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The Public Sector and the Misallocation of Labor: Evidence from a Policy Experiment in India

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Abstract
State-owned enterprises are often thought to represent a distortion in the labor market, but the implied efficiency losses have not been carefully estimated. This paper presents the first rigorous quantification of the aggregate productivity effects of privatization of public sector enterprises. We study historical episodes of privatization of public sector firms in India over the period 1991-2005, and find evidence of reallocation of labor away from the public sector following privatization. In turn, this reallocation appears to result in a substantial improvement in aggregate productivity and output.

Key words: labor, public sector, India
JEL Codes: J2

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1 Introduction

Since the 1980s, one of the most under-appreciated changes in the structure of modern economies has been the winding down of state-ownership and management of productive assets. Encouraged by the successful privatization experiments of the Thatcherite government in Britain, a number of countries embarked on a process of divestiture of state-owned enterprises, or SOEs (see Megginson 2005 for a historical account). Indeed, Djankov and Murrell (2002) refer to privatization as the "pre-eminent policy reform of the 1990s". Notwithstanding the steady decline in state ownership in most countries over the last few decades, the issue of privatization is receiving renewed attention due to a number of distinct factors. A first factor is the incipient divestiture of SOEs in China, the significance of which is highlighted by the fact that two of the three largest IPOs in history have occurred in the last ten years, and represented initial share offerings in two of the "Big Four" Chinese state-owned banks.[1] A second factor generating interest in state-ownership is the financial crisis of 2008, which resulted in a wave of partial nationalizations of banks and financial institutions around the world. Third, a sustained fall in global oil prices has led to pressures to privatize state-owned petroleum corporations, most notably in the Middle East.

The empirical literature on the effects of privatization has been dominated by classic questions arising in the field of finance, including the relationship between firm performance and ownership, and the structure and investment performance of share offerings in SOEs (Megginson and Netter 2001 and Birdsall and Nellis 2003 provide thorough surveys of this literature; see Megginson 2017 for a survey of the more recent literature). In contrast, a broader set of economic questions concerning the impact of state-ownership (and, by implication, privatization) on the rest of the economy, in

[1] The Agricultural Bank of China Ltd and the Industrial and Commercial Bank of China went public in 2010 and 2007 respectively, raising more than $19 billion each. As Megginson (2005) notes, the largest security offering in history was also associated with an SOE (Nippon Telegraph and Telephone in 1988).

terms of general equilibrium implications and labor market spillovers, has received descriptive attention (e.g. Haltiwanger and Singh 1998, Rama 1999), but virtually no rigorous evaluation.

In particular, the argument that state-ownership of firms distorts the allocation of resources (especially labor and credit) in the economy, and thereby gives rise to aggregate inefficiencies (over and above the effect of state-ownership on the efficiency of the SOE), is frequently made on theoretical grounds, and is supported by a number of stylized facts about the public sector: First, the public sector has historically accounted for a substantial percentage of employment in a number of economies; it is also significantly less efficient than the private sector (e.g. Hsieh and Klenow (2008) estimate that marginal factor productivity in SOEs in China is at least 40% lower than that of private enterprises; see also Dollar and Wei 2007); third, the public sector has tended to compensate workers at a rate that is not aligned with their productivity (Rama 1999), a factor that tends to "lock-in" the inefficient allocation of labor. There is ample evidence, therefore, suggesting that SOEs may be a significant source of distortion in local factor markets.

Focusing on the labor market distortions associated with state-ownership, the question of whether privatization will necessarily correct these distortions cannot be unambiguously answered, with the final outcome depending on a number of complementary factors such as the structure of the retrenchment process and the nature of labor regulations. To start with, the stringency of the labor regime (with respect to, for example, employment protection) will constrain the ability and/or willingness of the existing private sector to absorb workers shed by the SOE; second, the skill-composition of the retrenched workers will also affect their ability to find new employment. The available evidence indicates that retrenched workers are usually successful at obtaining employment relatively soon afterwards (see the country studies summarized in Mckenzie and Mookherjee 2003, and the case studies in Haltiwanger and Singh 1998),

which may partially reflect the findings of Chong, Guillen and Lopez-Silanes (2011), who show that retrenchment programs associated with privatization have typically had the effect of inducing the most productive workers to leave. These findings therefore beg the question of whether and to what extent worker reallocation following privatization actually increases the economy's efficiency in the aggregate.

By nature, such general equilibrium effects are difficult to estimate, however, because it is rarely the case that one can construct credible counterfactuals or control groups. We tackle this question by utilizing the natural experiments arising from historical privatization episodes in India, a country where the state continues to control a significant share of the formal manufacturing sector. Starting in the early 1990s, India initiated a process of disinvestment in public sector enterprises. We focus on the period 1991-2005 during which a number of SOEs were privatized. Our hypothesis is that privatization of SOEs may bring to bear market pressures that release labor to more productive sectors of the economy, and as a result, may improve overall productivity in the economy.

We argue that this is a nearly ideal setting for analyzing such effects, because the staggered implementation of privatization interacts with the local nature of labor markets to provide a credible identification strategy. We analyze the reallocational effects of the Indian privatization reforms by examining how the privatization of a factory in a particular district affects the allocation of resources (particularly labor), and hence aggregate output and productivity, in that district. We employ a difference-in-differences approach, examining how these outcomes changed in districts that had an SOE that was privatized, relative to comparable districts that did not. This strategy takes advantage of significant spatio-temporal variation in district-level "exposure" to privatization.

The results indicate that, on average, exposure to a privatization event increases aggregate output and productivity in the local economy by about 7%. We also find

a significant reallocation of labor from the public to the private sector; as we would expect, the reallocational gains are greater in industries that are more likely to provide close employment substitutes to jobs in the privatized firms. There is also evidence that the extent of labor reallocation is stronger in states with more flexible labor laws. In line with the predictions of a simple model, we also find that the increase in aggregate output is greater in labor markets characterized by a larger pre-reform productivity gap between the public and private firms. Finally, a simple decomposition suggests that the entire increase in aggregate productivity can be attributed to input reallocation between the public and private sectors. These results provide a rigorous confirmation of the hypothesis that SOEs represent a source of distortion in the labor market, while also providing the first quantification of the associated efficiency losses.

Our study makes two important contributions. First, we fill a gap in the debate regarding the social desirability of privatization by providing estimates of the aggregate efficiency losses from state-ownership. A notable aspect of the results is that in most cases of privatization in our data the state continued to retain majority ownership of the SOE, implying that even partial privatization is associated with significant rationalization and reallocational gains. Our results on successful labor reallocation are especially significant, given that the prospect of employment losses is a key issue surrounding privatizations in most countries, including India, where the phenomenon of "jobless growth" in the manufacturing sector has particularly intensified the opposition to privatization.

Our study also makes a contribution to a literature that has focused on explaining the persistent cross-country differences in productivity by examining factors that distort the allocation of resources within the economy. In an influential study, Hsieh and Klenow (2008) demonstrate that the manufacturing sector in countries like India and China is typified by stark and persistent “mis-allocations”, in which productive

resources such as labour and capital are locked into highly inefficient configurations.[2] From a theoretical standpoint, the underlying constraints may be usefully categorized as "frictions", but from a policy perspective it is critical to identify the particular institutional features that have given rise to these frictions. While attention has frequently centered on labor regulations,[3] our findings underscore the point that states' interference in labor markets sometimes takes more direct forms, with significant implications for the allocation of resources in the economy.

The paper proceeds as follows: Section 2 provides a description of public sector reforms in India; Section 3 describes the data; Section 4 explains the empirical strategy and presents the results; Section 5 concludes.

2 Privatization of public sector enterprises in India, 1991-2005

India is one of a number of countries that embarked on privatization of state firms during the 1990s (other examples include Indonesia, Bangladesh, and a number of transition economies in Eastern Europe). In India, the process was initiated as part of a broader set of economic reforms in response to a severe balance-of-payments crisis in 1991. Prior to the crisis, state-owned firms had been a long-standing and significant part of the economic landscape - as in other countries that had adopted the socialist model, state ownership in India was an important means of exercising control over the economy, and for fulfilling the 5-year economic plans. We focus on public sector enterprises operated by the national government, also referred to as Central Public Sec-

[2] A similar observation has been made in the context of the agricultural sector in developing countries (Adamopoulos and Restuccia 2014).

[3] There is now ample evidence of the adverse effects of such policies (e.g. see the chapters in the volume by Heckman and Pages 2004). Indeed, in their study of a number of developing countries in Africa, Asia and Latin America, McMillan and Rodrik (2011) find that employment flexibility consistently emerges as a key facilitator of structural transformation.

tor Enterprises. By 1991, CPSEs constituted nearly 85% of public sector capital assets. Over time, however, mounting losses in these firms became a drain on the exchequer and heightened the case for reform.

Kapur and Ramamurti (2002) and Makhija (2006) provide illuminating discussions of the process of privatization in India (see also Ghuman 1999). In line with most other countries that have undertaken privatization, privatization in India was a process that started as a result of an initial impetus (the crisis of 1991) but thereafter escalated only gradually. The term "privatization" itself was largely avoided; instead, the reform was described as a program of divestment. In the early phases of the reform, disinvestment was restricted to sales of up to 20% of the shares of each privatized company; this gradually increased to sales of up to 49% of ownership share (just short of relinquishing state control over the enterprises), and then to 74% of ownership share. It was only in the early 2000s that outright sale of SOEs occurred.

Table 1 lists the public sector companies that were subjected to disinvestment during the period under study, 1991-2005 (we focus only on manufacturing sector firms). Because CPSEs were all large multi-plant firms, the actual number of districts which experienced a "privatization event" is much larger than the number of firms privatized, as we discuss later. The majority of these privatizations took the form of share-issue privatization (SIP); a smaller number of firms were sold to single entities (so-called "asset sales"). Alongside the gradual increase in the degree of privatization, there was also a relaxation of strictures concerning the sectors that were open for privatization as well as a relaxation of rules regarding foreign ownership.

There is a general consensus that India's privatization in the 1990s significantly improved efficiency of the privatized firms, notwithstanding the relatively small portion of equity that was divested (Gupta 2005, Sarkar and Sensarma 2010, Pratap 2011; for studies of privatization in later years, see Mandiratta and Bhalla 2017, who arrive at the same conclusion). Consistent with evidence from privatization episodes in other

countries (Kikeri 1998, Megginson and Netter 2001, Birdsall and Nellis 2003), Pratap (2011) estimates a sharp decline in employment in the privatized firms, of approximately 18% in CPSEs that were privatized by share issue, and of approximately 30% in CPSEs privatized by asset sale (see also Naib 2003).[4]

It is worth noting that the estimated employment decline in privatized firms occurred on top of an overall decline in employment in public sector enterprises as a whole: Figure 1, which is based on employment statistics from the Public Enterprises Survey of the Government of India, shows that employment in Central Public Sector Enterprises (CPSEs) increased to a peak of 2.2 million workers in 1990, after which it began a steady decline in the post-reform era. This reduction is thought (Singh and Chittedi 2011, Nagaraj 2014) to largely reflect the introduction of Voluntary Retirement Schemes (VRS), which provided reasonably generous (voluntary) severance packages to workers. The Central Government also established a National Renewal Fund (NRF) in 1992, among whose objectives was to finance retraining programs for workers who had been made redundant in public sector enterprises. The specifics of VRS schemes varied across PSEs, and the lack of systematic data on these schemes implies that little is known about which workers were induced to leave and which ones stayed; we also know little about the subsequent labor market experiences of the retrenched workers. Haltiwanger and Singh (1999) discuss the effects of a large retrenchment in the public sector textile industry in India in 1993-94: Survey evidence cited by the authors suggests that all the retrenched workers stayed in the labor force, with nearly 80% of them being re-employed soon after. If these experiences are typical of other restructuring programs in India, a natural question is whether the associated reallocation of workers between the public and private sectors was efficiency-improving in the aggregate. Our study attempts to answer this question.

[4]Gupta (2005), using the data from the same source as Pratap (2011) but over a shorter time-span, does not find a significant change in employment.

3 Data

3.1 District-level privatization data

Data on the public sector companies that were subjected to disinvestment was compiled from two principal sources. The Bombay Stock Exchange Limited (BSE) maintains a databse on share offerings and sales of central public sector enterprises;[5] we cross-checked this database with data from the World Bank's Privatization Transactions Database. We then drew on public records, news reports, company websites, and other source material to identify the factory locations of central public sector enterprises (CPSEs) that were included in this wave of disinvestment. We used this information to construct a district-level dataset that records the timing and extent of exposure to disinvestment at the district level. To do so, we define a privatization "event" when at least one of the central public sector manufacturing enterprises in a district experienced disinvestment in a given year. In the remainder of the paper, we refer to districts that experienced at least one privatization event as "privatized districts".

Figure 1 presents a district map showing the location of privatized districts. As the figure shows, there is significant spatial variation in the distribution of privatized districts in our data, consistent with the breadth of the public sector presence in manufacturing. At the same time, there is evidence of some clustering of privatizations (which partially reflects spatial clustering in the presence of public sector factories) - for instance, the southern states of Andhra Pradesh, Karnataka and Tamil Nadu experienced a number of privatizations. This fact has a potentially important implication for our identification strategy, as we discuss later.

Our focus on districts as an appropriate level at which to analyze labor reallocation reflects an assumption that labour markets in India tend to be fairly local, a view that

[5] http://www.bsepsu.com/

is shared by a number of other studies (see, for example, Topalova 2007, Topalova 2010, Adhvaryu et al 2011). This assumption is also supported by the observation that rates of internal migration in India are among the lowest in the world (Bell 2015) and migration tends to be predominantly within-district (Kone et al 2016; see also Atkin 2016).

3.2 Factory data

The analysis uses factory data from the Annual Survey of Industries (ASI) spanning the period 1985-2009, with the exception of 1995-1997. [6] Each year pertains to an accounting period; for example, the 1985 survey pertains to the 1985-86 accounting year that spans April 1, 1985 to March 31, 1986.

The sampling universe for the ASI is all firms that are registered under sections 2m(i) and 2m(ii) of the Factories Act as well as firms registered under the Bidi & Cigar Workers Act and a number of utility and service providers. We restrict attention to manufacturing establishments. Large firms are surveyed every year (the "census" sector), while smaller firms are covered on a sampling basis (the "sample" sector). [7] Each unit surveyed is generally a factory (establishment); however, if an owner has two factories in the same state, sector (census versus sample) and industry, a joint return can be furnished. In the population of firms, fewer than 2 percent of the observations

[6] India's Ministry of Statistics and Programme Implementation does not make the data for 1995 available. The 1996 and 1997 data are available; however, we were unable to construct input measures consistent with those in other years. The structure of the questionnaires in these two years is different from those in other years, and examining input values constructed from 1996 and 1997 suggests that the aggregate input values are not in line with the general patterns observed during the rest of the time period. Thus, we considered the data from these years to be potentially incompatible with the other years and excluded them.

[7] The division between the two sectors depends on firm size, and changed several times between 1985 and 2009. For most years during this period, factories with 100 or more workers were covered in the census sector. During several years, though, the census sector was defined as including factories with 200 or more workers or with a certain value of output. In additional, factories in a number of "less industrially developed" states were also included in the census sector, although the specific states included changed to some extent during this period.

report having more than one factory.

The ASI data contain information on factory ownership that allow us to classify firms according to whether they are publicly or privately owned. Beyond identifying the districts in which they are located,[8] we do not attempt to pinpoint the privatized firms in the ASI data.[9] While this implies that we cannot make precise statements about the effect of privatization on outcomes for the privatized factory, we can and do examine outcomes for the set of public sector firms in a district - as we will see, this level of aggregation still allows for informative analysis of the effects of privatization.

We deflate output using industry-specific wholesale price indices (WPI) from the Government of India's Handbook of Industrial Statistics. Similarly, we construct material input deflators using the WPI along with India's 1993-94 Input-Output Transactions Table. Labor is measured as the total number of individuals employed by the factory, and capital is measured by deflating the book value of fixed assets by an industry-level capital deflator calculated using the perpetual inventory method. For the district-level analysis, we construct district aggregate estimates of output, labor, capital, and materials. We only include factories that are open and that report positive values of these variables.

For each factory (and year), we calculate total factory productivity using the chain-linked index number method proposed by Good et al (1997). We employ this method for two key reasons. First, in order to construct the long time series that covers the period of major privatizations in India, we must compile pooled cross-sectional data on factories, since panel data are not available until 1998. Thus, state-of-the-art production function estimation methods which rely on panel data cannot be applied in this

[8]During this period, a number of changes were made to the administrative boundaries of districts in India. To address this challenge, we constructed a district concordance that mapped each district in each year, to a common code. For example, if one district was split into two districts, we mapped each of the two new districts, and the original district, to the same district code.

[9]Doing so would effectively de-anonymize the data, which we cannot do under the data use agreement. In any event, the lack of panel identifiers in the data implies that it is not straightforward to identify the privatized factories even with knowledge of their industry and location.

context. These approaches also require an accurate identification of exit, which is not feasible in our data given that many of the factories are covered on a sampling basis, i.e. when we observe a factory in one year but not the next, we cannot determine whether it has exited, or whether the factory was simply not selected for sampling during the second year.

Given the cross-sectional nature of the data, we utilize the method of Good et al (1997). Using this method, we calculate TFP of firm i in industry j in year t as:

$$\begin{aligned} TFP_{ijt} \quad = \quad & \underbrace{(q_{ijt} - \overline{q_{jt}})}_{deviation\ from\ avg.\ q} + \underbrace{\sum_{r=2}^{t} (\overline{q_{jr}} - \overline{q_{jr-1}})}_{yearly\ change\ in\ q} \\ & - [\ \underbrace{\sum_{k=1}^{K} \frac{1}{2} (S_{ijt}^{k} + \overline{S_{jt}^{k}})(k_{ijt} - \overline{k_{jt}})}_{deviation\ from\ avg.\ k} + \underbrace{\sum_{r=2}^{t} \sum_{k=1}^{K} \frac{1}{2} (\overline{S_{jr}^{k}} + \overline{S_{jr-1}^{k}})(\overline{k_{jr}} - \overline{k_{jr-1}})}_{yearly\ change\ in\ k}\ ] \end{aligned} \tag{1}$$

The first two terms are the deviation of factory i's output relative to average output in industry j in year t, and the deviation between average output in industry j in year t, relative to the base year. The third and fourth terms are the deviation of factory i's inputs relative to average inputs in industry j in year t, and the deviation between average inputs in industry j in year t, relative to the base year. TFP is obtained by summing the first two terms and subtracting the third and fourth terms. The revenue share of labor is calculated as total emoluments divided by the value of output, while the revenue share of material is calculated as the cost of materials divided by the value of output. We assume that the capital share is equal to 1 minus the labor plus material shares. Input shares and average output and inputs are allowed to vary across industries.

3.3 Data on other reforms

India also undertook a series of other reforms after facing the balance-of-payments crisis in 1991. Between 1991 and 1997, tariffs were harmonized and dramatically reduced; the average final goods tariff on manufactured goods fell from 95 to 35 percent (Harrison et al. 2012). India also continued the dismantling of its licensing scheme, which had begun during the 1980s. Under the "License Raj", large firms had to obtain licenses, which included restrictions on their output and the types of goods they could produce. About one-third of industries were delicensed in 1985; most remaining industries were delicensed during the post-1991 reforms (Aghion et al. 2008). In addition, the 1991 reforms saw changes in foreign direct investment (FDI) policy; "automatic" approval of majority FDI was allowed in about one-third of industries, and further liberalization took place later in the decade (Sivadasan 2009).

We constructed a measure of final goods tariffs at the industry level by mapping applied tariff data from the Government of India's Customs Tariff Working Schedules and Trade Analysis and Information System (TRAINS) to India's three-digit National Industrial Classification (NIC-87) codes, using the concordance developed by Debroy and Santhanam (1993). We then calculated input tariffs at the industry level, using the industry-level final goods tariffs and India's 1993-94 Input-Output Transactions Table, following the method suggested by Amiti and Konings (2007). For the delicensing and FDI reforms, we started with liberalization data from Aghion et al. (2008), and added more recent information using Press Notes from the Ministry of Commerce and Industry. The delicensing and FDI reform variables are equal to one if any products in a three-digit industry have been liberalized and are equal to zero otherwise. For further details on these reforms, see Harrison et al. (2012).

4 Conceptual framework

The basic intuition underlying the static misallocation idea is formalized in general terms by Hsieh and Klenow (2008). We outline here a model that focuses attention on some specific considerations that arise in the context of labor allocation between public and private sector firms. Our objective here is not to construct a fully fleshed-out equilibrium model, but rather to lay out a simple framework that clarify the interpretation of our results, in addition to suggesting some testable hypotheses.

Consider a local economy (this would correspond to a district in our data), in which aggregate output is the sum of public and private sector output:

$$Y = Y^{pub} + Y^{priv}$$

Now consider a small reduction in public sector employment, ΔL^{pub}. Suppose that this labor is reallocated to other sectors, with the private sector in the district capturing a fraction π. Then, the change in aggregate output is approximately given by:

$$\begin{aligned} \Delta Y &\approx MP_L^{pub}\Delta L^{pub} - MP_L^{priv}(\pi \Delta L^{pub}) \\ &= (MP_L^{pub} - \pi MP_L^{priv})\Delta L^{pub} \end{aligned}$$

The resulting change in aggregate output therefore depends on (a) the difference in marginal productivities between the public and private sectors, (b) the amount of labor retrenched in the public sector, and (c) the amount of retrenched labor that is rehired in the private sector in the district. π could depend on the composition of the retrenched workers: If the retrenchment process results in "cream skimming", the retrenched workers will on average be relatively able and therefore likely to find new jobs quickly. It could also depend on the existing labor regime- e.g. if the economy is subjected to significant hiring/firing restrictions, then π may be very small, with the

majority of retrenched workers ending up in unemployment or in the informal sector.

The last point noted above highlights the fact that there are two potential "leakages" of labor that we cannot observe in our data, since we do not observe either informal output or the rate of unemployment in the district. A distinct "leakage" occurs when displaced workers move to other districts. While we can always estimate the effect on formal sector output, the effect on aggregate district output (including formal as well as informal sectors) would appear ambiguous. However, our thought experiment above clarifies that as long as the effect on formal sector output is positive, the total effect must also be positive - i.e. if the reduction in public sector output is already offset by output increases in the formal private sector, then the additional workers lost due to "leakage" can at worst add nothing to output (for instance, by entering the unemployment rolls), so that the total output effect must be at least as large as that observed in the formal sector. Thus, in this special case (which is what obtains in our data), the observed effect on formal sector output places a lower bound on the total effect. It follows by a similar argument that output per worker must also increase in the district.

5 Descriptives

To set the stage for the analysis, we begin with descriptive regressions that compare public and private sector enterprises on a number of dimensions. To do so, we regress each of a set of factory-level outcomes on an indicator for a (central) public sector enterprise (CPSE). Table 2 shows the results. CPSEs are 3-4 times bigger than private sector factories in terms of output, employment and capital; remarkably, the former are significantly less efficient in terms of TFP as well as labor productivity (Majumdar 1996 obtains a similar set of findings for the period 1973-1989) ; notwithstanding, real wages in the public sector are twice as large as those in the private sector. These conclu-

sions stand largely unchanged even if we control for 3-digit industry and district fixed effects (the results are reported in Appendix Table 1), implying that CPSEs are bigger and less productive than other firms in their sector, as well as relative to other firms in their local labor market. The latter observation in particular confirms the hypothesis that the public sector is a significant source of distortion in the labor market.

Our main analysis uses a district-level panel. Figure 2 shows the distribution of district-level privatization events (as defined earlier) over the sample period. There are two clusters of events, one in the early 1990s, immediately following the economic reforms of 1991, and a second cluster in the early 2000s. Table 3 presents means and standard deviations for aggregate district-level outcomes, separately for three groups:(i) Districts that did not have any CPSEs (of which there are 129), (ii) Districts that had at least one CPSE but did not experience a privatization event (of which there are 74), and (iii) Districts that experienced a privatization event (of which there are 67). Looking at the latter two sets of districts, we observe that on average CPSEs account for a significant share (approximately 10%) of district-level output and employment. It is also apparent that districts that have CPSEs are clearly bigger than those that do not, in terms of aggregate output, employment, capital and the number of factories. Districts that experienced a privatization tend to be slightly larger than districts that had CPSEs but were not subjected to privatization, although the differences in average public sector size are less pronounced. These observations have important implications for our identification strategy, as we explain in the next section.

6 Analysis

6.1 Empirical specification

We now turn to estimating the effects of privatization on district-level outcomes. As we saw in Table 3, districts that experienced a privatization are very different from districts that did not (and even more different from districts that did not have even have a CPSE), implying that cross-sectional comparisons of privatized and non-privatized districts will not identify the effect of privatization. We therefore adopt a difference-in-differences estimation strategy that compares changes in outcomes for privatized districts and non-privatized districts, while controlling for (time-invariant) differences between districts. The identification is now based on the assumption that the outcomes in privatized districts would not have changed differently from those in non-privatized districts in the absence of privatization.

We strengthen the plausibility of the parallel trends assumption in a number of ways. First, we restrict the sample in the main specification to only include districts that experienced at least one privatization, i.e. the identification is based on a comparison of privatized and not-yet-privatized districts - this addresses the concern that CPSEs that were privatized (and the districts that they were located in) were systematically different from CPSEs that were not privatized. Even within the sample of districts that were privatized, however, there may be a concern that the timing of privatization was endogenous to district outcomes. To address this issue, we include district-specific trends that account for the possibility that treated and control districts may have been characterized by different trends in the outcome variables. Third, to account for the spatial clustering of privatization events, we control for state x year fixed effects, in order to ensure that the effect of district-level privatization is not confounded with the effects of concurrent changes in state-level policies (such as changes in labor regula-

tion).

Some districts experience more than one privatization event. To fully utilize this policy variation, we specify our panel difference-in-differences regression as follows:

$$y_{dst} = \alpha + \beta Privatization_{dt} + \eta_d + \eta_{st} + \gamma_d t + e_{dst} \quad (2)$$

where y_{dt} denotes an outcome of interest for district d in year t; $Privatization_{dt}$ is a multi-valued treatment variable that counts the number of privatization events experienced by district d up to and including year t; η_d denotes a district fixed effect, η_{st} denotes a state x year fixed effect and $\gamma_d t$ denotes district-specific linear time trends. The standard errors in the regression are adjusted for clustering at the district level.

6.2 Effects on aggregate output, employment and capital

Table 4 presents the results from estimating equation (2), for each of a set of aggregate district-level outcomes including output, employment, capital, and the number of factories. For comparison, we show results from three samples, starting with the full sample of districts (Panel A), and then dropping districts that do not have any CPSEs (Panel B), before finally dropping districts that were not privatized (Panel C). The results obtained in the full sample are clearly different in sign and statistical significance from those obtained in the more restricted samples, which highlights the importance of appropriately restricting the control group in the analysis. A consistent finding in the restricted samples is that privatization has resulted in an increase in aggregate district-level output. The point estimates imply an approximately 5% increase in aggregate output following a privatization event. We do not, however, find a statistically significant increase in aggregate labor or capital inputs, or in the number of factories. While the estimates for output are similar in magnitude between the samples in Panels B and C, they are slightly larger in Panel C (the most restricted sample). In the remainder

of the analysis, we retain this sample restriction, i.e. dropping districts that were not privatized.

In the foregoing analysis, we have implicitly imposed the restriction that all privatization events in a district have the same effect. While the assumption is not unreasonable, one may still theorize that subsequent privatizations in a district may have had a different impact from the first event - for instance, the first event may represent a greater "shock" than subsequent ones, or conversely, it may require multiple events to kickstart the rationalization process in the public sector, in which case later privatizations may have larger effects. To test between these hypotheses, we estimate a modified specification in which the multi-valued treatment variable is separated out into dummies for each of the multiple values. The results are presented in Column 1 of Table 5. While it is clear that the effect on output cumulates with multiple privatization events (as one would have expected), there is some indication that subsequent privatizations may have larger impacts (however the standard errors are large enough that we are not able to rule out equality of the various effects).

A second refinement is to test whether the effects differ between minority-sale privatizations (in which the government retains majority ownership of the firm) and majority sales (in which at least 50% of company stock is sold). Accordingly, we further refine the previous specification to distinguish between the two kinds of privatization events (Column 2 of Table 5). Interestingly, the effects on aggregate output only appear to obtain for partial privatizations, but we should caution that the limited number of asset sales in the sample period as well as the truncation of the sample shortly thereafter places a limitation on our power to make a precise statement in this regard.

6.3 Validity checks

We now conduct a number of checks on the main result.

Testing for anticipatory changes: Although the policy of disinvestment was signaled in the 1991 Statement on Industrial Policy, the actual pace of privatization was slow. This raises a potential concern in terms of our identification: An observation that is commonly made in studies of privatization is that long gaps between the announcement and the actual implementation of reform induce anticipatory changes in the performance of public sector firms (see, for example, Yarrow 1992 for a discussion of this phenomenon in the context of privatization in the United Kingdom). The Indian case is notably different, as Kapur and Ramamurti (2002) argue, because of the government's low-key (or, alternatively, "stealth") approach to privatization in the initial decade, as a result of which managers at public sector firms "behaved as if the changes expected of them were incremental and marginal" (Kapur and Ramamurti 2002).

In order to formally assess whether there were any significant changes that anticipated the privatization event at the district-level, we estimate a specification that includes six leads and lags of the privatization treatment variable. The regression sample is now restricted to districts that experienced only one privatization event. The regression specification is as follows:

$$y_{dst} = \alpha + \sum_{k=-6, k\neq -1}^{k=6} \beta_{T+k} Privatized(T+k)_{dt} + \eta_d + \eta_{st} + \gamma_d t + \varepsilon_{jdt} \qquad (3)$$

where $Privatized(T+k)_{jdt}$ is an indicator for k years after the privatization event in the district, and so on. The coefficients β_{T+k} represent differential effects relative to the omitted lag of $T-1$. The size and significance of the lead coefficients β_{T-2}, β_{T-3}, etc can therefore be directly examined to test for any anticipatory effects. Table 6 reports the results from the leads and lags specification estimated for aggregate output, and Figure 3 plots the corresponding β coefficients and confidence intervals. The coefficients on the treatment lead variables are mostly small and statistically insignificant, and there is little indication of any increases in output that anticipate the privatization

event in the district, confirming our initial hypothesis.

Permutation test: Next, we implement a permutation test to provide an alternative assessment of the statistical significance of the results.[10] We first randomly assign privatization events across districts, while restricting the number of privatization events in each year to match the actual number of events in the data. We then estimate the difference-in-differences specification in Equation (2) for this placebo treatment assignment, and repeat this procedure 100 times. Figure 4 graphs the empirical cumulative distribution function (CDF) of the estimated treatment effects; the red vertical line marks the "actual" treatment effect that was obtained using the actual assignment of privatization events. The actual treatment effect lies well outside the range of the distribution of placebo treatment effects, providing a strong confirmation that our results are not a statistical artefact.

Robustness to controlling for background reforms: A potentially important threat to identification that we have not so far addressed arises from the fact that the privatization reforms overlapped with (and were in fact initiated at the same time as) a broader set of economic reforms that included tariff liberalization, FDI liberalization, and industrial deregulation (by means of abolition of the industrial licensing policy). If privatized districts were dominated by firms that were either heavily exposed or underexposed to these other reforms (relative to firms in districts that were privatized later), this would undermine our identification of the privatization reform. Given the industry-specific nature of the other reforms, we control for the effects of the background reforms by constructing a district-industry panel (whereas our previous analysis uses a district level panel). We estimate the following specification on the district-industry panel:

$$y_{jdst} = \alpha + \beta Privatization_{dt} + \boldsymbol{\gamma}\mathbf{X}_{jt} + \eta_d + \eta_j + \eta_{st} + \gamma_d t + e_{dst} \tag{4}$$

[10] See Chetty et al (2010) and Martin et al (2017) for analogous tests.

where y_{jdst} now denotes aggregate output for (3-digit) industry j in district d in year t; $\mathbf{X}_{jt}$ is a vector of time-varying industry-specific controls for (i) Output tariffs, (ii) Input tariffs, (iii) An indicator for FDI liberalization, and (iv) An indicator for licensing reform. The specification also controls for industry fixed effects (η_j). The specification can be strengthened further by allowing for industry x year fixed effects, η_{jt} - these will absorb all industry-specific shocks, including the effects of the background reforms. As before, the standard errors in the regression are adjusted for clustering at the district level.

The results are reported in Table 7. Column 1 reports the results of the specification with the background reform controls. The coefficients on the background reform variables are consistent with intuition as well as with the findings of the existing literature: Output increases sharply with a reduction in input tariffs, while an increase in output tariffs has a relatively small effect, echoing the findings of Harrison et al. (2012). Licensing reform has little effect on output, consistent with the findings of Aghion et al (2008),[11] as does FDI reform. Turning to the effects of privatization, the estimated effect remains statistically significant and retains its magnitude (approximately a 4% increase in output), and this conclusion is unaffected by the inclusion of industry x year fixed effects (Column 2). These results provide reassurance that our district-level identification strategy is robust to contemporaneous changes in other aspects of economic policy.

6.4 Between-sector labor reallocation

Our fundamental hypothesis was that privatization may result in a rationalization within the public sector that should effectively release workers to more efficient uses in the private sector (the latter, as we saw earlier, exhibits a higher marginal product of

[11] Instead, Aghion et al (2008) find that delicensing only has an effect in settings where labor regulations are less severe.

labor than the public sector). We now test this hypothesis in the data by estimating the effect of privatization on employment in the private and public sectors, respectively. The results are reported in Column 1-3 of Table 8 (Column 4-6 report the corresponding results for output). Consistent with our hypothesis, we observe an increase in private sector employment and output, while public sector output and employment register declines. The observed decline in public sector employment is consistent with other studies of privatization in India (Pratap 2011, Naib 2003), and can be partially explained by the fact that some privatized firms shed workers, but likely also reflects a freeze on new hiring in these enterprises, which would also have resulted in a relative reduction over time in the size of the public sector in each district.

The reallocational hypothesis can be tested with a more disaggregated analysis of the data. An intuitive hypothesis is that the workers shed by the public sector entity in a district would be more likely to be rehired in similar occupations elsewhere in the private sector (than in dissimilar occupations). We test this hypothesis by examining whether labor tends to flow towards firms that are "closer" to the public sector firm in terms of producing similar goods. To implement this test, we turn again to the district-industry panel. For each industry, we construct a measure of proximity to the public sector as the (absolute) difference between the 3-digit industry code of the industry and the industry code of the privatized firm in that district. We then estimate the following regression:

$$\begin{aligned} y_{jdst} &= \alpha + \beta_1 Privatization_{dt} + \beta_2 Privatization_{dt} \times Distance_{jd} + \beta_3 Distance_{jd} \\ &\quad + \eta_d + \eta_{jt} + \eta_{st} + \gamma_d t + e_{dst} \end{aligned} \tag{5}$$

where $Distance_{jd}$ denotes the proximity measure. Our hypothesis is that β_1 should be positive, while β_2 is negative, i.e. employment gains should be smaller the further away an industry is from the public sector firm. Table 9 reports the results of the regres-

sion, separately for output and employment.[12] Private sector employment and output increase by 9% in the industry of the privatized firm (for which the distance measure is zero), and this effect declines by approximately 0.2% for each unit of distance (i.e. an industry that is ten 3-digit codes away from the privatized industry will only see an employment gain of 7%, and so on). These results provide a sharp confirmation of the hypothesis that employment gains in the private sector reflect labor reallocation away from the public sector.

The discussion of the conceptual framework in Section 4 suggested that the extent to which the private sector can absorb the workers shed by the privatized firm is a function of the existing labor regime. In the Indian context, it is known that stringent employment protection legislation acts as a curb on hiring (see, for example, Adhvaryu, Chari and Sharma 2011): Private sector firms may be very reluctant to hire new workers if there are significant costs associated with downsizing in the future. We therefore test whether privatization results in a smaller increase in private sector employment in states characterized by more rigid labor laws. To implement this test, we rely on Besley and Burgess' (2004) coding of labor regulations to identify states that were classified as "pro-worker" in 1991 (prior to the reform) - these are states which have enacted significant restrictions on firing and dismissal of workers. We then test for heterogeneity in treatmnent effects along this dimension. Table 9.1 reports the estimated effects of privatization on private sector employment and output, separately for pro-worker and non pro-worker states. Consistent with our hypothesis, we find that the employment and output effects do appear to be smaller in magnitude (and not statistically significant) in pro-worker states, lending some substance to the idea that the aggregate effects of privatization depend on the features of the policy environment.

The conclusion from our various tests above is that there has been a significant

[12]Differently from the district-industry panel analysis in Table 6, employment and output are now only constructed for the private sector, in order to exclude outward labor flows from the public sector.

reallocation of labor from public to private sectors as a result of privatization. It is plausible, especially in light of our conceptual discussion, that this is the mechanism underlying the observed increase in economy-wide output. In the next section, we attempt to formalize the contribution of inter-sectoral labor reallocation to productivity growth. Before doing so, we consider here a second testable implication of the reallocational hypothesis: The extent to which labor reallocation increases net economy-wide output will depend on the productivity gap between public and private sectors. We test this hypothesis by constructing a measure of the difference between private sector and public sector worker productivity in the pre-reform period (i.e. pre-1991). We then divide districts into two categories based on whether they have above or below median productivity gap in the pre-reform period. We then ask whether the increase in aggregate output is greater in districts with an above-median baseline productivity gap. Table 9.2 presents the results: We do indeed find the effect on aggregate output is large and statistically significant in districts which exhibited a large productivity gap, whereas the effect is four times smaller and not statistically significant in the other districts.

6.5 Decomposing productivity gains

We now test the significance of the reallocational mechanism in terms of its contribution to the observed increase in aggregate output. To do so, we utilize the following decomposition of aggregate productivity suggested by Olley and Pakes (1993):

$$I_{dt} = \sum w_{idt}\phi_{idt} = E(\phi_{idt}|d,t) + \sum(w_{idt} - E(w_{idt}|d,t))(\phi_{idt} - E(\phi_{idt}|v,t))$$

where the index I_{dt} denotes district-level aggregate TFP at time t, and is defined as the labor-share weighted average of factory-level TFPs (in logarithms), where w_{idt} denotes the labor-share of the i-th factory in district d. The aggregate index is decom-

posed into two components: A component that measures the productivity of the average factory, $E(\phi_{idt}|d,t)$, and a "covariance" term that measures the extent to which size (measured here by labor share) is correlated with TFP.[13] As Bartelsman, Haltiwanger and Scarpetta (2013) show, the Olley-Pakes covariance term is a reliable measure of the efficiency of resource allocation that tends to vary systematically with changes in the economic regime. Our expectation is that privatization should result in an increase in the covariance term, as labor gets reallocated from low- to high-productivity factories. If productivity improves in the privatized firm, and/or if there are productivity spillovers to the other firms in the district, we may also observe an increase in average productivity (the first term in the Olley-Pakes decomposition).

We estimate the difference-in-differences specification on the separate terms of the decomposition to understand the sources of aggregate productivity growth. Table 10 reports the regression results. The point estimates indicate an aggregate TFP increase of approximately 10%, although this estimate is only statistically significant at the 10% level of significance. Remarkably, there is a significant increase in the covariance term, which accounts for nearly the entire increase in aggregate TFP. The remaining increase in aggregate TFP is attributed to an increase in average productivity, and this effect is not statistically distinguishable from zero. These results strongly suggest that the aggregate productivity effects of privatization arise from reallocations that improve aggregate productivity, in line with our basic hypothesis.

The reallocational hypothesis suggests more specifically that productivity gains flow from reallocation of labor between the private and public sector. To narrow in on this mechanism, we apply a decomposition of covariance that separates out the effect of labor reallocation within sectors from the effect of labor reallocation between

[13]The "covariance" term in this decomposition is really N times the covariance between labor shares and TFP, where N is the number of production units. That is, the decomposition can be seen to be a simple rewriting of the expression for the covariance between labor shares and TFP.

sectors:

$$\begin{aligned} cov^{OP} &\equiv \sum (w_{idt} - E(w_{idt}|d,t))(\phi_{idt} - E(\phi_{idt}|d,t)) \\ &= N * cov(w_{idt}, \phi_{idt}) \\ &= N * E[cov(w_{idt}, \phi_{idt}|s)] + N * cov(E(w_{idt}|s), E(\phi_{idt}|s) \end{aligned}$$

where s denotes sector (public or private), and we have now suppressed the conditioning on d and t in order to avoid notational clutter. The equation above shows that the Olley-Pakes covariance term can be decomposed into within-sector and between-sector components.[14]

Table 11 reports the results from estimating the difference-in-differences specification for each of the covariance components. Column 1 replays the results for the full covariance term, and Columns 2 and 3 display results for the within-sector and between-sector components, respectively. The separate effects are not well-estimated, but with that caveat, it appears that the increase in covariance is entirely accounted for by an increase in the between-sector covariance, implying in turn that the entire increase in aggregate productivity reflects a reallocation of labor between the private and public sectors. These results are a striking confirmation of the reallocational hypothesis.

7 Conclusion

In this paper, we take advantage of historical episodes of privatization in India to study the aggregate efficiency losses due to state-ownership of manufacturing enterprises. Our results indicate that privatization of a factory results in a substantial increase in aggregate output and productivity in the local district economy.

[14]This decomposition is sometimes referred to as the law of total covariance.

Our results constitute the first rigorous quantification of the effects of state-ownership on aggregate productivity, and confirm the importance of factor misallocation as a source of low aggregate productivity in developing countries, while focusing on a specific source of distortion that has not received rigorous evaluation. More directly, our study is an important contribution to the literature on SOEs that has so far largely focused on the implications of state-ownership and different modes of privatization on the productivity of the SOEs themselves. In doing so we fill a gap in the broader debate regarding the social desirability of privatization. First, our estimates suggest that the aggregate efficiency gains from privatization may in fact be more significant than the performance gains in the privatized enterprise. A notable aspect of the results is that in most cases of privatization in our data the state continued to retain majority ownership of the SOE, implying that even partial privatization is associated with significant rationalization, an implication that is consistent with the findings of Gupta (2005), Pratap (2011) and Bartel and Harrison (2005). Second, our results on labor reallocation are especially significant, given that the prospect of employment losses is a key issue surrounding privatizations in most countries, including India, where the phenomenon of "jobless growth" has particularly intensified the opposition to privatization. A caveat to these findings is that the largest employment losses are usually associated with full privatizations or asset sales (Pratap 2011), whereas the employment losses associated with partial privatizations (which are the focus of our study) are most modest, and there may accordingly be greater scope for laid-off workers to be absorbed into the private sector.

Our study is limited in scope in at least two important ways. First, we should emphasize that our results do not necessarily imply that privatization is the preferred alternative to state-ownership. Indeed, the literature has discussed a number of alternatives to privatization, including market deregulation, that may in some situations be just as effective as transfer of ownership of public sector firms (Vickers and Yarow 1992,

Trebilcock and Smith 1995, Bartel and Harrison 2002). Whether the broader package of market reforms initiated in India in the 1990s had a similar effect on public sector firms (and on their private sector neighbors) remains an open question. Second, we have focused on labor market distortions, but the external effects of privatization need not be limited to labour market channels. Privatization of upstream industries may impact the cost and quality of inputs to downstream industries, with implications for the latter's profitability. External effects flowing through forward linkages are likely to be of particular significance in countries where the public sector is heavily involved in primary industries, energy and infrastructure, as well as capital goods (as has been the case in countries following the Soviet model). Accounting for these external effects is a second important avenue for future research.

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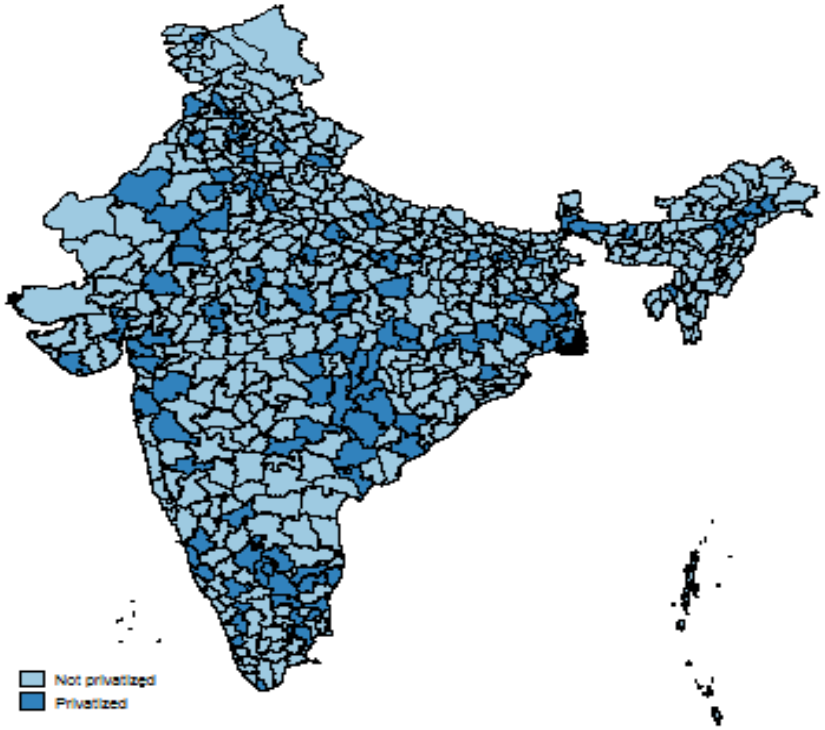

Figure 1: The map shows the location of privatized districts.

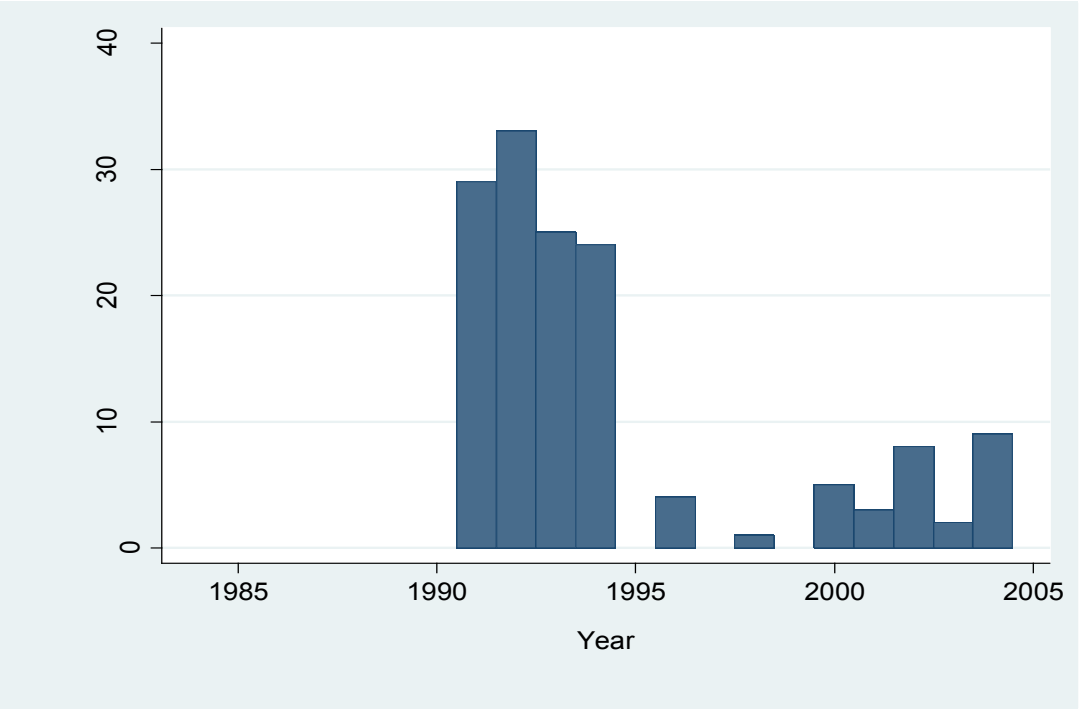

Figure 2. District-level privatization events in the sample period.

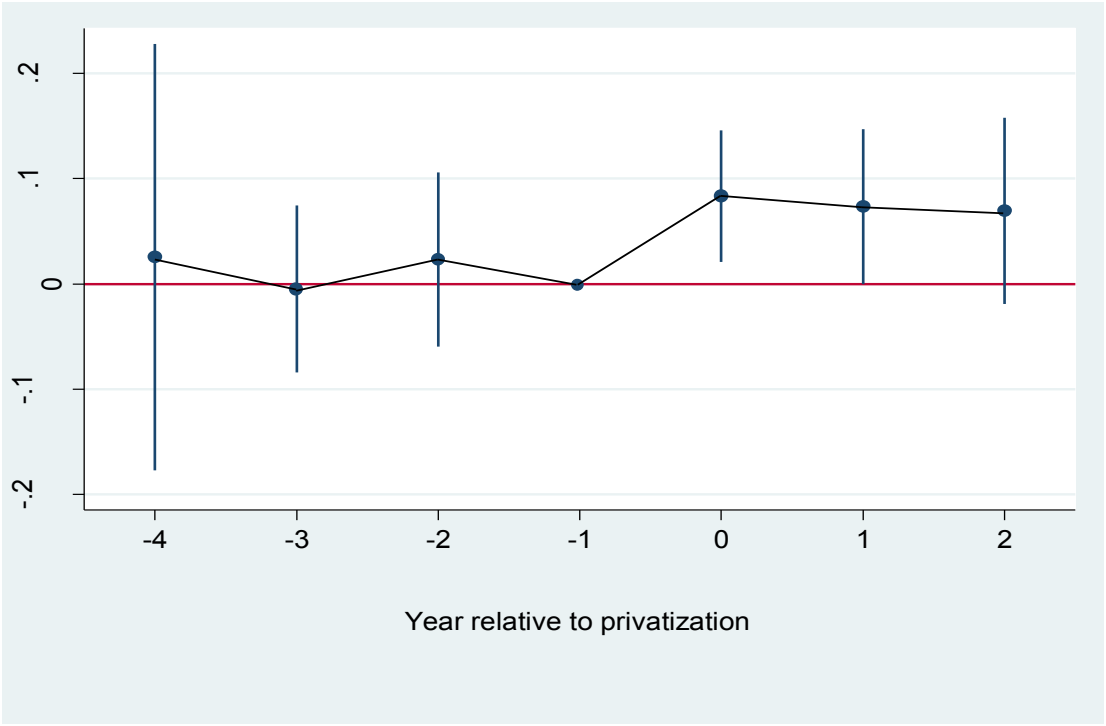

Figure 3. The figure plots the leads and lags of treatment effects on aggregate output, along with the associated 95% confidence intervals.

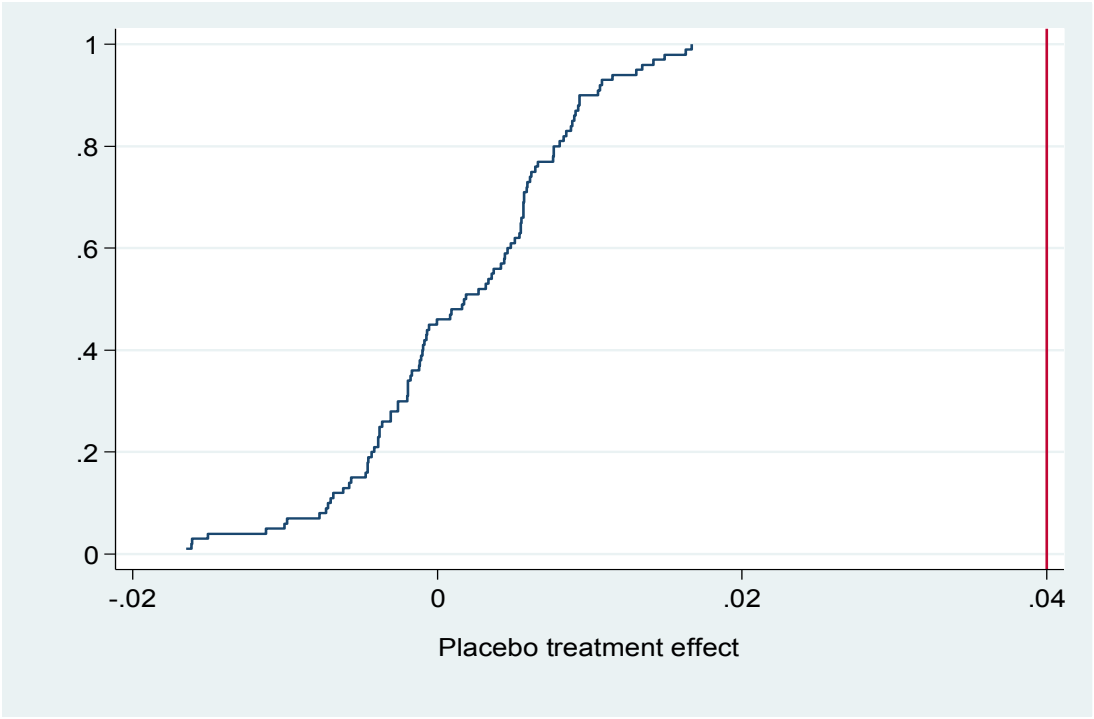

Figure 4. Empirical cumulative distribution function (CDF) of placebo treatment effects on aggregate output. The actual effect is shown by the red vertical line.

Table 1. CPSEs privatized in 1991-2004

Company	Mode of privatization
Andrew Yule and Company Limited	SIP
Bharat Earthmovers Ltd.	SIP
Bharat Electronics Ltd.	SIP
Bharat Heavy Electricals Ltd.	SIP
Bharat Petroleum Corp	SIP
Bongaigaon Refinery & Petrochemicals Ltd. (BRPL)	SIP
GAIL (India) Limited	SIP
HMT Ltd	SIP
Hindustan Cables Ltd.	SIP
Hindustan Copper	SIP
Hindustan Petroleum Corp	SIP
Hindustan Photofilms Manufacturing Co.	SIP
Hindustan Teleprinters	Asset sale
Hindustan Zinc Ltd.	SIP
HMT Ltd	SIP
Indian Petrochemical Corporation	SIP
Modern Food Industries	Asset sale
National Aluminium Co. Ltd.	SIP
National Fertilizers Ltd.	SIP
National Mineral Development Corp.	SIP
Neyveli Lignite Corp.	SIP
Oil & Natural Gas Corporation	SIP
Paradeep Phosphates	Asset sale
Rashtriya Chemicals & Fertilizers	SIP
Steel Authority of India	SIP
Travancore Fertilizer & Chemicals	SIP

Notes: the table lists Central Public Sector Enterprises (CPSEs) that were divested during the period 1991-2004. SIP refers to share-issue privatization, in which the government offers a share of equity in the CPSE to the public.

Table 2. Public vs private sector factories

	(1) Output	(2) Workers	(3) Capital	(4) TFP	(5) Marginal product	(6) Wage
Public	2.56***	2.42***	3.04***	-0.57***	-342.15*	1.02***
	(0.12)	(0.11)	(0.16)	(0.09)	(189.71)	(0.03)
Observations	277,371	272,658	284,834	263,156	283,000	278,340

Notes: Standard errors are clustered at district level. All variables are in logarithms, except for the marginal product of labor. The regressions control for district fixed effects and year fixed effects, and the sample is restricted to the pre-reform period 1985-1990.

Table 3. Summary statistics at district level

	Districts with no CPSEs		Districts with CPSEs but not privatized		Districts that experienced privatization	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Aggregate Output	0.18	0.26	0.37	0.40	0.45	0.45
Employment	0.83	2.03	1.48	1.73	1.62	1.69
Capital	0.08	0.14	0.17	0.29	0.24	0.42
No of firms	52.59	84.64	107.39	110.42	108.07	109.59
Public sector output share			0.10	0.20	0.12	0.22
Public sector employment share			0.10	0.17	0.10	0.18

Notes: The table presents means and standard deviations of district-level total output, employment, capital and number of firms, as well as the share of employment and output in the district accounted for by the public sector. Aggregate output and capital are measured in 10 billion rupees (base 1993); Employment is measured in units of 10,000 workers.

Table 4. Privatization and district outcomes

	(1)	(2)	(3)	(4)
	Output	Employment	Capital	No of factories
Panel A: All districts				
Privatization	0.019	-0.008	0.053	-1.786
	(0.019)	(0.058)	(0.036)	(2.883)
Observations	4,414	4,414	4,414	4,414
Panel B: Only districts with CPSE's				
Privatization	0.040**	0.028	0.070	1.302
	(0.020)	(0.081)	(0.045)	(3.818)
Observations	2,272	2,272	2,272	2,272
Panel C: Only districts that experienced privatization				
Privatization	0.046*	0.037	0.047	1.224
	(0.024)	(0.104)	(0.042)	(3.648)
Observations	1,128	1,128	1,128	1,128

Notes: All regressions include district fixed effects, state x year fixed effects and district-specific linear trends. Aggregate output and capital are measured in 10 billion rupees (base 1993); Employment is measured in units of 10,000 workers. Standard errors are clustered at district level. *Privatization* is a multi-valued treatment variable that counts the number of privatizations events in a district up to and including the current year.

Table 5. Alternative specifications

Dependent variable: Aggregate output	(1)	(2)
First privatization event	0.038	
	(0.033)	
Second privatization event	0.093*	
	(0.050)	
Third privatization event	0.211***	
	(0.068)	
First privatization event (SIP)		0.063*
		(0.033)
Second privatization event (SIP)		0.103**
		(0.049)
Third privatization event (SIP)		0.182***
		(0.055)
First privatization event (Asset sale)		-0.051
		(0.091)
Observations	1,126	1,126

Notes: Standard errors in parentheses clustered at district level. All regressions include district fixed effects, state x year fixed effects and district-specific linear trends. Aggregate output is measured in 10 billion rupees (base 1993). SIP refers to share-issue privatization, in which the government offers a share of equity in the CPSE to the public. Asset sale refers to an outright sale of a firm.

Table 6. Leads and lags of treatment effects

	(1)
	Output
Privatized (t-4)	0.025
	(0.101)
Privatized (t-3)	-0.005
	(0.040)
Privatized (t-2)	0.023
	(0.041)
Privatized (t=0)	0.083***
	(0.031)
Privatized (t+1)	0.073*
	(0.037)
Privatized (t+2)	0.069
	(0.044)
Observations	888

Notes: Standard errors in parentheses clustered at district level. All regressions include district fixed effects, state x year fixed effects and district-specific linear trends. Aggregate output is measured in 10 billion rupees (base 1993).

Table 7. Robustness to controlling for other reforms (district-industry panel)

Dependent variable: Aggregate output	(1)	(2)
Privatization	0.001**	0.002***
	(0.001)	(0.001)
FDI reform	0.001	
	(0.001)	
License reform	-0.001	
	(0.001)	
Input tariff	-0.031***	
	(0.005)	
Output tariff	0.002	
	(0.001)	
Observations	60,966	60,929

Notes: All regressions include district fixed effects, 3-digit industry fixed effects, state x year fixed effects and district-specific linear trends. The regression in Column 2 also controls for 3-digit industry x year fixed effects. Aggregate output is measured in 10 billion rupees (base 1993); Standard errors are clustered at district level. *Privatization* is a multi-valued treatment variable that counts the number of privatizations events in a district up to and including the current year.

Table 8. Between-sector reallocation of employment and output shares

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment			Output		
	Public	Private	Share public	Public	Private	Share public
Privatization	-0.451	0.073	-0.056**	-0.713	0.120	-0.051
	(0.805)	(0.098)	(0.024)	(1.496)	(0.129)	(0.033)
Observations	1,128	1,128	1,128	1,128	1,128	1,128

Notes: Standard errors in parentheses clustered at district level. All regressions include district fixed effects, state x year fixed effects and district-specific linear trends. The dependent variables in Column 1,2, 4 and 5 are in logarithms. Aggregate output is measured in 10 billion rupees (base 1993); Employment is measured in units of 10,000 workers. *Privatization* is a multi-valued treatment variable that counts the number of privatizations events in a district up to and including the current year.

Table 9. Proximity and reallocation (district-industry panel)

	(1) Employment	(2) Output
Privatization	0.090*	0.092**
	(0.051)	(0.039)
Privatization x Distance	-0.002**	-0.001***
	(0.001)	(0.000)
Distance	-0.001*	-0.001
	(0.001)	(0.001)
Observations	60,415	60,956

Notes: Standard errors in parentheses clustered at district level. All regressions include district fixed effects, 3-digit industry x year fixed effects, state x year fixed effects and district-specific linear trends. The dependent variables are in logarithms. Aggregate output is measured in 10 billion rupees (base 1993); Employment is measured in units of 10,000 workers. *Distance* is measured as the absolute difference between the 3-digit industry code of the district-industry cell and the 3-digit industry code of the privatized firm in that district. *Privatization* is a multi-valued treatment variable that counts the number of privatizations events in a district up to and including the current year.

Table 9.1. Labor and output reallocation, by labor regime rigidity

	(1) Labor	(2) Y	(3) Labor	(4) Y
Privatization	0.05	0.71	1.11	1.28*
	(0.23)	(0.46)	(0.76)	-0.68
Sample	Pro-worker states		Non pro-worker states	
Observations	342	342	565	565

Notes: All regressions include district fixed effects, state x year fixed effects and district-specific linear trends. Aggregate output and capital are measured in 10 billion rupees (base 1993); Employment is measured in units of 10,000 workers. Standard errors are clustered at district level. Privatization is a multi-valued treatment variable that counts the number of privatizations events in a district up to and including the current year.

Table 9.2. Aggregate output effects, by baseline productivity gap

Dependent variable: Aggregate output	(1)	(2)
Privatization	0.330	1.318**
	(0.580)	(0.606)
Sample	Below-median productivity gap	Above-median productivity gap
Observations	364	407

Notes: All regressions include district fixed effects, state x year fixed effects and district-specific linear trends. Aggregate output and capital are measured in 10 billion rupees (base 1993); Employment is measured in units of 10,000 workers. Standard errors are clustered at district level. Privatization is a multi-valued treatment variable that counts the number of privatizations events in a district up to and including the current year.

Table 10. Olley-Pakes decomposition of aggregate productivity

	(1) Aggregate productivity	(2) Average productivity	(3) Covariance
Privatization	0.075**	-0.007	0.082***
	(0.036)	(0.021)	(0.029)
Observations	1,128	1,128	1,128

Notes: Standard errors in parentheses clustered at district level. All regressions include district fixed effects, state x year fixed effects and district-specific linear trends. *Privatization* is a multi-valued treatment variable that counts the number of privatizations events in a district up to and including the current year.

Table 11. Within and between-sector reallocation

	(1) Covariance	(2) Within-sector covariance	(3) Between sector covariance
Privatization	0.082***	-0.092	0.174
	(0.029)	(0.165)	(0.153)
Observations	1,128	1,128	1,128

Notes: Standard errors in parentheses clustered at district level. All regressions include district fixed effects, state x year fixed effects and district-specific linear trends. *Privatization* is a multi-valued treatment variable that counts the number of privatizations events in a district up to and including the current year.

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