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Location-Based Tax Incentives: Evidence from India

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Abstract

While policies targeting particular geographic regions are widely used by governments, there have been few rigorous evaluations of their causal impacts. In this paper, I study the impact of a location-based tax incentive scheme in India. Using aggregated and firm-level panel data, I find large increases in employment, total output, fixed capital, and the number of firms as a result of the program. These increases are due to both the growth of existing firms as well as the entry of new firms. There is supporting evidence that the new firms entering the treated regions are larger and more productive. I find no evidence for relocation of firms or spillovers in industrial activity between treatment and control areas. Finally, using data from household surveys, I show that wages of workers rise but find no changes in housing rents or migration across the treated and control regions. My results therefore suggest that the policy increased welfare, and I also conclude that the policy was cost-effective. This provides support for "place-based" policies to correct for regional economic disparities, especially in settings with low labor mobility.

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I Introduction

Many countries in the world have massive economic disparities across regions. To reduce these regional inequalities, state and local governments often use "location-based" policies that seek to generate employment and productivity in particular regions.[1] These policies include tax exemptions, subsidies, land grants, and other infrastructural benefits to firms in order to incentivize them to locate to disadvantaged regions.[2] The benefits and distortions caused by these policies have long been debated by economists.[3] Whether such spatially targeted policies are able to generate economic gains in a cost-effective manner is largely an empirical question.

The empirical evaluation of location-based policies is complicated for three reasons. First, location-based policies are mostly directed towards underperforming regions (Neumark & Simpson (2015)) and because of this non-random policy placement, researchers need to carefully choose valid control groups to make comparisons with the treated areas. Second, these policies have both direct effects (on employment and output) and indirect effects (on local prices and migration), which require separate analysis. Finally, spillovers and relocation of economic activity between treated and untreated areas need to be taken in to account as they may bias the treatment effect estimates. Detailed micro data on firms, workers, migration, and local prices is needed in order to quantify their overall effects, and such data is often not available. This may partly explain the lack of empirical work assessing spatially targeted policies, especially in developing countries. I fill this gap in the literature by studying a place-based policy in India. Specifically, I examine the federally financed New Industrial Policy for the states of Uttarakhand and Himachal Pradesh. This policy provided tax exemptions and capital subsidies for new and existing firms starting in 2003, with the primary aim of inducing industrialization and generating employment in the two states.

The causal effect of the 2003 policy is identified using difference-in-differences (DID) and synthetic control methods [Abadie, Diamond & Hainmueller (2010)][4]. I use several different comparison groups, with varying levels of stringency, such as all major states, neighboring states, and bordering districts to ensure robustness in the identification strategy. To estimate the treatment effect on industrial outcomes, I use both an aggregated state-industry level dataset and a firm-level panel dataset. This allows me to look at the entry of new firms

[1] I use the terms location-based policies and place-based policies interchangeably.

[2] Some examples include Empowerment Zones and Enterprise Zones in the United States, Zones Franches Urbaines (ZFU) in France and Regional Selective Assistance in the United Kingdom.

[3] Theoretical arguments can be found in Glaeser (2001), Glaeser & Gottlieb (2008), Moretti (2011), and Kline & Moretti (2014*b*).

[4] Results for synthetic control methods are shown in the Web Supplement.

and the growth of existing firms.

At the aggregate state-industry level, I find large increases in employment (43 percent), number of factories (31 percent), total output (56 percent), fixed capital (71 percent), and industrial wage bill (41 percent) in treatment states relative to control states. Although the magnitude of the treatment effects are large, the results should be seen relative to the low industrial base in the two treated states before the policy came into effect. In terms of actual numbers, by 2007-08 (the last year in the data set), the policy had generated a total of around 33000 jobs, between 550-630 factories, and approximately 8 billion rupees in industrial wages. These results provide an estimate for the combined effect of entry of new firms and growth of existing firms as a result of the policy change. The firm-level results show the impact of the policy on existing firms. I find that on average, existing firms in the treated areas increased employment[5] (7.5 - 11 percent; from a mean of 39.63 employees before the policy change), output (8.7 - 18 percent; from a mean of around 110 million rupees before the policy change), and made additions to plant and machinery (25 - 28 percent) as compared to firms in control states.

Although the DID regressions show a differential effect of the policy on various outcomes, one concern is that the effects maybe driven by spillovers from the treated to the control regions. If the policy simply causes economic activity to relocate[6] from the control to the treated areas, then the estimated treatment effect might overstate the aggregate effect of the policy. Alternately, there could be positive spillovers due to agglomeration economies on the nearby control areas that might lead us to underestimate the effect of the policy change. I test for these channels explicitly by comparing outcomes in control regions located closer to the treated states relative to regions located further away and find no differential outcomes. I also test for differential firm closures across treated and control regions and whether multi-plant establishments are reallocating production across plants and find no evidence for these channels.

One economic justification for providing tax incentives to attract new firms is the possibility of agglomeration economies on the existing firms in the locality [Greenstone, Hornbeck & Moretti (2010), Kline & Moretti (2014*a*), Glaeser & Gottlieb (2008)]. If more productive firms enter a location, there might be positive spillovers on existing plants, leading to overall

[5]Busso, Gregory & Kline (2013) find that neighborhoods receiving EZ designation experienced between 13-19 percent increase in employment.

[6]Place-based policies have often been criticized for simply relocating economic activity across different locations without actually increasing aggregate output [Kline & Moretti (2014*a*), Glaeser & Gottlieb (2008), Mayer, Mayneris & Py (2012)].

growth.[7] I find that the policy change attracted larger and more productive plants to enter the treated areas. However, I do not find any differential effect on TFP (total factor productivity) for existing plants. One possible explanation for the lack of productivity spillovers could be that the existing firms take time to internalize the agglomeration economies generated by new plants. This paper however, only studies the short-term firm-level responses on productivity and hence might be unable to find evidence for such spillovers.

Having estimated the reduced form effects of the policy on industrial outcomes, I then analyze the impact on the local population. This is often complicated because place-based policies have general equilibrium effects. For instance, if workers are mobile, tax incentives for firms to locate to a particular region might be ineffective in raising real wages of residents. The increase in labor demand by firms and the consequent rise in nominal wages for residents might be partially or even completely offset by increases in housing rents and costs of living as new workers move into the area [Roback (1982), Moretti (2011)]. Hence, in order to get an estimate of the effects of the policy change on the local population, I combine data on wages and expenditures from household surveys with data on rents, migration, and a state-level price deflator. I find that real wages and real expenditure per capita increase (12 percent and 10 percent respectively) differentially in the treated areas, but there is no effect on housing rents. Theoretically, this is consistent with low mobility of workers across regions, as the increase in nominal wages following a labor demand shock is not offset by increases in the cost of living due to entry of new workers. I explicitly test for differential migration and find no statistically significant difference between the treated and control states. The results on low migration in India are also consistent with previous literature.[8]

Finally, I conduct a back-of-the-envelope quantitative assessment using the estimates from the paper for this location-based tax incentive scheme. I conclude that the policy was cost-effective as the gains in profits for firms and the total wage bill for workers in the treated states outweigh my estimates of the costs (including both actual costs of subsidies and foregone tax revenues).[9] My estimates suggest a gain of 6.5-21.3 billion rupees (102.3-335 million USD), which is around 0.1-0.4 percent of the GDP (in 2007-08) in the two states relative to what would have happened in the absence of it. The magnitude of these gains seem reasonable and add credibility to the treatment effects estimated in the paper.

[7] For example, when the elasticity of agglomeration with respect to economic density in the receiving region is higher, reallocating economic activity from one region to another leads to a long run increase in output [Glaeser & Gottlieb (2008)].

[8] See Munshi & Rosenzweig (2009), Topalova (2010), and Hnatkovska & Lahiri (2013).

[9] The deadweight loss in this setting where workers are not very mobile will be low and most of the benefits will accrue to local workers. [Busso, Gregory & Kline (2013)].

Prior empirical work evaluating place-based policies has primarily focused on developed countries, mostly in the United States and Europe. In the United States, the focus has been on Federal Empowerment Zones (EZ) and State Enterprise Zones (ENTZ); these are neighborhoods receiving tax breaks and job subsidies. The results on the efficacy of these zones in creating jobs have been mixed.[10] Other recent papers on place-based policies have studied programs in European countries. These include the "Regional Selective Assistance" in the United Kingdom [Criscuolo et al. (2012)], the French ZFUs [Mayer, Mayneris & Py (2012), Givord, Rathelot & Sillard (2013)] and Italy's Law 488/1992 [Bronzini & de Blasio (2006)]. My paper contributes to this growing literature by rigorously evaluating the incidence and welfare impacts of a location-based policy in a developing country. All other papers focusing on developing countries have studied the impact of Chinese SEZs [for example, Wang (2013), Cheng (2014), Lu, Wang & Zhu (2015) and Alder, Shao & Zilibotti (2016)]. I extend this relatively new literature studying location-based policies in developing countries by conducting an overall assessment of the policy change using a comprehensive set of outcomes (both detailed industrial- and household-level). I also carry out a cost-benefit analysis of the policy using my estimates and add to the small recent literature on local labor markets that studies the overall costs and benefits of place-based policies [Busso, Gregory & Kline (2013), Kline & Moretti (2014*a*)].

This paper also contributes to the literature on firms' location decisions in response to tax differentials. Duranton, Gobillon & Overman (2011) find a negative impact of local taxes on firm employment but no impact on firm entry in the United Kingdom. Rathelot & Sillard (2008) look at French micro data and find a weak response of firms' location decisions to higher taxes. In contrast, I document a large increase in the entry of firms as a result of the tax exemptions.

The results of this paper, thus, inform policy-makers about the efficacy of tax benefits for industrializing backward regions. In this context, I observe large responses of firms to tax benefits, but very little migration response by individuals. This suggests that it might be easier to provide incentives for firms to move to a particular location than to move people. Especially in settings with low labor mobility, such spatially targeted policies could be a cost-effective way to generate employment, output, and real earnings gains for workers.[11]

[10] Neumark & Kolko (2010), Greenbaum & Engberg (2004), and Bondonio & Greenbaum (2007) find no effects of enterprise zones on employment growth. However, Ham et al. (2011) find positive effects for EZs, ENTZs, and Federal Enterprise Communities. Busso, Gregory & Kline (2013) also find that the EZ program increased employment and wages inside the zones at moderate efficiency costs.

[11] Kline & Moretti (2014*b*) also note that "..in the idealized model..the most efficient demand side subsidy was one that yielded no mobility response at all and simply raised local wages."

The rest of the paper is organized as follows. Section II presents the background for the study and the details of the policy. Section III discusses the empirical strategy, Section IV describes the data, and the results are discussed in Section V. Finally, Section VI conducts a cost-benefit analysis and Section VII concludes.

II Background and Policy Details

Himachal Pradesh and Uttarakhand are two states in the north of India (see Figure 1). In November 2000, the northwestern districts of Uttar Pradesh were split off to form the state of Uttarakhand. After the formation of Uttarakhand, it was placed in the list of "special category" states that included Jammu and Kashmir, Himachal Pradesh, Arunachal Pradesh, Assam, Sikkim, Manipur, Meghalaya, Mizoram, Nagaland, and Tripura. These states get preferential treatment in terms of federal assistance. The basis on which a state is enlisted as a "special category state" includes hilly and difficult terrain, very low level of infrastructural development, low population density, significant tribal population, and strategic location with borders with neighboring countries. This decision is made by the National Development Council that comprises the Prime Minister, Union Ministers, Chief Ministers, and members of the Planning Commission.[12]

Both Uttarakhand and Himachal Pradesh are two of the smaller states in India, together covering roughly 3.5% of India's total area. They are predominantly covered by hilly areas and forests.[13] According to the 2001 Census, the total population of Himachal Pradesh and Uttarakhand was around 6.1 million and 8.5 million respectively (around 1.4% of India's total population). Industrialization was considered a policy challenge in the two states, owing to the topography. For instance, in 2000, the two states together accounted for less than 1% of the number of factories and industrial output in India. Furthermore, these states share international borders with China and Nepal. Beginning 2003, the Government of India (central government), in order to attract industrial investments and generate

[12]Note that the Bharatiya Janata Party (BJP) was the majority party in the central government in 2003, whereas the Indian National Congress (INC) was in power in the states of Uttarakhand and Himachal Pradesh until 2007. Thus in 2003, the Prime Minister and the Chief Ministers of the two states were from different parties. This seems to suggest that the policy was not directed to states that had the same party in power at the state-level as at the central level. All available sources seem to suggest that the decision to include these states in the "special category" list was made on the basis of the guidelines mentioned in the text.

[13]According to India State of Forest Report, 2011, forest area covered 66.5% of the area of Himachal Pradesh and 64.8% of the area in Uttarakhand.

employment in the states of Uttarakhand and Himachal Pradesh,[14] decided to provide the following incentive package:

I. New industrial units set up in 'designated' industrial estates/growth centers were entitled to:

(a) 100% excise duty exemption for a period of 10 years from the date of commencement of commercial production.

(b) 100% corporate income tax exemption for an initial period of five years and thereafter between 25-30% for a further period of five years.

(c) all new firms and existing units (upon substantial expansion) in the notified locations would be eligible for capital investment subsidy equaling 15% of their investment in plant and machinery, subject to a ceiling of Rs. 3 million (approximately USD 50,000).

All of these exemptions (a), (b), and (c) were available to existing industrial units depending on their "substantial expansion", i.e. if they increased the value of fixed capital investment in plant and machinery by at least 25%.

II. A list of 'thrust sector' industries was compiled that would be eligible for the benefits listed above irrespective of whether they located in an industrial estate or not.

These tax exemptions pertained to the taxes collected by the central government. In general, companies resident in India are taxed on their worldwide income arising from all sources at corporate income tax rates between 30% (for domestic corporations) and 40% (for foreign corporations). Central excise duty rates varied between 8%-16%. These tax exemptions were large enough to incentivize firms to enter the states.

The main distinction between the thrust sector industries and other industries related to their location within the state. Essentially, to receive the incentives, a firm in a thrust sector industry could open up anywhere in the state, whereas a firm in a non-thrust sector industry needed to open up in a "designated" area. However, a few months after the policy (in June 2003) was initiated, the Government of India issued a notification[15] designating the areas in the two states where industrial units (in non-thrust sectors) would be eligible to get these tax incentives. The notification included (i) existing industrial estates (ii) proposed industrial estates (iii) industrial activity in non-industrial area and (iv) expansion of existing

[14] According to the Ministry of Commerce and Industry, "*the objective of the scheme is to create an enabling environment for industrial development to provide a fillip to private investment in these States. Private entrepreneurs will be encouraged to set up more industrial units in Himachal Pradesh and Uttarakhand, leading to overall growth and industrialization of these States and generation of more employment.*"

[15] See Notification No. 50/2003 - Central Excise, Dated: June 10, 2003, available at http://himachal.nic.in/industry/welcomelat.htm

industrial estates. The notification through (ii), (iii) and (iv), made almost all of the existing industrial activity prior to 2003 and surrounding areas eligible for the benefits, and also added new areas. In practice, the notification made almost all areas where industrial activity was possible, a "designated" area. This meant that there was virtually no differential benefit to a firm in a thrust sector industry in these two states in terms of receiving the incentives.

The central excise tax exemption was removed on 31st March 2010, and the income tax exemption was removed on the 31st of March 2012. Essentially, any new industrial units set up or existing units undertaking substantial expansion in these states prior to the above dates would continue to be eligible for these benefits.

III Empirical Strategy

In this paper, I empirically test whether the centrally sponsored location-specific tax incentives led to differential increases in industrial outcomes in the treated areas as compared to control areas. The empirical strategy uses the 2003 policy change that provided tax incentives to firms in the two states of Uttarakhand and Himachal Pradesh in a difference-in-differences setup. I use this state-year variation to compare outcomes before and after the policy change (2003) in the treated areas to a set of control units. To the best of my knowledge, no other policy was implemented in these two states beginning 2003 that affected industrial outcomes differentially more or less than in other states, and this helps me to identify the treatment effect of the particular tax incentive scheme. Many other policies such as the Electricity Act 2003, and the Special Economic Zones Act of 2005, were national policies affecting the entire country and should not have affected industrial outcomes in the two states differentially. Furthermore, between 2003-07, there were no state assembly elections and the Indian National Congress (INC) remained in power in both states. The national parliamentary elections took place in 2004, and should not have resulted in differential industrial outcomes in the two states as compared to other states.

Ideally we would like to compare the treated states to an observationally similar control group. I consider a few different control groups for the analysis. I compare industrial outcomes in the treated states to all major states taken together and then to a set of neighboring states. The most stringent specification compares outcomes of firms in districts located on either side of the borders in the treated and control areas. This is a strict test on the identification, as districts on either side of the border tend to be more similar as compared to geographically distant locations. Finally, I also perform robustness checks using the synthetic control method where the control group is formed using a weighted average of all the non-treated states in order to best match the treated states. For all outcome variables, the synthetic control group comprises states both near and away from the treated states.

Indian firm-level data sets do not provide exact location identifiers (to the level of street address and zip codes) below the district level. Since each district in the two treated states had at least one designated area that was eligible for the incentives, an empirical strategy comparing firms or industries across districts within the treated states would not be possible.[16] Following visits to the respective state industry departments, it became clear that the notification brought almost all existing industrial activity within the ambit of the policy change and also added new areas. Therefore, unlike Mayer, Mayneris & Py (2012), the closing down of existing firms in ineligible areas to re-open in an eligible industrial area *within the state* is not a concern here.

It is thus reasonable to consider this policy as affecting the entire states of Himachal Pradesh and Uttarakhand. In this paper however, it will not be possible to separate out the effects of the tax incentives from the capital subsidy provided.

Before looking at regression specifications, Figures 2 and 3 plot the raw data over time for the variables of interest at the state-industry level. These plots show that the pre-2003 trends in employment, number of factories, total output, and fixed capital were similar across the treated and control states, only diverging after 2003. The pre-treatment trends look parallel and provide visual support to the use of difference-in-differences (DID) strategy in this context to estimate the causal effect of the policy change.[17]

I run two main types of regressions (DID specifications) to estimate the treatment effect of the policy change on industrial outcomes. First, I run *state×3-digit industry level* regressions of the form:

$$y_{sjt} = \delta_s + \lambda_{jt} + \beta_1(post_t \times treat_s) + \gamma_1(X_{st}) + \delta_s t + \varepsilon_{sjt} \quad (1)$$

where s,j,t indexes state, 3-digit industry, and time respectively, y_{sjt} represents an outcome variable such as employment, number of factories, total output, fixed capital, or industrial wage bill that varies at the state, industry, and year level, δ_s represents state fixed effects, λ_{jt} represents industry-year fixed effects, and X_{st} represents time-varying controls.[18]

[16] I use the terms industrial estate and designated area interchangeably because many non-industrial areas with existing industrial activity prior to 2003 were included as eligible areas for the policy through Notification 50/2003.

[17] The trends look similar even on adding all states. To be visually clear, these graphs only show the trends in the nearby states.

[18] Note that in all state-industry regressions, the control group is either neighboring states or all major states. District identifiers are not available in this dataset.

Note that the time-varying controls include *pre-treatment* state-level variables from the 2001 Census such as population, number of industrial and agricultural workers, number of main and marginal workers and number of literate people in the state, and I interact them with a time dummy for each year. One concern with the non-random placement of the policy (in these two states) is related to the differential pre-trends in different outcome variables and it is possible that the observed effects are due to these pre-trends rather than the policy itself. To address this concern, I also include state-level trends ($\delta_s t$) to control for differential pre-trends in the outcome variables in different states. The coefficient β_1, on the interaction term $post_t \times treat_s$, where

$$post_t = \begin{cases} 1 & \text{if year is 2003 or after} \\ 0 & \text{if year is pre 2003} \end{cases}$$

$$treat_s = \begin{cases} 1 & \text{if state is Uttarakhand or Himachal Pradesh} \\ 0 & \text{otherwise (control states),} \end{cases}$$

is then the causal effect of the policy change.[19] The regressions with employment, output, wage bill, and fixed capital therefore combine both the extensive (entry and exit of firms) and intensive margin (growth by existing firms) of the policy change. The regression with number of factories as the regressand gives us the extensive margin directly and is a cumulative effect that takes into account both entry and exit of firms.

The next set of regressions are at the *firm-level*:

$$y_{idjt} = \alpha_i + \lambda_{jt} + \beta_2(post_t \times treat_d) + \gamma_2(X_{idjt}) + \varepsilon_{idjt} \tag{2}$$

where i,d,j,t indexes firm, state or district (depending on choice of control group),[20] 4-digit industry and time respectively, and y_{idjt} represents a firm-level outcome variable such as employment, output, fixed capital, additions to fixed capital, additions to plant and machinery, or wage bill. I also use age and age-squared as controls in the regressions.[21] Note that the inclusion of firm fixed effects removes the effect of new firms entering after 2003. Hence, this regression looks at the impact of the policy change on the outcome measures

[19] These regressions are similar to the area level employment and number of plants regressions in Criscuolo et al. (2012).

[20] For the firm-level data, district identifiers are available and thus the control groups include neighboring states or bordering districts.

[21] Since a particular firm does not change location in the dataset, firm fixed effects subsume the state or district fixed effects.

only for incumbent firms and can be interpreted as the intensive margin of the policy change.

While the DID regressions estimate the differential effect of the policy between the treatment and control areas, it is conceivable that the results are being affected by changes caused by the policy in the control areas. For example, relocation of firms from the control states to the treated states might lead us to wrongfully attribute the observed effects as being caused by the policy change. To check whether firms close down in control states to reopen in the treated states, I take three approaches. First, I look at trends in the number of operational factories in the treated states and the neighboring control states. Then, I run a regression at the state-industry level with the number of closed firms[22] as the dependent variable to look at the differential impact on firm closures across the treated and control states, before and after the policy change.[23] Finally, I run regressions comparing the impact of the policy change in neighboring states to states further away from the treated states. The underlying assumption is that firms closer to the treatment states would be more likely to relocate production into those states in response to the policy. If there is substantial relocation, we would expect to see lower industrial activity in neighboring control states relative to states that are further away from the treated areas. Rather than closing down an existing plant in a control area and reopening in the treated states, a multi-establishment firm might move production between its various plants to take advantage of the tax benefits. To rule this out, I also run regressions that omit multi-establishment firms.

A related concern might be that the policy induces spillovers in the nearby control areas. Positive spillovers in industrial activity from the treated states to the neighboring control states would lead us to underestimate the effect of the policy change. Such externalities might be substantial, especially in control districts bordering the treated districts, and may lead to a differential response on firms in districts nearer to the treated states relative to those further away. To check whether the firm-level results are being influenced by spillovers, I run a regression specification comparing the firms along the border in the treated states to those in districts further away from the border in the control states (essentially omitting the bordering control districts from the regression). I also run a specification to see the effect of the policy change on firms in bordering control districts compared to firms in districts further away in the control states.

Finally, to look at the effects of the policy on wages, rents, and migration, I run regressions of the form:

[22]I define the number of closed firms as the difference between the total number of firms and the number of operational firms.

[23]If firms are relocating to the treated states, we would expect to see a larger number of firm closures in the control states as compared to the treated states.

$$y_{kdst} = \delta_d + \lambda_t + \beta_3(post_t \times treat_s) + \gamma_3(X_{kdst}) + \varepsilon_{kdst} \tag{3}$$

where k,d,s,t indexes household or individual, district, state and time respectively. y_{kdst} represents wages or migration status in the individual-level regressions and rents or monthly per capita expenditure in the household-level regressions. For the individual-level regressions, I control for age, sex, marital status, education status, and the industry of work. The regressions with housing rents use attributes of the house as controls, such as roof type, dwelling type, floor, number of rooms, and area.

IV Data

I combine data from multiple sources to evaluate the impact of the policy change. For industrial outcomes, I use two datasets: (i) the Annual Survey of Industries (ASI) state×3-digit industry panel (from 2000-01 to 2007-08) and (ii) ASI firm-level panel (2000-01 to 2007-08). To study the effects on individual and household outcomes, I use (a) Employment-Unemployment rounds of the National Sample Survey (NSS) for the years 1999-2000, 2004-05, 2005-06 and 2007-08 and (b) Housing Conditions rounds of the NSS for the years 2002 and 2008.

The Annual Survey of Industries (ASI), conducted by the Ministry of Statistics and Program Implementation (MoSPI), is the main source of industrial statistics in India. The ASI covers the entire Factory Sector comprising industrial units (called factories) registered under the Sections 2(m)(i) and 2(m)(ii) of the Factories Act, 1948. This includes all firms employing 10 or more workers using power and 20 or more workers without the use of power. Geographically, it covers the entire country except the states of Arunachal Pradesh, Mizoram, and Sikkim, and Union Territory of Lakshadweep for the surveys. The ASI dataset is well-suited to answer this question as it covers formal sector firms that are affected by tax changes.

For the state-industry level regressions, I use the ASI state×3-digit industry panel. Each observation is at the state-industry-year level. Industries are classified at the 3-digit National Industrial Classification (NIC) codes. This data set includes 65 industries (3-digit NIC), 8 years (2000-01 to 2007-08) and all major states. Table 1 Panel A shows descriptive statistics for the variables of interest at the state×3-digit industry level. As Table 1 Panel A shows, the two treated states of Himachal Pradesh and Uttarakhand had smaller industrial employment, number of factories, total output, fixed capital, and wage bill as compared to neighboring states or the rest of India before 2003. For example, average employment size in a 3-digit industry before 2003 in the treated states was 590 as compared to the figure for all the major states together (4952). Post-2003, the average size of industrial employment goes up throughout India, but the increase is highest in the treated states. Similar increases can be seen for number of factories, total output, and fixed capital after 2003 in the treated states as compared to other states. Mean total output and fixed capital at the state industry level rises almost three-folds in the treated states after 2003, much larger than the increase in the other states.

For the firm-level regressions, I use the ASI firm-level panel for the years 2000-01 to 2007-08. The ASI frame is divided into census (surveyed every year) and sample (sampled every few years) sectors. However, the definition of the two sectors has changed from time to time. Five industrially backward states[24] are always covered in the census sector. For the rest of India, the definition of the census sector has changed from 200 or more employees (1998-2000) to 100 or more employees (2001 onwards). To take into account the changes in the sampling frame, I run firm-level regressions using the sampling weights provided by ASI. I restrict the sample to the major states and union territories of India as covered by the ASI.[25]

"Firm" in this context means a factory, the unit of observation in the data set. Table 1 Panel B shows summary statistics for the different outcome variables at the firm-level broken up by treated states, major states, and neighboring states for periods before and after the policy change. I use the sampling weights from the data set to construct the summary statistics for the estimated population.[26] Average employment within the firm increases post 2003, irrespective of which group we look at. Median employment after 2003 however, increases by almost 56% for firms in the treated state whereas the increase is negligible for firms in the rest of the country. Mean output and fixed capital almost double for firms in the treated group after 2003. This increase is much larger as compared to any other group.

To study the welfare effects of the policy change, I use migration, wages, and house rents data from the National Sample Survey (NSS). NSS is a nationally representative household survey in India, also conducted by the MoSPI. Specifically, I use rounds 55 (1999-2000), 61 (2004-05), 62 (2005-06), and 64 (2007-08) of the employment-unemployment surveys of the NSS for the wages data. The survey provides information on wages and employment for each household member over the last seven days before the interview. To study migration, I use

[24]Manipur, Meghalaya, Nagaland, Tripura and Andaman and Nicobar Islands.

[25]I do not include Jammu & Kashmir or the states in the North-east namely Assam, Manipur, Meghalaya, Nagaland, and Tripura.

[26]See Harrison, Martin & Nataraj (2013) and Bollard & Sharma (2013).

NSS Rounds 55 (1999-2000) and Round 64 (2007-08)[27] - one round each before and after the policy change. The survey elicits information about the last usual place of residence for the household members. I define an external (internal) migrant as one whose last usual place of residence was another state or country (same state but another district). To look at the effect of the policy on rents, I use two NSS rounds of the Housing Conditions schedule for the years 2002 (round 58) and 2008 (round 65). These rounds include questions on housing rents and the attributes of the house such as total floor area, kitchen type, floor type, number of rooms, type of roof, and type of dwelling. Finally, I construct a state-level GDP deflator to deflate nominal values using the state GDP at constant and current prices from the Reserve Bank of India (RBI) Handbook of Statistics on the Indian Economy .[28] Summary statistics for these NSS data are shown in Table 1 Panel C.

V Results

I begin by reporting the results for the difference-in-differences regressions at the state-industry level for different outcome variables (Subsection V.1) and then look at the firm-level results (Subsection V.2). Readers interested in the synthetic control methods results are referred to the Web Supplement of the paper. In Subsection V.3, I discuss results on productivity and finally results on wages, rents, and migration are discussed in subsection V.4.

V.1 State-industry results

The state-industry DID regression results are reported in Table 2. Each of the panels from A to E, show the difference-in-difference results for the dependent variables mentioned next to them - Log of employment (Panel A), Log of total factories (Panel B), Log of total output (Panel C), Log of fixed capital (Panel D), and Log of wage bill (Panel E) in a 3-digit industry in a particular state. Table 2 uses all the major states of India as the control group. These include Haryana, Punjab, Uttar Pradesh, Chandigarh, Delhi, Rajasthan, Bihar, Andhra Pradesh, Chhattisgarh, Maharashtra, Madhya Pradesh, Orissa, Goa, Kerala, Karnataka, Tamil Nadu, Jharkhand, Gujarat, and West Bengal. Similar DID regressions with neighboring states as the control group are shown in Table Appendix A3. Neighboring states include Haryana, Punjab, Uttar Pradesh, Chandigarh, and Delhi. All the results in

[27] Only these two rounds have information on migration for the relevant time frame.
[28] See Appendix Table A2.

this subsection can be interpreted as the cumulative effect of the growth of existing firms and the entry of new firms at the state-industry level.

In Table 2, Column 1 includes state, year, and 3-digit industry fixed effects. State fixed effects control for time invariant state characteristics like the area and topography of the state. The year fixed effects control for macroeconomic shocks affecting all states and the industry fixed effects control for time invariant industry characteristics. Column 2 is a more flexible specification as it includes industry-year fixed effects which control for time-varying industry characteristics. This is important because some industries like pharmaceuticals and IT (information technology) have grown in India over the last decade, and the industry-year fixed effects controls for these changes.[29] Column 3 adds time-varying controls at the state level to the specification in Column 1. I include pre-2003 state-level variables from the 2001 Census such as population, agricultural, and industrial workers, number of main and marginal workers and number of literate people in the state and interact them with a time dummy for each year as control variables. Since these state-level variables were measured before the policy came into effect, they should not have been affected by the policy. The interaction of these state-level variables with a time dummy for each year is a flexible way to include these as controls in the regression. Column 4 includes the specification of Column 2 with time-varying controls. Finally, to control for the possibility of differential pre-trends in the outcome variables, I control for state-specific trends in column 5. For each regression, standard errors are clustered at the state level. Since the policy change only affected two states and number of state clusters are small, inference using standard cluster-robust techniques may lead to over rejection. Hence, I also report the wild cluster bootstrap-t p-values [Cameron, Gelbach & Miller (2008)] for the regressions in square brackets in the table.[30]

The dependent variable in Panel A is the log of employment at the 3-digit industry level. Mean employment at the industry level differentially increases in the treated states relative to the control states by around 43% - 45%. In columns 1 to 4, the coefficient of interest on the interaction *post*treat* is positive and significant at the 1% level with standard errors clustered at the state-level. These coefficients also remain significant at the 5% level when I use wild cluster bootstrap-t p-values. Column 5 controls for state-specific pre-trends. The coefficient on *post*treat* is smaller (34%) than in the other columns. However, the wild cluster bootstrap-t p-values are larger (p = 0.14) making the effect statistically insignificant. The magnitude of the treatment effect in column 5 is in the same ballpark as the coefficients

[29] A related concern might be that the industrial composition in the treated states is different than the control states and to control for this, I also run regressions controlling for state×industry fixed effects and find similar results.

[30] Also see Busso, Gregory & Kline (2013).

from the other columns, and this provides evidence that the observed effects are due to the policy change and not due to differential pre-trends. Corresponding results are shown in Panel A in Table Appendix A3 when neighboring states are used as the control group. The magnitudes of the treatment effect are similar and do not depend on the choice of the control group. This translates into approximately 33,000 additional jobs in the treated states in 2007-08 (the last year in the dataset) compared to what would have happened in the absence of the policy.

Panel B looks at the same specifications for log of total number of factories as the dependent variable. In this table, the coefficient on *post*treat* can be interpreted as the extensive margin of the policy change as it takes into account new entry by firms as a result of the policy change. Columns 1 through 4, show that the effect of the policy change on the average number of factories in an industry (in treated states relative to control states) is between 31% and 32%. The addition of state-level trends does not alter the magnitude of the treatment effect, although the wild cluster bootstrap-t p-values are large (p = 0.221). Panel B confirms that the policy change led to a large differential increase in the number of new firms coming in to the treated states relative to control states. These estimates translate into a total additional increase of 550-630 firms in the treated states in 2007-08. Table Appendix A3 Panel B shows corresponding DID results with the neighboring states as the control group.

Panel C reports the results for log of total output at the state-industry level. The effect of the policy change on total output (in treated states relative to control states) is even larger than the effect on total employment, ranging between 56% and 64% (columns 1 through 4). State-specific trends make the output results smaller (47.3%) with the wild cluster bootstrap-t p-value of 0.146. Results for log of fixed capital are shown in Panel D. The results show an increase of 71% in fixed capital and part of this can be attributed to the "substantial expansion" clause where existing firms needed to increase their investment of fixed capital by at least 25% to receive the tax exemptions. Furthermore, capital investment subsidies were also provided in these two states after 2003 to both new and existing firms, contributing to the massive increase in fixed capital at the industry level. Finally in Panel E, I show that the industrial wage bill differentially increases by 41% in the treated states as compared to the control states. For both fixed capital and industrial wage bill, the inclusion of state-specific trends lowers the magnitude of the treatment effect but is not substantially different from the coefficients estimated in columns 1 through 4. Across panels A to E, the inclusion of state-specific pre-trends does not significantly alter the magnitudes of the treatment effects and this provides some credence to the effects of the policy change.

The estimated coefficients over time along with standard errors at the 95% confidence level are plotted in Figure 4. These coefficients are obtained from a regression of the outcome variable (log employment, log factories, log output, log fixed capital, log wage bill, and log profits) on the interaction between *treat* (indicator variable for treated states) and time dummies after controlling for state, year, and industry fixed effects. Plotting these coefficients over time is one way to test for the validity of the parallel trends assumption for the difference-in-differences estimation. These graphs visually show that before 2003, there were no trends in the outcomes and the effects only show up after 2003. These graphs also provide visual evidence for the treatment effect of the policy change and show that the effect increases every year after 2003.

There may be some concern that firms close down in the neighboring control states to reopen in the treated states to take advantage of the tax incentives. To check for this, I take the following steps. First, I plot the number of operational factories in the treated states and the neighboring control states. If the policy change in 2003 caused factories to close down in the neighboring control states and reopen in the treated states, there should be a decline in the number of operational factories in the neighboring states. Figure 5 plots the trends in operational factories and there is no evidence that more factories closed down in control states compared to treated states. To check for differential closure of firms across treated and control states, I run difference-in-differences specifications with the number of firm closures as the dependent variable in Table 3.[31] I find no differential response in terms of firm closures across the treated and control states. In Table 4, I run regressions similar to placebo checks. I remove the treated states from the sample and run regressions assuming that the neighboring states got treated by the policy change. The underlying assumption is that the relocation of factories, workers, and capital is easier from nearby places as compared to regions further away from the treated states. Hence, the policy change should have differentially impacted neighboring states as opposed to states further away from the treated states. The results in Table 4 compare outcomes in the neighboring states to other major states before and after the policy change. There is no statistically significant systematic difference in the outcomes between the neighbors and all other states. Overall, there is no evidence of differential closure of firms or relocation of industrial activity across the treated and control states.

However, firms in control states might re-direct capital and produce output across their different plants to take advantage of these tax incentives without closing down. To rule out this channel completely, I would need to check whether multi-establishment firms are driving

[31] The number of closed firms is the difference between the total number of firms and the number of operational firms.

the results. I discuss this issue along with the firm-level results next.

V.2 Firm-level results

Firm-level regressions are reported in Table 5. I restrict the sample to open firms. Different rows show the results for the various outcomes of interest. All columns, 1 through 6, include firm, year, and 4-digit industry fixed effects. Columns 2, 4, and 6 also control for 4-digit industry-year fixed effects. Firm fixed effects control for any time invariant unobserved heterogeneity at the firm-level and 4-digit industry-year fixed effects take into account time-varying effects across industries. I also control for age and age-squared in all regressions. Columns 1 and 2 use firms in all major states as the control group. Columns 3 and 4 use firms in neighboring states as the control group. In columns 5 and 6, I restrict the sample to bordering districts.[32] Districts on the border along the treated and control states tend to be observationally similar, differing only because of differential benefits provided to firms. In these specifications, I compare outcomes for firms across bordering districts (in treated and neighboring control states) before and after the policy change. These regressions are a strict test on the identification strategy and provide credible support to my results from using firms in neighboring states and all major states as control groups. As mentioned earlier, these regressions only show the effect of the policy change on existing firms. This is because the effect of new firms entering after 2003 is removed by the inclusion of firm fixed effects.

In Table 5, the coefficients on the interaction term *post*treat* can be interpreted as the intensive margin of the policy change as it shows the effect of the policy change on incumbent firms (firms present both before and after the policy change). Columns 1 and 2 show that the mean employment for existing firms in the treated states differentially increases by 9.4-11.1% as compared to those in control states. In columns 3 and 4, the mean employment rises by 7.4-10.3%. The results in columns 5 and 6 with firms in bordering districts as the control group also shows a differential increase in mean employment in firms by around 11%. Total output and wage bill also differentially increase for existing firms in the treated states compared to those in control states. The differential increase in total output is between 8.7% and 23.7% depending on the choice of the control group. Wage bill also increases by 7.3% to 13%.

I also run regressions with fixed capital, additions to fixed capital, and additions to plant and machinery as outcome variables. Fixed capital includes depreciation whereas additions to fixed capital and additions to plant and machinery are measures of actual additions before

[32]List of bordering districts is shown in Appendix Table A1.

depreciation. The measure of stock of fixed capital is more likely to suffer from measurement error than the numbers for actual additions[33] made during the year. This is also clear from the regression results. For example, fixed capital shows an increase of around 5.8-8% but in most cases is statistically indistinguishable from zero. Actual additions to fixed capital however, increased by around 20-28% and additions to plant and machinery increased by around 14-26% for existing firms in treated areas compared to control areas. The coefficients from the bordering districts regressions also have similar magnitudes. These results provide suggestive evidence that existing firms took advantage of the "substantial expansion" clause and increased investment on fixed capital to receive the tax benefits. In this respect, these results confirm that the policy was successful in incentivizing firms to invest more in plant and machinery. Across all outcome variables, the coefficients of treatment effects are relatively stable irrespective of the choice of the control group.

It is conceivable that the firm-level results are being driven by multi-establishment firms reallocating production across their various plants to take advantage of the incentives. In Table 6 Panel A, I directly test for this by removing multi-establishment firms from the sample. I find similar coefficients using this sample and hence it is unlikely that the firm-level results are being driven by these establishments. A separate concern in the bordering districts regressions is the possibility that results are downward biased because of positive spillovers from treated to control areas. To check for this, I run a regression specification with the firms along the border in the treated states compared with firms further away from the bordering districts in the control states. These results are shown in Table 6 Panel B. For existing firms in the treated districts as compared to control districts, employment went up by 15% and output by 16% after 2003.[34] In Table 6 Panel C, I compare firms in districts in the neighboring control states that border the treated states to firms in districts further away from the treated states. If the policy resulted in spillovers on firms along the border in the neighboring control states, these firms should have differential outcomes as compared to firms away from the border in the neighboring states. I find no such differential effects in firm-level outcomes in this regression. The results from Table 6 Panels B and C taken together, suggest that spillovers do not play a substantial role in the firm-level results and especially lends support for the bordering districts regression specification.

To explore differences between new and existing firms, I plot kernel density graphs (figures 6 through 8) comparing firms across treated and control regions after the policy change. The

[33]Actual additions are similar to measures of investment.

[34]These magnitudes are a bit larger than those obtained in the firm-level regressions in Table 5, suggesting the possibility of a downward bias in the treatment effects.

graphs clearly indicate that the new firms entering the treated states are larger and more productive than both the existing firms (in the treated states) and the new firms entering the neighboring control states. Since informal firms tend to be smaller in size than formal firms, it is unlikely that the majority of the effects of the policy change are being driven by the formalization of informal firms. However, it is not possible to say where the new firms come from and where they might have set up in the absence of the policy.

V.3 Productivity results

A major economic rationale for providing tax incentives to firms to locate to a particular region is the possibility of agglomeration economies. New firms entering a region might lead to positive productivity spillovers on existing firms. To test for agglomeration economies, I look at the differential effect of the policy change on TFP (total factor productivity) measures in the treated states compared to the control states. I use two different measures of productivity. First, I construct industry and firm-level TFP measures using the methodology of Levinsohn & Petrin (2003). The gist of the method is as follows. Assume that a firm i in industry j at time t has a Cobb-Douglas production function

$$y_{ijt} = \alpha + \beta_l(l_{jt}) + \beta_p(p_{jt}) + \beta_m(m_{jt}) + \beta_k(k_{jt}) + \omega_{ijt} + \varepsilon_{ijt}$$

where y is output, l is labor, p is power and electricity expenditure, and m is expenditure on raw materials (all variables in logarithms). The simultaneity problem arises because firms observe their own productivity ω_{ijt}, before choosing their inputs of power, labor and other raw materials. However, this is not observable to the econometrician. Levinsohn & Petrin (2003) use raw material expenditure (m_{ijt}) as a proxy for the unobserved productivity shock. They show that if these raw material inputs are monotonic in the firm's productivity at all levels of capital, then it can be inverted to express productivity in terms of capital and raw materials.

$$\omega_{ijt} = \omega_{jt}(m_{ijt}, k_{ijt})$$

This function can then be inserted into the equation above. Then the estimation takes place in two stages. In the first stage, a flexible functional form of capital and raw materials is included and the coefficients on l and p are estimated using semi-parametric techniques. The second stage uses GMM techniques to recover the coefficients on k and m.[35] I use

[35] For details, see Levinsohn & Petrin (2003).

this method to estimate production function parameters separately for each 2 digit industry. Then, I use these estimates to construct firm-level productivity measures.

I also construct labor productivity measures defined as output per man-day and value added per man-day. Columns 1 through 3 in Table 7 show the results for the state-industry level DID regressions comparing aggregate productivity in the treated states to the control states (major states). I find large increases in aggregate productivity across various measures of TFP. This differential increase in productivity is the cumulative effect of both new and existing firms in the treated states. A natural question to ask is whether the entry of new firms led to increases in productivity for the existing firms. In columns 4 and 5, I run firm-level regressions to look at the effect of the policy change on the productivity of existing firms, and find no effects. This suggests that the most of the aggregate productivity gains in the treated states are being driven by the entry of new firms. Although surprising, it must be kept in mind that this paper looks only at the short to medium term effect of the policy change and generally existing firms take some time to internalize agglomeration economies.[36] These differences in productivity levels between new and old firms are also shown in Figure 8.

V.4 Wages, Rents, and Migration

After looking at various industrial outcomes, it is important to investigate changes in the local economy. First, I test whether the policy resulted in earnings gains for the residents of the treated states. It is conceivable that migrant workers move in from other states to take advantage of the jobs available after the policy change. This might partially or completely offset the nominal wage gains due to the increase in local prices and rents in the treated states. I explicitly test for real earnings gain and differential migration across treated and control areas. Table 8, column 1 reports the results of a difference-in-differences regression specification comparing total wages in treated states to neighboring states. This regression controls for district fixed effects and industry-year fixed effects along with individual controls such as age, sex, and education status. I find an 11% differential increase in nominal wages[37] for all workers in the treated states compared with the neighboring states. In column 2, I run the same specification restricted to districts along the border and find a 13% increase

[36]For example,Greenstone, Hornbeck & Moretti (2010) find that the total factor productivity (TFP) of incumbent plants grows five years after the opening of a large plant in their county. Also, Wang (2013) shows that there is positive TFP growth more than six years after the opening of an SEZ in China.

[37]The wages data comes from the NSS dataset that asks each household member their wages over the seven days preceding the interview.

in wages. The magnitude of this effect is similar across the two specifications. Columns 3 and 4 look at wages of workers involved in non-agricultural activities and these increase by 14.5%. In columns 5 and 6, I look at the wages of agricultural workers and find no differential increase across the treated and control groups. This provides additional support for the results as the policy was similar to a labor demand shock for the industrial sector and should not have affected the agricultural sector. In columns 7 and 8, I compare housing rents across the treated and control groups controlling for district and year fixed effects along with housing attributes such as floor area, dwelling type, roof type, number of rooms etc. I find no statistically significant differential effect of the policy change on housing rents. This suggests that nominal wages might have gone up without corresponding increases in local prices.

However, housing rents might not be an apt measure of overall price levels in India and we would need a state-level consumer price index (CPI) to deflate the nominal values. A state-level CPI is not readily available for different states going back to the early 2000s. I construct an alternate price index at the state level (1999-2000 as the base year) using state GDP at constant and current prices from the RBI Handbook of Statistics. The price index for the neighboring states is shown in Appendix Table A2. I deflate wages and monthly per capita expenditure and run difference-in-differences specification comparing these outcomes in treated and control areas. In Table 9, columns 1 and 2 show that real wages increase by 12% -15%. The magnitude is similar to the increase in nominal wages in Table 8, suggesting that the policy did not differentially affect the price levels. Furthermore, in columns 3 and 4, I also find a differential increase in monthly per capita expenditure by around 10%.[38] Finally, for column 5, I aggregate the total wages earned in the entire states (the state-level wage bill). I compare the total wage bill in the treated states to the neighboring control states and find a 52.8% increase in total wage bill.[39] This is comparable to the 41-44% increase in industrial wage bill estimated in Table 2.

These results suggest that the policy change did not induce differential migration into the treated regions from control states. I explicitly test for migration in treated areas compared with control areas in Table 10. The definition of migrants follows the questions in the NSS surveys that elicit information on the last usual place of residence of the respondent. Using this measure, I define an external migrant as a person whose last usual place of residence was another state. An internal migrant is defined as a person migrating across districts within

[38]In previous work analyzing place-based policies, the effects on per capita expenditure have mostly been ignored. These results are a major improvement over what has been done in the literature thus far.

[39]This is the differential increase in the total wage bill over seven days.

the state. I also run specifications with economic migrants who report that their reason for migration is work related. I find no statistically significant effect on external migrants and economic migrants. I find a negative differential effect on internal migrants. This might be because each district within the treated states had an industrial estate (and more jobs) leading to less within-state migration. Taken together, Table 10 suggests that there was no differential migration in response to the policy change in treated states compared with control states. This is consistent with previous literature documenting low migration in India.[40]

VI Cost-Benefit Analysis

Although the results in the paper suggest that the policy was successful in creating employment, output, and real earnings gains in the treated states, it might have come at large costs. I use the treatment effect coefficients to conduct a back-of-the-envelope cost-benefit analysis. I broadly follow Busso, Gregory & Kline (2013) for this analysis, but in addition I include firm profits (as a benefit of the policy).[41] The benefits of the policy accrue to firm owners, workers,[42] and landowners in the treated states, whereas the costs to the government include the foregone tax revenues and the actual cost of the capital subsidy. For the ease of comparison, I provide all numbers in terms of 2007-08 (the last year in my analysis).

The benefits in the treated states can be broken down into three components: (i) increase in profits of firms in the treated areas, (ii) real wage earnings increases in the treated areas, and (iii) rental rate gains for landowners in the treated areas.

For corporate profits and income, I first estimate the treatment effect coefficient using a state-industry level difference-in-differences specification similar to all the earlier specifications. This is shown in Table 11, where columns 1 and 2 look at the effect on corporate profits and columns 3 and 4 show the effects on corporate income. Table 11 shows that the policy had large effects on both corporate profits (77.5%) and income (69.5%). I use the estimated treatment effect coefficient on *post*treat* in the state-industry regressions, β_1, to calculate the magnitude of the total treatment effect. For all the cost-benefit calculations I

[40]For example, Munshi & Rosenzweig (2009) find that in rural areas permanent migration rates of men out of their origin villages were as low as 8.7 percent in 1999.

[41]Busso, Gregory & Kline (2013) measure the benefits of the EZ program as the total earnings increase of zone workers, earnings increase for non-resident commuters, improvements in local amenities, and value of rent reductions outside the zone due to decreases in population. They model firms as price-takers with constant returns to scale technology. Hence, profits of firms in their analysis is zero.

[42]Note that in my case, migration is zero and there are no non-resident commuters.

use the treatment effect coefficient from the difference-in-differences regression with all major states as the control group. Note that the total treatment effect is the difference between the actual total and the counterfactual total in 2007-08. The counterfactual total is the amount that would have accrued in the absence of the policy and equals Actual total/(1+β_1).

Similarly, for the real wage bill gains in the treated states, I use the estimates from the total real wage bill regressions in Table 9, column 5. I multiply the weekly total wage bill by 52 to get the yearly total wage bill. As shown in Table 8 columns 7 and 8, the rental rates do not change differentially and hence I assume the rental rate gains to landowners are negligible. I show the numbers in Table 12a. The total gains from the policy change are around 95.25 billion rupees, of which 65.06 billion rupees (USD 1.01 billion) accrues to firm owners[43] (as profits) and 30.19 billion rupees (USD 480 million) accrues to workers (as wage bill).

To calculate the costs, I take into account (i) foregone corporate income tax revenue, (ii) foregone central excise tax revenue, and (iii) actual costs of the capital subsidy. I estimate the foregone tax revenue by calculating the revenue that the government would have collected in the absence of the policy. I use the estimated coefficient on *post*treat* on corporate income (Table 11) and calculate the counterfactual.[44] I use a 35% corporate income tax rate to measure the total foregone revenue from the corporate income tax exemption. I use the same method to calculate the foregone revenue from the central excise tax exemption. The central excise tax is levied on the total value of output. I use the actual central excise tax receipts collected by the Government of India as a percentage of the value of output as the effective excise tax rate (7%).[45] These numbers are shown in Table 12b. The foregone revenues from corporate income taxes are 36.4 billion rupees and those from central excise taxes are 29.63 billion rupees.

The actual cost of the capital subsidies is also not readily available. In this analysis, I calculate an upper bound for the actual cost. The policy provided capital subsidies to new and old firms equaling 15% of their investment in plant and machinery up to a maximum amount of Rs. 3,000,000. I assume that every firm gets the maximum amount to provide an upper bound. In 2007-08, there were 2634 firms in the treated states.[46] Assuming each firm received Rs. 3,000,000, the total amount spent by the government would be Rs. 7.9

[43]Around 21% of the firms in the treated states were public limited companies. Individual proprietorships (13%), partnerships (26%) and private limited companies (38%) accounted for the bulk of the remaining firms. No other detailed shareholder information is available in the dataset.

[44]Counterfactual = Actual total/(1+β).

[45]This figure comes from the Comptroller and Auditor General of India, Report No. CA 20 of 2009-10 - Union Government (Indirect Taxes).

[46]Figure from the ASI state$\times$industry panel data.

billion. Summing up the three components for costs, the total loss for the government was approximately Rs. 73.9 billion (USD 1.15 billion).

Since these are public funds, they induce a direct tax burden and a marginal welfare cost associated with acquiring the revenues. This is given by the marginal cost of public funds (MCF). I use a value of MCF to equal 1.2[47] and this gives me a total cost to the government of Rs. 88.7 billion.[48] Hence, given the range of values of MCF, the cost to the government is in the range of Rs. 73.9-88.7 billion.

Comparing the costs and benefits, gives us a range of Rs. 6.5-21.3 billion (USD 101-332 million) in benefits from the policy change. This is roughly 0.11-0.36% of the combined GDP of the two treated states.[49] One caveat of the cost-benefit calculations is that profits to firm owners constitute a large proportion of the benefits. It is possible that the observed profits are in fact due to a reduction in tax avoidance because of the tax exemption scheme. If tax avoidance amounted to between 6.5-21.3 billion rupees (the net benefits of the policy), i.e. avoidance rates between 10%-33%, then the policy would have just broken even. Although these numbers provide suggestive evidence on the cost-effectiveness of the policy, I cannot conclude whether the policy was Pareto-improving or if the tax-incentive scheme was the most efficient transfer to the treated regions.

VII Conclusion

Many argue that a spatially targeted industrial policy is a waste of taxpayers' money as it simply reallocates economic activity across regions and does not lead to overall growth. Policy makers throughout the world however, use such location-based policies to help develop economically lagging regions. Whether location-based tax incentives are effective and help in industrialization and employment generation at the local level is largely an empirical question. In the last few years, there has been a growing empirical literature on place-based policies, mainly as more micro-data has become available. However, these policies have been understudied in developing countries where regional economic disparities can be large and labor mobility might be low.

In this paper, I critically examine a location-based tax incentive scheme that provided tax exemptions and capital subsidies to new and existing firms in two states in India, beginning

[47] Auriol & Warlters (2012) estimate the average MCF in 38 countries in Africa to be between 1.1 and 1.2. Browning (1976) calculated the MCF in the United States to be in the range 1.09-1.16.

[48] Rs. 73.9 billion × 1.2.

[49] Combined state GDP for Himachal Pradesh and Uttarakhand in 2007-08 was Rs. 598.6 billion (USD 9.6 billion).

2003. I find that the policy change resulted in large increases in employment, output, and capital - both due to entry of new firms and growth of existing firms. I document that the new firms entering the treated areas are larger and more productive but find no evidence for relocation of economic activity across the treated and control areas. The policy led to earnings increases for residents of the treated states without any corresponding increases in housing rents or local prices. I also show that these real earnings gain might be due to low migration in to the treated states. Finally, I use my estimates to conduct a simple cost-benefit analysis that suggests that the policy was cost-effective.

An important caveat is that these results are at best medium-term effects of the policy change on various economic outcomes. It will be interesting to look at the long run impacts of this policy after the removal of the incentives [see Kline & Moretti (2014*a*)]. Whether or not such policies have a lasting impact (for example, agglomeration economies)[50] or only attract fly-by-night operators that shut shop and relocate to the next area with such benefits is an important issue but beyond the current scope of this paper. With more data available in the following years, this seems to be a promising avenue for future research.

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[50] See Greenstone, Hornbeck & Moretti (2010).

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Table 1: Summary statistics

Panel A: state×3-digit industry level		**Treated states**		**Major states**		**Neighboring states**	
		N	mean	N	mean	N	mean
			[s.d]		[s.d]		[s.d]
Number employed	Pre-2003	231	589.68	2746	4952.17	709	3153.96
			[1214.31]		[14023.84]		[6118.20]
	Post-2003	424	834.16	4630	5237.32	1203	3554.21
			[1545.7]		[14814.82]		[6825.79]
Number of factories	Pre-2003	231	15.98	2746	134.31	709	103.90
			[27.82]		[282.95]		[163.59]
	Post-2003	424	21.15	4630	141.98	1203	110.14
			[32.17]		[304.79]		[188.27]
Total output	Pre-2003	231	15068.07	2746	103814.2	709	73225.33
(in ‘00,000 Rs.)			[26703.81]		[271097.10]		[128034.00]
	Post-2003	424	41973.7	4630	63988.66	1203	131358.60
			[94545.83]		[205354.2]		[256813.00]
Fixed capital	Pre-2003	231	7358.37	2746	44306.31	709	24231.10
(in ‘00,000 Rs.)			[20079.97]		[150713.20]		[57330.15]
	Post-2003	424	18860.29	4630	63988.66	1203	32682.40
			[61088.17]		[205354.2]		[79836.12]
Wage bill	Pre-2003	231	1167.24	2746	6775.8	709	4166.21
(in ‘00,000 Rs.)			[3509.69]		[15810.68]		[6912.09]
	Post-2003	424	1936.53	4630	9684.05	1203	6265.78
			[4662.96]		[22205.34]		[10883.73]
Panel B: firm-level							
Number employed	Pre-2003	1915	39.63	84879	41.55	18192	32.74
			[151.29]		[245.93]		[120.00]
	Post-2003	5222	43.43	176605	42.62	39710	36.03
			[127.28]		[202.64]		[125.26]
Total output	Pre-2003	1743	1097.01	80680	919.38	17234	800.43
(in ‘00,000 Rs.)			[4461.96]		[13874.97]		[6734.20]
	Post-2003	4733	2300.66	170818	1700.22	38348	1361.89
			[8030.17]		[33725.87]		[1361.89]
Fixed capital	Pre-2003	1961	471.02	87607	359.06	18741	240.86
(in ‘00,000 Rs.)			[3227.43]		[8383.13]		[3613.18]
	Post-2003	5461	928.29	185860	493.56	42523	309.13
			[9743.92]		[493.56]		[3426.12]
Wage bill	Pre-2003	1972	28.25	89306	23.05	18847	16.67
(in ‘00,000 Rs.)			[264.65]		[285.46]		[110.60]
	Post-2003	5537	31.39	190186	27.31	42922	20.74
			[292.88]		[344.27]		[131.72]
Panel C: migration, wages, housing rents							
External migrant	Pre-2003	15194	0.05	499783	0.03	118045	0.04
			[0.22]		[0.17]		[0.19]
	Post-2003	16575	0.09	463736	0.03	103797	0.05
			[0.28]		[0.18]		[0.22]
Internal migrant	Pre-2003	15194	0.09	499783	0.07	118045	0.09
			[0.28]		[0.26]		[0.28]
	Post-2003	16575	0.06	463736	0.09	103797	0.09
			[0.24]		[0.28]		[0.29]
Economic migrant	Pre-2003	15194	0.10	499783	0.07	118045	0.05
			[0.30]		[0.26]		[0.22]
	Post-2003	16575	0.11	463736	0.07	103797	0.06
			[0.31]		[0.25]		[0.23]
Total wages (in Rs.)	Pre-2003	1714	879.36	75292	496.24	11772	691.05
(all workers)			[861.36]		[1082.62]		[945.31]
	Post-2003	6327	1080.16	203618	662.72	31724	829.97
			[1117.18]		[1517.96]		[1075.31]
Housing rents	Pre-2003	392	422.92	12802	596.17	2384	656.28
(in Rs.)			[377.45]		[807.77]		[954.79]
	Post-2003	511	749.74	16627	1071.5	3517	1176.71
			[804.42]		[1335.22]		[1502.73]

Notes: Treated states: Uttarakhand and Himachal Pradesh; All major states: Haryana, Punjab, Delhi, Chandigarh, Uttar Pradesh, Rajasthan, Bihar, Andhra Pradesh, Chhattisgarh, Maharashtra, Madhya Pradesh, Orissa, Goa, Kerala, Karnataka, Tamil Nadu, Jharkhand, Gujarat, and West Bengal. Neighboring states: Haryana, Punjab, Delhi, Chandigarh, Uttar Pradesh.

Panel A: Includes summary statistics for state×3-digit-level data. Observations here are state×3-digit-year observations.

Panel B: Includes summary statistics for firm-level data. Observations here are firm-year observations.

Panel C: Includes summary statistics for migration, wages, and housing rents. External migrant is defined as a person whose last usual place of residence is outside the state; Internal migrant is one whose last usual place of residence is the same state but a different district; Economic migrant is one who migrated for a work related reason. Total wages are defined as the wages earned over the seven days preceding the interview.

Table 2: State×industry results

	(1)	(2)	(3)	(4)	(5)
Panel A: Log employment					
*post*treat*	0.427**	0.443**	0.439**	0.448***	0.340
	(0.0374)	(0.0442)	(0.0460)	(0.0506)	(0.0597)
	[0.039]	[0.031]	[0.011]	[0.010]	[0.140]
R-squared	0.625	0.634	0.626	0.634	0.634
Panel B: Log of total factories					
*post*treat*	0.310**	0.311**	0.315***	0.315***	0.320
	(0.0160)	(0.0170)	(0.0198)	(0.0191)	(0.0485)
	[0.008]	[0.008]	[0.004]	[0]	[0.221]
R-squared	0.715	0.719	0.716	0.719	0.719
Panel C: Log of total output					
*post*treat*	0.561**	0.577*	0.623**	0.639**	0.473
	(0.0408)	(0.0475)	(0.0463)	(0.0497)	(0.146)
	[0.036]	[0.067]	[0.041]	[0.033]	[0.154]
R-squared	0.611	0.622	0.611	0.622	0.622
Panel D: Log of fixed capital					
*post*treat*	0.711	0.728	0.776**	0.787**	0.564
	(0.0871)	(0.0734)	(0.0840)	(0.0713)	(0.197)
	[0.182]	[0.211]	[0.026]	[0.034]	[0.176]
R-squared	0.627	0.635	0.628	0.636	0.635
Panel E: Log of wage bill					
*post*treat*	0.412*	0.427*	0.437**	0.449**	0.253
	(0.0724)	(0.0900)	(0.0794)	(0.0948)	(0.0672)
	[0.053]	[0.070]	[0.050]	[0.031]	[0.197]
R-squared	0.604	0.611	0.604	0.612	0.612
Observations	8,028	8,028	8,028	8,028	8,028
state FE	Yes	Yes	Yes	Yes	Yes
year FE	Yes	Yes	Yes	Yes	Yes
3 digit industry FE	Yes	Yes	Yes	Yes	Yes
3 digit industry-year FE	No	Yes	No	Yes	Yes
time-varying controls	No	No	Yes	Yes	No
state level pre-trends	No	No	No	No	Yes
control group	Major states	Major states	Major states	Major states	Major states

Notes: The coefficient on the interaction term post*treat shows the treatment effect. Time varying controls include pre-treatment state-level variables (population, number of agricultural workers, number of industrial workers, number of main and marginal workers and number of illiterate people) interacted with a time dummy for each year. Standard errors in parentheses are clustered at the state level. Square brackets show p-values that are calculated using the Cameron, Gelbach & Miller (2008) wild cluster bootstrap-t procedure (999 replications). ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 3: Number of firm closures

	(1) closed	(2) closed	(3) closed	(4) closed
*post*treat*	0.0487	0.311	0.469	-0.377
	(0.397)	(0.330)	(0.387)	(0.319)
Observations	2,567	2,567	8,031	8,031
R-squared	0.312	0.321	0.275	0.277
state FE	Yes	Yes	Yes	Yes
3 digit industry-year FE	Yes	Yes	Yes	Yes
time-varying controls	No	Yes	No	Yes
Control Group	Neighboring states	Neighboring states	Major states	Major states

Notes: Dependent variable is the number of firm closures in a 3-digit industry in a particular state. The coefficient on the interaction term post*treat shows the treatment effect. Time varying controls include pre-treatment state-level variables (population, number of agricultural workers, number of industrial workers, number of main and marginal workers and number of illiterate people) interacted with a time dummy for each year. Standard errors in parentheses are clustered at the state level. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 4: Testing for relocation of industrial activity

	(1) Log (employed)	(2) Log (total factories)	(3) Log (total output)	(4) Log (fixed capital)	(5) Log (wage bill)
*post*neighbors*	0.0501	0.00974	-0.0371	-0.0273	0.0288
	(0.0449)	(0.0245)	(0.0639)	(0.00541)	(0.0513)
Observations	7,375	7,375	7,375	7,375	7,375
R-squared	0.629	0.716	0.621	0.635	0.607
state FE	Yes	Yes	Yes	Yes	Yes
year FE	Yes	Yes	Yes	Yes	Yes
3 digit industry FE	Yes	Yes	Yes	Yes	Yes

Notes: The coefficient on the interaction term post*neighbors shows the effect of the policy change on neighboring states as compared to all other major states (excluding the neighboring states). The two treated states are omitted in this regression. Standard errors are clustered at the state level. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 5: Firm-level regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Log (employed)						
*post*treat*	0.0944**	0.111**	0.0747*	0.103***	0.0740	0.110*
	(0.0382)	(0.0450)	(0.0400)	(0.0377)	(0.0577)	(0.0544)
Observations	262,458	262,458	63,629	63,629	13,185	13,185
R-squared	0.945	0.946	0.939	0.942	0.946	0.953
Log (total output)						
*post*treat*	0.0940*	0.113*	0.0866	0.114**	0.177**	0.237***
	(0.0511)	(0.0685)	(0.0538)	(0.0484)	(0.0711)	(0.0722)
Observations	251,767	251,767	60,664	60,664	12,315	12,315
R-squared	0.964	0.965	0.965	0.967	0.970	0.976
Log (wage bill)						
*post*treat*	0.0733**	0.0930**	0.0807**	0.113**	0.108**	0.129**
	(0.0341)	(0.0393)	(0.0359)	(0.0365)	(0.0466)	(0.0565)
Observations	262,516	262,516	63,650	63,650	13,193	13,193
R-squared	0.955	0.956	0.949	0.952	0.954	0.960
Log (fixed capital)						
*post*treat*	0.0579*	0.0756**	0.0552	0.0709**	0.0804	0.0812
	(0.0313)	(0.0293)	(0.0341)	(0.0304)	(0.0501)	(0.0495)
Observations	273,830	273,830	67,033	67,033	14,125	14,125
R-squared	0.969	0.970	0.973	0.974	0.978	0.981
Log (additions to fixed capital)						
*post*treat*	0.200***	0.235***	0.278***	0.275***	0.277	0.406**
	(0.0681)	(0.0686)	(0.0772)	(0.0687)	(0.170)	(0.154)
Observations	201,697	201,697	52,906	52,906	10,503	10,503
R-squared	0.868	0.871	0.856	0.864	0.875	0.899
Log (additions to plant and machinery)						
*post*treat*	0.144**	0.140**	0.255***	0.256***	0.199*	0.246
	(0.0674)	(0.0681)	(0.0833)	(0.0911)	(0.113)	(0.216)
Observations	159,338	159,338	41,674	41,674	8,222	8,222
R-squared	0.862	0.867	0.858	0.869	0.883	0.914
Control group	Major states	Major states	Neighboring states	Neighboring states	Border districts	Border districts
firm FE	Yes	Yes	Yes	Yes	Yes	Yes
year FE	Yes	Yes	Yes	Yes	Yes	Yes
4-digit industry FE	Yes	Yes	Yes	Yes	Yes	Yes
4-digit industry year FE	No	Yes	No	Yes	No	Yes
Age Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The coefficient on the interaction term post*treat shows the treatment effect. Standard errors are clustered at the district level. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 6: Testing for spillovers in firm-level regressions

	(1)	(2)	(3)
	Log (employed)	Log (total output)	Log (fixed capital)
Panel A: removing multi-establishment firms			
*post*treat*	0.0919**	0.144***	0.0656*
	(0.0437)	(0.0476)	(0.0383)
Observations	55,599	52,682	58,796
R-squared	0.939	0.969	0.973
Panel B: testing for spillovers 1			
*post*treat*	0.151***	0.162**	0.0388
	(0.0535)	(0.0698)	(0.0628)
Observations	17,456	16,316	18,139
R-squared	0.958	0.975	0.978
Panel C: testing for spillovers 2			
*post*neighboring-district*	0.0575	-0.128	-0.0749
	(0.0579)	(0.0799)	(0.0808)
Observations	17,451	16,679	18,708
R-squared	0.955	0.979	0.978
firm FE	Yes	Yes	Yes
4-digit industry-year FE	Yes	Yes	Yes
Age Controls	Yes	Yes	Yes

Notes:

Panel A: This regression removes multi-establishment firms and runs a difference-in-differences regression.

Panel B: This regression compares firms along the border in the treated states to firms away from the border in the neighboring control states.The coefficient on the interaction term post*treat shows the treatment effect.

Panel C: This regression compares firms along the border in the neighboring control states to firms away from the border in the neighboring control states.The coefficient on the interaction term post*neighboring-district shows the treatment effect.

Standard errors are clustered at the district level. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 7: Productivity regressions

	(1)	(2)	(3)	(4)	(5)
	Log (TFP) (Levinsohn-Petrin)	Log (labor productivity 1) (Value added / man-days)	Log (labor productivity 2) (Total output/man-days)	Log (labor productivity) (Total output/man-days)	Log (TFP) (Levinsohn-Petrin)
*post*treat*	0.240**	0.294***	0.133***	-0.0372	-0.0329
	(0.0983)	(0.0686)	(0.0266)	(0.0911)	(0.0380)
Observations	7,702	7,754	7,863	192,539	238,673
R-squared	0.632	0.389	0.518	0.943	0.862
state FE	Yes	Yes	Yes	-	-
year FE	Yes	Yes	Yes	Yes	Yes
3 digit industry FE	Yes	Yes	Yes	-	-
firm FE	No	No	No	Yes	Yes
4-digit industry year FE	No	No	No	Yes	Yes
Age Controls	No	No	No	Yes	Yes

Notes: The coefficient on the interaction term post*treat shows the treatment effect. Columns 1 through 3 show state-industry regressions with major states as the control group. Columns 4 and 5 show firm-level regressions with major states as the control group. Firm fixed effects subsume state fixed effects. Standard errors for columns 1-3 are clustered at the state level, and at the district level for columns 4 and 5 . ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 8: Nominal wages and Rents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log (wages) [all]	Log (wages) [all]	Log (wages) [non-agricultural]	Log (wages) [non-agricultural]	Log (wages) [agricultural]	Log (wages) [agricultural]	Log (rent) [all]	Log (rent) [all]
*post*treat*	0.111**	0.132*	0.147***	0.145*	0.0215	-0.0273	0.396	-0.0583
	(0.0531)	(0.0640)	(0.0561)	(0.0714)	(0.135)	(0.189)	(0.266)	(0.313)
Observations	51,455	10,189	40,964	8,387	10,491	1,802	3,500	603
R-squared	0.667	0.615	0.642	0.660	0.433	0.239	0.540	0.451
district FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No
controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
control group	Neighboring states	Bordering districts	Neighboring states	Bordering districts	Neighboring states	Bordering districts	Neighboring states	Bordering districts

Notes: The coefficient on the interaction term post*treat shows the treatment effect. Controls for columns 1 through 6 include age, sex, educational status, marital status and relationship to household head. Controls for columns 7 and 8 include attributes of the house such as number of rooms, kitchen type, dwelling type, roof type and floor type. Standard errors are clustered at the district level. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 9: Real wages and real monthly per capita expenditure

	(1) Log (real wages)	(2) Log (real wages)	(3) Log (real MPCE)	(4) Log (real MPCE)	(5) Log (real wage bill)
*post*treat*	0.122**	0.146**	0.0967**	0.107	0.528***
	(0.0495)	(0.0616)	(0.0408)	(0.0739)	(0.168)
Observations	51,455	10,189	88,731	17,157	531
R-squared	0.668	0.605	0.358	0.243	0.287
district FE	Yes	Yes	Yes	Yes	Yes
industry-year FE	Yes	Yes	No	No	No
Controls	Yes	Yes	Yes	Yes	No
control group	Neighboring states	Bordering districts	Neighboring states	Bordering districts	Neighboring states

Notes: The coefficient on the interaction term post*treat shows the treatment effect. Controls for columns 1 and 2 include age, sex, educational status, marital status and relationship to household head. Controls for columns 3 and 4 include household type, social group, rural-urban and religion. Standard errors are clustered at the district level. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 10: Testing for differential migration

	(1) external migrant	(2) external migrant	(3) internal migrant	(4) internal migrant	(5) economic migrant	(6) economic migrant
*post*treat*	0.0172	0.0396	-0.0252**	-0.0236	0.00486	0.0349
	(0.0272)	(0.0412)	(0.0120)	(0.0173)	(0.0244)	(0.0311)
Observations	253,611	45,301	253,611	45,301	253,611	45,301
R-squared	0.087	0.043	0.031	0.017	0.076	0.026
district FE	Yes	Yes	Yes	Yes	Yes	Yes
year FE	Yes	Yes	Yes	Yes	Yes	Yes
Control group	Neighboring states	Bordering districts	Neighboring states	Bordering districts	Neighboring states	Bordering districts

Notes: The coefficient on the interaction term post*treat shows the treatment effect. External migrant is one whose last usual place of residence was another state or country. Internal migrant's last usual place of residence was the same state but another district. An economic migrant migrated for work related reasons. Standard errors are clustered at the district level. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 11: Corporate profits and corporate income

	(1) Log (profit)	(2) Log (profit)	(3) Log (income)	(4) Log (income)
*post*treat*	0.930***	0.775***	0.775***	0.695***
	(0.241)	(0.205)	(0.104)	(0.0687)
Observations	2,021	6,032	2,404	7,440
R-squared	0.559	0.512	0.582	0.543
state FE	Yes	Yes	Yes	Yes
year FE	Yes	Yes	Yes	Yes
3 digit industry FE	Yes	Yes	Yes	Yes
control states	Neighboring states	Major states	Neighboring states	Major states

Notes: The coefficient on the interaction term post*treat shows the treatment effect. Standard errors are clustered at the state level. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 12a: Total Benefits

	Actual Total in 2007-08 (billion rupees)	Treatment effect coefficient	Total impact in 2007-08 (in billion rupees)
profits	148.85	0.775	65.06
total wage bill	87.36	0.528	30.19

Table 12b: Total Costs

	Actual Total in 2007-08 (in billion rupees)	Treatment effect coefficient	Counterfactual (in billion rupees)	Tax rate (percent)	Loss in revenue (in billion rupees)
corporate income	176.31	0.695	104.02	35	36.40
total output	660.75	0.561	423.29	7	29.63

Figure 1: Map of India

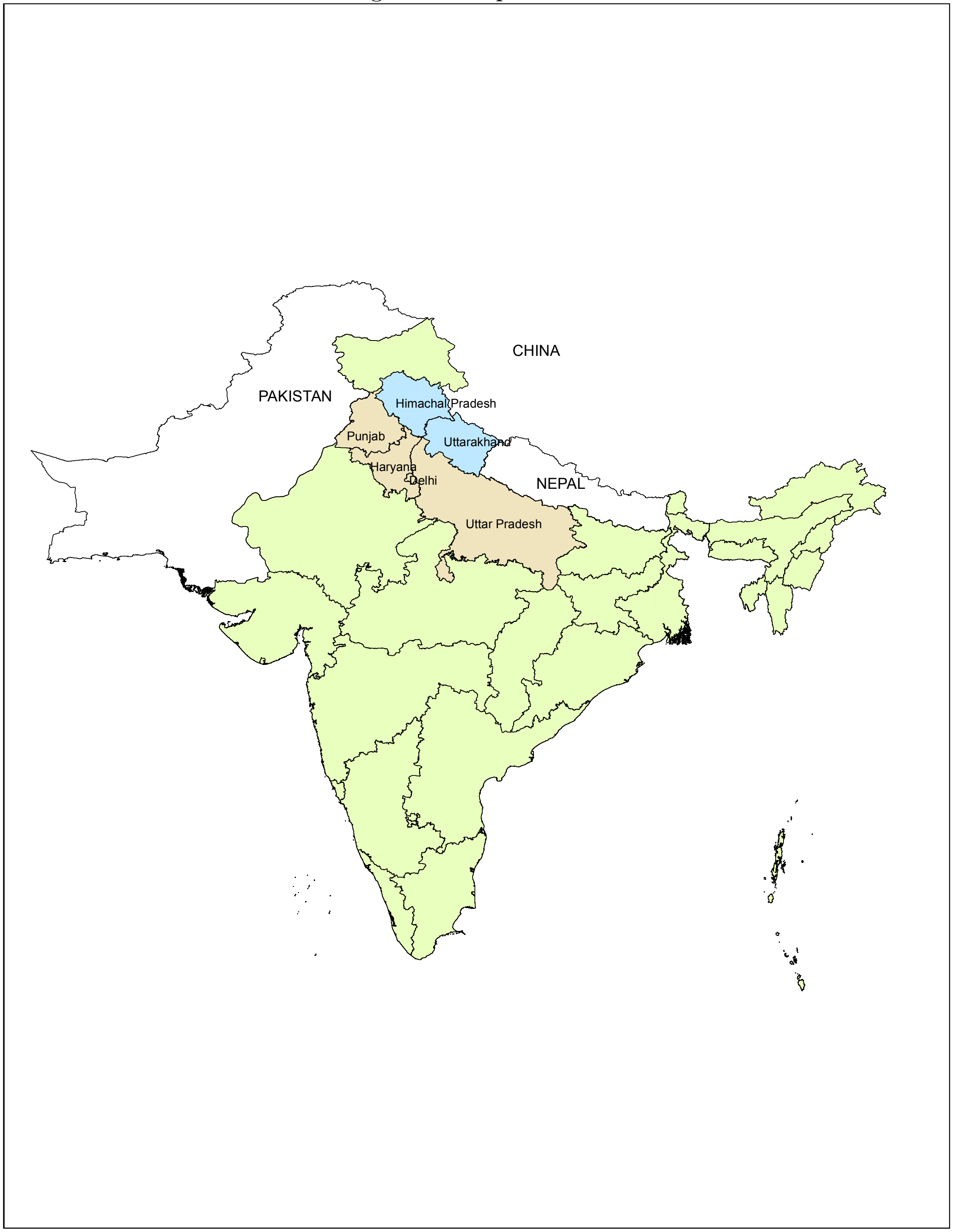
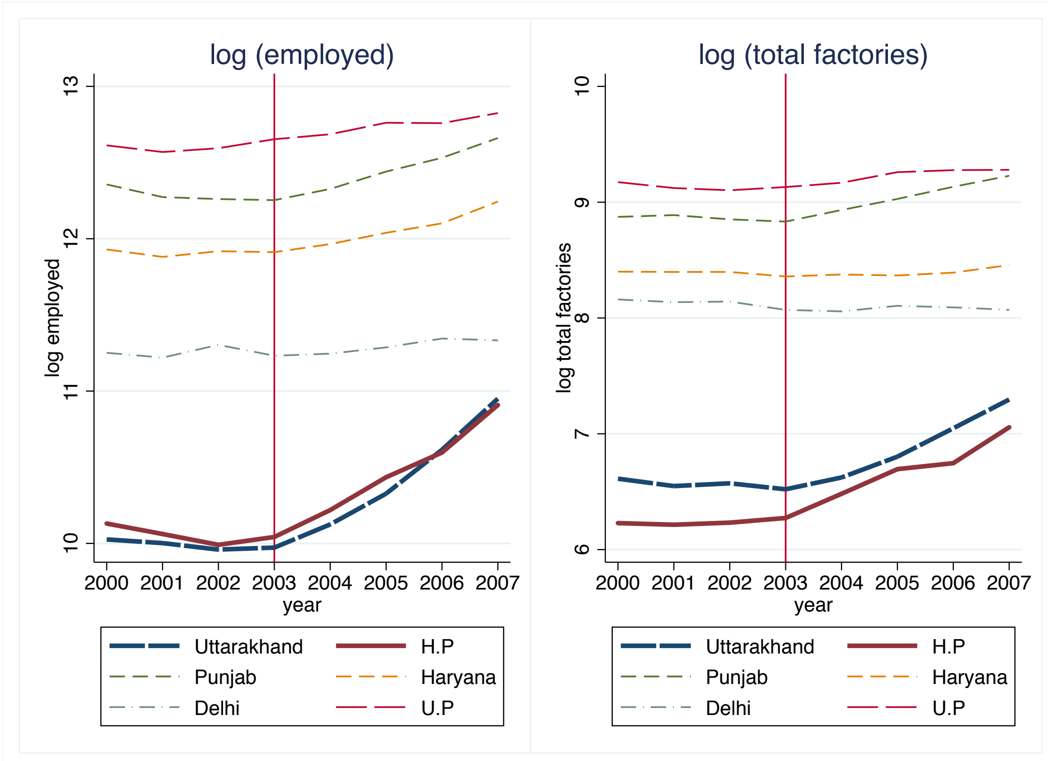
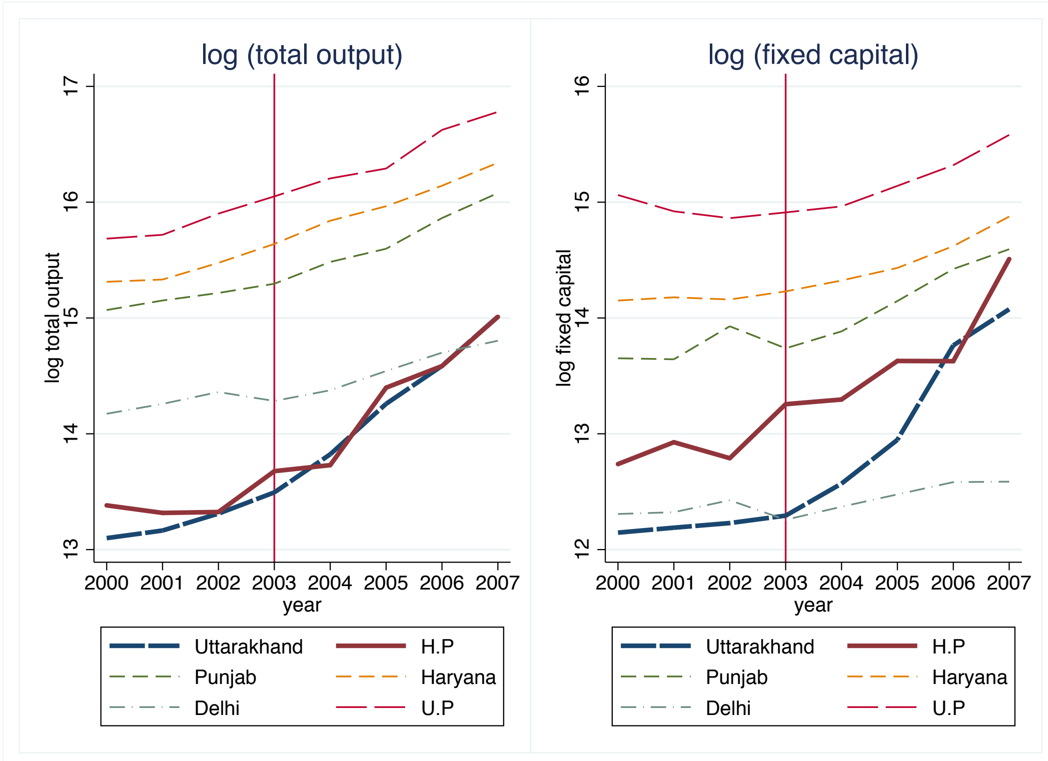


Figure 2: Trends in employment and number of factories



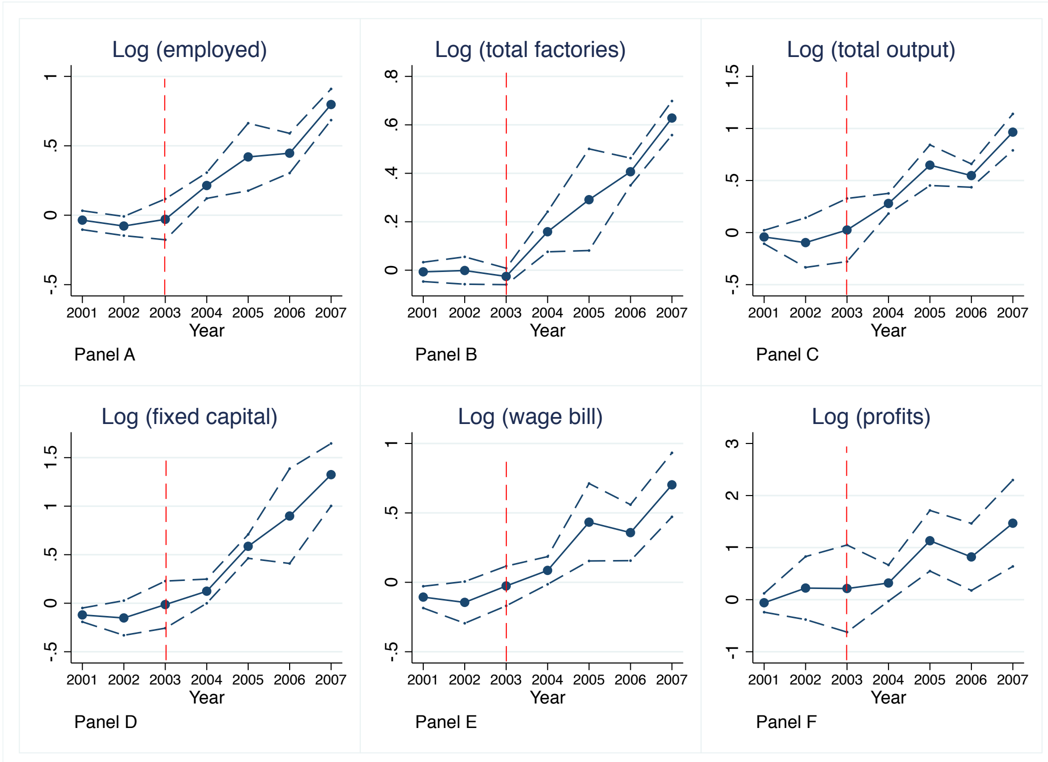
Notes: ASI state×industry data from 2000-01 to 2007-08.

Figure 3: Trends in total output and fixed capital



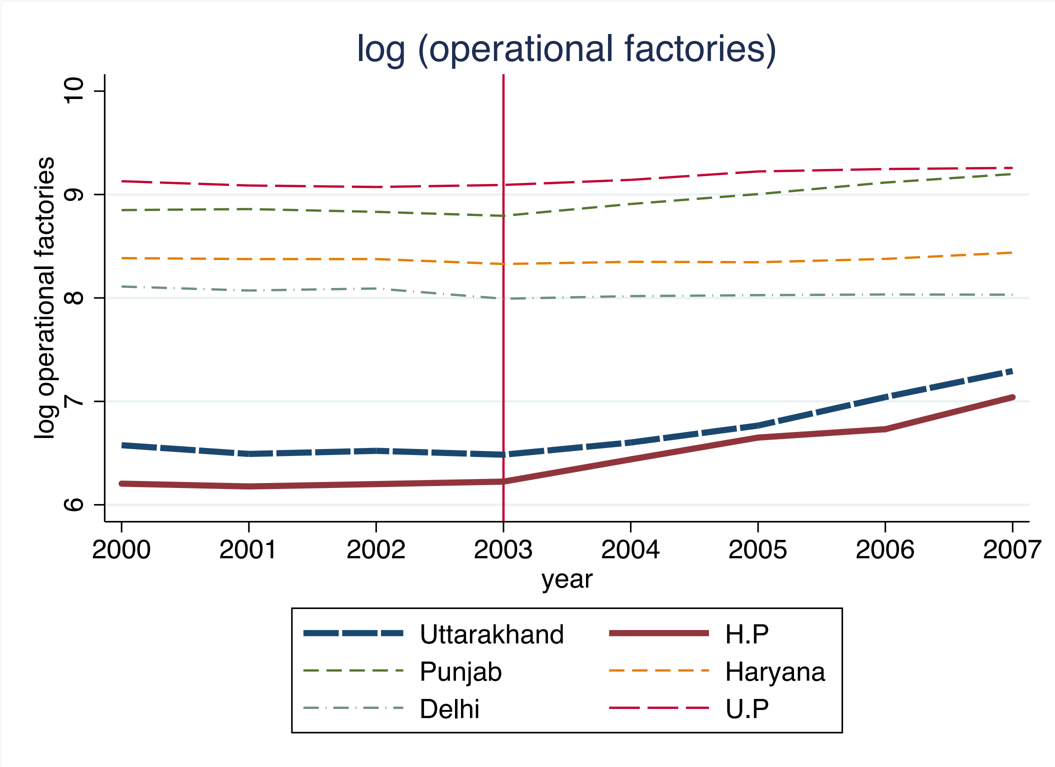
Notes: ASI state×industry data from 2000-01 to 2007-08.

Figure 4: Estimated coefficient graphs



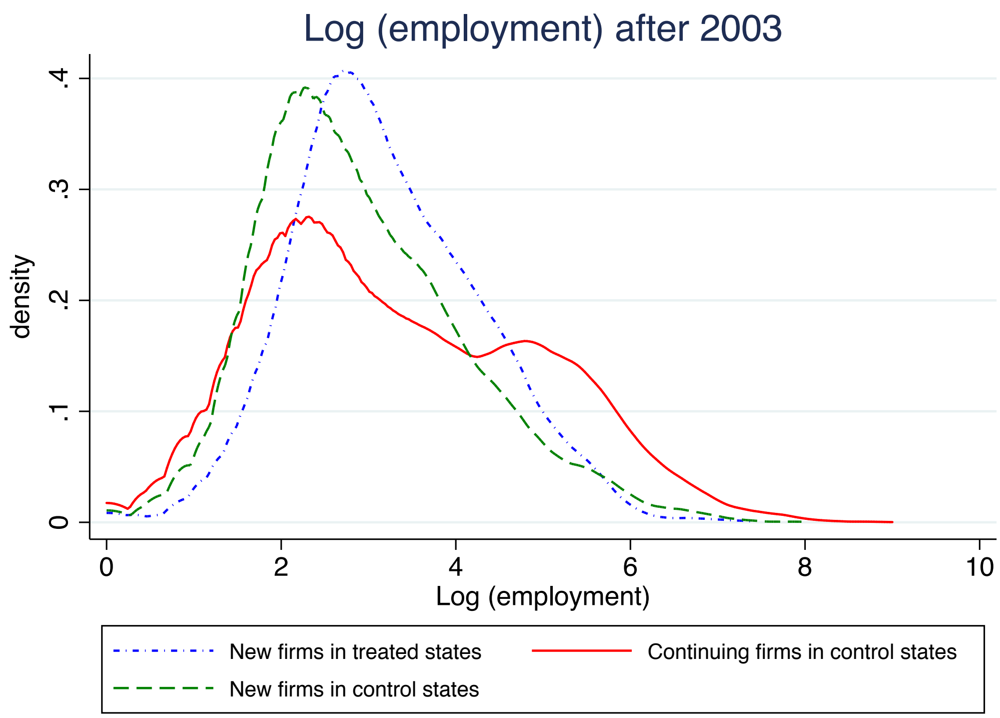
Notes: These graphs plot the coefficients obtained from a regression of the outcome variable (mentioned on top of the graph) on the interaction between the treated states dummy and year dummies. The regressions control for state, year, and 3-digit industry fixed effects. The Y-axis shows the estimated coefficients and the X-axis shows the various years. The control group here is all major states. Standard errors are clustered at the state level.

Figure 5: Trends in operational factories



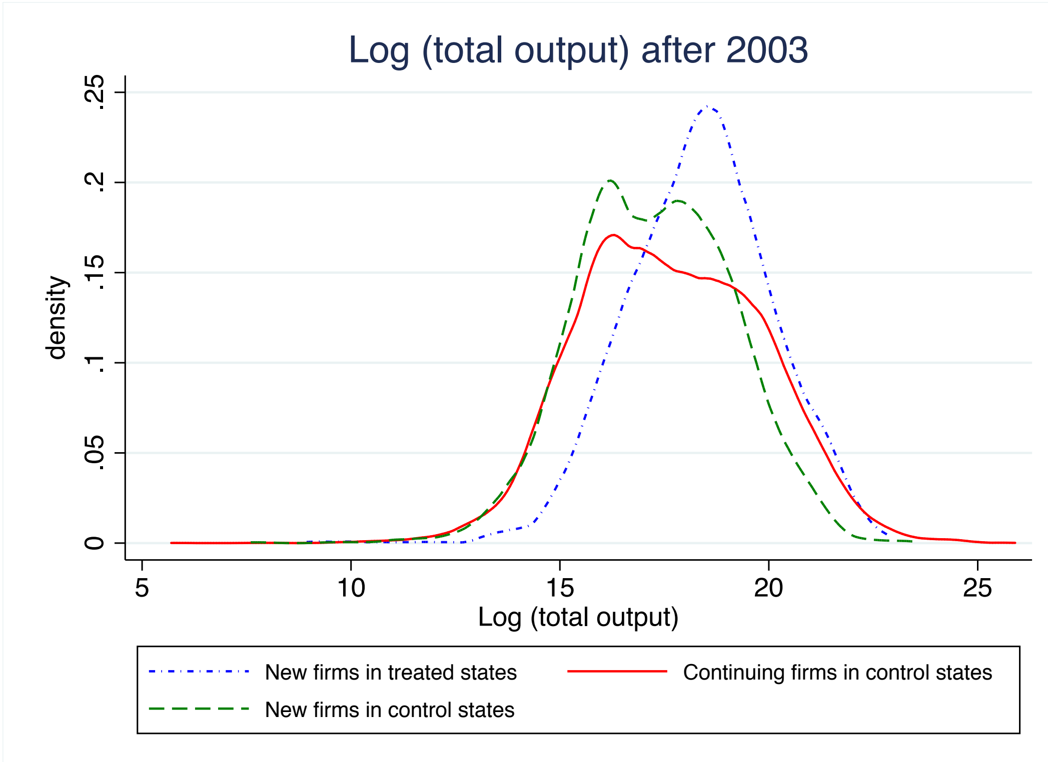
Notes: ASI state×industry data from 2000-01 to 2007-08.

Figure 6: Kernel density of Log (employed)



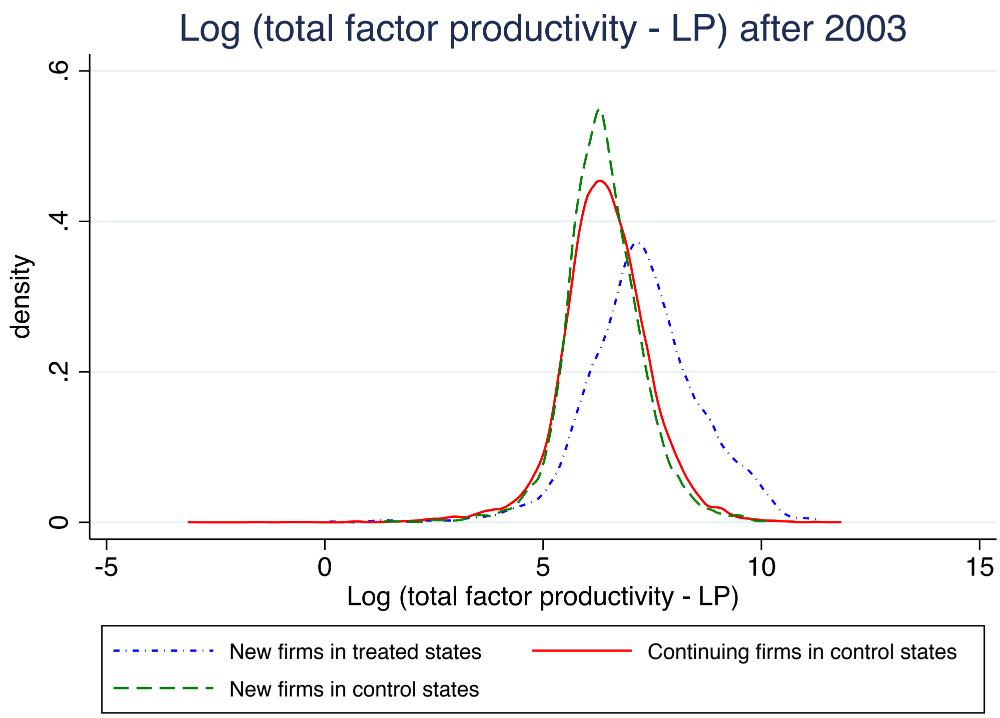
Notes: ASI firm-level data from 2000-01 to 2007-08.

Figure 7: Kernel density of Log (total output)



Notes: ASI firm-level data from 2000-01 to 2007-08.

Figure 8: Kernel density of Log (TFP-Levinsohn-Petrin)



Notes: ASI firm-level data from 2000-01 to 2007-08.

Appendix

Table A1: List of bordering districts

Himachal Pradesh	Uttarakhand	Uttar Pradesh	Haryana	Punjab
Sirmaur	Udham Singh Nagar	Pilibhit	Yamunanagar	Pathankot
Solan	Nainital	Bareilly	Ambala	Hoshiarpur
Bilaspur	Pauri	Rampur	Panchkula	Rupnagar
Una	Haridwar	Moradabad	-	SAS Nagar
Kangra	Dehradun	Bijnor	-	Gurdaspur
Chamba	-	Muzzafarnagar	-	-
-	-	Saharanpur	-	-

Table A2: State-level price index for neighboring states

States	1999-00	2000-01	2001-02	2002-03	2003-04	2004-05	2005-06	2006-07	2007-08
Himachal Pradesh	1.00	1.04	1.09	1.15	1.16	1.19	1.23	1.26	1.30
Uttarakhand	1.00	1.02	1.06	1.13	1.16	1.18	1.25	1.35	1.42
Haryana	1.00	1.05	1.09	1.14	1.18	1.23	1.28	1.36	1.49
Punjab	1.00	1.07	1.11	1.13	1.15	1.18	1.25	1.33	1.47
Uttar Pradesh	1.00	1.01	1.03	1.09	1.13	1.17	1.24	1.32	1.40
Chandigarh	1.00	1.03	1.09	1.14	1.19	1.28	1.39	1.46	1.51
Delhi	1.00	1.04	1.09	1.11	1.17	1.21	1.26	1.32	1.38

Notes: These deflators have been calculated using the state GDP at current and constant prices from the RBI Handbook of Statistics on the Indian Economy. Base year: 1999-2000.

Table A3: State×industry results for neighboring states

	(1)	(2)	(3)	(4)
Panel A: Log employment				
*post*treat*	0.371***	0.418	0.372	0.257
	(0.0414)	(0.0371)	(0.0416)	(0.119)
	[0]	[0.205]	[0.192]	[0.181]
R-squared	0.688	0.691	0.712	0.710
Panel B: Log of total factories				
*post*treat*	0.268***	0.291	0.270	0.253
	(0.0192)	(0.0311)	(0.0363)	(0.0777)
	[0.009]	[0.128]	[0.113]	[0.131]
R-squared	0.725	0.727	0.738	0.736
Panel C: Log of total output				
*post*treat*	0.579***	0.619	0.566	0.465
	(0.0500)	(0.0433)	(0.0606)	(0.211)
	[0]	[0.162]	[0.178]	[0.152]
R-squared	0.655	0.658	0.682	0.679
Panel D: Log of fixed capital				
*post*treat*	0.718***	0.878	0.866	0.625
	(0.0894)	(0.0538)	(0.0638)	(0.195)
	[0.004]	[0.183]	[0.157]	[0.123]
R-squared	0.668	0.672	0.695	0.691
Panel E: Log of wage bill				
*post*treat*	0.392*	0.322	0.275	0.271
	(0.0795)	(0.0514)	(0.0547)	(0.123)
	[0.066]	[0.199]	[0.157]	[0.179]
R-squared	0.660	0.663	0.685	0.683
Observations	2,567	2,567	2,567	2,567
state FE	Yes	Yes	Yes	Yes
year FE	Yes	Yes	Yes	Yes
3 digit industry FE	Yes	Yes	Yes	Yes
3 digit industry-year FE	No	No	Yes	Yes
time-varying controls	No	Yes	Yes	No
state level pre-trends	No	No	No	Yes
control group	Neighboring states	Neighboring states	Neighboring states	Neighboring states

Notes: The coefficient on the interaction term post*treat shows the treatment effect. Time varying controls include pre-treatment state-level variables (population, number of agricultural workers, number of industrial workers, number of main and marginal workers and number of illiterate people) interacted with a time dummy for each year. Standard errors in parentheses are clustered at the state level. Square brackets show p-values that are calculated using the Cameron, Gelbach & Miller (2008) wild cluster bootstrap-t procedure (999 replications). ***Significant at 1%, **significant at 5%, *significant at 10%.