Place-based Preferential Tax Policy and its Spatial Effects: Evidence from India's Program on Industrially Backward Districts

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ABSTRACT

We evaluate a tax-exemption program the Indian government initiated in 1994 to promote manufacturing in 123 industrially backward districts. The way the backward districts were identified enables us to employ a regression discontinuity design to evaluate the impacts of the program. We find that the program has led to a significant increase in firm entry and employment, particularly among the light manufacturing industries, in the better-off backward districts. Meanwhile, the program also resulted in significant displacement effects on districts which were neighboring these backward districts and relatively weak in economic activity. The findings emphasize that the spatial effects of place-based policies deserve greater attention from policy makers.

Keywords: place-based policy, spatial spillovers, preferential tax, backward districts, regression discontinuity
JEL codes: R12, O14, H32

1. Introduction

Place-based policies aimed at enhancing economic performance of certain areas within a country or within a region have been popular in both developed and developing countries. Examples of large scale place-based policies include the federal Empowerment Zone Program in the United States established in 1993, European Union (EU)'s various initiatives supported under its Structural Funds targeted at disadvantage areas and countries within EU, China's Special Economic Zones starting in late 1970s, just to name a few (see Neumark and Simpson, 2015 for a detailed review).

A common goal of place-based policies is to create jobs and spur economic activity by attracting new firms to and/or promoting local firm growth in the selected underdeveloped areas. The policies usually take one or a combination of the following forms: tax exemptions and subsidies, discretionary grants, special economic zones or industrial parks, and infrastructural support. In addition, place-based policies are often designed in a way with another popular category of development policies — industrial policies embedded in them. For instance, special economic zones or industrial parks are often set up to host certain industries such as high technology manufacturing.

The program studied in this paper is a typical placed-based policy which the Indian government initiated in 1994. The program identified 123 industrially backward districts out of 360 districts from 14 major states of India and offered tax exemption to new industrial firms located in those districts. The eligible firms, however, have to engage in manufacturing activities while those in services sectors were not covered by the program.

The interest in the impacts of a place-based policy is often two-fold. First, does the policy benefit the targeted areas as the policy makers expect? For a firm to locate in a particular place, it is possible that some basic conditions such as availability of basic infrastructure or human capital need to be met. Among several candidate places, the firm is likely to choose the optimal one based on an array of criteria while government policy may improve just one or some. Therefore, a preferential policy may not change the relative competitiveness of the targeted areas in attracting firms as much as expected. Second, what spatial effects does the policy generate? On the one hand, a place-based policy may result in positive spillovers to areas neighboring the policy-treated areas as increasing production and employment in the latter create more demand for inputs and services across geographical borders. On the other hand, the policy may produce considerable displacement effects in the untreated areas when firms in those areas choose to relocate to the treated areas for more profitable opportunities and/or are driven out of the market due to shrunk local demand or increased competition from the treated areas. The question of interest to both researchers and policy makers is which of the two opposite effects dominates.

This paper attempts to address three interrelated questions regarding the backward districts program. First, did the program lead to an increase in manufacturing firms and jobs in the targeted districts? Related to this, we also explore whether the program had different effects on light and heavy manufacturing industries and led to any positive spillovers to other untreated sectors through production linkages and agglomeration effects. Second, how did the program's impacts vary across the targeted districts that were in different degrees of backwardness? As discussed above, the program which essentially lowered the production costs through offering tax exemptions may not boost the attractiveness of all targeted districts to new firms. If so, it is important to understand who have actually benefited from such policies. Third, what are the net spatial effects generated by the program? How do they compare with the effects of the program on the treated districts?

A key challenge of evaluating the impact of placed-based policies lies in the fact that the targeted areas are not randomly selected in most cases. The non-backward districts do not necessarily constitute good counterfactuals for the backward districts. The approach that the Indian government used to identify backward districts, however, offers us a unique opportunity to assess the program's impacts more credibly. In short, the government assigned a gradation score to each district from India's 14 major states based on their historical indicators. The districts with scores below a cutoff point were designated backward districts and treated with the tax exemption policy (see next section for more detailed description). This setting allows us to use a (sharp) regression discontinuity (RD) design in estimating the causal effects of the program. Upon checking the pre-treatment district covariates, we show that the score is a good proxy for districts' demographic and development characteristics. Hence, the non-backward districts with scores right above the cutoff point are a sound control group for the backward districts right below the cutoff. Regressions with samples of districts near the cutoff point should yield plausible estimates of the program's effects. For estimating the program's spatial effects, we also resort to the gradation scores. We compare districts from the same score group with and without any backward district in their neighborhood. The identification works given that the districts with close gradation scores were highly similar at the beginning of the program. In an augmented model, we further control for the neighborhood characteristics of the districts to account for unobserved geographic factors that might have affected industrial development of a district.

Our main findings are as follows. First, the better-off backward districts, which account for about one third of the total with the gradation scores nearest to the cutoff, had experienced large increases in number of firms and employment in the light manufacturing industries (both more than 60%) by 1998, four years after the effectiveness of the policy. The effects on the heavy manufacturing industries are also positive but much attenuated. The policy also generated some moderate increases in the untreated sectors including construction, mining, and services in the backward districts.

There is also evidence suggesting that the program's impacts were largely concentrated in these better-off backward districts while the rest of the treated districts had not benefited much from the program. This implies that preferential tax treatment alone is unlikely to constitute sufficient condition for firm entry and employment growth in the more deeply challenged areas. Lack of infrastructure, human capital and financial market could remain as constraint to the economic development in those areas.

Our findings on the spatial interactions caused by the program turn to be more intriguing. The results show that there were considerable displacement effects, meaning fewer firms and smaller employment, in the relatively weak non-backward districts as well as the more difficult backward districts had they been neighboring with any better-off backward district. However, such spatial spillovers did not take place in the stronger non-backward districts whose gradation scores were far above the cutoff. Neither did the backward districts far below the cutoff point generate similar effects to their neighboring districts. Our estimation suggests that the numbers of firms and jobs displaced due to the spatial spillovers of the program outweighed those generated in the beneficial districts.

This paper contributes to a growing literature on place-based policies in the following ways.[1] Evidence on the effects of economic/enterprise zones in creating jobs, mainly from the developed countries so far, are generally mixed. For instance, Neumark and Kolko (2010) and Hanson (2009) found the California Enterprise Zones program and the Federal Empowerment

[1] See Neumark and Simpson (2015) for a comprehensive survey of the literature.

Zones program, respectively, had no significant effects in generating employment, whereas Freedman (2013) found positive effects for the Texas Enterprise Zones program as do Busso et al. (2013) for the Federal Empowerment Zones program. Our findings suggest the effects could vary with the economic conditions of the targeted areas with the relatively strong areas experiencing most positive effects.

Work on the spatial spillovers of relatively policies is still sparse. Givord et al. (2013) found strong displacement effects of the French Zones Franches Urbaines (ZFUs) program on the nearby non-ZFU areas, which were of comparable magnitude to the positive effects the program generated inside the ZFUs. However, the results for the US programs are quite mixed (e.g. Ham et al., 2011; Hanson and Rohlin, 2013). We add to this literature by showing that such policies could have significant displacements effects on the untreated areas which are economically close to the treated areas. The untreated areas which are substantially stronger than the neighboring treated areas may resist the negative spillovers of the policy however.

Placed-based policies are likely to bear more economic and political significance in developing countries as compared to developed countries. First of all, at the early stage of development, a country often expects to use placed-based policies to boost economic performance of certain areas such as the coastal regions, which are then anticipated to lead the development of the rest country. One such case is China's experiment with special economic zones. Wang (2013) found that SEZs increased the level of FDI and exports as well as average wages of workers in the hosting municipalities and generated a moderate displacement effect in the adjacent municipalities. Second, geographic disparities are arguably larger in developing countries. Placed-based policies targeting the weaker-performing areas often have dual objectives of promoting economic development and reducing inequality. Third, the underdeveloped areas often account for a larger share of population in a developing country. Policies favoring these areas are likely to win political support and considered important by politicians.

However, evidence about the impacts and spatial effects of the place-based policies in the developing countries is still limited. Closer to our paper is Chaurey (2016), which examines the New Industrial Policy of India's federal government that offered tax exemptions and capital subsidies for firms in two states, Uttarakhand and Himachal Pradesh, since 2003. Applying a difference-in-differences approach, the author showed that the policy resulted in large increases in outcomes such as employment, number of firms, and total output in the treatment states relative to the control states. In addition, he did not find any spatial displacement of firms underlying the impact of the policy.

Apart from similarities in policy content, the backward district program studied here differs from the New Industrial Policy along several dimensions. For instance, the program covered 14 major states of India, which are dispersed across the country and diverse in economic development. The New Industrial Policy provided tax exemption and capital subsidies to both new and existing firms while the backward district policy only provided tax exemption to the new firms. Moreover, the program was administered at the district level with selection of the treated districts based on pre-determined scores. This feature enables us to implement a distinctive identification strategy to examine the program's impacts and spatial effects. at a finer geographical level.

This paper is also connected with the literature on the location and growth of firms in response to local taxation. The available evidence is somewhat mixed. Some studies, e.g. Rathelot and Sillard (2008) find a weak response of firms' location choice to higher taxes, while others, e.g. Bartik (1991) and Guimaraes, Figueiredo and Woodward (2004), find a negative relationship. After correcting potential endogeneity issues, Duranton, Gobillon and Overman (2011) find that local taxation has a negative impact on firm employment but no effect on firm entry in the United Kingdom. Our results suggest that the taxation impacts may depend on the local economic conditions, which enter firms' decision functions as well.

2. Policy Background

Like many developing countries, the spatial pattern of economic activity in India is characterized by a concentration of industrial development around large cities and development skewed towards a few states. The coefficient of variation of per state domestic product had risen from 25% in 1950-51 to 35% in 1993-94 (Ghosh et al, 1996). In response, the Government of India has implemented various policies and programs to reduce regional imbalance and inequality since India's independence in 1947.

India's approach to balancing regional development can be distinguished into three phases: a first phase spanning 1948-80 characterized by heavy public sector involvement and direct central intervention (Singhi, 2012), then a period of nascent market oriented reforms in the 1980s, and finally a post-reform era commencing right after the dramatic trade and industrial policy reforms of 1991. In 1981, the National Committee on the Development of Backward Areas found subsidies and concessional finance to not be significant factors in motivating firms to locate their industrial units in disadvantaged areas (Planning Commission, 1981). Against the backdrop of political decentralization and a liberalizing global economy, the government eliminated many of the controls earlier exercised by the central government.

The Industrial Policy Statement of 1991 announced major changes to the government's role, reducing industrial licensing, relaxing industrial location policy, and allowing entry of large enterprises into small-scale industry sector (e.g. Martin et al. 2017; MCI, 1991). Despite the paradigm shift towards local planning in the post-reform era, a study conducted by the National Institute of Public Finance and Policy in 1987 suggested enlargement of central government tax incentives in the Income Tax Act to encourage entrepreneurship and industrial dispersal. Following this, the Finance Act of 1993 introduced a tax holiday scheme for new industrial undertakings[3] located in backward states and union territories.[4]

Immediately after the introduction of the 1993 Act, the Ministry of Finance commissioned a review to assess industrially-challenged districts located in the remaining 14 states, which were not designated backward. The Study Group on Fiscal Incentives adopted an index-based approach to select districts and proposed similar fiscal support to boost investment, industrialization, and job creation in these districts. Specifically, they developed a composite index based on 8 financial, infrastructural, and industrial indicators to approximate a district's degree of development. The individual scores on the indicators for each district are calculated as a percentage relative to India's nationwide average. The overall score is the weighted sum of the 8 individual scores with weights equal to 1, 2 or 3 (see Table A1 for details). We refer to the overall scores as "gradation scores" hereafter in that they were published in the "All India Gradation List" developed through the Finance Act of 1994 as Appendix III of the Income Tax Act.

Districts that had failed to score above 500 were accorded backward status, which qualified them for the preferential tax treatment enacted by the Finance Act of 1994. Out of the total 360 districts from the 14 states, 120 had gradation scores below 500 and were designated industrially

[3] Per section 3d of the Industries (Development and Regulation) Act of 1951, 'industrial undertaking' pertains to a scheduled industry carried on in one or more factories by any person or authority including the government.
[4] These are states and union territories located in the north-eastern and north-western parts of India and on the islands.

backward districts. Three additional districts were tagged as backward districts despite scoring above 500 due to non-score based characteristics including the district falling under the category of a "no industry"[5] district, or an inaccessible hill area district as indicated in the Eighth Plan Document, or if the district did not have a "railhead" as on April 1st, 1994.[6]

The map in Figure 1 shows that the backward districts spread contiguously across the eastern, central, and northern parts of India. Largely concentrated in Uttar Pradesh and Bihar, they extend from the southern border of Nepal to the northern districts of Madhya Pradesh and West Bengal and to the western border of Bangladesh. Outside this area, backward districts appear in a continuous belt in India's northwest through the states Rajasthan and Gujarat. Other contiguous areas are spread-out in central India within the states of Orissa and Andhra Pradesh. A notable feature of the country's spatial disparities is the presence of backward districts within advanced states such as Maharashtra and Karnataka.

The program, as stipulated in Section 80-IA of Income Tax Act, offered new industrial undertakings in the backward districts a tax holiday in which firms are granted tax deductions of 100% of profits and gains for the first five assessment years. After the initial five assessment years, deduction from the profits would be allowed at the normal rate of 30 per cent in the case of companies and 25 per cent in the case of non-corporate assessees. The deduction, at the enhanced rate and the normal rate together, was limited to twelve assessment years in the case of co-operative societies and ten assessment years in the case of other assessees. To be eligible for the benefits, the industrial undertaking had to "begin to manufacture or produce articles or things or to operate its cold storage plant or plants at any time during the period beginning on the 1st day of October, 1994 and ending on the 31st day of March, 2000." The program excluded a few industries or economic activities from receiving tax exemption such as manufacture of products of tobacco, alcohol spirits, confectionery, and aerated waters.[7]

The government further classified backward districts into categories A and B in September 1997. Those belonging to category A had scores of 250 or lower, or had scores between 251 and

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[5] The Government of India introduced the concept of 'no industry' districts in March 1982. The Government of India also introduced a scheme of assistance (basically to subsidize infrastructural development of the area) in April 1983. The 'no-industry' district is one where there is no industry requiring a capital investment in plant and machinery equal to or exceeding Rs 10 million. In such a district, the 'nucleus plant' or the 'mother industry' attempts to create ancillary industries over a widely dispersed area, and thereby tries to create employment opportunities for local people.

[6] The three districts are Idukki (618) and Wayanad (583) from Kerala and Jalapaiguri (728) from West Bengal. We do not include the three exceptions in our baseline analysis and show they have no influence on the results in our robustness checks.

[7] The full list of excluded items is specified in provisions of the 11th Schedule of the Income Tax Act, available at http://www.incometaxindia.gov.in/Acts/Income-tax%20Act,%201961/2013/102120000000027705.htm

500 and one of the non-score based characteristics as noted above. The full tax deduction was extended for another 5 years for category A districts and 3 years for category B districts.

3. Data

We combine establishment-level data from India's Economic Census of 1998 (EC 1998) and district-level data from the Primary Census Abstract of 1991 (PCA 1991) to evaluate the impact of the backward districts program.[8]

The EC, administered by the Central Statistical Organisation, Ministry of Statistics and Programme Implementation since 1977, provides a country-wide census of establishments engaged in all economic activities excluding crop production and plantations. As the fourth edition, EC 1998 contains key data on 30 million establishments from both rural and urban areas. For each establishment, we know the number of employees, major economic activity classified according to 1987 four-digit National Industry Classification (NIC 1987), location in terms of district and sub-district (e.g. towns and rural blocks), type of fuel used, and so on.

We collapse the micro-data to obtain the total number of firms and employment at the district level by 2-digit industry level for the analysis. Amongst 68 2-digit industries, which encompass 2,171 4-digit NICs, the treated industries include 7 light and 9 heavy manufacturing industries, and the untreated include 5 primary, 7 mining, 2 construction, 4 utilities and 35 services industries. In addition, we create an exclusion category to contain all the 4-digit manufacturing industries that were excluded or involved activities excluded from the program by the government. It is often the case that an excluded economic activity only represents a subset of industrial productions of a 4-digit NIC industry. For instance, the "latex foam sponge and polyurethane foam", which was listed as ineligible for the program, belongs to and accounts for a small portion of NIC 3020 "manufacture of plastics in primary forms; manufacture of synthetic rubber". Since we only observe data at 4-digit NIC level, treating an industry like NIC 3020 either as treated or untreated would result in some measurement bias. To address this issue, we apply three degrees of aggressiveness to categorizing a 4-digit NIC as an excluded industry and obtain three definitions of the exclusion category. The "middle path" definition including 50 4-digit NICs is used in the baseline analysis. In robustness checks we examine both the more conservative definition (25 NICs) and more aggressive definition (91 NICs).

[8] We use establishment and firm interchangeably in this study.

EC 1998 uses reorganized districts which had very different geographic boundaries from those in 1991. The latter, however, was used by the program in 1994. With regards to the 14 states under consideration in our study, there are 100 more districts in EC 1998 as opposed to the 360 in 1991. Fortunately, the reorganization of districts does not nullify our identification strategy in that the backward status accorded in 1994 was carried forward to the newly appointed districts. We construct the data following the 1991 definition of districts so as to match them with the gradation scores. For the most common cases whereby an old district in 1991 was split into multiple ones by 1998, we simply collapsed the data of the new districts into the old district. For a few complex cases whereby a new district was formed by parts carved out from multiple old districts, we partition the data of the new district using population weights developed in Kumar and Somanathan (2009), and merge them back into their original districts.

Our final dataset allows us to work with 24,840 district-industry units from 360 districts and 69 industry categories. 3,016 or 12% of the units equal zero implying there were no firms and employment in those districts by 2-digit industry cells. For the baseline results, we transform the number of firms and employment by log(Y+1) to be the dependent variables to keep all the units in the analysis. We also take log(Y) as dependent variables and leave those zero observations out of the sample in a robustness check. Although a more disaggregated unit is possible from the EC (e.g. sub-district by 3-digit level), the larger sample comes at the expense of obtaining extremely high frequencies of zero observations. By keeping our analysis at the district and 2-digit industry level, we strike a balance between sufficient non-zero observations and adequate sample size.

In our analysis, we divide all the districts into six groups from the most challenged to most advanced based on the gradation scores and label them from 1 to 6, which are Group 1: <250, Group 2: 251-350, Group 3: 351-500, Group 4: 501-650, Group 5: 651-850, and Group 6: >850. As such, Groups 1-3 were the treated districts and 4-6 were untreated. Groups 3 and 4 are the treated and untreated districts nearest the cutoff point, respectively, Groups 2 and 5 farther away, and so forth. Table 1 presents the state-wise distribution of backward and non-backward districts by groups. All groups except the most advanced Group 6 have approximately the same number of districts. 11 out of 14 states have both backward and non-backward districts. States Bihar and Uttar Pradesh host over a quarter of the backward districts each, while another quarter or so are located in Madhya Pradesh and Rajasthan. Most districts of the 3 states, Haryana, Punjab and Tamil Nadu, which do not have any backward districts, fall in Group 6.

Table 2 presents summary statistics of the numbers of firms and employment by light, heavy and other industries for each of the six district groups in 1998, four years after the program was in place. On average, there are more firms and employment in the light manufacturing industries than in the heavy manufacturing or remaining industries. As a general pattern, the average numbers of firms and employment go up with the district's gradation score. However, it is interesting to note that the rising pattern shows a downward break between Group 3 (score 351-500) and Group 4 (501-650) and resumes after Group 4, whereby the mean counts of firms and employment of Group 3 are considerably larger than those of Group 4. For instance, there are on average 1,179 firms and 2,803 employees in each 2-digit industry by district unit in the light manufacturing industries of the Group 3 districts whereas the numbers drop to 963 firms and 2,528 employees in the Group 4 districts. The break is particularly evident for the light and heavy manufacturing industries, i.e. the industries eligible for the program and less so for all other industries.

The Primary Census Abstract (PCA) was compiled district-level data based on the Indian census data conducted every decade. The 1991 edition of PCA covers all states except portions of the state Jammu and Kashmir, which fall outside the geographic scope of the program. The PCA provide us with reliable data on population, area, literacy rate, sectoral employment, etc., which serve as control variables in our regressions. Means and their standard errors of these covariates by treatment status are provided in the left panel of Table 3a.

4. Program Impacts on the Backward Districts

4.1 Empirical strategy

The major identification challenge to evaluating the effects of the backward district program is that the backward districts are likely to be substantially different from the non-backward districts on both observables and non-observables. However, the way the government used to designate the backward districts offers us a solution to credibly estimate the causal impacts of the program.

As described earlier, each district covered by the program was assigned a composite score in 1993 which was computed based on the district's 1991 census data. The treatment status was determined strictly on whether the score is above the cutoff point of 500 or not. It is hard to conceive of any way through which a district could manipulate its score to make itself eligible for the program. In this case, the variation in treatment status could be considered as good as randomized for districts in the neighborhood around the cutoff (Lee and Lemieux, 2010), which allows us to apply the (sharp) regression discontinuity (RD) design to estimate the program's impacts on the economic activity in the backwards districts.

To verify the assumption above, we compare the pre-treatment variables between the treated and untreated districts from various neighborhoods around the cutoff score. The left panel of Table 3a shows comparison between all backward districts and non-backward districts. The t-tests indicate that the two sets of districts differ statistically significantly in several characteristics: the backward districts had smaller population, fewer main workers (who had worked 6 months or more in the survey year), fewer workers in manufacturing and in trade and commerce, fewer residential units, and substantially lower literacy rate than the non-backward districts. We then narrow the bands for comparison by focusing on the districts with scores between 251 and 500 versus those between 501 and 850 (i.e. Groups 2 and 3 versus Groups 4 and 5) in the middle panel of Table 3a. The mean differences between the treated and untreated districts remain negative for all variables and statistically significant for majority of them, although the differences in absolute value decline in a pronounced way.

The right panel of Table 3a compares the backward districts from Group 3 (scoring 351 to 500) with the non-backward districts from Group 4 (501-650). The t-tests show none of the variables is statistically different between the two groups. More importantly, the vanishing of the statistical significance is mainly driven by the diminished mean differences (in absolute value) rather than the enlarged standard errors of the differences due to the smaller number of districts in the comparison. Moreover, the mean differences of the population, residential units and the employment variables between the treated and untreated districts turn positive.

Table 3b compares the residual means of the pre-treatment variables after regressing them on a 3rd order polynomial function of the gradation scores. The number of variables with statistically significant difference drops from 6 to 3 in the case of full sample comparison, and from 8 to 4 when the districts from two extreme ends, i.e. Groups 1 and 6, are removed from the comparison. Reaffirming the results with raw means, no variable is statistically different when districts from Group 3 are in contrast to those from Group 4.

Overall, the exercise confirms that in a neighborhood that consists of 38 treated districts and 39 untreated districts surrounding the threshold, the treatment status of the backward district program is assigned as randomized. Controlling for a flexible function of the gradation scores helps to balance the treated and untreated districts with respect to the pre-treatment characteristics when they are farer away from the cutoff point. However, the balancing appears imperfect given that a few characteristics remain statistically different across the treatment status.

We estimate three specifications of RD models. The baseline model is of the form:

$$Y_{id} = \beta_0 + \beta_1 T_d \times S_i + \gamma T_d \times (1 - S_i) + f(Z_d) + X_d^{1991}\phi + \eta_i + \eta_s + \varepsilon_{id} \quad (1)$$

where Y_{id} is log of number of firms or total employment in 2-digit industry i of district d in 1998. T_d is a binary indicator equal to 1 if district d is designated backward, and S_i is binary equal to 1 if industry i is a manufacturing industry eligible for the program. $f(Z_d)$ represents a flexible function of the gradation score and we use 3rd order polynomial function in actual estimation. To get meaningful coefficient estimates for the gradation scores, we use relative scores to the cutoff point, i.e. raw scores divided by 500 as Z_d. X_d^{1991} is an array of pre-treatment district covariates measured in 1991 Census including area, population, numbers of main workers, primary workers and manufacturing workers, all in log terms, worker participation rate and literacy rate. η_i and η_s are industry and state fixed effects, respectively.

In equation (1), β_1 and γ are the parameters of primary interest. We expect β_1 to be positive had the program directly impacted the manufacturing industries in the backward districts, and γ to be positive had the program generated positive spillovers to other industries within the districts through input-output linkage or other agglomeration channels.

There are notable differences between light and heavy manufacturing productions. Relative to heavy industry, light manufacturing may be characterized with smaller capital investment, less skilled labor force and application of less sophisticated technology. In the absence of industrial policies favoring the latter, light industries often get developed ahead of heavy industries in the underdeveloped regions. Therefore, it is reasonable to conjecture that the tax incentives offered by the program, which simply relieved some financial burden on the eligible firms, are more likely to promote growth in light manufacturing in the backward districts. In the second model, we explore possible heterogenous impacts of the program on light manufacturing and heavy manufacturing industries by estimating:

$$Y_{id} = \beta_0 + \beta_1 T_d \times S_i^l + \beta_2 T_d \times S_i^h + \gamma T_d \times (1 - S_i) + f(Z_d) + X_d^{1991}\phi + \eta_i + \eta_s + \varepsilon_{id} \quad (2)$$

where S_i^l (S_i^h) equals 1 if industry i is one of the treated light (heavy) manufacturing industries as explained in the data section. If the above conjecture holds valid, we expect that $\beta_1 > \beta_2 > 0$ in equation (2).

Considering that the strength of the input-output linkage between an untreated industry and the treated industries could vary substantially, which may affect the estimation of the spillover effects, we construct a measure of the input-output linkage between industry i and all the treated industries, W_{iT}^{IO},[9] and incorporate it into equation (2) to estimate:

$$Y_{id} = \beta_0 + \beta_1 T_d \times S_i^l + \beta_2 T_d \times S_i^h + \gamma T_d \times W_{iT}^{IO} \times (1 - S_i) + f(Z_d) + X_d^{1991}\phi + \eta_i + \eta_s + \varepsilon_{id} \quad (3)$$

Coefficient γ could be estimated more precisely in equation (3) than equation (2) if W_{iT}^{IO} captures the input-output channels between the treated and the untreated industries.

We estimate models (1)-(3) for a sample consisting of Groups 3 and 4, which are the respective treated and untreated districts closest to the cutoff point. Tests presented earlier indicate that the variation in the treatment status between districts of Group 3 and Group 4 could be regarded as random since the two groups exhibit statistical indifference along several pre-program characteristics. This lends credence to our estimates with this sample as our principal results. We also extend the estimation to 2 larger samples: one including districts from Groups 2 to 5 or scoring between 250 and 850 and the other with all the districts. We compare them with the principal estimates and discuss the possibility that the estimated treatment effects with these expanded samples may be confounded by potential unobserved factors given the differences in some pre-treatment covariates between the backward and non-backward districts in these two samples.

4.2 Results

Before reporting RD regression results, we plot the log transformed counts of firms and employment at 2-digit industry by district level against the district's gradation scores in Figure 2. The top, middle and bottom panels plot data of light manufacturing, heavy manufacturing and

[9] Details on the construction of W_{iT}^{IO} are presented in Appendix 1.

other industries, respectively. Each dot represents mean counts of firms (left panel) or employment (right panel) averaged across 2-digit industries within the category (i.e. light manufacturing, heavy manufacturing and other) and districts falling in a bin of size of 40 gradation scores. The solid line is local polynomial fit with degree 1 and bandwidth equal to 200. The dashed lines are 95% confidence interval of the local polynomial estimation.

In all six plots, we can see a downward gap between the two solid lines at the cutoff point where gradation score equals 500 although both lines increase in general with the scores. The gaps are larger for the light manufacturing than for the heavy manufacturing. Interestingly, visible gaps also exist for other industries which were not covered by the tax exemption program. Once we scrutinize the graphs, we can see the gaps are at least partly due to the segment of the left-hand lines near the cutoff point warping up. Finally, the graphic patterns shown on firm counts are identical to those on employment. Figure 2 implies that the program could have a positive impact on firm entry and employment in the backward districts, especially those closer to the cutoff point with relatively higher gradation scores. Moreover, the light manufacturing seems to have benefited more than the heavy manufacturing and industries not covered by the program may also have experienced growth due to the program. Below, we present regression analyses to further validate these initial findings.

Table 4 reports the main estimation results by contrasting the backward districts of Group 3 (i.e. scoring between 351 and 500) and non-backward districts of Group 4 (i.e. between 501 and 650). Two dependent variables, log transformed firm counts and employment, are examined, respectively. In addition to the state and 2-digit industry fixed effects, the control variables include a 3rd order polynomial function of the gradation score, where the raw score is divided by the cutoff value of 500 to obtain meaningful coefficient estimates, and pre-treatment district covariates. The robust standard errors are clustered at state level.

Regressions in columns (1) and (4), corresponding to equation (1), show that the number of firms of the eligible manufacturing industries in the backward districts of Group 3 is on average 46% higher than that in the non-backward districts of Group 4, and the employment is 45% higher. Both estimates are statistically significant at 1% level. Not only had the eligible industries experienced considerable growth in Group 3 districts, the rest industries, mainly the service sectors, in these districts also seemed to have benefited from the program. The estimation suggests that the number of firms and employment of the untreated industries are 14-15% larger in Group 3 districts than in Group 4 districts. This is not surprising since different industries are interlinked in multiple ways. One prominent linkage between the manufacturing and services sectors is the input-output channels. Agglomeration of manufacturing firms could increase demand for inputs from services providers as well as lower costs of supplies to the services.

Columns (2) and (5) show estimation results of equation (2) that differentiates eligible light manufacturing and eligible heavy manufacturing. As expected, the program had a greater impact on the light manufacturing industries than on the heavy industries. Compared to their counterparts in the non-backward districts of Group 4, the light manufacturing of Group 3 districts is on average 69% larger in firm counts and 68% larger in employment while the heavy manufacturing is 28% and 27% larger, respectively. The program's effects on the light manufacturing are nearly 3 times of the effects on the heavy manufacturing, which confirms our conjecture that relative to the light manufacturing, heavy manufacturing sectors face more severe constraints in the underdeveloped areas and thus are harder to be boosted by a tax incentive only. By model construction, the coefficient estimates for the interaction of ineligible industries and backward districts remain the same as in columns (1) and (4).

We further investigate to what extent the program's spillover effects on the untreated industries in the backward districts would change once the input-output linkage between an untreated industry and all the treated industries is quantified and taken into account as in equation (3). The results are presented in columns (3) and (6). The estimated impacts of the program on the treated industries decline slightly and the impacts on the heavy manufacturing turn statistically insignificant. The coefficient estimates of the interaction of the backward district dummy and the untreated industry dummy multiplied by the industry's input-output indicator W_{iT}^{IO} are very close to those in equation (2) without incorporating the input-output indicator: 0.123 versus 0.137 for firm counts and 0.142 versus 0.149 for employment, although the former is not statistically significant.

As far as the control variables are concerned, the patterns are stable across different specifications and dependent variables. None of the three terms with the relative gradation scores is statistically significant. This is possibly due to the fact that the two groups of districts are sufficiently close as measured by the gradation scores. Among the pre-treatment covariates, a district's area, population, labor participation rate in 1990 are positively correlated with its economic activity in 1998, but the coefficients are not statistically significant. Literacy rate, as a proxy for the district's human capital, plays a positive role in the region's development with 10%-level significance. Both numbers of primary workers and workers engaged in manufacturing before the program show pronounced positive effects on the district's non-agriculture production and employment. The former may serve as an indicator of the potential labor source for manufacturing and services. The latter may capture some unobserved factors such as local regulations or business convention that are related to the district's industrial development but missing by the gradation scores. Controlling for these covariates enhances the credibility of our estimates on the program impacts.

In sum, Table 4 shows that the backward district program had a considerable impact on the development of local manufacturing when we compare a subset of the treated districts with a subset of the untreated districts, both featuring gradation scores near the cutoff point and thus statistically identical in a series of pre-program characteristics. Moreover, the program had been more helpful for light manufacturing industries than heavy industries and generated moderate positive spillovers to the untargeted sectors such as services. Next, we would like to explore whether the same findings hold true for other backward districts.

Table 5 reports estimation coefficients of key variables for two expanded samples that contain districts from Groups 2 to 5 (i.e. gradation scores ranging from 251 to 850) and all the districts. Columns (1) and (4) in the upper panel show that the program had positive but statistically insignificant effects on the eligible industries in the treated districts of Groups 2 and 3 as opposed to the untreated districts of Groups 4 and 5. When we distinguish the light manufacturing and heavy manufacturing in the model, the results suggest that the program had led to an average of 30-35% increase in light manufacturing firm and about 31% employment growth in the sector. Both are statistically significant at 5% or 1% level. On the other hand, the heavy manufacturing sector did not seem to benefit much from the program. The coefficient estimates are small and positive for firm counts and negative 9% for employment while neither is statistically significant.

Comparing these estimates with those in Table 4, the impacts of the program on light manufacturing drop by half although remaining statistically significant. The impacts on heavy manufacturing as well as the ineligible industries turn smaller in general and not distinguishable from zero. This suggests that the program had virtually zero effect on Group 2 districts. When the two groups with similar numbers of districts (44 in Group 2 and 38 in Group 3) are pooled together, the estimates are approximately the average of the program's respective effects on Group 2 and Group 3 or half of the effect on Group 3 districts. This interpretation appears consistent with the graphic patterns shown in Figure 2.

The lower panel of Table 5 shows estimates based on the full sample including all the districts. Across all eligible manufacturing industries, the backward districts had 12% fewer firms and 43% less employment on average than the non-backward districts with the latter estimate statistically significant. The estimates diverge in opposite directions when we separate light and heavy industries. Columns (2) and (3) show that the backward districts had 22-26% more firms in the light manufacturing but 36-39% fewer firms in the heavy manufacturing. For employment, the difference in light manufacturing diminishes and turns insignificant while the difference in heavy manufacturing enlarges to negative 78-81%.

While the positive coefficient estimates for the firms counts in light manufacturing may still be attributed to the program intervention with most impacts falling on the Group 3 districts, it is unlikely that the program led to a considerable reduction in firm numbers and employment in the heavy industries in the treated districts. More plausibly, when the sample expands to include districts farther away from the cutoff point, the estimation results tend to reflect the greater initial development gaps between the treated and untreated districts, which were not fully captured by the control variables of the model.

Taking Tables 4 and 5 together, we find that the program had generated expected growth in the eligible manufacturing industries and moderate positive spillovers to the untargeted sectors. in the relatively better-off backward districts with the more challenged districts benefiting little. The results are sensible in that the cost disadvantages in those better-off backward districts could be marginal , so tax relief offered sufficient incentives for the investors and entrepreneurs to establish and run enterprises and thus promoted local industrial development. In contrast, the backward districts far below the threshold were facing deeper constraints on development. The resulting cost disadvantages in these areas are much more significant, which could not be compensated for merely by a reduction in the tax burdens. Hence, the tax exemption program failed to make a visible difference in those districts.

4.3 Robustness checks

We show that the main results in the preceding section are robust to variations in three respects. First, we use simple log-transformation of the number of firms and employment, i.e. log(Y), as the dependent variables instead of log(Y+1) transformation used in the baseline. The estimation results regarding the program impacts, presented in the Appendix Tables A2, are congruent with the main findings in the baseline despite the fact that about 12% of the observations under the simple log-transformation drop off due to zero counts of firms and employment in those district-by-industry units. In particular, we see that the program has increased the number of firms and employment in the light manufacturing industries in the Group 3 districts substantially when the sample contains backward and non-backward districts around the cutoff only. The effects on the heavy manufacturing are also positive and statistically significant but much smaller than those on the light manufacturing. Meanwhile, we note that the coefficient estimates for the untreated industries are not statistically significant in any case though being positive. When the sample expands to cover districts of Groups 2 and 5, the effects drop by half for the light manufacturing and turn undifferentiable from zero for the heavy manufacturing. The full-sample estimates also mimic those in the baseline. In sum, not including the zero-valued units in the analysis does not alter our findings.

Secondly, we use a more aggressive definition and a less aggressive definition of the exclusion category, respectively, to substitute for the intermediate definition used in the baseline analysis. Note a change to the exclusion category will cause some changes to the scope of the eligible industries too. We rerun all the models and obtain essentially identical estimation results.

Finally, we include the three districts which had gradation scores above 500 but were categorized as backward districts due to non-score based characteristics in the sample. We count them as Group 1 districts since they were classified into the same category as those scoring below 250 by the policy. Again, inclusion of these districts results in no difference to our baseline estimates.[10]

5. Spatial Effects of the Program

5.1 Empirical strategy

[10] The estimation results of the second and third robustness checks are available from authors upon request.

A place-based policy could have positive spatial effects as the policy-induced production will generate additional demand for inputs and services across the border. On the other hand, capital and entrepreneurs are footloose. They may move to the treated areas to access the benefits of the policy directly, especially when the moving cost is not prohibitively high. In this section, we investigate the spatial effects of the backward district program, i.e. whether the program has on net created a displacement or agglomeration impact on the neighbor districts. The strategy we employ is to compare districts from the same score group with and without any district from a treated group in their neighborhood. This allows us to compare districts which are similar to each other along the dimensions captured by the gradation scores but differ by whether being adjacent to a treated district.

The results in Section 4 suggest that the program treatment effects were largely concentrated in the relatively better-off backward districts, i.e. Group 3 districts with scores between 351 and 500. Thus, we start with a model as follows to estimate whether neighboring with a Group 3 district make any difference for each group of districts:

$$Y_{id} = \theta_0 + \theta_3 N_d^3 + f(Z_d) + X_d^{1991}\phi + \eta_i + \eta_s + \varepsilon_{id} \quad (4)$$

where N_d^3 is equal to 1 if district d has a neighbor district from group 3. θ_3 measures the net spatial effects of the program with negative (positive) estimates implying that the program's displacement (agglomeration) effect outweighed its agglomeration (displacement) effect. The equation (4) is estimated for six samples each consisting of districts from one of the six groups, and results are presented in Table 7.

There may be concerns that the districts with different neighbors may differ systematically in their geographic locations, which are also correlated with their industrial development, even though they have similar gradation scores. For instance, the districts from Group 1 that have no neighboring district from Group 3 may be clustered in a remote or hilly area while those with Group 3 districts as neighbors are located in more accessible or development-prone areas. To the extent that existing control variables fail to account for this geographic heterogeneity, estimation of equation (4) may yield biased spatial effects of the program.

To address this, we first characterize the neighborhood of districts by group. The top two rows of Table 6 show the number of districts in each group and their average gradation scores. Below them, the districts from each group are split into those with and without at least one district from the group as indicated in the panel heading. The counts and average gradation scores of these two subgroups are presented. For instance, Column (2) of Panel A shows that among 44 districts of Group 2 (scores between 251-350), 24 districts with average gradation score equal to 298 have one or more neighboring districts from Group 1 (scores equal to or below 250), and the rest 20 with average score of 306.5 do not have any neighboring districts from Group 1.

Browsing through Table 6, a pattern of clustering can be discerned. The proportion of districts neighbored with districts from their own group is higher than that with neighboring districts from another group. Districts from the low-score groups are more likely to have neighboring districts from the low-score groups than from the high-score groups, and likewise for the high-score districts.[11] While this echoes what we have seen in the map of Figure 1, clustering is far from the whole story. For each group, there are generally 2-digit numbers of districts which have or do not have any district from one of the six groups in their neighborhood except for a few cases in Panel F. Moreover, the average scores between the two subgroups within each pair are highly close to each other except for the most advanced Group 6. For instance, the difference in the average scores of two subgroups of Group 2 with and without a neighboring district from Group 1 is 8.5 or 2.8% of the group average (Panel A, column (2)). The picture painted here is that the geographic distribution of districts is fairly interlocking in view of the gradation scores. Within each group, the districts' industrial development did not differ dramatically with their neighbor characteristics.

Furthermore, we estimate an augmented model by adding dummies of neighboring districts from other groups to better proxy for the geographic features of a district as follows:

$$Y_{id} = \theta_0 + \sum_{g=1}^{6} \theta_g N_d^g + f(Z_d) + X_d^{1991}\phi + \eta_i + \eta_s + \varepsilon_{id} \tag{5}$$

where N_d^g indicates whether district d has a neighboring district from Group g. By controlling for such a series of neighborhood indicators, we hope the model could better capture the geographic characteristics of a district. For example, estimate of θ_3 is conditional on that the districts under comparison have the same neighboring districts from all groups other than Group 3, and thus is more likely to be unbiased.

[11] Each group has a number of neighboring districts from group 6 because group 6 contains more districts.

In addition, estimating equation (5) also allows us to carry out some kind of placebo tests. While we expect θ_g, where $g <= 3$, to measure spatial effects of the program, if any, we expect no measurable effects of having in their neighborhood the untreated districts, i.e. those from Groups 4-6, should our identification strategy be valid. Again, equation (5) is estimated for the districts from each of the six groups, respectively. Table 8 reports the estimation results.

5.2 Results

Table 7 reports the estimated θ_d^3 of equation (4) with number of firms or employment as the dependent variable and each column representing one group of districts as the sample. All models control for state dummy, 2-digit industry dummy, 3rd polynomial function of the gradation score and pretreatment district covariates. Take column (1) of the upper panel as an example. The coefficient estimate suggests that a Group 1 district neighboring with at least one district from Group 3 had 8% fewer firms than a Group 1 district without any neighbor district from Group 3 *ceteris paribus*, though it is statistically insignificant.

Table 7 shows three interesting results. First of all, districts from Group 2 and Group 4 neighboring with at least one Group 3 district had fewer firms and smaller employment as opposed to the group peers not neighboring with any Group 3 district. The estimates, 24% for Group 2 and 29% for Group 4, are both economically and statistically significant. Second, whether having a Group 3 district in the neighborhood does not seem to affect those from other groups, i.e. Groups 1, 3, 5 and 6. The magnitude of the estimates for these groups are smaller relatively to those for Groups 2 and 4, and none is statistically significant. Third, there is notable consistency between the estimates for firm counts and employment.[12]

Overall, the results suggest that the program had resulted in considerable displacement effects across the borders of Group 3 districts, the main beneficiary of the program. However, the displacement effects were largely confined to the districts not only spatially close, but also economically close to Group 3 districts. This is plausible since the development gaps between these districts and the Group 3 districts were relatively marginal and thus the policy favoring the latter appeared sufficiently attractive for the firms in the former to relocate.

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[12] We also estimated models that differentiate the treated and untreated industries, but did not find larger negative estimates for the treated industries. This means that many more firms and employment in the untreated industries were displaced than in the treated industries. We discussed this more in section 5.3 and speculate that it is because many untreated firms were not able to relocate.

Noteworthy is that the Group 2 districts neighboring with a Group 3 district also experienced negative spillovers due to the program even though Group 2 districts were also covered by the program. The displacement taking place in the Group 4 districts suggest that favorable taxation treatment could act to some extent as a substitute for a business-conductive environment in firms' location choice. On the contrary, the displacement effects on the Group 2 districts imply that tax incentives and better business environment could also work as complements to boost economic activities. Tax incentives could make the location with more productive environment more attractive to firms.

The results in Table 8 show the robustness of the preceding findings and reinforce them. Conditional on other neighboring group dummies, districts of Group 2 and Group 4 that had neighbor district(s) from Group 3 had on average 30% and 22% fewer firms and 28% and 22% smaller employment, respectively, as compared to their peers in the same group but not neighboring with any Group 3 district. The effects are statistically significant at 5% for Group 2 and 1% level for Group 4. Moreover, the most backward districts (Group 1) were also affected adversely due to neighboring with Group 3 district(s), to a lesser extent though. The estimates are -16% for both firm counts and employment and statistically significant at 10% for firm counts. At last, consistent with Table 7, the more advanced non-backward districts, i.e. those from Groups 5 and 6 did not seem to differ by whether neighboring with a Group 3 district.

It is intriguing to see that the above results are unambiguously supported by the regressions for Group 3 districts. Column (3) show that when a district of Group 3 had a neighbor from Group 2 or Group 4, it got better off with 20% or 30% more firms and 20% or 24% more employment correspondingly than the other Group 3 districts. Neighbor districts from Group 1 may have also contributed positively to Group 3 districts' firm number though the estimate is not statistically significantly (coefficient is 0.171 with standard error 0.0967). Furthermore, having a neighbor from Group 5 or 6 did not make a Group 3 district better or worse off.

The spatial interactions between the districts of Group 1, 2 and 4 and the Group 3 districts in their neighborhood are more attributable to the treatment of the program than any thing else. If neighboring with Group 3 districts proxies for some unobserved historical advantages or simply spillovers from a better neighbor for Group 1 and 2 districts, the coefficients should be positive instead of negative.[13] If districts of Group 4 with a neighbor from Group 3 were at disadvantage

[13] We do see positive but statistically insignificant coefficients for Groups 1 and 2 with neighboring districts from Group 4 and

compared to the rest Group 4 districts, as the negative coefficients might suggest, similar effects should be observed for those with neighbor districts from Groups 1 and 2.

It is worth noting that similar spatial effects did not rise with the two more disadvantaged groups, i.e. Groups 1 and 2. First, none of the estimates in Columns (2) and the second row of each panel in Table 8 is statistically significant except those involving Group 3 districts. This suggests that Group 2 districts did not produce much detectable spatial spillovers, positive or negative, to their neighbors. There are a few exceptions when it comes to Group 1. The districts of Group 1 and Group 6 with a neighbor from Group 1 had fewer firms and employment than those without. In addition, the Group 1 districts neighboring with a Group 4 or 5 district as neighbor had more employment than those without.

However, the differences are more likely to capture something related to geographic clustering instead of the consequence of the program. Neighboring with Group 1 districts implies a location in the more difficult areas, which are not fully accounted for by the rest of the model, and thus those areas were more underdeveloped industrially compared to their group peers. Moreover, had the effects in Group 6 districts been due to the treatment on Group 1 districts, we would expect to see a similar, if not stronger, displacement taking place in the less advanced areas, i.e. Group 4 and 5 districts, as well as a positive effect on Group 1 districts neighboring with Group 6 districts assembling the symmetry we see above on Group 3 districts. However, neither is the case in the results.

To sum, although districts of Group 1 and Group 2 received treatment from the program, we do not find any systematic evidence of spatial effects between them and their neighbor districts as we do for Group 3 districts. This also echoes our finding in the earlier section that the program impacts mainly fell on the districts of Group 3.

Regressions in Table 8 offer a chance for us to undertake placebo tests to validate the above results further. The idea is that if the spatial effects identified with respect to the Group 3 districts were attributable to some unobserved confounding factors, such as geographic or historical (dis)advantages imperfectly controlled for in the model rather than to the program treatment, it is likely that we would observe similar patterns of coefficients with respect to districts of Groups 4, 5 or 6 although they were not treated by the program.

above.

The estimation results dismiss such alternative explanations about the spatial effects. In general, neighboring with districts from Group 4, 5 or 6 did not lead to statistically significant effects, in particular negative effects, on districts with gradation scores below or above them. In addition, the Group 4, 5, or 6 districts did not benefit from neighboring with districts one group above or below them like Group 3 districts did. [14] Thus, the additional evidence is supportive to the finding that the program had generated significant displacement across borders of the relatively sound backward districts.

We again conducted robustness checks on the spatial effects estimation with different transformation of the dependent variables, alternative definitions of the excluded manufacturing industries and inclusion of the three districts not complying with the threshold. Tables A3 and A4 report the estimates when the dependent variables are simply log-transformed dropping the observations with zero counts. The other two sets of robustness results are available from authors upon request. The main findings in Tables 7 and 8 are all retained in these exercises.

All together, we reach the conclusion that the better-off backward districts, whom the program mainly benefited, had shown negative spillovers to the districts spatially as well as economically close to them. Well, there could be positive spillovers due to agglomeration effects for some industries, the displacement effects seem to be dominant on average. Firms in both the more challenged backward districts and the relatively weak non-backward districts were either attracted to relocate into the better-off backward districts or driven out of the market due to shrunk local market. For those who were substantially stronger than the best treated districts, the program did not have any adverse effect on them. Finally, the more challenged districts that accounted for a majority of the backward districts did not only fail to benefit from the program, but also lost opportunity of industrial development to their neighbors who were stronger and covered by the program as well.

5.3 Displacement effects vs. direct program impacts

This section quantifies the displacement effects in terms of the lost firms and jobs as opposed to the increased firms and jobs in the respective districts. We undertake the exercise

[14] There are positive effects on employment for Group 1 districts neighboring with Group 4 and 5 districts and for Groups 4 and 5 districts neighboring with Group 6 districts. These exceptions may be results of geographic clustering and pose no threat to findings on the program-induced spatial effects.

with our favorite estimates from columns (2) and (5) of Table 4 and those from columns (2) and (4) of Table 8. For program's direct impacts, we first predict the firm numbers and employment with the estimated regression coefficients in columns (2) and (5) of Table 4, respectively, for the Group 3 districts. Then, we set the backward district dummy to zero and re-predict the firm numbers and employment with the same regression coefficients. The differences between the two predictions are the increased firms and employment due to the program and summed across all Group 3 districts and 2-digit industries for light manufacturing, heavy manufacturing and untreated industries.

The procedure is similar for the displacement effects. Two sets of predictions are generated based on the estimated regressions in columns (2) and (4) of Table 8, one with the actual data and the other with the actual data but setting the dummy "with neighbor(s) from Group 3" to zero. The gaps between the two predictions are summed for Group 2 and Group 4 districts which are neighboring with one or more Group 3 districts.

The results are presented in Table 9. First of all, the estimates suggest that the program has increased about 344,000 firms and 812,000 employees across all industries in 38 Group 3 districts after four years since the launch of the program. About one third of the increased firms and employment came from the light manufacturing industries while those from the heavy manufacturing only accounted for 5% and 7% of the total, respectively. Although the coefficient estimates for the untreated sectors are moderate, they translate into large absolute numbers mainly because there is a larger number of 2-digit untreated industries.

In total, the lost firms in both Group 2 and 4 districts neighboring with Group 3 districts registered at nearly 600,000 and the lost employment at over 1.2 million, which exceed the estimated increases in firms and employment in Group 3 districts. Meanwhile, we note that the total displacement estimates are largely driven by those from the untreated industries. The displacement rates, calculated as the total loss in Group 2 and Group 4 districts as a percentage of increase in Group 3 districts, are only 50% in firm counts and employment for the light manufacturing industries and slightly above 100% for the heavy manufacturing industries. However, the displacement rates are as high as 250% for firms and 210% for employment for the untreated industries. The gaps for the untreated industries cover more than the overall gaps between the losses and increases of firms and jobs.

The differential displacement rates seem plausible as the policy-eligible firms, especially those doing light manufacturing, could benefit from relocation into the treated districts, but the prospects for the ineligible firms to relocate could be highly uncertain. Although they may be able to retain some of the original business relations through relocating with the eligible firms, they also expect to handle competition and develop new business in an unfamiliar environment so as to compensate for what they left in the hometown. Thus, many firms in the untreated industries of the adversely affected districts had no better choice but stay and exit the market as the local market shrunk.

The comparison suggests that the program has led to a net drop of about 420,000 employment to the country as a whole. However, it is necessary to understand how the program has affected the productivity of firms across industries and districts in order to make a comprehensive assessment of the program's welfare implications. This is beyond the scope that our current data allows.

6. Conclusion

Place-based policies have been popular in both developed and developing countries. However, rigorous assessments of their impacts have mainly focused on developed country experiences. As a step to fill this gap in the literature, this paper evaluates a nation-wide program initiated by the Indian government in 1994. Aiming to promote industrial development in the underdeveloped areas, the program designated districts into industrially backward and non-backward districts based on a continuous gradation score and a cutoff value, and provided 5-year tax exemption to the qualified manufacturers in the backward districts.

When these backward districts are subdivided into three groups of equal size, we find that the program only benefited the better-off backward districts, i.e. the group nearest to the cutoff point of the gradation score. Our estimates also suggest that it was the light manufacturing industries that experienced the greatest growth in these districts; heavy manufacturing and other untreated industries benefited to a lesser degree. The evidence suggests that the program worked in a limited way and it would take a lot more than tax exemption to promote industrial development in the more challenged areas and in the capital intensive sectors.

Examining the spatial spillovers of the program shows that the program has generated considerable displacement effects between the better-off backward districts and some of their neighbors. Such adversely affected districts include both backward districts that were weaker in development environment and non-backward districts that were just marginally better than the treated districts. Based on our estimates, the program has resulted in a net decrease in employment nationwide. The displacement rates were about 50% for the light manufacturing industries, 100% for heavy manufacturing and above 200% for the untreated sectors. Hence, in addition to a spatial substitution, the program favoring manufacturing also caused substitution across industries.

It is interesting to note that if the gap between the treated districts and their neighbors were sufficiently large, the negative cross-border spillovers were unlikely to take place. For example, districts from Group 3 did not adversely affected their neighboring districts from Group 5 or Group 6; districts from Group 1 or Group 2 did not generate any displacement in their neighborhood. This may help to explain why Chaurey (2016) does not find any spatial spillovers when he assessed India's New Industrial Policy. Himachal Pradesh, one of the two states covered by the policy, was designated backward state in 1993 while Punjab, its neighboring state used as comparison in the study, was a highly developed state. All the 12 districts of Punjab had very high gradation scores and were categorized into Group 6 in our sample (Table 1) .[15]

Other than policies that offer tax exemptions to promote industrial development in targeted areas, special economic zones or industrial parks have attracted attention of the policy makers in the developing countries. Compared to preferential tax programs, the latter may better address local disadvantages or help reveal the comparative advantages of different areas. It would be interesting for future research to compare these different place-based policies quantitatively. However, spatial spillovers of such policies remain a topic for careful assessment from the perspectives of overall development or welfare consequence.

Finally, India has had a relatively low internal migration rate (Munshi and Rosenzweig 2016). Industrialization may well progress faster in India with policies that facilitate the movement of labor to places where manufacturing has a comparative advantage (such as providing more affordable housing) than policies that try to take manufacturing to the disadvantaged regions. Future research concerning this issue would be a useful complement to the work presented here.

[15] Differences in the two policies may also explain the divergent findings regarding spatial effects across the two studies. The New Industrial Policy provided tax exemption and capital subsidies to both new and existing firms while the backward district policy only provided tax exemption to the new firms. This is a topic to be further investigated.

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Figure 1: Backward and Non-Backward Districts (1991 Census Borders)

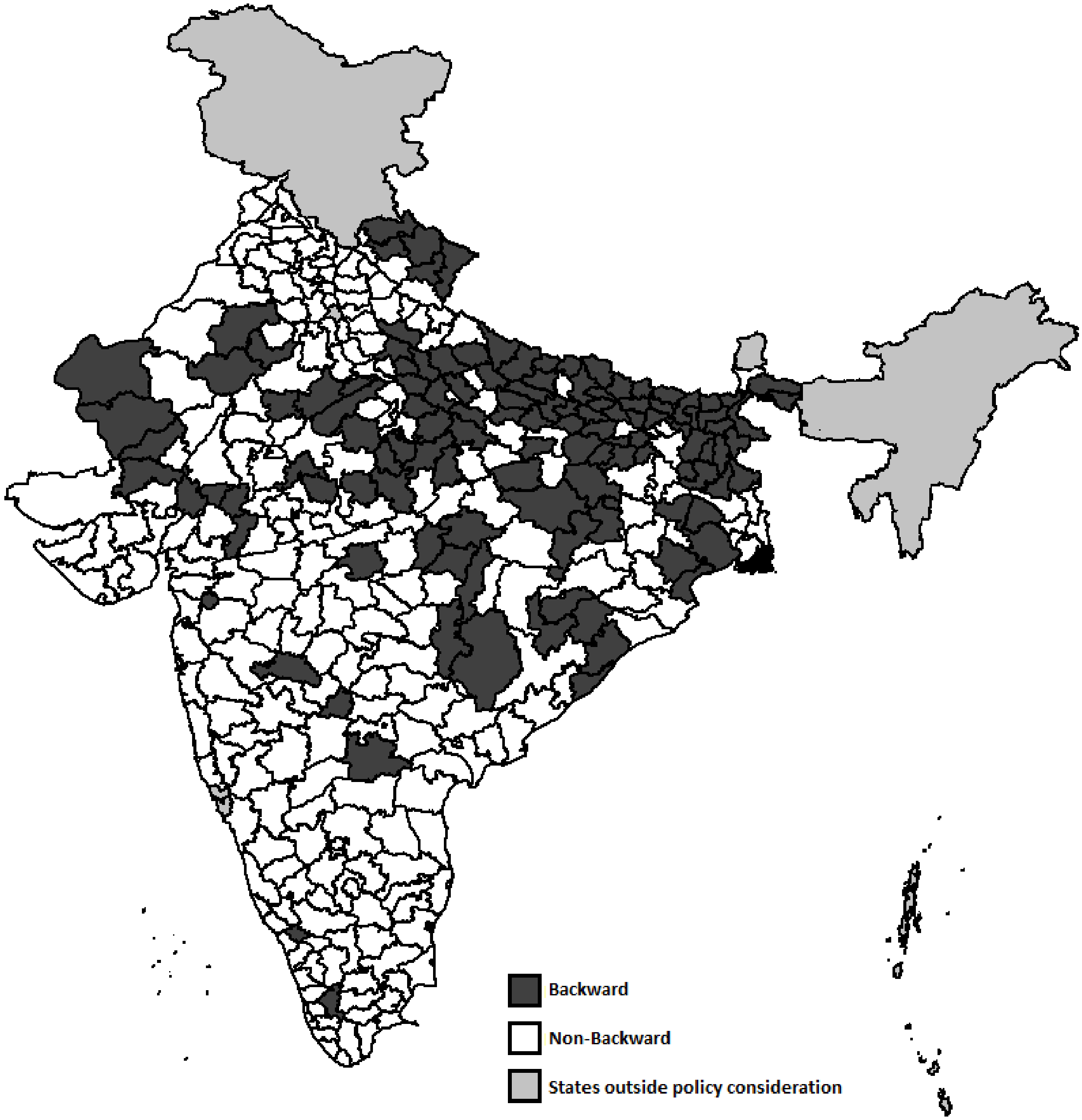
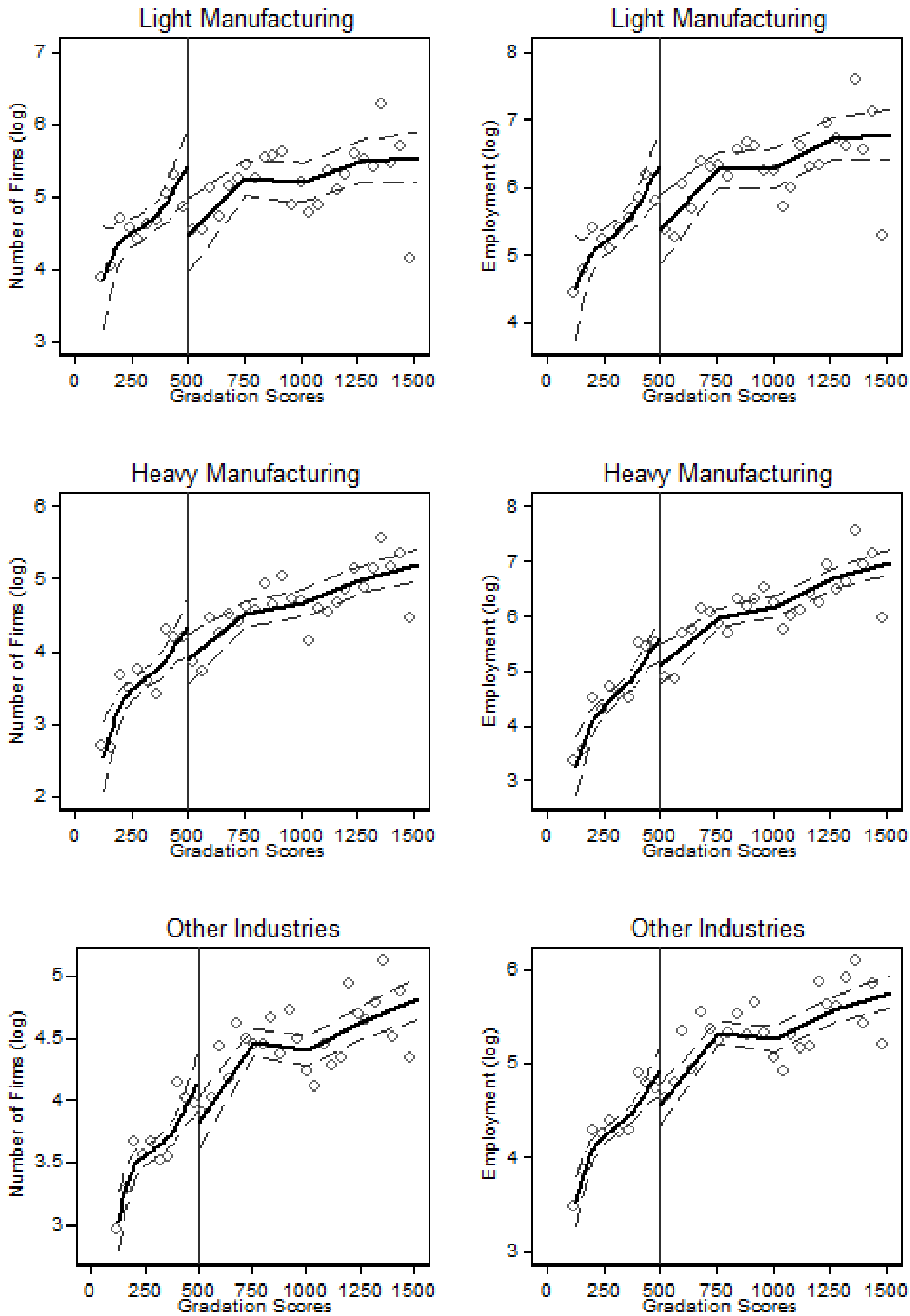


Figure 2. Mean counts of firms and employment of 2-digit industry by district relative to the gradation scores in 1998



Notes: Each dot represents mean counts of firms or employment averaged across 2-digit industries within the category (i.e. light manufacturing, heavy manufacturing and other) and districts falling in a bin of size of 40 gradation scores. The solid line is local polynomial fit with degree 1 and bandwidth equal to 200. The dashed lines are 95% confidence interval of the local polynomial estimation.

Table 1**:** State-wise distribution of backward and non-backward districts

	Backward				Non-backward			
State Name	Group 1	Group 2	Group 3	Total	Group 4	Group 5	Group 6	Total
Andhra Pradesh	-	-	2	2	-	9	12	21
Bihar	19	10	4	33	3	1	5	9
Gujarat	-	2	1	3	-	1	15	16
Haryana	-	-	-	-	-	1	15	16
Karnataka	-	-	1	1	3	4	12	19
Kerala	2	-	-	2	1	2	9	12
Madhya Pradesh	3	7	8	18	10	5	12	27
Maharashtra	1	-	1	2	7	6	15	28
Orissa	2	2	2	6	3	1	3	7
Punjab	-	-	-	-	-	-	12	12
Rajasthan	2	6	4	12	4	3	8	15
Tamil Nadu	-	-	-	-	2	3	16	21
Uttar Pradesh	6	16	13	35	5	5	18	28
West Bengal	6	2	1	9	1	-	5	6
Total	**41**	**44**	**38**	**123**	**39**	**41**	**157**	**237**

Notes: Sample were subdivided into groups per gradation scores. Group 1 contains districts with scores equal or below 250; Group 2 from 251 to 350; Group 3 from 351 to 500; Group 4 from 501 to 650; Group 5 from 651 to 850; Group 6 850 and above. Groups 1-3 were treated and groups 4-6 were untreated. Three districts with gradation scores exceeding 500 while tagged as category A backward districts, i.e. Idukki (618) and Wayanad (583) from Kerala and Jalapaiguri (728) from West Bengal, are included as group 1 since category A is districts scoring 250 or lower.

Table 2: Summary statistics of number of firms and employment by district groups and industrial category in 1998

		Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
VARIABLES		(1)	(2)	(3)	(4)	(5)	(6)
A. Number of Firms							
Light Manufacturing	*Mean*	787.6	900.7	1,179	962.7	1,109	1,295
	Std	1,734	3,878	3,385	2,105	1,766	2,941
	N	266	308	266	273	287	1,099
Heavy Manufacturing	*Mean*	227.6	211.1	306.6	264.5	371.5	556.9
	Std	505.7	519.1	712.1	478.0	641.5	1,142
	N	342	396	342	351	369	1,413
Other Industries	*Mean*	702.9	618.9	979.8	960.2	1,426	1,672
	Std	2,792	1,800	3,732	2,992	3,835	6,099
	N	2,014	2,332	2,014	2,067	2,173	8,321
B. Employment							
Light Manufacturing	*Mean*	1,686	2,088	2,803	2,528	3,092	5,529
	Std	3,778	9,441	8,021	6,815	5,543	14,637
	N	266	308	266	273	287	1,099
Heavy Manufacturing	*Mean*	728.3	777.1	1,072	875.2	1,621	3,936
	Std	1,923	2,101	2,865	1,719	3,118	8,805
	N	342	396	342	351	369	1,413
Other Industries	*Mean*	1,395	1,264	2,053	2,044	3,297	4,340
	Std	6,050	3,154	6,824	5,361	8,852	14,014
	N	2,014	2,332	2,014	2,067	2,173	8,321

Notes: Each block contains mean, standard deviation and number of the 2-digit industry by district observations of each industrial category (i.e. light manufacturing, heavy manufacturing and other) and district group. Group 1 contains districts with scores equal or below 250; Group 2 from 251 to 350; Group 3 from 351 to 500; Group 4 from 501 to 650; Group 5 from 651 to 850; Group 6 850 and above. Groups 1-3 were treated and groups 4-6 were untreated under the Backward Districts Program.

Table 3a: T-tests of pre-treatment district variables in 1991

	Full Sample (Groups 1 to 6)				Groups 2 to 5				Groups 3 and 4			
Variable	T=0 (N=237)	T=1 (N=120)	Diff.	p-value	T=0 (N=80)	T=1 (N=82)	Diff.	p-value	T=0 (N=39)	T=1 (N=38)	Diff	p-value
Population (in 1000)	2,341.7	1,901.3	-440.4	0.005	2,155.4	1,909.5	-245.9	0.14	1,882.3	2,050.5	168.1	0.517
	(101.2)	(94.1)	(156)		(111.6)	(122.1)	(166)		(159.0)	(202.7)	(258.4)	
Main Workers (in 1000)	820.7	610.1	-210.6	<.001	782.5	609.9	-172.6	0.003	641.7	678.8	37.1	0.649
	(34.3)	(28.5)	(51.8)		(43.2)	(37.0)	(56.7)		(49.4)	(64.2)	(81.3)	
Marginal Workers (in 1000)	73.1	73.5	0.4	0.954	82.7	74.9	-7.9	0.402	76.4	78.1	1.7	0.902
	(3.8)	(5.2)	(6.5)		(6.5)	(6.7)	(9.4)		(8.2)	(11.0)	(13.8)	
Number of occupied residential houses (in 1000 units)	423.4	306.8	-116.6	<.001	381.2	304.5	-76.7	0.009	321.1	333.1	12.0	0.783
	(19.6)	(15.4)	(29.4)		(21.2)	(19.7)	(28.9)		(26.5)	(34.2)	(43.4)	
Workers – Agri. Fishing Farming (in 1000)	733.0	662.9	-70.1	0.154	847.0	641.5	-205.5	0.003	680.8	707.8	27.0	0.769
	(31.2)	(31.8)	(49.0)		(53.8)	(41.1)	(67.3)		(59.2)	(69.7)	(91.6)	
Workers - Manufacturing (in 1000)	99.1	30.6	-68.6	<.001	51.4	31.6	-19.9	<.001	37.2	39.7	2.4	0.783
	(8.8)	(3.3)	(12.5)		(4.4)	(3.6)	(5.6)		(5.4)	(6.8)	(8.7)	
Workers - Trade and Commerce (in 1000)	69.4	26.4	-43.0	<.001	43.3	28.0	-15.3	<.001	33.8	32.6	-1.1	0.845
	(5.8)	(1.9)	(8.2)		(3.0)	(2.4)	(3.8)		(4.0)	(4.2)	(5.8)	
Area (square kilometers)	13.6	8.3	-5.2	0.344	13.8	9.9	-3.9	0.619	6.3	5.6	-0.8	0.528
	(3.5)	(3.6)	(5.5)		(6.0)	(5.2)	(7.9)		(0.7)	(0.9)	(1.2)	
Worker participation rate (%)	38.37	37.72	-0.65	0.406	40.39	37.84	-2.54	0.024	39.36	38.12	-1.24	0.431
	(0.45)	(0.67)	(0.78)		(0.76)	(0.82)	(1.12)		(1.09)	(1.11)	(1.56)	
Literacy rate (%)	54.95	39.38	-15.56	<.001	47.81	41.84	-5.96	0.002	47.07	45.23	-1.84	0.534
	(0.94)	(0.98)	(1.48)		(1.42)	(1.22)	(1.87)		(2.12)	(2.05)	(2.94)	
Number of females (per 1000 males)	93.30	92.52	-0.78	0.243	94.43	92.33	-2.09	0.037	93.56	92.69	-0.87	0.534
	(0.40)	(0.51)	(0.66)		(0.73)	(0.68)	(0.99)		(1.05)	(0.91)	(1.39)	

Notes: From left to right, t-tests on the means of the district covariates in 1991 between the treated and untreated districts are presented with increasingly narrower samples around the cutoff point of 500. Group 1 contains districts with scores equal or below 250; Group 2 from 251 to 350; Group 3 from 351 to 500; Group 4 from 501 to 650; Group 5 from 651 to 850; Group 6 850 and above. Groups 1-3 were treated and groups 4-6 were untreated under the Backward Districts Program. Standard errors are in parenthesis.

Table 3b: T-Tests of district pre-treatment variables in 1991 after controlling for 3^{rd} order polynomial of gradation scores

	Full Sample (Groups 1 to 6)				Groups 2 to 5				Groups 3 and 4			
Variable	T=0 (N=237)	T=1 (N=120)	Diff	p-value	T=0 (N=80)	T=1 (N=82)	Diff	p-value	T=0 (N=39)	T=1 (N=38)	Diff	p-value
Population (in 1000)	3.7	-7.1	-10.8	0.942	73.5	-26.9	-100.4	0.544	-156.8	81.1	237.9	0.361
	(95.6)	(93.7)	(149)		(110.3)	(121.8)	(164.9)		(158.8)	(203.3)	(258.8)	
Main Workers (in 1000)	15.5	-29.8	-45.3	0.36	69.6	-41.9	-111.4	0.048	-53.5	13.0	66.5	0.416
	(32.4)	(28.3)	(49.4)		(42.4)	(36.8)	(56.0)		(49.3)	(64.4)	(81.4)	
Marginal Workers (in 1000)	1.7	-3.3	-5.0	0.433	6.1	-1.9	-8.0	0.393	-0.4	1.3	1.7	0.905
	(3.7)	(5.2)	(6.4)		(6.5)	(6.7)	(9.4)		(8.2)	(11.0)	(13.8)	
Number of occupied residential houses (in 1000 units)	3.6	-6.9	-10.5	0.701	24.8	-16.1	-40.9	0.154	-24.7	4.4	29.1	0.505
	(18.1)	(15.4)	(27.4)		(20.9)	(19.6)	(28.6)		(26.4)	(34.3)	(43.4)	
Workers – Agri. Fishing Farming (in 1000)	35.3	-67.9	-103.2	0.033	110.1	-90.9	-201.0	0.003	-55.6	-26.1	29.4	0.749
	(30.5)	(31.8)	(48.2)		(53.8)	(41.1)	(67.2)		(59.2)	(69.7)	(91.6)	
Workers - Manufacturing (in 1000)	-3.0	5.8	8.8	0.36	-2.6	2.1	4.7	0.381	-9.5	4.7	14.2	0.107
	(6.7)	(3.2)	(9.6)		(4.2)	(3.4)	(5.4)		(5.4)	(6.8)	(8.7)	
Workers - Trade and Commerce (in 1000)	-1.0	2.0	3.0	0.644	1.7	0.9	-0.8	0.826	-3.6	2.2	5.8	0.323
	(4.6)	(1.8)	(6.5)		(2.8)	(2.3)	(3.6)		(4.0)	(4.2)	(5.8)	
Area (square kilometers)	1.3	-2.6	-3.9	0.476	1.4	-1.3	-2.7	0.733	-5.8	-5.9	-0.2	0.897
	(3.5)	(3.6)	(5.5)		(6.0)	(5.2)	(7.9)		(0.7)	(0.9)	(1.2)	
Worker participation rate (%)	0.53	-1.02	-1.55	0.045	1.77	-0.88	-2.65	0.019	0.69	-0.59	-1.29	0.413
	(0.43)	(0.67)	(0.77)		(0.76)	(0.82)	(1.12)		(1.10)	(1.12)	(1.56)	
Literacy rate (%)	0.48	-0.93	-1.42	0.284	1.70	0.62	-1.09	0.555	2.43	2.92	0.49	0.866
	(0.83)	(0.90)	(1.32)		(1.44)	(1.16)	(1.84)		(2.13)	(1.96)	(2.89)	
Number of females (per 1000 males)	0.38	-0.74	-1.13	0.088	1.13	-0.94	-2.07	0.038	0.27	-0.59	-0.86	0.54
	(0.39)	(0.51)	(0.66)		(0.73)	(0.68)	(0.99)		(1.05)	(0.91)	(1.39)	

Notes: From left to right, t-tests on the mean residuals of the district covariates in 1991 after controlling for 3^{rd} order polynomial function of the gradation scores between the treated and untreated districts are presented with increasingly narrower samples around the cutoff point of 500. Group 1 contains districts with scores equal or below 250; Group 2 from 251 to 350; Group 3 from 351 to 500; Group 4 from 501 to 650; Group 5 from 651 to 850; Group 6 850 and above. Groups 1-3 were treated and groups 4-6 were untreated under the Backward Districts Program. Standard errors are in parenthesis.

Table 4: Program impacts on number of firms and employment: sample of districts around the cutoff

	Firms			Employment		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Backward district * treated manufacturing industry	0.457***			0.445***		
	(0.124)			(0.101)		
Backward district * treated light manufacturing industry		0.688***	0.618***		0.675***	0.601***
		(0.197)	(0.169)		(0.177)	(0.178)
Backward district * treated heavy manufacturing industry		0.277**	0.207		0.266*	0.192
		(0.117)	(0.123)		(0.138)	(0.153)
Backward districts * untreated industry	0.137*	0.137*		0.149**	0.149**	
	(0.0669)	(0.0669)		(0.0555)	(0.0555)	
Backward district * untreated industries * I-O linkages			0.123			0.142
			(0.0887)			(0.130)
<u>Function of Gradation Score</u>						
Linear score	-10.29	-10.29	-6.954	-9.514	-9.514	-6.025
	(10.50)	(10.50)	(9.521)	(13.13)	(13.13)	(12.29)
Squared score	7.210	7.210	4.921	6.712	6.712	4.316
	(6.892)	(6.892)	(6.210)	(8.538)	(8.538)	(7.960)
Cubic score	-0.480	-0.480	-0.334	-0.422	-0.422	-0.269
	(0.454)	(0.454)	(0.413)	(0.568)	(0.568)	(0.532)
<u>1991 District Characteristics:</u>						
Area (log)	0.120	0.120	0.114	0.166	0.166	0.159
	(0.104)	(0.104)	(0.103)	(0.147)	(0.147)	(0.148)
Population (log)	0.849	0.849	0.846	0.721	0.721	0.717
	(0.497)	(0.497)	(0.503)	(0.557)	(0.557)	(0.567)
Worker participation rate	0.0168	0.0168	0.0172	0.0116	0.0116	0.0119
	(0.0133)	(0.0133)	(0.0134)	(0.0140)	(0.0140)	(0.0141)
Literacy rate	0.0110*	0.0110*	0.0110*	0.0118*	0.0118*	0.0117*
	(0.00591)	(0.00591)	(0.00597)	(0.00599)	(0.00599)	(0.00602)
Primary workers (log)	0.625**	0.625**	0.632**	0.405*	0.405*	0.413*
	(0.222)	(0.222)	(0.229)	(0.225)	(0.225)	(0.225)
Manufacturing workers (log)	0.227***	0.227***	0.237***	0.166***	0.166***	0.177***
	(0.0482)	(0.0482)	(0.0441)	(0.0259)	(0.0259)	(0.0285)
Main workers (log)	-0.947	-0.947	-0.954	-0.438	-0.438	-0.445
	(0.622)	(0.622)	(0.617)	(0.623)	(0.623)	(0.621)
State dummy	Y	Y	Y	Y	Y	Y
2-digit industry dummy	Y	Y	Y	Y	Y	Y
Observations	5,313	5,313	5,313	5,313	5,313	5,313
R-squared	0.827	0.828	0.828	0.781	0.795	0.795

Notes: The sample involves backward districts with gradation scores ranging from 351 to 500 (i.e. Group 3) and non-backward districts with scores from 501 to 650 (Group 4). The dependent variable was transformed as log(Y+1). Gradation score used are the original score divided by 500. Standard errors in parentheses are clustered at state level. *** Significant at 1%, ** Significant at 5%, * Significant at 10%.

Table 5: Program impacts on number of firms and employment: expanded samples of districts

	Firms			Employment		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Groups 2-3 versus Groups 4-5						
Backward district * treated manufacturing industry	0.139 (0.0853)			0.0863 (0.0991)		
Backward district * treated light manufacturing industry		0.303** (0.0995)	0.351*** (0.110)		0.317*** (0.0957)	0.312*** (0.101)
Backward district * treated heavy manufacturing industry		0.0120 (0.150)	0.0596 (0.134)		-0.0932 (0.174)	-0.0982 (0.157)
Backward districts * untreated industry	-0.0214 (0.0601)	-0.0214 (0.0601)		0.0963 (0.0710)	0.0963 (0.0710)	
Backward district * untreated industries * I-O linkages			0.0841 (0.0914)			0.202 (0.125)
Observations	11,178	11,178	11,178	11,178	11,178	11,178
R-squared	0.827	0.827	0.827	0.796	0.796	0.796
All districts (Groups 1-3 versus Groups 4-6)						
Backward district * treated manufacturing industry	-0.121 (0.0747)			-0.434*** (0.100)		
Backward district * treated light manufacturing industry		0.228** (0.0846)	0.263*** (0.0815)		0.0500 (0.0847)	0.0853 (0.0809)
Backward district * treated heavy manufacturing industry		-0.393*** (0.108)	-0.358*** (0.101)		-0.811*** (0.153)	-0.776*** (0.143)
Backward districts * untreated industry	-0.0346 (0.0520)	-0.0346 (0.0520)		0.0229 (0.0538)	0.0229 (0.0538)	
Backward district * untreated industries * I-O linkages			0.0700 (0.104)			0.202 (0.122)
Observations	24,564	24,564	24,564	24,564	24,564	24,564
R-squared	0.828	0.829	0.829	0.796	0.797	0.797
State dummy	Y	Y	Y	Y	Y	Y
2-digit industry dummy	Y	Y	Y	Y	Y	Y
3^{rd} order polynomials of gradation score	Y	Y	Y	Y	Y	Y
1991 district covariates	Y	Y	Y	Y	Y	Y

Notes: The sample in the upper panel involves backward districts with gradation scores ranging from 251 to 500 (i.e. Groups 2 and 3) and non-backward districts with scores from 501 to 850 (Groups 4 and 5). The sample in the lower panel involves all backward districts (i.e. Groups 1-3) and all non-backward districts (Groups 4-6). The dependent variable was transformed as log(Y+1). Gradation score used are the original score divided by 500. Standard errors in parentheses are clustered at state level. *** Significant at 1%, ** Significant at 5%, * Significant at 10%.

Table 6: Number and average gradation scores by neighboring districts

		Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
		(1)	(2)	(3)	(4)	(5)	(6)
Total							
	N	38	44	38	39	41	157
	Ave. Score	204.3	301.9	416.4	562.4	741.4	2,014.8
A. Neighbor(s) from Group 1							
Yes	*N*	30	24	13	10	10	19
	Ave. Score	200.7	298.0	403.5	548.2	735.4	1,676.2
No	*N*	8	20	25	29	31	138
	Ave. Score	217.8	306.5	423.2	567.2	743.4	2,061.4
B. Neighbor(s) from Group 2							
Yes	*N*	28	33	22	18	13	35
	Ave. Score	204.7	303.6	410.8	557.2	719.7	1,749.6
No	*N*	10	11	16	21	28	122
	Ave. Score	203.3	296.6	424.3	566.8	751.5	2,090.9
C. Neighbors from Group 3							
Yes	*N*	18	30	25	24	16	44
	Ave. Score	207.5	302.7	410.3	560.2	758.3	1,726.3
No	*N*	20	14	13	15	25	113
	Ave. Score	201.4	300.1	428.3	565.8	730.7	2,127.1
D. Neighbors from Group 4							
Yes	*N*	14	20	24	26	20	73
	Ave. Score	201.6	300.0	424.5	551.8	746.5	1,686.7
No	*N*	24	24	14	13	21	84
	Ave. Score	205.9	303.5	402.6	583.4	736.7	2,299.9
E. Neighbors from Group 5							
Yes	*N*	12	13	12	20	27	75
	Ave. Score	206.3	304.8	427.8	565.4	744.7	1,725.7
No	*N*	26	31	26	19	14	82
	Ave. Score	203.4	300.6	411.2	559.2	735.2	2,279.2
F. Neighbors from Group 6							
Yes	*N*	19	34	30	36	39	149
	Ave. Score	205.6	303.2	416.5	563.1	744.1	2,025.9
No	*N*	19	10	8	3	2	8
	Ave. Score	203.0	297.4	416.1	553.7	689.0	1,806.9

Notes: The first two rows report number of districts and their average gradation score in each group. Panels A-F show the numbers and averages scores of districts with and without any neighboring districts from groups 1-6, respectively. Group 1 contains districts with gradation scores equal or below 250; Group 2 from 251 to 350; Group 3 from 351 to 500; Group 4 from 501 to 650; Group 5 from 651 to 850; and Group 6 850 and above. Groups 1-3 were treated and groups 4-6 were untreated under the Backward Districts Program.

Table 7: Spatial effects of the program, with neighboring districts from 351-500

	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
A. Number of firms						
With neighbor(s) from Group 3	-0.0809	-0.248*	0.0414	-0.286***	0.0336	-0.0167
	(0.0769)	(0.103)	(0.0312)	(0.0700)	(0.0769)	(0.0521)
R-Squared	0.828	0.826	0.841	0.824	0.850	0.846
B. Employment						
With neighbor(s) from Group 3	-0.0497	-0.237*	-0.00589	-0.289***	0.146	-0.0317
	(0.0729)	(0.110)	(0.0593)	(0.0669)	(0.103)	(0.0520)
R-Squared	0.805	0.790	0.816	0.786	0.822	0.807
State dummy	Y	Y	Y	Y	Y	Y
2-digit industry dummy	Y	Y	Y	Y	Y	Y
3^{rd} order polynomial of gradation scores	Y	Y	Y	Y	Y	Y
1991 district characteristics	Y	Y	Y	Y	Y	Y
Observations	2,622	3,036	2,622	2,691	2,829	10,764

Notes: The dependent variables are number of firms and employment transformed as log(Y+1). Each column represents a regression estimated with districts in the same group. The coefficient in each cell indicates the difference between the districts with and without any neighboring district from each group. Gradation scores used are the original score divided by 500. The 1991 district covariates include log of area, population, and numbers of primary workers, manufacturing workers and main workers, worker participation rate, and literacy rate. Group 1 contains districts with scores from 351 to 500; Groups 1-3 were treated and groups 4-6 were untreated. Standard errors in parentheses are clustered at state level. *** Significant at 1%, ** Significant at 5%, * Significant at 10%.

Table 8: Spatial effects of the program

	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
C. Number of firms						
With neighbor(s) from Group 1	-0.345***	0.132	0.171	-0.0543	-0.0651	-0.196*
	(0.0428)	(0.113)	(0.0967)	(0.126)	(0.0831)	(0.102)
With neighbor(s) from Group 2	-0.103	0.145	0.195***	0.0235	0.111	-0.0425
	(0.127)	(0.118)	(0.0474)	(0.0609)	(0.0869)	(0.0750)
With neighbor(s) from Group 3	-0.157*	-0.300**	0.0764	-0.225***	0.0138	-0.0146
	(0.0712)	(0.0817)	(0.0739)	(0.0675)	(0.0780)	(0.0556)
With neighbor(s) from Group 4	0.105	0.123	0.301***	0.117	-0.0185	-0.00746
	(0.0590)	(0.0711)	(0.0524)	(0.0719)	(0.0270)	(0.0685)
With neighbor(s) from Group 5	0.137	0.0270	0.141	0.0703	-0.0418	-0.0547
	(0.0854)	(0.120)	(0.129)	(0.0512)	(0.0856)	(0.0601)
With neighbor(s) from Group 6	-0.0566	0.128	0.00403	0.175	0.332*	-0.111
	(0.0335)	(0.0996)	(0.0570)	(0.124)	(0.160)	(0.0942)
R-Squared	0.830	0.826	0.842	0.824	0.850	0.846
D. Employment						
With neighbor(s) from Group 1	-0.450***	0.116	-0.0465	-0.0124	-0.0801	-0.240*
	(0.0427)	(0.163)	(0.140)	(0.113)	(0.112)	(0.117)
With neighbor(s) from Group 2	-0.142	0.0840	0.195***	0.0425	0.165	0.0900
	(0.148)	(0.0928)	(0.0459)	(0.0688)	(0.113)	(0.0836)
With neighbor(s) from Group 3	-0.159	-0.279**	0.0508	-0.216***	0.135	-0.0270
	(0.0840)	(0.0929)	(0.0917)	(0.0633)	(0.108)	(0.0585)
With neighbor(s) from Group 4	0.165**	0.0846	0.243***	0.192	0.0900	-0.0557
	(0.0561)	(0.0678)	(0.0611)	(0.129)	(0.0573)	(0.0789)
With neighbor(s) from Group 5	0.254**	0.0199	0.207	0.0952	-0.0100	-0.0249
	(0.0926)	(0.116)	(0.150)	(0.0642)	(0.126)	(0.0631)
With neighbor(s) from Group 6	0.0481	0.142	-0.0863	0.219**	0.369**	-0.114
	(0.0725)	(0.122)	(0.0527)	(0.0867)	(0.160)	(0.119)
R-Squared	0.808	0.791	0.817	0.786	0.822	0.808
State dummy	Y	Y	Y	Y	Y	Y
2-digit industry dummy	Y	Y	Y	Y	Y	Y
3rd order polynomial of gradation scores	Y	Y	Y	Y	Y	Y
1991 district characteristics	Y	Y	Y	Y	Y	Y
Observations	2,622	3,036	2,622	2,691	2,829	10,764

Notes: The dependent variables are number of firms and employment transformed as log(Y+1). Each column represents a regression estimated with districts in the same group. The coefficient in each cell indicates the difference between the districts with and without any neighboring district from each group. Gradation scores used are the original score divided by 500. The 1991 district covariates include log of area, population, and numbers of primary workers, manufacturing workers and main workers, worker participation rate, and literacy rate. Group 1 contains districts with scores equal or below 250; Group 2 from 251 to 350; Group 3 from 351 to 500; Group 4 from 501 to 650; Group 5 from 651 to 850; Group 6 850 and above. Groups 1-3 were treated and groups 4-6 were untreated. Standard errors in parentheses are clustered at state level. *** Significant at 1%, ** Significant at 5%, * Significant at 10%.

Table 9. Displacement effects versus direct impacts of the program

Industry	Increase in Group 3 districts (1)	Loss in Group 2 (2)	Loss in Group 4 (3)	Total loss (4)
A. Firms				
Light manufacturing	118,307	37,616	22,175	59,790
Heavy manufacturing	17,914	11,652	9,276	20,928
Untreated industries	207,752	288,426	227,425	515,852
Total	**343,973**	**337,694**	**258,876**	**596,570**
B. Employment				
Light manufacturing	256,952	68,968	48,388	117,356
Heavy manufacturing	59,544	36,063	29,916	65,978
Untreated industries	495,384	566,310	486,018	1,052,328
Total	**811,880**	**671,341**	**564,322**	**1,235,662**

Note: the estimates are based regression results presented in columns (2) and (5) of Table 4 and columns (2) and (4) of Table 8.

Appendix: Measuring Input-Output Linkages across Industries

Input-Output linkages are calculated using India's Input-Output Transactions Table (IOTT) published by India's Ministry of Statistics for the year 1993-94.[1] The IOTT comes in the form of a 115-by-115 commodity-level matrix covering the whole economy of India. The rows of the matrix indicate how a sector's output (in Rupees) is distributed throughout the economy, while the columns describe the composition of inputs required by a particular sector to produce its output.

To utilize this input-output information, we set up a concordance between the IOTT sectors and the 1987 2-digit national industrial classification (NIC) in the Economic Census 1998 (available upon request). There are considerable differences on how economic activities are aggregated across these two sources. For instance, the IOTT enumerate more than twice as many primary sectors than does the 1987 NIC, while the former only has 13 services sectors as opposed to 33 in the NIC. We combine the IOTT sectors that belong to the same NIC into one industry. There are also cases where two or more NICs are lumped into one industry to correspond to the IOTT sector. This ultimately leaves us with a 44-by-44 industry-level input-output matrix instead of the original 115-by-115 matrix.

Following the literature (e.g. Ellison, Glaeser and Kerr 2010), we develop two indices to measure the share of the inputs of industry i that are purchased from industry j and the share of the outputs produced by industry i that are sold to industry j, respectively:

$$W^{I}_{i\leftarrow j} = \frac{inputs_{i\leftarrow j}}{all\_inputs_i} \text{ and } W^{O}_{i\rightarrow j} = \frac{outputs_{i\rightarrow j}}{all\_outputs_i}$$

where $inputs_{i\leftarrow j}$ denotes inputs of industry i from industry j, $outputs_{i\rightarrow j}$ denotes outputs of industry i sold to industry j, corresponding to the elements of the ith column and jth row and of the ith row and jth column in the above matrix, respectively; all_inputs_i ($all\_outputs_i$) denotes the sum of inputs (outputs) of industry i, corresponding to the ith column (row) of the matrix.

We then aggregate the indices of industry i across all treated industries:

$$W^{I}_{i\leftarrow q} = \sum_{j=1}^{Q} \frac{inputs_{i\leftarrow j}}{all\_inputs_i} \text{ and } W^{O}_{i\rightarrow q} = \sum_{j=1}^{Q} \frac{outputs_{i\rightarrow j}}{all\_outputs_i}$$

[1] Government of India, Ministry of Statistics and Programme Implementation, 1994. "Input Output Transactions Table 1993-94." http://mospi.nic.in/publication/input-output-transactions-table-1993-94

which measure the share of the inputs of industry i that are purchased from the treated industries (denoted to be 1 ~ Q), and the share of the outputs produced by industry i that are sold to the treated industries, respectively.

To construct a single index, we take the greater shares between industry i and the treated industries for input and output, respectively:

$$W_{iq}^{I} = \max\{W_{i\leftarrow q}^{I}, W_{q\leftarrow i}^{I}\} \text{ and } W_{iq}^{O} = \max\{W_{i\leftarrow q}^{O}, W_{q\leftarrow i}^{O}\}$$

and combine them into one index as:

$$W_{iq}^{IO} = \max\{W_{iq}^{I}, W_{iq}^{O}\}$$

Table a lists the calculated industrial linkages between each untreated industry and the treated industries by NIC code with column (1) showing the maximum input linkage, column (2) the maximum output linkage, and column (3) the final measure of input-output linkage. Note that when one industry in the 44-by-44 matrix contains two or more NICs , the same calculated input-output value is assigned to its corresponding 1987 NICs.

Table a. Input-output Linkage Measures for Untreated 1987 NICs

2-digit NIC	Classification at 2-digit level	Max Input Linkage (1)	Max Output Linkage (2)	IO Linkage (3)
2	Plantations and Raising of livestock	0.2102	0.4103	0.4103
3	Agricultural services	0.0453	0.0056	0.0453
4	Hunting, trapping and game propagation	0.0453	0.0056	0.0453
5	Forestry and logging	0.2331	0.6815	0.6815
6	Fishing (including collection of sea products)	0.6073	0.7210	0.7210
10	Mining of coal and lignite; extraction of peat	0.4517	0.4717	0.4717
11	Extraction of crude petroleum; production of natural gas	0.5343	0.8103	0.8103
12	Mining of iron ore	0.5288	0.9422	0.9422
13	Mining of metal ores other than iron ore	0.4081	0.9596	0.9596
14	Mining of uranium and thorium ores	0.3520	0.9643	0.9643
15	Mining of non-metallic minerals n.e.c.	0.4573	0.4273	0.4573
19	Mining services n.e.c.	0.4573	0.4273	0.4573
39	Repair of capital goods	0.4991	0.3248	0.4991
40	Electricity generation, transmission and distribution	0.0858	0.3426	0.3426
41	Gas and steam generation and distribution through pipes	0.0373	0.0409	0.0409
42	Water works and supply	0.0709	0.3203	0.3203
43	Non-conventional energy generation and distribution	0.0858	0.3426	0.3426
50	Construction	0.4917	0.1217	0.4917
51	Activities allied to construction	0.4917	0.1217	0.4917
60	Wholesale trade in agricultural raw materials live animals food, beverages, intoxicants and textiles	0.1935	0.4976	0.4976
61	Wholesale trade in wood, paper, skin, leather and fur, fuel and ores, and metals	0.1935	0.4976	0.4976
62	Wholesale trade in all types of machinery equipment including transport equipment	0.1935	0.4976	0.4976
63	Wholesale trade n.e.c.	0.1935	0.4976	0.4976
64	Commission agents	0.1935	0.4976	0.4976
65	Retail trade in food and food articles, beverages, tobacco and intoxicants	0.1935	0.4976	0.4976
66	Retail trade in textiles	0.1935	0.4976	0.4976
67	Retail trade in fuels and other household utilities and durables	0.1935	0.4976	0.4976
68	Retail trade n.e.c.	0.1935	0.4976	0.4976
69	Restaurants and hotels	0.1906	0.0075	0.1906
70	Land transport	0.4095	0.4586	0.4586
71	Water transport	0.4227	0.4705	0.4705
72	Air transport	0.4227	0.4705	0.4705

73	Services incidental to transport not elsewhere classified	0.4227	0.4705	0.4705
74	Storage and warehousing services	0.1430	0.0002	0.1430
75	Communication services	0.2755	0.3030	0.3030
80	Banking activities including financial services	0.0828	0.2905	0.2905
81	Provident and insurance services	0.1983	0.3773	0.3773
82	Real estate activities	0.4991	0.3248	0.4991
83	Legal services	0.4991	0.3248	0.4991
84	Operation of lotteries	0.4991	0.3248	0.4991
85	Renting and leasing (financial leasing is classified in financial activities) n.e.c.	0.0000	0.0000	0.0000
89	Business services n.e.c.	0.4991	0.3248	0.4991
90	Public administration and defense services	0.0000	0.0000	0.0000
91	Sanitary services	0.4991	0.3248	0.4991
92	Education, scientific and research services	0.1999	0.0025	0.1999
93	Health and medical services	0.5772	0.0205	0.5772
94	Community services	0.4991	0.3248	0.4991
95	Recreational and cultural services	0.4991	0.3248	0.4991
96	Personal services	0.4991	0.3248	0.4991
97	Repair services	0.4991	0.3248	0.4991
98	International and other extra territorial bodies	0.4991	0.3248	0.4991
99	Services n.e.c.	0.4991	0.3248	0.4991
E	Excluded Manufacturing	0.1987	0.0036	0.1987

Source: Author’s estimation using the 1993-1994 Input-Output Transactions Table of India

Table A1: Indicators used to construct gradation scores to identify backward districts

Criteria	Weights	Indicators
Financial	3	Per capital credit given by scheduled commercial banks
	2	Per capital deposit received by scheduled commercial banks
Infrastructural	1	Phones per thousand population
	2	Per capita power consumption
	1	Urbanisation (urban population of a district as a proportion of total population)
	1	Metaled roads per 100 square kilometers
Industrial	3	Workers in registered factories per thousand population (excluding electrical undertakings and bidi & cigar units)
	2	Per capital gross value added from registered manufacturing sector

Source: "All India Gradation List", Appendix III of the Income Tax Act. Notification of Government of India, Ministry of Finance, Department of Revenue (Central Board Direct Taxes) S.O. 635 (E); Accessed at http://ncrpb.nic.in/pdf_files/16_AnnexureVII_part1_cma.pdf.

Table A2: Program impacts on Firms and Employment at 2-digit industry by district level: Dependent Variable: Log(Y)

	Firms			Employment		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Groups 3 to 4						
Backward district * qualified industry	0.409***			0.369***		
	(0.122)			(0.0974)		
Backward district * light manufacturing		0.620***	0.559***		0.567***	0.524**
		(0.191)	(0.159)		(0.170)	(0.177)
Backward district * heavy manufacturing		0.247**	0.186		0.217*	0.174
		(0.106)	(0.116)		(0.113)	(0.151)
Untreated industries in backward districts	0.0953	0.0960		0.0950	0.0957	
	(0.0709)	(0.0713)		(0.0650)	(0.0652)	
Backward district*Untreated industries*Linkages			0.0577			0.107
			(0.183)			(0.262)
Observations	4,619	4,619	4,619	4,619	4,619	4,619
R-squared	0.783	0.783	0.783	0.729	0.729	0.729
Groups 2 to 5						
Backward district * qualified industry	0.149			0.0961		
	(0.0953)			(0.117)		
Backward district * light manufacturing		0.299**	0.309**		0.301**	0.266**
		(0.0993)	(0.102)		(0.104)	(0.0966)
Backward district * heavy manufacturing		0.0322	0.0416		-0.0624	-0.0975
		(0.150)	(0.136)		(0.176)	(0.169)
Untreated industries in backward districts	-0.0694	-0.0692		0.0670	0.0672	
	(0.0818)	(0.0817)		(0.0959)	(0.0959)	
Backward district*Untreated industries*Linkages			-0.137			0.0557
			(0.166)			(0.220)
Observations	9,689	9,689	9,689	9,689	9,689	9,689
R-squared	0.784	0.785	0.785	0.735	0.735	0.735
Full Sample (Groups 1 to 6)						
Backward district * qualified industry	-0.0606			-0.373***		
	(0.0734)			(0.0944)		
Backward district * light manufacturing		0.263**	0.268**		0.0659	0.0711
		(0.0919)	(0.0913)		(0.0923)	(0.0890)
Backward district * heavy manufacturing		-0.314***	-0.310***		-0.717***	-0.712***
		(0.0976)	(0.0970)		(0.134)	(0.131)
Untreated industries in backward districts	-0.0559	-0.0553		0.0178	0.0185	
	(0.0567)	(0.0567)		(0.0539)	(0.0539)	
Backward district*Untreated industries*Linkages			-0.111			0.0685
			(0.130)			(0.143)
Observations	21,581	21,581	21,581	21,581	21,581	21,581
R-squared	0.786	0.786	0.786	0.734	0.736	0.736
State dummy	Y	Y	Y	Y	Y	Y
2-digit industry dummy	Y	Y	Y	Y	Y	Y
1991 district covariates	Y	Y	Y	Y	Y	Y

Notes: The dependent variable was transformed as log(Y). Gradation scores used are the original score divided by 500. Group 1 contains districts with scores equal or below 250; Group 2 from 251 to 350; Group 3 from 351 to 500; Group 4 from 501 to 650; Group 5 from 651 to 850; Group 6 850 and above. Groups 1-3 were treated and groups 4-6 were untreated under the Backward Districts Program. Standard errors in parentheses are clustered at state level. *** Significant at 1%, ** Significant at 5%, * Significant at 10%.

Table A3: Spatial effects of the program, with neighboring districts from 351-500

	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
A. Number of firms						
With neighbor(s) from Group 3	-0.101	-0.226*	0.0431	-0.240***	0.00339	-0.0262
	(0.0845)	(0.101)	(0.0438)	(0.0657)	(0.0768)	(0.0619)
R-Squared	0.797	0.788	0.799	0.780	0.808	0.804
B. Employment						
With neighbor(s) from Group 3	-0.0805	-0.172	-0.00615	-0.195***	0.0923	-0.0508
	(0.0707)	(0.0921)	(0.0573)	(0.0600)	(0.0935)	(0.0638)
R-Squared	0.766	0.738	0.752	0.720	0.765	0.742
State dummy	Y	Y	Y	Y	Y	Y
2-digit industry dummy	Y	Y	Y	Y	Y	Y
3^{rd} order polynomial of gradation scores	Y	Y	Y	Y	Y	Y
1991 district characteristics	Y	Y	Y	Y	Y	Y
Observations	2,165	2,540	2,266	2,353	2,530	9,727

Notes: The dependent variables are number of firms and employment transformed as log(Y). Each column represents a regression estimated with districts in the same group. The coefficient in each cell indicates the difference between the districts with and without any neighboring district from each group. Gradation scores used are the original score divided by 500. The 1991 district covariates include log of area, population, and numbers of primary workers, manufacturing workers and main workers, worker participation rate, and literacy rate. Group 1 contains districts with scores from 351 to 500; Groups 1-3 were treated and groups 4-6 were untreated under the Backward Districts Program. Standard errors in parentheses are clustered at state level. *** Significant at 1%, ** Significant at 5%, * Significant at 10%.

Table A4: Spatial effects of the program (dependent variables log-transformed)

	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
A. Number of firms						
With neighbor(s) from Group 1	-0.282***	0.102	0.222*	-0.0850	-0.0557	-0.221*
	(0.0550)	(0.134)	(0.108)	(0.110)	(0.0735)	(0.106)
With neighbor(s) from Group 2	-0.0950	0.206	0.224***	0.0396	0.125	-0.0722
	(0.123)	(0.119)	(0.0605)	(0.0534)	(0.0921)	(0.0852)
With neighbor(s) from Group 3	-0.166*	-0.288***	0.0550	-0.173**	-0.00628	-0.0278
	(0.0716)	(0.0772)	(0.0814)	(0.0635)	(0.0680)	(0.0611)
With neighbor(s) from Group 4	0.0767	0.140	0.360***	0.0998*	-0.00376	-0.00415
	(0.0436)	(0.0805)	(0.0770)	(0.0498)	(0.0263)	(0.0710)
With neighbor(s) from Group 5	0.0341	0.0784	0.0960	-0.0322	-0.0161	-0.0648
	(0.0828)	(0.117)	(0.129)	(0.0523)	(0.0720)	(0.0590)
With neighbor(s) from Group 6	-0.0855**	0.222**	-0.0106	0.237**	0.334*	-0.148
	(0.0317)	(0.0716)	(0.0578)	(0.0989)	(0.165)	(0.111)
R-Squared	0.799	0.789	0.801	0.780	0.809	0.805
B. Employment						
With neighbor(s) from Group 1	-0.347***	0.0422	-0.00605	-0.0952	-0.0387	-0.273**
	(0.0758)	(0.181)	(0.138)	(0.0869)	(0.0955)	(0.115)
With neighbor(s) from Group 2	-0.120	0.141	0.230***	0.0467	0.179	0.0530
	(0.138)	(0.0975)	(0.0608)	(0.0534)	(0.107)	(0.0915)
With neighbor(s) from Group 3	-0.169*	-0.226**	0.0126	-0.120	0.0943	-0.0485
	(0.0825)	(0.0793)	(0.0939)	(0.0657)	(0.0888)	(0.0662)
With neighbor(s) from Group 4	0.115***	0.109	0.291***	0.138	0.121***	-0.0600
	(0.0255)	(0.0828)	(0.0845)	(0.0950)	(0.0299)	(0.0731)
With neighbor(s) from Group 5	0.113	0.104	0.117	-0.0795	0.0306	-0.0352
	(0.0887)	(0.108)	(0.126)	(0.0731)	(0.109)	(0.0531)
With neighbor(s) from Group 6	0.0430	0.255**	-0.104**	0.260***	0.380**	-0.154
	(0.0862)	(0.0887)	(0.0384)	(0.0542)	(0.156)	(0.135)
R-Squared	0.768	0.739	0.754	0.721	0.767	0.743
State dummy	Y	Y	Y	Y	Y	Y
2-digit industry dummy	Y	Y	Y	Y	Y	Y
3rd Order Polynomial Scores	Y	Y	Y	Y	Y	Y
1991 District Characteristics	Y	Y	Y	Y	Y	Y
Observations	2,165	2,540	2,266	2,353	2,530	9,727

Notes: The dependent variables are number of firms and employment transformed as log(Y). Each column represents a regression estimated with districts in the same group. The coefficient in each cell indicates the difference between the districts with and without any neighboring district from each group. Gradation scores used are the original score divided by 500. The 1991 district covariates include log of area, population, and numbers of primary workers, manufacturing workers and main workers, worker participation rate, and literacy rate. Group 1 contains districts with scores equal or below 250; Group 2 from 251 to 350; Group 3 from 351 to 500; Group 4 from 501 to 650; Group 5 from 651 to 850; Group 6 850 and above. Groups 1-3 were treated and groups 4-6 were untreated under the Backward Districts Program. Standard errors in parentheses are clustered at state level. *** Significant at 1%, ** Significant at 5%, * Significant at 10%.