

# Regional Privatisation and Firm Performance: Evidence from China\*

Sally Yang Kaya<sup>1</sup>

<sup>1</sup>The London School of Economics and Political Science, London, UK

[Click for latest version](#)

6 May 2022

## Abstract

I present empirical evidence that China's privatisation of state-owned enterprises (SOEs) improved firm performance. Between 1998 and 2007, a national policy caused industries to privatise at vastly different rates. This affected cities differentially due to regional variation in industry specialisations. I therefore construct a shift-share instrument for regional privatisation. Using a panel data set of firms and cities, I find that a 1 percentage point increase in local private sector employment share increased average firm total factor productivity (TFP) by 1.87%. Put differently, the average TFP will increase fivefold if a city goes from 0% to 100% privatised. My analysis indicates that regional privatisation also improved labour and capital productivity and increased SOE wages. However, it has not significantly affected firm profitability, investments, or within-city misallocation of capital and labour inputs. Further mechanisms analyses reveal that private sector expansion may be a key channel through which privatisation improved firm productivity.

---

\*I am deeply grateful to Professor Alessandro Gavazza for his invaluable guidance and feedback. I thank Dr Matthew Levy, Dr Rachael Meager, Eddy Zou and all members of the LSE BSc Econometrics and Mathematical Economics Class of 2022 for their advice and feedback. I also thank Professor Mark Schankerman, Professor Martin Pesendorfer, Dr Judith Shapiro, John Shannon, William Matcham and Brooklyn Han for insightful discussions in the preliminary stages of this enjoyably challenging project. Lastly, I thank my family for their love and support. Their personal experiences exemplify the importance of this research question. Feedback is appreciated. All errors are my own.

# Contents

<b>1</b>	<b>Introduction</b>	<b>1</b>
<b>2</b>	<b>Historical Context</b>	<b>2</b>
<b>3</b>	<b>Data</b>	<b>3</b>
3.1	Panel of Firms . . . . .	3
3.1.1	Ownership . . . . .	4
3.2	Panel of Cities . . . . .	5
3.2.1	Data Imputation . . . . .	5
<b>4</b>	<b>Empirical Strategy</b>	<b>6</b>
4.1	Specification . . . . .	6
4.2	Mechanisms . . . . .	8
4.3	Shift-Share Instrument . . . . .	10
4.4	Instrument Validity . . . . .	11
4.4.1	Industry-Level Orthogonality Condition . . . . .	11
4.4.2	Testing for Instrument Exogeneity . . . . .	12
4.4.3	Effective Sample Size . . . . .	13
4.4.4	Mutual Uncorrelatedness . . . . .	14
4.4.5	Alternative Identification Strategy: Share Exogeneity . . . . .	14
4.5	Measuring Firm Productivity . . . . .	15
4.5.1	Theory . . . . .	15
4.5.2	Application . . . . .	16
4.5.3	Measurement Error from Unobserved Prices . . . . .	17
4.5.4	Results on Input Elasticities . . . . .	17
4.6	Measuring Factor Misallocation . . . . .	18
<b>5</b>	<b>Results</b>	<b>19</b>
5.1	First Stage . . . . .	19
5.2	Profitability, Productivity and Private Firm Entry . . . . .	19
5.3	Labour Misallocation, Productivity and Compensation . . . . .	20
5.4	Capital Misallocation, Productivity and Investment . . . . .	20
<b>6</b>	<b>Robustness and Extensions</b>	<b>21</b>
<b>7</b>	<b>Limitations</b>	<b>22</b>
<b>8</b>	<b>Conclusion</b>	<b>23</b>
<b>9</b>	<b>Tables</b>	<b>24</b>
<b>10</b>	<b>Figures</b>	<b>33</b>

<b>A</b>	<b>Additional Tables</b>	<b>40</b>
<b>B</b>	<b>Additional Details on Empirical Strategy</b>	<b>47</b>
B.1	Translating the Shift-Share Instrument Orthogonality Condition into a Shock Orthogonality Condition . . . . .	47
B.2	Approaches to Estimating Firm Productivity . . . . .	48
B.2.1	Olley-Pakes (OP) Method . . . . .	48
B.2.2	Levinsohn-Petrin (LP) Method . . . . .	50
B.2.3	Akerberg-Caves-Frazer (ACF) Correction . . . . .	51
<b>C</b>	<b>Additional Details on Data and Definitions</b>	<b>53</b>
C.1	Index of Variables . . . . .	53
C.2	Nearest Neighbour Imputation: An Explanation . . . . .	53

# 1 Introduction

Whether China’s spectacular economic growth is a result of four decades of market-oriented reform is subject to great debate. The mass privatisation of state-owned enterprises (SOEs) is of particular interest due to its unprecedented scale: State industrial output declined from 41% in 1999 to 8% in 2012<sup>1</sup>.

A large economic literature relates within-firm changes in ownership to firm productivity and profitability. Most agree that SOEs still perform worse than always-private firms despite massive reforms, although privatised SOEs are closing the gap<sup>2</sup>. However, direct comparisons between firms of different ownership types, while informative of important trends, are ultimately descriptive. To interpret the productivity difference as being *caused* by state ownership ignores potential spillovers to other firms in the local economy.

General equilibrium effects are prevalent. The overall degree of privatisation in a region forms part of the market environment. This influences all firms within the local economy. For example, upon the announcement of the 2012 national anti-corruption campaign, stock market shares of private firms rose in more privatised regions but fell in less privatised regions (Lin et al., 2016). This is because local officials use SOEs to control factor inputs; Local SOE concentration determine whether private firms need to bribe local officials for resources.

I assess whether SOEs and private firms benefitted from regional privatisation even without direct changes to their internal ownership. In 1998–2007, a national policy privatised some industries but not others. This affected cities to varying degrees depending on their industry specialisations. I exploit this industry-level “shock” to construct a shift-share instrument for endogenous city privatisation. Using the 1998–2007 Chinese Annual Industrial Survey, I find that a one percentage point increase in a city’s share of private sector employment is associated with a 1.87% increase in the city’s average firm total factor productivity. This implies that going from 0% to 100% privatised would cause the average TFP level to increase by 536%. OLS returns lower estimates, suggesting that existing studies that do not account for potential endogeneity may understate productivity benefits. A heterogeneity analysis suggests that SOEs and private firms benefitted equally.

What explains privatisation’s effect on productivity? I do not attempt an exhaustive search of all possible mechanisms. Instead, I explore privatisation’s effect on three key determinants of productivity in the Chinese context: private sector growth, labour reallocation, capital reallocation. Results indicate a positive and significant effect of privatisation on private sector growth. I do not find evidence that privatisation reduced within-city factor misallocation. Furthermore, privatisation appears to have *increased* the SOE wage gap documented

---

<sup>1</sup>Source: CEIC Data.

<sup>2</sup>See Estrin et al. (2009) for a literature review. More recent studies include Brandt et al. (2012), Hsieh and Song (2015), Harrison et al. (2019) and Jurzyk and Ruane (2021).

by [Gustafsson and Wan \(2020\)](#). However, my results do find that privatisation statistically significantly improves labour and capital productivity. Thus, presence of the state sector (and the lack thereof) may not explain as much of regional factor misallocation as previous studies may indicate.

My study contributes to the literature assessing the effectiveness of Chinese reform of SOEs ([Song et al. \(2011\)](#), [Hsieh and Song \(2015\)](#), and references therein). It also contributes to the literature assessing how much of China’s factor misallocations can be explained by state influence ([Curtis, 2016](#); [Wu, 2018](#)).

Section 2 provides a background of China’s privatisation programme. Section 3 describes data sources and discusses sampling and imputation issues. Section 4 outlines the empirical strategy and steps taken to measure firm productivity and within-city factor misallocation. Section 5 presents results and mechanisms analysis. Section 6 presents robustness checks and extensions. Section 7 discusses limitations. Section 8 concludes.

## 2 Historical Context

China was a centrally planned economy when the Maoist regime ended in 1976. The state produced three quarters of the industrial output and employed more than 80% of the urban population ([Zhu, 2012](#)). In 1978, led by Deng Xiaoping, China began to transition into a market-oriented economy. A problem unique to China among transition economies is that privatisation was, and *is*, ideologically controversial ([Xu, 2011](#)). To minimise political dissent, the government pursued what [Lau et al. \(2000\)](#) call “reform without losers”: Certain industries were opened to private firm entry, but little was done to reform SOEs. Small-scale reforms were attempted under the radar ([Garnaut and Huang, 2001](#)), but the dual-track strategy was maintained well into the 1990s. State-controlled banks bailing out loss-making SOEs, which had reached a staggering 40% by the mid-1990s ([Brandt and Zhu, 2001](#)).

September 1997 was a milestone for SOE reform. In the 15th Communist Party Congress, the government legalised the existence of private firms and initiated ownership reform. An extraordinary wave of privatisation ensued. Figure 1 shows that between 1998 and 2007, the share of private industrial firms increased from 62% to 94%, and the revenue share increased from 48% to 70%.

This period of SOE reform can be summarised using the CCP’s 1999 slogan: *Zhua Da Fang Xiao* (“Grasp the Large, Release the Small”). The plan was for the state to consolidate control over larger SOEs, while allowing smaller SOEs to be privatised. While the slogan suggests that SOEs were selected to privatise solely by their size, [Hsieh and Song \(2015\)](#) note that SOEs that ultimately remained under state control are often located in sectors

of national importance. I complement their findings using data from the Annual Industrial Survey (data described in Section 3): Figure 3 plots the degree of privatisation of each industry, defined as non-state employment share, between 1998 and 2007. All of the five least privatised industries, which include oil, mining and utilities, are of strategic importance<sup>3</sup>. Meanwhile, the five most privatised industries tend to manufacture consumer goods.

Substantial scope for reform remains: SOEs still control 40% of industrial assets in 2019 (Cerdeiro and Ruane, 2022). I attempt to empirically assess if continuing the reform is truly beneficial to firms.

## 3 Data

I use a panel of firm financial statistics to estimate performance outcomes, then take averages at the city level, weighted by value added. I also aggregate firm data to city and industry levels to calculate the treatment and instrument.

I also use a panel of city characteristics to check for instrument validity and as controls in regressions.

### 3.1 Panel of Firms

Firm-level information are obtained from the Annual Industrial Survey (AIS). Run by the Chinese National Bureau of Statistics, it tracks industrial firms in mainland China from 1998 to 2012. I discard observations from 2008 and beyond due to missing data and sampling criteria inconsistencies.

The AIS includes all SOEs and “above-scale” private enterprises, defined as annual sales above 5 million RMB (625,000 USD)<sup>4</sup>. While it only covers 20% of all industrial firms, a comparison with the 2004 Industrial Census shows it accounted for 71.2% of the industrial workforce, 90.1% of output and 97.5% of exports (Brandt et al., 2012). Thus, despite concerns over data quality, the AIS remains one of the most comprehensive and oft-used surveys of Chinese industrial firms<sup>5</sup>.

Table 1 summarises key variables used in this study. Table 2 provides a breakdown by year.

---

<sup>3</sup>Tobacco is arguably an exception. The state strictly controls its distribution as it is regarded as an addictive substance.

<sup>4</sup>I use an exchange rate of 1 USD = 8 RMB throughout since I deflate monetary values to 1998 levels. In recent years, it is closer to 1 USD = 6 RMB.

<sup>5</sup>Recent research using the AIS include Du et al. (2014), Aghion et al. (2015), Hsieh and Song (2015), Harrison et al. (2019) and Brandt et al. (2020).

Consistent with records of Chinese macroeconomic growth, the number of firms, average output, revenue, return on assets and compensation increased over time. Average employment and firm age fell, likely due to increasing private firm entry during the sample period (Section 5.2. See also [Cerdeiro and Ruane \(2022\)](#)).

Each firm is assigned a 4-digit industry under the China’s Industrial Classification (CIC) system<sup>6</sup>. Each firm belongs to a prefecture-level division, or “city”<sup>7</sup>, which in turn belongs to one of 31 provinces.

Using the data, I illustrate differential privatisation of cities over the sample period in Figure 2. The number of cities observed remained stable, from 287 in 1998 to 292 in 2007. However, information from northwestern China, especially Xinjiang and Tibet, remained limited throughout. I cannot observe if a firm operates beyond the city it is headquartered in, though it is presumably not the case for >95% of firms surveyed which are “single-plant” firms ([Brandt et al., 2012](#)). Only 0.15% of firms surveyed relocated to another city within the sample period.

I eliminate firms with missing, negative or zero fixed assets, industrial output, intermediate input, liabilities or added value, less than 8 employees, with a pre-1900 establishment date, or missing industrial classification. I deflate nominal variables, such as output, input and fixed assets to 1998 levels to ensure comparability across time and industries. Details of deflation matter when estimating productivity. They are discussed in Section 4.5.3.

### 3.1.1 Ownership

Measuring city- and industry-level privatisation requires accurate information on firm ownership. Instead of using firms’ legal registration status, I follow [Hsieh and Song \(2015\)](#) and [Harrison et al. \(2019\)](#) and classify a firm as state-owned if the controlling shareholder is the state<sup>8</sup>. All non-state-owned firms are coded as private.

Despite my best efforts to pinpoint ownership, the data cannot identify critical channels of state influence. Even if a firm is privatised, the state may retain *de facto* control rights over factors of production ([Shleifer and Vishny, 1994](#)) if the new management is deeply connected to CCP officials. This generates false variation in the treatment variable (calculated as the share of a city’s workers in private firms). If such a firm experiences worse-than-average TFP or ROA growth, estimates of the treatment effect may be negatively biased.

---

<sup>6</sup>The CIC system changed in 2003. I use [Brandt et al.’s \(2012\)](#)’s crosswalk to harmonise classifications.

<sup>7</sup>I manually match cities across datasets and time to ensure consistency. While the AIS provides official region codes, it changed over time and cannot be synced to other data I use.

<sup>8</sup>[Hsieh and Song \(2015\)](#) find that aggregate statistics calculated using this classification method closely matches those published in the Chinese Statistical Yearbooks over the sample period. They manually check a subsample of firms and found no misclassifications.

A related issue is sampling bias. As all SOEs are included but only “above-scale” private firms are sampled, the calculated share of a city’s workers in private firms likely underestimates the true value. As my analysis employs a first-differenced specification and control for year fixed effects, sampling bias will only be problematic if it varies across city-year. There is no evidence to suggest this is true.

## 3.2 Panel of Cities

I retrieve all available city characteristics from the CEIC Database. The raw data is an unbalanced panel of 200 city covariates.

The CEIC Database cite city-level Statistical Bureaus as their sources. Upon investigation, the main cause of data missingness appears to be that different cities began reporting local statistics in different years: Beijing has records dating back to 1949, whereas Wuwei only reports statistics from 2002 onwards. However, this does not perfectly explain data missingness. Furthermore, some variables, especially price indices, are missing for majority of the observations.

I drop variables for which  $>10\%$  of observations are missing. I assume that remaining variables are missing at random—missing due to certain factors (e.g. city and year), but not the actual value of the missing entry. I then impute missing observations using stochastic chained nearest neighbour imputation<sup>9</sup>, explained below.

The final product is a balanced panel of 114 city-level characteristics summarised in Appendix Table A1. For ease of reading, I categorise them into Accessibility, Economy, Education, Environment, Health, Infrastructure, Labour Force, Population, Public Finance, Trade and Tourism, Energy and Utilities.

Information on local politics are taken from other sources. I obtain statistics on city politicians from the China Political Elite Database (Jiang, 2018) and process them à la Han (2021) to obtain local politicians’ average years of schooling and the share of politicians identified as corrupt. They serve as imperfect proxies for politician experience and corruption. Politicians’ average career incentive is obtained from Wang et al.’s (2020) replication data.

### 3.2.1 Data Imputation

According to the motivational speaker Jim Rohn: “You’re the average of the five people you spend the most time with.”. This captures the intuition for nearest neighbour (NN)

---

<sup>9</sup>To be explicit, I implement this in Stata using the command `mi impute chained (pmm, knn(1))`.



imputation, though it does not describe the process entirely accurately. NN matching is a special case of predictive mean matching (which selects randomly from  $k$  nearest neighbours) where  $k = 1$ ; Appendix C.2 explains the statistical process in detail.

I employ NN matching as a compromise between time and accuracy. The city-level dataset contains not just running variables, but also percentages (some converted from binomially distributed variables), probabilities, counts and truncated variables. Functional forms relating different variables differ, and are not necessarily linear. There is no one-size-fits-all parametric imputation model. Specifying a model per variable exceeds the timeframe for this study, and the models chosen may well be wrong. NN matching, as a semi-parametric method, stands out for its overall compatibility across a broad range of variable types and underlying relationships. Morris et al. (2014) show through simulations that data imputed through predictive mean matching may enable more robust inference than those imputed using misspecified parametric models. Furthermore, NN matching imputes using actual, existing values. This constrains imputed values to a reasonable range.

The data is missing non-monotonically: variables *cannot* be ordered such that if a variable is missing, all following variables are missing. Thus, I impute through chained equations. This method is best explained by White et al. (2011).

I note a major limitation. The standard practice is *multiple* imputation—repeating the above process to generate multiple sets of imputed data. Subsequent estimations are performed on all sets and results aggregated to fully account for the uncertainty. For tractability, I only run the process once. Furthermore, I treat the imputed value as given, rather than the estimates that they are. This ignores the underlying uncertainty imputation introduces. Consequently, estimates computed using the imputed data tend to be overprecise. Nevertheless, the estimates are consistent when the data is missing at random, subject to regularity conditions (Jamshidian and Mata, 2007).

## 4 Empirical Strategy

### 4.1 Specification

**Baseline.** I aim to estimate  $\beta$  in the *city-level* structural equation

$$y_{ct} = \alpha_c + \lambda_t + \beta Priv_{ct} + \mathbb{X}'_{ct}\gamma + u_{ct} \quad (1)$$

$\beta$  captures the effect of privatising city  $c$  on the average performance of its firms in year  $t$ . Also included are city fixed effects  $\alpha_c$  and year fixed effects  $\lambda_t$ .  $\mathbb{X}_{ct}$  contains observable time-varying city-level determinants of firm performance;  $u_{ct}$  captures unobservable ones.

**Endogeneity.** Ideally, if Chinese government randomly assigned cities to privatise,  $\hat{\beta}_{OLS}$  will be consistent. But even if the central government sets such a policy, city leaders’ compliance may vary due to what Xu (2011) calls the *regionally decentralised* structure of economic policymaking. City leaders have considerable freedom in implementing economic policies, even when the directive may come from above. 50% of public expenditure took place at sub-provincial levels from 1994 to 2000, a figure that ranks high internationally (Wong and Bhattasali, 2003). Privatisation is the result of city governments maximising the payoff from a complex set of economic, fiscal, and political goals. This makes it highly probable that certain unobserved, time-varying determinants of local firm performance also affect local privatisation. Possible examples are tax revenues (Mickiewicz, 2010), social stability (Wen, 2020), and politicians’ career incentives (Huang et al., 2020).

Reverse causality is also a concern. Unlike in other transition economies, privatisation is ideologically controversial in China. Local officials may therefore wait until earlier reforms improved SOE performance before privatising them—once initiated, success is imperative to minimising public discontent (Du and Liu, 2015).

**First Differences.** I take first differences of Equation 1 to eliminate time-invariant unobserved heterogeneity across cities.

$$\Delta y_{ct} = \lambda_t + \beta \Delta Priv_{ct} + \Delta \mathbb{X}'_{ct} \gamma + \Delta u_{ct} \quad (2)$$

I prefer first-differencing to controlling for city fixed effects due to possible serial correlation in  $u_{ct}$ <sup>10</sup>. However, first differencing will induce negative serial correlation in  $\Delta u_{ct}$  if  $u_{ct}$  is, in fact, serially uncorrelated. I account for this possibility by later clustering robust standard errors at the city level.

Year fixed effects eliminate time trends that affect cities equally. This typically encompasses institutional changes, such as the 2004 constitutional amendment strengthening private property protection<sup>11</sup>.

**Shift-Share Instrument.**  $\hat{\beta}_{FD}$  will be consistent if the model in Equation 1 is correct and  $E(\Delta Priv_{ct} \Delta u_{ct} | \Delta \mathbb{X}_{ct})$ <sup>12</sup>. However, first differencing may not remove confounding from time-varying city unobservables. I thus construct a shift-share instrument  $\widehat{\Delta Priv_{ct}}$  for  $\Delta Priv_{ct}$ .

---

<sup>10</sup>For example, Huang et al. (2020) and Han (2021) show that even the city leaders’ age can affect their willingness to reform due to changing career incentives. If cross-city reshuffling of politicians is uncorrelated with their age, age will be a random walk that can be first-differenced out. Nonetheless, Section 4.4 checks for instrument exogeneity against local politicians’ career incentive.

<sup>11</sup>Enforcement may still vary across cities. That would be problematic.

<sup>12</sup>This assumption does not guarantee unbiasedness. For unbiasedness, we also need to assume strict exogeneity  $E(u_{ct} | Priv_{c1}, \dots, Priv_{cT}, \mathbb{X}_{ct}) = 0$ . I omit the rank condition from my discussions for brevity.

The final model is

$$\text{First Stage:} \quad \Delta Priv_{ct} = \lambda_t + \delta \widehat{\Delta Priv_{ct}} + \Delta \mathbb{X}'_{ct} \gamma + \Delta v_{ct} \quad (3)$$

$$\text{Second Stage:} \quad \Delta y_{ct} = \lambda_t + \beta \Delta Priv_{ct} + \Delta \mathbb{X}'_{ct} \gamma + \Delta u_{ct} \quad (4)$$

**Treatment.** The endogenous treatment  $Priv_{ct}$  is calculated as the share of city  $c$ 's industry workers employed in private firms in year  $t$

$$Priv_{ct} = \frac{\text{No of Workers in Private Firms}_{ct}}{\text{No of Workers}_{ct}} \in [0, 1] \quad (5)$$

**Outcomes.**  $y_{ct}$  is the average performance of firms in city  $c$ . These measures are first calculated for each firm, then averaged to city level, weighted by firm output. Firm profitability is measured by return on assets (ROA), the ratio of total profits to total assets. Firm productivity is measured by total factor productivity (TFP) estimation (Section 4.5).

**Weights.** As the outcome variables are averages taken over firms in a city, I weight each city-year observation using the number of firms. Weights  $e_{ct}$  are such that  $\sum_{c,t} e_{ct} = 1$ .

## 4.2 Mechanisms

I explore three primary channels through which privatisation may improve firm productivity.

**Labour Misallocation.** The presence of SOEs distorts the optimal allocation of labour. Though earlier reforms relaxed constraints on hiring and job search (Meng, 2000), over 70% of jobs were still state-assigned by 2002 (He et al., 2018). Local officials may allocate labour in a way that favours SOEs over private firms (Brandt et al., 2013). Privatisation may reduce local officials' influence over job assignments, thereby allowing labour to flow towards more productive firms (Bai and Cheng, 2016)

Regional privatisation may also increase local job market density. Massive layoffs from SOEs—around 7 million workers every year between 1997 to 2002 (Cai et al., 2008)—increased labour supply. Privatisation also indirectly boosted labour demand through encouraging private firm entry (Section 5.2). The resultant agglomeration may improve job matching (Combes and Gobillon, 2015) and hence reduce labour misallocation.

Under this theory, we should see improvements in labour productivity and more competitive wages. In Section 5.3, I estimate the effect of regional privatisation on labour produc-

tivity (value added per worker) and per worker compensation (annual wage and welfare payments)<sup>13</sup>. I also attempt to directly quantify within-city labour misallocation using the Olley and Pakes (1996) covariance approach (Section 4.6).

**Capital Misallocation.** The presence of SOEs distort the credit market and introduce financial frictions. The state controls the largest four banks. Consequently, SOEs often gain preferential access to credit at the expense of private, possibly more productive firms (Lin et al., 2020). In a seminal paper, Hsieh and Klenow (2009) show that within-sector dispersions in the marginal products of capital is a sufficient statistic for aggregated sector TFP, assuming that marginal products and productivity are joint log-normally distributed<sup>14</sup>.

In Section 5.4, I estimate the effect of regional privatisation on capital productivity (value added per unit capital), real investments and within-city capital misallocation.

**Private Sector Expansion.** New private firms may bring new technologies and innovation, facilitating productivity growth through Schumpeter’s process of creative destruction (Aghion and Howitt, 1992). It may also incentivise incumbents to improve own product varieties (Garcia-Macia et al., 2019). Cerdeiro and Ruane (2022) connect private firm entry to TFP improvements in the Chinese firms setting. The natural next step is to determine whether privatisation facilitated this process.

I measure private firm entry by the ratio of industrial output by young private firms to that of older firms. A firm is classified as young if it is three years or younger.

**Treatment Effect Heterogeneity.** I check if the effect of privatisation on various outcomes differ depending on firm ownership type. I average each firm-level outcome over only SOEs and only over private firms. I then stack these outcomes, and interact all RHS variables with a categorical variable  $D_{ct}$  indicating which subsample of firms I averaged over. This allows the coefficients on all variables, including controls and fixed effects, to vary by subsample. I then test the null hypothesis that the coefficient on  $D_{ct} \times \Delta Priv_{ct}$  is equal to zero, clustering robust standard errors at the city level.

---

<sup>13</sup>I deflate compensation using industry output (i.e. producer) prices rather than consumer prices. This is to accurately reflect costs of production (Stansbury and Summers, 2018).

<sup>14</sup>Recent studies have questioned the realism of this assumption. Lanteri and Medina (2017) show that violations can significantly affect results.

### 4.3 Shift-Share Instrument

**Conceptual Framework.** To give intuition to the shift-share instrument, it is useful to deconstruct city privatisation into a weighted average of that of each industry operating within the city

$$\Delta Priv_{ct} = \sum_n s_{cnt} \times \Delta Priv_{cnt} \quad (6)$$

Industry shares  $s_{cnt}$  measure the importance of industry  $n$  to city  $c$  and captures the city’s *exposure* to changes in industry  $n$ . Assume for now that  $\sum_n s_{cnt} = 1$ .  $\Delta Priv_{cnt}$  is city-industry specific growth in privatisation. Decompose it into an nationwide industry average and an idiosyncratic city-industry component

$$\Delta Priv_{cnt} = \Delta Priv_{nt} + \widetilde{\Delta Priv_{cnt}} \quad (7)$$

where

$$Priv_{nt} = \frac{\text{No of Workers in Private Firms}_{nt}}{\text{No of Workers}_{nt}} \in [0, 1] \quad (8)$$

In 1998–2007, the *Zhua Da Fang Xiao* policy privatised certain industries while restricting private firm entry in certain “strategic” industries. This generated industry-level variation that is plausibly exogenous to city-level unobservables, represented by  $\Delta Priv_{nt}$ .  $\widetilde{\Delta Priv_{cnt}}$  represents treatment variation due to local industry-specific factors.

I provide a simple example motivated by Xu’s (2011) Chinese political economy framework. Suppose that the central government sets an overall privatisation goal of  $\Delta Priv_{nt}$  for industry  $n$ , motivated by strategic concerns. City governments have considerable autonomy over its implementation and need not fully comply. Competition among city administrators and political patronage may drive selection on which local industries to privatise and when (Wang and Yang, 2021). Politicians may also face conflicting goals: Wen (2020) finds that state employment is used to quell local social unrest. This causes the actual privatisation levels of industry  $n$ ,  $\Delta Priv_{cnt}$ , to vary by city in a way that is correlated with city unobservables.  $\widetilde{\Delta Priv_{cnt}} = \Delta Priv_{cnt} - \Delta Priv_{nt}$  captures the endogenous variation.

However, political power remains centralised. While each city may deviate from target, the central government is ultimately able to facilitate a collective agreement between city leaders through personnel appointments and promotions incentives. Collective decision-making internalises between-city externalities (Xu, 2011), ultimately achieving the national target  $\Delta Priv_{nt}$ .

The shift share instrument  $\widehat{\Delta Priv_{ct}}$  therefore relies on the national policy to generate plausibly exogenous industry-level *shocks*  $\Delta Priv_{nt}$ , and distributes these shocks to cities through

industry shares  $s_{cnt}$ .

$$\widehat{\Delta Priv}_{ct} = \sum_n s_{cnt} \times \Delta Priv_{nt} \quad (9)$$

$\widehat{\Delta Priv}_{ct}$  essentially predicts  $\Delta Priv_{ct}$  using each city's industrial specialisation and national trends in each industry's privatisation<sup>15</sup>. It represents the hypothetical privatisation level of city  $c$  if all its industries had experienced the same degree of privatisation as the overall economy ( $\Delta Priv_{ct} = \widehat{\Delta Priv}_{ct}$  if  $\Delta Priv_{cnt} = \Delta Priv_{nt} \forall n$ ).

The effect of privatisation on firm performance may differ by city. [Borusyak et al. \(2022\)](#) show that 2SLS identifies a convex weighted average of heterogeneous treatment effects under a first-stage monotonicity and large sample assumption.

**Construction.** I now take the model to data and construct the instrument using Equation 9.  $\Delta Priv_{nt}$  is the first-differenced proportion of industry  $n$ 's workers in private firms.  $s_{cnt}$  is calculated as the proportion of city  $c$ 's workers employed in industry  $n$ . If all industries within a city are observed,  $\sum_n s_{cnt} = 1$ . However, the AIS data covers industrial firms only. I therefore construct shares such that  $\sum_n s_{cnt} = S_{ct} \leq 1$ , where  $S_{ct}$  is the share of city  $c$ 's total workforce in the industrial sector obtained from CEIC data. Since the instrument implicitly uses the variation in  $S_{ct}$  over time, I follow recommendations by [Borusyak et al. \(2022\)](#) and control for  $S_{ct}$  interacted with year fixed effects in all regressions.

Following common practice (e.g. [Boustan, 2010](#); [Autor et al., 2013](#); [Derenoncourt, 2022](#)), I fix industry shares  $s_{cnt}$  to their first-observed values to alleviate reverse causality concerns. However, I do not rely on the exogeneity of shares for identification (discussed in Section 4.4.5).

## 4.4 Instrument Validity

### 4.4.1 Industry-Level Orthogonality Condition

The exogeneity of shocks  $\Delta Priv_{nt}$  is sufficient for the exogeneity of the instrument  $\widehat{\Delta Priv}_{ct}$  ([Borusyak et al., 2022](#)). The standard IV assumption is orthogonality between the instrument and second-stage residual in Equation 4, weighted by regression weights  $e_{ct}$  ( $\sum_{c,t} e_{ct} = 1$ ). In Appendix B.1, I prove that this is equivalent to orthogonality between industry-level shocks

---

<sup>15</sup>Indeed, Section 5.1 reports an almost one-to-one first stage relationship between  $\widehat{\Delta Priv}_{ct}$  and  $\Delta Priv_{ct}$ .

$\Delta Priv_{nt}$  and a weighted average of city characteristics:

$$E \left[ \sum_{c,t} e_{ct} \widehat{\Delta Priv_{ct}} \Delta u_{ct} \middle| \sum_{c,t} e_{ct} \Delta \mathbb{X}_{ct} \right] = E \left[ \sum_{n,t} s_{nt} \Delta Priv_{nt} \overline{\Delta u_{nt}} \middle| \sum_{n,t} s_{nt} \overline{\Delta \mathbb{X}_{nt}} \right] = 0 \quad (10)$$

where  $s_{nt} := \sum_c e_{ct} s_{cnt}$ ,  $\overline{\Delta x_{nt}} := \frac{\sum_c e_{ct} s_{cnt} \Delta x_{ct}}{\sum_c e_{ct} s_{cnt}}$ .

$s_{nt}$ , the *industry-level average exposure*, are essentially weights in the industry-level regression since  $\sum_{n,t} s_{nt} = 1$ . This means that, together with the number of industries,  $s_{nt}$  plays an important role in determining the effective sample size of the regression (Section 4.4.3).

$\overline{\Delta u_{nt}}$  is an *exposure-weighted average of city characteristics*  $\Delta u_{ct}$  that determine firm performance. For any given industry  $n$ , the characteristic of a city  $\Delta u_{ct}$  will contribute more to  $\overline{\Delta u_{nt}}$  if city  $c$  is industry  $n$ 's primary market (i.e. high  $s_{cnt}$ ). As an extreme example, if the mining industry operates exclusively in China's "Nickel Capital" Jinchang, then  $\overline{\Delta u_{Mining,t}} = \Delta u_{Jinchang,t}$  exactly.

Identification therefore requires that the shocks be uncorrelated with average city-level characteristics that determine firm performance in each industry's primary markets. Industries that are about to be privatised must not sort themselves into cities that are about to experience a shock to average firm performance.

#### 4.4.2 Testing for Instrument Exogeneity

Technically speaking, instrument exogeneity cannot be formally tested. However, one can provide informal support for (or evidence against) it by checking if cities with different predicted privatisation is balanced against observable city-level determinants of firm performance. However, Equation 10 demonstrates that only the shock component of the instrument needs to be exogenous. I therefore transform each city-level characteristic  $\Delta q_{ct}$  into its exposure-weighted industry equivalent  $\overline{\Delta q_{nt}} = \frac{\sum_c e_{ct} s_{cnt} \Delta q_{ct}}{\sum_c e_{ct} s_{cnt}}$  using Borusyak et al.'s (2022) Stata module `ssaggregate`. I then run a bivariate regression of industry privatisation shocks on the transformed characteristic

$$\Delta Priv_{nt} = \lambda_t + \pi \overline{\Delta q_{nt}} + \epsilon_{nt} \quad (11)$$

using weights  $s_{nt}$  and controlling for year fixed effects.

I pay particular attention to checking for balance against local politics, government expenditure and trade, as they are more likely to be correlated with privatisation. For full transparency, I use an extensive panel of 114 indicators on a city's accessibility, economy, education, environment, infrastructure and population. While not all characteristics used may determine firm performance, a lack of balance nonetheless threatens the random assignment

assumption of  $\Delta Priv_{nt}$  and suggests that other unobservables may also be unbalanced (Pei et al., 2019).

Column 1 of Table 3 reports results. Only 13 out of 114 covariates are correlated with the shocks. In Column 2, I instead regress the endogenous treatment variable  $\Delta Priv_{ct}$  on each city covariate. As expected, many characteristics show up as unbalanced. The contrast between Columns 1 and 2 lends very informal support for shock exogeneity: Shocks are uncorrelated with most city characteristics whereas the endogenous treatment is.

For ease of reading, Table 4 collects the characteristics that are likely correlated with shocks ( $p < 0.1$ ). At first glance, it appears that the privatisation of an industry is correlated with the level of education, accessibility, environment, healthcare quality, labour force size and economic growth in its primary markets. However, I treat their individual statistical significance with caution as running 114 bivariate regressions results in overrejection of the null hypothesis (that the shock is uncorrelated with the covariate)<sup>16</sup>.

Thus, I run a multivariate regression of the shock on all city covariates at once test their joint significance in explaining the shock. The answer appears to be a disappointing yes ( $p < 0.01$ ), though it may be due to the the imputation method used for city-level covariates (Section 3.2.1) biasing standard errors downwards. I include all unbalanced characteristics as baseline controls in regressions.

It is at least reassuring that shocks are not predicted by local politics, government expenditure or trade, even though the treatment is. This lends support for the claim that industry-level privatisation is largely guided by the central government through a nation-wide strategic policy, rather than pushed by local politicians or multinational companies in the industries' primary markets.

#### 4.4.3 Effective Sample Size

Since the IV exogeneity condition has been reinstated in terms of industry-level shocks, it follows that, to check if the law of large numbers holds, one should examine the sample size and instrument variation at the industry, rather than city, level.

Table 5 therefore reports industry-level summary statistics on industry-level average exposure  $s_{nt} = \sum_c e_{ct} s_{cnt}$  and shocks  $\Delta Priv_{nt}$ . Reassuringly, the largest  $s_{nt}$  is only 0.006<sup>17</sup>. The effective sample size (inverse of Herfindahl–Hirschman Index of average shares,  $1/\sum_{n,t} s_{nt}^2$ )

---

<sup>16</sup>Alternatively, I can retrieve unbalanced characteristics from the multivariate regression used in the next paragraph (see for e.g. Dray, 2022) However, such an approach will likely suffer from a whole host of issues, including near multicollinearity.

<sup>17</sup>As  $s_{nt}$  function like regression weights such that  $\sum_{n,t} s_{nt} = 1$ , if the largest  $s_{nt} = 1$ , then regressions effectly use only one observation.



is also large, at 1,097.

Panels B and C provide support for instrument relevance. Shocks are numerous (7,604 industry-years) and have decent variation (mean = 0.03, s.d. = 0.09). This echoes Figure 3 in showing that industries privatised at vastly different rates during the sample period. Section 5.1 reports formal first stage results.

#### 4.4.4 Mutual Uncorrelatedness

Another crucial condition for identification is no spillovers between industries. This can happen if, for example, the privatisation of one industry causes laid off workers to enter a different 4-digit industry within the same 2-digit or 3-digit industry group. They may not immediately adjust to using new equipment, thereby biasing productivity downwards. If true, shocks should be recalculated at higher (2- or 3-digit industry) levels. Hence, I test for the presence of shock correlation at the CIC 2-digit, 3-digit and 4-digit (unit) industry level.

I estimate intraclass correlation coefficients (ICCs) with an unweighted mixed effects model:

$$\Delta Priv_{nt} = \mu_t + a_{CIC2(n),t} + b_{CIC3(n),t} + c_n + e_{nt}$$

In the style of [Borusyak et al. \(2022\)](#), I use time-varying CIC2 and CIC3 random effects and CIC4 and year fixed effects. I implement this through the Stata command `mixed`. Table 6 reports ICC estimates. The ICCs on 2-digit and 3-digit industry groups are not significantly different from zero at conventional levels of statistical significance; while Stata failed to estimate robust standard errors on the 4-digit industry level ICC<sup>18</sup>, the point estimate is indistinguishable from zero.

Thus, I use the shocks as they are computed at the 4-digit industry level.

#### 4.4.5 Alternative Identification Strategy: Share Exogeneity

[Goldsmith-Pinkham et al. \(2020\)](#) show that the exogeneity of shares  $s_{cnt}$  is also sufficient for identification. This is more common and intuitive in the SSIV literature; Indeed, this is the argument used by [Brandt et al. \(2020\)](#), the only existing study I am aware of that uses a SSIV in a setting similar to mine. They instrument for regional state presence using a similar formulation, and fix shares  $s_{cnt}$  to first-observed (1998) values, arguing that these shares are exogenous to local characteristics.

I disagree with [Brandt et al.](#)'s view on share exogeneity. Cities industry specialisations are a

---

<sup>18</sup>The true cause regrettably eludes me. It has been suggested that this could result from an almost perfect fit—in other words, a *very* precisely estimated zero.

complex function of local economic, social, political and environmental conditions. China's shift from an industrial development strategy into one that is driven by regional comparative advantages (Yue and Hua, 2002) is one particular threat to share exogeneity. As regional comparative advantages evolve slowly, they constitute a time-varying, serially correlated unobservable that fixing shares to 1998 values may not fully eliminate.

Ultimately, I find that the *Zhuhua Da Fang Xiao* policy makes shock exogeneity more plausible. Unique to my study is the attempt to address potential violations to shock exogeneity arising from the characteristics of industries' primary markets.

## 4.5 Measuring Firm Productivity

### 4.5.1 Theory

I construct a measure of firm-level total factor productivity (TFP) and run the estimation procedure separately for each 3-digit industry subset.

I assume that the value-added production function of firm  $i$  in year  $t$  is Cobb-Douglas<sup>19</sup> and take logs:

$$Y_{it} = K_{it}^{\beta_k} L_{it}^{\beta_l} X_{it} \quad (12)$$

$$\Rightarrow y_{it} = \beta_k k_{it} + \beta_l l_{it} + \omega_{it} + \varepsilon_{it} \quad (13)$$

$y_{it}$  is the log of value added (outputs minus intermediate inputs),  $k_{it}$  is the log of capital inputs and  $l_{it}$  log of labour inputs. Total factor productivity is the variation in value added that factor inputs cannot explain: Solow residuals  $\omega_{it}$  and  $\varepsilon_{it}$ , where  $\omega_{it} + \varepsilon_{it} = \log X_{it}$ . Both represent firm-specific productivity shocks that are not observed by the econometrician. They differ in that firm  $i$  observes  $\omega_{it}$ , but not  $\varepsilon_{it}$ , before it chooses inputs  $k_{it}$  and  $l_{it}$ . Common examples of  $\omega_{it}$  include managerial talent and expected rainfall.  $\varepsilon_{it}$  typically contain events unforeseen by firm  $i$  (e.g. unexpectedly high rainfall) only observed after choosing inputs  $k_{it}$  and  $l_{it}$ . It may also contain measurement error in  $y_{it}$ .

Since the firm observes  $\omega_{it}$ , its input choice is likely a function of  $\omega_{it}$ . This biases OLS estimates of input elasticities  $\beta_k$  and  $\beta_l$  in *ex ante* unclear directions. For example, OLS may overstate the elasticity of labour inputs while understating that of capital if labour is easier to adjust than capital in the short run, or if automation technology breakthroughs cause firms to replace employees with machinery (Acemoglu and Restrepo, 2020).

---

<sup>19</sup>While convenient, this imposes restrictive assumptions. A more sophisticated approach can nest the Cobb-Douglas case in a more general form, e.g. Constant Elasticity of Substitution. See Caselli (2005) for a discussion.

Olley and Pakes (1996) (OP) propose using investment as a proxy for productivity. Key to their method is the assumption that firms' investment demand is given by

$$i_{it} = f_t(k_{it}, \omega_{it}) \quad (14)$$

where  $f_t$  is strictly increasing in  $\omega_{it}$ . Strict monotonicity allows for  $\omega_{it}$  to be “inverted out”

$$\omega_{it} = f_t^{-1}(k_{it}, i_{it}) \quad (15)$$

and substituted into the production function to obtain an equation consisting of purely observables and white noise  $\varepsilon_{it}$ . With additional assumptions on the evolution process of  $\omega_{it}$ , one can obtain consistent estimates of  $\beta_k$  and  $\beta_l$  in a two-step procedure.

OP's method risks discarding a lot of valuable information, as many firm-level surveys show bunching at  $i_{it} = 0$ . Furthermore, firm-level unobserved heterogeneity in investment prices or capital adjustment costs may violate the assumed one-to-one relationship between  $i_{it}$  and productivity  $\omega_{it}$  (conditional on  $k_{it}$ ).

Levinsohn and Petrin (2003) (LP) therefore use intermediate inputs to proxy for productivity instead, essentially replacing  $i_{it}$  with log intermediate inputs  $m_{it}$  in Equation 14.

Both OP and LP assume that labour  $l_{it}$  is non-dynamic and chosen at time  $t$ . Akerberg et al. (2015) relax this assumption by adding  $l_{it}$  to the proxy variable function (Equation 14). This allows for  $\beta_l$  to be correctly identified and for firm-specific shocks to labour costs to enter the function through  $l_{it}$ .

Appendix B.2 discusses OP, LP and ACF assumptions and estimation processes in detail.

#### 4.5.2 Application

I estimate TFP using the LP method, applying the ACF correction. LP allows for firm-level unobserved heterogeneity in capital adjustment costs or investment prices while OP does not. Firms are also much more likely to report positive intermediate inputs than investment. Nonetheless, I also estimate TFP through the OP method as a robustness check. I use the Stata module `prodest` (Rovigatti and Mollisi, 2016) to obtain both sets of estimates.

I use industrial output minus materials costs for  $Y_{it}$  and materials costs for  $M_{it}$ , both reported in thousand RMB. I use the total number of employees, for  $L_{it}$ , thereby assuming away firm-level heterogeneity in worker quality and hours worked per worker. Using the popular approach of Brandt et al. (2012), I construct a measure of real capital stock  $K_{it}$  from the net value of fixed assets at original purchase prices, deflated by the Perkins and Rawski (2008) investment index. Real investment  $I_{it}$  is calculated using the perpetual inventory method

$I_{it} = K_{it+1} - (1 - \delta)K_{it}$ <sup>20</sup>, assuming a constant depreciation rate of  $\delta = 0.09$ .

### 4.5.3 Measurement Error from Unobserved Prices

Finally, I address an important concern on using monetary terms in TFP estimation: measurement error. Ideally, one should have data on physical output and input quantities, but I do not. As I deflate nominal values using 4-digit-industry-level input and output deflators, firm-level log TFP estimates will technically be contaminated by unobserved firm-specific deviations in markups (output price – input price) from the industry average (De Loecker and Warzynski, 2012). The second stage error term in Equation 4 therefore contains the city-level average of markup deviations. Due to the inability to purge firm-level price effects, the estimate I obtain is what is sometimes referred to as *revenue* productivity (TFPR). One may be concerned that the measurement error in the outcome may be correlated with privatisation—for example, if firms in more privatised cities export more and face higher or lower prices as a result.

However, I argue that the use of a shift-share instrument preserves the identification of  $\beta$ , the effect of regional privatisation on TFP. Since I deflate nominal values by industry, firm-level markup deviations average out to zero at the industry level. Therefore, it is uncorrelated with industry-level shocks to privatisation. This translates into the exogeneity of the instrument with the city-level average of markup deviations (Borusyak et al., 2022). In other words, while the levels of TFP estimates may be biased, the 2SLS estimates of  $\beta$  remain consistent.

### 4.5.4 Results on Input Elasticities

I separately estimate input elasticities  $\beta_k$  and  $\beta_l$  by 3-digit industry subsets since some industries may be more capital intensive (e.g. telecommunications versus textiles). Table 7 reports the average estimated value-added elasticities of capital and labour  $\hat{\beta}_k$  and  $\hat{\beta}_l$  by OLS, OP and LP methods.

The capital coefficient estimate is unambiguously lower when OLS is used. This is consistent with OP and LP’s original analyses, though they give different interpretations. OP argue through attrition: When productivity shocks causes  $y_{it}$  to fall, the observed  $k_{it}$  is likely to increase as firms with larger capital stocks escape insolvency. LP analyse the OLS estimator of  $\beta_k$  and show that a positive correlation between capital and labour generates a downward bias. Both are probable in the Chinese firms setting.

---

<sup>20</sup>This requires data on firm  $i$ ’s nominal capital value in its founding year,  $NK_{it_{i0}}$ . As I do not observe  $NK_{it_{i0}}$  for firms founded before 1998, I assume  $NK_{it_{i0}} = NK_{it_{i1}} / (1 + g)^{t_{i1} - t_{i0}}$ , where  $t_{i1}$  is the year in which firm  $i$  is first observed in the data.  $g$  is the the average province-industry growth rate of nominal capital between 1993 and  $t_{i1}$ , obtained from the 1993 Annual Enterprise Survey.

Interestingly, OP returns lower labour coefficient estimates  $\hat{\beta}_l$  while LP returns higher  $\hat{\beta}_l$  compared to OLS. Possible explanations exist for both upward and downward biases in  $\hat{\beta}_l$ , but it remains a daunting task (and somewhat tangential to this paper) to determine if OP and LP estimates are mutually consistent. Nonetheless, I check if my main results are robust to using OP estimates of TFP in Section 6.

## 4.6 Measuring Factor Misallocation

I quantify the extent of within-city misallocation of factor inputs using Olley and Pakes's (1996) established approach, also known as the *OP gap*. An influential study by Bartelsman et al. (2013) compares different misallocation measures and finds the OP gap to be both theoretically and empirically robust. Additionally, the OP gap is simple to compute and the assumptions are reasonable.

To derive the OP gap, I decompose city  $c$ 's labour or capital productivity  $\theta_{ct}$  into a weighted average of firm productivities  $\theta_{it}$

$$\theta_{ct} = \sum_{i \in G(c,t)} q_{it} \theta_{it}$$

where  $G(c, t)$  contains the firms in city  $c$  in year  $t$ .

Weights  $q_{it} = \frac{Y_{it}}{\sum_{i \in G(c,t)} Y_{it}}$  are calculated using value added. They indicate a firm's size.

Let  $\bar{\theta}_{ct}$  and  $\bar{q}_{ct}$  denote the *unweighted* average productivity and value added share. We can write firm  $i$ 's deviation from the unweighted average as  $q_{it} - \bar{q}_{ct}$  and  $\theta_{it} - \bar{\theta}_{ct}$ . Then city  $c$ 's productivity can be decomposed into the unweighted firm-level average and a covariance term between firm productivity and size

$$\begin{aligned} \theta_{ct} &= \sum_{i \in G(c,t)} (\bar{q}_{ct} + (q_{it} - \bar{q}_{ct}))(\bar{\theta}_{ct} + (\theta_{it} - \bar{\theta}_{ct})) \\ &= n(G(c, t)) \times \bar{q}_{ct} \bar{\theta}_{ct} + \sum_{i \in G(c,t)} (q_{it} - \bar{q}_{ct})(\theta_{it} - \bar{\theta}_{ct}) \\ &= \bar{\theta}_{ct} + \sum_{i \in G(c,t)} (q_{it} - \bar{q}_{ct})(\theta_{it} - \bar{\theta}_{ct}) \end{aligned}$$

where  $n(G(c, t))$  denotes the number of firms in city  $c$  and time  $t$ .

The OP gap is the covariance term  $\sum_{i \in G(c,t)} (q_{it} - \bar{q}_{ct})(\theta_{it} - \bar{\theta}_{ct})$ . An increase in the OP gap indicates that production is reallocated to more productive firms. Assuming that more productive firms produce more and use more inputs in an allocatively efficient economy, the OP gap is *inversely* related to the level of within-city misallocation.

## 5 Results

### 5.1 First Stage

Figure 4 shows a binned scatter plot of the relationship between the predicted share of non-SOE employment  $\widehat{\Delta Priv_{ct}}$  and actual increase  $\Delta Priv_{ct}$ . I include baseline controls, which includes unbalanced city characteristics from the shock-level balance test in Section 4.4 and the sum of industry shares  $S_{ct} = \sum_n s_{cnt}$  interacted with year fixed effects.

A 1 pp increase in the predicted share is associated with a 1.3 pp increase in the actual share. This positive and almost-one-to-one correlation suggests that SSIV is indeed a good, though imperfect, predictor of treatment. The first-stage F statistic ( $= t^2$ ) is 115, which is well above the [Stock and Staiger \(1997\)](#) rule-of-thumb of 10.

All in all, instrument relevance appears satisfied. However, the threshold of 10 has only been proven valid under homoscedasticity. I thus report p-values from the [Anderson and Rubin \(1949\)](#) (AR) test, which is robust to weak instruments, in all 2SLS regressions<sup>21</sup>.

### 5.2 Profitability, Productivity and Private Firm Entry

Table 8 reports 2SLS and OLS estimates of the relationship between city privatisation and average profitability, productivity and private firm entry.

The overall relationship between privatisation and performance is summarised in Columns 1 and 4 of Panel C. A 1 percentage point (pp) increase in the share of non-state employment is associated with a 0.086 pp increase in return on assets (mean = 7.1%, sd = 6.3pp). However, this effect is imprecisely estimated.

A 1pp increase in the share of non-state employment is also associated with a  $100\% \times (e^{1.85 \times 0.01} - 1) = 1.87\%$  increase in the level of TFP (mean = 3,151, sd = 4,320), statistically significant at the 5% level. The result is economically significant: If a city goes from 0% to 100% privatisation, firm TFP will increase by  $100\% \times (e^{1.85 \times 1} - 1) = 536\%$ .

Column 2 averages ROA over only private enterprises, and Column 3 for private enterprises. While the higher point estimates in Column 2 may suggest that private firms benefitted more from regional privatisation, formal statistical testing cannot reject the null hypothesis that the coefficients of interest are equivalent ( $p = 0.38$ ).

Comparing Columns 5 and 6, SOEs and private firms also do not appear to receive statisti-

---

<sup>21</sup>The null hypothesis of the AR test  $\beta = 0$  relates to the coefficient of interest  $\beta$ , not the first stage relationship  $\delta$ . The AR test statistic has a chi-squared distribution under the null.

cally significantly different TFP gains. In fact, the point estimates are very similar.

Column 7 presents evidence that privatisation encouraged private firm entry. A 1pp increase in the share of non-state employment is associated with a statistically significant 0.45pp increase in the ratio of industrial output by new private firms to that of old firms. (mean = 14%, sd = 14pp).

The results affirm that regional privatisation made firms more productive. Furthermore, private sector growth appears to be a key mechanism. A unique insight from this paper is that existing studies may understate the extent of success: Comparing the OLS and 2SLS estimates in Columns 4 and 7, OLS appears to systematically underestimate privatisation benefits. This suggests that time-varying city-level unobservables strongly bias the effect downwards.

### 5.3 Labour Misallocation, Productivity and Compensation

In Column 1 of Table 9, I find no significant effect of privatisation on labour misallocation. Column 2 shows that a 1pp increase in the share of non-state employment is associated with a  $100\% \times (e^{2.25 \times 0.01} - 1) = 2.3\%$  increase in labour productivity (value added per worker) (mean = 0.12, sd = 0.09).

Column 6 shows that privatisation is also associated with a 290 RMB (36 USD) increase in SOEs' annual per worker compensation (mean = 15,984 RMB, sd = 8,056 RMB). Furthermore, this estimated effect on SOE compensation is statistically significantly different from that of private firm compensation ( $p = 0.02$ ). The finding may indicate that more efficient SOEs survive in a privatised market: Past literature has documented that failing SOEs borrowed heavily from state banks just to pay their workers subsistence wages (e.g. Brandt and Zhu, 2001). Such SOEs are likely to pay below average compared to other SOEs. If regional privatisation was able to cause such SOEs to exit the market, the resultant average SOE pay may indeed increase.

### 5.4 Capital Misallocation, Productivity and Investment

Column 1 of Table 10 fail to find significant effects of privatisation on capital misallocation. Column 2 finds that a 1pp increase in the share of non-state employment is associated with a statistically significant  $100\% \times (e^{1.37 \times 0.01} - 1) = 1.38\%$  change in value added per unit real capital (mean = 1.29, sd = 1.11). While private firms appear to be more positively affected, the effect does not statistically significantly differ from that of SOEs.

Columns 5–7 report the estimated effect of privatisation on real investment (mean = 16,

sd = 12) when averaged over different firm types. All estimates are positive but noisily estimated, and there is no evidence that private firms' investment response to privatisation differs from that of SOEs.

## 6 Robustness and Extensions

I present findings from robustness checks and extensions. Instrument validity checks are in Section 4.3 as I incorporate their findings in baseline.

**Non-linearity.** I add the first-differenced squared treatment to the regression and instrument for it using the first-differenced squared instrument. I then rerun regressions on all outcomes used in this study.

Appendix Table A2 reports results. None of the squared terms display any statistical significance. I also note that the F statistic is much lower at 16. While this is above 10, there may still be a weak instrument problem introducing bias<sup>22</sup>. The F statistic's dramatic fall is unsurprising: there is generally no reason why a strong first stage relationship should persist when both treatment and instrument are squared.

**Alternative TFP estimation.** I prefer the LP method to the OP method of TFP estimation as it additionally allows for firm-level heterogeneity in investment prices, which is highly probable in China. Out of curiosity, I reestimate the effect of privatisation on TFP using the OP estimate.

Appendix Table A3 reports results. Comparing the first and last three columns of Panel C, the estimates are now much lower in magnitude and statistically insignificant. The results reinforce the notion that OP and LP estimates may not be mutually consistent. One possibility is that heterogeneity in capital adjustment costs or investment prices significantly bias the OP estimate of TFP. There could also be imprecision from data loss, as OP estimation is run on fewer observations. This finding is not well-documented, at least in the context of Chinese firms, and may prove a fruitful avenue of further research.

**Exclude privatised firms when averaging outcome.** Of interest is whether the heterogeneous effect by private firms and SOEs further varies when excluding privatised firms. I find no evidence of this.

---

<sup>22</sup>The Anderson-Rubin p-value is lower than 0.10 in some columns. However, here it tests the *joint* significance of the endogenous treatment and its squared term in explaining the outcome.



In Appendix Table A4, odd-numbered columns report baseline estimates when averaging outcomes over SOE and private firm subsets. In even-numbered columns, I instead exclude privatised firms<sup>23</sup> when taking averages of the outcome. Then, I test whether the coefficient of interest now differs. This yields high p-values. Excluding privatised firms do not appear to cause the coefficient of interest to differ significantly.

**Dropping municipalities.** A municipality is directly administered—both a city and a province. All four Chinese municipalities (Beijing, Shanghai, Tianjin and Chongqing) are large, concentrated cities of national importance. Municipality leaders, with their outsized influence, may have been able to manipulate the *Zhua Da Fang Xiao* policy in favour of industries with a high presence in their municipalities. To address this possible source of endogeneity, Appendix Table A5 shows that results are robust to dropping municipalities.

**Clustering robust standard errors by province.** The political economy framework in Section 4.3 casts endogeneity in city privatisation as a result of competition between city leaders. This argument can be elevated to province level. Indeed, Appendix Table A5 shows that clustering by province instead of city returns extremely similar results.

## 7 Limitations

This paper may have defined regional privatisation too narrowly. Non-state employment share may not fully capture the presence or importance of the local private sector. Better protection of private property rights, for example, may promote market competition fairness and allow private firms greater say in transactions. Future research may therefore consider incorporating business leaders’ perceptions of the legal environment, similar to those attempted by Wang et al. (2007). This issue is common across studies within this strand of literature that employ similar definitions.

Also, this study is run on pre-2007 data. I suspect that the Great Financial Crisis may dampen the negative relationship between regional state presence and productivity. This is because SOEs, with their “soft budget constraints”, are often used to reduce unemployment when it threatens social stability (Wen, 2020). The presence of SOEs may therefore improve local sentiment in times of great uncertainty. Also, this study is run on industrial firms. Future research can test if this paper’s results can be generalised to post-2008 China and to non-industrial sectors.

Finally, I reiterate that the imputation method may lead to overrejection (Section 3.2.1),

---

<sup>23</sup>A firm is defined as privatised if its ownership is observed to change at least once in the sample.

and industry-level shocks are technically jointly predicted by the combination of 114 city characteristics (Section 4.4). The former may also have affected the latter.

## 8 Conclusion

Using a shift-share instrumental variables strategy, this paper presents evidence that regional privatisation significantly improved firm productivity. Further, it suggests that studies that fail to account for the endogeneity in regional privatisation may underestimate gains to productivity and private business dynamism.

I consider three possible channels through which productivity gains may manifest: private sector expansion, reduced labour misallocation, and reduced capital misallocation. I find that privatisation strongly encourages private sector expansion.

The investigation on labour and capital misallocation ultimately proved inconclusive. Privatisation appears to have no significant impact on direct measures of factor misallocation, yet is shown to improve typical outcomes of lower misallocation. Thus, the presence of the state sector (and the lack thereof) may not explain as much of factor misallocation as previous studies may indicate. While surprising, this discovery appears to be in line with that of a very recent study by [Ouyang \(2022\)](#), who uses a different metric for misallocation and finds similar results for within-*sector* misallocations. Therefore, while the effect of China’s massive privatisation programme will likely be subject to continued scrutiny, policymakers should also consider the role of other market-oriented reforms in improving productivity and factor misallocations.

## 9 Tables

Table 1: Firm Financial Statistics Summary

	N(Firms)	Mean	SD	Min	Max
Industrial Output	2,071,881	81	779.4	0.0	186,000
Intermediate Input	2,071,881	61	590.0	0.0	173,000
Revenue	2,071,863	80	807.6	-101.6	195,988
Net Value of Fixed Assets	2,071,881	33	489.8	-30.3	150,000
Liabilities	2,071,881	51	474.5	0.0	79,310
Average Compensation	2,071,881	.014	0.0	0.0	0
Firm Age	2,071,881	12	12.8	0.0	107
Employment	2,071,881	288	1,331.6	9.0	194,410

Note: This table reports summary statistics on the cleaned Chinese Annual Industrial Survey. Monetary values in RMB 1,000,000. Firm count, age and employment in raw numbers. Return on assets is the ratio of profits to assets. Means are unweighted. Values may differ from that of other studies due to cleaning methods and sample period restrictions.

Table 2: Average Firm Financial Statistics by Year

Year	N(Firms)	Industrial Output	Interm. Input	Revenue	Net Value of Fixed Assets	Liabilities	Average Comp.	Firm Age	Emp.	ROA
1998	146,035	44	33	41	27	44	0.008	16	392	0.04
1999	143,899	48	36	46	31	47	0.009	16	376	0.04
2000	145,024	56	42	55	33	49	0.010	16	356	0.05
2001	155,853	59	44	57	33	48	0.011	15	327	0.05
2002	166,631	64	48	63	33	48	0.011	14	312	0.06
2003	184,114	75	56	75	34	51	0.012	13	298	0.07
2004	258,350	75	57	74	29	46	0.014	10	244	0.07
2005	259,465	94	70	93	38	52	0.016	10	255	0.08
2006	288,477	106	79	105	34	55	0.018	10	246	0.09
2007	324,033	121	91	120	36	60	0.020	10	235	0.11

Note: This table reports yearly unweighted average firm statistics on the cleaned Chinese Annual Industrial Survey. Monetary values in RMB 1,000,000. Firm count, age and employment in raw numbers. Return on assets is the ratio of profits to assets. Values may differ from that of other studies due to cleaning methods and sample restrictions.

Table 3: Balance Test of City Characteristics

	(1)		(2)	
	Shock		Endogenous Treatment	
	$\Delta Priv_{nt}$		$\Delta Priv_{ct}$	
	$\Delta$ Share of Industry		$\Delta$ Share of City	
	Non-State Employment		Non-State Employment	
<i>Accessibility</i>				
No of Public Buses per Capita	0.0021	(0.021)	-0.088**	(0.041)
Highway: Freight Traffic	-0.0053	(0.017)	-0.0078	(0.018)
Highway: Length of Highway	0.043**	(0.020)	0.00040	(0.017)
Highway: Passenger Traffic	0.045***	(0.016)	-0.011	(0.021)
Highway: Passenger Traffic: Public Transport	-0.0082	(0.018)	-0.029*	(0.016)
Star-Rated Hotel: Number of Hotel	0.00035	(0.022)	-0.034*	(0.020)
No of Motor Vehicles per Capita	0.0019	(0.014)	-0.035*	(0.019)
Post and Telecom: Business Volume	0.0030	(0.023)	-0.039***	(0.015)
No of Rental Vehicles per Capita	-0.019	(0.016)	-0.052**	(0.027)
Railway: Freight Traffic	-0.055***	(0.020)	-0.0053	(0.023)
Area of Paved Road	-0.0096	(0.016)	-0.024**	(0.012)
Railway: Passenger Traffic	-0.011	(0.019)	-0.021	(0.017)
<i>Economy</i>				
Commodity Building Sold	-0.012	(0.020)	-0.045***	(0.016)
Consumption Expenditure per Capita: Rural	-0.048*	(0.025)	-0.045**	(0.018)
Consumption Expenditure per Capita: Rural: Food	-0.022	(0.028)	-0.019	(0.019)
Consumption Expenditure per Capita: Urban	-0.010	(0.028)	-0.057***	(0.016)
Consumption Expenditure per Capita: Urban: Food	-0.0082	(0.026)	-0.041***	(0.015)
Deposit	-0.0054	(0.019)	-0.032**	(0.015)
Deposit by Enterprise	-0.0067	(0.018)	-0.040**	(0.016)
Saving Deposit	-0.0081	(0.019)	-0.014	(0.013)
Saving Deposit: Demand	-0.0058	(0.021)	-0.048***	(0.013)
Saving Deposit: Time	-0.014	(0.017)	-0.039**	(0.016)
Disposable Income per Capita: Urban	-0.0049	(0.031)	-0.051***	(0.016)
Floor Space Sold: Commodity	0.0033	(0.018)	-0.022	(0.015)
GDP	-0.00068	(0.018)	-0.074**	(0.032)
GDP: per Capita	-0.034	(0.038)	-0.071***	(0.025)
GDP: Primary Industry	0.056***	(0.016)	0.044	(0.037)
GDP: Secondary Industry	-0.0028	(0.018)	-0.091***	(0.034)
GDP: Tertiary Industry	-0.0033	(0.018)	-0.064**	(0.027)
Gross Industrial Output	-0.012	(0.018)	-0.080***	(0.021)
GIO: Domestic Funded Enterprise	-0.014	(0.017)	-0.074***	(0.027)
GIO: Foreign Funded Enterprise	-0.019	(0.019)	-0.050**	(0.020)
GIO: HMT Funded Enterprise	0.0023	(0.027)	-0.075***	(0.014)
Loan	-0.0036	(0.018)	-0.034**	(0.016)
Loan to Agricultural Sector	0.055***	(0.021)	0.010	(0.015)
Loan to Commercial Sector	0.0020	(0.019)	-0.0042	(0.025)
Loan to Industrial Sector	-0.011	(0.016)	-0.031**	(0.014)
No of Enterprise: Industrial	-0.016	(0.016)	-0.050***	(0.019)
No of Enterprise: Industrial: DF	-0.023	(0.018)	-0.050***	(0.017)
No of Enterprise: Industrial: FF	-0.021	(0.017)	-0.050***	(0.014)
No of Enterprise: Industrial: HMT	-0.018	(0.021)	-0.057***	(0.017)
Property Price	-0.025	(0.025)	-0.054***	(0.019)
Real Estate Investment	-0.0031	(0.019)	-0.035*	(0.018)
Real Estate Investment: Residential	-0.0026	(0.019)	-0.044***	(0.017)
Wholesale & Retail Sales	-0.021	(0.020)	-0.022**	(0.0092)
Wholesale & Retail: No of Enterprise	0.0071	(0.027)	-0.042**	(0.017)
Retail Sales of Consumer Goods	0.00049	(0.019)	-0.060**	(0.023)
<i>Education</i>				
No of School: Higher Institution	-0.011	(0.018)	-0.0040	(0.015)
No of School: Primary School	0.041***	(0.012)	0.021	(0.023)
No of School: Secondary School	0.025	(0.016)	0.050	(0.032)
Student-Teacher Ratio: Higher Inst	0.016	(0.023)	0.00091	(0.021)
Student-Teacher Ratio: Primary School	0.050**	(0.022)	0.046**	(0.021)

Student-Teacher Ratio: Secondary School	-0.0052	(0.012)	-0.015***	(0.0047)
<i>Environment</i>				
% Green Space	0.0073	(0.028)	-0.038***	(0.014)
Sulphur Dioxide Emission: Industry	0.0039	(0.018)	-0.014	(0.017)
Ratio of Industrial Solid Waste Utilized	0.030	(0.026)	-0.0093	(0.018)
Treatment Rate of Living Waste	-0.015	(0.023)	-0.021	(0.026)
Treatment Rate of Living Waste Water	-0.022	(0.014)	-0.032	(0.028)
Waste Water Discharge: Industry	0.013	(0.017)	-0.0057	(0.018)
Waste Water Discharge: Industry: Meet Discharge Standard	0.011	(0.017)	-0.0069	(0.019)
<i>Health</i>				
No of Beds in Hospitals per Capita	-0.0098	(0.012)	-0.072**	(0.035)
No of Hospitals per Capita	-0.027*	(0.015)	-0.025	(0.028)
<i>Infrastructure</i>				
% Population Subscribed to Broadband	0.026	(0.017)	-0.021	(0.033)
Developed Area of City Construction	-0.0099	(0.018)	-0.013	(0.015)
Floor Area of Residential Building per Capita: Rural	0.026	(0.018)	-0.015	(0.018)
Floor Area of Residential Building per Capita: Urban	0.015	(0.017)	-0.0059	(0.017)
Land Area of Administrative Zone	0.022*	(0.012)	-0.016	(0.013)
% Population with Landline	0.0042	(0.015)	-0.11**	(0.047)
Transport: Freight Traffic	-0.0097	(0.017)	-0.0067	(0.016)
% Population with Mobile Phone	-0.0066	(0.020)	-0.076***	(0.022)
Transport: Passenger Traffic	0.045***	(0.017)	-0.012	(0.022)
% Employed in Primary Industry	0.0060	(0.019)	0.044	(0.040)
No of Employee: Tertiary Industry	0.019	(0.019)	-0.0091	(0.015)
% Self Employed	0.020	(0.022)	0.044	(0.030)
% Unemployed	0.0019	(0.021)	-0.11	(0.088)
<i>Politics</i>				
Politicians Average Yrs of Schooling	-0.0098	(0.021)	-0.0083	(0.017)
Politicians Average Career Incentive	-0.032	(0.024)	0.012	(0.022)
% of Corrupt Politicians	0.0074	(0.024)	0.048**	(0.022)
<i>Population</i>				
Population: Census	0.035**	(0.015)	0.061**	(0.031)
% Male Population	-0.025	(0.045)	-0.014	(0.018)
Average Household Size	0.015	(0.028)	-0.00019	(0.016)
% Urban Population	-0.036	(0.026)	-0.028	(0.026)
% Population with Access to Gas	-0.026*	(0.015)	-0.076	(0.056)
<i>Public Finance</i>				
Govt Expenditure	-0.0035	(0.018)	-0.026*	(0.015)
Govt Expenditure: Education	-0.0053	(0.017)	-0.021	(0.017)
Govt Expenditure: Science	-0.025	(0.020)	-0.019*	(0.0098)
Govt Expenditure: Social Security	-0.031	(0.038)	-0.0041	(0.011)
Govt Revenue	-0.0063	(0.018)	-0.048**	(0.021)
Govt Revenue: Tax	-0.0088	(0.018)	-0.041**	(0.019)
Govt Revenue: Tax: Enterprises Income	-0.0098	(0.017)	-0.018	(0.013)
Govt Revenue: Tax: Individual Income	-0.0093	(0.017)	-0.046***	(0.015)
Govt Revenue: Tax: Operation	-0.0033	(0.019)	-0.021	(0.013)
Govt Revenue: Tax: Value Added	-0.011	(0.018)	-0.068***	(0.018)
<i>Trade and Tourism</i>				
Export	0.016	(0.028)	-0.066***	(0.022)
Fixed Asset Investment	-0.0017	(0.018)	-0.027	(0.019)
FDI: Contract Value	-0.011	(0.018)	-0.017	(0.014)
FDI: No of Contract	0.0034	(0.018)	-0.011	(0.016)
FDI: Utilized	-0.015	(0.015)	-0.020	(0.012)
Import	0.0014	(0.023)	-0.045***	(0.016)
Tourism Revenue: Domestic	-0.0093	(0.016)	-0.036**	(0.014)
Tourism Revenue: Foreign Currency	-0.0057	(0.020)	-0.038***	(0.015)
Visitor Arrival	0.016	(0.027)	-0.046***	(0.016)
Domestic Tourist	-0.0043	(0.017)	-0.018	(0.015)
Trade Balance	0.011	(0.017)	-0.052**	(0.024)
<i>Energy and Utilities</i>				
Coal & Natural Gas Supply	-0.0040	(0.021)	-0.0079	(0.013)

Coal & Natural Gas Supply: Residential	-0.0044	(0.016)	0.018	(0.012)
Electricity Consumption	-0.0079	(0.018)	-0.060**	(0.026)
Electricity Consumption: Industry	-0.023	(0.018)	-0.018	(0.015)
Electricity Consumption per Capita	0.0039	(0.015)	-0.10**	(0.048)
Electricity Consumption: Residential	-0.0083	(0.012)	0.0048	(0.024)
LPG Supply	0.016	(0.026)	-0.013	(0.018)
LPG Supply: Non-Residential	0.020	(0.023)	-0.0071	(0.018)
Water Supply	-0.0064	(0.018)	-0.022	(0.026)
Water Supply: Residential	-0.0077	(0.018)	-0.026	(0.017)
No of Units	809		295	
No of Units×Years	7,596		2,911	
Joint Significance: P-value	0.00		0.00	
Year FE	Yes		Yes	

Note: This table reports estimates from bivariate regressions of the shock and the endogenous treatment on city-level characteristics. All variables are standardised to have a mean of 0 and variance of 1. Column 1 is an industry-level regression: city-level characteristics are first residualised on year dummies interacted with a city's start-of-period share of industrial sector employment, then transformed into industry-level equivalents through industry shares  $s_{cnt}$ . Column 2 is a standard city-level regression and controls for year fixed effects. Robust standard errors are clustered at the city level and reported in parentheses. The regression in Column 1 is weighted by the industry-level average exposure  $s_{nt} = \sum_c e_{ct}s_{cnt}$ . The F statistic from a joint significant test of all city covariates is reported. Exposure-robust standard errors are reported in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Table 4: Unbalanced Characteristics from Balance Test on Shocks

	<b>Shock</b> <b><math>\Delta Priv_{nt}</math></b> $\Delta$ Share of Industry Non-State Employment	
Highway: Length of Highway	0.043**	(0.020)
Highway: Passenger Traffic	0.045***	(0.016)
Railway: Freight Traffic	-0.055***	(0.020)
Consumption Expenditure per Capita: Rural	-0.048*	(0.025)
GDP: Primary Industry	0.056***	(0.016)
Loan to Agricultural Sector	0.055***	(0.021)
No of School: Primary School	0.041***	(0.012)
Student-Teacher Ratio: Primary School	0.050**	(0.022)
No of Hospitals per Capita	-0.027*	(0.015)
Land Area of Administrative Zone	0.022*	(0.012)
Transport: Passenger Traffic	0.045***	(0.017)
Population: Census	0.035**	(0.015)
% Population with Access to Gas	-0.026*	(0.015)
No of Industries	809	
No of Industries $\times$ Years	7,596	
Year FE	Yes	

Note: This table reports coefficient estimates only for city characteristics that are likely correlated with the shock ( $p < 0.1$ ) from Table 3. All variables are standardised to have a mean of 0 and variance of 1. Exposure-robust standard errors reported in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Table 5: Summary Statistics on Shares and Shocks

<i>Panel A. Industry-Level Average Exposure <math>s_{nt}</math></i>		
Mean		0.0001
Standard Deviation		0.0003
Maximum		0.006
<i>Panel B. Shocks <math>\Delta Priv_{nt}</math></i>		
Mean		0.03
Standard Deviation		0.09
Interquartile Range		0.06
<i>Panel C. Shock Sample Size</i>		
Number of Industries		809
Number of Industry-Year Shocks		7596
Effective Sample Size ( $1/\sum_{n,t} s_{nt}^2$ )		1096.7

Note: This table reports statistics on industry-level average exposure  $s_{nt} = \sum_c e_{ct} s_{cnt}$  and shocks  $\Delta Priv_{nt}$ .

Table 6: Intraclass Correlation of  $\Delta Priv_{nt}$ 

CIC2	0.007	(0.03)
CIC3	0.018	(0.04)
CIC4	0.000	(.)

Note: This table reports the estimated intraclass correlation of shocks  $\Delta Priv_{nt}$ . Industries are grouped by the Chinese Industry Classification (CIC) system. 4 digits (CIC4) correspond to the unit level. The ICC is estimated using maximum likelihood, with year fixed effects and an exchangeable covariance structure imposed on the CIC2 and CIC3 random effects. Robust standard errors reported in parentheses.  $*p < 0.1$ ,  $**p < 0.05$ ,  $***p < 0.01$ .

Table 7: Elasticity Estimates by Method

	Value-Added Elasticity of Capital, $\hat{\beta}_k$	Value-Added Elasticity of Labour, $\hat{\beta}_l$
OLS	0.316***	0.458***
Olley-Pakes	0.354***	0.441***
Levinsohn-Petrin	0.336***	0.484***

Note: This table reports the average  $\hat{\beta}$  values of each estimation method on 3-digit industry subsets. The Akerberg-Caves-Frazer correction is applied to Olley-Pakes and Levinsohn-Petrin estimation techniques.  $*p < 0.1$ ,  $**p < 0.05$ ,  $***p < 0.01$ .



Table 8: Main Results

	$\Delta$ Return On Assets			$\Delta$ Log TFP			$\Delta$ Entry
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All Firms	Private	SOE	All Firms	Private	SOE	All Firms
<i>Panel A. OLS</i>							
$\Delta$ Share of Non-State Employment	0.047*** (0.017)	0.0084 (0.019)	-0.021 (0.024)	1.76*** (0.55)	-0.084 (0.25)	1.98* (1.19)	0.31*** (0.037)
(2) $\neq$ (3)/(5) $\neq$ (6): P-value			0.35			0.10	
$R^2$	0.08	0.06	0.10	0.04	0.02	0.03	0.26
<i>Panel B. Reduced Form</i>							
Predicted $\Delta$ Share of Non-State Employment	0.11 (0.077)	0.088 (0.083)	-0.025 (0.10)	2.34** (1.10)	1.20 (0.74)	1.19 (2.26)	0.56*** (0.14)
$R^2$	0.07	0.06	0.10	0.02	0.02	0.03	0.23
<i>Panel C. 2SLS</i>							
$\Delta$ Share of Non-State Employment	0.086 (0.061)	0.070 (0.067)	-0.020 (0.083)	1.85** (0.87)	0.95 (0.60)	0.94 (1.79)	0.45*** (0.11)
(2) $\neq$ (3)/(5) $\neq$ (6): P-value			0.38			0.97	
Kleibergen Wald rk F	115	115	115	115	115	115	115
AR P-value	0.15	0.28	0.81	0.03	0.10	0.60	0.00
No of Cities	294	294	294	294	294	294	294
No of Cities $\times$ Years	2612	2612	2612	2612	2612	2612	2612

Note: Outcomes are first-differenced average deviations in returns to assets (total profits/total assets), log TFP (estimated using the LP method with ACF correction), and private firm entry (ratio of output by firms aged 3 or younger to that of firms aged 4 and older) from the yearly mean. City average outcomes are taken over all firms in Columns 1 4 and 7, private firms in Columns 2 and 5, and SOEs in Columns 3 and 6, weighted by firm value added. Baseline controls include city start-of-period share of industrial sector employment  $S_{ct} = \sum_n s_{cnt}$  interacted with year fixed effects, as well as city characteristics that are unbalanced against industry-level shocks. Regressions are weighted by the number of firms in each city-year. All monetary values used in calculations are deflated to 1998 levels. P-values from testing the equivalence of the coefficient of interests when the outcome is averaged over different subset of firms are reported in Columns 3 and 6. Kleibergen-Paap rk Wald F-statistics and Anderson-Rubin weak-instrument-robust Wald test p-values are reported for 2SLS Regressions. RSEs clustered at city level and reported in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Table 9: Results on Labour Misallocation, Productivity and Average Real Compensation

	$\Delta$ Labour OP Gap	$\Delta$ Log Labour Productivity			$\Delta$ Per Worker Compensation		
	(1) All Firms	(2) All Firms	(3) Private	(4) SOE	(5) All Firms	(6) Private	(7) SOE
<i>Panel A. OLS</i>							
$\Delta$ Share of Non-State Employment	-0.38** (0.16)	0.088 (0.13)	0.019 (0.15)	-0.033 (0.23)	-0.0046** (0.0020)	0.0013 (0.0029)	-0.0011 (0.0031)
(2) $\neq$ (3)/(5) $\neq$ (6): P-value				0.89			0.63
$R^2$	0.08	0.04	0.04	0.06	0.10	0.12	0.05
<i>Panel B. Reduced Form</i>							
Predicted $\Delta$ Share of Non-State Employment	-0.038 (0.56)	2.84*** (0.50)	2.94*** (0.49)	3.08*** (0.91)	-0.0073 (0.0085)	-0.0014 (0.0084)	0.037** (0.015)
$R^2$	0.08	0.06	0.06	0.06	0.09	0.12	0.06
<i>Panel C. 2SLS</i>							
$\Delta$ Share of Non-State Employment	-0.030 (0.44)	2.25*** (0.42)	2.33*** (0.44)	2.44*** (0.70)	-0.0058 (0.0067)	-0.0011 (0.0066)	0.029** (0.012)
(2) $\neq$ (3)/(5) $\neq$ (6): P-value				0.85			0.02
Kleibergen Wald rk F	115	115	115	115	115	115	115
AR P-value	0.95	0.00	0.00	0.00	0.38	0.87	0.01
No of Cities	294	294	294	294	294	294	294
No of Cities $\times$ Years	2612	2612	2612	2612	2612	2612	2612

Note: Outcomes are first-differenced average deviations in the labour OP gap (covariance between labour productivity and value added), log labour productivity (value added/employment) and per worker compensation (sum of total wages and welfare payments/employment) from the yearly mean. City average outcomes are taken over all firms in Columns 1, 2 and 5, private firms in Columns 3 and 6, and SOEs in Columns 4 and 7, weighted by firm value added. Baseline controls include city start-of-period share of industrial sector employment  $S_{ct} = \sum_n s_{cnt}$  interacted with year fixed effects, as well as city characteristics that are unbalanced against industry-level shocks. Regressions are weighted by the number of firms in each city-year. All monetary values used in calculations are deflated to 1998 levels. P-values from testing the equivalence of the coefficient of interests when the outcome is averaged over different subset of firms are reported in Columns 4 and 7. Kleibergen-Paap rk Wald F-statistics and Anderson-Rubin weak-instrument-robust Wald test p-values are reported for 2SLS Regressions. RSEs clustered at city level and reported in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Table 10: Results on Capital Misallocation, Productivity and Real Investment

	$\Delta$ Capital OP Gap	$\Delta$ Log Capital Productivity			$\Delta$ Investment		
	(1) All Firms	(2) All Firms	(3) Private	(4) SOE	(5) All Firms	(6) Private	(7) SOE
<i>Panel A. OLS</i>							
$\Delta$ Share of Non-State Employment	-0.22* (0.13)	0.43*** (0.11)	-0.14 (0.11)	-0.37* (0.20)	-5.94 (3.71)	9.79** (4.34)	-10.3* (6.11)
(3) $\neq$ (4)/(6) $\neq$ (7): P-value				0.40			0.01
$R^2$	0.05	0.10	0.06	0.09	0.10	0.07	0.07
<i>Panel B. Reduced Form</i>							
Predicted $\Delta$ Share of Non-State Employment	0.49 (0.44)	1.73*** (0.47)	0.55 (0.51)	-0.030 (0.85)	10.5 (14.9)	14.1 (14.5)	46.4 (29.8)
$R^2$	0.04	0.09	0.06	0.09	0.10	0.06	0.07
<i>Panel C. 2SLS</i>							
$\Delta$ Share of Non-State Employment	0.39 (0.36)	1.37*** (0.41)	0.44 (0.42)	-0.024 (0.68)	8.29 (11.9)	11.2 (11.5)	36.8 (24.2)
(3) $\neq$ (4)/(6) $\neq$ (7): P-value				0.59			0.31
Kleibergen Wald rk F	115	115	115	115	115	115	115
AR P-value	0.26	0.00	0.28	0.97	0.48	0.33	0.12
No of Cities	294	294	294	294	294	294	294
No of Cities $\times$ Years	2612	2612	2612	2612	2612	2612	2612

Note: Outcomes are first-differenced average deviations in the capital OP gap (covariance between capital productivity and value added), log capital productivity (value added/capital stock) and real investment from the yearly mean. City average outcomes are taken over all firms in Columns 1, 2 and 5, private firms in Column 3 and 6, and SOEs in Columns 4 and 7, weighted by firm value added. Baseline controls include city start-of-period share of industrial sector employment  $S_{ct} = \sum_n s_{cnt}$  interacted with year fixed effects, as well as city characteristics that are unbalanced against industry-level shocks. Regressions are weighted by the number of firms in each city-year. All monetary values used in calculations are deflated to 1998 levels. P-values from testing the equivalence of the coefficient of interests when the outcome is averaged over different subset of firms are reported in Columns 4 and 7. Kleibergen-Paap rk Wald F-statistics and Anderson-Rubin weak-instrument-robust Wald test p-values are reported for 2SLS Regressions. RSEs clustered at city level and reported in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## 10 Figures

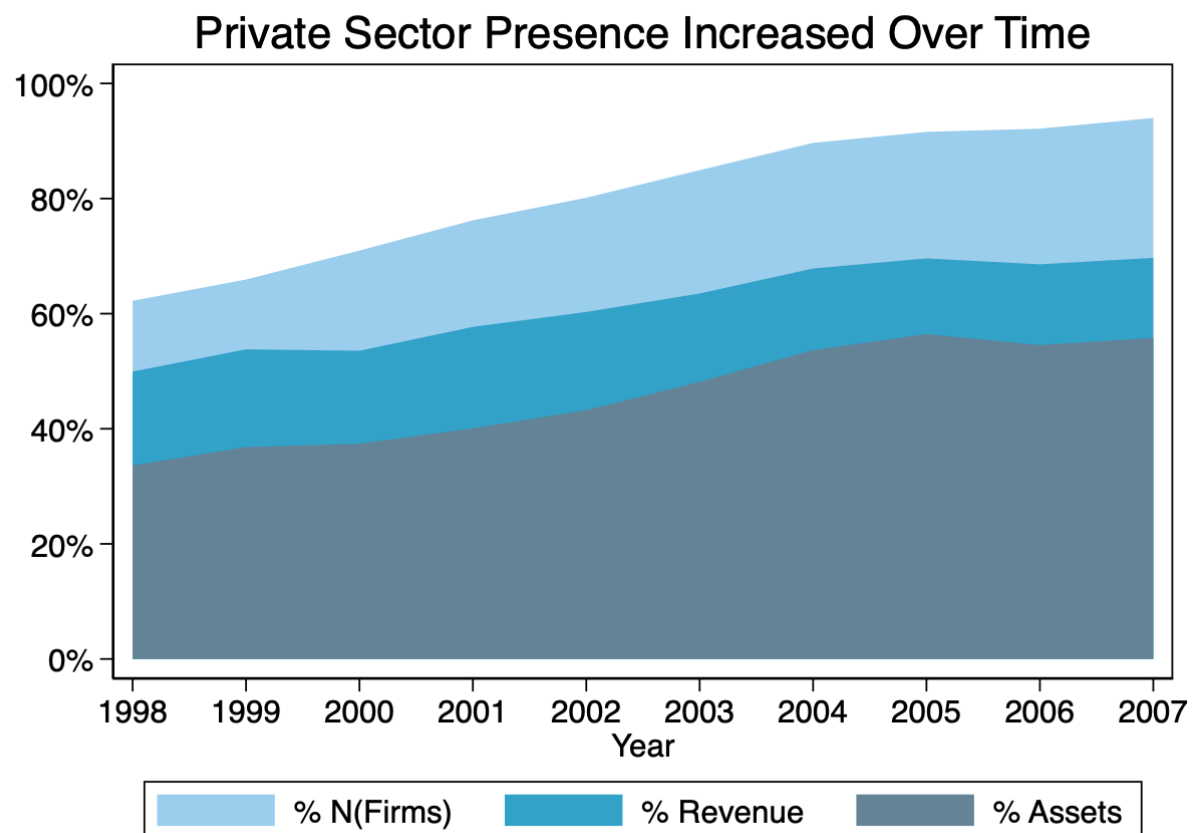
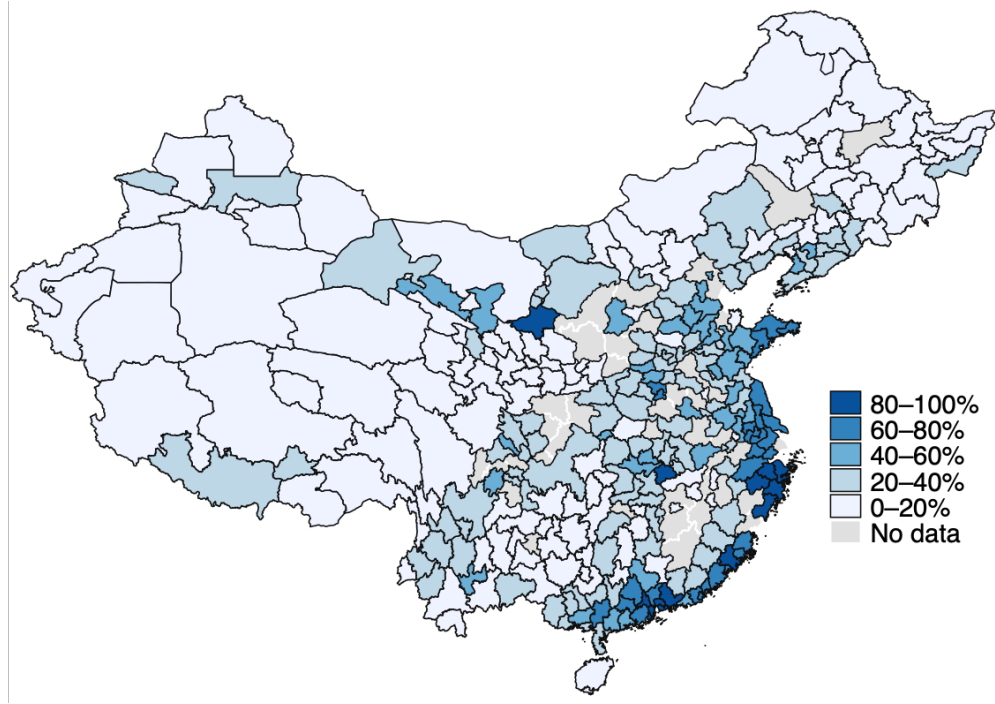
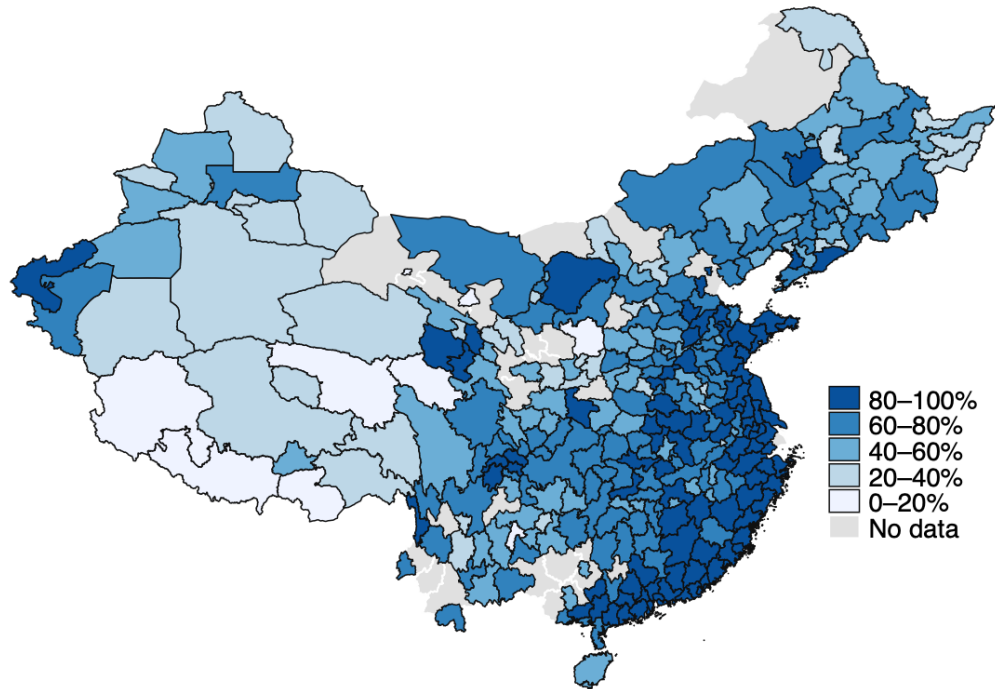


Figure 1: Non-State Sector Presence Increased Over Time

NOTE: This graph documents the change in private firms' share of the industrial sector's total assets, revenue and number of firms during the sample period. Source: Annual Industrial Survey.



(a) 1998



(b) 2007

Figure 2: Share of Non-State Employment by Mainland Chinese City

NOTE: Mapping to geospatial data from [China Data Lab \(2020\)](#) requires usage of the China's official area codes. Area code revisions over time cause both figures to have missing cities and Subfigure 2a to have more. In subsequent analyses, I manually match cities over time and across datasets to ensure consistency.

Source: Annual Industrial Survey.

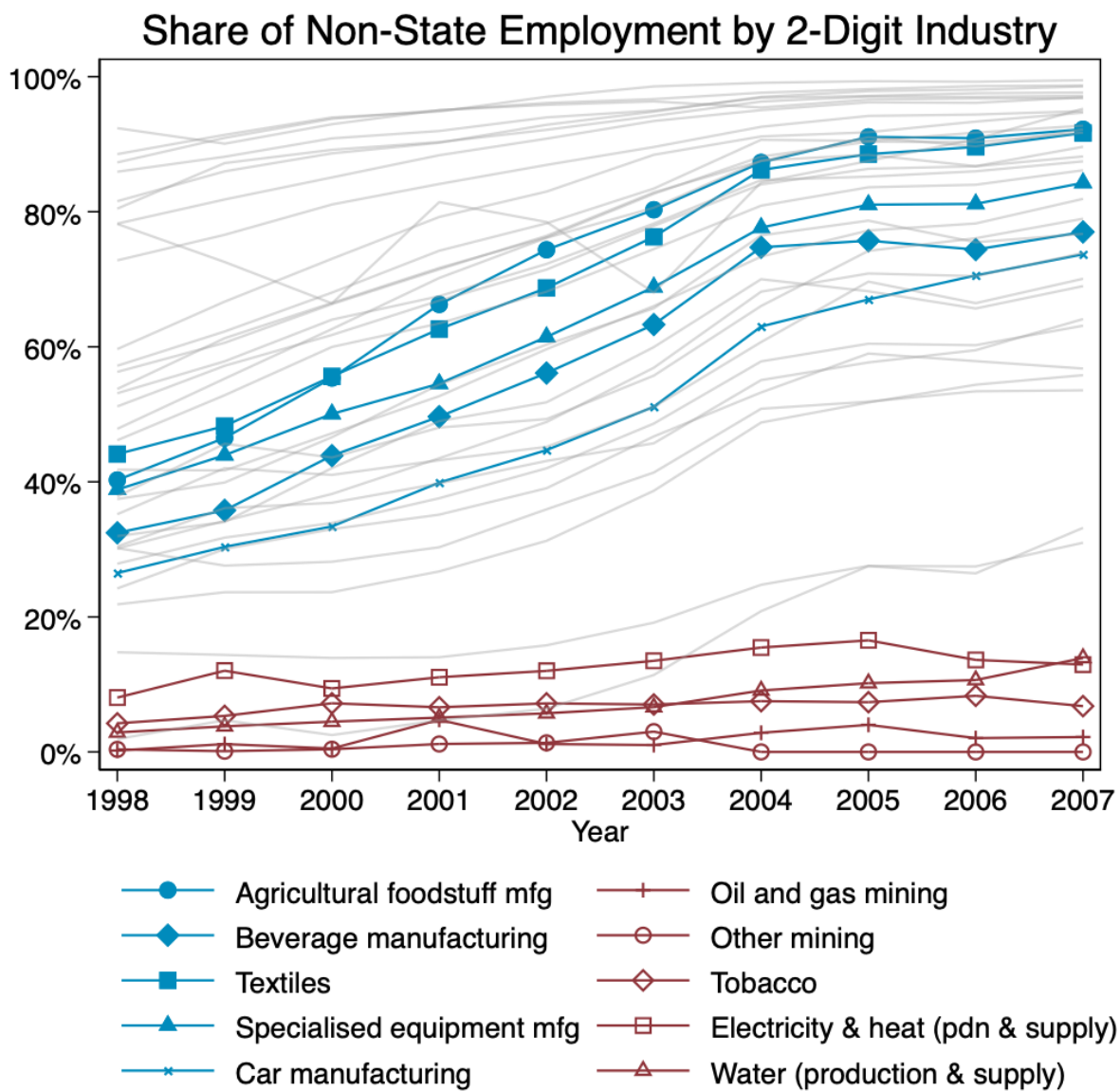


Figure 3: Share of Non-State Employment by 2-Digit Industry  
 NOTE: Blue lines indicate the five most privatised industries by net increase in share of non-state employment between 1998 and 2007. Red lines indicate the five least privatised industries. Source: Annual Industrial Survey.

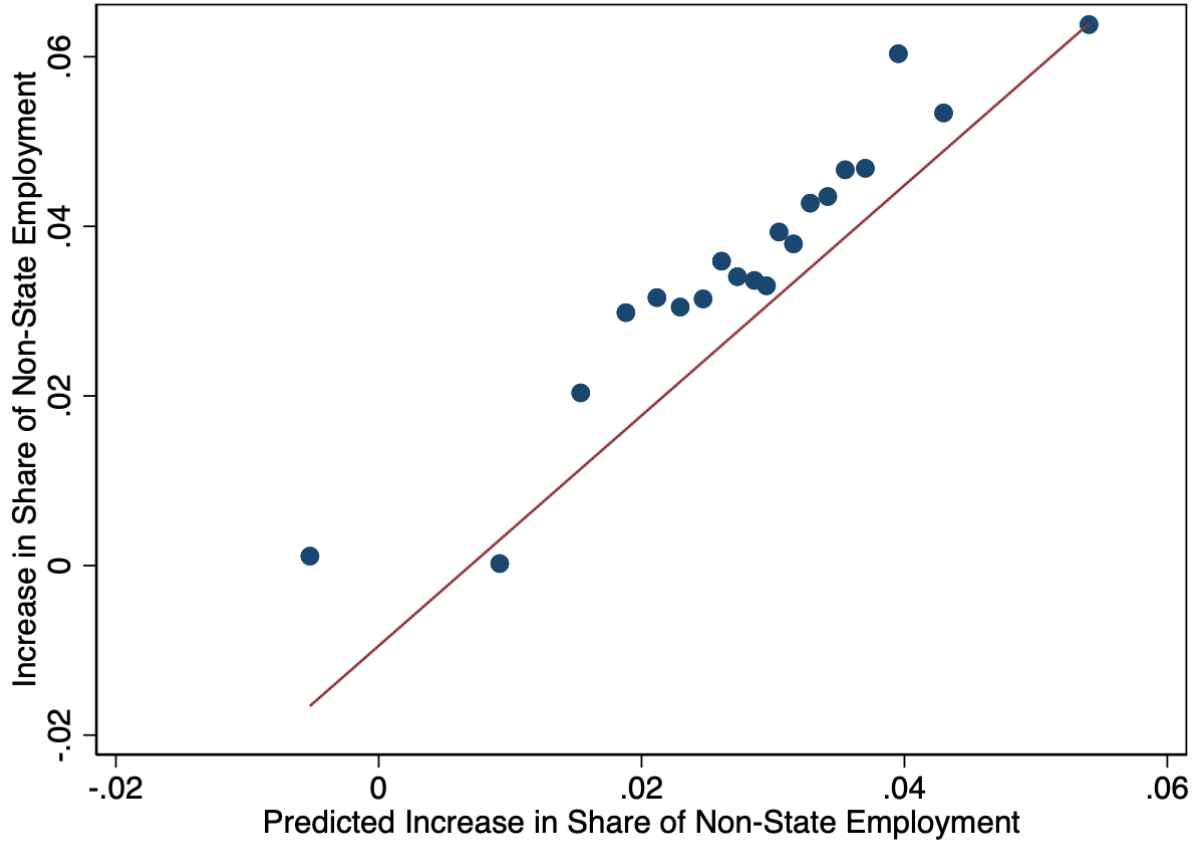


Figure 4: First Stage: Instrument Strongly Predicts Treatment

Note: This binned scatterplot shows the relationship between predicted and actual increase in the share of non-state employment in a city. The (endogenous) treatment  $\Delta Priv_{ct}$ , as depicted on the vertical axis, is grouped into 20 5-percentile bins. Both treatment and instrument were residualised on city characteristics that are unbalanced against industry shocks and city start-of-period share of industrial sector employment

$S_{ct} = \sum_n s_{cnt}$  interacted with year fixed effects. The first stage F-statistic is 115.

# References

- ACEMOGLU, D. AND P. RESTREPO (2020): “Robots and Jobs: Evidence from US Labor Markets,” *Journal of Political Economy*, 128, 2188–2244.
- ACKERBERG, D. A., K. CAVES, AND G. FRAZER (2015): “Identification Properties of Recent Production Function Estimators,” *Econometrica*, 83, 2411–2451.
- AGHION, P., J. CAI, M. DEWATRIPONT, L. DU, A. HARRISON, AND P. LEGROS (2015): “Industrial Policy and Competition,” *American Economic Journal: Macroeconomics*, 7, 1–32.
- AGHION, P. AND P. HOWITT (1992): “A Model of Growth Through Creative Destruction,” *Econometrica*, 60, 323–351.
- ANDERSON, T. W. AND H. RUBIN (1949): “Estimation of the Parameters of a Single Equation in a Complete System of Stochastic Equations,” *The Annals of Mathematical Statistics*, 20, 46 – 63.
- AUTOR, D. H., D. DORN, AND G. H. HANSON (2013): “The China Syndrome: Local Labor Market Effects of Import Competition in the United States,” *American Economic Review*, 103, 2121–68.
- BAI, P. AND W. CHENG (2016): “Labour Misallocation in China: 1980–2010,” *Applied Economics*, 48, 2321–2332.
- BARTELSMAN, E., J. HALTIWANGER, AND S. SCARPETTA (2013): “Cross-Country Differences in Productivity: The Role of Allocation and Selection,” *American Economic Review*, 103, 305–34.
- BORUSYAK, K., P. HULL, AND X. JARAVEL (2022): “Quasi-Experimental Shift-Share Research Designs,” *Review of Economic Studies*, 89, 181–213.
- BOUSTAN, L. P. (2010): “Was Postwar Suburbanization “White Flight”? Evidence from the Black Migration,” *Quarterly Journal of Economics*, 125, 417–443.
- BRANDT, L., G. KAMBOUROV, AND K. STORESLETTEN (2020): “Barriers to Entry and Regional Economic Growth in China,” CEPR Discussion Paper No. DP14965.
- BRANDT, L., T. TOMBE, AND X. ZHU (2013): “Factor Market Distortions across Time, Space and Sectors in China,” *Review of Economic Dynamics*, 16, 39–58.
- BRANDT, L., J. VAN BIESEBROECK, AND Y. ZHANG (2012): “Creative Accounting or Creative Destruction? Firm-level Productivity Growth in Chinese Manufacturing,” *Journal of Development Economics*, 97, 339–351.
- BRANDT, L. AND X. ZHU (2001): “Soft Budget Constraint and Inflation Cycles: a Positive Model of the Macro-Dynamics in China during Transition,” *Journal of Development Economics*, 64, 437–457.
- CAI, F., A. PARK, AND Y. ZHAO (2008): *The Chinese Labor Market in the Reform Era*, Cambridge University Press, 167–214.
- CASELLI, F. (2005): “Accounting for Cross-Country Income Differences,” *Handbook of Economic Growth*, 1, 679–741.
- CERDEIRO, D. AND C. RUANE (2022): “China’s Declining Business Dynamism,” Imf working paper.
- CHINA DATA LAB (2020): “China COVID-19 Daily Cases with Basemap,” .
- COMBES, P.-P. AND L. GOBILLON (2015): “The Empirics of Agglomeration Economies,” in *Handbook of Regional and Urban Economics*, ed. by G. Duranton, J. V. Henderson, and W. C. Strange, Elsevier, vol. 5 of *Handbook of Regional and Urban Economics*, 247–348.
- CURTIS, C. C. (2016): “Economic Reforms and the Evolution of China’s Total Factor Productivity,” *Review of Economic Dynamics*, 21, 225–245.
- DE LOECKER, J. AND F. WARZYNSKI (2012): “Markups and Firm-level Export Status,” *American Economic Review*, 102, 2437–71.
- DERENONCOURT, E. (2022): “Can you Move to Opportunity? Evidence from the Great Migration,”



- American Economic Review*, 112, 369–408.
- DRAY, S. (2022): “Local Fiscal Capacity: Historical Evidence from U.S. Cities,” Working paper, London School of Economics and Political Science.
- DU, J. AND X. LIU (2015): “Selection, Staging, and Sequencing in the Recent Chinese Privatization,” *The Journal of Law & Economics*, 58, 657–682.
- DU, L., A. HARRISON, AND G. JEFFERSON (2014): “FDI Spillovers and Industrial Policy: The Role of Tariffs and Tax Holidays,” *World Development*, 64, 366–383.
- ESTRIN, S., J. HANOUSEK, E. KOCENDA, AND J. SVEJNAR (2009): “The Effects of Privatization and Ownership in Transition Economies,” *Journal of Economic Literature*, 47, 699–728.
- GARCIA-MACIA, D., C.-T. HSIEH, AND P. J. KLENOW (2019): “How Destructive is Innovation?” *Econometrica*, 87, 1507–1541.
- GARNAUT, R. AND Y. HUANG (2001): *Growth without Miracles: Readings on the Chinese Economy in the Era of Reform*, Oxford University Press on Demand.
- GOLDSMITH-PINKHAM, P., I. SORKIN, AND H. SWIFT (2020): “Bartik instruments: What, When, Why, and How,” *American Economic Review*, 110, 2586–2624.
- GUSTAFSSON, B. AND H. WAN (2020): “Wage Growth and Inequality in Urban China: 1988–2013,” *China Economic Review*, 62, 101462.
- HAN, B. (2021): “The Other Side of the Industrial Coin: The Causal Impact of Government Ownership on the Productivity of Private Chinese Firms.” Undergraduate thesis.
- HARRISON, A., M. MEYER, P. WANG, L. ZHAO, AND M. ZHAO (2019): “Can a Tiger Change Its Stripes? Reform of Chinese State-Owned Enterprises in the Penumbra of the State,” Working Paper 25475, National Bureau of Economic Research.
- HE, H., F. HUANG, Z. LIU, AND D. ZHU (2018): “Breaking the “Iron Rice Bowl”: Evidence of Precautionary Savings from the Chinese State-Owned Enterprises Reform,” *Journal of Monetary Economics*, 94, 94–113.
- HSIEH, C.-T. AND P. J. KLENOW (2009): “Misallocation and Manufacturing TFP in China and India,” *Quarterly Journal of Economics*, 124, 1403–1448.
- HSIEH, C.-T. AND Z. M. SONG (2015): “Grasp the Large, Let Go of the Small: The Transformation of the State Sector in China,” Working Paper 21006, National Bureau of Economic Research.
- HUANG, Z., J. LIU, G. MA, AND L. C. XU (2020): “The Transformative Effects of Privatization in China: A Natural Experiment Based on Politician Career Concern,” World Bank Policy Research Working Paper 9261.
- JAMSHIDIAN, M. AND M. MATA (2007): “2 - Advances in Analysis of Mean and Covariance Structure when Data are Incomplete.” in *Handbook of Latent Variable and Related Models*, ed. by S.-Y. Lee, Amsterdam: North-Holland, Handbook of Computing and Statistics with Applications, 21–44.
- JIANG, J. (2018): “Making Bureaucracy Work: Patronage Networks, Performance Incentives, and Economic Development in China,” *American Journal of Political Science*, 62, 982–999.
- JURZYK, M. E. M. AND M. C. RUANE (2021): *Resource Misallocation Among Listed Firms in China: The Evolving Role of State-Owned Enterprises*, International Monetary Fund.
- LANTERI, A. AND P. MEDINA (2017): “Capital Specificity, the Distribution of Marginal Products and Aggregate Productivity,” .
- LAU, L. J., Y. QIAN, AND G. ROLAND (2000): “Reform without Losers: An Interpretation of China’s Dual-Track Approach to Transition,” *Journal of Political Economy*, 108, 120–143.
- LEVINSOHN, J. AND A. PETRIN (2003): “Estimating Production Functions Using Inputs to Control for Unobservables,” *Review of Economic Studies*, 70, 317–341.
- LIN, C., R. MORCK, B. Y. YEUNG, AND X. ZHAO (2016): “Anti-Corruption Reforms and Shareholder Valuations: Event Study Evidence from China,” *Available at SSRN 2729087*.
- LIN, K. J., X. LU, J. ZHANG, AND Y. ZHENG (2020): “State-Owned Enterprises in China: A

- Review of 40 Years of Research and Practice,” *China Journal of Accounting Research*, 13, 31–55.
- MENG, X. (2000): *Labour Market Reform in China*, Cambridge University Press.
- MICKIEWICZ, T. (2010): *Economics of Institutional Change: Central and Eastern Europe Revisited*, Springer.
- MORRIS, T. P., I. R. WHITE, AND P. ROYSTON (2014): “Tuning multiple imputation by predictive mean matching and local residual draws,” *BMC medical research methodology*, 14, 1–13.
- OLLEY, G. S. AND A. PAKES (1996): “The Dynamics of Productivity in the Telecommunications Equipment Industry,” *Econometrica*, 64, 1263–1297.
- OUYANG, D. (2022): “Factor Distortions and Resource Misallocation across Chinese Manufacturing Firms,” *Available at SSRN 4071472*.
- PEI, Z., J.-S. PISCHKE, AND H. SCHWANDT (2019): “Poorly Measured Confounders are More Useful on the Left than on the Right,” *Journal of Business & Economic Statistics*, 37, 205–216.
- PERKINS, D. H. AND T. G. RAWSKI (2008): “Forecasting China’s Economic Growth to 2025,” *China’s Great Economic Transformation*, 829–86.
- ROVIGATTI, G. AND V. MOLLISI (2016): “PRODEST: Stata module for production function estimation based on the control function approach,” .
- SHLEIFER, A. AND R. W. VISHNY (1994): “Politicians and Firms,” *Quarterly Journal of Economics*, 109, 995–1025.
- SONG, Z., K. STORESLETTEN, AND F. ZILIBOTTI (2011): “Growing Like China,” *American Economic Review*, 101, 196–233.
- STANSBURY, A. M. AND L. H. SUMMERS (2018): “Productivity and Pay: Is the Link Broken?” Tech. rep., National Bureau of Economic Research.
- STOCK, J. AND D. STAIGER (1997): “Instrumental Variables Regression with Weak Instruments,” *Econometrica*, 65, 557–586.
- WANG, S. AND D. Y. YANG (2021): “Policy Experimentation in China: the Political Economy of Policy Learning,” Tech. rep., National Bureau of Economic Research.
- WANG, X., G. FAN, AND H. ZHU (2007): “Marketisation in China, progress and contribution to growth,” *China: Linking Markets for Growth*, 30–44.
- WANG, Z., Q. ZHANG, AND L.-A. ZHOU (2020): “Career Incentives of City Leaders and Urban Spatial Expansion in China,” *Review of Economics and Statistics*, 102, 897–911.
- WEN, J. (2020): “The Political Economy of State Employment and Instability in China,” *Job market paper, Yale University*.
- WHITE, I. R., P. ROYSTON, AND A. M. WOOD (2011): “Multiple Imputation Using Chained Equations: Issues and Guidance for Practice,” *Statistics in Medicine*, 30, 377–399.
- WONG, C. P. W. AND D. BHATTASALI (2003): “China - National Development and Sub-National Finance : A Review of Provincial Expenditures,” .
- WOOLDRIDGE, J. M. (2009): “On Estimating Firm-Level Production Functions using Proxy Variables to Control for Unobservables,” *Economics Letters*, 104, 112–114.
- WU, G. L. (2018): “Capital Misallocation in China: Financial Frictions or Policy Distortions?” *Journal of Development Economics*, 130, 203–223.
- XU, C. (2011): “The Fundamental Institutions of China’s Reforms and Development,” *Journal of Economic Literature*, 49, 1076–1151.
- YUE, C. AND P. HUA (2002): “Does Comparative Advantage Explain Export Patterns in China?” *China Economic Review*, 13, 276–296.
- ZHU, X. (2012): “Understanding China’s Growth: Past, Present, and Future,” *Journal of Economic Perspectives*, 26, 103–24.

# A Additional Tables

Table A1: Panel on City Characteristics: Summary Statistics

	Mean	SD	Min	Max
<i>Accessibility</i>				
No of Public Buses per Capita	0.0002	0.0003	0.00000	0
Highway: Freight Traffic	39.5	43.3	0.2	420
Highway: Length of Highway	5847.1	5409.2	212	104,705
Highway: Passenger Traffic	52.9	60.3	0.7	733
Highway: Passenger Traffic: Public Transport	129.0	347.0	0	4,498
Star-Rated Hotel: Number of Hotel	31.5	47.5	1	806
No of Motor Vehicles per Capita	0.03	0.06	0.001	2
Post and Telecom: Business Volume	1767.6	3585.0	46.8	63,492
No of Rental Vehicles per Capita	0.0008	0.001	0.00000	0
Railway: Freight Traffic	9.1	13.6	0.0	147
Area of Paved Road	8.7	14.0	0.0	215
Railway: Passenger Traffic	4.2	7.2	0	101
<i>Economy</i>				
Commodity Building Sold	4134.2	14737.3	10	308,935
Consumption Expenditure per Capita: Rural	2293.2	1162.5	707	10,783
Consumption Expenditure per Capita: Rural: Food	1010.0	440.9	249	4,440
Consumption Expenditure per Capita: Urban	6095.8	2344.4	1378	24,376
Consumption Expenditure per Capita: Urban: Food	2264.7	856.8	1065	7,778
Deposit	75325.1	198644.3	1840	3,770,025
Deposit by Enterprise	23145.7	92586.5	570	2,042,104
Saving Deposit	40525.6	71912.9	500.8	1,181,909
Saving Deposit: Demand	12315.6	23513.1	160	346,711
Saving Deposit: Time	22588.4	39902.4	130	616,060
Disposable Income per Capita: Urban	8000.4	3260.3	0	28,209
Floor Space Sold: Commodity	1290.6	2611.2	4	36,950
GDP	51.9	81.7	0.8	1,288
GDP: per Capita	11557.3	10711.8	0.001	98,938
GDP: Primary Industry	6.3	5.0	0.0	47
GDP: Secondary Industry	25.0	39.0	0.3	568
GDP: Tertiary Industry	20.7	43.5	0.4	791
Gross Industrial Output	59641.8	133310.3	20	2,310,863
GIO: Domestic Funded Enterprise	40818.3	66994.7	20	761,113
GIO: Foreign Funded Enterprise	11444.1	52132.4	0.6	1,201,160
GIO: HMT Funded Enterprise	6864.4	24394.9	0.5	372,032
Loan	55007.5	130332.5	855	2,170,995
Loan to Agricultural Sector	3083.2	3228.5	0	43,953
Loan to Commercial Sector	5691.5	8043.1	5	105,622
Loan to Industrial Sector	7220.3	14621.5	0	205,020
No of Enterprise: Industrial	730.9	1238.3	4	15,766
No of Enterprise: Industrial: DF	508.2	814.7	15	9,217
No of Enterprise: Industrial: FF	67.3	234.8	0	4,086
No of Enterprise: Industrial: HMT	70.0	215.3	0	2,498
Property Price	1614.0	991.5	439	14,615
Real Estate Investment	3772.3	11256.6	0	199,582
Real Estate Investment: Residential	2736.2	7157.6	0	99,166
Wholesale & Retail Sales	23.9	86.3	0.0	1,666
Wholesale & Retail: No of Enterprise	122.8	272.0	0	6,088
Retail Sales of Consumer Goods	13.7	20.4	0.4	283
<i>Education</i>				
No of School: Higher Institution	5.4	9.5	0	83
No of School: Primary School	1412.5	1212.6	24	15,737
No of School: Secondary School	250.9	166.6	9	1,607
Student-Teacher Ratio: Higher Inst	14.9	5.4	0.5	59
Student-Teacher Ratio: Primary School	20.5	4.9	9.0	38
Student-Teacher Ratio: Secondary School	18.3	22.5	7.4	1,245

<i>Environment</i>				
% Green Space	0.005	0.02	0.00001	0
Sulphur Dioxide Emission: Industry	64.6	69.1	0.1	759
Ratio of Industrial Solid Waste Utilized	61.3	27.8	1.9	100
Treatment Rate of Living Waste	70.5	28.9	1	100
Treatment Rate of Living Waste Water	38.3	24.5	0	100
Waste Water Discharge: Industry	94.6	108.9	0.2	940
Waste Water Discharge: Industry: Meet Discharge Standard	85.2	101.0	0.0	881
<i>Health</i>				
No of Beds in Hospitals per Capita	0.003	0.002	0.000	0
No of Hospitals per Capita	0.00006	0.00008	0.00000	0
<i>Infrastructure</i>				
% Population Subscribed to Broadband	0.05	0.09	0.000	2
Developed Area of City Construction	82.3	144.6	0	2,429
Floor Area of Residential Building per Capita: Rural	30.5	8.6	8.1	68
Floor Area of Residential Building per Capita: Urban	19.5	4.8	0	63
Land Area of Administrative Zone	16398.8	24593.2	236	391,817
% Population with Landline	0.2	0.2	0.01	3
Transport: Freight Traffic	54.2	63.0	0.3	781
% Population with Mobile Phone	0.3	0.5	0.01	10
Transport: Passenger Traffic	58.2	64.7	0.9	772
% Employed in Primary Industry	0.08	0.2	0.0001	7
No of Employee: Tertiary Industry	252.8	531.2	8.3	7,365
% Self Employed	0.6	0.4	0.0	3
% Unemployed	0.06	0.04	0.001	0
<i>Politics</i>				
Politicians Average Yrs of Schooling	17.0	2.4	0	23
Politicians Average Career Incentive	0.4	0.2	0.1	1
% of Corrupt Politicians	0.10	0.2	0	1
<i>Population</i>				
Population: Census	3994.2	2839.6	159.2	32,353
% Male Population	0.5	0.06	0.1	1
Average Household Size	3.4	0.6	2.0	18
% Urban Population	0.4	0.2	0.1	1
% Population with Access to Gas	0.1	0.2	0.00000	3
<i>Public Finance</i>				
Govt Expenditure	4898.8	10435.8	330.5	220,192
Govt Expenditure: Education	852.3	1409.2	8.3	28,333
Govt Expenditure: Science	77.9	338.8	0.3	10,577
Govt Expenditure: Social Security	344.9	903.9	2	27,422
Govt Revenue	3195.1	8948.6	78.7	210,263
Govt Revenue: Tax	2617.9	8198.2	97	197,548
Govt Revenue: Tax: Enterprises Income	512.3	1616.6	3	42,563
Govt Revenue: Tax: Individual Income	212.2	760.6	5	16,944
Govt Revenue: Tax: Operation	987.1	3188.1	46	71,460
Govt Revenue: Tax: Value Added	516.5	1438.9	11	31,342
<i>Trade and Tourism</i>				
Export	2146.5	8149.5	0.0	168,493
Fixed Asset Investment	19956.7	34582.9	476.2	445,861
FDI: Contract Value	439.6	1319.1	0	18,363
FDI: No of Contract	118.1	326.5	0	4,334
FDI: Utilized	255.9	669.8	0	7,920
Import	1704.5	8162.2	-36.8	144,073
Tourism Revenue: Domestic	3878.2	10333.4	0.2	175,360
Tourism Revenue: Foreign Currency	541.4	2390.3	0	35,542
Visitor Arrival	137.5	506.7	0	8,313
Domestic Tourist	5784.6	10183.3	0	142,800
Trade Balance	433.5	3623.3	-95147.0	49,453
<i>Energy and Utilities</i>				
Coal & Natural Gas Supply	158.5	664.1	0	16,508
Coal & Natural Gas Supply: Residential	34.7	99.3	0	2,053

Electricity Consumption	4.0	7.7	0.0	107
Electricity Consumption: Industry	-416187.1	1196584.9	-14252480.8	73
Electricity Consumption per Capita	0.001	0.002	0.0000	0
Electricity Consumption: Residential	4.1	6.4	0.01	15
LPG Supply	51708.7	92775.6	0	984,190
LPG Supply: Non-Residential	32774.0	69367.6	-150917	752,252
Water Supply	149.5	271.3	0.3	3,461
Water Supply: Residential	52.5	93.8	0.3	1,260
Observations	3020			

Note: This table reports summary statistics on city-level data. Data on local politics are taken from sources described in the main text. All other variables are from CEIC.

Table A2: Robustness: Test for Non-Linearity

	(1) $\Delta ROA$	(2) $\Delta \text{Log TFP}$	(3) $\Delta \text{Log Lab Pdy}$	(4) $\Delta \text{Comp}$	(5) $\Delta \text{Log Cap Pdy}$	(6) $\Delta \text{Inv}$	(7) Lab OP Gap	(8) Cap OP Gap	(9) $\Delta \text{Entry}$
<i>Panel A. OLS</i>									
$\Delta$ Share of Non-State Employment	0.060* (0.032)	1.82** (0.85)	1.13*** (0.25)	0.00022 (0.0029)	0.94*** (0.21)	-3.86 (6.25)	-0.53** (0.25)	-0.26 (0.22)	0.071 (0.068)
$\Delta$ Share of Non-State Employment (Squared)	-0.013 (0.032)	-0.056 (1.02)	-0.96*** (0.23)	-0.0044 (0.0033)	-0.47** (0.19)	-1.91 (6.39)	0.13 (0.27)	0.040 (0.23)	0.22*** (0.064)
$R^2$	0.08	0.04	0.05	0.10	0.10	0.10	0.09	0.05	0.26
<i>Panel B. Reduced Form</i>									
Predicted $\Delta$ Share of Non-State Employment	0.17* (0.095)	2.88*** (1.08)	3.39*** (0.66)	-0.0092 (0.0090)	1.90*** (0.56)	14.3 (14.6)	0.22 (0.58)	0.62 (0.49)	0.75*** (0.15)
Predicted $\Delta$ Share of Non-State Emp. (Squared)	-1.45 (1.23)	-11.4 (18.7)	-12.6* (7.34)	0.074 (0.12)	-10.4 (6.78)	-24.6 (205.2)	-4.77 (8.55)	-4.28 (8.73)	-3.97 (2.50)
$R^2$	0.08	0.02	0.06	0.09	0.07	0.05	0.08	0.05	0.22
<i>Panel C. 2SLS</i>									
$\Delta$ Share of Non-State Employment	-0.39 (0.44)	-1.46 (7.16)	-1.41 (2.89)	0.019 (0.045)	-2.07 (2.53)	4.89 (78.8)	-1.60 (3.19)	-1.02 (3.17)	-0.76 (0.92)
$\Delta$ Share of Non-State Employment (Squared)	0.39 (0.35)	2.81 (5.41)	3.07 (2.38)	-0.020 (0.035)	2.68 (1.98)	4.81 (59.7)	1.33 (2.45)	1.13 (2.48)	1.02 (0.72)
Kleibergen Wald rk F	16.06	16.06	16.06	16.06	16.06	16.06	16.06	16.06	16.06
AR P-value	0.20	0.03	0.00	0.57	0.00	0.61	0.83	0.44	0.00
No of Cities	294	294	294	294	294	294	294	294	294
No of Cities $\times$ Years	2320	2320	2320	2320	2320	2320	2320	2320	2320

Note: Baseline controls include city start-of-period share of industrial sector employment  $S_{ct} = \sum_n s_{cnt}$  interacted with year fixed effects, as well as city characteristics that are unbalanced against industry-level shocks. All monetary values used in calculations are deflated to 1998 levels. P-values from testing the equivalence of the coefficient of interests when the outcome is averaged over different subset of firms are reported in Columns 4 and 7. Kleibergen-Paap rk Wald F-statistics and Anderson-Rubin weak-instrument-robust Wald test p-values are reported for 2SLS Regressions. RSEs clustered at city level and reported in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Table A3: Robustness: Alternative Productivity Measurement

	$\Delta \text{Log TFP} - \text{LP (Baseline)}$			$\Delta \text{Log TFP} - \text{OP}$		
	(1) All Firms	(2) Private	(3) SOE	(4) All Firms	(5) Private	(6) SOE
<i>Panel A. OLS</i>						
$\Delta$ Share of Non-State Employment	1.76*** (0.55)	-0.084 (0.25)	1.98* (1.19)	1.23** (0.50)	0.58 (0.92)	0.36** (0.15)
(2) $\neq$ (3)/(5) $\neq$ (6): P-value			0.10		0.83	
$R^2$	0.04	0.02	0.03	0.03	0.02	0.02
<i>Panel B. Reduced Form</i>						
Predicted $\Delta$ Share of Non-State Employment	2.34** (1.10)	1.20 (0.74)	1.19 (2.26)	0.57 (1.11)	0.65 (0.50)	-0.079 (2.45)
$R^2$	0.02	0.02	0.03	0.02	0.02	0.02
<i>Panel C. 2SLS</i>						
$\Delta$ Share of Non-State Employment	1.85** (0.87)	0.95 (0.60)	0.94 (1.79)	0.46 (0.87)	0.52 (0.39)	-0.063 (1.94)
(2) $\neq$ (3)/(5) $\neq$ (6): P-value			0.97			0.75
Kleibergen Wald rk F	115	115	115	115	115	115
AR P-value	0.03	0.10	0.60	0.60	0.18	0.97
No of Cities	294	294	294	294	294	294
No of Cities $\times$ Years	2612	2612	2612	2607	2607	2607

Note: This table compare results using log TFP estimated using the Levinsohn-Petrin method as in baseline, and when estimated using the Olley-Pakes method. All regressions control for the sum of industry shares  $S_{ct} = \sum_n s_{cnt}$  interacted with year fixed effects. Kleibergen-Paap rk Wald F-statistics and Anderson-Rubin weak-instrument-robust Wald test p-values are reported for 2SLS Regressions. RSEs clustered at city level and reported in parentheses.  $*p < 0.1$ ,  $**p < 0.05$ ,  $***p < 0.01$ .

Table A4: Robustness: Exclude Privatised Firms

	$\Delta$ Return On Assets				$\Delta$ Log TFP			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	SOE	Always SOE	Private	Always Private	SOE	Always SOE	Private	Always Private
<i>Panel A. OLS</i>								
$\Delta$ Share of Non-State Employment	-0.021 (0.024)	0.0034 (0.030)	0.0084 (0.019)	0.025 (0.021)	1.98* (1.19)	1.99 (1.24)	-0.084 (0.25)	0.19 (0.17)
Cross-Eq Test: P-value		0.25		0.00		0.99		0.15
$R^2$	0.10	0.07	0.06	0.05	0.03	0.03	0.02	0.02
<i>Panel B. Reduced Form</i>								
Predicted $\Delta$ Share of Non-State Employment	-0.025 (0.10)	0.060 (0.15)	0.088 (0.083)	0.12 (0.092)	1.19 (2.26)	-2.34 (6.03)	1.20 (0.74)	1.14* (0.66)
$R^2$	0.10	0.07	0.06	0.05	0.03	0.03	0.02	0.02
<i>Panel C. 2SLS</i>								
$\Delta$ Share of Non-State Employment	-0.0200 (0.0832)	0.0477 (0.120)	0.0699 (0.0669)	0.0919 (0.0747)	0.942 (1.789)	-1.856 (4.794)	0.954 (0.597)	0.908* (0.532)
Cross-Eq Test: P-value		0.56		0.24		0.47		0.92
Kleibergen Wald rk F	115	115	115	115	115	115	115	115
AR P-value	0.81	0.69	0.28	0.21	0.60	0.70	0.10	0.08
No of Cities	294	294	294	294	294	294	294	294
No of Cities $\times$ Years	2612	2612	2612	2612	2612	2612	2612	2612

Note: This table compares results when averaging outcomes over different subsets of firms. All regressions control for the sum of industry shares  $S_{ct} = \sum_n s_{cnt}$  interacted with year fixed effects. Kleibergen-Paap rk Wald F-statistics and Anderson-Rubin weak-instrument-robust Wald test p-values are reported for 2SLS Regressions. RSEs clustered at city level and reported in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .



Table A5: Additional Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$\Delta$ Return On Assets			$\Delta$ Log TFP			$\Delta$ Per Worker Compensation		
$\Delta$ Share of Non-State Employment (OLS)	0.047*** (0.017)	0.047** (0.022)	0.048*** (0.017)	1.76*** (0.55)	1.76*** (0.56)	1.76*** (0.56)	-0.0046** (0.0020)	-0.0046** (0.0020)	-0.0037* (0.0020)
$R^2$ (OLS)	0.08	0.08	0.07	0.04	0.04	0.04	0.10	0.10	0.09
$\Delta$ Share of Non-State Employment (IV)	0.086 (0.061)	0.086 (0.068)	0.092 (0.062)	1.85** (0.87)	1.85 (1.17)	1.85** (0.89)	-0.0058 (0.0067)	-0.0058 (0.0070)	-0.0053 (0.0067)
Kleibergen Wald rk F	114.67	87.97	117.12	114.67	87.97	117.12	114.67	87.97	117.12
AR P-value	0.15	0.20	0.13	0.03	0.13	0.04	0.38	0.41	0.42
	$\Delta$ Log Labour Productivity			$\Delta$ Log Capital Productivity			$\Delta$ Investment		
$\Delta$ Share of Non-State Employment (OLS)	2.25*** (0.42)	2.25*** (0.35)	2.36*** (0.43)	0.43*** (0.11)	0.43*** (0.14)	0.42*** (0.11)	-5.94 (3.71)	-5.94 (3.73)	-4.97 (3.63)
$R^2$ (OLS)	-0.26	-0.26	-0.29	0.10	0.10	0.07	0.10	0.10	0.07
$\Delta$ Share of Non-State Employment (IV)	2.25*** (0.42)	2.25*** (0.35)	2.36*** (0.43)	1.37*** (0.41)	1.37*** (0.44)	1.34*** (0.42)	8.29 (11.9)	8.29 (17.4)	8.91 (12.1)
Kleibergen Wald rk F	114.67	87.97	117.12	114.67	87.97	117.12	114.67	87.97	117.12
AR P-value	0.00	0.00	0.00	0.00	0.00	0.00	0.48	0.62	0.45
	$\Delta$ Labour OP Gap			$\Delta$ Capital OP Gap			$\Delta$ Entry		
$\Delta$ Share of Non-State Employment (OLS)	-0.38** (0.16)	-0.38** (0.18)	-0.38** (0.16)	-0.22* (0.13)	-0.22 (0.18)	-0.20 (0.13)	0.31*** (0.037)	0.31*** (0.037)	0.30*** (0.037)
$R^2$ (OLS)	0.08	0.08	0.05	0.05	0.05	0.02	0.26	0.26	0.27
$\Delta$ Share of Non-State Employment (IV)	-0.030 (0.44)	-0.030 (0.52)	-0.017 (0.45)	0.39 (0.36)	0.39 (0.26)	0.36 (0.36)	0.45*** (0.11)	0.45*** (0.14)	0.47*** (0.10)
Kleibergen Wald rk F	114.67	87.97	117.12	114.67	87.97	117.12	114.67	87.97	117.12
AR P-value	0.95	0.95	0.97	0.26	0.11	0.30	0.00	0.00	0.00
Is Baseline	Yes			Yes			Yes		
Cluster by Province		Yes			Yes			Yes	
Drop Municipalities			Yes			Yes			Yes
No of Clusters	294	31	290	294	31	290	294	31	290
No of Observations	2612	2612	2579	2612	2612	2579	2612	2612	2579

Note: All regressions control for the sum of industry shares  $S_{ct} = \sum_n s_{cnt}$  interacted with year fixed effects. Kleibergen-Paap rk Wald F-statistics and Anderson-Rubin weak-instrument-robust Wald test p-values are reported for 2SLS Regressions. RSEs clustered at city level and reported in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## B Additional Details on Empirical Strategy

### B.1 Translating the Shift-Share Instrument Orthogonality Condition into a Shock Orthogonality Condition

This proof is adapted from [Borusyak et al. \(2022\)](#). I begin with a reminder that the second stage equation is

$$\Delta y_{ct} = \lambda_t + \beta \Delta Priv_{ct} + \Delta \mathbb{X}'_{ct} \gamma + \Delta u_{ct}$$

and the instrument is

$$\widehat{\Delta Priv_{ct}} = \sum_n s_{cnt} \times \Delta Priv_{nt}$$

The usual condition for identification is for the instrument to be orthogonal with the second stage residual, conditional on city-level controls  $\Delta \mathbb{X}_{ct}$  and weighted by regression weights by  $e_{ct}$  ( $\sum_{c,t} e_{ct} = 1$ ).

$$E \left[ \sum_t \sum_c e_{ct} \widehat{\Delta Priv_{ct}} \Delta u_{ct} \middle| \sum_t \sum_c e_{ct} \Delta \mathbb{X}_{ct} \right] = 0$$

This is stated at the city level, but can be transformed into an industry-level condition, stated in terms of shocks. Using the interchangeability of summation operators and the assumption that  $\sum_n s_{cnt} = 1$ ,

$$\begin{aligned} & E \left[ \sum_t \sum_c e_{ct} \widehat{\Delta Priv_{ct}} \Delta u_{ct} \middle| \sum_t \sum_c e_{ct} \Delta \mathbb{X}_{ct} \right] \\ &= E \left[ \sum_t \sum_c \sum_n e_{ct} s_{cnt} \Delta Priv_{nt} \Delta u_{ct} \middle| \sum_t \sum_c e_{ct} \sum_n s_{cnt} \Delta \mathbb{X}_{ct} \right] \\ &= E \left[ \sum_t \sum_n \Delta Priv_{nt} \sum_c e_{ct} s_{cnt} \Delta u_{ct} \middle| \sum_t \sum_c \sum_n e_{ct} s_{cnt} \Delta \mathbb{X}_{ct} \right] \\ &= E \left[ \sum_t \sum_n \Delta Priv_{nt} \sum_c e_{ct} s_{cnt} \frac{\sum_c e_{ct} s_{cnt} \Delta u_{ct}}{\sum_c e_{ct} s_{cnt}} \middle| \sum_t \sum_n \sum_c e_{ct} s_{cnt} \frac{\sum_c e_{ct} s_{cnt} \Delta \mathbb{X}_{ct}}{\sum_c e_{ct} s_{cnt}} \right] \\ &= E \left[ \sum_t \sum_n \sum_c e_{ct} s_{cnt} \Delta Priv_{nt} \frac{\sum_c e_{ct} s_{cnt} \Delta u_{ct}}{\sum_c e_{ct} s_{cnt}} \middle| \sum_t \sum_n \sum_c e_{ct} s_{cnt} \frac{\sum_c e_{ct} s_{cnt} \Delta \mathbb{X}_{ct}}{\sum_c e_{ct} s_{cnt}} \right] \\ &= E \left[ \sum_t \sum_n s_{nt} \Delta Priv_{nt} \overline{\Delta u_{nt}} \middle| \sum_t \sum_n s_{nt} \overline{\Delta \mathbb{X}_{nt}} \right] = 0 \end{aligned}$$

where  $s_{nt} := \sum_c e_{ct} s_{cnt}$ ,  $\overline{\Delta x_{nt}} := \frac{\sum_c e_{ct} s_{cnt} \Delta x_{ct}}{\sum_c e_{ct} s_{cnt}}$ .

$s_{nt}$  can be viewed as regression weights in this industry-level condition since

$$\sum_t \sum_n s_{nt} = \sum_t \sum_n \sum_c e_{ct} s_{cnt} = \sum_t \sum_c e_{ct} \sum_n s_{cnt} = 1 \quad (16)$$

This, however, assumes that  $\sum_n s_{cnt} = 1$ , which is only possible if the data records information for all industries. Since  $\sum_n s_{cnt} = S_{ct} \leq 1$  in my data, I control for  $S_{ct}$  interacted with year dummies when transforming city-level variables into industry-level equivalents.

## B.2 Approaches to Estimating Firm Productivity

Consider a Cobb-Douglas value-added production function in logs for firm  $i$  at time  $t$ :

$$y_{it} = \beta_k k_{it} + \beta_l l_{it} + \omega_{it} + \varepsilon_{it} \quad (17)$$

where  $y_{it}$  is the log of value added (outputs minus materials),  $k_{it}$  is the log of capital inputs and  $l_{it}$  log of labour inputs. Both  $\omega_{it}$  and  $\varepsilon_{it}$  represent firm-specific productivity shocks unobserved by the econometrician. They differ in that firm  $i$  observes  $\omega_{it}$ , but not  $\varepsilon_{it}$ , before it chooses inputs  $k_{it}$  and  $l_{it}$ . Common examples of  $\omega_{it}$  include managerial talent, expected rainfall and technological breakthroughs.  $\varepsilon_{it}$  typically contain unforeseen ‘outlier’ events only observed by firm  $i$  after choosing inputs  $k_{it}$  and  $l_{it}$ , or measurement error in  $y_{it}$ .

$\omega_{it}$  influences input choice in profit maximisation as long as returns to scale are not constant (formal proof in [Han \(2021\)](#)). This biases OLS estimates of input elasticities  $\beta_k$  and  $\beta_l$  in *ex ante* unclear directions. For example, OLS may overstate the elasticity of labour inputs while understating that of capital if labour is easier to adjust than capital in the short run, or if automation technology breakthroughs cause firms to replace employees with machinery ([Acemoglu and Restrepo, 2020](#)).

### B.2.1 Olley-Pakes (OP) Method

[Olley and Pakes \(1996\)](#) propose the use of firm investment  $i_{it}$  as a proxy for productivity  $\omega_{it}$ . First, they make two assumptions about the data-generating process to yield two error terms with zero conditional means:

**Assumption OP1** *The firm observes current and past productivity shocks, but not future ones at time  $t$ . Its information set  $I_{it}$  contains  $w_{i0}, \dots, w_{it-1}, w_{it}$ , but not  $w_{it+1}, w_{it+2}, \dots$ . Also,  $\varepsilon_{it}$  satisfies  $E[\varepsilon_{it}|I_{it}] = 0$ .*

**Assumption OP2** *Productivity shocks are first-order Markov.*

$$F(\omega_{it}|I_{it-1}) = F(\omega_{it}|\omega_{it-1})$$

One can thus write  $\omega_{it} = g(\omega_{it-1}) + \xi_{it}$ , where  $E[\xi_{it}|I_{it-1}] = 0$  and  $\xi_{it}$  can be interpreted as innovation. For simplicity, I assume  $g$  is a random walk:

$$\omega_{it} = \omega_{it-1} + \xi_{it} \tag{18}$$

Olley and Pakes (1996) then assume that firm accumulate capital as some unknown function of past capital and investment. I present a perpetual inventory system (in levels) for simplicity:

**Assumption OP3** *The firm accumulates capital by*

$$K_{it} = (1 - \delta)K_{it-1} + I_{it-1}$$

*with depreciation rate  $\delta$ .*

Assumption OP3 implicitly assumes investment takes one period to become capital and implies that current capital  $k_{it}$  was fully determined by the firm in the previous period. This is in contrast to labour  $l_{it}$ :

**Assumption OP4** *Labour is non-dynamic and chosen at time  $t$ .*

Olley and Pakes (1996) then derive the properties of firms' investment demand from profit maximisation under additional assumptions about the DGP. I present these properties as an assumption to keep things simple:

**Assumption OP5** *Firms' investment demand is given by*

$$i_{it} = f_t(k_{it}, \omega_{it})$$

*where  $f_t$  is strictly increasing in  $\omega_{it}$ .*

This requires the investment function to be uniform across firms<sup>24</sup>. Strict monotonicity requires firms with the same  $i_{it}$  and  $k_{it}$  to share the same  $\omega_{it}$ . Assumption OP5 allows  $f_t$  to

---

<sup>24</sup>The investment function  $f_t$  can vary across time  $t$ . In my estimations I further allow the investment functions to vary across 3-digit industries.

be inverted and substituted into the production function:

$$\omega_{it} = f_t^{-1}(k_{it}, i_{it}) \quad (19)$$

$$y_{it} = \beta_k k_{it} + \beta_l l_{it} + f_t^{-1}(k_{it}, i_{it}) + \varepsilon_{it} \quad (20)$$

$$= \beta_l l_{it} + \Phi_t(k_{it}, i_{it}) + \varepsilon_{it} \quad (21)$$

To avoid additional assumptions about the investment process, one will have to treat  $f_t$  non-parametrically. Thus,  $\beta_k$  cannot be identified, and are combined into a composite function  $\Phi_t(k_{it}, i_{it}) = \beta_k k_{it} + f_t^{-1}(k_{it}, i_{it})$ .

Equation 21 writes the production function in terms of observables and  $\varepsilon_{it}$  where  $E[\varepsilon_{it}|I_{it}] = 0$  by Assumption OP1. One can thus use GMM to yield consistent “first stage” estimates  $\hat{\beta}_l$  and  $\hat{\Phi}_t(k_{it}, i_{it})$  using the moment condition

$$E[\varepsilon_{it}|I_{it}] = E[y_{it} - \beta_l l_{it} - \Phi_t(k_{it}, i_{it})|I_{it}] = 0 \quad (22)$$

Most studies use a third-degree polynomial to approximate  $\Phi_t(i_{it}, k_{it})$  and run OLS.

To identify  $\beta_l$ , I substitute  $\omega_{it} = \omega_{it-1} + \xi_{it}$  into the production function:

$$\omega_{it} = \omega_{it-1} + \xi_{it} \quad (23)$$

$$y_{it} = \beta_k k_{it} + \beta_l l_{it} + \omega_{it-1} + \xi_{it} + \varepsilon_{it} \quad (24)$$

$$= \beta_k k_{it} + \beta_l l_{it} + (\Phi_{t-1}(k_{it-1}, i_{it-1}) - \beta_k k_{it-1}) + \xi_{it} + \varepsilon_{it} \quad (25)$$

$E[\varepsilon_{it}|I_{it}] = E[\varepsilon_{it}|I_{it-1}] = 0$  by Assumption OP1.  $E[\xi_{it}|I_{it-1}] = 0$  by Assumption OP2. One can thus derive the “second stage” moment condition

$$E[\xi_{it} + \varepsilon_{it}|I_{it-1}] = E[y_{it} - \beta_k k_{it} - \beta_l l_{it} - (\Phi_{t-1}(k_{it-1}, i_{it-1}) - \beta_k k_{it-1})|I_{it-1}] = 0 \quad (26)$$

and “plug in”  $\hat{\beta}_l$  and  $\hat{\Phi}_{t-1}(k_{it-1}, i_{it-1})$  to yield a consistent estimate of  $\beta_k$ .

### B.2.2 Levinsohn-Petrin (LP) Method

Levinsohn and Petrin (2003) note that Assumption OP5 is unlikely to hold in reality. Many firms, often with same  $k_{it}$  but likely different levels of  $\omega_{it}$ , report  $i_{it} = 0$  in the data. While the assumption can be modified to work for just  $i_{it} \neq 0$ , these firms must be dropped during estimation, making the OP method prone to bias and efficiency loss. Furthermore, firm-level (unobserved) heterogeneity in capital adjustment costs or investment prices threatens the one-to-one relationship between  $i_{it}$  and  $\omega_{it}$  (conditional on  $k_{it}$ ) required for  $f_t$  to be invertible.

Thus, [Levinsohn and Petrin \(2003\)](#) propose the use of intermediate inputs  $m_{it}$ , which is often much better reported in the data, as a proxy for productivity instead of investment. They keep OP's assumptions, but replace Assumption [OP5](#) with

**Assumption LP5** *Firms' intermediate input demand is given by*

$$m_{it} = f_t(k_{it}, \omega_{it})$$

where  $f_t$  is strictly increasing in  $\omega_{it}$ .

The two-step estimation procedure is the same.

### B.2.3 Akerberg-Caves-Frazer (ACF) Correction

[Akerberg et al. \(2015\)](#) notice that OP/LP fails when labour  $l_{it}$  is chosen purely as a function of productivity and the state and proxy variables ( $\omega_{it}$ ,  $k_{it}$  and  $i_{it}$ ). If all of the variation in  $l_{it}$  is caused by variation in  $i_{it}$ ,  $k_{it}$  or  $\omega_{it}$ , then there is no unique variation in  $l_{it}$  for  $\beta_l$  to be separately identified. For example, consider a non-dynamic labour demand

$$l_{it} = h_t(k_{it}, i_{it}, \omega_{it}) \tag{27}$$

$$= h_t(k_{it}, i_{it}, f_t^{-1}(k_{it}, i_{it})) \tag{28}$$

Then Equation [22](#), the first stage moment in OP (or similarly LP) becomes

$$E[\varepsilon_{it}|I_{it}] = E[y_{it} - \beta_l l_{it} - \Phi_t(k_{it}, i_{it})|I_{it}] \tag{29}$$

$$= E[y_{it} - \beta_l h_t(i_{it}, k_{it}, f_t^{-1}(k_{it}, i_{it})) - \Phi_t(k_{it}, i_{it})|I_{it}] = 0 \tag{30}$$

Since  $l_{it}$  is not even present in the moment condition, its elasticity  $\beta_l$  cannot be separately identified from the first stage.

One can do GMM on the first and second stages simultaneously; the second stage will identify  $\beta_l$  if the first stage does not ([Wooldridge, 2009](#)). While this addresses the nonidentification issue, it is computationally intensive. I turn to the work of [Akerberg et al. \(2015\)](#), who allow labour to be dynamic and include it in the proxy variable decision function. ACF replace Assumptions [OP4](#) and [OP5](#) with

**Assumption ACF4** *Labour  $l_{it}$  is dynamic and possibly chosen before time  $t$ .*

**Assumption ACF5** *The demand for the proxy variable (here investment  $i_{it}$ ) is given by*

$$i_{it} = \tilde{f}_t(k_{it}, \omega_{it}, l_{it})$$

where  $\tilde{f}_t$  is strictly increasing in  $\omega_{it}$ .

Then the first stage moment becomes

$$\begin{aligned} E[\varepsilon_{it}|I_{it}] &= E\left[y_{it} - \beta_k k_{it} - \beta_l l_{it} - \tilde{f}_t^{-1}(k_{it}, i_{it}, l_{it})|I_{it}\right] \\ &= E\left[y_{it} - \tilde{\Phi}_t(k_{it}, i_{it}, l_{it})|I_{it}\right] = 0 \end{aligned}$$

And the second stage moment becomes

$$\begin{aligned} E[\xi_{it} + \varepsilon_{it}|I_{it-1}] &= E[y_{it} - \beta_k k_{it} - \beta_l l_{it} \\ &\quad - (\tilde{\Phi}_{t-1}(k_{it-1}, i_{it-1}, l_{it-1}) - \beta_k k_{it-1} - \beta_l l_{it-1})|I_{it-1}] = 0 \end{aligned}$$

This allows for  $\beta_l$  to be identified when labour is dynamic. It also allows firm-level unobserved heterogeneity in labour costs to enter  $\tilde{f}_t$  through  $l_{it}$ .

## C Additional Details on Data and Definitions

### C.1 Index of Variables

Variable	Definition	Construction	Source(s)
$Priv_{ct}$	% of city $c$ 's total industrial workforce in non-state firms in year $t$	$\frac{N(\text{Workers in Industry Non-SOEs})_{ct}}{N(\text{Workers in Industry})_{ct}}$	AIS
$Priv_{nt}$	% of CIC 4-digit industry $n$ 's total workforce in non-state firms in year $t$	$\frac{N(\text{Workers in Non-SOEs})_{nt}}{N(\text{Workers})_{nt}}$	AIS
$S_{ct}$	% of city $c$ 's total workforce in the industrial sector, fixed to first-observed values for each $c$	$\frac{N(\text{Workers in Industry})_{ct}}{N(\text{Workers})_{ct}}$	CEIC
$s_{cnt}$	% of city $c$ 's total workforce in industry $n$ , fixed to first-observed values for each $(c, n)$ . Thus, $\sum_n s_{cnt} = S_{ct}$ by construct	$\frac{N(\text{Workers})_{cnt}}{N(\text{Workers in Industry})_{ct}} \times S_{ct}$	AIS, CEIC
$\widehat{\Delta Priv_{cnt}}$	Predicted increase in % of city $c$ 's total industrial workforce in non-state firms in year $t$	$\sum_n s_{cnt} \times \Delta Priv_{nt}$	AIS, CEIC
$s_{nt}$	Average exposure of industry $n$ to city characteristics in year $t$	$\sum_c e_{ct} s_{cnt}$	AIS, CEIC
$e_{ct}$	Number of firms in city $c$ in year $t$ , divided by total number of firms in data (thus, $\sum_{c,t} e_{ct} = 1$ )	$\frac{N(\text{Firms})_{ct}}{\sum_{c,t} N(\text{Firms})_{ct}}$	AIS

### C.2 Nearest Neighbour Imputation: An Explanation

This section explains nearest neighbour imputation I implemented in Stata. Consider the univariate missing case for simplicity. Let  $\mathbf{x}$  be an  $n \times 1$  vector and  $\mathbf{Z}$  an  $n \times k$  matrix of  $k$



variables

$$\mathbf{x} = \begin{bmatrix} x_1 \\ \vdots \\ x_n \end{bmatrix}, \mathbf{Z} = \begin{bmatrix} z_{11} \dots z_{1k} \\ \vdots \\ z_{n1} \dots z_{nk} \end{bmatrix}$$

$\mathbf{x}$  contains missing values.  $\mathbf{Z}$  does not contain missing values, and may include a constant (a column of 1s). Assume that the true relationship is linear

$$\mathbf{x} = \mathbf{Z}\boldsymbol{\beta} + \boldsymbol{\varepsilon}$$

where  $Var(\varepsilon_i) = \sigma^2 \forall i = 1, \dots, n$ .

I first fit a linear regression on the set of complete cases—observations that are not missing the entry for  $x$ —to obtain estimates  $(\hat{\boldsymbol{\beta}}, \hat{\sigma}^2)$ .

Using the estimates, I derive a predictive distribution for the true  $\boldsymbol{\beta}$  and  $\sigma^2$  using the “conventional noninformative improper prior”  $Pr(\boldsymbol{\beta}, \sigma^2) \propto 1/\sigma^2$ . This makes the process an inherently Bayesian one.

I then make one random draw  $(\boldsymbol{\beta}_*, \sigma_*^2)$  from the distribution (hence “stochastic” imputation), and make predictions  $\hat{x}_1, \dots, \hat{x}_n$  using  $(\boldsymbol{\beta}_*, \sigma_*^2)$  and  $\mathbf{Z}$ .

For each observation  $i$  that is missing  $x_i$ , I impute  $x_i = x_j$  where  $j$  is the nearest non-missing neighbour with the closest predicted  $x$ . Formally,  $j = \operatorname{argmin}_k |\hat{x}_i - \hat{x}_k|$  such that  $x_k$  is not missing.