i,1998} + \lambda \mathrm{X}_i^{-1} + u_i \tag{3.15}$$

where everything follows from the OLS setting (equation 3.12) except that the prefecture-level ambient SO_2 is predicted by the first stage (equation 3.14). The outcome variables are population and employment measures from the 2000 and 2010 censuses, as well as employment and firm counts by ownership and pollutant types aggregated from the Annual Medium- and Large-Enterprise Surveys between 1999 and 2007.

From census data, I compute the following main outcome variables: prefecture population, high-skilled population (college degree and above), total employment and employment by sector (primary, secondary[14], and tertiary).

From the firm surveys, I compute industrial employment numbers as well as total number of firms aggregated at the prefecture level. Although the coverage of this dataset emits small firms whose annual sales are less than 5 million RMB (Brandt et al., 2014), its rich details allow me to compute variables for checking heterogeneous effects. I define heavy and light SO_2-emitting firms by their intensity of coal usage according to 1997 Input-Output Table of China. The input-output table covers 106 3-digit secondary industries. I define the top 12 industries in coal usage (in monetary value) as heavy users. Together they consume 70% of coal in China, while each consume 2% and above. Additionally, sources of paid-in capital allow me to distinguish state- and privately owned firms. Thus, heterogeneous effects can be examined for light versus heavy coal users, state versus privately owned firms, and combinations of coal usage and ownership types.

Before turning to the results, it is important to show that variations in the instrument do not correlate to pre-existing growth trends of cities that may themselves affect outcomes of interest. Table 3.6 runs 2SLS on long differences of several population and employment measures between 1982-1990 or 1990-2000. The predicted

[14] Secondary sector is industry plus raw material extraction.

ambient SO_2 levels in 1998 have effectively zero impact on prior trends in prefecture size, high-skilled worker population, or total employment and manufacturing employment[15]. This falsification test further substantiates the validity of this instrumental variable, as prefectures across the instrumented variation in 1998 SO_2 pollution levels do not differ in terms of population, skill composition, and local labor market characteristics predating pollution regulations and *hukou* reform.

3.5 Results

Tables 3.7 and 3.8 firstly presents prefecture results based on census data. Next, table 3.9 focuses on results in the secondary sector for analyzing the direct impact of APPCL on firms. Heterogeneous effects by coal usage and ownership types are presented. Last but not least, tables 3.12 and 3.13 present sensitivity tests to showcase IV validity and robustness of main baseline outcomes.

3.5.1 Population and employment effects

Tables 3.7 and 3.8 show how prefecture population and employment are affected by local SO_2 pollution levels and APPCL. Tables 3.7 firstly examines how changes in employment between 2000 and 2010 by sector are affected by APPCL implementation. As it lowers productivity in high SO_2prefectures, which mainly affects local secondary industry, column 2 shows that indeed there is a large negative employment effect on this sector. While China's industrial sector has been growing rapidly between 2000 and 2010, a 10% higher local SO_2 and hence stricter regulation leads to a 1.6% adverse employment effect on a prefecture's secondary sector. Equivalently, a one standard deviation increase in 1998 log SO_2 level, which is approximately a 114% increase in ambient SO_2 levels (see table 3.14), leads to a reduction in local secondary employment of around 11%. This estimate has the same sign but much smaller magnitude than findings on the US Clean Air Act (Greenstone, 2002; Walker, 2011). Part of this could be linked to regulation enforcement, as discussed in section 3.2.2.

[15]For column 3 only, I drop change in population of college degree and above as it is the dependent variable.

Meanwhile, columns 1 and 3 show that employment in primary and tertiary sectors do not differ across prefectures with different pollution regulatory intensity. In terms of national sectoral composition, primary and tertiary sectors account for 51% and 26% of total employment in 2010, while secondary accounts for 22% (see table 3.14). This composition potentially contribute to the lack of effect on the change of total employment in column 4. Nonetheless, the effect shown on secondary sectors suggests a strong job displacement brought about by APPCL regulation. The next table provides answer to where the displacement goes, if not to other sectors.

Table 3.8 present results of changes in low-, high-skilled, and total population. Column 1 shows a slightly negative but not statistically significant effect for changes in low-skilled population. So far for low-skilled workers in highly regulated prefectures, we see their employment decline in the secondary sector but their population changes little. In light of equation 3.8, this translates to reduction in local productivity being potentially offset by $\alpha g\Delta(n_a - n_b)$ and/or Δd_a^l, and/or being scale down by s^l. We know from the estimation results that overall population does not change, implying little change in local housing price. That leaves the likely case of which *hukou* reform did not substantially lower migration costs for the low-skilled (i.e., $\Delta d_a^l < 0$ but of small magnitude), or that low-skilled workers have very strong attachment to their *hukou* location (i.e., s^l very large), and possibly a combination of both. Existing evidence in the literature, notably from Au and Henderson (2006b) and Tombe and Zhu (2017), leans toward the high migration costs on low-skilled workers stemming from the *hukou* system even after its 2000 reform.

Column 2 of table 3.8 shows that once *hukou* reform happens, a 10% higher ambient SO_2 causes a prefecture's high-skilled population (university degree and above) to decline by around 1%. As informed by equation 3.9, prefectures with higher levels of pollution could not retain high-skilled workers once migration costs decline. This is driven by their stronger preference for air quality being reflected once migration costs go down. This effect is consistent with findings in existing literature that high-skilled workers in developing countries respond sensitively to amenity differences such as air quality (e.g., Chen et al., 2017a). It is largely working through the amenity channel of air quality as opposed to the productivity channel in labor market, because high-skilled workers account for a very small proportion of total employment in industries directly affected by APPCL[16]. Comparing the non-effects in falsification tests (table 3.6) for decades prior to 2000, this sorting of high-skilled population is made possible because of *hukou* reform.

Column 3 shows changes in overall population. Since the high- versus low-skilled composition in an average Chinese prefecture has a ratio of roughly 1:9 (see table 3.14), it is not surprisingly to find no effect statistically on the overall population change.

Panel B of these two tables show OLS estimates for outcomes parallel to their panel above. These estimates show the necessity of using the IV strategy, as OLS estimates would lead to different conclusions in both signing and magnitude. For example, the OLS estimates would suggest no effect of APPCL on secondary employment. This upward bias as discussed in section 3.4 is related to that high pollution prefectures may be more developed prior to APPCL implementation.

3.5.2 APPCL effects on the secondary sector

Next, I turn to results based on the industrial surveys to examine heterogeneous effects by SO_2-polluting intensity and ownership type. Table 3.9 exhibits several interesting patterns. Firstly, column 1 shows changes in total secondary employment. The estimate of -0.10% between 1999 and 2007 on secondary employment change offers a similar story to that from census data. Their magnitudes differ, potentially because industrial surveys leave out small firms in their sampling. This is important to note while interpreting results based on the industrial survey.

Secondly, employment in heavy coal users (and hence SO_2 polluting firms) in highly polluted prefectures declines while light coal users (non-polluting firms) does not. When further breaking down by ownership, state-owned firms are shown to be the ones bearing the regulatory costs. While the state sector is on a general decline nation-wide due to privatization, prefectures with 10% higher existing pollution and facing stricter regulation see employment decrease by almost 5% in high SO_2 polluting, state-owned industries. This is moderated by a slight increase of employment

[16]Based on my calculation from Chinese Household Income Project data, share of high-skilled workers in the secondary industry is less than 5% in around this period of study.

in state-owned light SO_2-polluting firms (3% with marginal statistical significance). On the other hand, no significant patterns is shown for privately owned firms.

These patterns suggest that not all firms are regulated equally under APPCL. While this large adverse impact concentrated on the state-owned firms may seem surprising given vast evidence in the literature showing favoritism toward them in factor markets (e.g., Chen et al., 2017b; Hsieh and Klenow, 2009), existing research on environmental regulations shows no convincing evidence for any ownership advantages by SOE. For example, Wang and Jin Yanhong (2007) show that state- and privately owned polluting firms behave similarly under regulation.

Two plausible explanations come from the enforcement side, rather than differences in firm behaviors by ownership. The first reason relates to limited resources in environmental regulations. Becker and Henderson (2000) document non-uniform enforcement of regulation across firm size in the US under the Clean Air Act. As a result of limited regulatory resources, heavy polluters and hence large firms are usually the targets of regulation whereas small polluters are not monitored as closely. This offers a good explanation for the patterns shown here. Figures 3.6 and 3.7 show kernel density of firm size for state versus privately owned firms in heavy and light SO_2 polluting industries respectively. State owned firms in the heavy SO_2 polluting industries are much larger in size compared to privately owned firms, and this pattern is consistent over time in 1999 and 2007. For non-polluting industries, however, firm size distributions are much more similar across ownership types. Therefore, it is likely that state-owned firms in heavy polluting industries attract greater regulatory attention simply because they are large and conspicuous in polluting activities.

The second explanation relates to the Chinese contexts. There is evidence that the State in fact targets state-owned enterprises (SOEs) under APPCL implementation during this time as China undergoes its national privatization process (section 3.2.4). Heavy polluting SOEs are deemed unproductive by the State and need to be eliminated. For example, a State Council 1999 announcement declares, "SOEs that waste resources, pollute heavily, employ outdated technologies need to be force into bankruptcy and shut down" (China State Council, 1999). Therefore, it is likely that environmental regulations accelerated the downward employment shock on polluting

SOEs during the national privatization.

3.5.3 APPCL efficacy

To further ensure that the adverse employment shock comes from APPCL regulation, I make use of the annual frequency of industrial survey data. As shown in figures 3.1 and discussed in section 3.2.2, China's total SO_2 emission is on the decline between 1998 and 2003, followed by sharp increase from 2003 to 2007 before declining again. Given the coverage of industrial firm survey data between 1999 to 2007, I divide the change in outcomes into two periods: 1999-2003 and 2003-2007 in tables 3.10 and 3.11.

In table 3.10, I repeat employment results from 3.9 in panel A, and show the divided two periods in panels B and C. Comparing changes in employment between 99-03 versus 03-07, it is clear that the negative labor market shock occurs during the first period, when SO_2 pollution levels is kept stable by APPCL. Next, table 3.11 follows table 3.10 with total prefecture firm count (in log) as the dependent variable. Unfortunately, I do not have measures of firm births and exits. Results here again shows that the negative APPCL effect concentrates in state-owned polluting firms (column 4), and in particularly during the first five years of APPCL implementation. It is worth noting that many point estimates here are positive, showing a general increase in stock of firms across the board as the Chinese industrial sector grows rapidly. Therefore, it is more significant that the regulatory costs imposed by APPCL slows down firm growth in the polluting industries.

3.5.4 Robustness checks

In Tables 3.12 and 3.13, I present comprehensive tests to exhibit the robustness of the identification strategy and results.

Table 3.12 checks the validity of the instrumental variable. With all key results laid out across the columns, panel A re-prints the IV baseline estimates for comparison. Panel B removes the set of covariates that are themselves factors in the suitability construction. Results only deviate slightly in the magnitude of point estimates. Their similarity suggests that this instrumental variable works through the weights assigned by engineers for each factor, rather than the factor values.

Panel C uses an alternative instrumental variable where the suitability measure is constructed with a limited subset of factors, printed in bold in Table 3.1. This alternative IV is hence based on factors that are less likely to have economic impacts themselves. For example, distance to small rivers is kept while that to large rivers is dropped. Arriving at close estimates in this panel shows that the most effective parts of the baseline IV are contributed by non-economic factors.

Panel D addresses the concern over whether the prefecture level mean of suitability measure is a sensible choice. Results presented here use the top quartile of suitability measure at the prefecture level as IV. Using the top quartile rather than the mean shows a slightly better first-stage F-stat, while the point estimates are very similar.

On the other hand, table 3.13 explore the sensitivity of estimates across various alternative specifications and treatment variables, with baseline results again printed in panel A. Panel B excludes 25% of the sample that are in the bottom quartile of 1998 SO_2 pollution level. This address the concern that some prefectures are too clean and undeveloped for comparison. Using the top 75% of the sample arrives at the same results, although point estimates are slightly larger in magnitude as expected. Panel C includes the initial level of the outcome variable as covariate. While including the initial level deals with concern over regression toward the mean, it does not change the conclusion. Panel D answers the question that whether or not initial industrial composition affect baseline results. Having included 1995 share of employment in heavy coal-usage industries, results are consistent while point estimates vary slightly.

Panels E and F present estimates based on alternative treatment variables: two control zone status dummy, and SO_2 level in 2010. In Panel E, the TCZ dummy is set to 1 if the prefecture has been listed as the key target prefecture by the State Council in 1998, and 0 otherwise. This is a more direct treatment variable proxying for regulatory cost imposed on relevant industries and prefectures. However its binary nature is unable to capture for as much variation as a continuous variable such as the 1998 SO_2 level. Moreover, the TCZ status is not measuring regulation versus no regulation. Rather, it switches on for being on the State Council's list, potentially facing stricter enforcement and closer monitoring than the rest. Lastly, using 2010 SO_2 levels in panel F reiterates the fact that the spatial variation of ambient SO_2 levels had not changed much between 1998 and 2010, a point made earlier in section 3.4.

3.6 Conclusion

In the past decades, China went through rapid economic growth while its air quality worsened drastically. As per capital income rises, more people may demand for cleaner air. At such a turning point of the environmental Kuznets curve, the government would be under greater pressure to tighten up environmental regulations. This is a common pattern also seen in many fast growing developing countries in the recent decades.

The descriptive time trends in this paper show that China's effort in curbing air pollution began in 1998 had sustained SO_2 pollution at a constant level, but only for about five years. Since 2003, ambient SO_2 pollution levels began to go up. While the effectiveness of APPCL in improving air pollution levels is beyond the scope of this paper, the overall impact of environmental regulation is assessed.

My findings show that China's APPCL caused local manufacturing employment in heavily polluted prefectures to decline. This impact is concentrated on firms in the polluting industries, and particularly state-owned firms that are of larger firm size. Some unemployment is reallocated to firms not affected by APPCL (non-polluting industries). However, the overall net effect is negative. Moreover, low-skilled workers affected by APPCL are not reallocating to other prefectures, implying migration costs could still be high for them. On the other hand, high-skilled workers whose job prospects are unlikely to be affected by APPCL migrate out of highly polluted prefectures as migration costs fall. They value clean air and this loss of human capital has long term implications for highly polluted prefectures.

"Produce and pollution first, clean up later" is a frequent choice made by policy makers. Low efficiency fuels are the cheaper way to generate energy and increase economic output, but the later cost of cleaning up and losing human capital can be substantial. Notably, these costs are in addition to health costs of heavy pollution.

Findings in this paper also shed light on the distributional impacts of environmental regulations and migration reform by skill. Low-skilled workers are the group bearing the regulatory costs. The high migration costs they face further worsens the adverse shock. Migration reform is biased toward those with college degree and above, giving them easier access to places where they are better off.

Lastly, this environmental regulation is not uniformly enforced across firm size and ownership. As a result of limited regulatory resources, larger firms become the main target. There is also evidence to suggest that the State used environmental regulation as a mean to eliminate firms with outdated technology, which is largely SOEs in this case.

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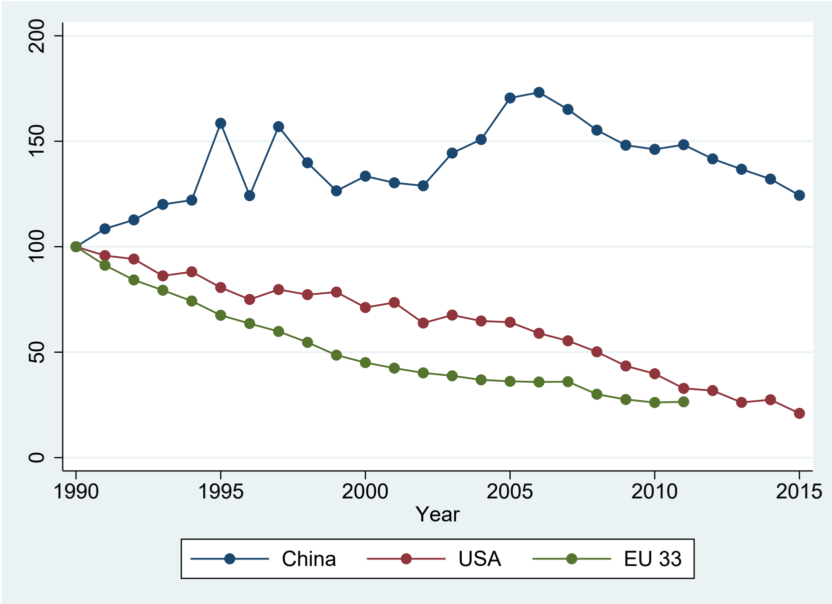
Figures and Tables

Figures

Figure 3.1: SO_2 emission trends, 1990-2015



Data is converted to relative terms with respect to 1990 (=100).
Data sources: China Ministry of Environmental Protection; U.S. Environmental Protection Agency; European Environment Agency. Accessed on August 9, 2017.

Figure 3.2: Kernel density of ambient SO_2 levels in 1998 and 2010 by TCZ prefectures

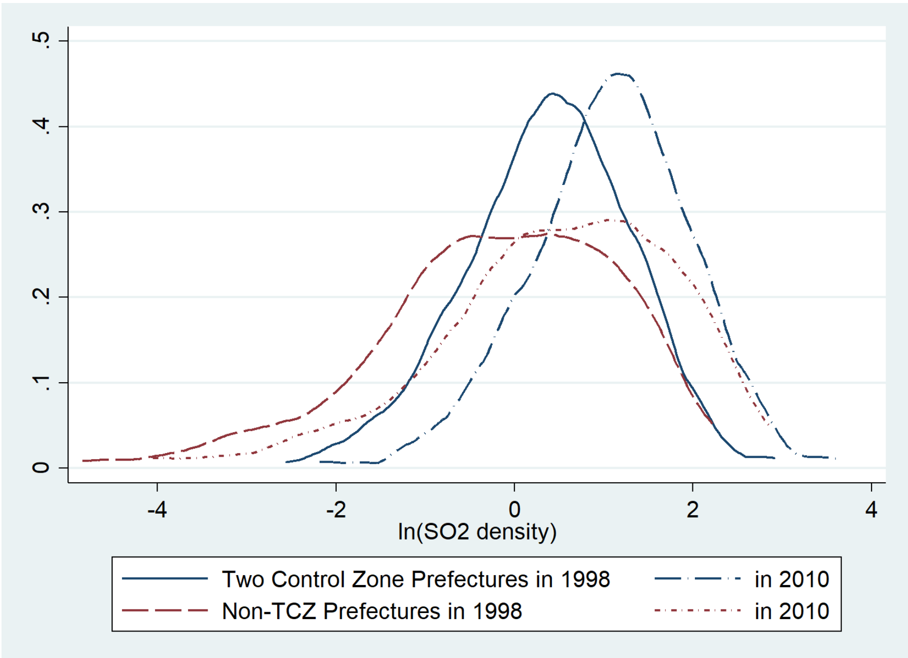


Figure 3.3: Ambient SO_2 change between 1998 and 2010 by TCZ status

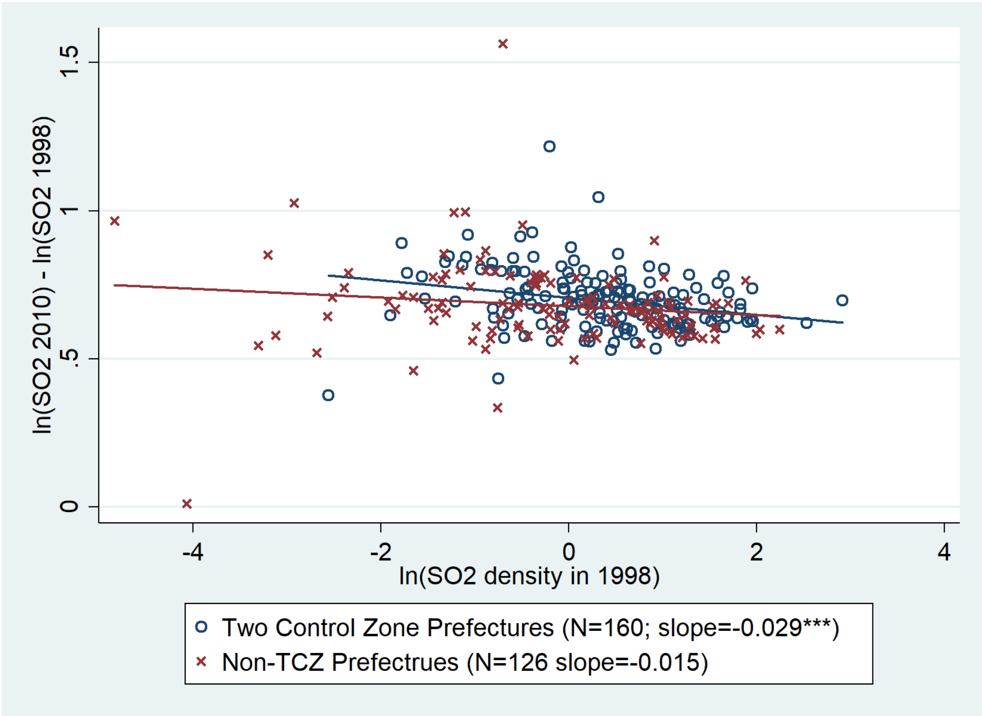


Figure 3.4: Relative ranking of ambient SO_2 levels between 1998 and 2010 by TCZ status

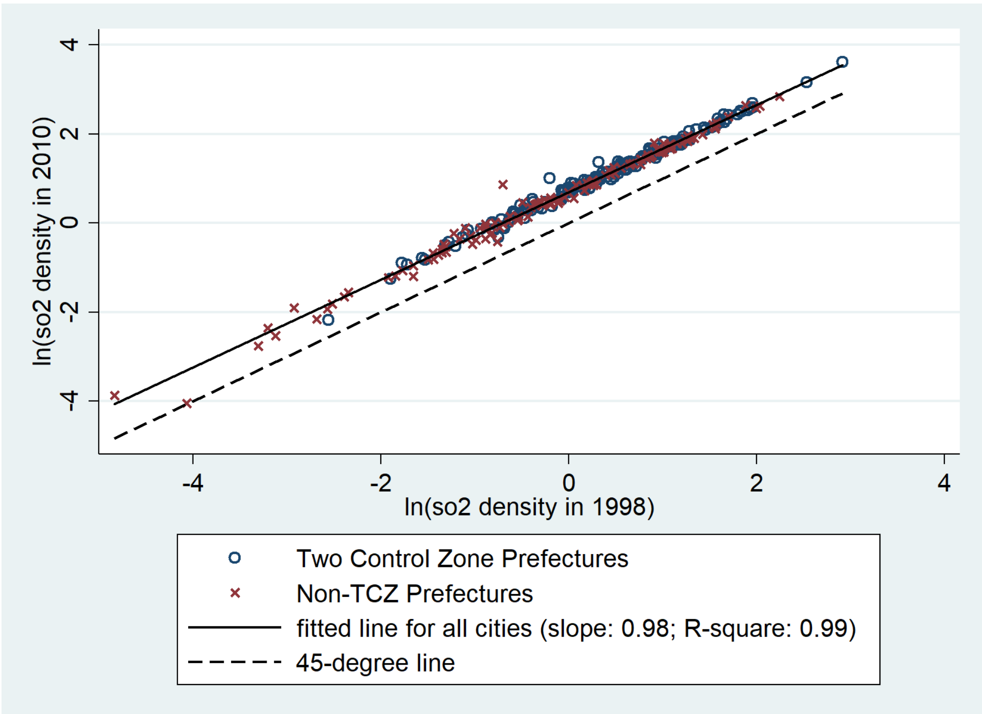
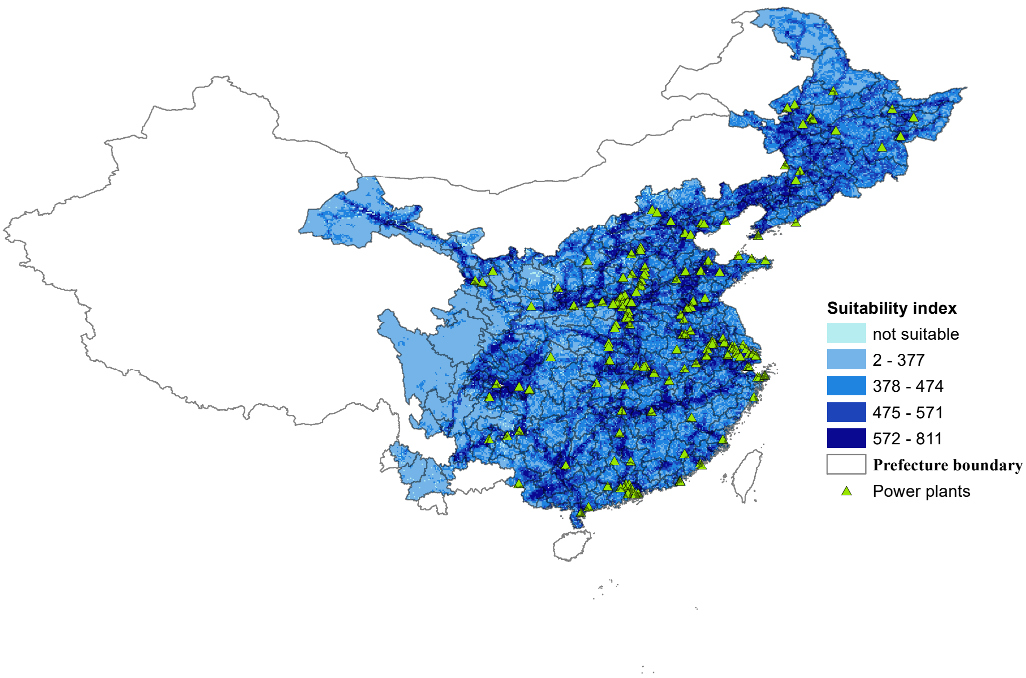
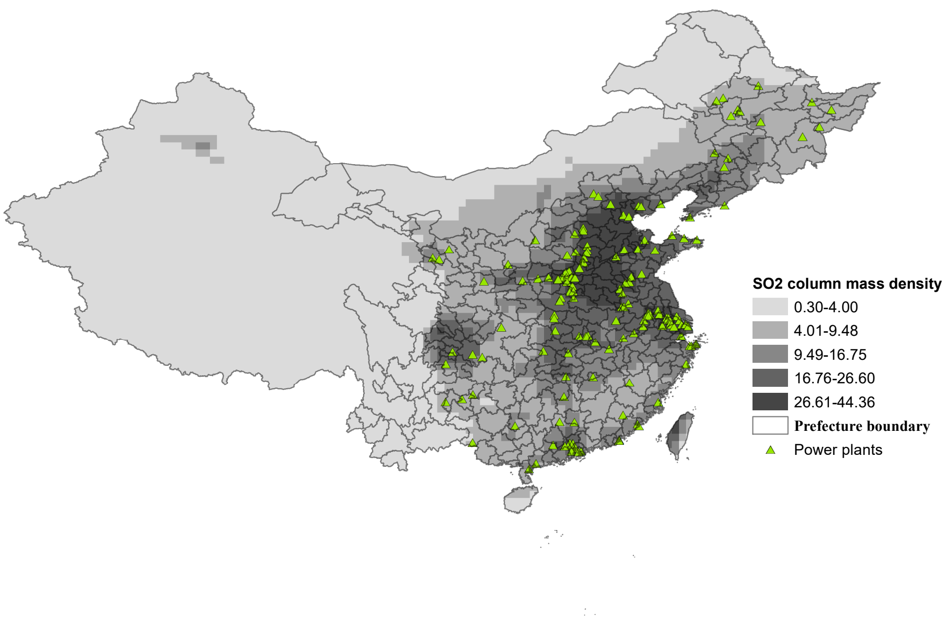


Figure 3.5: Spatial distributions of coal-fired power plants by 1998, suitability index, and ambient SO_2 density



(a) Coal-fired power plant suitability and actual locations for those built by 1998



(b) Ambient SO_2 density and coal-fired power plant locations for those built by 1998

Figure 3.6: Kernel density plot of firm size by ownership types, **heavy** SO_2 polluters

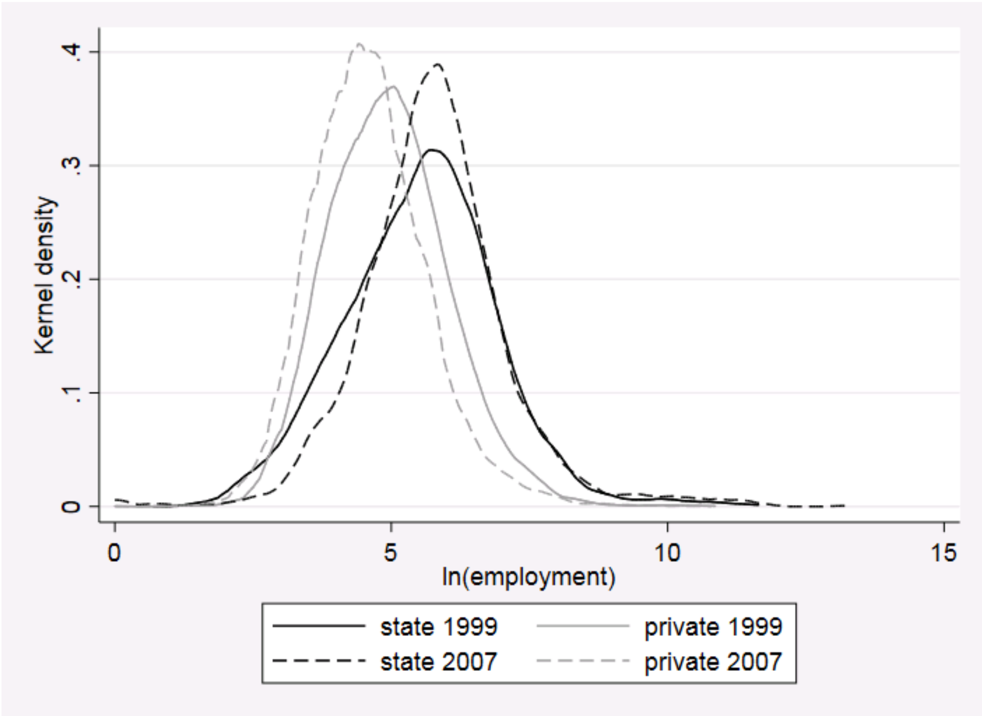
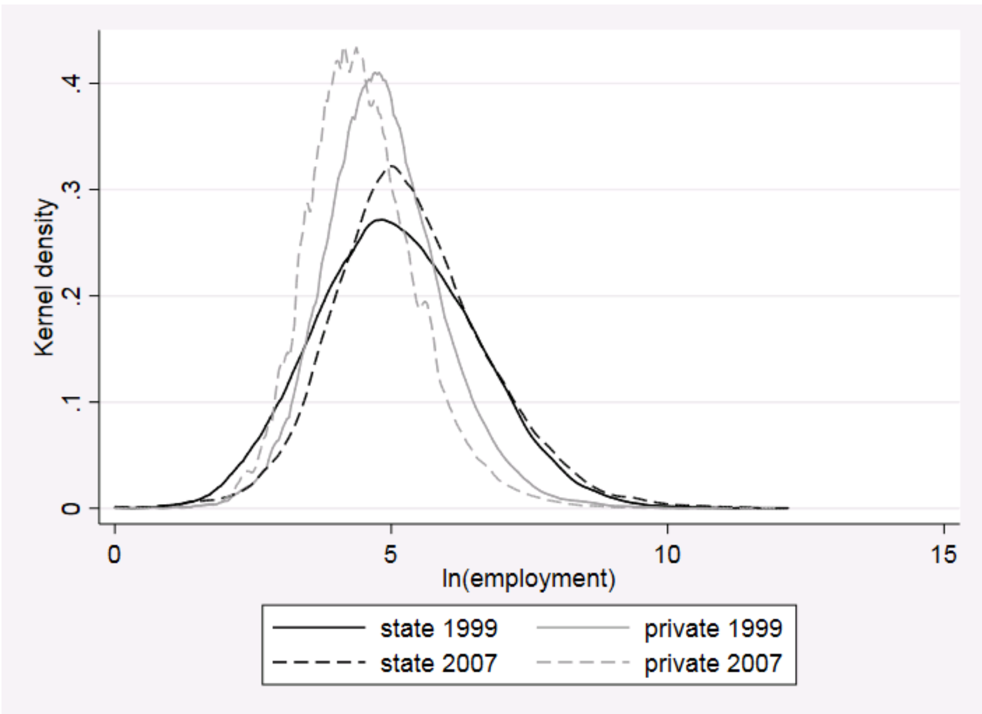


Figure 3.7: Kernel density plot of firm size by ownership types, **light** SO_2 polluters



Tables

Table 3.1: Detailed weights for suitability index construction

Factor	Sub-factor value	Reclassify values
Elevation	0-1000 m	10
0.06	1000-1400 m	8
	1400- 1800 m	4
	>1800 m	0
Slope	0-6 %	10
0.05	6-10 %	7
	>10 %	0
Road	0-500 m	0
0.08	0.5 - 10 Km	10
	10-20 Km	7
	20-40 Km	3
	>40 Km	0
Rail	0 - 500 m	0
0.14	0.5 - 10 Km	10
	10-20 Km	7
	20-40 Km	3
	>40 Km	0
Distance to urban area	0-10km	0
0.05	10-20km	10
	20-50km	7
	50-100km	4
	>100km	0
Distance to coal sources	0-5km	10
0.05	5-50km	5
	> 50km	0
Cultivation	Yes	5
0.04	No	10
Gas pipe line	0-500 m	0
0.08	0.5- 5 Km	10
	5-10 Km	8
	10-20 Km	6
	20-40 Km	3
	>40 Km	0
Large river	0- 500 m	0
0.08	0.5 - 10 Km	10
	10 - 20 Km	5
	>20 Km	0
Small river	0- 500 m	0
0.07	0.5 - 10 Km	10
	10 - 20 Km	5
	>20 Km	0
Distance to earthquake spots	0-1km	0
0.05	>1km	10
Distance to volcanoes	0-1km	0
0.02	>1km	10
Distance to airfields	0-5km	0
0.05	>5km	10
Distance to coal mines	0-5km	10
0.1	5-50km	5
	> 50km	0
Distance to oil and gas fields	0-5km	10
0.08	5-50km	5
	> 50km	0

Table 3.2: Prefectures hosting thermal power plants by 1998 pollute more

Dependent variable:	SO_2 in 1998			max NO_2 in 2005		
	(1)	(2)	(3)	(4)	(5)	(6)
1 if hosting thermal power plants by 1998	0.605***	0.269**		0.386***	0.190***	
	(0.130)	(0.108)		(0.061)	(0.055)	
ln(thermal power generating capacity by 1998)			0.044***			0.030***
			(0.017)			(0.008)
N	286	286	286	286	286	286
R^2	0.06	0.43	0.43	0.13	0.43	0.43
economic and climatic characteristics	No	Yes	Yes	No	Yes	Yes

* p<0.1, ** p<0.05, *** p<0.01. The pollutants are measured in log terms. Since power generating capacity is 0 for prefectures not hosting any thermal power plants, this variable is calculated as the log of 1 plus the actual generating capacity. City-level NO_2 measure in use is the max pixel value within the city boundary (mean value gives identical relationship). 2005 is the earliest year that this data is available with NASA. All regressions controlled for prefecture economic and climatic characteristics. Robust standard errors in parentheses.

Table 3.3: Suitability and likelihood to host thermal power plants

Dependent variable:	hosting power plants		higher power capacity		new power plants, 98-10	
	(1)	(2)	(3)	(4)	(5)	(6)
Prefecture suitability: mean	0.559***		1.048***		-0.163	
	(0.184)		(0.357)		(0.227)	
Prefecture suitability: top quartile		0.459***		0.861***		-0.085
		(0.164)		(0.320)		(0.193)
N	286	286	286	286	185	185
R^2			0.23	0.23		

* p<0.1, ** p<0.05, *** p<0.01. Columns 3 & 4 are OLS regressions while the rest is probit regression with raw coefficients. Since power generating capacity is 0 for prefectures not hosting any thermal power plants, this variable is calculated as the log of 1 plus the actual generating capacity. Columns 5 & 6 restrict the sample to cities not hosting thermal power plants by 1998. All regressions controlled for prefecture economic characteristics, climatic characteristics, and mean values of all factors in the suitability computation. Robust standard errors in parentheses.

Table 3.4: Likelihood of being on the list of "Two Control Zone" cities

	(1)	(2)	(3)
ln(SO2 density in 1998)	0.281***	0.234***	0.410***
	(0.066)	(0.077)	(0.109)
Prefecture economic characteristics	No	Yes	Yes
Prefecture climatic characteristics	No	No	Yes
Prefecture geographic characteristics	No	No	Yes

* p<0.1, ** p<0.05, *** p<0.01. The dependent variable is a dummy that equals to 1 if on the list and 0 otherwise. All regressions are probit regressions with raw coefficients reported. Robust standard errors in parentheses.

Table 3.5: Predictive power of suitability index on levels of main coal-firing power plant pollutants (IV first stage)

Dependent variable:	SO_2 in 1998		NO_2 in 2005	
	(1)	(2)	(3)	(4)
Prefecture suitability: mean	0.962***		0.789***	
	(0.085)		(0.069)	
Prefecture suitability: top quartile		0.830***		0.683***
		(0.072)		(0.058)
N	286	286	286	286
R^2	0.74	0.73	0.73	0.72

* p<0.1, ** p<0.05, *** p<0.01. The pollutants are measured in log terms. NO_2 data is available only from 2005 onward. All regressions controlled for prefecture economic and climatic characteristics. Robust standard errors in parentheses.

Table 3.6: Balancing tests using outcomes prior to treatment period 2000

Dependent variable: changes between	population 1982-1990	pop high skill 1982-2000	pop college 1990-2000	employment 1982-1990	manu emp 1982-1990
	(1)	(2)	(3)	(4)	(5)
ln(SO2 density in 1998)	0.019	0.033	-0.010	0.025	0.000
	(0.014)	(0.040)	(0.031)	(0.016)	(0.045)
N	286	286	286	286	286
First-stage F stat	127.44	127.44	129.14	127.44	127.44
Mean of dependent variable	0.13	0.71	1.14	0.24	0.48

* p<0.1, ** p<0.05, *** p<0.01. The pollutants are measured in log terms. NO_2 data is available only from 2005 onward. All regressions controlled for prefecture economic and climatic characteristics. Robust standard errors in parentheses.

Table 3.7: Impact of APPCL and air quality on employment, 2000-2010

Employment:	Primary sector (1)	Secondary sector (2)	Tertiary sector (3)	Total (4)
Panel A: IV results				
ln(SO2 density in 1998)	-0.007	-0.160***	0.005	0.020
	(0.022)	(0.039)	(0.017)	(0.015)
N	286	286	286	286
First-stage F stat	127.44	127.44	127.44	127.44
Mean of dependent variable	-0.25	0.46	0.40	0.05
Panel B: OLS results				
ln(SO2 density in 1998)	-0.043***	-0.027	0.028**	-0.004
	(0.013)	(0.025)	(0.013)	(0.009)
N	286	286	286	286
R^2	0.45	0.44	0.25	0.28

* $p<0.1$, ** $p<0.05$, *** $p<0.01$. All regressions controlled for prefecture economic and climatic characteristics. Robust standard errors in parentheses.

Table 3.8: Impact of APPCL and air quality on population, 2000-2010

Population:	Low-skilled (1)	High-skilled (2)	Total (3)
Panel A: IV results			
ln(SO2 density in 1998)	-0.001	-0.102***	0.010
	(0.013)	(0.021)	(0.013)
N	286	286	286
First-stage F stat	127.44	127.44	127.44
Mean of dependent variable	0.01	0.99	0.05
Panel B: OLS results			
ln(SO2 density in 1998)	-0.013*	-0.038***	-0.011*
	(0.007)	(0.014)	(0.006)
N	286	286	286
R^2	0.24	0.39	0.38

* $p<0.1$, ** $p<0.05$, *** $p<0.01$. All regressions controlled for prefecture economic and climatic characteristics. Robust standard errors in parentheses.

Table 3.9: Impact of SO_2 regulation on industrial employment, 1999-2007

ownership type:		all firms		state-owned firms		privately owned firms	
coal usage intensity:	all	heavy	light	heavy	light	heavy	light
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: IV results							
ln(SO2 density in 1998)	-0.098**	-0.188**	0.058	-0.478***	0.308**	0.046	-0.102
	(0.042)	(0.079)	(0.080)	(0.159)	(0.144)	(0.119)	(0.091)
N	286	286	286	283	286	281	285
First-stage F stat	127.44	127.44	127.44	118.34	127.44	124.97	127.51
Mean of dependent variable	0.19	-0.05	0.22	-1.04	-1.51	1.45	1.55
Panel B: OLS results							
ln(SO2 density in 1998)	-0.039	-0.085**	0.009	-0.192**	0.139**	-0.016	-0.106**
	(0.030)	(0.041)	(0.029)	(0.077)	(0.065)	(0.073)	(0.048)
N	286	286	286	283	286	281	285
R^2	0.38	0.14	0.41	0.15	0.17	0.20	0.11

* p<0.1, ** p<0.05, *** p<0.01. Industrial total employment and wage by SO_2 polluting industry are derived from the annual firm surveys. All regressions controlled for prefecture economic and climatic characteristics. Robust standard errors in parentheses.

Table 3.10: Long changes in employment based on industrial surveys

ownership type:		all		state-owned firms		privately owned firms	
coal usage intensity:	all	heavy	light	heavy	light	heavy	light
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: 1999-2007							
ln(SO2 density in 1998)	-0.098**	-0.188**	0.058	-0.478***	0.308**	0.046	-0.102
	(0.042)	(0.079)	(0.080)	(0.159)	(0.144)	(0.119)	(0.091)
N	286	286	286	283	286	281	285
First-stage F stat	127.44	127.44	127.44	118.34	127.44	124.97	127.51
Mean of dependent variable	0.19	-0.05	0.22	-1.04	-1.51	1.45	1.55
Panel B: 1999-2003							
ln(SO2 density in 1998)	-0.038	-0.200**	0.102	-0.481***	0.131	0.009	-0.038
	(0.025)	(0.080)	(0.074)	(0.134)	(0.123)	(0.120)	(0.076)
N	286	286	286	285	286	281	285
First-stage F stat	127.44	127.44	127.44	126.39	127.44	124.97	127.51
Mean of dependent variable	-0.05	-0.16	-0.03	-0.58	-0.76	0.85	0.95
Panel C: 2003-2007							
ln(SO2 density in 1998)	-0.060**	0.012	-0.044	-0.022	0.177**	0.030	-0.072
	(0.029)	(0.040)	(0.029)	(0.112)	(0.089)	(0.083)	(0.058)
N	286	286	286	283	286	285	286
First-stage F stat	127.44	127.44	127.44	118.34	127.44	127.51	127.44
Mean of dependent variable	0.24	0.11	0.25	-0.47	-0.74	0.61	0.61

* p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.11: Long changes in net firm count based on industrial surveys

ownership type:		all		state-owned firms		privately owned firms	
coal usage intensity:	all	heavy	light	heavy	light	heavy	light
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: 1999-2007							
ln(SO2 density in 1998)	0.177***	0.139**	0.225***	-0.109	0.246***	0.055	0.099
	(0.049)	(0.065)	(0.051)	(0.090)	(0.065)	(0.092)	(0.070)
N	286	286	286	283	286	281	285
First-stage F stat	127.44	127.44	127.44	118.34	127.44	124.97	127.51
Mean of dependent variable	0.62	0.42	0.61	-1.08	-1.65	1.72	1.85
Panel B: 1999-2003							
ln(SO2 density in 1998)	0.054*	0.013	0.090***	-0.163***	0.045	-0.014	0.045
	(0.029)	(0.042)	(0.031)	(0.058)	(0.046)	(0.084)	(0.061)
N	286	286	286	285	286	281	285
First-stage F stat	127.44	127.44	127.44	126.39	127.44	124.97	127.51
Mean of dependent variable	0.13	0.04	0.13	-0.54	-0.77	0.92	1.03
Panel C: 2003-2007							
ln(SO2 density in 1998)	0.123***	0.126**	0.134***	0.054	0.200***	0.062	0.050
	(0.039)	(0.053)	(0.039)	(0.072)	(0.057)	(0.080)	(0.047)
N	286	286	286	283	286	285	286
First-stage F stat	127.44	127.44	127.44	118.34	127.44	127.51	127.44
Mean of dependent variable	0.49	0.39	0.49	-0.54	-0.88	0.81	0.82

* p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.12: IV validity checks

	college edu 00-10	total pop 00-10	secondary emp 00-10	employment 00-10	coal emp 99-07	coal emp state 99-07
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: IV baseline						
ln(SO2 density in 1998)	-0.102***	0.010	-0.160***	0.020	-0.188**	-0.478***
	(0.021)	(0.013)	(0.039)	(0.015)	(0.079)	(0.159)
N	286	286	286	286	286	283
First-stage F stat	127.44	127.44	127.44	127.44	127.44	118.34
Panel B: excluding IV components as covariates						
ln(SO2 density in 1998)	-0.127***	-0.003	-0.143***	0.006	-0.170**	-0.416***
	(0.025)	(0.013)	(0.037)	(0.014)	(0.075)	(0.140)
N	286	286	286	286	286	283
First-stage F stat	154.46	154.46	154.46	154.46	154.46	152.46
Panel C: IV constructed with limited geographic factors						
ln(SO2 density in 1998)	-0.103***	0.010	-0.159***	0.022	-0.183**	-0.467***
	(0.021)	(0.013)	(0.038)	(0.015)	(0.077)	(0.154)
N	286	286	286	286	286	283
First-stage F stat	139.00	139.00	139.00	139.00	139.00	128.14
Panel D: using top quartile suitability as instrument						
ln(SO2 density in 1998)	-0.115***	0.012	-0.173***	0.021	-0.178**	-0.453***
	(0.022)	(0.013)	(0.042)	(0.015)	(0.077)	(0.149)
N	286	286	286	286	286	283
First-stage F stat	131.98	131.98	131.98	131.98	131.98	125.50

Table 3.13: Sensitivity tests

	college edu 00-10 (1)	total pop 00-10 (2)	secondary emp 00-10 (3)	employment 00-10 (4)	coal emp 99-07 (5)	coal emp state 99-07 (6)
Panel A: IV baseline						
ln(SO2 density in 1998)	-0.102***	0.010	-0.160***	0.020	-0.188**	-0.478***
	(0.021)	(0.013)	(0.039)	(0.015)	(0.079)	(0.159)
N	286	286	286	286	286	283
First-stage F stat	127.44	127.44	127.44	127.44	127.44	118.34
Panel B: excluding bottom quartile in 1998 SO_2						
ln(SO2 density in 1998)	-0.093**	0.040*	-0.322***	0.052**	-0.206*	-0.582**
	(0.039)	(0.020)	(0.067)	(0.024)	(0.114)	(0.268)
N	214	214	214	214	214	211
First-stage F stat	87.49	87.49	87.49	87.49	87.49	78.54
Panel C: conditional on initial levels						
ln(SO2 density in 1998)	-0.095***	0.003	-0.150***	0.014	-0.111	-0.474***
	(0.021)	(0.012)	(0.038)	(0.014)	(0.078)	(0.177)
N	286	286	286	286	286	278
First-stage F stat	116.88	133.47	117.96	134.68	102.34	92.66
Panel D: exclude share of heavy polluting employment in 1995						
ln(SO2 density in 1998)	-0.094***	0.009	-0.159***	0.008	-0.152**	-0.390***
	(0.020)	(0.013)	(0.037)	(0.014)	(0.077)	(0.151)
N	286	286	286	286	286	283
First-stage F stat	132.64	132.64	132.64	132.64	132.64	124.16
Panel E: treatment variable is Two-Control Zone status						
Two-control zone	-0.464***	0.047	-0.722***	0.092	-0.851**	-2.156**
	(0.147)	(0.062)	(0.232)	(0.072)	(0.427)	(0.875)
N	286	286	286	286	286	283

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Table 3.13 – *Continued from previous page*

	college edu 00-10 (1)	total pop 00-10 (2)	secondary emp 00-10 (3)	employment 00-10 (4)	coal emp 99-07 (5)	coal emp state 99-07 (6)
First-stage F stat	11.91	11.91	11.91	11.91	11.91	10.95
Panel F: treatment variable is 2010 SO_2 levels						
ln(SO2 in 2010)	-0.105***	0.011	-0.164***	0.021	-0.193**	-0.490***
	(0.022)	(0.014)	(0.040)	(0.015)	(0.083)	(0.165)
N	286	286	286	286	286	283
First-stage F stat	102.95	102.95	102.95	102.95	102.95	95.70

Appendix

3.6.1 Summary statistics

Table 3.14: Summary statistics

Variable	Mean	SD	Min	Median	Max
ln(SO2 density in 1998, city average)	0.13	1.14	-4.84	0.26	2.91
ln(NO2 density in 2005, city max)	13.65	0.51	12.60	13.55	14.71
1 if hosting thermal power plant by 1998	0.35	0.48	0.00	0.00	1.00
ln(thermal power generating capacity)	2.25	3.10	0.00	0.00	8.29
1 if hosting thermal power plants after 1998 & before 2010	0.36	0.48	0.00	0.00	1.00
suitability index, city average	4.52	0.61	2.53	4.52	5.96
suitability index, city 75th percentile	5.18	0.67	2.90	5.23	6.75
two-control zone city: SO2 zone	0.20	0.40	0.00	0.00	1.00
two-control zone city: acid rain zone	0.36	0.48	0.00	0.00	1.00
provincial capital or provincial-level city	0.09	0.28	0.00	0.00	1.00
total road and rail rays in 1999	4.40	2.50	0.00	4.00	13.00
share of agriculture employment in 2000	0.65	0.19	0.02	0.69	0.91
ln(distance to nearest coast in km)	5.34	1.84	-5.38	5.97	7.40
ln(distance to nearest river in km)	4.05	1.88	-1.26	4.66	6.47
mean city temperature in 2005	286.60	5.18	273.49	287.99	297.02
mean city wind speed in 2005	3.17	0.76	1.75	3.00	6.09
ln(average elevation)	5.45	1.53	0.12	5.55	8.34
ln(min distance to energy sources)	0.69	0.84	0.00	0.69	3.78
1 if near volcanoes, airfields, or earthquake spots	0.66	0.47	0.00	1.00	1.00
percentage of gas coverage	71.84	29.79	0.00	82.45	100.00
ln(population in 1982)	14.80	0.70	11.30	14.90	17.11
ln(population in 1990)	14.93	0.68	11.61	15.00	17.18
ln(population in 2000)	15.02	0.67	11.98	15.08	17.23

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Table 3.14 – *Continued from previous page*

Variable	Mean	SD	Min	Median	Max
ln(population in 2010)	15.07	0.68	12.35	15.10	17.18
share of population with below college education, 2000	0.90	0.03	0.56	0.90	0.94
share of population with below college education, 2010	0.86	0.04	0.64	0.87	1.06
share of population with college edu and above, 2000	0.03	0.02	0.01	0.02	0.17
share of population with college edu and above, 2010	0.08	0.05	0.02	0.07	0.31
Birth rate, 2000	0.12	0.03	0.05	0.11	0.25
Birth rate, 2010	0.10	0.03	0.03	0.10	0.20
Crude death rate, 2000	0.06	0.01	0.01	0.06	0.09
Crude death rate, 2010	0.06	0.01	0.01	0.06	0.09
share of migrants, 2000	0.06	0.09	0.00	0.03	0.83
share of migrants, 2010	0.10	0.11	0.01	0.05	0.80
share of migrants (from other provinces), 2000	0.03	0.07	0.00	0.01	0.64
share of migrants (from other provinces), 2010	0.04	0.08	0.00	0.01	0.65
share of manufacturing employment in 2000	0.12	0.11	0.01	0.08	0.76
share of manufacturing employment, 2010	0.15	0.12	0.01	0.11	0.74
share of primary employment, 2000	0.65	0.19	0.02	0.69	0.91
share of primary employment, 2010	0.51	0.20	0.00	0.55	0.84
share of secondary employment, 2000	0.16	0.13	0.02	0.13	0.79
share of secondary employment, 2010	0.22	0.13	0.05	0.20	0.76
share of tertiary employment, 2000	0.19	0.08	0.06	0.17	0.56
share of tertiary employment, 2010	0.26	0.10	0.10	0.25	0.71
share of employmnet by heavy coal-using firms, 1999	0.28	0.19	0.02	0.23	0.97
share of employmnet by heavy coal-using firms, 2007	0.25	0.21	0.01	0.18	0.98
share of industrial employment by private firms, 1999	0.20	0.13	0.02	0.18	0.73
share of industrial employment by private firms, 2007	0.65	0.18	0.04	0.68	0.97
share of industrial employment by state firms, 1999	0.58	0.21	0.03	0.63	0.97
share of industrial employment by state firms, 2007	0.20	0.17	0.00	0.15	0.96

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Table 3.14 – *Continued from previous page*

Variable	Mean	SD	Min	Median	Max
share of employmnet by private heavy coal-using firms, 1999	0.04	0.03	0.00	0.03	0.21
share of employmnet by private heavy coal-using firms, 2007	0.14	0.12	0.00	0.10	0.62
share of employmnet by state heavy coal-using firms, 1999	0.20	0.17	0.00	0.14	0.95
share of employmnet by state heavy coal-using firms, 2007	0.10	0.14	0.00	0.05	0.96
share of value added by heavy coal-using firms, 1999	0.26	0.20	-0.50	0.20	0.98
share of value added by heavy coal-using firms, 2007	0.24	0.20	0.01	0.17	0.93
share of industrial value added by private firms, 1999	0.24	0.15	-0.26	0.22	0.84
share of industrial value added by private firms, 2007	0.65	0.19	0.03	0.69	0.98
share of industrial value added by state firms, 1999	0.53	0.24	-0.03	0.53	1.14
share of industrial value added by state firms, 2007	0.21	0.19	0.00	0.15	0.97
share of value added by private heavy coal-using firms, 1999	0.05	0.08	-0.64	0.03	0.56
share of value added by private heavy coal-using firms, 2007	0.14	0.12	0.00	0.11	0.76
share of value added by state heavy coal-using firms, 1999	0.18	0.17	-0.01	0.12	0.97
share of value added by state heavy coal-using firms, 2007	0.09	0.12	-0.03	0.04	0.70

3.6.2 Additional robustness checks

The following table presents robustness checks using alternative pollutant measures as the treatment variable. The 2005 SO_2 and NO_2 variables are data from much finer satellite images (NASA OMI mission) providing greater precision. But this mission only dates back to late 2003. Their point estimates are very similar to those using coarser 1998 measures, showing spatial resolution is not of concern in the baseline results. Lastly, I check the estimation consistency using 1998 $PM_{2.5}$ measures. Their point estimates are much larger in magnitude and the first-stage is weaker, presumably because $PM_{2.5}$ comes from many sources and therefore power plant contribution is much less important.

Table 3.15: Additional sensitivity tests

	college edu 00-10 (1)	total pop 00-10 (2)	secondary emp 00-10 (3)	employment 00-10 (4)	coal emp 99-07 (5)	coal emp state 99-07 (6)
Panel A: IV baseline						
ln(SO2 density in 1998)	-0.102***	0.010	-0.160***	0.020	-0.188**	-0.478***
	(0.021)	(0.013)	(0.039)	(0.015)	(0.079)	(0.159)
N	286	286	286	286	286	283
First-stage F stat	127.44	127.44	127.44	127.44	127.44	118.34
Panel B: treatment variable is 2005 SO_2 levels						
ln(SO2 density in 2005)	-0.091***	0.009	-0.141***	0.018	-0.166**	-0.425***
	(0.021)	(0.012)	(0.033)	(0.013)	(0.070)	(0.146)
N	285	285	285	285	285	282
First-stage F stat	93.63	93.63	93.63	93.63	93.63	94.09
Panel C: treatment variable is 2005 NO_2 levels						
ln(NO2 density in 2005)	-0.125***	0.013	-0.195***	0.025	-0.229**	-0.583***
	(0.029)	(0.016)	(0.045)	(0.017)	(0.092)	(0.188)
N	286	286	286	286	286	283
First-stage F stat	131.05	131.05	131.05	131.05	131.05	133.15
Panel D: treatment variable is 1998 $PM_{2.5}$						

Continued on next page

Table 3.15 – *Continued from previous page*

	college edu 00-10 (1)	total pop 00-10 (2)	secondary emp 00-10 (3)	employment 00-10 (4)	coal emp 99-07 (5)	coal emp state 99-07 (6)
ln(PM2.5 in 1998)	-0.413***	0.042	-0.643***	0.082	-0.757**	-1.958***
	(0.100)	(0.054)	(0.179)	(0.062)	(0.306)	(0.691)
N	286	286	286	286	286	283
First-stage F stat	54.59	54.59	54.59	54.59	54.59	49.00

3.6.3 SO_2 trends by power plant hosting status

The following figures are analogues to figures 3.2, 3.3, and 3.4. Instead of grouping by TCZ status, the grouping here is by their thermal power plant hosting status. They convey the same messages as those in section 3.2.2.

Figure 3.8: Kernel density of ambient SO_2 levels in 1998 and 2010

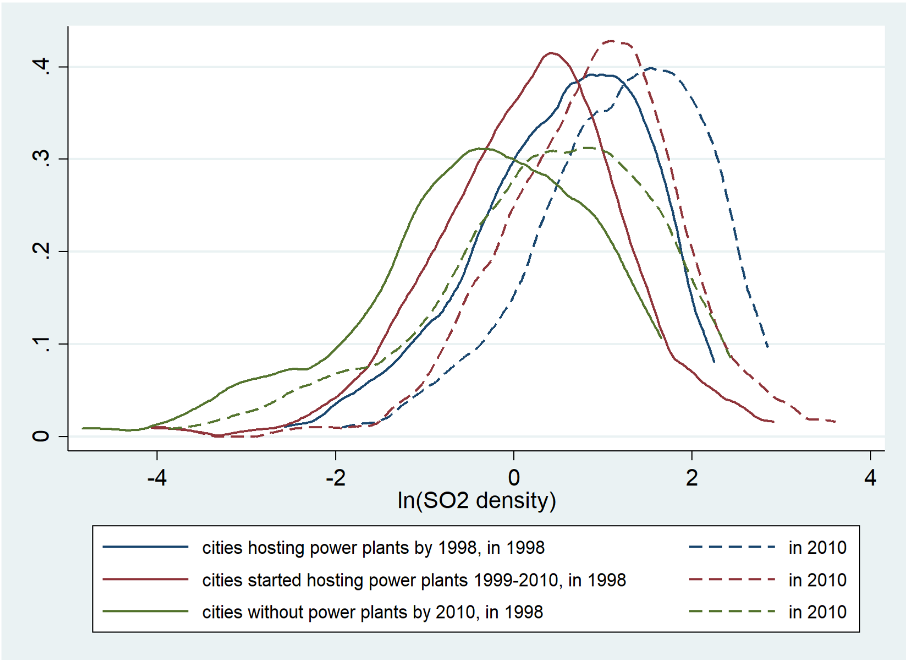


Figure 3.9: Ambient SO_2 change and power plant hosting

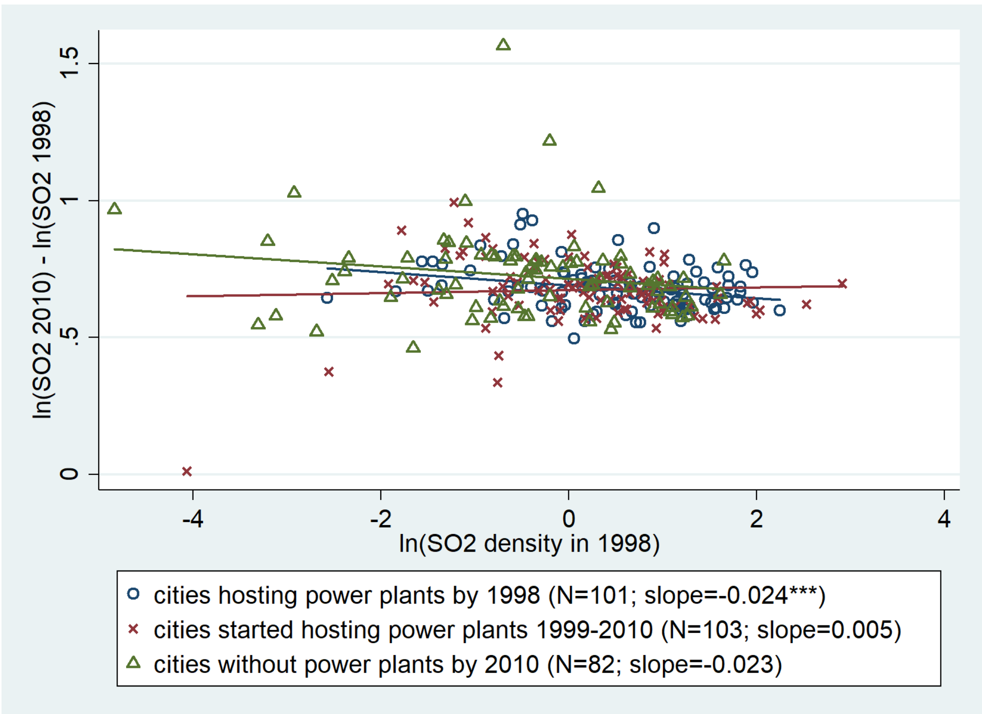
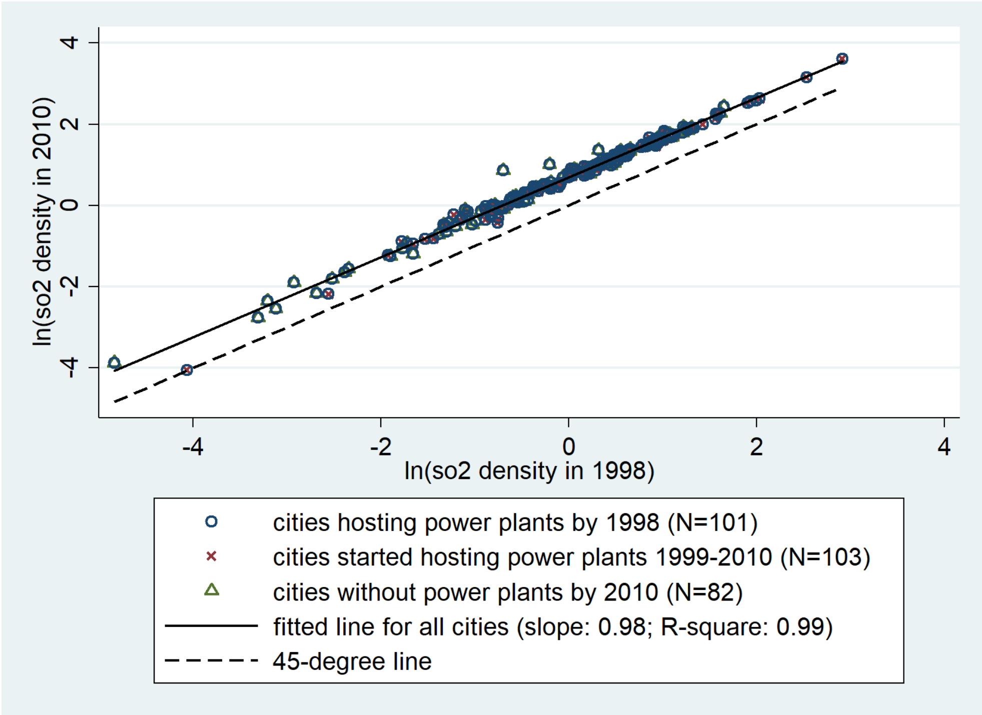


Figure 3.10: Relative ranking of ambient SO_2 levels between 1998 and 2010 by power plant hosting status



Productivity on surviving firms

As the firm survey dataset is of cross-sectional nature and repeated annually, I uncover a small subset of identical firms over time based on firm ID, county name, and open year and month. This forms a sample of surviving firms, with low match rate. Between 1999-2007, 9% of 1999 firms and 4% of 2007 firms are matched. For shorter time periods 1999-2003 and 2003-2007, the shares of matching improve to the range of 16-27%. One the one hand, this sample allows for the inclusion of firm fixed effect to tune down potential bias. On the other hand, this small sample is far from representative, and prone to noise.

In keeping with the literature, I define firm-level employment (value added) growth as the difference in employment (value added) between two time periods, divided by the average employment (value added) of the two. I estimate the following firm-level regression:

$$\Delta \ln(y_{fi}) = \eta + \kappa \widehat{\mathrm{SO}_2}_{i,1998} + \beta \mathrm{X}_f^{-1} + \lambda \mathrm{X}_i^{-1} + u_{fi}, \tag{3.16}$$

which is at the firm level, f. Firm-level characteristics such as ownership type is included for pooled samples. Error term is clustered at the prefecture level. The rest follows exactly the baseline regression.

Table 3.16 presents results on employment and value added growth by SO_2 polluting and ownership types. Most results are not statistically significant due to noisy small samples. Despite this, column 4 results show that surviving heavy SO_2 polluting firms in the state sector declined faster between 1999 and 2003 in both employment and value added growth. This confirms that the labor market impact of APPCL works through the productivity channel.

Table 3.16: How did the regulation affect the surviving firms

ownership type:		all		state-owned firms		privately owned firms	
coal usage intensity:	all	heavy	light	heavy	light	heavy	light
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1999-2007:							
Employment							
ln(SO2 density in 1998)	-0.037	-0.025	-0.037	-0.025	-0.040	-0.028	-0.033
	(0.035)	(0.036)	(0.043)	(0.050)	(0.037)	(0.059)	(0.045)
N	12888	1868	11020	806	2199	527	3251
First-stage F stat	55.27	148.60	45.90	141.40	97.48	82.95	59.79
Value-added							
ln(SO2 density in 1998)	-0.197	0.011	-0.235	0.218	-0.946	0.165	-0.057
	(0.326)	(0.228)	(0.434)	(0.452)	(1.108)	(0.308)	(0.143)
N	12888	1868	11020	806	2199	527	3251
First-stage F stat	55.27	148.60	45.90	141.40	97.48	82.95	59.79
1999-2003:							
Employment							
ln(SO2 density in 1998)	0.006	-0.003	0.010	-0.032*	-0.018	0.021	-0.006
	(0.016)	(0.015)	(0.019)	(0.018)	(0.018)	(0.027)	(0.030)
N	30176	4519	25657	2114	7141	1103	7387
First-stage F stat	75.19	172.93	63.66	197.25	128.62	88.99	64.32
Value-added							
ln(SO2 density in 1998)	-0.040	-0.161	-0.036	-0.276*	-0.076	-0.161	0.099
	(0.168)	(0.190)	(0.219)	(0.163)	(0.417)	(0.384)	(0.184)
N	30175	4519	25656	2114	7140	1103	7387
First-stage F stat	75.21	172.93	63.68	197.25	128.72	88.99	64.32
2003-2007:							
Employment							
ln(SO2 density in 1998)	-0.028	0.003	-0.030	-0.026	-0.032	0.039*	-0.010
	(0.021)	(0.017)	(0.024)	(0.028)	(0.022)	(0.021)	(0.019)
N	50590	5442	45148	1286	3456	3105	27582
First-stage F stat	86.92	189.63	77.17	167.34	113.96	166.74	100.24
Value-added							
ln(SO2 density in 1998)	0.155	0.345	0.135	2.001	-0.006	-0.058	0.034
	(0.128)	(0.393)	(0.136)	(1.828)	(1.039)	(0.355)	(0.077)
N	50590	5442	45148	1286	3456	3105	27582
First-stage F stat	86.92	189.63	77.17	167.34	113.96	166.74	100.24

Data sources

Table 3.17: Sources of data for the computation of suitability index

Name:	Source:	Details:
SO_2 column density	NASA Global Modeling and Assimilation Office	Time span: 1998-2010, annual means Scale: $0.5° \times 0.625°$; unit: kg m^{-2}
Infrastructure & land use	United States Geological Survey: Coal Geology, Land Use, and Human Health in China Compiled by Alex W. Karlsen et al	List of shapefiles compiled by 2001 Key variables: roads, railroads, airfields, small/large rivers, earthquake spots, volcanoes, coal bearing fields, etc.
Coal mines, oil & gas fields	USGS Compilation of GIS Data Representing Coal Mines and Coal-Bearing Areas in China	Surveyed by 2001 but first fully published in Jan 2015 Key info: coal mines, oil and gas fields
Gas pipelines	China Natural Gas Pipelines Dataset, Harvard ChinaMap	Gas pipelines and nodes
Elevation	Harvard China Historical GIS V5 DEM	1km pixel resolution based on GTOPO-30 data from USGS
Land cover	Global Land Cover Facility, MODIS Land Cover	$5' \times 5' \approx 0.083°$ resolution, yearly 2001-2012
Soil type	ISRIC/World Soil Information Soil and Terrain Database for China	soil information is compiled in 2004

Chapter 4

Cholera in Times of Floods: Weather Shocks & Health in Dar es Salaam

4.1 Introduction

Climate change will have a significant impact on the lives of the poor in the years ahead as extreme weather events such as floods, heavy precipitation and droughts are expected to become more frequent (Harrington et al. 2016). Populations at different stages of development are affected differently by the same weather variations (Dasgupta, 2010; Burgess et al. 2017). As cities in developing countries continue to urbanize at an unprecedented pace, the question of how their urban dwellers are impacted by weather shocks is becoming increasingly relevant. On the one hand, urban residents may seem better prepared than their rural counterparts against weather extremes; their livelihoods are for instance less dependent on weather phenomena. Yet, rapid urbanization has often led to *unplanned* cities with poor infrastructure, limited public service provision, and with large segments of the population living in informal settlements.

Empirical evidence is scant concerning the impact of weather shocks in developing country cities. This paper tries to make progress on this issue by looking at the effect of rainfall and flooding on cholera incidence in Dar es Salaam. Looking at health outcomes is important. Contagion is one "downside of density" (Glaeser & Sims 2015). Throughout history, cities with low-quality infrastructure and poor sanitation have been pockets of epidemics (for instance 19^{th} century London or Paris, Kesztenbaum & Rosenthal 2016). Poor health and disease not only lower productivity in the short-term, they also hinder long-term economic growth (Well 2007).

We examine this question in the context of a cholera outbreak in Dar es Salaam in 2015 and 2016, during which almost five thousand cases were recorded. Cholera is a water and food-borne disease and its transmission is closely linked to inadequate access to clean water and sanitation facilities. Weather shocks such as rainfall can affect health through two main channels. The first one is related to direct mechanisms operating via human physiology and disease. In this case, heavy rainfall and floods can increase exposure to vibrio cholera bacteria, which survives better in wet environments (Lipp et al. 2002, Osei et al. 2010). Droughts have also been linked to cholera outbreaks, as population use unfit water for their needs (Sasaki et al. 2008, Taylor et al. 2015). The second sets of mechanisms are indirect ones, through the effect weather shocks may have on real incomes. In the case considered here, there are many ways through which floods and rainfall can reduce accessibility to work. The resulting lower income may in turn lower the consumption of health-improving goods (i.e. safe drinking water, medicines), increasing the exposure to the bacteria.

Our empirical analysis uses finely disaggregated ward-level panel data containing weekly recorded cholera cases and weekly accumulated precipitation for all the municipalities in the city. We are therefore testing whether exogenous weekly rainfall variation at the ward-level affects cholera occurrence. Sorting between neighborhoods is taken care of with ward-fixed effects. The short time frame considered excludes any concern related to changes in sorting patterns. The use of high-frequency data and the fine geographical detail thus allow us to estimate with precision our relationship of interest. We focus on reduced-form specifications; different spatial and time-lag models support our choice. We complement our data with ward-level infrastructure characteristics (i.e. roads, foot-ways, drains, water wells and housing informality) to understand the relationship between infrastructure quality, precipitation, and cholera. Dar es Salaam is a city of more than 4 million people where close to 70% of its residents live in informal settlements. Access to improved sanitation is very low and only 37% of the city has regular refuse collection (World Bank 2017).

We find robust evidence that weekly accumulated rainfall and flooding leads to higher ward-level cholera occurrence. On average, a 10 mm increase in weekly accumulated precipitation leads to an increase of between 1.5% to 3.5% of weekly recorded cholera cases, significant at 1% level. The effect is found to be much larger when considering a more flexible quartile specification. On average, a single additional week of rainfall falling above the 75$^{\text{th}}$ percentile of the total rainfall distribution (extreme rainfall), increases the number of effective cholera cases by up to 20.3% relative to a week with very light rain ($<$0.30 mm). Further, we find that the impact of heavy rainfall ($>$75$^{\text{th}}$ percentile) is close to 20 percentage points higher in wards at greater risk of flooding (i.e. higher flood-prone area), all else equal. A dry week (0 mm rainfall) is also positively related to cholera incidence, but the coefficient is not statistically significant. These findings are consistent with the direct and indirect mechanisms put forward earlier. Particularly, the inundation of drains, water systems, and pit latrines greatly enhances the risk of exposure to contaminated water and food. These results are robust to alternative estimators and specifications, including an instrumental variable strategy and controlling for the spatial autocorrelation of standard-errors.

Remarkably, we find little to no spatial spillovers from precipitation in neighboring wards. Only when considering the relative elevation of contiguous wards there is a small significant effect from precipitation in downhill wards. That is, a 10 mm increase in weekly accumulated rainfall in downhill neighboring wards increases cholera cases in the ward by 0.01% to 0.03% . This finding is consistent with water source contamination. In contrast, our results reveal moderate time-dynamic effects. Using a distributed lag model, we find significant positive effects of past rainfall on cholera incidence to up to five preceding weeks. Contemporaneous rainfall remains the largest determinant, with a stable size close to 3% and statistical significance at or above 5% level. All lags decrease in size the farther in time.

We explore the non-linear relationship of rainfall and cholera incidence with respect to various ward-level infrastructure characteristics as well. We find the effect of weekly rainfall on cholera cases to be consistently higher for wards with larger shares of informal housing and a higher density of foot-ways (i.e. informal roads). These results are consistent with the two possible mechanisms outlined earlier. Neighborhoods with limited access to sanitation and low-quality infrastructure are likely to be more exposed to the cholera bacteria when surfaces are washed and drains are overflown by severe precipitation. Vulnerable populations in these wards are also more likely to suffer from negative income shocks during extreme weather events.

Our findings relate to three different bodies of research. The first one is a large economics literature looking at weather shocks and economic events mostly in advanced economies (Munshi 2003; Miguel et al. 2004; Barrios et al. 2010), and including health outcomes of human populations (Deschenes & Greenstone 2007; Deschenes & Moretti 2009; Burgess et al. 2011; Deschenes 2011). Our paper follows their empirical methods (for a review see Dell et al. 2014). The second is the large literature in development economics and public health studying policy interventions, mechanism of transmission and health outcomes in developing countries (Banerjee et al. 2004; Miguel & Kremer 2004; Dunkle et al. 2010; Penrose et al. 2010; Devoto et al. 2012). Here, we contribute to that literature by focusing on urban areas and by studying general mechanisms beyond specific policy interventions. Our findings use robust econometrics techniques to discern the relationship between disease, infrastructure and weather shocks. Finally, we particularly contribute to the nascent literature in urban economics studying the effects of weather phenomena on urban areas in developing country cities (Kocornik-Mina et al. 2015; Glaeser & Henderson 2017; Henderson et al. 2017). To the best of our knowledge, our paper is the first to empirically study the effect of weather shocks on disease transmission within a city of a developing country. Our findings have important policy implications as extreme weather events become more frequent in the next decades. Cities in developing countries need to address infrastructure gaps to contain the risk of recurrent epidemic outbreaks in vulnerable populations and neighborhoods. Investing in resilient infrastructure, with the proper servicing of informal settlements or related measures such a regulating waste dumping may prove to be more beneficial in the long-term than the use of short-term palliative measures during outbreaks. Evidence on large-scale policy interventions in urban areas is still limited and more is needed to understand how to prevent contagion of treatable diseases in developing cities.

This paper is organized as follows. The next section formalizes the relationship between health and weather shocks, specifically here cholera and rainfall in cities. Section 3 describes the context of Dar es Salaam, the cholera disease and the data. In section 4 we present our empirical strategy. Section 5 presents the estimates of the effect of rainfall and flooding on cholera incidence and different extensions and robustness checks. Section 6 concludes.

4.2 Theoretical framework: Weather & Health

In this section, we describe a simple theoretical framework to examine the various channels through which weather shocks (i.e. here, heavy precipitation) can affect cholera incidence in an urban setting. It relies on endogenous health models in a simplified fashion (for more details see Becker 2007; Deschenes 2012; Burgess et al. 2017).

Cholera is an acute diarrheal infection caused by ingestion of food or water contaminated with the bacterium vibrio cholera. It can affect both children and adults and can kill within hours if left untreated. The main reservoirs of the cholera bacterium are people and warm salt water bodies such as estuaries and coastal areas. Cholera transmission is closely linked to inadequate access to clean water and sanitation facilities (WHO 2017)[1]. Here we assume that weather shocks such as extreme rainfall, droughts and flooding can affect human health (i.e. cholera prevalence) both directly (through higher exposure to the bacteria, for instance), and indirectly (due to the negative income-shocks that may arise through the weather shock).

Consider a city with a large number of agents indexed by i. Agents seek to maximize their lifetime utility u_i depending on consumption c_{it} and health status h_{it} . These two are complements. This city is partitioned into several neighborhoods and each agent lives in a given neighborhood (or ward) indexed by n. We assume that agents are exogenously allocated to neighborhoods with different characteristics such as better infrastructure, and the latter are exogenously determined. It follows that agents have limited scope for affecting local infrastructure, as well as other public goods and services, and take these as given (mobility is limited in the short-term scenario considered here).

Agent i's health status, resident of neighborhood n at time t, (h_{int}) is thus determined by her consumption c_{it} and random health shocks z_{int}. We do not specify a precise relationship between health outcomes and consumption, but it is assumed that an individual can improve her health by increasing her consumption, particularly by purchasing health-improving goods. All told, the agent's health status is thus given by:

[1] http://www.who.int/mediacentre/factsheets/fs107/en/, accessed on 28 June 2017.

$$h_{int} = h_i(c_{it}, z_{int}) \tag{4.1}$$

where $h_i(\cdot)$ is increasing in c_{it} and decreasing in z_{int} (adverse health shocks). The function h_i is unrestricted and can differ across heterogeneous individuals. The vector z_{int} includes weather-shocks w_t such as flooding, droughts, and heavy rainfall or temperature extremes. We assume further that the effect of the weather shock is conditional on the quality of local neighborhood infrastructures (q_n such as access to drinking water and waste water systems, or road and pavement material. The adverse health shock is thus a function of the weather shock that varies with local infrastructure, $z_{int}(w_t(q_n))$

Consumption[2] c_{it} is financed through labor income in period t, which depends on the agent's productivity a_{it} . Productivity is agent-specific but also depends on weather shocks w_t :

$$a_{it} = a_i(w_t) \tag{4.2}$$

Here the effect on productivity can stem from the weather shock's impact on z_{int} , hampering the agent's ability to work efficiently, or deter accessibility to jobs or other factors of production (i.e. machinery, location)[3]. A given weather shock w_t thus affects an agent's health status through both consumption (via productivity) and health idiosyncratic shocks (via z_{int}). In other words, there are two fundamental mechanisms through which weather shocks (extreme precipitation for the purpose of our empirical analysis) can potentially harm an agent's health status here.

First, through direct health effects: random shocks z_{int} , enter the agent's health status directly as in equation (4.1). That is, holding constant the agent's income,

[2] We assume the consumption good is produced using an aggregate production function that requires capital and labor inputs; it exhibits decreasing return to scale. Goods can be bought and sold at the market price, which is exogenously determined. Agents are subject to budget constraints in each period which are a function of the labor income (in turn dependent on productivity and adverse weather shocks), as well as prices and quantities of goods consumed. We assume imperfect credit and savings markets which prevents agents from smoothing their consumption in time.

[3] We assume, where refers to complements to work such as accessibility to jobs, machinery or location, that can be affected by weather shocks.

location and consumption decisions, we expect a negative weather shock to impact this agent's health adversely (w_t impacts z_{int} in the language of our model). In the case considered here, heavy rainfall and flooding can directly impact one's health status through greater exposure to and contact with contaminated water and food. An extensive public health literature discusses the potential for cholera prevalence in wet environments and in cases of heavy precipitation and flooding (see for example, Osei et al. 2012). Further, the magnitude of the effect can be expected to depend on the relationship the weather phenomena and the disease pathogens have with local infrastructure characteristics. Cholera strives in stagnant water and poor hygiene conditions as explained earlier.

The second, more indirect mechanism through which weather can affect health in this model is through the agents' productivity in equation (4.2). This term depends on weather shocks that may affect the agent's ability to work via z_{int} . Flooding and heavy rainfall may also significantly affect work-places and accessibility in contexts where poor roads and infrastructure is widespread (see footnote 4). Reduced productivity can translate in lower earnings and reduced consumption of healthier quality goods such as clean water or medicines. The dependency of productivity and hence, labor income, on this type of shocks is extremely likely in low-income countries where informal jobs dominate employment. It is estimated that close to 80% of jobs in the services sector are informal in Tanzania (UNDP 2015). Note that this assumes imperfect credit and savings markets preventing agents from smoothing their consumption when hit by economic hardship. Given the Tanzanian context, this assumption does not seem unrealistic.

The main implication of this exercise is to expect an increase in cholera cases due to a weather shock such as extreme precipitation and flooding. The increase should be larger in neighborhoods with poorer infrastructure. Conversely, the impact of heavy rainfall should be mitigated in areas with a supply of higher-quality local public goods. Linking this section with our empirical analysis, we expect to see non-linear effects of rainfall on cholera occurrence.

Finally, one additional implication of this simple formalization concerns policy interventions. In the face of potential weather shocks, any agent i would seek to minimize the damage that the negative shock has on their utility, $u_i(c_{it}, h_{it})$

by consuming health-improving goods or potentially by reallocating resources between periods. This latter option here is limited due to credit and savings constraints. These potential shock-minimizing strategies have strong implications for policy. In the theoretical framework considered here, governments can reduce the adverse effect of weather on health outcomes by directly increasing the quality of infrastructure that is related to the pathogens' transmission (i.e. pavement, sanitation, water drainage, sewage), and thus directly limit the potential effect of an adverse health shock, z_{itn}. But they can also intervene by supporting the agent's shock minimization strategies through subsidized health goods or direct transfers.

4.3 Background & Data

This section provides further details on cholera-specific characteristics as well as Dar es Salaam's context. It also describes the data in detail and provides basic summary statistics.

4.3.1 Cholera

Cholera is an acute diarrheal infection of fecal-oral transmission. It is caused by the ingestion of food or water contaminated with the bacterium vibrio cholera. It takes between twelve hours and five days for a person to show symptoms after ingesting contaminated food or water. It can affect both children and adults and can kill within hours if left untreated; there is a 50% death rate if untreated, but all deaths are avoidable otherwise. Main treatments include antibiotics and Oral Re-hydration Salts (ORS). Roughly 1.3 to 4.0 million cases are recorded worldwide every year, and the disease is endemic to many parts of sub-Saharan Africa and South Asia (WHO 2017).

There are multiple pathways for cholera transmission (Clasen et al. 2007). The disease is closely linked to inadequate access to clean water and sanitation facilities. Risk factors are also considered to be high population density and crowding, all of which are often common in urban slum areas (Penrose et al. 2010).

Cholera incidence has been found to be highest in highly urbanized areas (Osei

& Duker 2008; Sur et al. 2005). The main reservoirs of the cholera bacterium are people and warm salty water bodies such as estuaries and coastal areas. Global warming and rising sea levels are believed to create a favorable environment for cholera bacterium growth (WHO 2017). Heavy rainfall and flooding have all been associated with a higher likelihood of cholera outbreak. Surface runoff from point sources (pit-latrines, waste dump site, water wastes) may cause increased contamination of water sources, while stagnation and slow flowing of waterways may lead to increased exposure to cholera vibrios (Osei et al. 2010).

4.3.2 Dar es Salaam

Dar es Salaam is one of the largest cities in eastern Africa. It is located in the east of Tanzania by the Indian Ocean. Its urban population grew at 6.5% yearly between 2002 and 2012 (Wenban-Smith 2014), and today the city counts with more than 4.4 million people. Since 2016, it is divided in five municipal districts: Ilala, Temeke, Kinondoni, Kigamboni and Ubungo[4]. These municipalities are further divided up into 90 wards.

The rapid pace of urbanization has led to large infrastructure deficits. Close to 70% of Dar es Salaam's residents live in informal settlements without adequate access to clean water, proper drainage system and waste collection (UN-HABITAT 2010; Natty 2013). Only 13% of the city's residents have adequate sewage systems and 37% of the solid waste is properly collected. The World Bank (2015) estimates that only 50% of residents have access to improved sanitation. The most common form of improved sanitation is improved pit-latrines (other forms are rare). About two-thirds of households in the city share their toilet facilities. Access to piped water is also very limited, with only 17% of city-centre dwellers having piped-water.

Dar es Salaam's geography and coastal location makes it vulnerable to climatic hazards, particularly floods, sea level rise and coastal erosion (Kebede and Nicholls 2010). There are two rainy seasons every year, the short (October to December) and long (March to May) seasons, and average annual precipitation is above 1,000 mm.

[4] In the analysis, we only use 3 municipal districts as these were the ones that existed at the time cholera cases were recorded during the last outbreak. Ubungo and Kigamboni were created in 2016 from dividing Kinondoni and Ilala further so this does not impact our findings in any way.

The combination of high informality and climatic vulnerability makes flood risk one of the main challenges for sustainability, exposing infrastructure and residents to safety and health hazards from vector-borne diseases such as malaria and cholera (World Bank 2017).

Cholera has been endemic in Tanzania since the 1970s and Dar es Salaam has historically been the most affected region[5]. During the 2015-2016 outbreak, there were over 24,000 cases recorded nationally, with more than one fifth in Dar es Salaam (Figure A1 in appendix). Previous outbreaks occurring between 2002 and 2006 reported over 30,000 cases nationally, with nearly 18,000 in the capital city (WHO 2008). Given the city's poor sanitary conditions, high population density, lack of access to safe drinking water, and limited drainage, continuous heavy rainfall makes stagnant and unsanitary water a widespread health risk for common water borne diseases. The lack of storm water drains, frequently blocked by unregulated waste dumping, means that heavy rainfall quickly leads to flooding and contaminates water wells (Pan-African START 2011).

4.3.3 Data

To examine the relationship between weather variation and cholera incidence outlined in our theoretical framework, we collect data from several sources and put together a comprehensive ward-level panel dataset for each week between the first week of March 2015 and the first week of September 2016. The choice of the time frame is data constrained - that is, we use the first week for which precipitation data is available and the last week for which cholera cases were recorded to avoid measurement error from unrecorded cases. We cover all the 90 wards of the city[6]. The use of high-frequency data and the fine geographical detail allow us to estimate with precision our relationship of interest. The basic panel thus consists of weekly cholera cases registered according to the ward of residence. We combine this data with weekly accumulated precipitation and weekly air-temperature in these wards.

[5] The largest cholera epidemic in Tanzania to date took place in 1997 where 40,000 cases were reported. The epidemic is said to have started in Dar es Salaam. Dar es Salaam has had the most cholera cases since 2002 of all regions of the country (Penrose et al. 2010).

[6] Since mid-2016 there are 5 municipalities in Dar es Salaam as two municipalities were further sub-divided. We use the original administrative units at the time of the outbreak in our regression analysis for simplicity and coherence with the recorded cholera dataset. This should not affect any of the results.

Further, we add data on ward-level infrastructure, geographical characteristics (i.e., elevation, flood-prone surface) and population (census). We outline below the different data sources. Main summary statistics are in Tables 4.1 and 4.2.

Cholera cases

The key data in this analysis are the new ward-level cholera cases collected from the Regional Medical Office and Municipal Health Officers for all the wards of Dar es Salaam and covering the entire 2015-2016 outbreak. The data was registered daily for each individual presenting symptoms of severe diarrhea in a medical facility. It includes basic socio-demographic characteristics (age, sex) of the individual, the ward and sub-ward of residence, as well as the date of the first symptom and registration at the hospital. Cases were tested for the vibrio cholera bacteria, and the dataset also includes lab results. We exclude all cases tested negative and focus on effective cholera cases only[7]. No positive case is reported earlier than mid-August 2015 (epidemic week zero). The outbreak officially lasted from August 2015 until May 2016. We aggregate the daily cases by week to account for the fact that the incubation period is between 12 hours to 5 days.

Measurement error is a potential problem. The biggest threat concerns the possibility that not all cholera cases are reported in the non-outbreak period. It is also possible that not all registered cases during the peak of the epidemic are effective cholera cases (see footnote 8). There are mitigating factors against both these risks. First, cholera is one of the few diseases that require reporting to the World Health Organization (WHO) by the International Health Regulations as it can quickly spread if left untreated and result in explosive outbreaks. This implies careful monitoring of the disease as well as frequent laboratory testing. Further, we focus our analysis during an outbreak where monitoring is more likely to be enforced. Finally, our baseline estimates are weighted by the population of the ward, to account for the difference in precision concerning cholera measurement from larger and smaller populated wards. While bias from measurement error in our dependent

[7] We include both positive and untested cases. Most untested cases are at the peak of the outbreak when all patients presenting symptoms are treated as cholera patients. Measurement error is possible but should be limited as tests are frequently carried, particularly at the beginning and end of the outbreak period.

variable is still possible, it should not be large.

Overall, close to 5 thousand cases of cholera were reported positive in Dar es Salaam in the period analyzed (4964 of total 5698 tested), with the bulk taking place during the first 10 weeks of the outbreak (Figures 4.1 & 4.2). On average, during the period covered there were 0.72 effective cases weekly per ward. The number is larger during the first 10 weeks of the epidemic (3.16) as well as the first 20 weeks of the epidemic (2.54) (Table 4.2). Cholera cases were more pronounced in Kinondoni and Ilala, reporting totals of 2428 and 1796, respectively. Temeke was the least affected (Figure A2). Most cases took place within 15 km from the Dar es Salaam CBD (Figure 4.3); only 2 of the 90 wards reported zero positive cases throughout the period.

Weather & Geography

Rainfall - The weather datasets in this paper are from NASA. The daily precipitation measures by ward are derived from the Integrated Multi-satellite Retrievals (IMERG) for Global Precipitation Mission (GPM), where rainfall is comprehensively measured at the highest accuracy and finest spatial resolution to date (Huffman, 2016). We use the near-real-time total daily rainfall defined as precipitation accumulated in the past 24 hours by 23:59pm (Coordinated Universal Time) of each day. We calculate weekly accumulated precipitation from the daily data. In terms of the spatial resolution, rainfall is measured at squared pixels of $0.1\,^\circ \times 0.1^\circ$ (roughly 120km^2).

As ward boundaries are irregularly shaped, we compute ward-level daily rainfall accumulation by weighting recorded rainfall with the ward overlay with satellite pixels. We first union these two layers to create polygons at the ward-pixel level. These ward-pixel polygons all have consistent rainfall measurement, and their respective area is computed. We then sum up the ward-pixel rainfall measures for each ward by weighting by their ward area share. This gives us the area-weighted weekly rainfall accumulation at the ward level (Figure A3). The choice of focusing on rainfall accumulation (i.e. total weekly rainfall) stems from the fact that precipitation is 'readily stored' in the soil, tanks, or water wells. It is stagnant water that might breed cholera and thus, measuring average rainfall instead would fail to take this important dimension into account.

Because satellite data are subject to error (Dell et al. 2014), we also use an additional and independent gridded data set to address potential measurement issues and obtain instrumental variables (IV) estimates. We use precipitation obtained from IMERG's predecessor technology, the Tropical Rainfall Measuring Mission (TRMM) (Goddard Earth Sciences Data and Information Services Center 2016). Despite the fact that TRMM is less accurate (Shari et al. 2016; Chen and Li. 2016; Wang et al. 2017) and its resolution coarser, it has been widely used since 1997. Its algorithm inter-calibrates all existing satellite microwave precipitation measures, microwave-calibrated infrared satellite estimates, and precipitation gauge analysis. The near-real-time data is chosen over the production data as it is recommended for flood and crop forecasting (NASA Precipitation Measurement Missions 2016). The instrumental variables approach is motivated by the fact that both satellite measures assign weather variables to grid points and contain measurement error in their 'true' representation of rainfall. In that case, the IV estimates can correct for measurement error bias under the assumption that errors in both variables are uncorrelated (Burgess et al. 2017).

Temperature - The daily temperature data also comes from NASA. We obtained near-surface air temperature (i.e., temperature at the height of most human activities) from the FLDAS Noah Land Surface Model (McNally 2016). The spatial resolution of this dataset is also $0.1\ ° \times 0.1\ °$, so ward-level daily temperature is computed similarly to rainfall above. Average weekly temperature is later computed at the weekly level.

Elevation - The elevation calculation is based on the Japan Aerospace Exploration Agency (JAXA) global digital surface model. The measurement is at 30-meter spatial resolution, based on the most precise global-scale elevation data at this time acquired by the Advanced Land Observing Satellite. Mean ward-level elevation is computed across all grids that fall inside each ward.

Flood-prone surface - To estimate the surface of a ward that is prone to flooding, we use data collected by the NGO Dar Ramani Huria (RH) in OpenStreetMap

(OSM) format. Using community-based mapping RH is able to create highly accurate maps of infrastructure and flood-prone areas in Dar es Salaam. We complement their detailed mapping of drainage, waterways, and wetlands with GeoFabrik's OSM data for missing wards. The data is less accurate but allows us to have a larger coverage. We then use InaSAFE[8] to model build-areas prone to inundation and calculate the total share of the ward area that is flood-prone. We compare our estimates to the more precise-ones of RH for available wards. The pairwise correlation is 0.81.

Basic summary statistics of weather and geographical variables are displayed in Table 4.2. The average weekly rainfall in Dar es Salaam according to the meteorological agency amounts to 20.6 mm. This is consistent with our weekly accumulation from both TRMM and GPM's measures. On average, in the period covered there were 20.1 mm of accumulated rainfall weekly, with a median of 2.9 mm. The rainiest month is usually April, which is seconded by our dataset. There is little spatial variation of temperature across the city's' wards, the average recorded weekly is 26.7^{o} C with a standard deviation of 0.37^{o} C. On average 10% of the area of a ward is prone to flooding, but there are significant disparities across wards (the standard deviation being 16%).

Infrastructure & Population

Infrastructure - Infrastructure data at the ward-level is also obtained from data collected by RH's in OSM format, and complemented with GeoFabrik's for missing wards. We focus on the following characteristics which are likely to be correlated with cholera incidence: drains, roads, foot-ways (i.e. unpaved roads) and water wells. For the first four variables, we use their density, calculated as the number of km per square km. Aside from roads where we can distinguish between roads and foot-ways, we have no specific measure of quality of the infrastructure. A general assumption is to think that a higher density of roads and drains reflect higher-quality infrastructure, while a higher density of foot-ways reflects lower-quality. The distinction in practice is hard to make, particularly for drains. Anecdotal evidence

[8] InaSAFE is a free software that produces realistic natural hazard impact scenarios. It was developed by the government of Indonesia, the Australian government and the World Bank. For more details see http://inasafe.org/ (last accessed on July 21st 2017).

suggests drains often get clogged by unregulated waste dumping due to heavy rainfall and quickly contaminate surfaces. We are thus agnostic concerning the expected signs of these coefficients. We have unfortunately no data on sewerages[9].

We obtained a dataset of formal and informal plots from the municipalities' database of surveyed plots, and are then able to estimate the share of the ward's area that houses informal settlements. Not all municipalities have mapped their informal plots fully[10] which explains the smaller sample when using this data. For the wards for which we have information, 34% of the total areas are on average informal. The large number reflects the fact that 70% of the population of Dar es Salaam lives in informal settlements.

Population- We make use of the population data from the Census 2012 to weight our regressions by ward population size. The interest in this is twofold. First, cholera incidence in wards with large populations is likely to be more precise, so weighting corrects for heteroskedasticity associated with these differences in precision (Burgess et al. 2017). Second, rather than on the average ward, the results reveal the impact on the average person, which is more meaningful here. We also use this data to calculate ward-level population density. The average ward of Dar es Salaam was populated with 48.5 thousand people in 2012; population density was 11.53 per square km (Table 4.1).

4.4 Empirical Strategy

In this section, we describe the econometric methods we use to estimate the effect of precipitation on cholera occurrence. As the relationship between rainfall and new cholera cases is expected to be non-linear, we adopt both parametric and flexible semi-parametric specifications. We begin by presenting specifications measuring the contemporaneous effect of precipitation. We then consider models allowing for the effect of rainfall to be associated with local public goods provision and other ward characteristics. We also assess the importance of the spatial spillovers of precipitation. Lastly, the last sub-section details a more general dynamic model

[9] Basic sanitation data in Table 4.1 is obtained from the 10% sample of the Census 2012. Unfortunately, these are only used in the descriptive section because of the lack of consistency in the sample.
[10] Only the Municipality of Kinondoni has.

including various precipitation time lags.

4.4.1 Contemporaneous Effects

To quantify the contemporaneous effect of rainfall on cholera incidence in any given ward and week, we begin by estimating a baseline panel log-linear model relating the logarithm of cholera cases[11] to weekly rainfall accumulation for this ward:

$$C_{wmt} = \alpha.R_{wt} + \gamma.T_{wt} + \mu_w + \delta_t + \theta_m.t + \varepsilon_{wmt} \tag{4.3}$$

where C_{wmt} is the outcome variable (log of total cholera cases) in ward w in week t. The key explanatory variable of interest is R_{wt} , measuring weekly accumulated rainfall. We also control for ward daily temperatures measured as weekly averages (T_{wt}) as temperature variation is likely to be correlated with rainfall variation. Since our focus is on precipitation and spatial variation in temperature in Dar es Salaam is limited, we model a linear temperature effect. The specification in equation (4.3) also includes a full set of ward fixed effects, μ_w , absorbing unobserved time-fixed ward idiosyncratic characteristics. Permanent differences in access to health care for instance will therefore not confound the estimates. Their inclusion also addresses the potential issue of sorting across neighborhoods. We also include week fixed effects, δ_t , to control for time-varying influences common across wards. The equation also includes municipality linear time trends to account for time-varying factors that differ across administrative boundaries and affect health. We also estimate equation (4.3) with municipality-week fixed effects to flexibly control for unobserved municipality-wide time shocks. We use only three municipalities in the analysis as these were the administrative divisions existing at the time of data collection. Further, the main three hospitals are located in these municipalities. As shown later, our estimations across these specifications are consistent and robust. ε_{wmt} is

[11] Since no cholera cases are recorded in several wards and weeks in our sample, we add one to all cholera cases and take the logarithm of that expression. In mathematical terms: . We test the results to linear regressions where the dependent variable is the ratio of cholera cases for every ten thousand people of the ward. Results are unchanged (see Appendix) and we prefer the specification that considers the non-linear relationship between rainfall and cholera.

an error term clustered at the ward level. Finally, we weight our regressions by ward population as explained earlier. Unweighted regressions are in Appendix. Results are unchanged.

To take into account non-linear relationships more rigorously, we also estimate contemporaneous rainfall effects using the following flexible model:

$$C_{wmt} = \sum_{k=1}^{4} \beta_k.1\{R_{wt} inquartilek\} + \phi.T_{wt} + \mu_w + \delta_t + \theta_m.t + \eta_{wmt} \tag{4.4}$$

where the independent variables we are mainly interested in capturing are the distribution of weekly rainfall in Dar es Salaam. The regressors 1{ R_{wt} in quartile k} calculate whether the total amount of rainfall R_{wt} in week t and ward w was in the first, second, third, or fourth quartile of the rainfall distribution of our study period. We estimate a separate coefficient on each of these quartile variables and treat the second quartile as the omitted reference category. The other regressors are as defined in equation (4.3). This approach has two benefits. The first one is to allow for more flexibility in the response function. The second one, more relevant here, is that it also allows us to specifically distinguish the effect of intense and light rain. The upper quartile (>75$^{\text{th}}$ percentile) is generally used as a proxy for flooding (Chen et al. 2017).

The parameters in equations (4.3) and (4.4) are thus identified from ward-specific deviations in rainfall from the ward average remaining after controlling for week fixed effects and municipality linear trends. Given the relatively short time period of analysis we argue that this variation is as good as exogenous and uncorrelated with other unobserved determinants of cholera incidence.

Equations (4.3) and (4.4) make several important assumptions about the effect of rainfall on cholera. First, they assume that the impact depends on weekly accumulation alone. It ignores the possibility of within week variation in rainfall having an effect on health. In addition, equation (4.4) assumes that the impact of rainfall is constant within a given quartile. While this might be restrictive, we estimate separate quartile coefficients to improve on equation (4.3) and its parametric assumptions. Third, by estimating contemporaneous effects, we assume that past weekly rainfall does not affect health outcomes. We also ignore the possibility of neighboring wards' rainfall influencing a given ward's cholera outcomes. We relax some of these assumptions in what follows.

A final concern is spatial dependence. In this case, within-cluster correlations in the specification of the error covariance matrix (i.e., standard-errors clustered at the ward level) may not be enough (Barrios et al. 2012). To account for this issue, we also compute equations (4.3) and (4.4) using Conley (1999) spatial standard-errors[12]. The implicit assumption is that spatial dependence is linearly decreasing in the distance from the wards centroids up to a cutoff distance, for which we chose 50 km based on Dar es Salaam's extent. This technique ensures that uncertainty in α and β is adjusted to account for heteroscedasticity, ward-specific serial correlation, and cross-sectional spatial correlation. Statistical significance is generally unchanged. We consider these results as robustness checks in Appendix.

We are interested in reduced-forms here. However, we are conscious that the true (unknown) relationship may include some time dependency in the dependent variable. That is, past cholera may determine contemporaneous cholera. To test the validity of our fixed-effects model, we compute a dependent-lagged model instead in Table A15 in Appendix. While we find the effect of lagged cholera cases significant, and positive up to 5 weeks, the size of the coefficient for contemporaneous precipitation always remain stable and statistically significant. Further, as mentioned, there is strong reason to believe contemporaneous rainfall is orthogonal to past cholera. In that case, including the lags would only increase the precision of our point estimates but should not alter the identifying assumptions.

4.4.2 Non-linear Effects and Spatial Spillovers

Ward characteristics, such as population density or the number of water wells, may affect the impact of rainfall on health as outlined in our theoretical framework. To account for this possibility, we estimate variations of equation (4.3) that include interactions between rainfall and ward features. While local public goods are not

[12] We use the Stata code developed by Fetzer (2010) and Hsiang (2010).

exogenously allocated to wards, there are several reasons to believe this is not a problem here. First, the use of ward fixed-effects should deal with neighborhood sorting. Further, the lack of proper infrastructure is widespread in Dar es Salaam and public health evidence suggests households from all income-levels may be affected by cholera. Using the 2015-16 Tanzania Demographic and Health Survey and Malaria Indicator Survey (DHS) we test the relationship between income, wealth, and incidence of diarrheal diseases in the city (Appendix). We find no evidence in favor of a wealth bias regarding the risk of contracting a diarrheal disease.

We also measure whether contemporaneous precipitation in neighboring wards affect cholera cases in a given ward. We focus on first contiguity wards and consider total neighboring accumulated rainfall to begin with. We then distinguish between rainfall recorded in uphill and downhill neighboring wards. We calculate the average elevation of each unit and classify as uphill the neighboring wards with a relatively higher elevation. Downhill neighboring wards have a lower or equal average elevation. Formally the model we estimate is as follows:

$$C_{wt} = \rho_1.R_{wt} + \rho_2.UR_{wt} + \rho_3.DR_{wt} + \pi.T_{wt} + \mu_w + \delta_t + \varsigma_{wt} \tag{4.5}$$

where UR_{wt} and DR_{wt} measure weekly accumulated rainfall in uphill and downhill neighbors, respectively. The other regressors are defined as in equations (4.3) and (4.4).

4.4.3 Dynamic effects

The empirical approaches discussed so far do not address the possibility of a dynamic relationship between precipitation and new cholera cases. Rainfall in one week might result in increased cholera incidence in the following weeks due its incubation period and the manner in which the disease spreads. This delayed response would imply that the contemporaneous estimates from equation (4.3) underestimate the true impact of rainfall. We investigate this possibility by including a distributed lag structure in our models:

$$C_{wt} = \sum_{j=0}^{J} \lambda_j.R_{wt-j} + \rho.T_{wt} + \mu_w + \delta_t + \zeta_{wt} \quad (4.6)$$

This model allows the effect of rainfall up to J weeks in the past to affect cholera incidence in a given week. In equation (4.6), the total dynamic effect of rainfall on cholera cases is obtained by summing the coefficients on the contemporaneous and lagged rainfall variables. Different lag structures potentially generate different estimates of the dynamic causal effect. As a consequence, we experiment with several time lags and use up to 5 lagged weekly accumulated precipitation in our regressions.

4.5 Main Results

This section presents our empirical results on the relationship between precipitation and cholera incidence. We begin with discussing baseline contemporaneous estimates of both rainfall and flooding. We then assess the importance of non-linear effects, spatial spillovers and measure dynamic effects last.

4.5.1 Baseline Effects

Our baseline results concern the effect of rainfall and precipitation on weekly-ward cholera occurrence. Tables 4.3-4.6 report baseline estimates of population-weighted regressions. Appendix additionally report unweighted regressions and results with Conley HAC standard-errors. Conclusions remain unchanged irrespective of the specification.

Table 4.3 reports estimates based on equation (4.3). The first column shows coefficients obtained with ward and week fixed effects only. Precipitation is found to have a positive and statistically significant effect on cholera. The point estimate suggests that a 10 mm increase in weekly accumulated rainfall causes a 2% increase in recorded cholera cases in a given ward. Including municipality linear trends does not affect the results much (column 2). Municipality-week fixed effects are controlled for instead in column 3. While the impact of precipitation remains statistically significant at the 1% level, its magnitude increases; that is, there are 3.4% additional cholera cases per ward every 10 mm increases in rainfall. Overall, these reduced form estimates consistently show a positive impact of precipitation on cholera incidence.

All subsequent tables are organized in the same fashion, with municipality trends added in column 2 and municipality-week fixed effects added in column 3. To test the sensitivity of our results to measurement error in the recorded rainfall data, we instrument our main precipitation variable with rainfall recorded by the TRMM satellite as explained in section 3.2.2. The potential sources of measurement error in these two datasets are likely to be unrelated, and therefore uncorrelated. Results are displayed in Table 4.4. The two satellite-based precipitation variables are strongly correlated, and first stage F statistics range between 24 and 37 across specifications (see fourth row). Our two-stages least squares coefficient estimates remain positive but become larger as attenuation bias theory would predict. Including municipality-week fixed effects results in a loss of statistical significance (column 3). The first stage F-statistic is also lower however, inflating standard errors to some degree. On the whole, our findings are supported by the IV results. There is a strong positive relationship between cholera occurrence in a given ward and precipitation. In the interest of proceeding conservatively we continue to stress the OLS results hereafter, but Table 4.4 suggests that the true impact of precipitation on cholera may be even larger.

Table 4.5 explores the impact of rainfall using the more flexible quartile specification detailed in equation (4.4). The second rainfall quartile (light rain or precipitation between 0 to 2.9 mm weekly) is used as omitted category. Notably, the semi-parametric relationship between weekly accumulated rainfall and cholera occurrence show particularly large effects at the upper-end of the rainfall distribution. Indeed, the estimated coefficients in the three columns consistently indicate that extreme precipitation has a strong impact on cholera incidence. For instance, a single additional week with recorded rainfall falling in the fourth quartile ($>75^{\text{th}}$ percentile, between 26.9 mm and 408.6 mm weekly), relative to a week with light rain, increases the number of new cholera cases by 20.3% (column 2). The first quartile coefficients, measuring loosely speaking the effect of a dry week relative to little rainfall, are positive but not statistically significant. These results are key findings in our paper. Clearly, extreme rainfall has a higher incidence on ward-level cholera occurrence than light rain, suggesting not all ranges of precipitation are necessarily related to cholera occurrence. Moreover, upper-quartile rain has been consistently used in the literature as a proxy for flooding (Chen et al. 2017), and implies water stagnation may be a likely mechanism.

To explore further the role of extreme precipitation, Table 4.6 puts the emphasis on flooding and attempts to measure its impact in various ways. We begin with assessing whether the impact of rainfall is non-linear and depends on the extent to which a ward is prone to flooding. We use our measure of the share of the ward that is subject to flooding and interact it with weekly accumulated rainfall. Our results presented in panel A show a positive interaction term as theory would predict. The interaction is non-statistically significant however. The coefficient of the uninteracted precipitation measure remains in the same order of magnitude as the coefficients of Table 4.3. In panel B we measure the effect of the fourth quartile precipitation relative to the rest of the precipitation distribution. Here flooded is a dummy variable for weekly accumulated rainfall falling on the upper-quartile of the overall rainfall distribution. Our estimates are positive, significant at the 1% level, and stable across alternative specifications. In panel C we interact the flooded dummy with our flood-prone area share defined as above. The interaction term is now positive and significant at 5% level, implying that the impact of heavy rainfall is much higher in wards at greater risk of flooding all else equal.

Overall, the results of this section support the theoretical mechanisms described in section 2 and the channels put forward in the public health literature. There are various reasons why heavy rainfall and flooding could lead to an increase in cholera, as mentioned earlier. Not only the bacterium survives longer in wet humid surfaces, but the risk of increased contamination is higher. The inundation of drains, water systems, and pit latrines, greatly enhances the probability of exposure to contaminated water and food. Further, behavioral changes during periods of weather shocks may also increase the probability of contagion (WHO). Finally, indirect mechanisms through income-shocks due to the inundation of job locations or inaccessibility to the work-place may further contribute to the adoption of risky behavior.

4.5.2 Non-Linear Effects: A Story of Infrastructure Quality?

We now explore further the relationship between rainfall and cholera incidence and assess potential non-linearities related to ward-level characteristics. As explained above, the size of the weather shock in a given ward is likely to depend on the quality of the infrastructure such as the availability of well-functioning drains, paved roads and improved sanitation. This section concentrates on the correlation that rainfall and several indicators of ward infrastructure and 'neighborhood quality' has with cholera incidence. Our choice of ward characteristics is in part dictated by data availability. We focus on population density, road density, as well as the density of drains and foot-ways, and the number of water wells. We also include the percentage of the ward's area that hosts informal and formal housing.

As mentioned earlier, we are constrained when it comes to measuring the quality of infrastructure and focus on quantity when no distinction is possible. Because of this, while we expect higher population density to increase the measured effect of rainfall on cholera through a heightened risk of contagion, we are agnostic with respect to the influence of road and water infrastructure measures. On the one hand, greater physical supply of water wells and drains could be negatively associated with cholera by efficiently evacuating used-water and rain. On the other hand, it could magnify the impact of heavy precipitation on cholera when the quality is low, for instance if because of unregulated dumping, drains and evacuation canals are clogged in times of heavy rain.

Table 4.7 reports baseline estimates of population-weighted regressions. Estimates of unweighted regressions are in Appendix Table 5, and results with Conley HAC standard-errors are in Appendix Tables 10. The size of the coefficients is stable across our different specifications. Further, contemporaneous precipitation remains consistently positive and statistically significant at between 1 to 5% levels.

The first seven columns of the tables separately estimate each interaction term, while in the last three columns we estimate all interactions jointly. Since we lose a large number of observations when we include certain interactions, we report results using three alternative samples. Overall, almost all characteristics considered individually are positive and significant at various levels of significance. Yet, only foot-way densities and housing informality are consistently so across the different specifications. The mechanisms here are intuitive. Foot-ways are unpaved roads. Contaminated water might stagnate easier in muddy surfaces. At the same time, foot-ways could just reflect informality. Indeed, informality displays the larger size, with on average weekly accumulated rainfall increasing cholera incidence by 2.3% to 4.5% more in wards with higher shares of informal housing.

Once we introduce all interaction terms together in columns (8) to (10) significance and signs are considerably changed suggesting individual interactions may be suffering from omitted variable bias. Nonetheless, some important patterns remain. First, the only consistently positive and statistically significant coefficient (at 5% level) across the various specifications is the non-linear informal housing correlation. While, the sample size is much smaller, results suggest living on informal housing *increases* cholera incidence due to weekly accumulated rainfall by between 2.3% and 2.7% . This finding is far from surprising. Informal settlements are usually located in flood-prone areas. They suffer from poor quality infrastructure and deprived water and sanitation conditions. Penrose et al. (2010) find similar patterns when investigating a previous cholera episode in Dar es Salaam. Two other infrastructure interactions display stable sizes and signs. The non-linear water wells correlation is negative but almost never statistically significant. Related to informality, wards with a higher density of foot-ways have again a higher likelihood of accumulated rainfall affecting weekly cholera incidence. The size is small (between 0.2% to 2%) and fails to be statistically significant.

These results support the theoretical mechanisms outlined earlier. First, informal housing and unpaved roads increase the effect of the weather shock. They are thus likely to affect individuals directly and indirectly through health and productivity shocks. Our results so far, despite being imperfectly measured, suggest the quality of infrastructure is highly correlated with the detrimental effect accumulated rainfall and flooding have on cholera prevalence.

4.5.3 Spatial Spillovers: Neighbors Contagion.

Next, we focus on spatial precipitation spillovers as in equation (4.5). Precipitation recorded in adjacent wards might exacerbate pressure on water infrastructures. They might also contaminate common water sources, particularly if wards are at different levels of elevation. In this case, uphill rainfall may also wash down contaminated waste or soil material, harming wards below. Table 4.8 contains our baseline results, while Appendix Tables 6 and 13 contain the usual alternative specifications.

We first estimate the average effect of weekly accumulated rain in neighboring wards on the cholera incidence of a given ward. We do not find evidence of an effect here. The estimated coefficients are almost no different from zero and insignificant across econometric specifications. In Appendix Table 11 we further look at the effect of one and two-weeks lags of neighboring rainfall. None of these spatial lagged variables seem to matter.

We then differentiate between rainfall accumulation in uphill and downhill neighboring wards. Results here are more nuanced. We register a small but significant effect from accumulated weekly precipitation in adjacent downhill wards. That is, a 10 mm increase in weekly accumulated rainfall in downhill neighboring wards increases cholera cases in the ward by 0.01% to 0.03% . This finding is consistent with water source contamination from relatively lower wards. The size is negligible suggesting almost no spatial spillovers from rainfall in contiguous areas. Further, precipitation in one's own ward is almost always significant at 1% level, retaining the size of baseline estimates. Again allowing for time lags yields no significant effect (Table A12).

Failing to detect any spatial spillovers of precipitation in contiguous wards is unexpected. It suggests only local contamination prevails. This is consistent with findings in Ambrus et al. (2015) on the 1854 cholera epidemic in London's Soho neighborhood. Their identification strategy and results suggest cholera is contained within a very specific area. Implications concerning channels of transmission are many but go beyond the scope of this paper.

4.5.4 Time Dynamic Effects.

So far, we have not taken into account the possibility of a dynamic relationship between rainfall and cholera incidence. If cholera responds to precipitation with a delay, that is, if precipitation in previous weeks or days also impacts cholera in the current week, the estimates of Table 4.3 could underestimate the true effect. While cholera symptoms can manifest 12 hours after an individual being in contact with the bacteria, they can also take up to 5 days. These might not coincide with our weekly definition. Further, rainfall is easily stored and stagnation from previous weeks may contribute heavily to contagion.

To test for dynamic effects we estimate distributed lag models (equation 4.6) and allow rainfall to affect health up to five weeks later. The sixth lag of rain (not shown) is not statistically significant. We also report the contemporaneous coefficient and the sum of the six week period. Table 4.9 displays our point estimates. We gradually introduce additional rainfall lags in our model, which includes municipality-week fixed effects.

First, this exercise allows us to confirm that including time-lags does not change our conclusions. The contemporaneous rainfall effect on cholera remains in the same order of magnitude as in Table 4.3, close to 3% and statistically significant at 1% level. Second, the results in the table clearly show that past rainfall up to five preceding weeks impact cholera incidence in the current week. All lags decrease in size the farther in time, suggesting the contemporaneous effect matters most. The total effect of precipitation is obtained by summing the coefficients on the contemporaneous and lagged precipitation variables. The total cumulated impact amounts to 0.12 points (last row). That is, six-week cumulated rainfall increases current cholera incidence in a ward by up to 12% . The key message of this table is that weekly cumulated rainfall promotes cholera occurrence immediately and with a lag of up to 5 weeks.

We repeat the exercise with our more flexible semi-parametric specification in Table 4.10 (quartiles). The table is as Table 4.9 except that we only include lags up to two weeks later. The third lags (not shown) are not statistically significant.

Again, the predominant effect is that of extreme rainfall or flooding captured in the upper-quartile, and up to two prior weeks. As before, all lags decrease in size the farther in time, suggesting the contemporaneous effect matters most. Further, we confirm that the inclusion of the lags does not affect our conclusions as the size and statistical significance are unchanged for contemporaneous coefficients. The total cumulative effect of each quartile is also computed in the last row. The total cumulated impact amounts to 0.37 points for the upper-quartile. It is interesting to highlight that the size of the two-week lag of the first quartile (no rain) remains positive but increases in size. It is even statistically significant at 10% level for one specification. This supports theories according to which dryness also matters for cholera incidence by increasing the risk of drinking unsafe water. The time lag is consistent with this type of behavioral changes.

4.6 Conclusion

Rapid urbanization in developing countries has often led to unplanned cities, particularly in sub-Saharan Africa, with large shares of the urban population living in informal settlements, with poor transport infrastructure and limited access to water and sanitation. Under these conditions, developing countries' city dwellers have become more vulnerable to disease transmission and epidemics. Global warming is expected to exacerbate these health-related risks. The World Bank (2016) estimates that climate change may push up to 77 million more urban residents into poverty by 2030. As extreme weather events become more frequent, understanding the relationship between disease transmission, infrastructure quality and weather shocks in urban areas is important. We make significant advances on this issue.

The key contribution of this paper has been to show that heavy rainfall has a strong positive effect on weekly cholera incidence within wards. We assemble a panel dataset defined at the ward level containing weekly information on cholera incidence, precipitation, and infrastructure quality from various sources. On average, we find that a 10 mm increase in weekly accumulated precipitation leads to an increase of up to 3.5% of weekly recorded cholera cases. Extreme rainfall has a larger impact: a single additional week of rainfall falling above the 75th percentile of the total rainfall distribution increases the number of effective cholera cases by up to 20.3% relative to a week with very light rain. The impact is even higher in wards at greater risk of flooding.

Results in the paper also emphasize the key role of local infrastructure. We find the effect of weekly rainfall on cholera cases to be consistently higher in wards with larger shares of informal housing and a higher density of foot-ways (i.e. informal roads). These results are in line with the mechanisms outlined. Neighborhoods with low-quality infrastructure are likely to be more exposed to the cholera bacteria when surfaces are washed and drains are overflown by severe precipitation. Vulnerable populations in these wards are also more likely to suffer from negative income shocks during extreme weather events.

Findings here have important policy implications. Cities in developing countries need to address infrastructure gaps to contain the risk of recurrent epidemic outbreaks in fragile environments. Policies that improve the quality of local infrastructures and housing conditions should mitigate the negative impact of rainfall on health. Given the transmission channels of cholera, the proper servicing of informal areas, including sewerages connections and the pavement of informal roads, as well as the regulation of waste-dumping, may prove to be more beneficial in the long-term than the use of short-term palliative measures during outbreaks. Interventions improving access to drinking water as well as access to sanitation should also greatly reduce cholera risk. In the theoretical framework considered, governments can also reduce the adverse effect of weather on health outcomes by supporting households in periods of health-shocks through subsidized health goods or direct transfers. These policies also need to be taken into account given the large room for increasing social safety nets in urban areas. Evidence on large-scale policy interventions in urban areas are limited and more is needed to understand priority-investments that increase resilience and prevent contagion of treatable diseases in developing cities if these are to become engines of growth (Glaeser 2011).

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Figures

Figure 4.1: Distribution of cholera effective cholera cases (epidemic weeks)

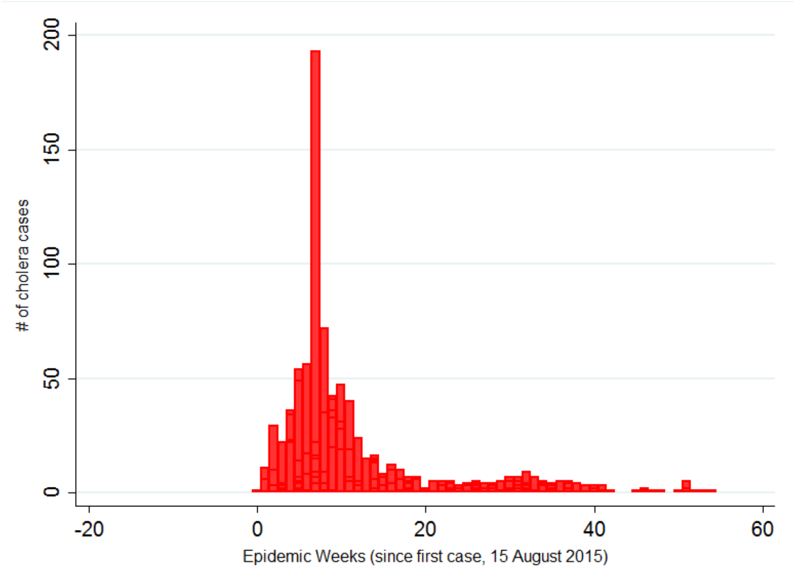


Figure 4.2: Distribution of effective cholera cases, by age (epidemic weeks)

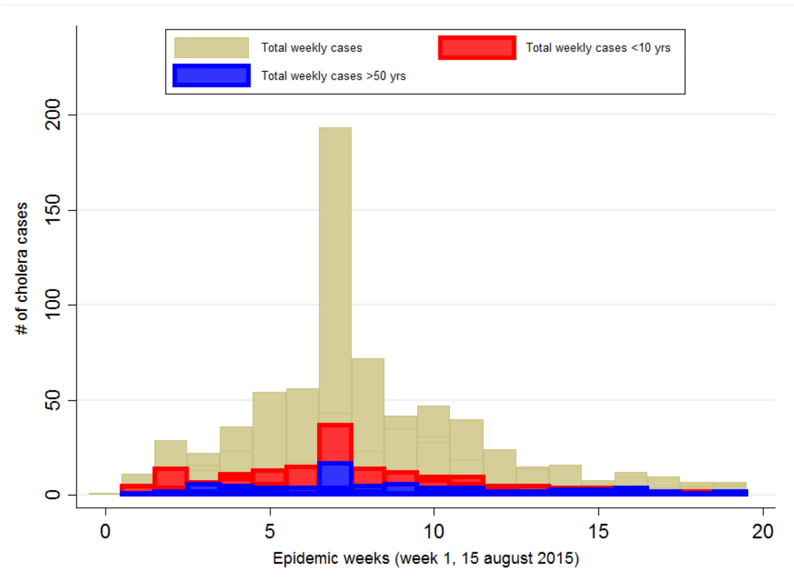
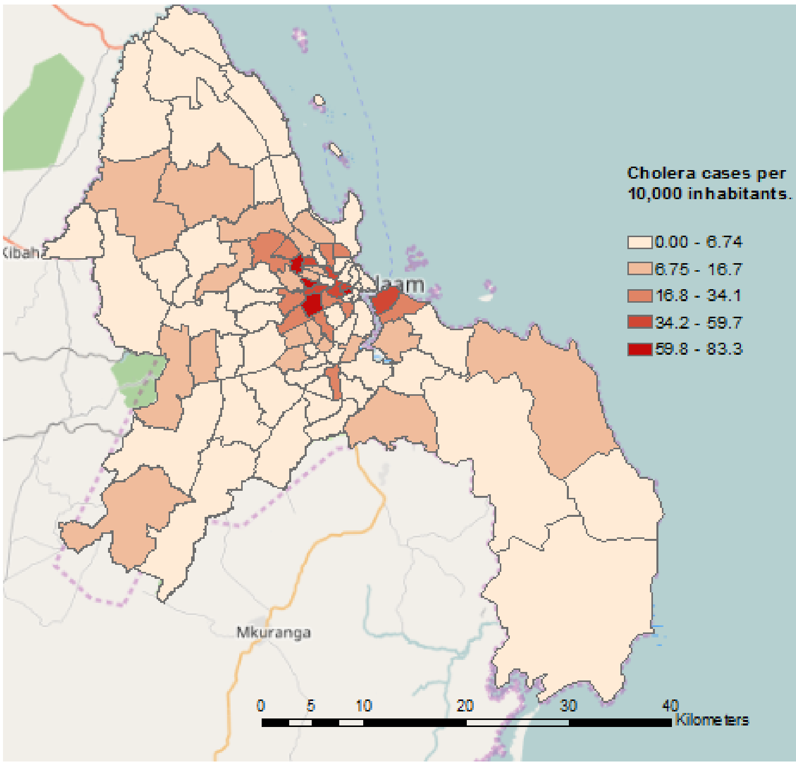


Figure 4.3: Spatial distribution of cholera incidence, total outbreak (cases per 10,000 inhabitants)



Tables

Table 4.1: Summary Statistics: Ward characteristics

	Mean	**Std. Dev.**	**Min.**	**Max.**	**N**
Area (km^2)	18.12	31.15	0.414	209.55	90
Pop (c2012)	48,495	26,064	6,411	106,946	90
HH size (c2012)	4.00	0.21	3.60	4.40	90
Density (km^2)	11.53	11.12	0.05	46.74	90
Improved sanitation	0.08	0.10	0.00	0.48	90
Electricity	0.06	0.08	0.00	0.37	90
Drinking water	0.07	0.08	0.00	0.42	90
Hospital per 10tho.	0.19	0.43	0.00	2.97	90
Density of roads*	4.00	3.84	0.00	18	88
Density of foot-ways*	8.83	15.76	0.01	75.99	63
Density of waterways*	5.00	5.61	0.00	24.81	78
Density of drains*	3.28	2.66	0.02	11.04	45
# water wells	1.25	2.99	0.00	21	76
% area informal	34	28	0.00	88	23

Notes: c2012 refers to data from census 2012, all of the infrastructure density measures (*) are measured in km per square km

Table 4.2: Summary Statistics: Weather and Cholera

	Mean	**Std. Dev.**	**Min.**	**Max.**	**N**
Weather:					
% flooded area	10.00	16.00	0.00	73.00	90
Average temperature (C)	26.73	0.37	25.43	27.17	90
Total rainfall (10mm)	162.36	18.15	134.27	216.42	90
Average weekly temperature (C)	26.73	1.57	22.92	29.99	6930
Weekly rainfall accumulation (10mm), GPM	2.11	4.05	0.00	40.86	6930
Weekly rainfall accumulation (10mm), TRMM	2.67	5.53	0.00	44.62	6930
Cholera:					
Total cases 2015-2016	63.32	94.01	0.00	588	90
Total weekly cases per ward (excl. neg)	0.72	3.84	0.00	192	6930
Total weekly cases female	0.36	1.85	0.00	90	6930
Total weekly cases below 5 yrs	0.08	0.49	0.00	14	6930
Total weekly cases tested neg	0.11	0.56	0.00	20	6930
Total effective cases epiweek10	3.16	9.40	0.00	192	890
Total effective cases epiweek20	2.54	7.25	0.00	192	1780
Total effective cases epiweek30	1.77	6.03	0.00	192	2670

Notes: Temperature are degrees Celsius; all measures of rainfall are accumulated rainfall (units: 10mm), cholera cases are total numbers. 88 of 90 wards where affected throughout the outbreak.

Table 4.3: Impact of Weekly Precipitation on Cholera Incidence

	Cholera cases (log)		
	(1)	(2)	(3)
Precipitation	0.0198***	0.0208***	0.0344***
	(0.0072)	(0.0073)	(0.0077)
N	6930	6930	6930
R^2	0.4491	0.4502	0.5254
Ward FE	Yes	Yes	Yes
Week FE	Yes	Yes	
Municipal time trend		Yes	
Municipality × week FE			Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature; they are weighted by the population of the ward (census 2012). The period covered is from the first week of March 2015 to the first week of September 2016. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table 4.4: Impact of Weekly Precipitation on Cholera Incidence (Instrumental Variable Estimates)

	Cholera cases (log)		
	(1)	(2)	(3)
Precipitation	0.0689**	0.0779**	0.0451
	(0.0341)	(0.0356)	(0.0476)
N	6930	6930	6930
First Stage F-test	36.812	35.354	24.202
Ward FE	Yes	Yes	Yes
Week FE	Yes	Yes	
Municipal time trend		Yes	
Municipality × week FE			Yes

Notes: Robust standard errors in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016; they are weighted by the population of the ward (census 2012). Nasa GPM v3 precipitation measurement is instrumented with NASA TRMM measurement. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table 4.5: Impact of Weekly Quartiles of Precipitation on Cholera Incidence

	Cholera cases (log)		
	(1)	(2)	(3)
Quartile 1	0.0010	0.0040	0.0049
	(0.0197)	(0.0193)	(0.0177)
Quartile 3	-0.0360	-0.0286	-0.0536*
	(0.0324)	(0.0329)	(0.0317)
Quartile 4	0.1867***	0.2032***	0.1525**
	(0.0594)	(0.0610)	(0.0611)
N	6930	6930	6930
R^2	0.4510	0.4522	0.5254
Ward FE	Yes	Yes	Yes
Week FE	Yes	Yes	
Municipal time trend		Yes	
Municipality $\times$ week FE			Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature; they are weighted by the population of the ward (census 2012). The period covered is from the first week of March 2015 to the first week of September 2016. The quartiles of the rainfall distribution are defined as follows: Q1 (0mm), Q2(0-0.29mm), Q3(0.29-2.69mm), Q4(2.69-40.86mm). *$p \leq 0.10$ ** $p \leq 0.05$ *** $p \leq 0.01$

Table 4.6: Impact of Flooding on Cholera Incidence

	Cholera cases (log)		
	(1)	(2)	(3)
Panel A:			
Precipitation	0.0192***	0.0204***	0.0340***
	(0.0071)	(0.0073)	(0.0077)
Precipitation × % Flood-prone area	0.0091*	0.0076	0.0043
	(0.0048)	(0.0046)	(0.0047)
Panel B:			
Flooded (precipitation ≥75th p)	0.2189***	0.2280***	0.2029***
	(0.0485)	(0.0487)	(0.0498)
Panel C:			
Flooded (precipitation ≥75th p)	0.2007***	0.2103***	0.1970***
	(0.0470)	(0.0472)	(0.0517)
Flooded × % Flood-prone area	0.2262**	0.2176**	0.2076**
	(0.1019)	(0.1007)	(0.1028)
N	6930	6930	6930
Ward FE	Yes	Yes	Yes
Week FE	Yes	Yes	
Municipal time trend		Yes	
Municipality × week FE			Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. All panels are independent regressions. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. Flooded is a dummy variable for weekly precipitation falling above the 75th percentile of the total rainfall distribution. Flood-prone area is the total area of the ward that is prone to flooding. All regressions control for weekly ward air temperature;they are weighted by the population of the ward (census 2012). The period covered is from the first week of March 2015 to the first week of September 2016. *$p \leq 0.10$ ** $p \leq 0.05$ *** $p \leq 0.01$

Table 4.7: Impact of Weekly Precipitation on Cholera Incidence: Infrastructure & Ward Characteristics

	Cholera cases (log)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Precipitation	0.0364***	0.0355***	0.0439***	0.0354***	0.0668**	0.0023	0.0525***	0.0518***	0.0696**	0.0058
	(0.0087)	(0.0080)	(0.0116)	(0.0079)	(0.0296)	(0.0135)	(0.0179)	(0.0126)	(0.0314)	(0.0244)
Precipitation × Pop. density	0.0002**							0.0000	-0.0002	-0.0007
	(0.0001)							(0.0002)	(0.0003)	(0.0004)
Precipitation × Roads density		0.0017*						0.0007	0.0019	-0.0054
		(0.0009)						(0.0016)	(0.0020)	(0.0066)
Precipitation × Footways density			0.0020*					0.0023	0.0033	0.0054
			(0.0011)					(0.0021)	(0.0028)	(0.0054)
Precipitation × # Water wells				0.0009				-0.0002	-0.0000	-0.0014
				(0.0007)				(0.0012)	(0.0013)	(0.0023)
Precipitation × Drains density					0.0018				0.0002	0.0039
					(0.0016)				(0.0021)	(0.0032)
Precipitation × % Informal housing						0.0168*				0.0264**
						(0.0083)				(0.0092)
Precipitation × % Formal housing							0.0032			
							(0.0038)			
N	6930	6776	4851	5852	3465	1771	2618	4004	2926	1463
R^2	0.5229	0.5335	0.5703	0.5370	0.6182	0.6606	0.5383	0.5860	0.6398	0.6768
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE										
Municipality × week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Separate regressions in columns (1-7). Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature; they are weighted by the population of the ward (census 2012). The period covered is from the first week of March 2015 to the first week of September 2016. Population density is the number of inhabitants per square km (census 2012), Roads density, foot-way density, and drains density are the meters of roads, foot-ways and drains per km (OSM), % informal and formal houses in the ward are obtained from surveyed plots (not all plots are surveyed). *p ≤ 0.10 ** p≤0.05 *** p≤0.01

Table 4.8: Impact of Neighbors' Weekly Precipitation on Cholera Incidence: Spatial Spillovers

	Cholera cases (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Precipitation	0.0183**	0.0192***	0.0196***	0.0204***	0.0316***	0.0320***
	(0.0071)	(0.0070)	(0.0073)	(0.0072)	(0.0077)	(0.0076)
Neighbours precipitation	0.0004		0.0004		0.0009	
	(0.0007)		(0.0007)		(0.0008)	
Uphill neighbours precipitation		-0.0007		-0.0007		0.0001
		(0.0010)		(0.0009)		(0.0009)
Downhill neighbours precipitation		0.0011		0.0010		0.0013*
		(0.0007)		(0.0007)		(0.0007)
N	6930	6930	6930	6930	6930	6930
R^2	0.4491	0.4494	0.4502	0.4505	0.5255	0.5256
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes		
Municipal time trend			Yes	Yes		
Municipality × week FE					Yes	Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature; they are weighted by the population of the ward (census 2012). The period covered is from the first week of March 2015 to the first week of September 2016. Neighbors' precipitation measures weekly accumulated rainfall in a neighboring ward. Uphill and downhill measures are for neighboring wards at a higher or lower elevation than the given ward. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table 4.9: Dynamic Effects: Lags of Weekly Precipitation on Cholera Incidence

	Cholera cases (log)				
	(1)	(2)	(3)	(4)	(5)
Precipitation	0.0337***	0.0313***	0.0300***	0.0305***	0.0298**
	(0.0075)	(0.0071)	(0.0073)	(0.0073)	(0.0072)
Precipitation $_{(w-1)}$	0.0249***	0.0243***	0.0214***	0.0200**	0.0205**
	(0.0086)	(0.0085)	(0.0081)	(0.0082)	(0.0083)
Precipitation $_{(w-2)}$		0.0222***	0.0215***	0.0195***	0.0185**
		(0.0071)	(0.0071)	(0.0068)	(0.0070)
Precipitation $_{(w-3)}$			0.0256***	0.0252***	0.0240***
			(0.0077)	(0.0076)	(0.0073)
Precipitation $_{(w-4)}$				0.0182**	0.0180**
				(0.0076)	(0.0075)
Precipitation $_{(w-5)}$					0.0113*
					(0.0061)
Cumulative (6 weeks)					0.1221***
					(0.0288)
N	6840	6750	6660	6570	6480
R^2	0.5263	0.5267	0.5274	0.5275	0.5270
Ward FE	Yes	Yes	Yes	Yes	Yes
Municipality × week FE	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature; they are weighted by the population of the ward (census 2012). The period covered is from the first week of March 2015 to the first week of September 2016. Precipitation $_{w-n}$ are the lags of weekly precipitation up to n weeks. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table 4.10: Dynamic Effects: Lags of Quartiles of Weekly Precipitation on Cholera Incidence

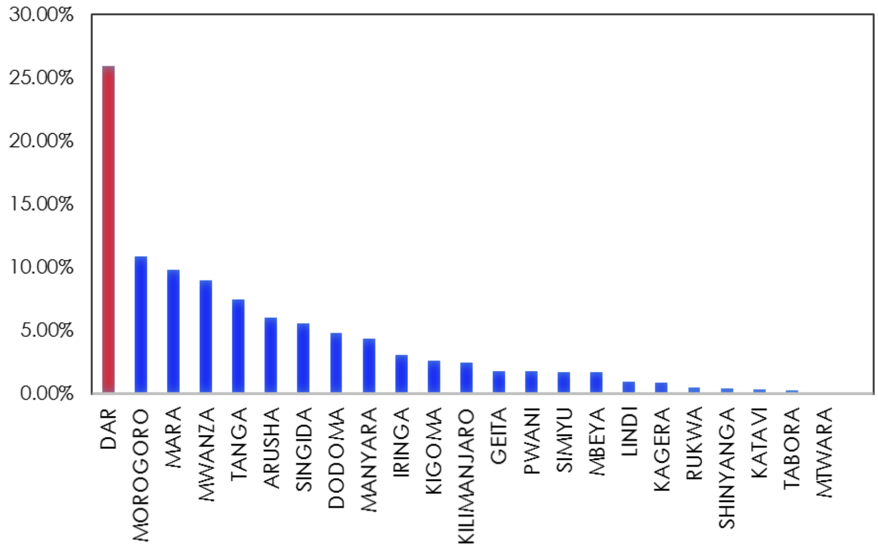
	Cholera cases (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Q1	0.0013	-0.0020	0.0039	0.0005	0.0061	0.0058
	(0.0194)	(0.0190)	(0.0190)	(0.0187)	(0.0175)	(0.0171)
Q3	-0.0335	-0.0372	-0.0262	-0.0301	-0.0519*	-0.0551*
	(0.0309)	(0.0300)	(0.0315)	(0.0307)	(0.0300)	(0.0296)
Q4	0.1825***	0.1753***	0.1982***	0.1908***	0.1504**	0.1442**
	(0.0559)	(0.0538)	(0.0578)	(0.0556)	(0.0574)	(0.0554)
$Q1_{w-1}$	-0.0034	-0.0066	0.0004	-0.0030	-0.0049	-0.0071
	(0.0218)	(0.0222)	(0.0220)	(0.0224)	(0.0198)	(0.0202)
$Q3_{w-1}$	-0.0198	-0.0215	-0.0137	-0.0154	-0.0227	-0.0271
	(0.0352)	(0.0346)	(0.0350)	(0.0344)	(0.0314)	(0.0302)
$Q4_{w-1}$	0.1212**	0.1139**	0.1355**	0.1277**	0.1368**	0.1256**
	(0.0590)	(0.0565)	(0.0592)	(0.0569)	(0.0605)	(0.0566)
$Q1_{w-2}$		0.0312		0.0347*		0.0158
		(0.0192)		(0.0192)		(0.0206)
$Q3_{w-2}$		0.0320		0.0372		0.0495
		(0.0356)		(0.0357)		(0.0352)
$Q4_{w-2}$		0.0903*		0.1029*		0.1052*
		(0.0543)		(0.0549)		(0.0568)
Q1 (Cumulative 3 weeks)						0.0145
						(0.350)
Q3 (Cumulative 3 weeks)						-0.0327
						(-0.490)
Q4 (Cumulative 3 weeks)						0.3750***
						(0.1348)
N	6930	6930	6930	6930	6930	6930
R^2	0.4522	0.4526	0.4535	0.4540	0.5267	0.5271
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes		
Municipal time trend			Yes	Yes		
Municipality × week FE					Yes	Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature; they are weighted by the population of the ward (census 2012). The period covered is from the first week of March 2015 to the first week of September 2016. The quartiles are defined as in Table 4. Quartile_{w-n} are the lags of the quartiles of weekly precipitation up to n weeks. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Appendix

Figures

Figure A1. Cholera cases during 2015-2016 outbreak, by region



Notes: Data obtained from the Red Cross. Total cases (vs. effective in analysis) up until April 2016.

Notes: There are currently 5 municipal districts in Dar es Salaam. Here we use the three that existed when the cholera outbreak started and at the levels at which the data was collected.

Figure A2. Distribution of effective cholera cases (epidemic week), by district municipality

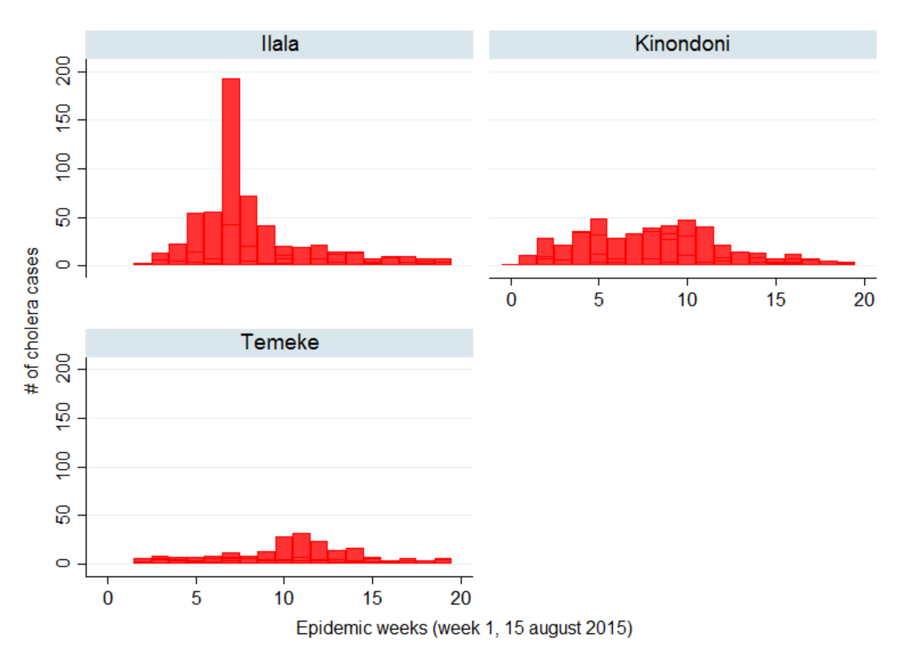
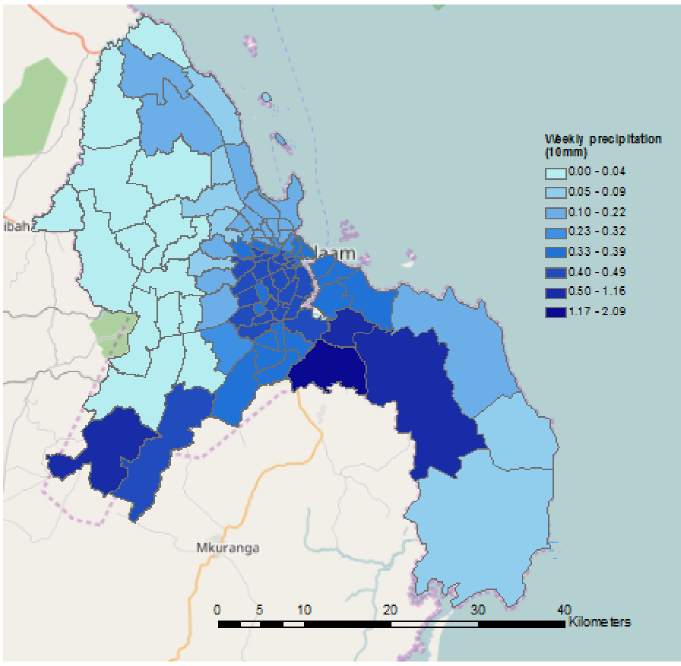


Figure A3. Ward-level weekly rainfall accumulation (area-weighted)



Tables

Unweighed Main Regressions

Table A1: Impact of Weekly Precipitation on Cholera Incidence (unweighted)

	Cholera cases (log)		
	(1)	(2)	(3)
Precipitation	0.0157**	0.0165***	0.0231***
	(0.0060)	(0.0061)	(0.0063)
N	6930	6930	6930
R^2	0.4049	0.4057	0.4638
Ward FE	Yes	Yes	Yes
Week FE	Yes	Yes	
Municipal time trend		Yes	
Municipality × week FE			Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table A2: Impact of Weekly Precipitation on Cholera Incidence (IV Estimates, unweighted)

	Cholera cases (log)		
	(1)	(2)	(3)
Precipitation	0.0431**	0.0477**	0.0394
	(0.0188)	(0.0194)	(0.0266)
N	6930	6930	6930
First Stage F-test	40.822	40.21	27.985
Ward FE	Yes	Yes	Yes
Week FE	Yes	Yes	
Municipal time trend		Yes	
Municipality × week FE			Yes

Notes: Robust standard errors in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. Nasa GPM v3 precipitation measurement is instrumented with NASA TRMM measurement. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table A3: Impact of Weekly Quartiles of Precipitation on Cholera Incidence (unweighted)

	Cholera cases (log)		
	(1)	(2)	(3)
Q1	-0.0089	-0.0058	-0.0030
	(0.0160)	(0.0157)	(0.0155)
Q3	-0.0301	-0.0250	-0.0359
	(0.0264)	(0.0265)	(0.0269)
Q4	0.1592***	0.1706***	0.1521***
	(0.0521)	(0.0528)	(0.0565)
N	6930	6930	6930
R^2	0.4065	0.4074	0.4645
Ward FE	Yes	Yes	Yes
Week FE	Yes	Yes	
Municipal time trend		Yes	
Municipality × week FE			Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. The quartiles of the rainfall distribution are defined as follows: Q1 (0mm), Q2(0-0.29mm), Q3(0.29-2.69mm), Q4(2.69-40.86mm)*p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table A4: Impact of Flooding on Cholera Incidence (unweighted)

	Cholera cases (log)		
	(1)	(2)	(3)
Panel A:			
Precipitation	0.0154**	0.0162***	0.0227***
	(0.0060)	(0.0061)	(0.0062)
Precipitation × % Flood-prone area	0.0079	0.0066	0.0059
	(0.0051)	(0.0050)	(0.0052)
Panel B:			
Flooded (precipitation ≥75th p)	0.1865***	0.1930***	0.1856***
	(0.0424)	(0.0425)	(0.0456)
Panel C:			
Flooded (precipitation ≥75th p)	0.1688***	0.1757***	0.1658***
	(0.0420)	(0.0421)	(0.0451)
Flooded × %Flood-prone area	0.2181***	0.2113***	0.2136**
	(0.0795)	(0.0786)	(0.0879)
N	6930	6930	6930
Ward FE	Yes	Yes	Yes
Week FE	Yes	Yes	
Municipal time trend		Yes	
Municipality × week FE			Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. All panels are independent regressions. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. Flooded is a dummy variable for weekly precipitation falling above the 75th percentile of the total rainfall distribution. Flood-prone area is the total area of the ward that is prone to flooding. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016.*p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table A5: Impact of Weekly Precipitation on Cholera Incidence: Infrastructure & Ward Characteristics (unweighted)

	Cholera cases (log)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Precipitation	0.0238***	0.0243***	0.0246**	0.0221***	0.0245	-0.0008	0.0509***	0.0242*	0.0258	0.0063
	(0.0069)	(0.0067)	(0.0121)	(0.0064)	(0.0225)	(0.0128)	(0.0173)	(0.0138)	(0.0253)	(0.0221)
Precipitation × Pop. density	0.0002***							0.0001	0.0000	-0.0008
	(0.0001)							(0.0002)	(0.0003)	(0.0005)
Precipitation × Roads density		0.0014						0.0007	0.0024	-0.0055
		(0.0009)						(0.0016)	(0.0020)	(0.0064)
Precipitation × Footways density			0.0027**					0.0023	0.0024	0.0063
			(0.0012)					(0.0019)	(0.0023)	(0.0055)
Precipitation × # Water wells				0.0013*				-0.0004	-0.0001	-0.0015
				(0.0007)				(0.0012)	(0.0013)	(0.0023)
Precipitation × Drains density					0.0029*				0.0006	0.0046
					(0.0015)				(0.0019)	(0.0038)
Precipitation × % Informal housing						0.0178**				0.0233**
						(0.0072)				(0.0083)
Precipitation × % Formal housing							0.0023			
							(0.0038)			
N	6930	6776	4851	5852	3465	1771	2618	4004	2926	1463
R^2	0.4633	0.4728	0.5160	0.4668	0.5534	0.6435	0.5398	0.5303	0.5858	0.6693
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE										
Municipality × week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Separate regressions in columns (1-7). Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. Population density is the number of inhabitants per square km (census 2012), Roads density, foot-way density, and drains density are the meters of roads, foot-ways and drains per km (OSM), % informal and formal houses in the ward are obtained from surveyed plots (not all plots are surveyed).*$p \leq 0.10$ ** $p \leq 0.05$ *** $p \leq 0.01$

Table A6: Impact of Neighbors' Weekly Precipitation on Cholera Incidence: Spillovers (unweighted)

	Cholera cases (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Precipitation	0.0158**	0.0162***	0.0166***	0.0170***	0.0226***	0.0227***
	(0.0061)	(0.0061)	(0.0062)	(0.0062)	(0.0063)	(0.0062)
Neighbours' precipitation	-0.0000		-0.0001		0.0002	
	(0.0007)		(0.0007)		(0.0008)	
Uphill neighbours' precipitation		-0.0011		-0.0011		-0.0007
		(0.0008)		(0.0008)		(0.0009)
Downhill neighbours' precipitation		0.0008		0.0007		0.0008
		(0.0007)		(0.0007)		(0.0007)
N	6930	6930	6930	6930	6930	6930
R^2	0.4049	0.4052	0.4057	0.4061	0.4638	0.4640
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes		
Municipal time trend			Yes	Yes		
Municipality $\times$ week FE					Yes	Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. Neighbors' precipitation measures weekly accumulated rainfall in a neighboring ward. Uphill and downhill measures are for neighboring wards at a higher or lower elevation than the given ward. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Main Regressions, Spatial Auto-correlation of Standard Errors (Conley HAC SE)

Table A7: Impact of Weekly Precipitation on Cholera Incidence (HAC SE)

	Cholera cases (log)	
	(1)	(2)
Precipitation	0.0157***	0.0231***
	(0.0051)	(0.0080)
N	6930	6930
R^2	0.0015	0.0049
Ward FE	Yes	Yes
Week FE	Yes	
Municipality × week FE		Yes

Notes: Conley HAC standard errors in parenthesis (Conley 1999, 2008). Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table A8: Impact of Weekly Quartiles of Precipitation on Cholera Incidence (HAC SE)

	Cholera cases (log)	
	(1)	(2)
Q1	-0.0089	-0.0030
	(0.0327)	(0.0259)
Q3	-0.0301	-0.0359*
	(0.0211)	(0.0193)
Q4	0.1592***	0.1521***
	(0.0508)	(0.0567)
N	6930	6930
R^2	0.0043	0.0062
Ward FE	Yes	Yes
Week FE	Yes	
Municipality × week FE		Yes

Notes: Conley HAC standard errors in parenthesis (Conley 1999, 2008). Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. The quartiles of the rainfall distribution are defined as follows: Q1 (0mm), Q2(0-0.29mm), Q3(0.29-2.69mm), Q4(2.69-40.86mm). *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table A9: Impact of Flooding on Cholera Incidence (HAC SE)

	Cholera cases (log)	
	(1)	(2)
Panel A:		
Precipitation	0.0154***	0.0227***
	(0.0051)	(0.0072)
Precipitation × % Flood-prone area	0.0079	0.0059
	(0.0223)	(0.0169)
Panel B:		
Flooded (precipitation ≥75th p)	0.1865***	0.1856***
	(0.0455)	(0.0541)
Panel C:		
Flooded (precipitation ≥75th p)	0.1688***	0.1658***
	(0.0430)	(0.0495)
Flooded × % Flood-prone area	0.2181	0.2136
	(0.2266)	(0.2035)
N	6930	6930
Ward FE	Yes	Yes
Week FE	Yes	
Municipality × week FE		Yes

Notes: Conley HAC standard errors in parenthesis (Conley 1999, 2008). All panels are independent regressions. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. Flooded is a dummy variable for weekly precipitation falling above the 75th percentile of the total rainfall distribution. Flood-prone area is the total area of the ward that is prone to flooding. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. *p ≤ 0.10 ** p≤0.05 *** p≤0.01

Table A10: Impact of Weekly Precipitation on Cholera Incidence: Infrastructure & Ward Characteristics (HAC SE)

	Cholera cases (log)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Precipitation	0.0222***	0.0243***	0.0246**	0.0221***	0.0245	-0.0008	0.0509***	0.0242*	0.0258	0.0063
	(0.0070)	(0.0085)	(0.0102)	(0.0083)	(0.0249)	(0.0200)	(0.0126)	(0.0133)	(0.0266)	(0.0281)
Precipitation × Pop. density	0.0002							0.0001	0.0000	-0.0008
	(0.0003)							(0.0001)	(0.0001)	(0.0011)
Precipitation × Roads density		0.0014						0.0007	0.0024	-0.0055
		(0.0017)						(0.0007)	(0.0016)	(0.0074)
Precipitation × Footways density			0.0027					0.0023	0.0024	0.0063
			(0.0048)					(0.0053)	(0.0033)	(0.0103)
Precipitation × # Water wells				0.0013				-0.0004	-0.0001	-0.0015
				(0.0026)				(0.0011)	(0.0011)	(0.0011)
Precipitation × drains density					0.0029				0.0006	0.0046
					(0.0064)				(0.0040)	(0.0086)
Precipitation × % Informal housing						0.0178				0.0233
						(0.0202)				(0.0202)
Precipitation × % Formal housing							0.0023			
							(0.0058)			
N	6930	6776	4851	5852	3465	1771	2618	4004	2926	1463
R^2	0.0056	0.0053	0.0026	0.0059	0.0022	0.0062	0.0157	0.0037	0.0025	0.0069
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE									Yes	
Municipality × week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes:Conley HAC standard errors in parenthesis (Conley 1999, 2008). Separate regressions in columns (1-7). Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. Roads density, foot-way density, and drains density are the meters of roads, foot-ways and drains per km (OSM), % informal and formal houses in the ward are obtained from surveyed plots (not all plots are surveyed). *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Extensions

Table A11: Impact of Neighbors' Weekly Lagged Precipitation on Cholera Incidence (1)

	Cholera cases (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Precipitation	0.0285**	0.0288**	0.0292**	0.0293**	0.0346***	0.0335***
	(0.0115)	(0.0113)	(0.0117)	(0.0115)	(0.0111)	(0.0110)
Neighbours precipitation	-0.0046	-0.0049	-0.0044	-0.0045	-0.0007	0.0008
	(0.0036)	(0.0047)	(0.0036)	(0.0047)	(0.0034)	(0.0047)
Neighbours precipitation $_{w-1}$	0.0005	0.0007	0.0005	0.0005	0.0002	-0.0007
	(0.0003)	(0.0013)	(0.0003)	(0.0013)	(0.0003)	(0.0013)
Neighbours precipitation $_{w-2}$		-0.0001		-0.0001		0.0007
		(0.0011)		(0.0011)		(0.0011)
N	6929	6924	6929	6924	6929	6924
R^2	0.4493	0.4493	0.4504	0.4504	0.5255	0.5256
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes		
Municipal time trend			Yes	Yes		
Municipality $\times$ week FE					Yes	Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature; they are weighted by the population of the ward (census 2012). The period covered is from the first week of March 2015 to the first week of September 2016. Neighbors' precipitation measures weekly accumulated rainfall in a neighboring ward. Neighbor's Precipitation$_{w-n}$ are the lags of weekly accumulated rainfall in neighboring wards up to n weeks. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table A12: Impact of Neighbors' Weekly Lagged Precipitation by Elevation on Cholera Incidence (2)

	Cholera cases (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Precipitation	0.0218**	0.0214**	0.0233**	0.0230**	0.0355***	0.0354***
	(0.0097)	(0.0105)	(0.0100)	(0.0108)	(0.0098)	(0.0105)
Uphill neighbours precipitation	-0.0042	-0.0044	-0.0047	-0.0047	-0.0052	-0.0049
	(0.0051)	(0.0048)	(0.0052)	(0.0049)	(0.0051)	(0.0050)
Downhill neighbours precipitation	-0.0032	-0.0033	-0.0036	-0.0037	-0.0047	-0.0045
	(0.0052)	(0.0051)	(0.0053)	(0.0051)	(0.0051)	(0.0051)
Uphill N's precipitation $_{w-1}$	0.0037	0.0042	0.0040	0.0044	0.0055	0.0046
	(0.0055)	(0.0069)	(0.0055)	(0.0069)	(0.0053)	(0.0070)
Downhill N's precipitation$_{w-1}$	0.0043	0.0062	0.0047	0.0063	0.0062	0.0070
	(0.0054)	(0.0073)	(0.0054)	(0.0072)	(0.0052)	(0.0073)
Uphill N's precipitation$_{w-2}$		-0.0004		-0.0002		0.0007
		(0.0042)		(0.0043)		(0.0042)
Downhill N's precipitation$_{w-2}$		-0.0018		-0.0016		-0.0012
		(0.0043)		(0.0043)		(0.0043)
N	6929	6924	6929	6924	6929	6924
R^2	0.4494	0.4495	0.4505	0.4506	0.5257	0.5258
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes		
Municipal time trend			Yes	Yes		
Municipality × week FE					Yes	Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature; they are weighted by the population of the ward (census 2012). The period covered is from the first week of March 2015 to the first week of September 2016. Neighbors' precipitation measures weekly accumulated rainfall in a neighboring ward. Uphill and downhill measures are for neighboring wards at a higher or lower elevation than the given ward. N's Precipitation Uphill/Downhill $_{w-n}$ are the lags of weekly accumulated rainfall in neighboring wards up to n weeks according to their elevation with respect to the given ward. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table A13: Impact of Neighbors' Weekly Precipitation on Cholera Incidence: Spillovers (HAC SE) (1)

	Cholera cases (log)			
	(1)	(2)	(3)	(4)
Precipitation	0.0158***	0.0162***	0.0226***	0.0227***
	(0.0047)	(0.0047)	(0.0075)	(0.0076)
Neighbours precipitation	-0.0000		0.0002	
	(0.0007)		(0.0007)	
Uphill neighbours precipitation		-0.0011		-0.0007
		(0.0009)		(0.0010)
Downhill neighbours precipitation		0.0008		0.0008
		(0.0009)		(0.0008)
N	6930	6930	6930	6930
R^2	0.0015	0.0021	0.0049	0.0053
Ward FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes		
Municipality × week FE			Yes	Yes

Notes:Conley HAC standard errors in parenthesis (Conley 1999, 2008). Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. Neighbors' precipitation measures weekly accumulated rainfall in a neighboring ward. Uphill and downhill measures are for neighboring wards at a higher or lower elevation than the given ward. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table A14: Impact of Neighbors' Weekly Lagged Precipitation on Cholera Incidence: Spillovers (HAC SE) (2)

	Cholera cases (log)			
	(1)	(2)	(3)	(4)
Precipitation	0.0189***	0.0187***	0.0259***	0.0257***
	(0.0051)	(0.0060)	(0.0078)	(0.0083)
Uphill neighbours precipitation	-0.0043	-0.0044	-0.0050	-0.0050
	(0.0037)	(0.0042)	(0.0033)	(0.0051)
Downhill neighbours precipitation	-0.0032	-0.0033	-0.0042	-0.0044
	(0.0032)	(0.0051)	(0.0042)	(0.0062)
Uphill N's precipitation $_{w-1}$	0.0033	0.0033	0.0044	0.0046
	(0.0036)	(0.0108)	(0.0034)	(0.0115)
Downhill N's precipitation $_{w-1}$	0.0041	0.0052	0.0052	0.0067
	(0.0031)	(0.0117)	(0.0043)	(0.0124)
Uphill N's precipitation $_{w-2}$		0.0000		-0.0001
		(0.0079)		(0.0071)
Downhill N's precipitation $_{w-2}$		-0.0010		-0.0015
		(0.0076)		(0.0069)
N	6929	6924	6929	6924
R^2	0.0022	0.0023	0.0056	0.0057
Ward FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes		
Municipality × week FE			Yes	Yes

Notes:Conley HAC standard errors in parenthesis (Conley 1999, 2008). Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. Neighbors' precipitation measures weekly accumulated rainfall in a neighboring ward. Uphill and downhill measures are for neighboring wards at a higher or lower elevation than the given ward. N's Precipitation Uphill/Downhill $_{w-n}$ are the lags of weekly accumulated rainfall in neighboring wards up to n weeks according to their elevation with respect to the given ward. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table A15: Lagged Dependent Variable Model

	Cholera cases (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Precipitation	0.0199***	0.0155***	0.0119**	0.0111**	0.0103**	0.0103**
	(0.0054)	(0.0051)	(0.0051)	(0.0051)	(0.0051)	(0.0051)
Cholera cases $_{w-1}$	0.6111***	0.4430***	0.4037***	0.3943***	0.3913***	0.3914***
	(0.0457)	(0.0336)	(0.0348)	(0.0360)	(0.0368)	(0.0371)
Cholera cases $_{w-2}$		0.2747***	0.2112***	0.1975***	0.1922***	0.1923***
		(0.0230)	(0.0285)	(0.0268)	(0.0281)	(0.0283)
Cholera cases $_{w-3}$			0.1435***	0.1172***	0.1085***	0.1087***
			(0.0206)	(0.0245)	(0.0255)	(0.0257)
Cholera cases $_{w-4}$				0.0652***	0.0476**	0.0479**
				(0.0208)	(0.0203)	(0.0197)
Cholera cases $_{w-5}$					0.0447**	0.0455**
					(0.0214)	(0.0217)
Cholera cases $_{w-6}$						-0.0019
						(0.0185)
N	6930	6930	6930	6930	6930	6930
R^2	0.6837	0.7076	0.7136	0.7148	0.7153	0.7153
Ward FE						
Municipality × week FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the log of effective (tested positive) weekly cholera cases in a given ward. All regressions control for weekly ward air temperature; they are weighted by the population of the ward (census 2012). The period covered is from the first week of March 2015 to the first week of September 2016. Cholera cases$_{w-n}$ are lagged effective cholera cases up week n. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Robustness check: Linear-Linear Regressions

Table A16: Impact of Weekly Precipitation on Cholera Incidence (per 10,000s people)

	Cholera Ward Incidence Rate (per 10,000s)		
	(1)	(2)	(3)
Precipitation	0.0272***	0.0280***	0.0359***
	(0.0081)	(0.0082)	(0.0099)
N	6930	6930	6930
R^2	0.1908	0.1915	0.2367
Ward FE	Yes	Yes	Yes
Week FE	Yes	Yes	
Municipal time trend		Yes	
Municipality $\times$ week FE			Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the number of weekly cholera cases (tested positive) divided by the population of the ward (10,000s), i.e. cholera cases every 10 thousand people in a ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. *p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table A17: Impact of Weekly Quartiles of Precipitation on Cholera Incidence (per 10,000s people)

	Cholera Ward Incidence Rate (per 10,000s)		
	(1)	(2)	(3)
Q1	0.0196	0.0235	0.0152
	(0.0152)	(0.0155)	(0.0163)
Q3	-0.0215	-0.0158	-0.0216
	(0.0250)	(0.0248)	(0.0276)
Q4	0.0946**	0.1069**	0.0862*
	(0.0462)	(0.0473)	(0.0509)
N	6930	6930	6930
R^2	0.1899	0.1905	0.2349
Ward FE	Yes	Yes	Yes
Week FE	Yes	Yes	
Municipal time trend		Yes	
Municipality $\times$ week FE			Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the number of weekly cholera cases (tested positive) divided by the population of the ward (10,000s), i.e. cholera cases every 10 thousand people in a ward. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016. The quartiles of the rainfall distribution are defined as follows: Q1 (0mm), Q2(0-0.29mm), Q3(0.29-2.69mm), Q4(2.69-40.86mm)*p $\leq$ 0.10 ** p$\leq$0.05 *** p$\leq$0.01

Table A18: Impact of Flooding on Cholera Incidence (per 10,000s people)

	Cholera Ward Incidence Rate (per 1000s)		
	(1)	(2)	(3)
Panel A:			
Precipitation	0.0268***	0.0276***	0.0354***
	(0.0080)	(0.0081)	(0.0097)
Precipitation × % Flood-prone area	0.0094	0.0080	0.0078
	(0.0141)	(0.0138)	(0.0139)
Panel B:			
Flooded (precipitation ≥75th p)	0.1164***	0.1231***	0.1077***
	(0.0344)	(0.0351)	(0.0395)
Panel C:			
Flooded (precipitation ≥75th p)	0.1004***	0.1074***	0.0877**
	(0.0350)	(0.0353)	(0.0389)
Flooded × %Flood-prone area	0.1981	0.1910	0.2162
	(0.1471)	(0.1451)	(0.1480)
N	6930	6930	6930
R^2	0.1901	0.1908	0.2352
Ward FE	Yes	Yes	Yes
Week FE	Yes	Yes	
Municipal time trend		Yes	
Municipality × week FE			Yes

Notes: Robust standard errors clustered at the ward level in parenthesis. All panels are independent regressions. Precipitation is measured as the weekly accumulated rainfall in a given ward (10mm units), cholera cases are the number of weekly cholera cases (tested positive) divided by the population of the ward (10,000s), i.e. cholera cases every 10 thousand people in a ward. Flooded is a dummy variable for weekly precipitation falling above the 75th percentile of the total rainfall distribution. Flood-prone area is the total area of the ward that is prone to flooding. All regressions control for weekly ward air temperature. The period covered is from the first week of March 2015 to the first week of September 2016.*p ≤ 0.10 ** p≤0.05 *** p≤0.01