Regional Privatisation and Firm Performance: Evidence from China*

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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.

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Contents

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[1].

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[2]. 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

[1]Source: CEIC Data.

[2]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[3]. 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)[4]. 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[5].

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

[3] Tobacco is arguably an exception. The state strictly controls its distribution as it is regarded as an addictive substance.

[4] 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.

[5] 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[6]. Each firm belongs to a prefecture-level division, or "city"[7], 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[8]. 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.

[6]The CIC system changed in 2003. I use Brandt et al.'s (2012)'s crosswalk to harmonise classifications.

[7]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.

[8]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[9], 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)

[9] 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 β in the *city-level* structural equation

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

β captures the effect of privatising city c on the average performance of its firms in year t. Also included are city fixed effects α_c and year fixed effects λ_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 privatisating 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.

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

I prefer first-differencing to controlling for city fixed effects due to possible serial correlation in u_{ct}[10]. However, first differencing will induce negative serial correlation in Δu_{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[11].

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

[10] 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.

[11] Enforcement may still vary across cities. That would be problematic.

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

The final model is

$$\text{First Stage:} \qquad \Delta Priv_{ct} = \lambda_t + \delta \widehat{\Delta Priv}_{ct} + \Delta \mathbb{X}'_{ct}\gamma + \Delta v_{ct} \tag{3}$$

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

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

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

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 productivity (value added per worker) and per worker compensation (annual wage and welfare payments)[13]. 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[14].

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.

[13] 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).

[14] 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

$$\Delta Priv_{ct} = \sum_n s_{cnt} \times \Delta Priv_{cnt} \tag{6}$$

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

$$\Delta Priv_{cnt} = \Delta Priv_{nt} + \Delta \widetilde{Priv}_{cnt} \tag{7}$$

where

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

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 $\Delta Priv_{nt}$. $\Delta \widetilde{Priv}_{cnt}$ 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. $\Delta \widetilde{Priv}_{cnt} = \Delta Priv_{cnt} - \Delta Priv_{nt}$ 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}.

$$\widehat{\Delta Priv}_{ct} = \sum_{n} s_{cnt} \times \Delta Priv_{nt} \tag{9}$$

$\widehat{\Delta Priv}_{ct}$ essentially predicts $\Delta Priv_{ct}$ using each city's industrial specialisation and national trends in each industry's privatisation[15]. 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

[15] 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:

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

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* Δu_{ct} that determine firm performance. For any given industry n, the characteristic of a city Δu_{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 Δq_{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

$$\Delta Priv_{nt} = \lambda_t + \pi\overline{\Delta q}_{nt} + \epsilon_{nt} \qquad (11)$$

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 accessibilty, 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)[16].

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 nationwide 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[17]. The effective sample size (inverse of Herfindahl–Hirschman Index of average shares, $1/\sum_{n,t} s_{nt}^2$)

[16] 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.

[17] 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[18], 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

[18]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 *Zhua 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[19] and take logs:

$$Y_{it} = K_{it}^{\beta_k} L_{it}^{\beta_l} X_{it} \tag{12}$$

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

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 ω_{it} and ε_{it}, where $\omega_{it} + \varepsilon_{it} = logX_{it}$. Both represent firm-specific productivity shocks that are not observed by the econometrician. They differ in that firm i observes ω_{it}, but not ε_{it}, before it chooses inputs k_{it} and l_{it}. Common examples of ω_{it} include managerial talent and expected rainfall. ε_{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 ω_{it}, its input choice is likely a function of ω_{it}. This biases OLS estimates of input elasticities β_k and β_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).

[19] 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

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

where f_t is strictly increasing in ω_{it}. Strict monotonicity allows for ω_{it} to be "inverted out"

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

and substituted into the production function to obtain an equation consisting of purely observables and white noise ε_{it}. With additional assumptions on the evolution process of ω_{it}, one can obtain consistent estimates of β_k and β_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 ω_{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. Ackerberg et al. (2015) relax this assumption by adding l_{it} to the proxy variable function (Equation 14). This allows for β_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 hetereogeneity 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

$I_{it} = K_{it+1} - (1-\delta)K_{it}$[20], 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 β, 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 β remain consistent.

4.5.4 Results on Input Elasticities

I separately estimate input elasticities β_k and β_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 β_k and show that a positive correlation between capital and labour generates a downward bias. Both are probable in the Chinese firms setting.

[20]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 θ_{ct} into a a weighted average of firm productivities θ_{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[21].

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-

[21] The null hypothesis of the AR test $\beta = 0$ relates to the coefficient of interest β, not the first stage relationship δ. 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[22]. 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 heteorogeneity 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.

[22]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[23] 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),

[23] 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	2,137,231	81	770.4	0.0	186,000
Intermediate Input	2,137,231	60	582.4	0.0	173,000
Revenue	2,137,206	80	798.1	-101.6	195,988
Net Value of Fixed Assets	2,137,231	38	540.3	0.0	156,000
Liabilities	2,137,231	51	473.0	0.0	79,310
Average Compensation	2,137,231	.014	0.0	0.0	0
Firm Age	2,137,231	12	13.1	0.0	190
Employment	2,137,231	291	1,347.3	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	154,515	43	33	41	33	44	0.008	16		0.04
1999	151,199	47	35	45	36	47	0.009	16		0.04
2000	151,715	55	42	54	38	49	0.010	16		0.05
2001	161,801	58	43	57	38	48	0.011	15		0.05
2002	172,401	63	47	62	38	48	0.011	14		0.06
2003	189,664	74	55	75	39	52	0.012	13		0.06
2004	264,443	75	56	74	34	46	0.014	10		0.07
2005	265,718	94	70	92	33	52	0.016	11		0.08
2006	295,089	106	79	105	41	56	0.018	10		0.09
2007	330,686	121	91	120	44	61	0.020	10		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) Shock $\boldsymbol{\Delta Priv_{nt}}$ Δ Share of Industry Non-State Employment		(2) Endogenous Treatment $\boldsymbol{\Delta Priv_{ct}}$ Δ Share of City Non-State Employment	
Accessibility				
No of Public Buses per Capita	0.0028	(0.033)	-0.0013	(0.027)
Highway: Freight Traffic	-0.019	(0.024)	0.033	(0.037)
Highway: Length of Highway	0.028	(0.024)	0.0064	(0.022)
Highway: Passenger Traffic	0.039*	(0.022)	0.062	(0.058)
Highway: Passenger Traffic: Public Transport	-0.012	(0.023)	0.031	(0.032)
Star-Rated Hotel: Number of Hotel	-0.0016	(0.025)	0.055	(0.034)
No of Motor Vehicles per Capita	0.0085	(0.018)	-0.011	(0.019)
Post and Telecom: Business Volume	0.0031	(0.026)	-0.029	(0.019)
No of Rental Vehicles per Capita	-0.026	(0.029)	0.0032	(0.021)
Railway: Freight Traffic	-0.045**	(0.022)	0.00045	(0.025)
Area of Paved Road	-0.024	(0.023)	-0.032***	(0.011)
Railway: Passenger Traffic	-0.013	(0.023)	0.0061	(0.026)
Economy				
Commodity Building Sold	-0.025	(0.027)	-0.018	(0.014)
Consumption Expenditure per Capita: Rural	-0.025	(0.037)	0.0091	(0.027)
Consumption Expenditure per Capita: Rural: Food	-0.033	(0.040)	0.031	(0.030)
Consumption Expenditure per Capita: Urban	-0.0090	(0.036)	-0.033*	(0.019)
Consumption Expenditure per Capita: Urban: Food	-0.021	(0.040)	-0.016	(0.025)
Deposit	-0.013	(0.023)	0.0080	(0.023)
Deposit by Enterprise	-0.014	(0.022)	-0.0077	(0.017)
Saving Deposit	-0.015	(0.023)	0.015	(0.021)
Saving Deposit: Demand	-0.0036	(0.027)	-0.0022	(0.026)
Saving Deposit: Time	-0.019	(0.021)	-0.012	(0.018)
Disposable Income per Capita: Urban	-0.0065	(0.036)	-0.027	(0.019)
Floor Space Sold: Commodity	-0.011	(0.025)	-0.0067	(0.027)
GDP	-0.013	(0.025)	-0.013	(0.026)
GDP: per Capita	-0.075	(0.054)	-0.040	(0.026)
GDP: Primary Industry	0.043*	(0.024)	0.080***	(0.026)
GDP: Secondary Industry	-0.020	(0.025)	-0.039*	(0.021)
GDP: Tertiary Industry	-0.012	(0.024)	-0.0085	(0.028)
Gross Industrial Output	-0.027	(0.025)	-0.045**	(0.018)
GIO: Domestic Funded Enterprise	-0.035	(0.022)	-0.019	(0.021)
GIO: Foreign Funded Enterprise	-0.030	(0.026)	-0.045**	(0.020)
GIO: HMT Funded Enterprise	0.0021	(0.037)	-0.063***	(0.018)
Loan	-0.015	(0.024)	0.018	(0.025)
Loan to Agricultural Sector	0.045	(0.041)	0.015	(0.017)
Loan to Commercial Sector	0.0024	(0.026)	0.034*	(0.019)
Loan to Industrial Sector	-0.024	(0.024)	-0.0083	(0.022)
No of Enterprise: Industrial	-0.031	(0.022)	0.0045	(0.023)
No of Enterprise: Industrial: DF	-0.014	(0.051)	-0.027	(0.025)
No of Enterprise: Industrial: FF	-0.034	(0.023)	-0.047**	(0.022)
No of Enterprise: Industrial: HMT	-0.025	(0.030)	-0.067***	(0.020)
Property Price	-0.018	(0.028)	-0.039*	(0.022)
Real Estate Investment	-0.012	(0.024)	0.0078	(0.034)
Real Estate Investment: Residential	-0.014	(0.025)	0.0064	(0.032)
Wholesale & Retail Sales	-0.018	(0.030)	-0.024	(0.018)
Wholesale & Retail: No of Enterprise	0.026	(0.030)	-0.034*	(0.018)
Retail Sales of Consumer Goods	-0.0039	(0.024)	0.010	(0.032)
Education				
No of School: Higher Institution	-0.020	(0.023)	0.023	(0.039)
No of School: Primary School	0.035*	(0.019)	-0.023	(0.030)
No of School: Secondary School	0.017	(0.025)	0.0000035	(0.026)
Student-Teacher Ratio: Higher Inst	0.026*	(0.014)	-0.0055	(0.022)
Student-Teacher Ratio: Primary School	0.037	(0.029)	0.0098	(0.021)

Student-Teacher Ratio: Secondary School	0.0055	(0.021)	-0.035***	(0.0023)
Environment				
% Green Space	0.044	(0.032)	-0.043*	(0.026)
Sulphur Dioxide Emission: Industry	0.0052	(0.028)	0.025	(0.027)
Ratio of Industrial Solid Waste Utilized	0.014	(0.022)	-0.0016	(0.018)
Treatment Rate of Living Waste	-0.0083	(0.022)	-0.022	(0.023)
Treatment Rate of Living Waste Water	-0.033	(0.024)	-0.050*	(0.026)
Waste Water Discharge: Industry	-0.0091	(0.025)	0.034	(0.036)
Waste Water Discharge: Industry: Meet Discharge Standard	-0.043	(0.028)	0.020	(0.029)
Health				
No of Beds in Hospitals per Capita	-0.035	(0.022)	-0.012	(0.023)
No of Hospitals per Capita	0.013	(0.013)	0.012	(0.025)
Infrastructure				
% Population Subscribed to Broadband	0.035	(0.027)	0.012	(0.018)
Developed Area of City Construction	-0.020	(0.022)	0.034**	(0.015)
Floor Area of Residential Building per Capita: Rural	0.0084	(0.032)	-0.00096	(0.022)
Floor Area of Residential Building per Capita: Urban	0.028	(0.029)	0.025	(0.021)
Land Area of Administrative Zone	0.029**	(0.015)	0.00029	(0.015)
% Population with Landline	-0.016	(0.030)	-0.011	(0.027)
Transport: Freight Traffic	-0.023	(0.024)	0.027	(0.038)
% Population with Mobile Phone	-0.0029	(0.027)	-0.0053	(0.022)
Transport: Passenger Traffic	0.038*	(0.023)	0.055	(0.055)
% Employed in Primary Industry	0.036	(0.026)	0.023	(0.035)
No of Employee: Tertiary Industry	0.0039	(0.025)	-0.023	(0.020)
% Self Employed	0.042	(0.033)	0.020	(0.026)
% Unemployed	0.033	(0.028)	-0.064	(0.051)
Politics				
Politicians Average Yrs of Schooling	0.0075	(0.026)	-0.029*	(0.017)
Politicians Average Career Incentive	0.019	(0.024)	0.025	(0.025)
% of Corrupt Politicians	0.045	(0.031)	0.040**	(0.019)
Population				
Population: Census	0.032	(0.024)	0.046	(0.031)
% Male Population	0.073	(0.049)	-0.015	(0.019)
Average Household Size	0.015	(0.047)	0.038	(0.027)
% Urban Population	-0.0021	(0.032)	-0.013	(0.019)
% Population with Access to Gas	-0.067***	(0.024)	-0.027	(0.018)
Public Finance				
Govt Expenditure	-0.015	(0.025)	-0.013	(0.016)
Govt Expenditure: Education	-0.016	(0.024)	-0.053***	(0.015)
Govt Expenditure: Science	0.024	(0.021)	-0.024	(0.020)
Govt Expenditure: Social Security	0.068**	(0.034)	-0.015	(0.016)
Govt Revenue	-0.016	(0.025)	-0.017	(0.015)
Govt Revenue: Tax	-0.019	(0.024)	-0.021	(0.019)
Govt Revenue: Tax: Enterprises Income	-0.020	(0.023)	-0.011	(0.021)
Govt Revenue: Tax: Individual Income	-0.015	(0.022)	-0.0090	(0.012)
Govt Revenue: Tax: Operation	-0.015	(0.025)	-0.0023	(0.016)
Govt Revenue: Tax: Value Added	-0.023	(0.025)	-0.047***	(0.013)
Trade and Tourism				
Export	0.011	(0.037)	-0.024	(0.015)
Fixed Asset Investment	-0.018	(0.024)	0.0054	(0.026)
FDI: Contract Value	-0.025	(0.023)	-0.0087	(0.018)
FDI: No of Contract	-0.0077	(0.025)	0.017	(0.019)
FDI: Utilized	-0.022	(0.020)	0.021	(0.020)
Import	0.00024	(0.028)	-0.038***	(0.012)
Tourism Revenue: Domestic	-0.021	(0.021)	0.0020	(0.020)
Tourism Revenue: Foreign Currency	-0.0044	(0.024)	-0.021	(0.019)
Visitor Arrival	0.029	(0.029)	-0.029**	(0.013)
Domestic Tourist	-0.014	(0.022)	0.020	(0.030)
Trade Balance	0.0087	(0.022)	0.014	(0.017)
Energy and Utilities				
Coal & Natural Gas Supply	-0.024	(0.025)	-0.0077	(0.018)

Coal & Natural Gas Supply: Residential	-0.016	(0.022)	-0.015	(0.028)
Electricity Consumption	-0.022	(0.025)	-0.029	(0.026)
Electricity Consumption: Industry	-0.013	(0.023)	-0.024	(0.022)
Electricity Consumption per Capita	0.013	(0.023)	-0.016	(0.023)
Electricity Consumption: Residential	-0.023	(0.015)	0.021	(0.038)
LPG Supply	0.0056	(0.033)	-0.031*	(0.017)
LPG Supply: Non-Residential	0.013	(0.030)	-0.032*	(0.017)
Water Supply	-0.017	(0.025)	0.029	(0.047)
Water Supply: Residential	-0.015	(0.029)	0.013	(0.033)
No of Units	800		295	
No of Units×Years	7,491		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

	Shock $\boldsymbol{\Delta Priv_{nt}}$ Δ Share of Industry Non-State Employment	
Highway: Passenger Traffic	0.039*	(0.022)
Railway: Freight Traffic	-0.045**	(0.022)
GDP: Primary Industry	0.043*	(0.024)
No of School: Primary School	0.035*	(0.019)
Student-Teacher Ratio: Higher Inst	0.026*	(0.014)
Land Area of Administrative Zone	0.029**	(0.015)
Transport: Passenger Traffic	0.038*	(0.023)
% Population with Access to Gas	-0.067***	(0.024)
Govt Expenditure: Social Security	0.068**	(0.034)
No of Industries	800	
No of Industries×Years	7,491	
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

Panel A. Industry-Level Average Exposure s_{nt}	
Mean	0.0001
Standard Deviation	0.0003
Maximum	0.007
Panel B. Shocks $\Delta Priv_{nt}$	
Mean	0.03
Standard Deviation	0.09
Interquartile Range	0.06
Panel C. Shock Sample Size	
Number of Industries	800
Number of Industry-Year Shocks	7491
Effective Sample Size ($1/\sum_{n,t} s_{nt}^2$)	984.6

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	0.009	(.)
CIC3	0.004	(.)
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^{***}	0.461^{***}
Olley-Pakes	0.353^{***}	0.445^{***}
Levinsohn-Petrin	0.334^{***}	0.482^{***}

Note: This table reports the average $\hat{\beta}$ values of each estimation method on 3-digit industry subsets. The Ackerberg-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

	ΔReturn On Assets			ΔLog TFP			ΔEntry
	(1) All Firms	(2) Private	(3) SOE	(4) All Firms	(5) Private	(6) SOE	(7) All Firms
Panel A. OLS							
Δ Share of Non-State Employment	0.031*	-0.0061	-0.030	1.17**	-0.13	-0.55	0.24***
	(0.017)	(0.019)	(0.023)	(0.46)	(0.21)	(1.09)	(0.032)
(2)≠(3)/(5)≠(6): P-value			0.34			0.67	
R^2	0.08	0.07	0.09	0.04	0.04	0.04	0.26
Panel B. Reduced Form							
Predicted Δ Share of Non-State Employment	0.061	0.034	-0.11	0.72	-0.44	-4.20	0.53***
	(0.070)	(0.073)	(0.13)	(0.76)	(0.77)	(4.16)	(0.13)
R^2	0.08	0.07	0.09	0.03	0.04	0.04	0.24
Panel C. 2SLS							
Δ Share of Non-State Employment	0.045	0.025	-0.078	0.53	-0.33	-3.10	0.39***
	(0.052)	(0.054)	(0.093)	(0.56)	(0.57)	(3.03)	(0.091)
(2)≠(3)/(5)≠(6): P-value			0.28			0.39	
Kleibergen Wald rk F	153	153	153	153	153	153	153
AR P-value	0.38	0.63	0.40	0.34	0.56	0.31	0.00
No of Cities	293	293	293	293	293	293	293
No of Cities×Years	2606	2606	2606	2606	2606	2606	2606

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

	ΔLabour OP Gap	**ΔLog Labour Productivity**			**ΔPer Worker Compensation**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All Firms	All Firms	Private	SOE	All Firms	Private	SOE
Panel A. OLS							
Δ Share of Non-State	-0.37**	0.031	-0.085	-0.084	-0.0030	0.0023	-0.0026
Employment	(0.14)	(0.12)	(0.14)	(0.21)	(0.0020)	(0.0028)	(0.0027)
(2)≠(3)/(5)≠(6): P-value				0.91			0.23
R^2	0.08	0.04	0.05	0.05	0.09	0.10	0.04
Panel B. Reduced Form							
Predicted Δ Share of	-0.31	2.38***	2.24***	2.44**	-0.00072	0.0034	0.016
Non-State Employment	(0.58)	(0.52)	(0.45)	(1.13)	(0.0072)	(0.0078)	(0.018)
R^2	0.07	0.06	0.06	0.05	0.09	0.10	0.04
Panel C. 2SLS							
Δ Share of Non-State	-0.23	1.75***	1.65***	1.79**	-0.00053	0.0025	0.012
Employment	(0.42)	(0.40)	(0.34)	(0.86)	(0.0053)	(0.0057)	(0.013)
(2)≠(3)/(5)≠(6): P-value				0.84			0.56
Kleibergen Wald rk F	153	153	153	153	153	153	153
AR P-value	0.59	0.00	0.00	0.03	0.92	0.66	0.38
No of Cities	293	293	293	293	293	293	293
No of Cities×Years	2606	2606	2606	2606	2606	2606	2606

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

	ΔCapital OP Gap	**ΔLog Capital Productivity**			**ΔInvestment**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All Firms	All Firms	Private	SOE	All Firms	Private	SOE
Panel A. OLS							
Δ Share of Non-State	-0.27**	0.26***	-0.40***	-0.23	-5.40	12.2***	-21.1***
Employment	(0.12)	(0.10)	(0.11)	(0.21)	(3.47)	(4.14)	(6.80)
(3)≠(4)/(6)≠(7): P-value				0.48			0.00
R^2	0.04	0.10	0.06	0.06	0.10	0.07	0.07
Panel B. Reduced Form							
Predicted Δ Share of	0.17	0.94*	-0.87	0.58	4.63	19.0	39.2
Non-State Employment	(0.49)	(0.49)	(0.53)	(1.56)	(13.9)	(13.2)	(33.1)
R^2	0.04	0.10	0.06	0.06	0.10	0.06	0.06
Panel C. 2SLS							
Δ Share of Non-State	0.12	0.69*	-0.64*	0.43	3.41	14.0	28.9
Employment	(0.37)	(0.37)	(0.38)	(1.16)	(10.3)	(9.65)	(24.8)
(3)≠(4)/(6)≠(7): P-value				0.34			0.55
Kleibergen Wald rk F	153	153	153	153	153	153	153
AR P-value	0.74	0.05	0.10	0.71	0.74	0.15	0.23
No of Cities	293	293	293	293	293	293	293
No of Cities×Years	2606	2606	2606	2606	2606	2606	2606

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 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

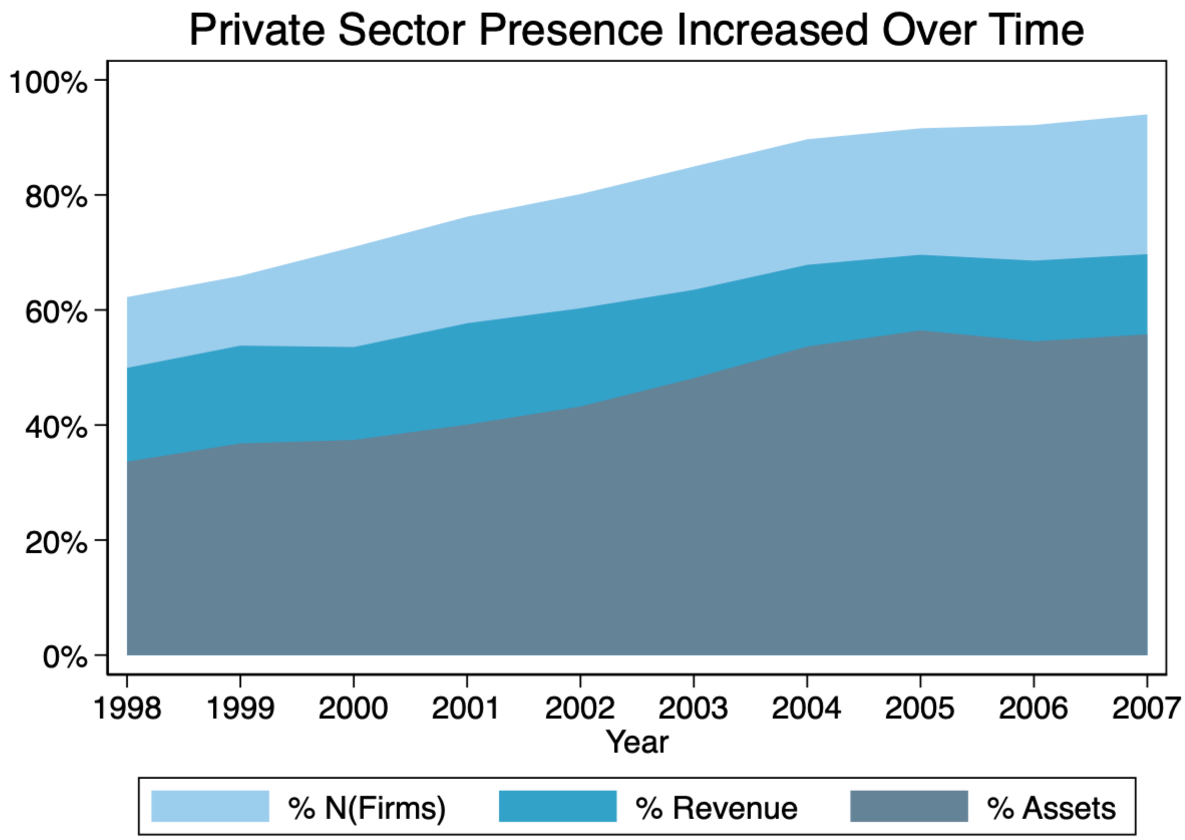
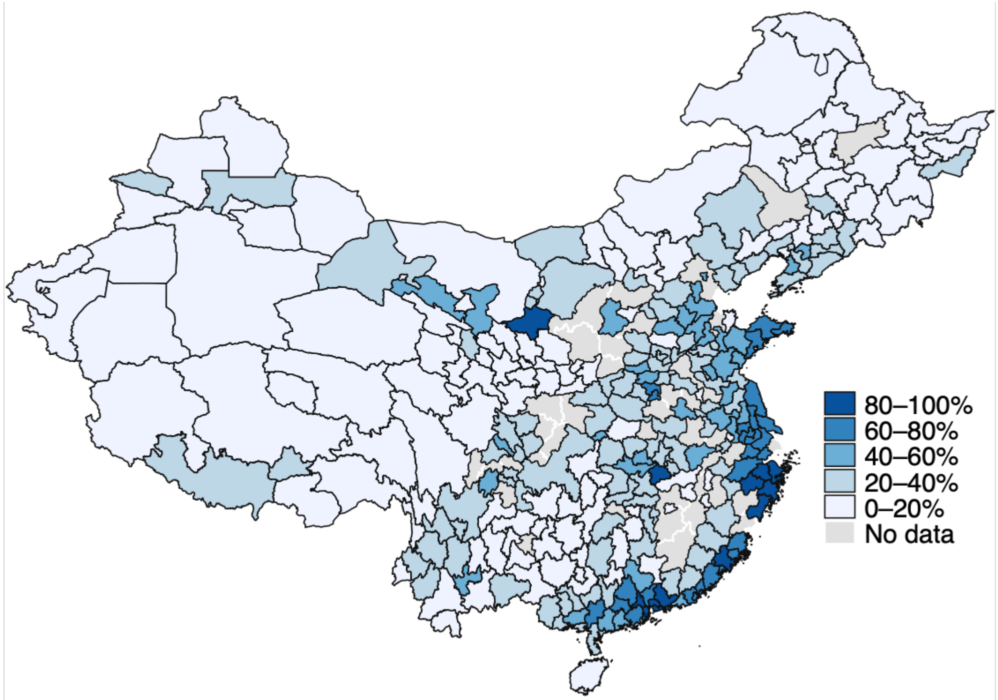
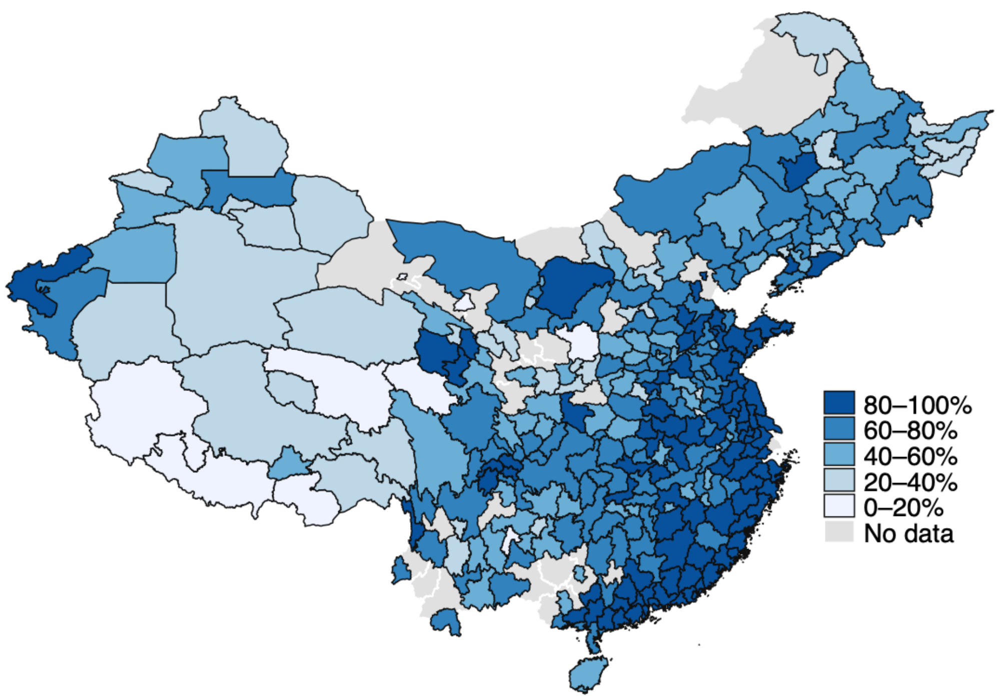


Figure 1: Non-State Sector Presence Increased Over Time
NOTE: This graph documents the change in private firms' share of the industrial sector's total assets, revenue and number of firms during the sample period. Source: Annual Industrial Survey.



(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.

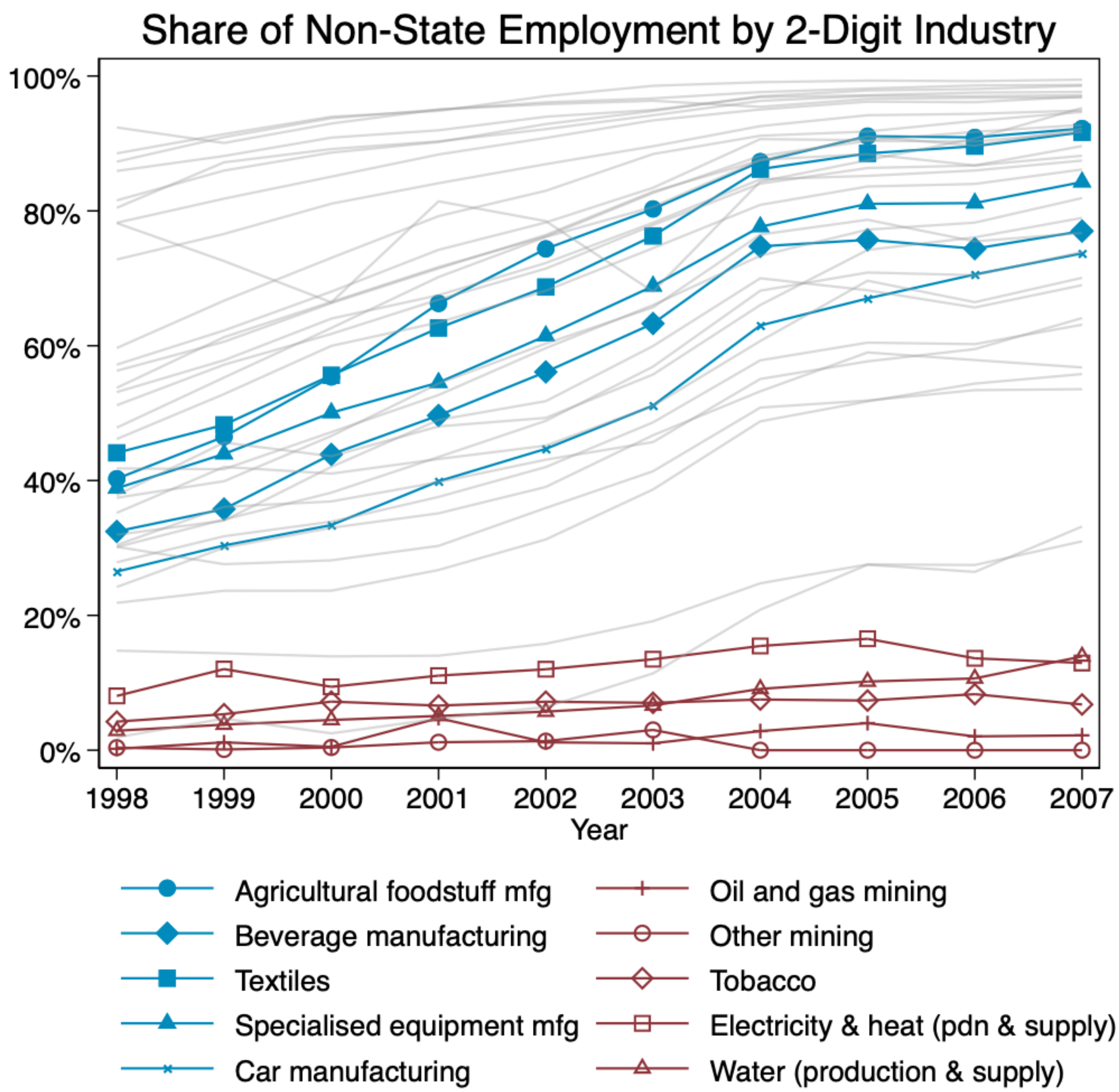


Figure 3: Share of Non-State Employment by 2-Digit Industry
NOTE: Blue lines indicate the five most privatised industries by net increase in share of non-state employment between 1998 and 2007. Red lines indicate the five least privatised industries. Source: Annual Industrial Survey.

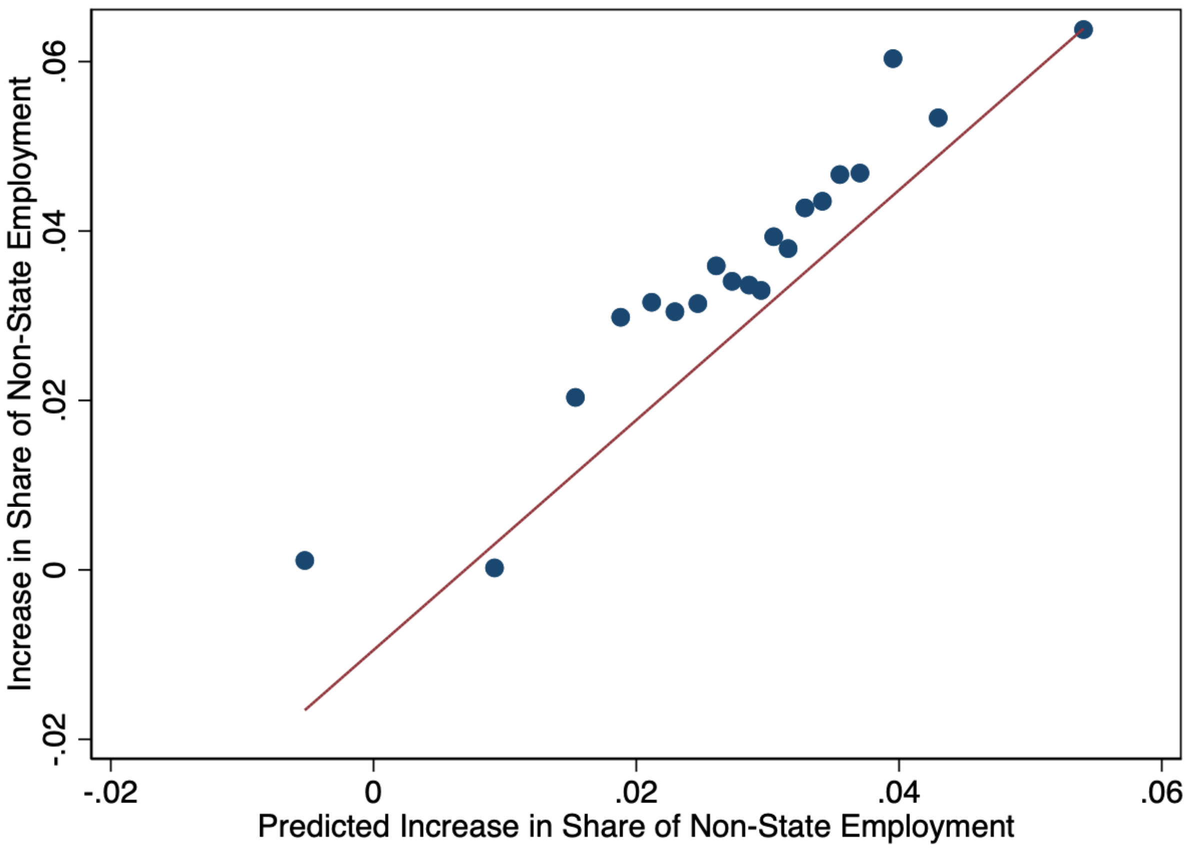


Figure 4: First Stage: Instrument Strongly Predicts Treatment

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.

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A Additional Tables

Table A1: Panel on City Characteristics: Summary Statistics

	Mean	SD	Min	Max
Accessibility				
No of Public Buses per Capita	0.0002	0.0003	0.000001	0
Highway: Freight Traffic	39.6	42.9	0.2	420
Highway: Length of Highway	5889.5	5423.4	212	104,705
Highway: Passenger Traffic	52.4	59.2	0.7	733
Highway: Passenger Traffic: Public Transport	129.3	348.1	0	4,498
Star-Rated Hotel: Number of Hotel	31.4	47.6	1	806
No of Motor Vehicles per Capita	0.03	0.04	0.000	1
Post and Telecom: Business Volume	1748.8	3594.9	46.8	63,492
No of Rental Vehicles per Capita	0.0008	0.001	0.00000	0
Railway: Freight Traffic	9.2	13.7	0.0	147
Area of Paved Road	8.5	14.3	0.0	215
Railway: Passenger Traffic	4.2	7.1	0	101
Economy				
Commodity Building Sold	4334.6	14684.0	10	308,935
Consumption Expenditure per Capita: Rural	2309.3	1211.5	707	10,783
Consumption Expenditure per Capita: Rural: Food	1016.8	439.1	249	4,260
Consumption Expenditure per Capita: Urban	6128.2	2346.5	1378	24,376
Consumption Expenditure per Capita: Urban: Food	2275.6	857.4	1065	7,778
Deposit	74174.5	198744.6	1840	3,770,025
Deposit by Enterprise	24095.1	92823.0	570	2,042,104
Saving Deposit	38890.2	71065.6	500.8	1,181,909
Saving Deposit: Demand	12408.0	23357.2	160	346,711
Saving Deposit: Time	22728.8	40329.6	130	616,060
Disposable Income per Capita: Urban	8015.2	3249.1	0	28,209
Floor Space Sold: Commodity	1278.2	2619.8	4	36,950
GDP	51.9	81.8	0.8	1,288
GDP: per Capita	11611.9	10790.9	0.001	98,938
GDP: Primary Industry	6.3	4.9	0.0	47
GDP: Secondary Industry	24.9	39.0	0.3	568
GDP: Tertiary Industry	20.7	43.6	0.4	791
Gross Industrial Output	59259.4	133348.1	20	2,310,863
GIO: Domestic Funded Enterprise	40397.7	66960.3	20	761,113
GIO: Foreign Funded Enterprise	11568.9	52227.3	0.6	1,201,160
GIO: HMT Funded Enterprise	6829.6	24444.4	0.5	372,032
Loan	54525.4	130574.7	920	2,170,995
Loan to Agricultural Sector	3170.2	3234.6	0	43,953
Loan to Commercial Sector	5738.6	8062.6	10	105,622
Loan to Industrial Sector	7468.9	15340.6	0	205,020
No of Enterprise: Industrial	734.6	1233.6	3	15,766
No of Enterprise: Industrial: DF	460.4	812.4	15	9,217
No of Enterprise: Industrial: FF	62.7	229.8	0	4,086
No of Enterprise: Industrial: HMT	63.7	215.1	0	2,498
Property Price	1628.6	958.3	439	14,050
Real Estate Investment	3774.5	11234.6	0	199,582
Real Estate Investment: Residential	2728.6	7144.0	0	99,166
Wholesale & Retail Sales	23.4	84.5	0.0	1,666
Wholesale & Retail: No of Enterprise	140.8	282.1	0	6,088
Retail Sales of Consumer Goods	13.6	20.4	0.4	283
Education				
No of School: Higher Institution	5.4	9.5	0	83
No of School: Primary School	1408.1	1187.8	24	15,737
No of School: Secondary School	250.5	164.9	9	1,607
Student-Teacher Ratio: Higher Inst	14.8	5.4	0.5	59
Student-Teacher Ratio: Primary School	20.6	5.0	9.0	38
Student-Teacher Ratio: Secondary School	18.3	22.6	7.4	1,245

Environment				
% Green Space	0.005	0.02	0.00000	0
Sulphur Dioxide Emission: Industry	59.3	63.6	0.1	759
Ratio of Industrial Solid Waste Utilized	64.0	25.6	1.9	100
Treatment Rate of Living Waste	73.3	28.4	0.4	100
Treatment Rate of Living Waste Water	38.1	24.0	0	100
Waste Water Discharge: Industry	126.6	125.7	0.2	940
Waste Water Discharge: Industry: Meet Discharge Standard	68.0	102.7	0.0	881
Health				
No of Beds in Hospitals per Capita	0.003	0.002	0.000	0
No of Hospitals per Capita	0.00006	0.00006	0.00000	0
Infrastructure				
% Population Subscribed to Broadband	0.05	0.08	0.0001	2
Developed Area of City Construction	83.1	152.5	0	2,429
Floor Area of Residential Building per Capita: Rural	30.1	8.7	8.1	69
Floor Area of Residential Building per Capita: Urban	19.5	4.9	0	63
Land Area of Administrative Zone	16422.3	24873.6	236	391,817
% Population with Landline	0.2	0.2	0.01	3
Transport: Freight Traffic	54.1	62.5	0.3	781
% Population with Mobile Phone	0.3	0.5	0.00	9
Transport: Passenger Traffic	57.6	63.8	0.9	772
% Employed in Primary Industry	0.08	0.1	0.000	2
No of Employee: Tertiary Industry	248.4	520.2	8.3	6,537
% Self Employed	0.6	0.4	0.01	3
% Unemployed	0.06	0.04	0.001	0
Politics				
Politicians Average Yrs of Schooling	15.4	3.8	0	23
Politicians Average Career Incentive	0.4	0.2	0.1	1
% of Corrupt Politicians	0.10	0.2	0	1
Population				
Population: Census	3970.2	2772.4	159.2	32,353
% Male Population	0.5	0.06	0.1	1
Average Household Size	3.4	0.7	2.0	18
% Urban Population	0.4	0.2	0.1	1
% Population with Access to Gas	0.09	0.2	0.00000	7
Public Finance				
Govt Expenditure	4803.9	10455.4	330.5	220,192
Govt Expenditure: Education	802.9	1437.7	8.3	28,333
Govt Expenditure: Science	5390.7	9264.5	0.3	21,531
Govt Expenditure: Social Security	391.8	925.2	2	27,422
Govt Revenue	3199.1	8967.7	78.7	210,263
Govt Revenue: Tax	2732.8	8195.1	97	197,548
Govt Revenue: Tax: Enterprises Income	702.2	1689.8	3	42,563
Govt Revenue: Tax: Individual Income	382.7	820.3	5	16,944
Govt Revenue: Tax: Operation	877.4	3198.4	46	71,460
Govt Revenue: Tax: Value Added	500.0	1452.7	11	31,342
Trade and Tourism				
Export	2059.0	8139.2	0.0	168,493
Fixed Asset Investment	19891.0	34408.1	232.1	445,861
FDI: Contract Value	438.5	1320.2	0	18,363
FDI: No of Contract	118.1	327.3	0	4,334
FDI: Utilized	255.1	666.6	0	7,920
Import	1699.4	8202.5	-36.8	144,073
Tourism Revenue: Domestic	3944.9	10405.4	0.2	175,360
Tourism Revenue: Foreign Currency	540.3	2394.5	0	35,542
Visitor Arrival	136.3	504.0	0	8,313
Domestic Tourist	5773.2	10143.1	0	142,800
Trade Balance	350.4	3607.4	-95147.0	49,453
Energy and Utilities				
Coal & Natural Gas Supply	143.2	653.8	0	16,508
Coal & Natural Gas Supply: Residential	36.0	102.5	0	2,053

Electricity Consumption	4.0	7.7	0.0	107
Electricity Consumption: Industry	-405665.4	1163458.8	-14252480.8	72
Electricity Consumption per Capita	0.001	0.002	0.0000	0
Electricity Consumption: Residential	4.1	6.4	0.01	15
LPG Supply	40839.9	84482.5	0	1,004,716
LPG Supply: Non-Residential	21233.3	58750.2	-168962	752,252
Water Supply	149.5	272.3	0.3	3,461
Water Supply: Residential	59.6	89.3	0.3	1,260
Observations	3010			

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) ΔROA	(2) ΔLog TFP	(3) ΔLog Lab Pdy	(4) ΔComp	(5) ΔLog Cap Pdy	(6) ΔInv	(7) Lab OP Gap	(8) Cap OP Gap.	(9) ΔEntry
Panel A. OLS									
Δ Share of	0.065**	1.02	1.18***	0.0015	0.75***	2.60	-0.34	-0.20	0.081
Non-State Employment	(0.027)	(0.65)	(0.23)	(0.0031)	(0.19)	(6.96)	(0.24)	(0.22)	(0.071)
Δ Share of	-0.031	0.13	-1.03***	-0.0041	-0.44***	-7.19	-0.029	-0.066	0.14**
Non-State Employment (Squared)	(0.029)	(0.85)	(0.21)	(0.0038)	(0.16)	(6.88)	(0.25)	(0.24)	(0.062)
R^2	0.08	0.04	0.06	0.09	0.10	0.10	0.08	0.04	0.26
Panel B. Reduced Form									
Predicted Δ Share	0.13	0.91	2.87***	-0.00086	1.14**	16.0	-0.025	0.20	0.67***
of Non-State Employment	(0.079)	(0.75)	(0.62)	(0.0084)	(0.51)	(15.7)	(0.58)	(0.48)	(0.14)
Predicted Δ Share	-1.90*	-1.93	-14.2**	0.043	-12.4*	-161.3	-9.73	-7.06	-3.90*
of Non-State Emp. (Squared)	(0.99)	(10.7)	(6.62)	(0.096)	(6.47)	(218.8)	(8.69)	(8.41)	(2.15)
R^2	0.09	0.03	0.07	0.09	0.08	0.05	0.08	0.05	0.23
Panel C. 2SLS									
Δ Share of	-1.14*	-1.12	-8.41*	0.026	-7.40*	-96.5	-5.88	-4.25	-2.32*
Non-State Employment	(0.64)	(6.56)	(4.74)	(0.058)	(4.22)	(138.1)	(5.37)	(5.14)	(1.30)
Δ Share of	0.97*	1.44	8.37**	-0.021	6.50*	85.5	4.59	3.45	2.23**
Non-State Employment (Squared)	(0.51)	(5.17)	(3.93)	(0.047)	(3.37)	(112.2)	(4.28)	(4.11)	(1.04)
Kleibergen Wald rk F	9.08	9.08	9.08	9.08	9.08	9.08	9.08	9.08	9.08
AR P-value	0.09	0.47	0.00	0.90	0.02	0.55	0.52	0.68	0.00
No of Cities	293	293	293	293	293	293	293	293	293
No of Cities×Years	2314	2314	2314	2314	2314	2314	2314	2314	2314

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

	ΔLog TFP - LP (Baseline)			**ΔLog TFP - OP**		
	(1) All Firms	(2) Private	(3) SOE	(4) All Firms	(5) Private	(6) SOE
Panel A. OLS						
Δ Share of Non-State Employment	1.17**	-0.13	-0.55	0.82**	-0.56	0.20
	(0.46)	(0.21)	(1.09)	(0.41)	(0.80)	(0.14)
(2)≠(3)/(5)≠(6): P-value			0.67		0.32	
R^2	0.04	0.04	0.04	0.03	0.05	0.02
Panel B. Reduced Form						
Predicted Δ Share of Non-State Employment	0.72	-0.44	-4.20	0.91	0.35	-0.10
	(0.76)	(0.77)	(4.16)	(1.04)	(0.46)	(3.70)
R^2	0.03	0.04	0.04	0.02	0.02	0.05
Panel C. 2SLS						
Δ Share of Non-State Employment	0.53	-0.33	-3.10	0.67	0.26	-0.074
	(0.56)	(0.57)	(3.03)	(0.77)	(0.34)	(2.72)
(2)≠(3)/(5)≠(6): P-value			0.39			0.91
Kleibergen Wald rk F	153	153	153	153	153	153
AR P-value	0.34	0.56	0.31	0.38	0.45	0.98
No of Cities	293	293	293	293	293	293
No of Cities×Years	2606	2606	2606	2601	2601	2601

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

	ΔReturn On Assets				ΔLog TFP			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	SOE	Always SOE	Private	Always Private	SOE	Always SOE	Private	Always Private
Panel A. OLS								
Δ Share of Non-State Employment	-0.030	-0.012	-0.0061	0.011	-0.55	1.28	-0.13	0.091
	(0.023)	(0.028)	(0.019)	(0.019)	(1.09)	(1.13)	(0.21)	(0.16)
Cross-Eq Test: P-value		0.39		0.00		0.00		0.16
R^2	0.09	0.06	0.07	0.06	0.04	0.02	0.04	0.03
Panel B. Reduced Form								
Predicted Δ Share of Non-State Employment	-0.11	-0.020	0.034	0.049	-4.20	-8.23	-0.44	0.21
	(0.13)	(0.20)	(0.073)	(0.084)	(4.16)	(6.38)	(0.77)	(0.58)
R^2	0.09	0.06	0.07	0.06	0.04	0.03	0.04	0.03
Panel C. 2SLS								
Δ Share of Non-State Employment	-0.0776	-0.0151	0.0254	0.0363	-3.096	-6.059	-0.328	0.154
	(0.0935)	(0.147)	(0.0541)	(0.0626)	(3.028)	(4.680)	(0.571)	(0.424)
Cross-Eq Test: P-value		0.72		0.56		0.40		0.21
Kleibergen Wald rk F	153	153	153	153	153	153	153	153
AR P-value	0.40	0.92	0.63	0.56	0.31	0.19	0.56	0.72
No of Cities	293	293	293	293	293	293	293	293
No of Cities×Years	2606	2606	2606	2606	2606	2606	2606	2606

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)
	ΔReturn On Assets			ΔLog TFP			ΔPer Worker Compensation		
[1em] Δ Share of Non-State Employment (OLS)	0.031*	0.031	0.031*	1.17**	1.17**	1.13**	-0.0030	-0.0030**	-0.0030
	(0.017)	(0.021)	(0.017)	(0.46)	(0.55)	(0.47)	(0.0020)	(0.0012)	(0.0020)
R^2 (OLS)	0.08	0.08	0.09	0.04	0.04	0.04	0.09	0.09	0.09
Δ Share of Non-State Employment (IV)	0.045	0.045	0.041	0.53	0.53	0.47	-0.00053	-0.00053	-0.00078
	(0.052)	(0.059)	(0.052)	(0.56)	(0.66)	(0.57)	(0.0053)	(0.0047)	(0.0054)
Kleibergen Wald rk F	152.93	103.40	151.08	152.93	103.40	151.08	152.93	103.40	151.08
AR P-value	0.38	0.44	0.43	0.34	0.43	0.41	0.92	0.91	0.88
	ΔLog Labour Productivity			ΔLog Capital Productivity			ΔInvestment		
[1em] Δ Share of Non-State Employment (OLS)	1.75***	1.75***	1.75***	0.26***	0.26*	0.28***	-5.40	-5.40	-5.20
	(0.40)	(0.33)	(0.41)	(0.10)	(0.14)	(0.10)	(3.47)	(4.35)	(3.49)
R^2 (OLS)	-0.20	-0.20	-0.20	0.10	0.10	0.10	0.10	0.10	0.07
Δ Share of Non-State Employment (IV)	1.75***	1.75***	1.75***	0.69*	0.69*	0.69*	3.41	3.41	3.58
	(0.40)	(0.33)	(0.41)	(0.37)	(0.40)	(0.38)	(10.3)	(10.4)	(10.5)
Kleibergen Wald rk F	152.93	103.40	151.08	152.93	103.40	151.08	152.93	103.40	151.08
AR P-value	0.00	0.00	0.00	0.05	0.07	0.06	0.74	0.73	0.73
	ΔLabour OP Gap			ΔCapital OP Gap			ΔEntry		
[1em] Δ Share of Non-State Employment (OLS)	-0.37**	-0.37**	-0.38***	-0.27**	-0.27	-0.26**	0.24***	0.24***	0.23***
	(0.14)	(0.17)	(0.14)	(0.12)	(0.17)	(0.12)	(0.032)	(0.035)	(0.033)
R^2 (OLS)	0.08	0.08	0.09	0.04	0.04	0.06	0.26	0.26	0.26
Δ Share of Non-State Employment (IV)	-0.23	-0.23	-0.22	0.12	0.12	0.19	0.39***	0.39***	0.40***
	(0.42)	(0.43)	(0.44)	(0.37)	(0.27)	(0.38)	(0.091)	(0.11)	(0.091)
Kleibergen Wald rk F	152.93	103.40	151.08	152.93	103.40	151.08	152.93	103.40	151.08
AR P-value	0.59	0.60	0.62	0.74	0.63	0.61	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	293	30	289	293	30	289	293	30	289
No of Observations	2606	2606	2570	2606	2606	2570	2606	2606	2570

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

$$\sum_t \sum_n s_{nt} = \sum_t \sum_n \sum_c e_{ct} s_{cnt} = \sum_t \sum_c e_{ct} \sum_n s_{cnt} = 1 \tag{16}$$

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:

$$y_{it} = \beta_k k_{it} + \beta_l l_{it} + \omega_{it} + \varepsilon_{it} \tag{17}$$

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 ω_{it} and ε_{it} represent firm-specific productivity shocks unobserved by the econometrician. They differ in that firm i observes ω_{it}, but not ε_{it}, before it chooses inputs k_{it} and l_{it}. Common examples of ω_{it} include managerial talent, expected rainfall and technological breakthroughs. ε_{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}.

ω_{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 β_k and β_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 ω_{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 $w_{i0}, ..., w_{it-1}, w_{it}$, but not $w_{it+1}, w_{it+2}, ...$. Also, ε_{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 ξ_{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 δ.

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 ω_{it}.

This requires the investment function to be uniform across firms[24]. Strict monotonicity requires firms with the same i_{it} and k_{it} to share the same ω_{it}. Assumption OP5 allows f_t to

[24]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:

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

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

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

To avoid additional assumptions about the investment process, one will have to treat f_t non-parametrically. Thus, β_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 ε_{it} where $E\left[\varepsilon_{it}|I_{it}\right] = 0$ 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

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

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

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

$$\omega_{it} = \omega_{it-1} + \xi_{it} \tag{23}$$

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

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

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

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

and "plug in" $\hat{\beta}_l$ and $\hat{\Phi}_{t-1}(k_{it-1}, i_{it-1})$ to yield a consistent estimate of β_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 ω_{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 ω_{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*

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

where f_t is strictly increasing in ω_{it}.

The two-step estimation procedure is the same.

B.2.3 Ackerberg-Caves-Frazer (ACF) Correction

Ackerberg 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 (ω_{it}, k_{it} and i_{it}). If all of the variation in l_{it} is caused by variation in i_{it}, k_{it} or ω_{it}, then there is no unique variation in l_{it} for β_l to be separately identified. For example, consider a non-dynamic labour demand

$$\begin{aligned} l_{it} &= h_t\left(k_{it}, i_{it}, \omega_{it}\right) && (27) \\ &= h_t\left(k_{it}, i_{it}, f_t^{-1}\left(k_{it}, i_{it}\right)\right) && (28) \end{aligned}$$

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

$$\begin{aligned} E\left[\varepsilon_{it}|I_{it}\right] &= E\left[y_{it} - \beta_l l_{it} - \Phi_t(k_{it}, i_{it})|I_{it}\right] && (29) \\ &= E\left[y_{it} - \beta_l h_t\left(i_{it}, k_{it}, f_t^{-1}\left(k_{it}, i_{it}\right)\right) - \Phi_t(k_{it}, i_{it})|I_{it}\right] = 0 && (30) \end{aligned}$$

Since l_{it} is not even present in the moment condition, its elasticity β_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 β_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 Ackerberg 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 ω_{it}.

Then the first stage moment becomes

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

And the second stage moment becomes

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

This allows for β_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	$\frac{\text{N(Workers in Industry Non-SOEs)}_{ct}}{\text{N(Workers in Industry)}_{ct}}$	AIS
$Priv_{nt}$	% of CIC 4-digit industry n's total workforce in non-state firms in year t	$\frac{\text{N(Workers in Non-SOEs)}_{nt}}{\text{N(Workers)}_{nt}}$	AIS
S_{ct}	% of city c's total workforce in the industrial sector, fixed to first-observed values for each c	$\frac{\text{N(Workers in Industry)}_{ct}}{\text{N(Workers)}_{ct}}$	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	$\frac{\text{N(Workers)}_{cnt}}{\text{N(Workers in Industry)}_{ct}} \times S_{ct}$	AIS, CEIC
$\Delta \widehat{Priv}_{cnt}$	Predicted increase in % of city c's total industrial workfore 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, $\sum_{c,t} e_{ct} = 1$)	$\frac{\text{N(Firms)}_{ct}}{\sum_{c,t} \text{N(Firms)}_{ct}}$	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 $Var(\varepsilon_i) = \sigma^2 \ \forall i = 1, ..., n$.

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 σ^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 $\hat{x}_1, ..., \hat{x}_n$ 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, $j = \text{argmin}_k |\hat{x}_i - \hat{x}_k|$ such that x_k is not missing.