Inter-Regional Barriers and Economic Growth: Evidence from China

Yi Han, Mingqin Wu*

Abstract

This paper examines the impact of a policy reform in China that removed inter-regional market barriers by incorporating counties into larger prefecture-level divisions. Employing a difference-in-differences approach, we compare the economic performance of incorporated counties before and after the reform to two control groups: counties that applied for incorporation but were unsuccessful, and counties that were incorporated at a later time. Our findings suggest that the reform had an immediate and sustained positive effect on the economic growth of incorporated counties. Using firm-level data, we provide evidence that the reform reduced policy-induced frictions, leading to increased regional specialization in industries with comparative advantage, more entries of new firms, and more exits of low-profit-margin firms. Overall, the research highlights the importance of reducing inter-regional market barriers in promoting economic growth in developing countries.

1 Introduction

Along with the reduction in transportation costs in the last two centuries, policy-induced frictions have become increasingly important obstacles to further productivity growth (e.g.

*Han: School of Applied Economics, Renmin University of China, yihanecon@ruc.edu.cn; Wu: School of Economics and Management, South China Normal University mingqinwu@163.com. We are grateful for generous feedback from Daniel Berkowitz, Thomas Rawski, Jason Cook, Yogita Shamdasani, and participants at the Labor/Development Seminar at University of Pittsburgh.

Faber, 2014; Donaldson and Hornbeck, 2016; Storeygard, 2016; Donaldson, 2018). Trade restrictions such as tariffs and quotas, regulations for industrial establishments such as entry and exit restrictions, and financial frictions such as unequal access to low interest rates based on non-economic factors all potentially lead to misallocations of resources and hinder economic growth.

Policy-induced frictions not only exist at the national level but also between localities, as local governments often shield local firms from competition. This protectionism occurs across different regimes, as evidenced by research conducted in the US by Eyer and Kahn (2017), which found that coal states provide large financial incentives to encourage power plants to purchase locally mined coal. In China, local governments impose various inter-regional restrictions to protect firms within their jurisdiction. These measures include subsidies to encourage local purchases, regulations that discriminate against non-local firms, restrictions on cross-regional trade and migration, and favoring local suppliers in procurement (Young, 2000; Poncet, 2005; Barwick et al., 2017; Tombe and Zhu, 2019). This paper examines the impact of eliminating policy-induced frictions between jurisdictions on local economic growth, specifically by analyzing the effect of the *incorporating counties into prefectures reform (chexian shequ)* in China.

The incorporation of counties into prefectures is a centralization effort by the Chinese central government. In China, counties were independent administrative entities with their own local governments. Prefecture-level divisions (referred to as "prefecture" hereafter) were higher-level administrative units that oversaw regional policies and coordination between counties.[1] Before the reform, there were often regulatory and administrative barriers between counties and prefectures, which could lead to inefficient resource allocation. The incorporation reform merged a county into the prefecture that used to oversee it, resulting in the county governments

[1] In China, there are four types of prefecture-level divisions: "di ji shi" (prefecture-level city), "di qu" (prefecture), "zi zhi zhou" (autonomous prefecture), and "meng" (league). As of 2019, China has 299 prefecture-level cities, 7 prefectures, 30 autonomous prefectures, and 3 leagues.

losing some of their autonomy and becoming agencies of the prefecture-level government. The barriers between the county and the prefecture disappeared, and the two local markets were expected to integrate more closely.

To investigate the impact of the incorporating-counties-into-prefectures reform on local economic growth, we utilize county-level panel data from 1995 to 2013 and employ a difference-in-differences empirical strategy. We use GDP and nighttime light intensity to measure aggregate outcomes. Nighttime light intensity, gathered from weather satellite recordings, has been increasingly used by economists to measure economic activity in developing countries, especially at the local level (Henderson et al., 2012; Hodler and Raschky, 2014; Storeygard, 2016). Because nighttime light intensity is not susceptible to political manipulations, it complements GDP as a measure of economic activity.

Two challenges need to be addressed to empirically test the effect of eliminating market frictions, as pointed out by Donaldson (2015). The first challenge is finding a suitable control group to address the endogeneity issue. Misallocations may not be the only difference between the treatment and the control group. The second challenge, as proposed by Rubin (1980), is that the control and treatment groups should not interact; otherwise, there may be over-estimation or under-estimation, depending on the direction of the spillover effect.

Firstly, we construct a list of counties chosen by prefecture-level cities to be incorporated but not approved by higher-level governments due to various political and geographical reasons. This allows us to compare the treatment group to an *applied-but-failed* group, which helps adjust for the nonrandom selection of counties by prefectures.

Secondly, we take advantage of the exogenous timing of the incorporation for those successfully incorporated counties. Specifically, we compare counties that experienced incorporation in year y to those that would experience incorporation at least τ years later, for example, in year $y+\tau$ and onwards. The two control groups partially solve the second identification problem, as the treatment and control counties are relatively isolated geographically.

Our results indicate that the reform significantly increased economic growth in the incorporated counties between 1995 and 2013. Compared to the *applied-but-failed* counties, the reform raised incorporated counties' GDP per capita by 11.7 percent and nighttime light in-

tensity per square kilometer by 4.8 percent in ten years. Using the second approach, we find that the reform led to a 10.3 percent increase in GDP per capita and a 3.9 percent increase in nighttime light intensity per square kilometer for counties that experienced current incorporation, compared to counties that would experience the reform several years later. The effect of the reform is both immediate and persistent. In the first year following the reform, the treated counties' economic growth surpassed that of the control counties, which is possible if there were "immediate" reallocations of resources due to the lifting of inter-regional barriers. We also find that the treated counties still grew faster than the control counties ten years after the reform, suggesting that the effects of the reform were persistent.

To address concerns that the positive effects of the reform on treated counties were due to involuntary migration of economic activity from prefecture-level cities without any gain in overall economic development, we examine the economic growth of both the treated counties and the prefecture-level cities they were incorporated into. If the positive effects of the reform were solely due to a transfer of economic activities, we would expect the overall impact on the treated counties plus their prefecture-level cities to be zero. However, our analysis reveals that the reform increased the total GDP per capita of the treated counties and their prefecture-level cities by 6.6 percent. This finding suggests that the positive effects of the reform were not solely driven by a transfer of economic activities, but rather by genuine increases in aggregate economic development.

After establishing the positive impact of the reform that incorporated counties into prefectures, we explore its underlying mechanisms. We first explore whether the reduction in market barriers can potentially explain the positive effect of the reform. Constrained by available data, we examine the empirical evidence for reductions in frictions in the product market, the labor market, and firm entry and exit. To achieve this, we utilize the Annual Industrial Surveys, a comprehensive firm panel dataset conducted by the National Bureau of Statistics.[2]

In the product market, our investigation initially focuses on whether inter-regional trade

[2]We accessed this dataset from Tsinghua China Data Center: http://www.tcdc.sem.tsinghua.edu.cn/index.htm.

barriers have been reduced since counties were incorporated. To measure the degree of such trade barriers, we follow Bai et al. (2004) and examine the negative correlation between an industry's share of SOEs and its level of geographical concentration. This is because local governments tend to prioritize protection for SOEs over other types of enterprises, leading to less competition and geographic specialization in industries with high shares of SOEs. We expand upon Bai et al. (2004)'s provincial-level findings by examining this negative correlation at the county level, where we find that it still holds true. Importantly, our analysis reveals a significant reduction in this negative correlation within treated counties compared to control counties following the reform.

Next, we explore whether treated counties reallocated towards their most productive sectors following the reform. According to Melitz (2003), international trades induce reallocations towards more productive firms where they enter export markets and absorb shares from less productive ones. Our hypothesis is that since the reform eliminated product market barriers, treated counties should specialize more in industries where they have comparative advantages. Our findings show that production shares for the most productive sector increased by 2 percentage points on average after the reform within treated counties - representing a 25 percent increase from their original levels.[3]

In the labor market, we investigate whether there were any population increases in treated counties following the removal of labor market barriers through reform. However, our findings did not reveal any significant changes in the size of the registered population within these treated counties.[4] It is possible that there were no substantial migration barriers between counties and prefectures even before the reform. Alternatively, it could also be plausible that people - especially those who were already registered in prefectures prior to incorporation - migrated to these treated counties without changing their residence. Unfortunately, due to data limitations,

[3]The sector is defined at a 2-digit industry level

[4]Registered population refers to individuals who are officially registered with their local government and possess a local *Hukou*.

we cannot verify this hypothesis.

Lastly, we demonstrate that the reform reduces barriers to entry and exit for firms. We observe an immediate and consistent increase in firm entries in treated counties compared to control counties after the implementation of the reform. This immediacy suggests that infrastructure enhancements are not responsible for these entries since they require time to establish. Regarding barriers to exit, our findings indicate that less profitable firms in treated counties were significantly more likely to leave after the reform was implemented. This aligns with the mechanism that reducing entry and exit barriers increases competition among firms, which compels less profitable ones to withdraw (Melitz, 2003).

An alternative explanation for the positive effect is that this reform alters government structure and behavior, leading to increased growth in incorporated counties. We explore several potential changes in government behavior resulting from this reform. Firstly, an increase in inter-governmental fiscal transfers from the prefecture-level government may allow governments of incorporated counties to spend more. Secondly, it could be because more state-owned enterprises are being subordinated by the prefecture to incorporated counties which contribute to economic growth there. Thirdly, treated counties may become more likely targets for subsequent growth-oriented reforms which drive output increases; one such example is the "economic zones" reform, which has been shown by Lu et al. (2019) to significantly promote growth in counties with these zones.[5] However, after examining each of these changes individually, we found no significant difference between incorporated and control counties - suggesting that none of these factors can fully explain our results.

Our work adds to the literature on the economic impact of market frictions. Misallocations caused by policy induced distortions are believed to be one of the main reasons for the income gap between developing and developed counties (Bertrand and Kramarz, 2002; Guner et al., 2008; Hsieh and Klenow, 2009; Herrendorf and Teixeira, 2011; Tombe and Zhu, 2019;

[5]An Economic Zone refers to a designated geographic area where the government has implemented special economic policies and measures to promote economic growth.

Fajgelbaum, 2020). For example, Tombe and Zhu (2019) quantifies the trade and migration cost in China and the contribution of their reductions to aggregate outcomes between 2000 and 2005. They find that trade liberalization and migration cost reduction together account for about half of the growth in productivity in China during the study period. Previous literature mainly rely on quantitative computable general equilibrium models to quantify the effect of reducing market frictions, and well identified empirical work is lacking. This paper fills the gap well. Specifically, we explore a unique set of institutional reforms in China that provide a plausible way to identify the effect of market frictions within countries on aggregate outcomes. Our paper also contributes to this literature by employing two novel control groups to solve the problems of endogeneity and spillover. The latter is possible because our treated counties and control counties are relatively geographically isolated.

The findings also contribute to the literature on inter-regional barriers, especially in China (Young, 2000; Bai et al., 2004; Holz, 2009; Lu and Tao, 2009; Barwick et al., 2017). Using a province-level regional specialization index, previous studies have identified the existence of inter-regional barriers in China by showing insufficient specialization and an otherwise unexplained correlation between regional specialization and industry characteristics. Our contributions to this literature are twofold. First, we extend the previous analysis from province level to county level, a much finer unit. The county is the basic administrative unit in China. The importance of the county economy after the tax-sharing reform in 1994 becomes even more significant.[6] The fact that protection practices widely existed at all local levels indicates that inter-regional barriers are deeply rooted in China's political system. Second, even though it was suggested in previous studies that *the regionally decentralized authoritarian* regime is the cause of inter-regional barriers, we empirically show that a reform re-centralized authority

[6]Compared with provincial governments, county-level governments have been granted land use transfer rights so that they can promote economic growth through a variety of methods, such as selling land at low prices. In addition, county-level governments have also been given more power through various reforms, one of which is the reform of incorporating counties into prefectures reform.

indeed reduced market frictions.

Lastly, the paper is also related to previous studies on the policy impact of the incorporating counties into prefectures reform (Tang and Hewings, 2017; Liu et al., 2019). We contribute to this literature by constructing two novel control groups to solve the problem of endogeneity. More importantly, we provide extensive firm-level evidence to show that the mechanism this reform promoted economic growth is that it eliminated policy-induced frictions between the incorporated counties and the prefectures.

The paper proceeds as follows. Section 2 briefly describes the institutional background and two important features of the incorporating counties into prefectures reform. Section 3 describes the data. Section 4 introduces the empirical model, discussing the potential threats to the identification. Section 5 presents the main results. In Section 6, we analyze the channels through which the reform increased economic development. Section 7 concludes.

2 Background

2.1 Policy-induced Frictions in China

The problem of inter-regional trade and migration barriers has haunted China over the whole reform era. It was hypothesized that the *regionally decentralized authoritarian* (RDA) regime is the cause of inter-regional barriers in China (Xu, 2011). Under RDA, the promotion of local officials heavily depends on local GDP growth (Li and Zhou, 2005). Therefore, local governments have strong incentives to protect local firms whose production contributes towards local GDP. In addition, under RDA, local governments have a high degree of fiscal autonomy. Tax revenues collected from firms account for about a third of the local government total revenues.[7] The administrative authority is decentralized under RDA too. Local authorities can make localized regulations, or even laws in some cases, to favor local firms. They also directly manage some SOEs and can directly invest in some privately owned firms.

[7]Data source: Ministry of Finance of the People's Republic of China (http://www.mof.gov.cn/).

In the market for goods and services, local governments have been known to employ various tactics such as local market entry barriers, discriminatory regulations, discriminatory subsidies, resource allocation and others to limit competition from outside of their region. It was not uncommon for these governments to prevent non-local firms from selling their products by blocking the local market. For instance, in 1995-1996 around half of counties in Heilongjiang Province reportedly blocked the sale of beer from non-local companies through methods like setting up checkpoints on highways and conducting surprise inspections on distribution points. This matter prompted an urgent notice from the provincial government (Document No. [1996]10) entitled "On Prohibiting the Blocking of the Alcoholic Beverage Market," which terminated administrative monopolies in these regions. Another way local governments show favoritism towards regional manufacturers is to restrict the movement of low-priced raw materials and reserving them for local companies (Watson et al., 1989; Bernstein and Lü, 2000). The central government issued an official order in 1982 to prohibit those practices.

Over time, protectionist activities have become increasingly implicit and harder to identify. One tactic used to achieve this is through the provision of subsidies by the government to locally-owned SOEs. These SOEs are able to persist even in the face of losses, with their business decisions being heavily influenced by local authorities. For example, it has been widely reported that certain taxi companies exclusively purchase vehicles from local car manufacturers (Barwick et al., 2017).

Local governments can also protect local firms through favorable regulations and selective law enforcement. They can can tailor their regulations so that only the products of local firms can comply with them. For example, the 2018 market entry requirements for new energy automobiles in Shanghai effectively exclude all cars but those made in Shanghai.[8] Additionally, they can choose to enforce laws or policies selectively on non-local firms while exempting local ones from scrutiny. For instance, during a quality control test on electronic bikes conducted by

[8] See http://news.bitauto.com/hao/wenzhang/629965.

the Liuzhou city government in 2015, all local brands passed while all non-local brands failed.[9]

In the labor market, China's household registration (Hukou) system was established in 1958 to regulate internal migration. Although people can change their Hukou status, local governments tightly control this process, particularly in major cities across the country. The standards and requirements for obtaining a Hukou vary between cities and counties. A person can only obtain a Hukou if they meet specific criteria related to investments, tax payments, real estate purchases, employment status, college enrollment status, joining relatives or making special contributions (Sieg et al., 2022). These rural-urban migration barriers have led to significant labor misallocation in China (Meng, 2012; Brandt et al., 2013).

In the realm of finance, local governments have the ability to manipulate the movement of capital between regions by retaining it within their own jurisdiction. Capital mobility in China is comparatively limited due to this manipulation. Local government at different administrative levels exert influence over the operation of capital markets by controlling the flow of capital and directing bank loans (Boyreau-Debray and Wei, 2005). Low-interest loans are frequently allocated exclusively to local state-owned enterprises and businesses that make substantial contributions to their respective region's gross domestic product.

2.2 Institutional Background

While the explicit objectives of the incorporating counties into prefectures reform were not detailed in publicly accessible documents, it is plausible that the reform aimed to address regional administrative barriers and foster a unified market within the prefecture. Beginning with the Ninth Five-Year Plan (1996-2000), a strategic blueprint for China's economic development, there was a noticeable shift in the central government's urban planning strategy. Instead of constraining the growth of major cities, the emphasis shifted towards leveraging them as primary engines of development. Notably, the term "central cities" was absent in prior five-year plans

[9]For more information, see the website of Administration for Industry and Commerce, Liuzhou, Guangxi Province (http://gsj.liuzhou.gov.cn/tzgg/index.html).

but appeared twice in the Ninth Plan. The timing of this plan's publication closely aligns with the rapid implementation of the reform. The Ninth Plan explicitly states: "In accordance with the laws of the market economy, relying on central cities and major transportation routes, further form and develop several economic regions that break through administrative boundaries.

The hierarchical structure of administrative divisions in China plays a crucial role in comprehending this reform. The central government holds the highest position in this hierarchy, followed by provincial-level governments, prefecture-level governments, and county-level governments (as illustrated in Figure 1). Each level is responsible for supervising the work conducted by lower levels within the administrative hierarchy.

Within the county level, there are counties and districts and there are substantial differences between them. Districts, formally city-governed districts, are subdivisions of a prefecture-level division or a direct-administered municipality. In the year 1993, a set of prerequisites for forming a district was formulated by the Ministry of Civil Affairs. These requirements included having a populace of at least 250,000 individuals and no less than 100,000 non-agricultural residents. District governments are agencies of the prefecture governments, and are in charge of implementing the policies made by the prefecture governments. In contrast, counties are officially recognized as administrative subdivisions of a province. Prefecture-level divisions oversee counties on behalf of the provinces.

Counties enjoy greater autonomy than districts in terms of both fiscal revenue and spending. Following the Tax-Sharing Reform in 1994, county-level governments' fiscal revenue can be broadly divided into three categories: budgetary revenue, extra-budgetary revenue, and off-budget revenue.

The allocation of budgetary revenue, which includes value-added tax, business tax, local enterprise income tax and personal income tax, is divided between county-level governments and prefectural/provincial governments. However, the proportion of budgetary revenue that county-level governments need to share with prefecture governments is significantly lower than that for district governments. This discrepancy can be attributed in part to the fact that prefecture government has more control over district government's revenue sharing while needing to negotiate with provincial government for a higher share of county government's revenue.

The extra-budgetary revenue comprises various non-tax items such as paid use of state-owned resources and assets, state-owned capital operating income, fines, confiscations, etc. The upper-level government does not require county-level governments to share most forms of the extra-budgetary revenue. However, counties are responsible for collecting more sources of extra-budgetary income than districts and have greater independence in this category. In recent years, land conveyance fees - a revenue generated from selling the land use rights to a private party - have become increasingly important (Han and Kung, 2015). County governments not only possess greater administrative authority to approve land transfers compared to district governments but also share less of the income with prefecture governments. Regarding fiscal spending, districts have less autonomy than counties since their spending is controlled by prefectures while counties can freely utilize their revenue provided they comply with regulatory mandates.

As an agency of the prefecture governments, district governments are administratively less independent than county governments. They have fewer functions compared to county governments as many government functions have been centralized at the prefectural level. For instance, the public security sub-bureau in districts is managed by the prefecture public security bureau and therefore not under the jurisdiction of district government. The district government has no final say in relevant assessments or appointments and removals of cadres within that department. However, county governments directly control their respective public security bureaus. Additionally, land resources and planning, industry and commerce, quality supervision, tobacco monopoly, inspection and quarantine in districts are also directly managed by prefectural bureaus.

As districts possess comparatively less fiscal and administrative autonomy than counties, the conversion of counties into districts essentially results in the re-centralization of authority to the prefectures.

Although we use the term "counties" to refer to all non-district county-level divisions, there are significant differences within this group. Counties can be classified as either province-directly-administered counties (*sheng zhi guan xian*) or standard counties based on their governance structure. A province-directly-administered county shares a larger portion of its tax rev-

enue with the province and less with the prefecture than a standard county. Province-directly-administered counties also have higher fiscal and administrative autonomy than standard counties.

Counties can also be categorized as either county-level cities (*xian ji shi*) or county-level counties depending on their level of urbanization and industrialization; this dimension is orthogonal to the governance structure dimension. Despite having an identical administrative hierarchy to that of a county-level county, governments of county-level cities possess greater political authority. Unlike county-level counties, the party secretaries of county-level cities can be appointed as members of the party standing committee at the prefecture government (Fan et al., 2012). Additionally, they have increased rights for land approval and quotas for construction land (Chung and Lam, 2004).

2.3 Two Special Features of the Incorporating-Counties-into-Prefectures Reform

Two special features of the reform enable us to causally estimate its impact. The first special feature is that, due to the administrative procedure for this reform, there exists a natural control group. In order for a county to be integrated into a prefecture, the prefecture government will first need to communicate with the county governments to obtain their consent. If both the county government and people's congress agree, then the prefecture government can request approval from the provincial government. Finally, if approved by the provincial government, it goes to central authority which ultimately decides whether or not a county can become a district and when such transformation may occur.

In our first approach, we utilize a control group consisting of counties that were selected by prefectures for absorption but had not yet received approval from the central government until at least 2013 - which is the last year covered in our study. We obtained the list of these counties from the city planning books published by the respective prefecture governments. For instance, Suzhou City in Jiangsu Province announced its plan to absorb six counties - Zhangjiagang, Taicang, Changshu, Kunshan, Wuxian and Wujiang - in its 1996 planning yearbook (Suzhou City Planning Manual 1996-2010).

Not all proposed incorporations were approved. As argued by Lu and Tsai (2019), there was strong inter-governmental vertical competition in China's urbanization process, leading provinces to have stronger incentives to turn normal counties into province-directly-administered ones or at least maintain their county status. Therefore, most incorporation applications were blocked by provincial governments. For instance, Suzhou could only absorb two out of six counties on its incorporation list until 2019 (Wuxian and Wujiang). In exchange for turning Wujiang into a district, it had to allow Jiangsu province to directly administer Kunshan - the most developed counties on its incorporation list (Cartier, 2016).

The central government may also reject or significantly delay applications of incorporation for various political and cultural concerns. For example, Shijiazhuang, the capital city of Hebei Province, planned to incorporate Zhengding, Luancheng, Gaocheng, and Luquan in both 2001 and 2006. While the other three have been successfully incorporated, Zhengding is still a county as of today. It's speculated that the reason might be political, given that President Xi Jinping once held a leadership position in Zhengding county. Some believe that to honor his legacy, Zhengding has retained its county status, despite having the highest GDP per capita among the four counties in Shijiazhuang's list and its proximity to Shijiazhuang's city center. It's also observed that the birthplaces of former general secretaries of the Communist Party of China have maintained their statuses. In addition, counties on the list of National Famous Historical and Cultural Cities are also immune to incorporation to keep their historic county names.[10]

The second feature of this market integration reform is that the timing of the reform varied substantially between and within prefectures. In Suzhou's case, even though both Wuxian and Wujiang were on Suzhou's incorporation list in 1996, the former was incorporated in 2000, while the latter was incorporated in 2012. Changzhou, a city also in Jiangsu Province, planned to absorb Wujin, Liyang, and Jintan in its 1996 city planning book. Wujin was successfully in-

[10]As of 2015, there are 127 National Famous Historical and Cultural Cities in China. For the complete list, please see http://news.youth.cn/jsxw/201509/t20150901_7072311_1.html

corporated into Changzhou city in 2002, while neither Jintan nor Liyang were incorporated by 2013. Many factors may affect the timing of the final incorporation. Occasionally, the central government blocked the applications of incorporation for several years. For example, no applications from Jiangsu province were approved between 2005 and 2008. Provincial and central government transitions may also affect the application process. For example, Wuhan, a city in Hubei Province, started its application for incorporating Huangpi county in 1996, and the application was passed to the central government in 1997. However, due to government transition in 1997, the application was approved in late 1998 and Huangpi was formally incorporated in 1999.[11]

3 Data

We use data from four different sources to estimate the effect of the reform of incorporating counties into prefectures on economic development. This section presents an overview of these data sources and the construction of the variables we use for the analysis.

We have created a panel dataset consisting of counties that underwent the reform and those that applied for incorporation but were not approved by 2013. The information on treated counties was sourced from the Ministry of Civil Affairs of China's website.[12] To identify control counties, we obtained lists from each prefecture's city planning books (*chengshi zongti guihua*), which are published or edited approximately every five years.[13] It is important to

[11] Source in Chinese: https://hb.ifeng.com/a/20181227/7127581_0.html

[12] We excluded counties that merged with a district into a new district as we cannot determine the outcome after the reform.

[13] The authors hand-collected the county list. The city planning books provided a clear indication of which counties were to be submitted as applications to the provincial government, and this information was thoroughly examined. For instance, in Xuzhou's 2007 city planning book, a prefecture-level city, it was explicitly stated that Tongshan County would be incorporated into an urban district. Eventually, in 2010, Tongshan County was transformed into Tongshan District. In total, we checked 332 such books with the help of 5 research assistants.

note that while we can identify whether a county applied for integration or not, we cannot determine when it first applied for reform. Figure 2 displays the geographical distribution of incorporated and applied-but-failed counties. Although this is a national reform, there are still geographic patterns; most treated counties were located in eastern and central provinces, while some applied-but-failed ones were scattered throughout western provinces. Therefore, our analysis focuses solely on variations within provinces between treated and applied-but-failed counties.

The nighttime light intensity data is obtained from the Defense Meteorological Satellite Program-Operational Linescan System (DMSP-OLS) nighttime satellite data. The values of lights are integers ranges from 0 (no light) to 63. We calculate the average lights per square kilometer within the boundary of a county/district as a measure of local economic development.

We collect county-level GDP (including the shares of manufacturing and tertiary industry), population, government expenditure, and revenue for the years 1995-2013 from provincial and prefectural statistical yearbooks.

In addition, we observe firms' activities from China's Annual Industrial Survey from 1998-2007. The survey includes information for all state-owned industrial firms and non-state owned firms with prime operation revenue above 5 million RMB. From the annual survey data, we can observe a firm's profit, revenue, employment, industry codes, location at the county level, the year that the firm was founded, the year that the firm exited. Using firm-level data, we estimate firms' productivity, identify firms' entry and exit, and calculate the geographical concentration of industries at the prefecture-year level for the mechanism analysis in Section 6.

For the main empirical estimation, we limit the sample to the time period 1995-2013. The years before 1995 are excluded because a nation-wide tax-sharing reform took place in 1994. The reason we choose to look at years up to 2013 is that 2013 is the last year of available nighttime lights data.

Table 1 presents county-level characteristics for incorporated counties and applied-but-failed counties (columns 1 and 2) in the baseline year. There are 74 counties that experienced the reform, and 185 counties that applied for incorporation but failed. Columns 3 and 4 report differences and p-values conditional on province fixed effects. Compared to the applied-but-

failed counties, the incorporated counties have more population, food possession per capita, a lower share of rural population, savings and loans, and a student-teacher ratio, a higher share of manufacturing and tertiary industry. Notably, only one of the eleven variables is statistically different at the 10 percent level, and one is at the 1 percent level. Overall, table 1 shows that the research design comparing incorporated counties to applied-but-failed counties balances many (although not all) observable covariates, once accounting for average characteristics in the province.

4 Empirical strategy

DID comparing incorporated counties to applied-but-failed counties. In the first approach, we estimate the effects of the reform on a set of county-level outcomes in a difference-in-differences framework. Specifically, we compare the evolution of outcomes for counties that were successfully incorporated to counties that applied for the incorporation but failed for various political and historical reasons. The equation takes the following form:

$$y_{cpt} = \beta Reform_{ct} + \theta_c + \delta_{pt} + \varepsilon_{cpt} \tag{1}$$

where y_{cpt} is the outcome variable of interest for county c in province p at year t. Here we look at the log of GDP per capita and log of (1+nighttime lights per square kilometer). The main coefficient of interest is $Reform_{ct}$, an indicator variable that equals 1 for the treated county in years after the incorporation, and 0 for all other cases. θ_c and δ_{pt} are full sets of county and province$\times$year fixed effects. County fixed effects absorbs all time-invariant county-specific characteristics. Province-year fixed effects accounts for cross-year common changes in provinces that occur even in the absence of the incorporation. Lastly, ε_{cpt} is the error term. Standard errors are clustered at the county level to allow for correlation over time within a county.

The difference-in-differences specification relies on the assumption that, in the absence of the reform, the change of outcomes in incorporated counties and applied-but-failed counties before the reform should have parallel trends. We test the validity of this assumption by plot-

ting coefficients of β_τs and the corresponding 95 percent confidence intervals of the following equation:

$$y_{cpt} = \sum_{i=-5}^{10} \beta_\tau DtoReform_{ct}^{\tau} + \theta_c + \delta_{pt} + \varepsilon_{cpt} \tag{2}$$

where $DtoReform_{ct}^{\tau}$ are indicator variables for whether year t is τ years after (or before, if negative) the year of incorporation; for control counties, it equals 0 in all years. The indicator of "the year before the incorporation" is omitted as the reference year. The coefficients of interest are β_τs, capturing the effect of the reform τ years later. For GDP per capita and nighttime lights, none of the coefficients before the incorporation are significantly different from zero. They are also small in magnitude, but they become consistently positive after the incorporation (Figure 3).

DID using variation in the timing of incorporation. One natural concern for the first approach is that there might still be some other systematic differences between the incorporated counties and the applied-but-failed counties other than changes in policy-induced barriers. To address this issue, we exploit the variation in the timing of the reforms as an alternative estimation approach. We employ a difference-in-differences strategy that compares economic growth in counties that experience the current incorporation to counties that would experience the reform several years later, before and after the current reform. Even though the treated counties may not be randomly selected, we show evidence that the timing of the incorporation is arguably exogenous (i.e., no observable characteristics can persistently predict the timing of incoporations) in Appendix Table A1.[14]

[14]To investigate whether the timing of the reform can be predicted by county-level characteristics, we restrict the sample to counties that haven't been incorporated in that year but will be turned into districts in the future. We estimate the following equation:

$$ReformY_c = \alpha + \Gamma X_c + \varepsilon_c$$

where $ReformY_C$ is an indicator variable that equals 1 if the county received the treatment in the year indicated in the column heading. X_c is a vector of county-level characteristics, including population (lag), the manufac-

We construct the sample following Deshpande and Li (2019). For each of the incorporations, we define the county that experienced the current incorporation as the treated county, and construct the corresponding control group as counties that would experience incorporation more than five years in the future. The year of the incorporation is set to be year 0. We restrict our sample to event years from -5 to 5 such that the control counties haven't experienced the reform yet. Lastly, we combine all 74 incorporations and build one dataset.

There's a trade-off in the choice of year gap between the treatment and control group experiencing the reform (Fadlon and Nielsen, 2015). While a small year gap is preferable since the control counties are more closely comparable to the treatment counties, it also imposes an upper bound on the time horizon of the analysis (i.e., we can only identify the effect up to that year gap). Our main results use a five-year gap to identify effects up to five years after the reform. In robustness checks, we demonstrate that the results are robust if we change the year gap to three-year, four-year and six-year gaps (Appendix Figure A1).

To estimate the effects of the reform using only time variation, we estimate the following equation:

$$y_{cpit} = \theta_c + \delta_{pt} + \beta_0 Treated_{ci} + \delta_0 Post_{it} + \beta(Treated_{ci} \times Post_{it}) + \varepsilon_{cpit} \tag{3}$$

where y_{cpit} is the outcome for county c in province p for incorporation i at year t. $Treated_{ci}$ is an indicator variable equal to 1 if county c is a treated county for incorporation i. $Post_{it}$ is an indicator variable equal to 1 if year t is after the incorporation i. θ_c and δ_{pt} are county fixed effects and province$\times$year fixed effects. We cluster standard errors at the county level. The coefficient of interest is βs, capturing the difference in economic growth between treated and control counties after the incorporation.

turing share of GDP (lag), the tertiary share of GDP (lag), the ratio of government expenditure to government revenue (lag), the ratio of government revenue to GDP (lag), the ratio of government expenditure to GDP (lag), the log of lights per square kilometer (lag), the dummy of provincial capital, the dummy of direct-administered municipalities of China.

The validity of our difference-in-difference strategy requires that in the absence of the reform, the change of economic growth in counties that experienced the earlier incorporation would have parallel trends to those experienced incorporation several years later. To validate this approach, we re-run the test on the parallel-trends assumption with this new control group using the following dynamic difference-in-difference model:

$$y_{cpit} = \theta_c + \delta_{pt} + \beta_0 Treated_{ci} + \sum_{\tau} D_{it}^{\tau} + \sum_{\tau} \beta_{\tau}(Treated_{ci} \times D_{it}^{\tau}) + \varepsilon_{cpit} \tag{4}$$

where D_{it}^{τ} are indicator variables for whether year t is τ years after (or before, if negative) the year of the incorporation. Figure 4 shows that the counties incorporated earlier and counties incorporated later do show parallel trends in the years before the incorporation, both in nighttime lights and in GDP per capita.

5 Estimates of the effect of the reform on economic growth

In this section, we present our treatment effects estimates using two novel control groups and conduct a series of robustness checks. We then discuss two main alternative explanations for the empirical patterns and show that none are supported in the data.

5.1 DID comparing incorporated counties to applied-but-failed counties

We begin by estimating the difference-in-differences model in equation 1 comparing incorporated counties to applied-but-failed counties. In Table 2, columns 1 and 3 are the baseline estimates that includes only county and province×year fixed effects. We add time-varying county-level controls in columns 2 and 4, including the manufacturing share of GDP, the tertiary industry share of GDP, and the ratio of government expenditure to government revenue.

Figure 3 plots the effect of the reform on the log of GDP per capita and lights per square kilometer respectively, based on estimates from equation 2. Year -1, the year before a treated county was incorporated into the prefecture, is set to be the baseline year. We first look at Year -6 to Year -1 to see whether the parallel trend holds. Neither the GDP per capita and the night-

time lights are significantly different between treated counties and applied-but-failed counties in any year before the reform, which implies that applied-but-failed counties are appropriate control groups for the treated counties.

GDP per capita in the treated counties increased by 11.7 percent relative to the applied-but-failed counties on average in the 10 years following the incorporation (Table 2). To put this number into perspective, Tombe and Zhu (2019) estimate the gains from a reduction in internal trade cost and migration cost to be around 22 percent. Our estimates of gains from easing internal barriers across counties and districts are roughly half as large as the between-province and between-sector gains identified by Tombe and Zhu (2019).

Panel A of Figure 3 shows that there is an immediate increase of GDP per capita in the year of the incorporation, suggesting that lifting inter-regional barriers can release a large amount of economic potential in a short period of time. The difference between the treated counties and the control counties becomes larger over time, which suggests that the *incorporating counties into prefectures reform* does not merely bring a one-time boost to the economy in the treated counties.

We then look at the impact of the reform on nighttime light intensity, which is not susceptible to political manipulations. Consistent with the results on GDP per capita, the reform increases the growth of lights per square kilometer by 4.8 percent in the treated counties, compared to the applied-but-failed counties. The magnitude of the effect on nighttime lights is smaller than that of GDP probably because not all economic activities can be measured through nighttime light intensity. Similar to results on GDP growth, nighttime light intensity in treated counties also increases immediately and persistently relative to the control counties (Panel b, Figure 3).

Lastly, we investigate whether the reform has varying effects based on whether the county incorporated is urban or rural. Out of the 259 counties examined during our study period, 53 were county-level cities while 206 were county-level counties. Notably, the urbanization rate is considerably higher for county-level cities. The estimated impacts of this reform are presented in Table 3, comparing county-level cities and counties. We find that the reform successfully promotes growth in both types of counties without any significant difference between them.

5.2 DID using variation in the timing of incorporation

To deal with the concern that there might be systematic differences between incorporated counties and applied-but-failed counties besides the incorporation, we exploit variation in the timing of the reform, i.e., we compare counties that experience the current reform to counties that experience the reform several years later. Appendix table A1 shows that no observable characteristics can persistently predict the timing of incorporations, so the timing of incorporations is arguably exogenous.

We compare counties that experience the current reform to counties that experience the reform five years later in our main analysis. We demonstrate that the main results are robust to different year gaps in Section 5.3. Figure 4 shows that the treated and control counties display parallel trends in GDP per capita and nighttime lights prior to event year 0, based on estimates from equation 4.

In Table 4, columns 1 and 3 are the baseline estimates that includes only county and province$\times$year fixed effects. We add time-varying county-level controls in columns 2 and 4, including the manufacturing share of GDP, the tertiary industry share of GDP, and the ratio of government expenditure to government revenue. GDP per capita and lights per square kilometer increase by 10 percent and 4 percent respectively in counties that experience the current reform relative to counties that experience the reform five years later (Table 4). The magnitudes are quite similar to the ones estimated using the applied-but-failed counties control group.

5.3 Robustness

One concern about the first approach is that the reform itself might be selected by the central government, and this selection could lead to changes in economic development in those counties. The central government might tend to choose richer counties for the reform. To address this concern, we re-run the main results without observations from the direct-administered mu-

nicipalities of China[15]. We find consistent results (in Appendix Figure A2) with even bigger magnitudes, suggesting that the positive impact of the reform is not driven by selection.

We use the five-year gap in the second approach as our main results. As a robustness check, we estimate the effects of the reform using alternative year gaps. Appendix Figure A1 shows that the results are robust if we change the year gap to a three-year, four-year, or six-year gap. Specifically, using alternative year gaps, the treated and control counties display parallel trends in GDP per capita and nighttime lights prior to event year 0; and the reform has a significantly positive impact on GDP per capita and lights per square kilometer.

Different clustering. We next check the extent to which our main results are influenced by alternative choices of clustering. In our main results, we cluster standard errors at the county level, allowing error terms to be correlated across time in the same county. In Appendix Table A6, we cluster standard errors at both the county and the year levels. The statistical inferences of our main results are not affected by alternative choices of clustering.

Pseudo-treatment placebo test. We demonstrate the statistical power of the inferences using our main specification by conducting a placebo test based on another pseudo-OCP exposure. Specifically, we randomly select 74 of the 259 counties in the sample for treatment, and also randomly assign the timing of adoption of the reform, constructing a variable of $reform_{ct}^{false}$. If our identifying assumption is satisfied, the coefficient of $reform_{ct}^{false}$ should be zero. To have sufficient power to reject the null that $reform_{ct}^{false}$ is zero, we repeat this test 500 times. In the Appendix, the distribution of the coefficients for $reform_{ct}^{false}$ is plotted in Figure A3. As evident from the figure, the effect estimates from the actual data fall in the extreme right tail of the distribution of pseudo-treatment, suggesting that it is unlikely that the effects we identify are due to chance.

5.4 Alternative Explanations

A migration of economic activities.

[15]The Direct-administered municipalities of China are Beijing, Tianjin, Shanghai, and Chongqing.

One potential concern for the positive impact of the reform is that the positive effects of the reform on treated counties were driven by involuntary migration of economic activity from prefectures to the treated counties with no gain on aggregate productivity. To provide evidence against this alternative explanation, we explore the overall impact of the reform on the economic growth of the treated counties and the corresponding prefectures as a whole. The unit of observation for overall effect is prefecture-year. Using the same empirical strategy, we compare prefectures that experienced the reform to prefectures that applied for the reform but failed, before and after the reform. For prefectures that have experienced several incorporations, we only focus on the first reform. We estimate a prefecture-level version of equation 1.

Figure 5 depicts the effect of the reform for the treated counties and the corresponding prefectures as a whole. The treated and control prefectures exhibit parallel trends in GDP per capita and nighttime lights before event year 0, based on estimates from a prefecture-level version of equation 2. Table 5 shows that the treated counties and prefectures as a whole gain in GDP per capita by 6.6 percent as a result of the reform. And the estimates are significant at 5 percent level. Notice that the results on nighttime lights are consistent with the results on GDP qualitatively but they are statistically insignificant. The possible reason is that the intensity of nighttime lights is capped at 63 and there is little brightness potential for the already-lighted prefectures.

Contemporary policy reforms.

It is possible that the positive effect we identified is due to contemporary policy reforms, such as the **province-managing-county** reform. This reform grants more fiscal and administrative autonomy to province-directly-administered counties compared to standard counties, which has been found by Li et al. (2016) to result in worse economic growth performance for these counties. It is plausible that the observed positive effect of incorporating-counties-into-prefectures reform may be driven by the negative impact of the province-managing-county reform on control counties.

To address this concern, we re-ran our regression excluding counties under direct provincial administration. The results are presented in Table 6, where it can be seen that even though removing these samples resulted in a 24% reduction in sample size, our main regression results

remained largely unchanged.

6 Mechanisms

The findings from the previous section suggest that the reform of incorporating counties into prefectures has a significantly positive impact on economic growth. The question now is: what are the underlying mechanisms responsible for this positive impact? One hypothesis is that it results from eliminating policy-induced frictions. By utilizing data from the Annual Industrial Surveys, we examine for evidence of friction reduction. The other explanation is that local government's behaviors change after the reform and those changes drive the beneficial effect. We first look into the friction reduction explanation.

6.1 Evidence of reduction in policy-induced frictions

Product market barriers. In the first piece of evidence, we demonstrate a decrease in inter-regional trade barriers as measured by Bai et al. (2004). According to Bai et al. (2004), local governments tend to favor State-Owned Enterprises (SOEs) over other types of enterprises due to the greater benefits they can derive from them. Therefore, if inter-regional barriers exist, industries with high shares of SOEs are less geographically concentrated. Firstly, we illustrate that the negative correlation between an industry's share of SOEs and its level of geographical concentration still holds at the county level. Secondly, we show that the reform significantly reduced this negative correlation in treated counties compared to control counties.

Following Ellison and Glaeser (1997), we calculate one industry's geographic concentration within a prefecture as follows:

$$\gamma_{ij} \equiv \frac{G_{ij} - (1 - \sum_c x_{cj}^2) H_{ij}}{(1 - \sum_c x_{cj}^2)(1 - H_{ij})} \tag{5}$$

where γ_{ij} is the Ellison-Glaeser index calculated at industry i within prefecture j. $G_{ij} \equiv \sum_c (s_{cij} - x_{cj})^2$ is the raw concentration, where s_{cij} is the share of employment for any county c within prefecture j in industry i and x_{cj} is the share of total employment of all industries in

county c. $H_{ij} \equiv \sum_{ij} z_{ij}^2$ is the Herfindahl index of industry i in prefecture j, with z_{ij} representing the employment share of a particular firm in industry i in prefecture j. A larger value of Ellison-Glaeser index represents a higher degree of geographic concentration.

To examine the above hypothesis, we employ a triple-differences framework as follows:

$$\begin{aligned}\gamma_{ijpt} = \alpha_i + \theta_j + \delta_{pt} + \beta_1 ssoe_{it} + \beta_2 ssoe_{it} \times treatCity_j + \beta_3 (ssoe_{it} \times Reform_{jt}) \\ + \beta_4 Reform_{jt} + \sum_t \beta_t (ssoe_{it} \times \delta_t) + \varepsilon_{ijpt}\end{aligned} \tag{6}$$

where γ_{ijpt} is the Ellison-Glaeser index in industry i, prefecture j of province p in year t. $ssoe_{it}$ is the share of SOEs in industry i in year t. The coefficients of interest are β_1 and β_3. α_i, θ_j and δ_{pt} are full sets of industry, prefecture and province-year fixed effects.

Column 1 of Table 7 firstly establishes the negative correlation found in Bai et al. (2004), namely industries with a high share of SOEs were less geographically concentrated. While Bai et al. (2004) study this correlation at the provincial level, our results show that inter-regional barriers also exists at the county level. Column 2 further shows that the reform significantly decreased the negative correlation between SOE share and concentration for treated counties after the reform compared to the control counties. More specifically, in the control counties, industries with high shares of SOEs were still less geographically concentrated. And the estimate is statistically significant at 1 percent level. While the negative correlation disappeared in the treated counties after the reform and the magnitude of the correlation is not statistically different from zero.

Second, following Melitz (2003)'s analysis on the effect of exposure to trade on inter-firm reallocations, we test the impact of the pro-trade reform on the inter-sector reallocations among the treated counties. The hypothesis is that the administrative reform eliminated frictions in the product market, and the treated counties should specialize more in industries in which they have comparative advantages. First, we estimate firms' productivity (or total factor productivity, TFP) using Ackerberg et al. (2006)'s method, which is commonly used in the literature. Then we find the most productive sectors at the 2-digit industry level in the baseline year by aggregating TFP at the county-sector-year level. We compare the evolution of production shares of the most productive sectors for treated counties to counties that applied but failed using the

following equation:

$$ProductionShare_{scpt} = \beta Reform_{sct} + \theta_c + \delta_{pt} + \varepsilon_{scpt} \tag{7}$$

where $ProductionShare_{sct}$ is the production share of sector s in county c of province p at year t. Each sector's production share is defined as the sector's output as a percentage of that county's total output.

Table 8 shows that compared to the applied-but-failed counties, the reform caused 1 percentage point increase (a 12-percent increase) in production share for each of the three most productive sectors (p-value=0.034) and a 2 percentage point increase (a 25-percent increase) for the most productive sector (p-value=0.066) in the treated counties. Estimates are statistically significant and economically large. Figure 6 further shows that the increase in production share is not driven by pre-trends.

Entry and exit barriers for firms. As the reform integrated counties into prefectures, it was expected that entry and exit barriers for firms would decrease. Our focus is on examining the entry of firms in treated counties. To do so, we have created a firm entry panel for both treatment and control counties each year using data from the Annual Industrial Survey.

Throughout the analyzed period, it was observed that incorporated counties had a significantly higher average of 82.6 companies entering each county every year compared to control counties with an average of only 18.0 companies entering per year. The difference is significant at the 1% level.

The newly-entered companies are similar in terms of ownership types and firm sizes. Private enterprises are the most prevalent among newly-entered businesses, accounting for 64% and 67.6% in incorporated and control counties respectively, while state-owned enterprises make up only a small percentage (1.1% in incorporated counties and 2.7% in control counties). We use output values and the number of employees to measure the size of newly-entered firms. On average, newly-entered firms in incorporated counties had an output value of approximately 106,110,000 yuan with around 216 employees; whereas those in control countries had an average output value of about 97,441,000 yuan with an employee count averaging at around

206.

We then run a regression to validate the results. Figure 7 (which is based on estimating equation 2) clearly illustrates an immediate entry of firms in the treated counties as compared to control counties. This immediate entry suggests that infrastructure improvement is unlikely to be the primary cause of entry since it takes time to establish. It's worth noting that firms began entering even one year before the formal announcement of the reform (as shown in Figure 7). One possible explanation for this could be that both firms and treated counties were already aware of the incorporation ahead of its formal announcement by the central government. To further demonstrate that this immediate entry was not driven by a positive impact in the year prior to reform, we have dummied out one year before reform in Table 9, columns 2 and 4. The results are consistent.

Subsequently, we aim to test the hypothesis that a decrease in barriers to entry and exit leads to increased competition among firms, resulting in less profitable firms being forced out of the market. The Annual Industrial Survey allows us to determine with precision the year in which a firm exits. Specifically, if a firm is present in the dataset for the year y, but not for the year $y+1$ and beyond, we define its exit year as year $y+1$. However, due to limitations within this survey, only State-Owned Enterprises' (SOEs) exits can be accurately observed.[16] Therefore our analysis will focus solely on SOE exits.

Throughout the study period, counties that were incorporated displayed a higher average of 2.9 SOE exits per year compared to the control group's average of 1.5 SOE exits per year, with a significant difference at the 1% level. Furthermore, when examining exiting firms' size, those located in incorporated counties had output value and number of employees averaging at 56,877,650 yuan and 411 people respectively; whereas their counterparts in control counties

[16]The Annual Industrial Survey includes all SOEs; however, non-state firms are only included if their sales exceed 5 million RMB. It is possible that some private firms may disappear from the annual survey either because they have exited or because their size has fallen below this threshold. Unfortunately, we cannot distinguish between these two scenarios.

had an average output value and number of employees amounting to 44,107,220 yuan and 343 people.

We utilize a triple-differences methodology, as outlined in equation 8, to examine whether companies which have a low profit-margin are more likely to exit within incorporated counties compared to control counties.

$$Exit_{icpt} = \theta_c + \delta_{pt} + \beta_1 profitMargin_{ic,t-1} + \beta_2 profitMargin_{ic,t-1} \times treatCounty_c + \beta_3 (profitMargin_{ic,t-1} \times Reform_{ct}) + \beta_4 Reform_{ct} + \sum_t \beta_t (profitMargin_{ic,t-1} \times \delta_t) + \varepsilon_{icpt} \tag{8}$$

where $Exit_{icpt}$ is an indicator variable for whether firm i in county c of province p exits at year t. $profitMargin_{ic,t-1}$ is the profit margin, which is defined as profit as a percentage of revenue, for firm i in year t-1. $treatCounty_c$ is an indicator equals to 1 if county c is incorporated.

We find that the firms with lower profit margins in the treated counties had a significantly higher probability of closing down after the reform in comparison to similar firms in the control counties (Table 10). Column 1 shows the less profitable firms were in general more likely to exit than more profitable firms, which is consistent with basic economic intuition. Column 2 presents that the probability of exiting for a firm with a low profit-margin in the treated counties is three times larger than that of a firm in the control group, which could be an outcome of the reduction in barriers.

Impacts on labor market. While we provide four pieces of evidence suggesting that the reform reduces frictions in the product market and barriers to entry and exit, we do not find any significant impact on the labor market. Table 11 shows that the reform did not significantly change the population in treated counties, compared to applied-but-failed counties. However, we need to interpret the result with caution. Here, we can only observe the change in population with local *Hukou* (residence permit), not the change in population who work in treated counties. It is possible that the population with local *Hukou* barely changes, but it attracts more people to work in treated counties.

6.2 Change in Government Behaviors

In this subsection, we examine whether the changes in government actions after the reform could potentially explain our findings. We analyze state-owned enterprise ownership, inter-governmental fiscal transfers, fiscal spending, and subsequent growth-oriented reforms. However, we have not discovered evidence in favor of this channel.

Ownership of SOEs. After the reform, there may be a potential change where more SOEs are subordinated to incorporated counties by the prefecture-level government. This could lead to economic growth in these counties. To investigate this possibility, we analyzed data from Annual Industrial Surveys. Our findings show that on average, 0.45 SOEs were subordinated from the prefecture level to the county level in incorporated counties during our study period, while control counties had an average of 0.29 SOEs under their jurisdiction. However, statistical analysis did not reveal any significant difference between the two numbers. We further validated this result through regression analysis which showed no significant increase in SOE ownership among treated counties (Table A9).

Inter-governmental fiscal transfers and fiscal spending. Another potential explanation for how the reform can stimulate economic growth is that counties that are treated may be granted additional preferential policies, such as increased transfer payments from higher-level governments, which would enable them to spend more.

To address this issue, we collected data on inter-governmental transfer and spending from Fiscal Statistical Compendium for All Prefectures and Counties, 1995 to 2009[17], and compared

[17]The dataset has detailed information on public income and expenditures at the county level. Two important public transfers from higher-level government to local government are general transfer and special transfer. General transfer refers to the public funds that higher-level government allocates to local governments with financial gaps (mainly in the central and western regions in China). Special transfer refers to the special grant funds from higher level government, and local governments need to use the funds according to the specified purpose. Special transfers usually concentrate on education, poverty, culture, unemployment, health, environment, and so on. The different functions of general transfer and special transfer from central government to local government can be found on the website of Ministry of Finance of China: http://www.mof.gov.cn/zhuantihuigu/cczqzyzfglbf/

the changes in transfer payments between treated counties and control counties before and after the reform. Table 12 shows the estimates of the effect of the reform on both general transfer per capita and special transfer per capita. As can be seen from the table, the reform did not significantly increase either form of transfer for treated counties compared to control counties.

Concerning fiscal spending, we have not discovered any indication of increased government spending in the treated counties. As demonstrated by Table A10, there is no significant rise in either overall government expenditure or expenditure per capita.

Subsequent policy reforms. Another alternative interpretation of the positive effects is that treated counties are more likely targeted for additional growth-oriented reforms due to the improved policy environment, and the growth we observed in the treated counties are driven by those subsequent reforms.

One salient growth-oriented reform in 1995–2013 period is China's economic zones. Economic zones, as one type of place-based economic development policy, are aimed at fostering economic growth in a specific area. Economic zones can potentially influence the location of economic activities, as well as wages, employment, investment, output, and productivity in the targeted area (Kline and Moretti, 2014; Lu et al., 2019).

To address this concern, we investigate whether the reform motivates local governments to set up economic zones or increase the number of economic zones within jurisdictions. The data of economic zones comes from National Development and Reform Commission.[18] Appendix Figure A2 plots the effect of the reform on the number of economic zones based on estimates from equation 2. Notice that the treated and the applied-but-failed counties display parallel trends in the number of economic zones prior to event year 0. We find that incorporated counties were not more likely to establish economic zones compared to control counties (Table 13). Both coefficients in columns 1 and 2 are positive but not statistically significant, indicating that the reform does not boost economic growth by establishing economic zones.

[18]Data source: https://www.ndrc.gov.cn/fggz/lywzjw/zcfg/201803/t20180302_1047056.html?code=&state=123

7 Conclusion

This paper examines the effects of removing inter-regional barriers on economic growth by analyzing a policy reform in China. Despite living in an era of globalization, various government policies create obstacles that hinder economic activities across different regions. Although China has made a significant effort to transition from a centrally planned economy to a market-oriented one, local governments still use their administrative autonomy to impede the free movement of goods, people, and capital. The reform under investigation aims to address this issue by integrating counties into prefectures and reducing friction between them.

We find that counties that were incorporated into prefectures experienced higher economic growth after the reform, in comparison to those counties that attempted incorporation but were unsuccessful and those that were incorporated later on. We also provide suggestive evidence that the reform effectively decreased policy-induced frictions in the product market, as well as lowered barriers for firms entering and exiting.

This paper highlights the significance of determining the ideal degree of decentralization in the context of policy and institutional design. Drawing from empirical evidence over the past three decades, decentralization has been shown to enhance economic growth by improving the efficiency of local public services (Davoodi and Zou, 1998; Zhang and Zou, 1998; Xie et al., 1999). However, it can also introduce policy-induced frictions that impede the flow of elements across different jurisdictions (Jin et al., 2005; Qiao et al., 2008; Gemmell et al., 2013). In 2022, the State Council of China released the file "Opinions on Accelerating the Construction of a Large National Unified Market", acknowledging the challenges posed by local protectionism and regional regulatory barriers, and emphasizing the need for a unified domestic market. Based on our findings, to achieve this objective, an effective approach is to reduce administrative hierarchies and pursue a balanced degree of decentralization.

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Figures

Figure 1: Government Structure in China

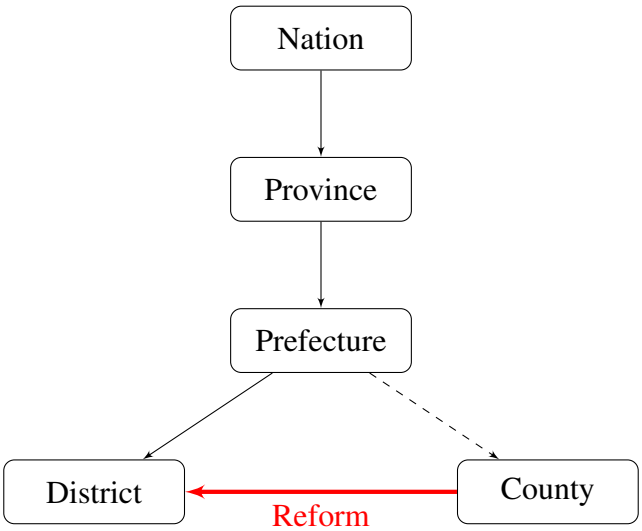
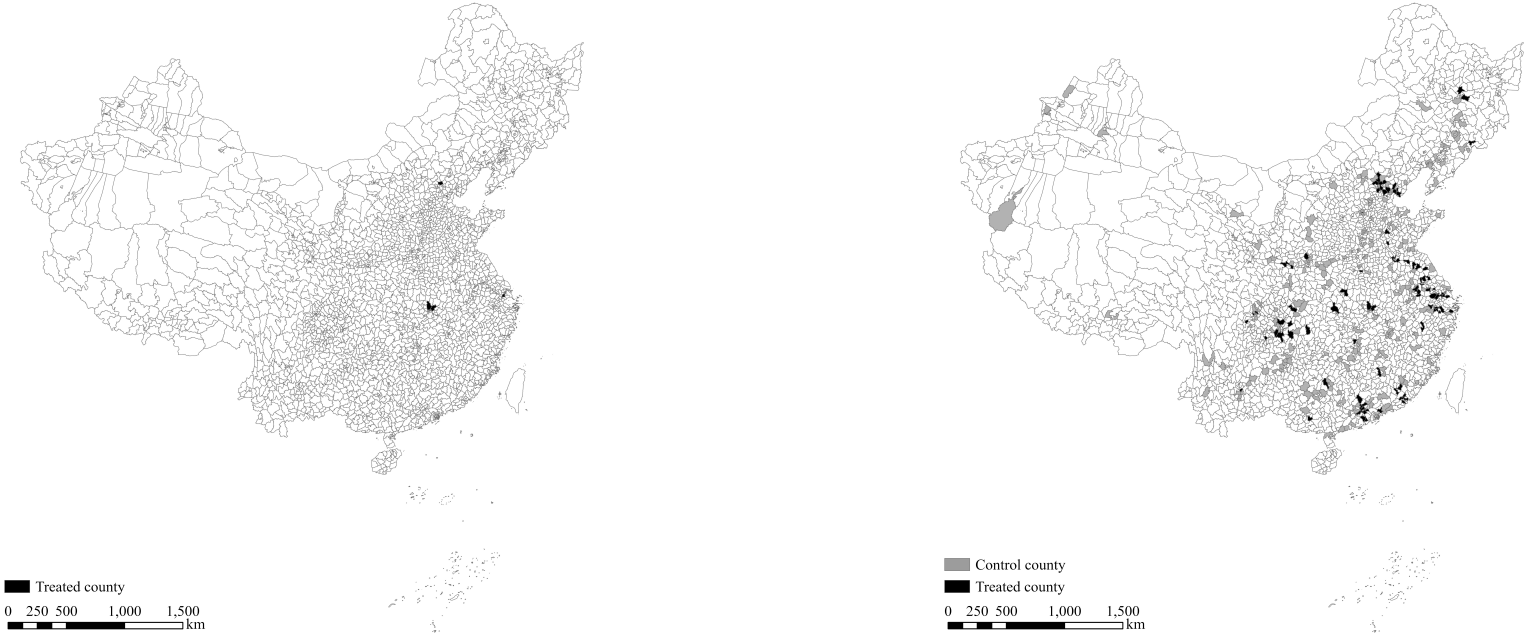


Figure 2: Geographical Distribution of Treated and Control Counties

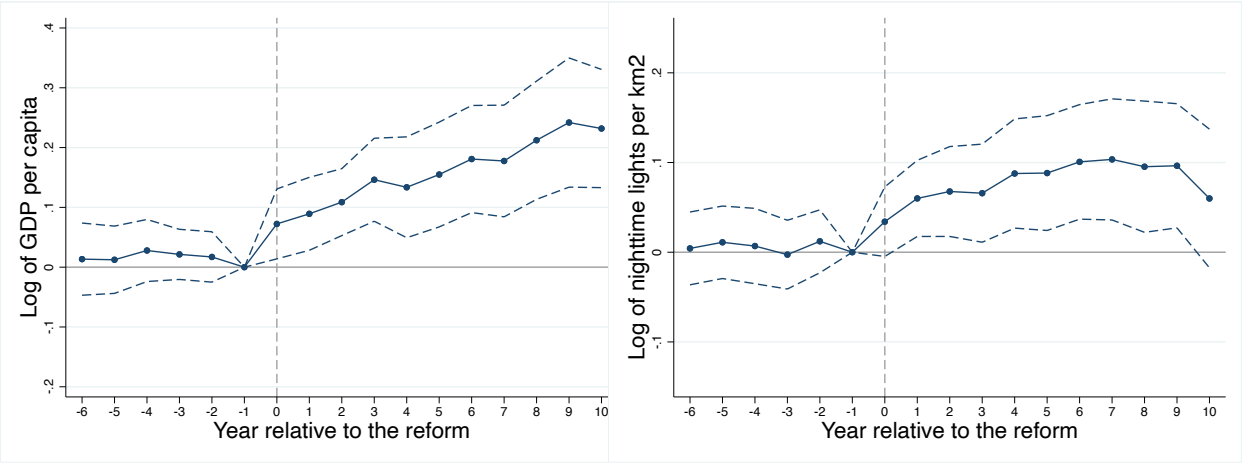


(a) Year 1998

(b) Year 2013

The map presents locations of the incorporated counties and applied-but-failed counties.
Source: Author's mapping based on data from the Ministry of Civil Affairs of China and prefectures' city-planning books.

Figure 3: The Impact of Market Integration on Economic Development (Approach I)

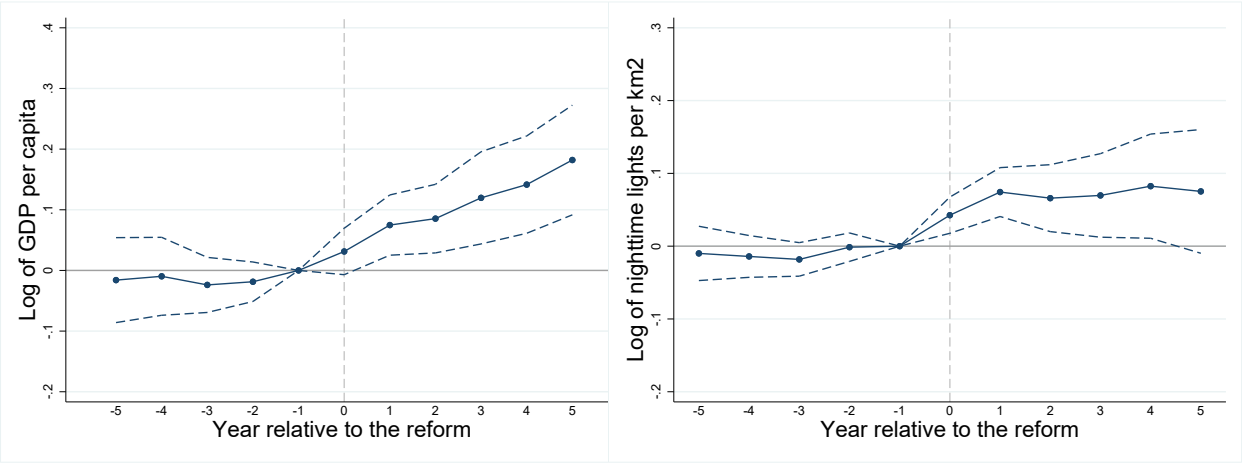


(a) The Impact on GDP per capita

(b) The Impact on Lights per km^2

Notes: The figure plots point estimates and 95% confidence intervals for the effect of the incorporation reform on GDP and nighttime lights before and after the reform, based on estimates of coefficients from equation 2. Outcome variables are the log of GDP per capita or the log of nighttime lights per km^2.

Figure 4: The Impact of Market Integration on Economic Development (Approach II)

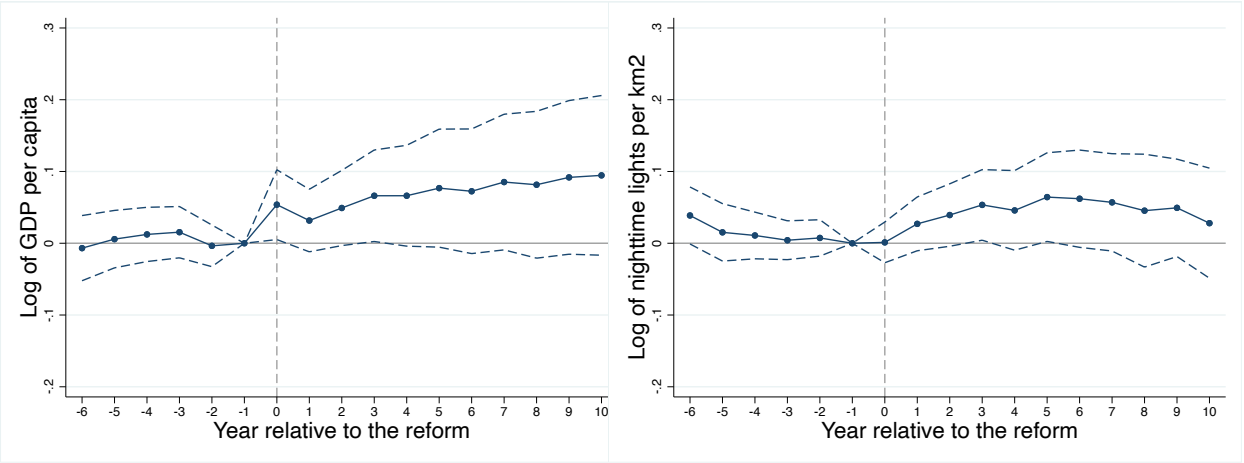


(a) The impact on GDP per capita

(b) The Impact on Lights per km^2

Notes: The figure plots point estimates and 95% confidence intervals for the effect of the incorporation reform on GDP and nighttime lights , based on estimates of coefficients from equation 4. Outcome variables are the log of GDP per capita or the log of nighttime lights per km^2.

Figure 5: The Overall Impact of Market Integration on Economic Development

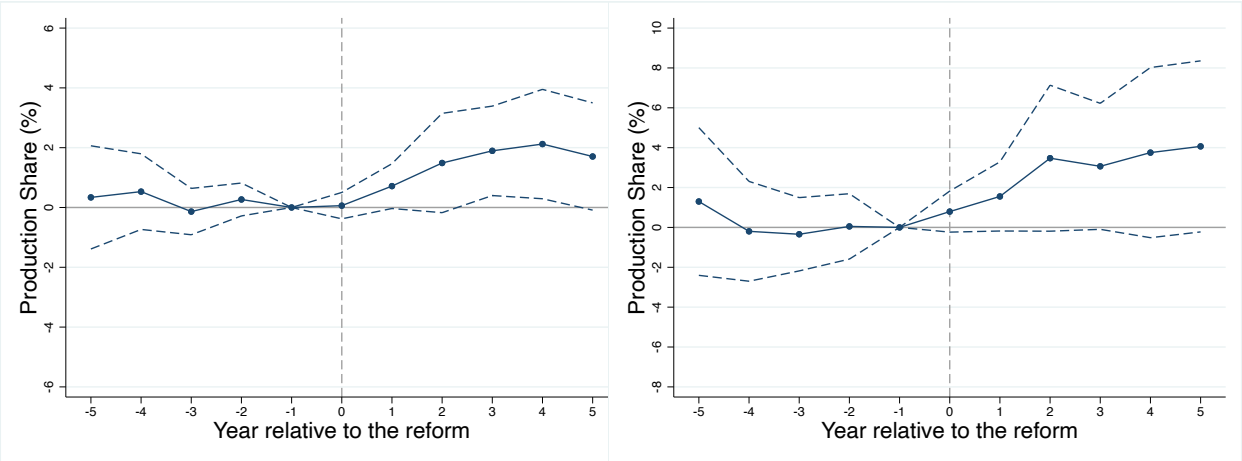


(a) The Impact on GDP per capita

(b) The Impact on Lights per km^2

Notes: The figure plots point estimates and 95% confidence intervals for the effect of the incorporation reform on GDP and nighttime lights, based on estimates of coefficients from equation 2. Outcome variables are the log of GDP per capita or the log of nighttime lights per km^2. The observation unit is prefecture-year.

Figure 6: The Effect of Market Integration on Reallocation

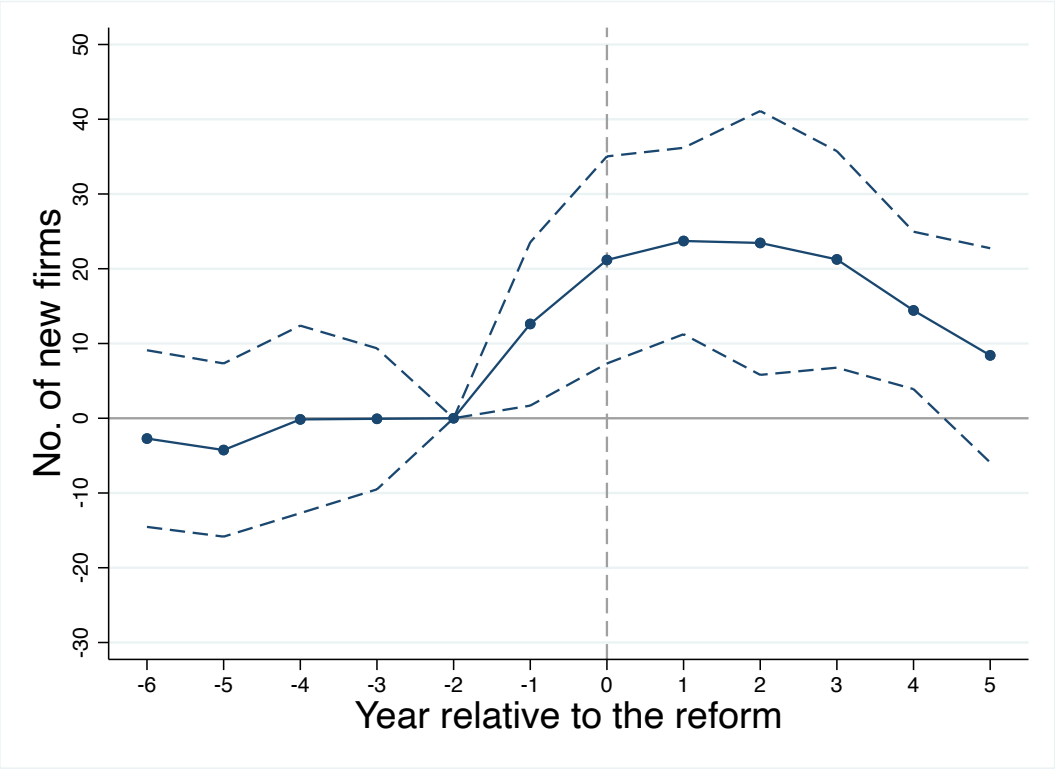


(a) Top 3 productive sectors

(b) Most productive sector

Notes: The figure plots point estimates and 95% confidence intervals for the effect of the incorporation reform on production share of the most productive sectors, based on estimates of coefficients from equation 2. The dependent variable is the production share of the most productive sectors at the 2-digit industry level, which is defined as the output of the sector as a percentage of the county's total output. The sample contains the top three productive sectors in panel (a) and the most productive sector in panel (b).

Figure 7: The Effect of Market integration on Firms' Entry



Notes: The figure plots point estimates and 95% confidence intervals for the effect of the incorporation reform on the number of new firms before and after the reform, based on estimates of coefficients from equation 2. The dependent variable is number of new firms in the counties.

Tables

Table 1: Summary Statistics (Baseline Year)

	Incorporated counties (1)	Applied-but-failed counties (2)	Difference (3)	p-Value (4)
Population (log)	4.179	3.804	.177	0.035
	(.518)	(.632)		
Share of rural population	.866	.869	-.006	0.672
	(.124)	(.116)		
Share of rural labor participation	.454	.433	.008	0.372
	(.087)	(.089)		
Food possession per capita	497.6	488.5	25.47	0.374
	(211.4)	(258.5)		
Manufacturing share of GDP	.466	.353	.079	0.000
	(.099)	(.128)		
Tertiary industry share of GDP	.267	.250	.010	0.246
	(.060)	(.060)		
Ratio of gov. expenditure to gov. revenue	1.579	1.847	-.106	0.362
	(.677)	(1.003)		
Saving share of GDP	.477	.514	.010	0.863
	(.232)	(.752)		
Loan share of GDP	.579	.615	.016	0.792
	(.329)	(.596)		
Students per 10000 people	1558.7	1655.9	-31.54	0.460
	(324.1)	(318.5)		
Student-teacher ratio	20.88	19.04	.558	0.684
	(17.69)	(4.761)		
Number of Counties	74	185	-	-

Note: The table reports the summary statistics of the treatment and applied-but-failed counties. Columns 3 and 4 report differences and p-values conditional on province fixed effects.

Table 2: Estimated Effects of the Reform on Economic Growth

Dependent variable	Log of GDP per capita		Log of lights per km^2	
	(1)	(2)	(3)	(4)
Reform	0.124***	0.117***	0.056**	0.048**
	(0.035)	(0.031)	(0.024)	(0.023)
County-level controls		Y		Y
County FE	Y	Y	Y	Y
Province×Year FE	Y	Y	Y	Y
Observations	4,610	4,610	4,604	4,604
R-squared	0.965	0.971	0.984	0.984
Mean DV	9.017	9.017	1.749	1.749
Std.Dev. DV	0.915	0.915	0.846	0.846

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 1. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the county level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP, ratio of government expenditure to government revenue. Log of population is also included as the county-level control for the results on nighttime lights.

Table 3: Estimated Heterogeneous Effects of the Reform on Economic Growth - Comparing Counties and County-level Cities

Dependent variable	Log of GDP per capita		Log of lights per km^2	
	(1)	(2)	(3)	(4)
Reform	0.117***	0.090***	0.063**	0.054**
	(0.038)	(0.032)	(0.026)	(0.026)
Reform × County-level cities	0.018	0.065	-0.016	-0.015
	(0.061)	(0.058)	(0.043)	(0.042)
County-level controls		Y		Y
County FE	Y	Y	Y	Y
Province×Year FE	Y	Y	Y	Y
Observations	4,610	4,610	4,604	4,604
R-squared	0.965	0.971	0.984	0.984
Mean DV	9.017	9.017	1.749	1.749
Std.Dev. DV	0.915	0.915	0.846	0.846

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 1. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the county level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP, ratio of government expenditure to government revenue. Log of population is also included as the county-level control for the results on nighttime lights.

Table 4: Estimated Effects of the Reform on Economic Growth: Only Use Time Variation

Dependent variable	Log of GDP per capita		Log of lights per km^2	
	(1)	(2)	(3)	(4)
Treatment×Post	0.106***	0.103***	0.056**	0.039*
	(0.037)	(0.036)	(0.024)	(0.023)
County-level controls		Y		Y
County FE	Y	Y	Y	Y
Province×Year FE	Y	Y	Y	Y
Observations	14,604	14,604	14,592	14,592
R-squared	0.986	0.990	0.990	0.990
Mean DV	8.920	8.920	1.834	1.834
Std.Dev. DV	0.759	0.759	0.713	0.713

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 3. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the incorporation level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP, ratio of government expenditure to government revenue. Log of population is also included as the county-level control for the results on nighttime lights.

Table 5: Overall Effect of the Reform on Prefectures' Economic Growth

Dependent variable	Log of GDP per capita	Log of lights per km^2
	(1)	(2)
Treatment×Post	0.066**	0.024
	(0.032)	(0.023)
Prefecture FE	Y	Y
Year FE	Y	Y
Observations	2,833	2,848
R-squared	0.966	0.984
Mean DV	9.393	2.052
Std.Dev. DV	0.914	0.826

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns present estimates of β_1 from equation 1 at the prefecture level. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the prefecture level. Log of the population is included as the prefecture-level control for the results on nighttime lights.

Table 6: Estimated Effects of the Reform on Economic Growth - Without the Province-managing-counties

Dependent variable	Log of GDP per capita		Log of lights per km^2	
	(1)	(2)	(3)	(4)
Reform	0.139***	0.128***	0.050*	0.041
	(0.042)	(0.036)	(0.028)	(0.026)
County-level controls		Y		Y
County FE	Y	Y	Y	Y
Province×Year FE	Y	Y	Y	Y
Observations	3,525	3,525	3,519	3,519
R-squared	0.960	0.967	0.983	0.984
Mean DV	9.042	9.042	1.765	1.765
Std.Dev. DV	0.874	0.874	0.846	0.846

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 1. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the county level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP, ratio of government expenditure to government revenue. Log of population is also included as the county-level control for the results on nighttime lights.

Table 7: Mechanism: Geographical Concentration

Dependent variable	Concentration Index	
	(1)	(2)
Share of SOEs	-0.076**	-0.127***
	(0.032)	(0.041)
Share of SOEs×Treat		0.029
		(0.050)
Share of SOEs×Treat×Post		0.176***
		(0.054)
Industry FE	Y	Y
Prefecture FE	Y	Y
Province×Year FE	Y	Y
Observations	213,516	213,516
R-squared	0.082	0.082
Mean DV	0.220	0.220
Std.Dev. DV	0.549	0.549

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1, β_2 and β_3 from equation 6. All regressions include county fixed effects and province×year fixed effects (not reported). In parentheses are standard errors clustered by incorporation. Number of clusters: 152. The industry-level controls include the number of firms (log), average profit (log) and average employment (log). Number of prefectures: 152 (45 prefectures in treatment and 107 in control). Number of manufacturing industries defined by the four-digit classifications: 424. The observation unit is at the industry-prefecture-year level.

Table 8: Mechanism: Inter-sector Reallocation

Dependent variable	Production Shares for Most Productive Sectors	
	Top three sectors	Top sector
	(1)	(2)
Reform	1.050**	2.189*
	(0.493)	(1.187)
County FE	Y	Y
Province * Year FE	Y	Y
Observations	5,837	1,863
R-squared	0.343	0.868
Mean DV	8.550	8.594
Std.Dev. DV	13.181	12.863

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the production share of the most productive sectors at the 2-digit industry level, which is defined as the output of the sector as a percentage of the county's total output. The columns presents estimates of β_1 from equation 1. All regressions include county fixed effects and province×year fixed effects. The sample contains the top three productive sectors in column (1) and the most productive sector in column (2). Robust standard errors are in parentheses, clustered at the county level. The observation unit is at the county-sector-year level.

Table 9: Mechanism: Firms' Entry

Dependent variable	Number of New Firms	
	(1)	(2)
Reform	16.463***	19.487***
	(5.748)	(6.602)
One Year relative		14.312**
		(6.679)
County FE	Y	Y
Province×Year FE	Y	Y
Observations	3,281	3,281
R-squared	0.905	0.905
Mean DV	37.468	37.468
Std.Dev. DV	68.830	68.830

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the number of new firms. The columns presents estimates of β_1 from equation 1. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the county level. The observation unit is at the county-year level.

Table 10: Mechanism: Firms' Exit

Dependent variable	Dummy for Exit	
	(1)	(2)
Profit margin(lag)	-0.022***	-0.018**
	(0.004)	(0.008)
Profit margin(lag)×Treat		-0.001
		(0.007)
Profit margin(lag)×Treat×Post		-0.034***
		(0.009)
Industry FE	Y	Y
County FE	Y	Y
Province×Year FE	Y	Y
Observations	18,258	18,258
R-squared	0.130	0.135
Mean DV	0.119	0.119
Std.Dev. DV	0.324	0.324

Notes: Profit margin is defines as profit as a percentage of revenue. *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1, β_2 and β_3 from equation 8. All regressions include county fixed effects, industry (at the 4-digit level) fixed effects and province×year fixed effects (not reported). In parentheses are standard errors clustered by county. The observation unit is at the firm-year level.

Table 11: Mechanism: Labor Market Frictions

Dependent variable	Log of Population	
	(1)	(2)
Reform	-0.027	-0.025
	(0.018)	(0.018)
County-level controls		Y
County FE	Y	Y
Province×Year FE	Y	Y
Observations	4,610	4,610
R-squared	0.988	0.988
Mean DV	3.964	3.964
Std.Dev. DV	0.611	0.611

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 1. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the county level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP, ratio of government expenditure to government revenue. The observation unit is at the county-year level.

Table 12: Estimated Effects of the Reform on Public Transfer

Dependent variable	Log of general transfer per capita		Log of special transfer per capita	
	(1)	(2)	(3)	(4)
Reform	-0.109	-0.100	-0.026	-0.022
	(0.172)	(0.162)	(0.075)	(0.074)
County-level controls		Y		Y
County FE	Y	Y	Y	Y
Province×Year FE	Y	Y	Y	Y
Observations	1,907	1,907	3,417	3,417
R-squared	0.833	0.834	0.830	0.830
Mean DV	4.166	4.166	4.602	4.602
Std.Dev. DV	2.002	2.002	1.387	1.387

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 1. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the county level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP, ratio of government expenditure to government revenue. The sample period is 2000-2009 in Columns (1) and (2), and it is 1995-2009 in Columns (3) and (4).

Table 13: Estimated Effects of the Reform on Number of Economic Zones

Dependent variable	Number of Economic Zones	
	(1)	(2)
Reform	0.064	0.065
	(0.059)	(0.059)
County-level controls		Y
County FE	Y	Y
Province×Year FE	Y	Y
Observations	4,639	4,639
R-squared	0.686	0.686
Mean DV	9.017	9.017
Std.Dev. DV	0.915	0.915

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 1. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the county level. The county-level controls include the manufacturing share of GDP, tertiary industry share of GDP, and ratio of government expenditure to government revenue.

Appendix

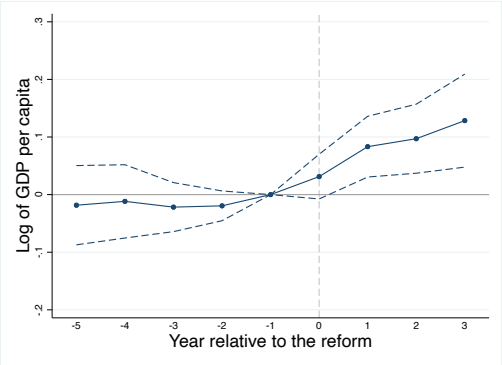
A0.1 Nighttime Light Data

The nighttime light intensity data is collected by the Operational Linescan System (OLS) sensor of Defense Meteorological Satellite Program (DMSP). The website for the data is https://ngdc.noaa.gov/eog/download.html. The DMSP-OLS sensor has the ability to detect nighttime lights, ranging from modest residential areas to traffic signals. This is in contrast with the obscure rural surroundings that prevail during nighttime hours. As a result, these luminous nightlights can serve as excellent data sources for human activities.

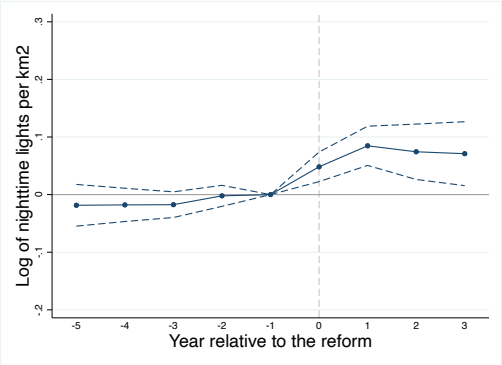
We utilize the reliable nighttime illumination data from the DMSP-OLS dataset. This information is derived by computing the mean of visible light and gray values for each year, while also eliminating any sporadic disturbances such as clouds or firelights. The digital number value for stable nighttime light falls within a range of 0 to 63.

To obtain data on China, we utilized worldwide stable nighttime light data and input it into ArcMap software. We then extracted the administrative border of China by converting the projection of the global light image data into a Lambert conformal conic projection and cutting out China's boundaries. By using digit numbers assigned to each grid cell, we calculated the average lights per square kilometer within each county/district boundary. This value serves as an alternative measure for local economic development.

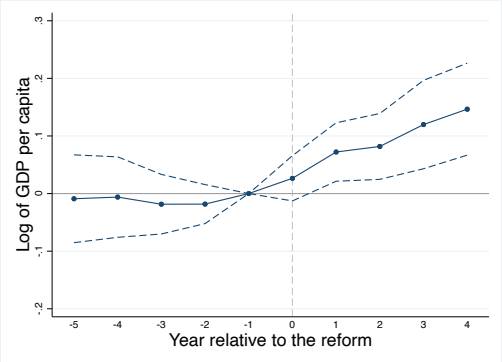
Figure A1: Robustness: The Impact of Market Integration on Economic Development (Approach II)



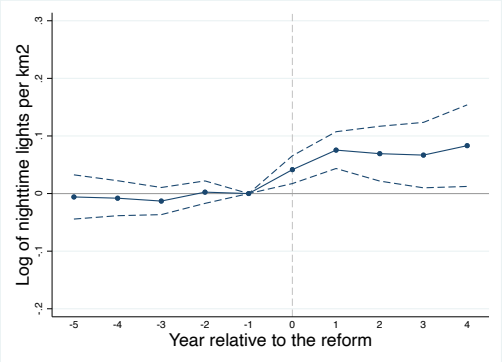
(a) The impact on GDP per capita (3-year gap)



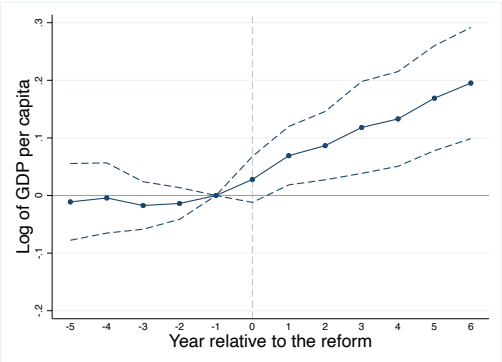
(b) The Impact on Lights per km^2 (3-year gap)



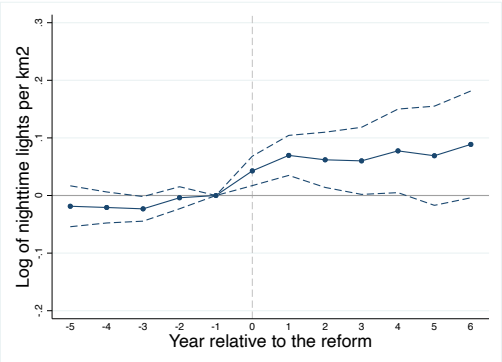
(c) The impact on GDP per capita (4-year gap)



(d) The Impact on Lights per km^2 (4-year gap)



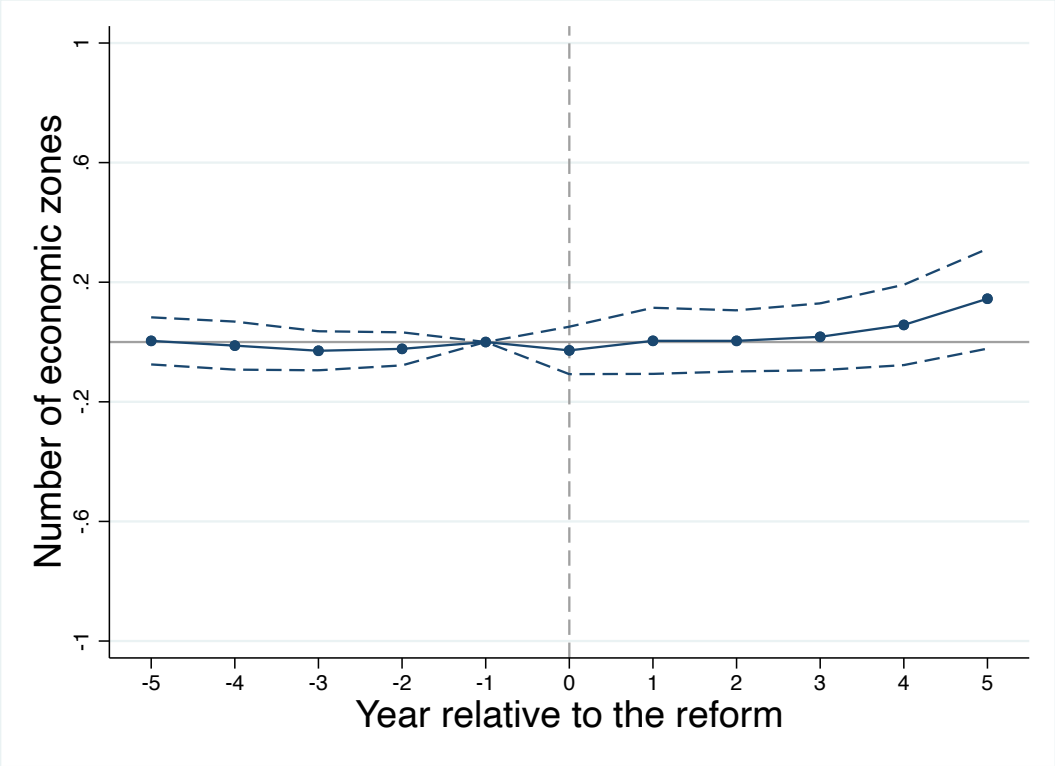
(e) The impact on GDP per capita (6-year gap)



(f) The Impact on Lights per km^2 (6-year gap)

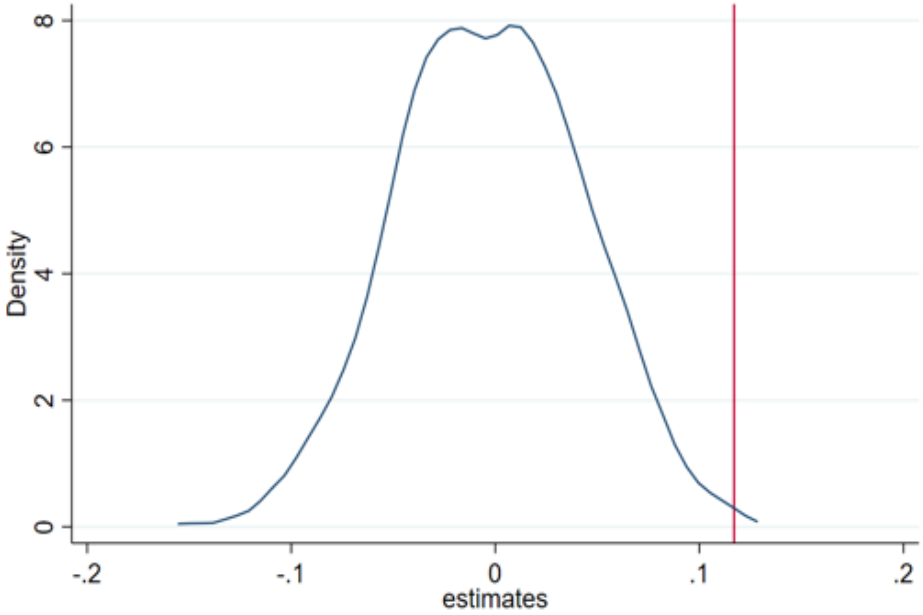
Notes: Robustness check for Approach II. I compare counties that experience the current incorporation to counties that would experience the reform three, four or six years later respectively. The figure plots estimates of the effect of the reform on GDP and nighttime lights in treated counties in the years before and after the reform, based on estimates of coefficients from equation 4. The dependent variables are the log of GDP per capita or the log of nighttime lights per km^2. Dashed lines are 95 percent confidence intervals.

Figure A2: Event Study of the Reform on the Number of Economic Zones



Notes: The figure plots point estimates and 95% confidence intervals for the effect of the incorporation reform on the number of economic zones before and after the reform, based on estimates of coefficients from equation 2 at the county level. The dependent variable is the number of economic zones.

Figure A3: The distribution of placebo test for GDP



Notes: We randomly assign the treatment status to the counties, and re-estimate the baseline specification. The mean value for the estimates is -0.0023. The mean value for the p-value is 0.501. Most of the estimated coefficients are not significant.

Table A1: Factors that Predict Timing of Incorporations

	Timing of incorporations			
	1998	2002	2006	2011
	(1)	(2)	(3)	(4)
Population (lag)	0.000	-0.000	0.001	-0.001
	(0.000)	(0.002)	(0.002)	(0.007)
Manufacturing share of GDP (lag)	-0.087	1.087	0.496	1.341
	(0.179)	(0.971)	(0.918)	(3.662)
Tertiary share of GDP (lag)	-0.184	1.278	0.106	1.169
	(0.239)	(1.323)	(1.207)	(3.965)
Ratio of gov. expenditure to gov. revenue (lag)	-0.007	-0.099	-0.019	0.004
	(0.014)	(0.075)	(0.080)	(0.153)
Ratio of gov. revenue to GDP (lag)	0.113	-5.198	-17.900*	3.014
	(1.177)	(8.029)	(10.154)	(9.246)
Ratio of gov. expenditure to GDP (lag)	-0.338	7.381	8.496	1.146
	(0.944)	(5.561)	(5.204)	(8.022)
Log of lights per km^2 (lag)	0.032	0.265**	0.285**	-0.156
	(0.041)	(0.112)	(0.126)	(0.464)
Dummy of county level city	-0.045	-0.095	0.294	0.037
	(0.037)	(0.209)	(0.198)	(0.369)
Dummy of provincial capital	0.053	-0.063	0.390	0.308
	(0.061)	(0.209)	(0.237)	(0.565)
Dummy of direct-administered municipalities of China	0.100	0.118	1.054***	-
	(0.103)	(0.174)	(0.103)	-
Observations	73	49	28	20

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

Table A2: Estimated Effects of the Reform on Economic Growth: Approach I (Without Sample of Direct-administered Municipalities of China)

Dependent variable	Log of GDP per capita		Log of lights per km^2	
	(1)	(2)	(3)	(4)
Reform	0.163***	0.151***	0.063**	0.053**
	(0.040)	(0.036)	(0.028)	(0.026)
County-level controls		Y		Y
County FE	Y	Y	Y	Y
Province×Year FE	Y	Y	Y	Y
Observations	4,140	4,140	4,134	4,134
R-squared	0.965	0.971	0.983	0.984
Mean DV	8.995	8.995	1.746	1.746
Std.Dev. DV	0.925	0.925	0.838	0.838

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 1. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the county level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP, ratio of government expenditure to government revenue. Log of population is also included as the county-level control for the results on nighttime lights.

Table A3: Estimated Effects of the Reform on Economic Growth: Approach II (Without sample of Direct-administered Municipalities of China)

Dependent variable	Log of GDP per capita		Log of lights per km^2	
	(1)	(2)	(3)	(4)
Treatment×Post	0.113***	0.113***	0.062**	0.042*
	(0.040)	(0.039)	(0.026)	(0.024)
County-level controls		Y		Y
County FE	Y	Y	Y	Y
Province×Year FE	Y	Y	Y	Y
Observations	13,938	13,938	13,926	13,926
R-squared	0.986	0.990	0.989	0.990
Mean DV	8.931	8.931	1.872	1.872
Std.Dev. DV	0.772	0.772	0.693	0.693

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 3. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the incorporation level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP, ratio of government expenditure to government revenue. Log of population is also included as the county-level control for the results on nighttime lights.

Table A4: Estimated Effects of the Reform on Land Price

Dependent variable	Log of residential land price		Log of commercial land price	
	(1)	(2)	(3)	(4)
Reform	-0.224	-0.208	0.124	0.090
	(0.162)	(0.177)	(0.118)	(0.105)
County-level controls		Y		Y
County FE	Y	Y	Y	Y
Province×Year FE	Y	Y	Y	Y
Observations	5,994	5,994	1,344	1,344
R-squared	0.579	0.579	0.592	0.595
Mean DV	9.646	9.646	9.857	9.857
Std.Dev. DV	0.617	0.617	0.573	0.573

Notes: Parcel-level data on land transaction are collected from the official websites of China's Ministry of Land and Resources (http://landchina.mlr.gov.cn) and this records most land transactions after 2007. Each land parcel transaction includes the transacted price, hectares of the land, location of the land, name of the company who rented the land and what types of land are transacted. *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 1. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the incorporation level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP, ratio of government expenditure to government revenue.

Table A5: Estimated Effects of the Reform on Economic Growth

Dependent variable	Log of GDP per capita		Log of lights per km^2	
	(1)	(2)	(3)	(4)
Reform	0.124***	0.117***	0.056**	0.048**
	(0.035)	(0.031)	(0.024)	(0.023)
Log of population				-0.037
				(0.052)
Manufacturing share of GDP		1.553***		0.286***
		(0.194)		(0.103)
Tertiary industry share of GDP		1.234***		-0.002
		(0.197)		(0.113)
Ratio of government expenditure		-0.020***		-0.011**
to government revenue		(0.006)		(0.005)
County FE	Y	Y	Y	Y
Province×Year FE	Y	Y	Y	Y
Observations	4,610	4,610	4,604	4,604
R-squared	0.965	0.971	0.984	0.984
Mean DV	9.017	9.017	1.749	1.749
Std.Dev. DV	0.915	0.915	0.846	0.846

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 1. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the county level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP, ratio of government expenditure to government revenue. Log of population is also included as the county-level control for the results on nighttime lights.

Table A6: Estimated Effects of the Reform on Economic Growth - Twoway Clustering

Dependent variable	Log of residential land price		Log of commercial land price	
	(1)	(2)	(3)	(4)
Reform	0.124***	0.117***	0.056**	0.048**
	(0.033)	(0.031)	(0.022)	(0.021)
County-level controls		Y		Y
County FE	Y	Y	Y	Y
Province×Year FE	Y	Y	Y	Y
Observations	4,610	4,610	4,604	4,604
R-squared	0.965	0.971	0.984	0.984
Mean DV	9.017	9.017	1.749	1.749
Std.Dev. DV	0.915	0.915	0.846	0.846

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 1. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the incorporation level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP, ratio of government expenditure to government revenue. Log of population is also included as the county-level control for the results on nighttime lights.

Table A7: Estimated Heterogeneous Effects of the Reform on Economic Growth - Compare Inland and Coastal Counties

Dependent variable	Log of GDP per capita		Log of lights per km^2	
	(1)	(2)	(3)	(4)
Reform	0.144***	0.128***	0.059**	0.048*
	(0.039)	(0.036)	(0.027)	(0.025)
Reform × coastal	-0.121*	-0.068	-0.020	-0.005
	(0.069)	(0.070)	(0.051)	(0.050)
County-level controls		Y		Y
County FE	Y	Y	Y	Y
Province×Year FE	Y	Y	Y	Y
Observations	4,610	4,610	4,604	4,604
R-squared	0.965	0.971	0.984	0.984
Mean DV	9.017	9.017	1.749	1.749
Std.Dev. DV	0.915	0.915	0.846	0.846

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 1. Coastal is an indicator variable where coastal counties equal one. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the county level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP, ratio of government expenditure to government revenue. Log of population is also included as the county-level control for the results on nighttime lights.

Table A8: Factors that Predict Incorporations

Dependent variable	Incorporated	
	(1)	(2)
County level city	0.179**	0.083
	(0.077)	(0.082)
Population (lag)		0.003***
		(0.001)
Manufacturing share of GDP(lag)		0.816***
		(0.273)
Tertiary share of GDP (lag)		-0.199
		(0.462)
Ratio of gov. expenditure to gov. revenue (lag)		-0.018
		(0.022)
Province FE	Y	Y
Observations	215	215
R-squared	0.400	0.455
Mean DV	0.321	0.321
Std.Dev. DV	0.468	0.468

Notes: *** p<0.01, ** p<0.05, * p<0.1. All regressions include province fixed effects.

Table A9: Estimated Effects of the Reform on SOE Ownership

Dependent variable	Change ownership
	(1)
Reform	-.0000757
	(0.006)
County FE	Y
Province×Year FE	Y
Observations	34,768
R-squared	0.091
Mean DV	0.009
Std.Dev. DV	0.096

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is whether a SOE was subordinated from the prefecture level to the county level. The regression includes county fixed effects and province×year fixed effects.

Table A10: Estimated Effects of the Reform on Government Expenditure

Dependent variable	Log of total gov. expenditure		Log of gov. expenditure per capita	
	(1)	(2)	(3)	(4)
Reform	0.024	0.024	0.049	0.047
	(0.035)	(0.035)	(0.030)	(0.029)
County-level controls		Y		Y
County FE	Y	Y	Y	Y
Province×Year FE	Y	Y	Y	Y
Observations	4,643	4,643	4,614	4,614
R-squared	0.982	0.982	0.981	0.981
Mean DV	10.859	10.859	6.901	6.901
Std.Dev. DV	1.248	1.248	1.184	1.184

Notes: *** p<0.01, ** p<0.05, * p<0.1. The columns presents estimates of β_1 from equation 1. Coastal is an indicator variable where coastal counties equal one. All regressions include county fixed effects and province×year fixed effects. Robust standard errors are in parentheses, clustered at the county level. The county-level controls include manufacturing share of GDP, tertiary industry share of GDP.