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The Effect of Place-Based Policies on Migration

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Abstract

Place-based policies have become a common policy tool for reducing geographic economic disparities. These policies may, however, have the unintended consequence of affecting migration, which may in turn, affect the intended effects of the policies. This paper examines the potential for migration to respond to place-based policies. I analyze the effect of a place-based tax policy in India in the 1990s that provided tax exemptions for newly created firms in eligible districts. I hypothesize that this policy could have an unintended effect of increasing migration to districts that were affected by the policy. Using a regression discontinuity design that exploits the policy design, I find that there was indeed an increase in migration both within treated districts and into treated districts. The main reasons for migrating were for a job transfer or to move with the primary earner of the household. I also find that while nominal wages increased in treated districts, and firms there were more likely to hire workers, an individual's probability of being employed did not increase, implying that the policy had a mixed effect on welfare, possibly due to the migration response dampening the effect of the policy. My results, therefore, provide some insight into the unintended consequences of place-based policies and are useful for considering general equilibrium effects when designing place-based policies.

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1. Introduction

This thesis analyzes the effect of place-based policies on migration. Just as there are economic disparities among countries, there are economic disparities among regions within a country. This can have political consequences: for example, voting patterns in the United States presidential and midterm elections show a political divide between the rust belt and the coast, and between rural and urban areas. In both these cases, the more economically disadvantaged rust belt and rural regions tended to vote alike, citing dissatisfaction with the political status quo.

Place-based policies are a way to treat regions with different conditions and needs. (Austin, Glaeser and Summers, 2018). They could aim to either promote growth in underdeveloped regions or further improve already developed ones (Neumark and Simpson, 2015). Examples of place-based policies are special economic zones, and opportunity zones in the 2017 United States tax reform bill. Such policies usually take the form of tax breaks or capital grants to boost investment in areas selected by the government. As such, they differ from "people-based policies" which target a certain demographic regardless of location (Neumark and Simpson, 2015); for example, Medicare targets people above age sixty-five anywhere in the United States.

It is important to consider migration when evaluating place-based policies. A policy of this kind is likely to have migration spillovers that could temper its intended effects. For example, if eliminating taxes in one location creates new businesses, or if people believe it did, others might migrate in. There is, however, little empirical evidence on the effect of place-based policies on migration. The aim of this thesis is to fill this gap in the literature. The results of my analysis will provide some insight into whether place-based policies have spillover effects on migration that could reduce the intended impact of the policy. Thus, the findings of this thesis

could potentially be of use to policymakers who are considering the most effective way to boost growth in underdeveloped regions.

I provide empirical evidence on the migration effect of place-based policies by analyzing how internal migration responded to a place-based tax policy in India. In 1993, the Indian government announced a policy that would provide tax exemptions to districts designated industrially "backward". A district was considered backward if it received a score, based on economic indicators, below a certain threshold. This design of the policy allows me to use a regression discontinuity design to analyze its effects. If the threshold was random and there was no manipulation of the score, then all other factors, other than receipt of the policy, should have changed continuously around the threshold. Therefore, any discontinuous changes in migration and other outcomes can arguably be attributed to the policy.

Using the regression discontinuity approach, I find that treated districts, those who received the policy, experienced more migration into the district. This migration was mostly because of a job transfer (in the case of men) or because the household's primary earner had migrated (in the case of women). I also find that although treated districts appeared to experience an increase in firm and job creation and in nominal wages, the probability of an individual being employed in these districts did not increase relative to untreated districts. This finding suggests that the migration response may have dampened the employment effects of the policy on the treated districts.

The rest of the paper is organized as follows. In Section 2, I provide a brief overview of the existing literature on migration and place-based policies. In Section 3, I describe the place-based tax policy in India that I analyze. In Section 4, I describe the data used in my analysis. I

present the empirical strategy and results in Sections 5 and 6, respectively. I then discuss the results and future research in Sections 7 and 8, and conclude in Section 9.

2. Place-Based Policies and Migration

While there is little empirical work on migration effects of place-based policies, the theoretical literature on place-based policies has several hypotheses on how migration can influence the effects of these policies.

For one, an influx of migrants could increase house prices and rents by creating excess demand for them (Neumark and Simpson, 2015; Austin, Glaeser and Summers, 2018). In this case, landowners accrue the benefits of the placed-based policy while tenants are hurt, which could increase income inequality within the disadvantaged region. Moreover, higher prices, whether in housing or in other consumer goods which would also face increased demand, imply lower real wages. This is not to say that real wages are necessarily lower than what they would be in the absence of the policy, but that, eventually, migration could reduce some of the wage increase that the policy achieved in the short run. It is also possible that migration could decrease nominal wages by driving up labor supply and thus hurt the locals in the place the policy intends to help. However, empirical evidence on the impact of migration on wages is mixed. While some research shows that in-migration decreases wages (Borjas, 2003), others have found it has no impact on wages (Butcher and Card, 1991).

However, not all effects of migration are adverse. Migration could increase productivity if skilled workers cluster in an area (Neumark and Simpson, 2015; Austin, Glaeser and Summers, 2018). Agglomeration economics theorizes that when more skilled workers move to

an urban area, a phenomenon called positive sorting occurs, where firms and workers match according to their skill level. This increases productivity. Human capital spillovers, the spread and exchange of knowledge and skills among migrants and between migrants and non-migrants, are also likely to increase productivity and innovation (Glaeser and Gottlieb, 2007).

Migration spillovers affect not only the places to which people migrate, but also the ones to which they migrate. For example, places people migrate from could be deprived of skilled workers (Duranton and Venables, 2018). On the other hand, one could argue that skilled workers migrate from low income to high income areas regardless, so place-based policies to improve underdeveloped places could mitigate the existing outmigration. It should be noted, however, that migration from low income areas to high income ones has declined in recent years as the cost of living in the latter has reached unaffordable heights (Austin, Glaeser and Summers, 2018).

Overall, while there are many hypotheses, there is little empirical research on the effect of place-based policies on migration (see, for example, a comprehensive literature review on place-based policies by Neumark and Simpson (2015)). This thesis hopes to fill this empirical gap. The only empirical study I am aware of is Betz and Partridge (2012), which looks at a policy that targeted the Appalachian region. It concludes that the policy achieved its objective of boosting growth in the region and was not diminished by migration spillovers, because migration since 2000 had not been very responsive to employment and growth differences between regions.

Moreover, most studies on place-based policies have been on developed countries, but one could argue that these policies have stronger impacts in developing countries, which tend to have greater geographic disparities in income (Hasan et al., 2017). This thesis, by studying the

impact of a place-based policy in India, sheds light on migration responses to place-based policies in a developing country.

3. India's Place-Based Tax Policy

In 1993, the Indian government announced in its Finance Act its intention to develop districts considered industrially "backward". Eight states and six union territories in India had already been considered entirely backward prior to 1993 and were subject to various government undertakings, so they were not affected by this announcement. All districts in the remaining fourteen states in the country[1] were assessed. The government planned to achieve growth by giving a tax exemption to all manufacturing firms (except those in the tobacco and alcohol industries) created in those districts between 1994 and 2000. A firm would be 100 percent exempt from taxes in the first five years after its inception, and after that period, 25 percent exempt until 2005. As an example, if a firm was founded in 1994, it would get a 100 percent exemption until 1999 and 25 percent until 2005, while a firm founded in 2000 would get a 100 percent exemption until 2005 and no exemption after that.

The potential of the tax break to affect firm and job growth and, hence, migration would depend on how sizeable a benefit it afforded to firms. Corporate tax rates in India were high enough that this tax break should have provided substantial benefits to firms. In 1993, the domestic corporate income tax rate in India was 40 percent. It was gradually reduced to 35 percent in 1997and 30 percent in 2005 (Rao and Rao, 2006). However, even if the tax break was substantial, with widespread tax evasion, it may not have changed business conditions in

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[1] These fourteen states were Andhra Pradesh, Bihar, Gujarat, Haryana, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh, and West Bengal.

practice. However, this is unlikely to be the situation here since while tax evasion is common among individuals in India, it is less so among corporations. A UN study ranks India 43rd out of 173 countries in terms of corporate tax revenue lost as a percentage of GDP (Cobham and Jansky, 2017). While this ranking does not indicate a lack of corruption, it is not egregious either; as a comparison, the US ranks 52nd in this list. Therefore, the high tax rates are not only de jure but de facto as well. Moreover, in enterprise surveys carried out by the World Bank in 2014 (Enterprise Surveys, 2014), Indian firms cite tax rates as one of the top three obstacles they face, out of an option of 15 obstacles presented to them.[2] Given these high tax rates and firms' negative perception of them, the tax exemptions in the policy provided substantial benefits to firms.

To administer the tax break, an ad hoc study group in the Finance Ministry set the guidelines for measuring how industrially developed a district was, which resulted in a development score. All districts that scored below 500 were designated backward and received the tax break, while those who scored 500 or greater did not. Figure 1 shows the geographic distribution of districts that did or did not receive the tax break. Figure 2 shows the distribution of treated and untreated districts in the five states for which I will analyze the policy's impact: Bihar, Madhya Pradesh, Orissa, Rajasthan, and Uttar Pradesh.[3]

A district's industrial development was measured by assigning it a score based on how eight industrial, financial and infrastructural indicators within it compared to a weighted national average. Financial indicators assessed people's access to finance, by measuring per capita credit

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[2] The top 10 obstacles are, in order: corruption, lack of electricity, tax rates, the informal sector, limited access to finance, labour regulations, limited access to land, tax administration, political instability, and poorly educated workers.

[3] I will explain why I restrict the analysis to these five states in the empirical strategy section.

and per capita deposit. Infrastructural indicators measured the percentage of urbanization, the number of phone lines per 1000 people, per capita power, and the number of roads per 100 square kilometers. Industrial indicators measured the number of factory workers per 1000 people, and per capita gross value added from manufacturing. Figure 3 shows an example of the calculation of the development score. These indicators had already been measured in the publicly available 1991 Census, two years before the government decided to enact this policy.

To my knowledge, the only other study of this policy is Hasan et al. (2017), which examines its effect on the creation of firms and finds an increase in firm entry and employment.

4. Data

This thesis uses household and individual-level data from national employment and migration surveys conducted by the Indian Ministry of Statistics and Program Implementation (MOSPI). Each dataset has a sample of over 100,000 households. I use data on employment and migration collected in 1987-1888 (NSSO 43rd round) and 1999-2000 (NSSO 55th round). The 1999-2000 survey is the earliest survey with migration outcomes after the tax policy came into effect. I also use data on businesses from the 1998 economic census of India, which is the earliest economic census after the policy came into effect. This dataset includes all businesses, both informal (unregistered) and formal (registered). MOSPI conducts thorough surveys to identify any production activity and stresses that the surveys are strictly confidential and are to be used solely for statistical purposes, rather than, for instance, for tax purposes. This reduces the likelihood that unregistered businesses are not captured in the surveys. I also use data on house rents available from national surveys on housing prices and conditions (also conducted by

MOSPI) for the year 2002 (NSSO 58th round), again the earliest of its kind after the policy came into effect.

The specific migration outcomes of interest to this thesis are an individual's migration status and reason for migrating. The first is a simple binary variable that indicates if a person is a migrant or not. "Migrant" is defined in this paper as it is in the survey: a person whose current usual place of residence is different from their last usual place of residence. The data has follow-up responses on how many years have passed since the person migrated at the time of the survey, so I can infer if they migrated before or after the policy was implemented. The second set of outcomes is several dummy variables, each describing a different possible reason for migrating: to find employment, because of a job transfer, for housing-related issues- either to buy or acquire a new house or because of high rents or scarcity of housing in their previous place of residence, for marriage, for education, for political security[4], for health, or for the sake of moving with the primary earner in the household.

I will look at five levels of migration: i) overall migration, ii) all migration within a district (intra-district), iii) migration within a district that is between geographical areas- either urban to rural, or, as is more common, rural to urban, (rural-urban intra-district), iv) migration between districts within the same state (inter-district), and v) migration from another state (inter-state).

Table 1 shows summary statistics on overall migration levels in the states of Bihar, Madhya Pradesh, Rajasthan, Uttar Pradesh, and Orissa (BIMARU states) - for everyone, and for women and men of working age separately.[5] Women are more likely than men to migrate. This

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[4] For example, to escape from communal riots.
[5] Working age is defined as ages 15 to 64.

is probably because while overall migration levels are low, sociocultural expectations in most Indian regions, including these five states, compel women to move to their husbands' residences after marriage, and not vice versa. The statistics for the reasons for migrating support this explanation, as they show that women are indeed vastly more likely than men to migrate for marriage. They are also more likely than men to cite the migration of the primary earner in their household as a reason for their own migration, which is also unsurprising as they are less likely to themselves be primary earners, due to female paid labor force participation being lower than male labor force participation in India (Verick, 2017). Men are more likely to migrate for reasons of employment- whether to find a job, or because they have been transferred for one, and for education, socio-political and housing-related issues. Their numbers are, however, not as high as those of women who move for marriage, and they are hence a minority among migrants.

I will also analyze changes in the number of businesses created and total number of workers hired to see if the expectations of people - largely men- who move to search for employment are realistic. I will also examine whether there are differences in wages and in an individual's probability of being hired, which would provide some insight into the welfare implications of the policy. Additionally, I will look at changes in house rents to see if, as some of the literature predicts, they increase as migration does and, hence, increase the cost of living and depress real wages. However, even if this association is true, the causal link is unclear; intuitively one would expect migration to increase prices by raising the demand for housing, but it is possible that people migrate to places with high house prices intending to invest in them if they expect prices to continue rising. Thus, while I can establish a relationship between the policy and migration, and between the policy and rents, any relationship between rents and migration is suggestive at best.

5. Empirical Strategy

5.1. Establishing causal impact

One has to be careful when evaluating the effects of this policy as districts selected for the tax break were those that scored below a backwardness threshold - that is, less than 500 points, and therefore have different characteristics from districts that were not selected. This means that any difference in outcomes could be attributed to the differences in characteristics rather than to the policy itself. The key to resolving this issue is to notice that while the selection might not be random, there is no evidence that the cut-off score of 500 is anything but random. Hence, we should expect characteristics to change continuously as we move across the score threshold. The only discontinuous change is the treatment, as districts below get the policy and districts above do not. This implies that any discontinuous change in migration outcomes is likely a result of the policy and not of any other factors.

The empirical strategy, described earlier, is formally a sharp regression discontinuity design. It is sharp as the treatment is deterministic: scoring below 500 implies a district received the tax break, while scoring above it implies it did not. The equation used to estimate the effect of the tax break is

$$Y_{ids} = \beta_0 + \beta_1 f(X_{ds}) + \rho D_{ds} + \alpha_s + \eta_{ids}$$

i denotes an individual, d denotes a district, and s denotes a state. Y is a migration outcome, $f(X_{ds})$ is a second-degree polynomial function of the development score used to control for any existing relationship between a district's backwardness and migration outcomes, D is 1 if the district received the tax break and 0 if not, α are state fixed effects, and η is the error term. I

cluster standard errors by score to allow for correlations across individuals in districts with the same score. To ensure that my estimates are representative of the population, in all regressions using sample survey data, I weight observations with the sampling multipliers provided with the dataset.

β_1 measures the effect of backwardness on the migration outcome. The coefficient of interest is ρ which measures the discontinuous change in the migration outcome for districts that received the tax break compared to districts that did not.

I will estimate the migration outcome for two time periods. First, for the years before the policy, using data collected in 1987-1988. This can be considered a placebo regression as there should be no difference in outcomes between districts that later got the tax break and those that did not. If a significant difference is observed, that would imply that the districts around the cut-off of 500 differ in factors other than whether they got the place-based policy or not, which would make my empirical strategy flawed and unable to establish a causal link. If there is no significant difference in this period, it strengthens the assumption that there are no discontinuous changes besides the policy around the cut-off in the 1990s.

I will examine the policy's effect using data collected in 1999-2000 (on migration), 2002 (on house prices and rents), and 1998 (on firms and workers) when it was still in place. For migration and worker outcomes, I will look at its overall effect and its separate effects on women and men of working age.

Before analyzing the effect on migration, it is crucial to test the validity of the empirical strategy. The main identification assumption is that there is no discontinuity in any predetermined characteristics of districts around the cut-off score of 500 that could affect the

migration outcomes. This assumption would be threatened if the score could have been manipulated. For example, some districts might have had the political clout to lobby to receive a score below 500 so that they would receive the policy.

This is, however, unlikely. The strongest evidence against there being any manipulation of scores is that, as detailed earlier, the data on district characteristics to create the development score in 1993 was taken from the publicly available 1991 population census. There is also no evidence of political manipulation. Election archives for districts in Rajasthan in Table 2, for example, reveal that these districts were not more or less likely to be represented by a member of parliament from either the ruling party or the opposition party at the time the scores were assigned.[6] Nevertheless, one could still argue that the decision on what characteristics and weights to use for the score was subjective.

To test this, I note that if the strategy is valid, any potential factors other than the tax break should be smooth across districts around the cut-off of 500. In other words, there should be no significant discontinuity in these factors among districts that received the policy and those that did not. I test this assumption for four available district characteristics: the district's population, its population growth rate, population density (per square kilometer) and labor force participation rate. Data on these characteristics are available from the 1991 population census.

Additionally, a density histogram for the scores of the districts should appear fairly smooth- there should be no sharp change in the number of districts that score just above and below 500 if the identifying assumption holds. Figure 4 shows this is the case, and thus supports the veracity of the identifying assumption. Finally, estimating the outcome for the placebo years

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[6] To be precise, the scores are for districts while the members of parliament represent constituencies. While these are not identical groups, they overlap enough for me to infer the member of parliament for the districts.

before the policy, 1987-1988, will also help verify the assumption. Since the policy was not yet in place during this period, these should be no discontinuous changes in migration between the treated and untreated districts.

5.2. Sample of states

Of the fourteen states affected by the policy, all other than the five states of Bihar, Madhya Pradesh, Rajasthan, Uttar Pradesh, and Orissa had almost all of their districts untreated, and are hence not included in my analysis. These five states are socio-economically similar and are all located in the north-central region of India. They have lower economic growth rates, higher fertility rates, higher population growth rates, lower literacy rates, and lower life expectancy than the national average (Kawadia and Philips, 2014). The predominant languages spoken are Hindi or one of its dialects (except in Orissa, where Odia is more common), and these states also fall into the region known as the Hindi Belt. A common acronym for these states, BIMARU, bears an unfortunate resemblance to *bimar*, the Hindi word for 'ill'. Given the similarity of these states, there should be relatively few socioeconomic disparities among the districts in these states such that a policy that offers substantial benefits to only some districts will have the potential to generate strong migration responses.

Ever changing district borders in India pose a problem. Between 1993, when the policy was implemented, and 1999-2000, when the data were collected, several districts were split or redrawn. I have been unable to obtain information on how the implementation of the tax break changed with borders - for example, it is unclear whether sawing an originally treated district into two implied that both the new districts continued to receive the tax break or if only the less industrialized one of them did- so it is hard to infer causality between the policy and outcomes in those districts. Also, in such a case, if a respondent in the surveys used for the analysis indicated

that their current usual place of residence was different from their last usual place of residence, it is not clear if they indeed migrated or if their district was simply redrawn and renamed.

While the creation of new districts or changes in district boundaries have been common occurrences in every decade, the heightened frequency of this phenomenon in the late 1990s may have been related to the impending formation in late 2000 of three new states carved out of old ones: Chhattisgarh from Madhya Pradesh, Jharkhand from Bihar, and Uttarakhand from Uttar Pradesh. Orissa was not carved into a new state, but still underwent several district changes. All but three districts in Rajasthan remained intact.

Using data on the date of district boundary changes, I therefore select a sample of only districts whose boundaries did not change between 1987 and 1999 since I am estimating migration outcomes using data from surveys that were conducted over this period. Out of the 339 districts in the five states, 198 did not change their boundaries during the time period in which I estimate migration outcomes, so I limit my analysis to this sample. As Figure 5 shows, the density histogram for the scores of these districts (limited to those less than 1200 to avoid outliers) is, like the equivalent histogram for all districts in Figure 4, evenly distributed, which supports my identifying assumption.

6. Results

6.1. Verifying the identification assumption

I first analyze whether treated districts differed from untreated districts in 1991 with respect to district characteristics including population, population growth, density, and labor force participation, using data from that year's census. This is a robustness check to see if the

two groups of districts differed before the policy was implemented, and I estimate this with two bandwidths: the first time using only districts that have a score less than 1200, which excludes any outlier districts that could distort results, and the second time using all districts.

Table 3 reports these results. None of these differences are statistically significant, which supports my identifying assumption that there is no discontinuity between treated and untreated districts other than the treatment itself.

6.2. Results on business and job creation

In this section, I analyze whether the intended firm and job creation effects of the tax policy, which might have motivated migration, did indeed occur. It is important to note though that, since the tax break was expected to generate an increase in firms and employment, there could be a migration response even if there was no actual job creation as long as people expected an increase in jobs. I use data from the economic census which surveys all firms in the country to analyze business and job creation. I use the 1998 census which is the first one available after the policy came into effect. I analyze the effects of the policy on all firms in the manufacturing sector, excluding the tobacco and alcohol industries since these were ineligible for the policy. As a placebo test, I also analyze the effects of the policy on firms in the service sector, which did not receive the tax break.

In Panel A of Table 4, I find some evidence that treated districts had more firms in the manufacturing sector. In this sector, treated districts also had more workers: overall, male adult, and female adult. This group, which I refer to as "total workers" includes both paid and unpaid workers.[7] When I conduct a separate analysis for paid workers, I find that treated districts saw an

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[7] Unpaid workers are usually family members who help out by doing odd jobs or overseeing the reception desk, etc.

increase in only paid male workers. Hence, the policy increased paid opportunities for male workers, but not for female workers, whose opportunities were limited to unpaid ones. The estimates for the coefficient on the treated dummy for total workers in the manufacturing sector range from 0.56 to 0.70 when I use the bandwidth of scores less than 1200, and from 0.36 to 0.46 percent when I use the bandwidth of all scores.[8] Hasan et al. (2017), who look at the effect of this policy on employment in manufacturing industries, find a difference of around 53 percent, in line with the magnitudes I find.

In Panel B of Table 4, I find that treated districts also had more firms in the service sector, but the magnitude of this estimate is lower than it is for manufacturing firms. In the service sector, they also had more total workers- overall and male, but again, magnitudes are not as large as in the manufacturing sector. In theory, although the policy does not directly affect the service sector, since the service sector tends to complement the manufacturing sector, any expansion in the latter could also generate an expansion in the former. It is also possible that if growth in the manufacturing sector attracted migrants and not all of them could find a job, the available pool of labor would increase, which may have incentivized the creation of new firms and more hires in the service sector too. Overall, there is some evidence that the policy may have achieved its intended effects of generating an increase in businesses and jobs.

6.3. Results on migration

As a placebo test, I first conduct my analysis of migration using data collected in 1987-1988, over five years before the policy, with the same districts, same two bandwidths and same

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[8] That my bandwidth choice results in such a difference in magnitude suggests that the most developed districts-ones that scored above 1200- continued to have business creation and employment opportunities despite being ineligible for the tax break. Thus, including them in the estimate narrows the difference in outcomes between treated and untreated districts.

definition of migration that I use in my main analysis of outcomes after the policy. If the empirical strategy is valid, then there should be no difference in migration outcomes between treated and untreated districts in these 'placebo' years. Table 5 shows that migration levels were not significantly different between treated and untreated districts in these years, which is consistent with my hypothesis. There are also no significant differences in migration levels for particular reasons.

I estimate the effect of the policy on migration using data from a national survey conducted in 1999-2000, first for districts with a score less than 1200, and then for all districts. To isolate migrants who could plausibly have been motivated by the effects of the tax break on the district from anyone who may have migrated at any point in time, I limit the definition of migration to 'having lived in a different "usual place of residence" any time in the past six years.[9]

Before presenting the regression estimates, I provide a visual representation of my main results. Figure 6 plots the averages by score of inter-district migration for both the placebo period and the policy period. The figure also shows a quadratic of best fit with 95 percent confidence intervals. While the figure in the left panel from the placebo shows no jump in inter-district migration for treated districts, the figure in the right panel shows this is the case during the policy period, suggesting a migration response to the policy. Figures 7 and 8 suggest that this inter-district migration is driven by migration for job transfers and because of migration of the household's primary earner.

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[9] I use six years because the policy was announced in late 1993, while the survey was conducted in mid-1999 to mid-2000.

Table 6 presents the regression estimates. People in treated districts are more likely than ones in untreated districts to have migrated overall, migrated from within the same district, migrated from a different geographical area within the same district (e.g. from a rural to an urban area or vice versa, although there is no information in the survey that allows me to distinguish between these two), from a different district within the same state, and from another state.

All of these results are statistically significant, except for inter-district and inter-state migration levels when I restrict my analysis to districts with scores under 1200 bandwidth. When I include the full set of scores, inter-district migration is statistically significant too. These results on increased migration within and into treated districts is consistent with the hypothesis that the tax break motivated migration.

I find more insightful results when I analyze the specific reasons for migrating. Table 6 shows that treated districts are overall more likely to have people who migrated into the district to find employment or because of a job transfer. It is also not surprising that the policy appears to increase more intra-district than inter-district migration for people who are looking for a job, while migration for job transfers occurs at all levels, as the costs of migration- transportation, resettling, etc.- do not encourage migration over long distances unless one is sure that a job awaits at the other end.

These results on migration for employment and job transfers are consistent with an intuitive explanation that a district-based tax break stimulates new businesses, which lure people to work there if they secure a job and encourages them to search for one if they are unemployed. While the tax break is district-based, it probably affects urban areas more than rural ones in terms of firm and job creation, so one would also expect to see an increase in migration within a district between geographical areas, which is what I find. Unfortunately, I do not have

information on whether intra-district migration between geographical areas is urban to rural or rural to urban for a given individual, but it is more likely that economic growth in a district would motivate the latter.

In sum, as shown in the inter-district migration results in Panel C of Table 6, it appears that while the tax policy was put into place to help specific districts, people migrated into these districts from other districts to potentially take advantage of any benefits of the policy.

This inter-district migration was specifically occurring for employment reasons. None of the estimates for the other inter-district migration reasons, not directly related to employment, are significantly higher in treated districts, as shown in Panel D of Table 6. There is no significant difference for marriage despite summary statistics showing a high incidence of women migrating for this reason. There are also no significant differences in migrating for studies, health or housing reasons. The absence of effects on these migration reasons, coupled with the presence of effects for migration reasons seen in Panel C of Table 6, supports the idea that the migration response observed is due to the place-based tax policy which was expected to drive firm and job creation.

A major reason observed for migration into the treated districts is a job transfer. The survey, unfortunately, does not provide detailed information on the nature of these job transfers. It is plausible that businesses tried to take advantage of the tax break by registering themselves in districts that received the break or by opening up new branches in these districts. Even businesses already in treated districts might have moved their operations to places in the district that had a high number of newly created firms in the same industry, to take advantage of external economies of scale. Job transfers may have, therefore, occurred as businesses moved their existing workers to these new locations. One might hypothesize that firms are more likely to

transfer their skilled workers relative to low-skilled workers because it may be more difficult to find and train skilled workers in a new location relative to low-skilled workers. To determine whether this scenario may have happened in this setting, I conduct the migration analysis separately for more educated and less educated people, using education as a proxy for skill. I define 'less educated' as having less than a primary school education and 'more educated' as having at least completed primary school. The results from this analysis are reported in Table 7. I find that that more educated people were more likely to be transferred for a job. These results support the hypothesis that firms were transferring their skilled workers; there being less likely a need to transfer low-skilled workers since such workers may be relatively easy to hire and train.

In Panel B of Table 7, I also find differences between men and women. Men are more likely than women to migrate to find employment or for a job transfer, while women are more likely than men to migrate because the primary earner migrated. These estimates are consistent with the low levels of female labor force participation rates in India (Fletcher et al., 2015).[10]

To summarize, the place-based tax policy generated an increase in migration into the treated districts. Men were more likely to migrate for employment-related reasons including finding employment and because of a job transfer. Women were more likely to migrate because the primary earner in the household had migrated. The people migrating because of job transfers were also more likely to be more educated.

6.4. Results on employment and wages

Although I show above in Section 6.2 that there was an increase in firms and hiring in treated districts, given the increase in migration into these districts, it is not necessarily the case

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[10] I also run these regressions for children and find inter-district migration for children because of migration of the primary household earner.

that an individual jobseeker would have had better prospects. Using the 1999-2000 employment and migration survey, I find in Table 8 that an individual was not more likely to be employed in treated districts. Men are even less likely to be employed in treated districts. These results imply that while the policy may have increased labor demand, the migration it caused increased labor supply by even more, especially for men. Nominal wages, however, were higher in treated districts, which means that while people with jobs may have benefited from the policy (conditional on the cost of living not rising by a greater amount and, hence, depressing real wages), the chances of finding a job did not increase.

6.5. Results on house rents

In relation to the policy potentially driving up the cost of living, I also check if the policy had any effect on house rents, since one would expect more migration to increase demand for housing. The results of this analysis are shown in Table 9. The first row provides estimates for rent, whether rent reported by a tenant or imputed rent reported by a homeowner, while the next two rows look at these types of rents separately. While the estimates in Table 9 are large and positive, particularly for tenant rent, none of them are statistically significant. It would, however, still be premature to conclude that the policy had no effect on rent, because these data were collected in 2002, two years after the end of the 100 percent tax break. Hence, while my results suggest that the policy did not have a medium-term impact on house prices and rents, they do not rule out a short-term effect.

7. Discussion

While my analysis shows that a place-based tax policy had effects on migration - specifically by increasing male migration for employment reasons and female migration because of migration of the primary earner – it is important to consider the external validity of these results. Can these results be generalized outside the Indian context? Migration in India is very low compared to other developing countries, especially for employment reasons (Kone et al., 2017).[11] A study of 80 countries finds that India has the lowest level of internal migration (Bell et al., 2015). This is because in the absence of a reliable government-run social safety net, people depend on their caste network for social security, and are reluctant to migrate as they do not want to lose access to that network (Munshi and Rosenweig, 2016). Given that I find a significant migration response to the policy in spite of this, migration effects of place-based policies may be even stronger in countries with higher internal labor mobility than India.

While I am able to determine the effect of the policy on migration, I am not able to definitively establish the welfare impact of this migration response. I find some evidence that it may have negatively affected welfare by preventing an increase in or even reducing employment rates in the treated districts. On the other hand, I do find an increase in nominal wages for people who are employed, although I am unable to draw conclusions on the impacts on real wages given the limited data on cost of living.

Finally, the survey responses are terse, especially the options given for the reasons for migration, and therefore do not provide much qualitative insight into the effects of the policy. For example, while the policy appears to increase people migrating because of job transfers, the survey does not describe the process of a job transfer or what instigated it. Therefore, while I

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[11] The exception to this is women who move for marriage.

hypothesize that business owners sought to take advantage of the tax break by moving their business to or creating new branches in a treated district, I do not have direct supporting evidence of this.

8. Further Research

There is scope for more nuanced analysis after these initial estimations. In 1997, when the policy was in still in effect, the government extended the 100 percent exemption for districts that scored below 250 by two more years, after the original five. After the two extra years, they got a 25 percent tax exemption until 2005. Districts scoring between 250 and 500 had a 100 percent exemption only from the first 5 years after which it was 25 percent. Future research could use a regression discontinuity estimate around the 250 cut-off to estimate if the length of the policy affected outcomes.

Finally, the existing research (including this one) on the effects of place-based policies has not examined whether they are "self-sustaining"- that is, if the effects last even when the policy is no longer in effect (Neumark and Simpson, 2015). Hence, it would be useful to estimate migration and other economic outcomes a few years after the policy ended.

9. Conclusion

This paper examines the effect of a place-based tax policy in India in the 1990s, where firms created during the five years after the policy in districts considered backward were eligible for a 100% tax exemption, while firms created during that period in developed districts were not. I

hypothesize that this policy could have had an unintended effect of increasing migration to districts that were affected by the policy. I find that there was indeed an increase in migration into treated districts, with the main reasons for migrating being for a job transfer or to move with the primary earner of the household. I also find that while nominal wages increased in treated districts, and firms there were more likely to hire workers, an individual's probability of being employed did not increase (and even decreased for men), implying that the policy had a mixed effect on welfare. My results, hence, provide some insight into the unintended consequences of place-based policies and are, therefore, useful for policymakers as they design place-based policies to promote development and reduce inequality.

10. Figures and Tables

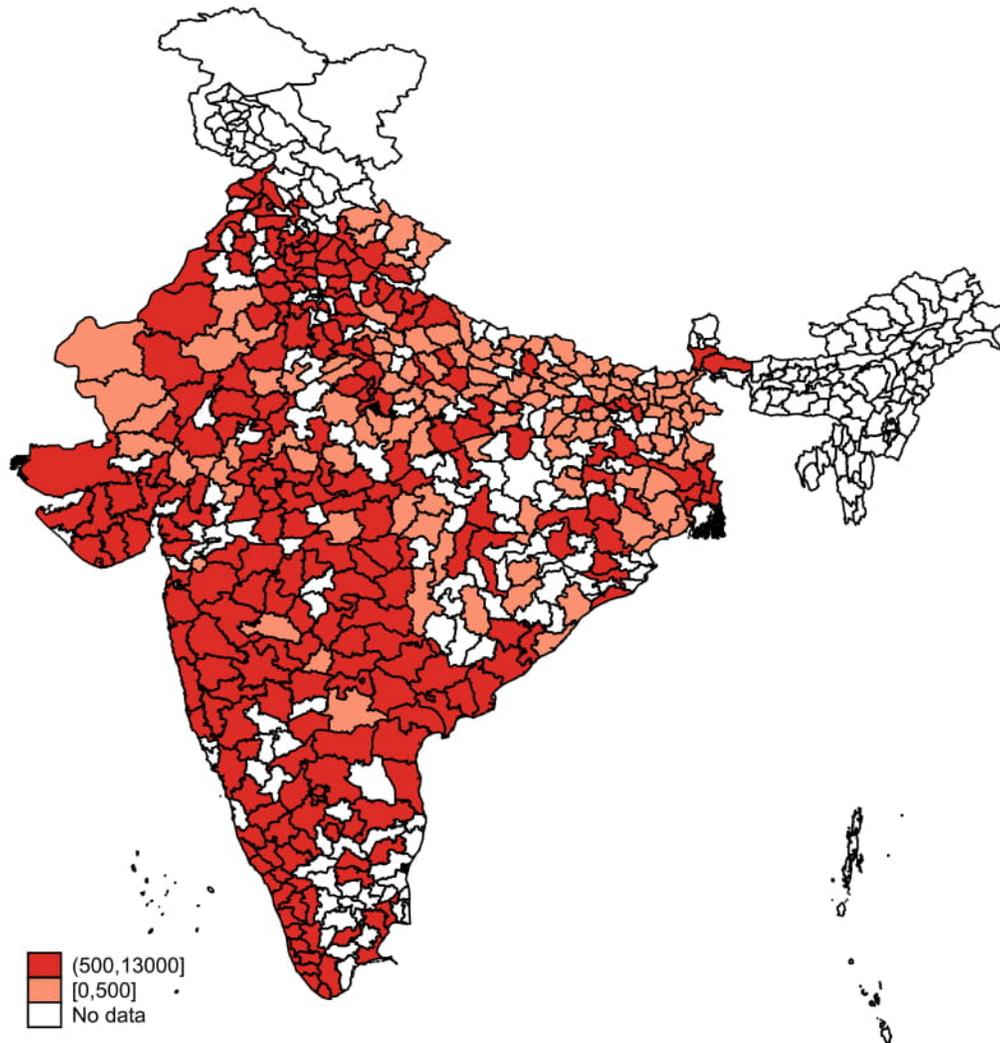Figure 1: Geographic distribution of all treated (orange) and untreated (red) districts.

Figure 2: Geographic distribution of treated (orange) and untreated (red) districts in the following states: Bihar, Madhya Pradesh, Orissa, Rajasthan, Uttar Pradesh.
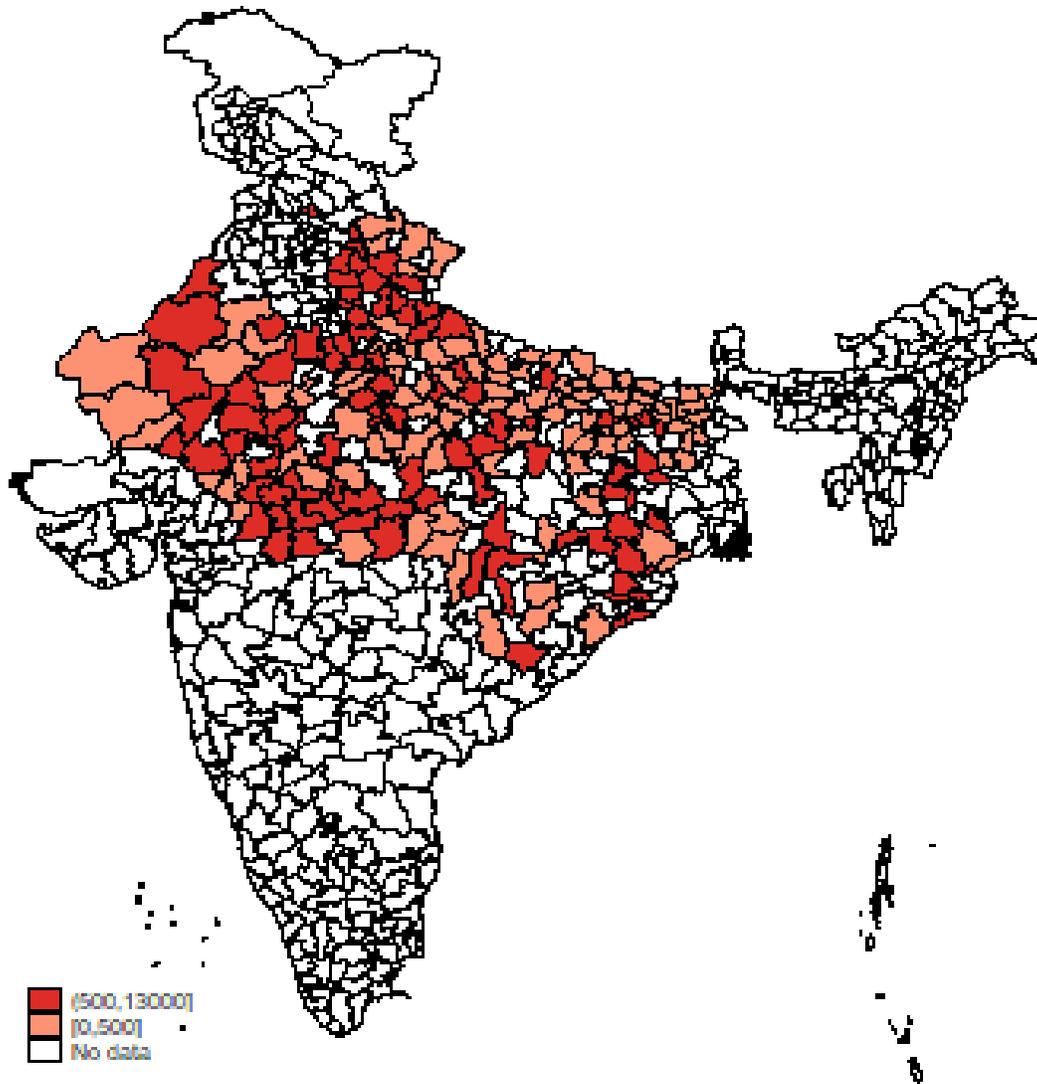
Figure 3: An extract from the list of districts, their states, their development scores for each indicator and their total score. Each indicator has been set a national average of 100 with different weights. For example, in the first district Godda, the per capita credit is 45 and the weight for that indicator is 3 which means that the per capita credit in that district is 15% of the national average.

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APPENDIX - III

ALL INDIA GRADATION LIST

NAME OF DISTRICTS		FINANCIAL		INFRASTRUCTURAL				INDUSTRIAL		TOTAL WEIGHTED INDEX COUNT
		Per capita credit	Per capita deposit	urbani sation %	Phone per 1000	Per capita POWER	Road per 100 Sq. kms	Factory workers per 1000	Per capita GVA from manuf.	
		(AS A PERCENTAGE OF ALL INDIA AVERAGE FOR EACH INDICATOR)								
ALL INDIA AVERAGE INDEX		100	100	100	100	100	100	100	100	
WEIGHTS		3	2	1	1	2	1	3	2	
ALL INDIA WEIGHTED INDEX		300	200	100	100	200	100	300	200	1500
Godda	<< BH	45	38	11	4	22	9	0	0	129
Gumla	<< BH	30	48	17	16	8	14	0	0	142
Araria	<< BH	42	24	25	12	4	16	21	4	148
Gadchiroli	MH	30	32	34	16	8	35	3	2	160
Madhepura	<< BH	45	26	25	13	6	46	0	0	161
Sidharthanagar	<< UP	30	40	14	3	38	40	0	0	165
Dumka	BH	36	46	24	9	12	36	3	4	170
Mandla	MP	33	24	30	19	26	29	6	4	171
Khagaria	<< BH	42	32	23	9	14	42	18	0	180
Kishanganj	<< BH	39	20	39	20	8	20	33	6	185
Malda	<< WB	48	42	28	13	16	41	0	0	188
Palamau	BH	42	46	21	19	22	16	15	18	199
Phulbani	<< OR	51	28	23	25	32	40	0	0	179
Madhubani	<< BH	42	32	14	11	8	67	24	2	200
Kalahandi	<< OR	63	22	25	13	22	42	15	2	204
Jehanabad	<< BH	51	52	25	13	20	29	12	4	206
Saharsa	<< BH	33	24	27	13	18	49	36	2	208
West Dinajpur	WB	39	32	52	13	12	41	18	2	209
Nawadah	BH	54	40	27	14	22	40	12	2	211
Bahraich	UP	51	40	31	8	22	25	27	6	211
Sitamarhi	BH	45	28	22	19	6	53	27	8	213
Sahebganj	BH	36	44	29	11	4	18	63	10	214
Murshidabad	WB	42	42	40	13	18	57	6	2	220
Cooch Behar	<< WB	51	31	31	13	20	40	27	4	220
Bankura	<< WB	48	54	32	13	14	40	15	4	220
Panna	MP	39	38	51	22	44	20	6	0	220
Pratapgarh	UP	42	66	22	5	24	59	3	2	221
Maharajganj	<< UP	42	44	19	4	*72	41	3	0	225
Jalore	RJ	33	38	28	25	88	19	3	2	227
Aurangabad	BH	60	52	30	15	34	30	6	0	227

Figure 4: District frequency by score.
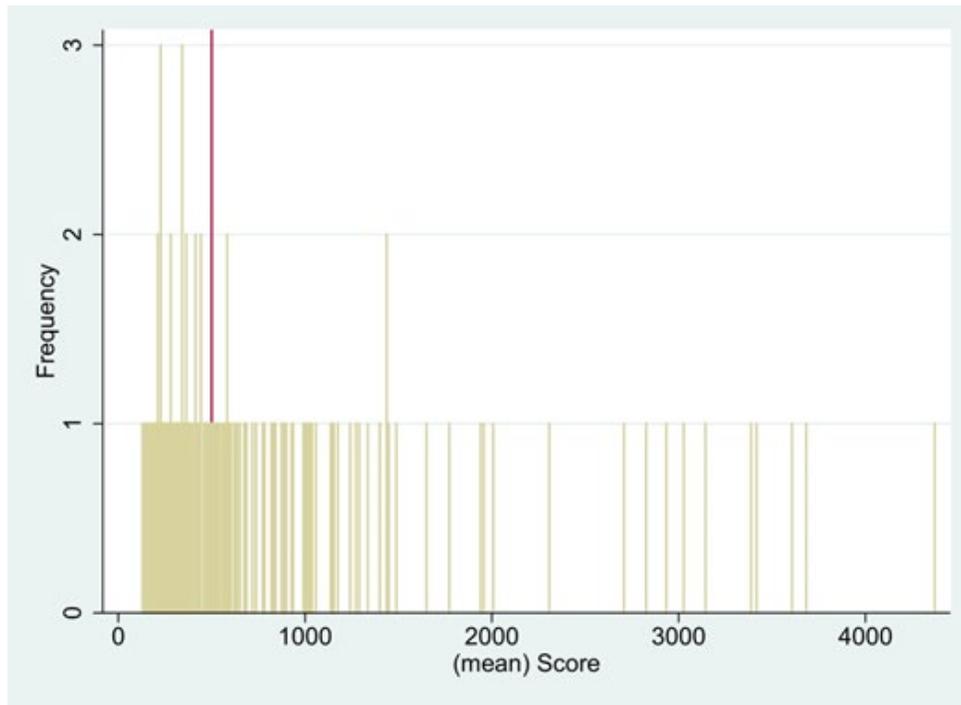
Figure 5: District frequency by score for districts that scored less than 1200 in the BIMARU states.
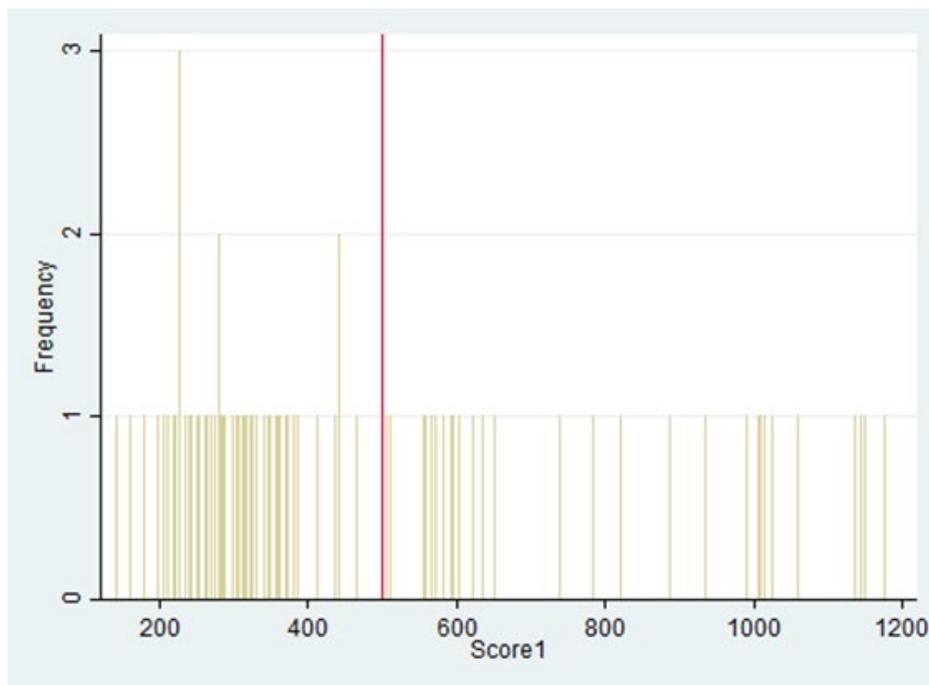
Figure 6: Inter-district migration: before policy vs. after policy.
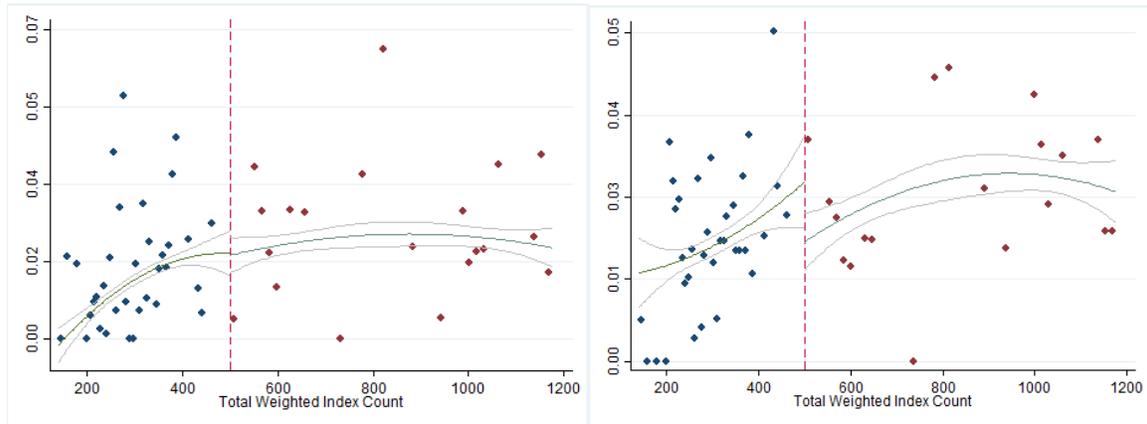
Figure 7: Inter-district migration for job transfer: before policy vs. after policy.
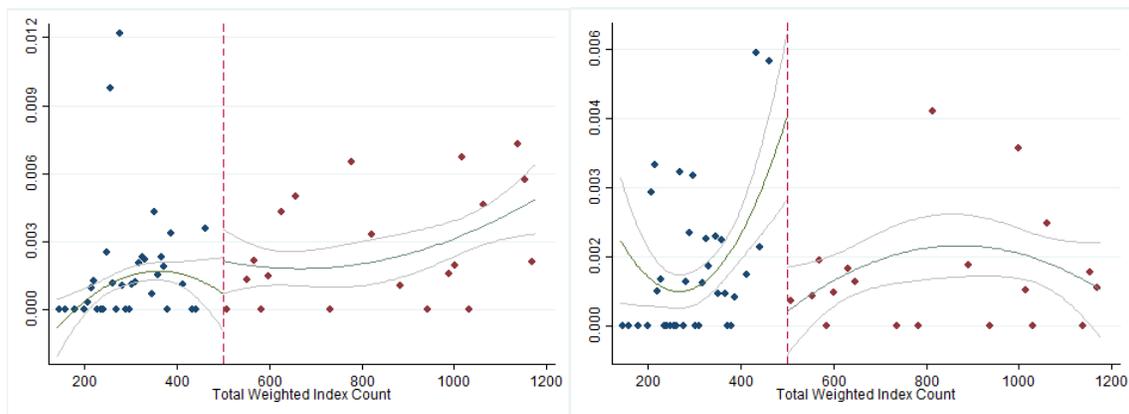
Figure 8: Inter-district migration to move with primary household earner: before policy vs. after policy.
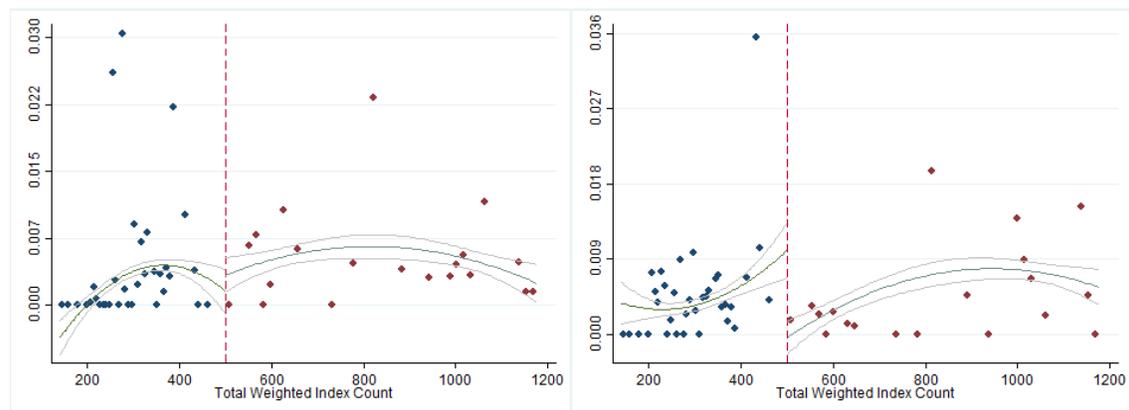
Table 1: Summary statistics for overall migration levels.

Variable	All	Female (working age)	Male (working age)
Migrant	0.072	0.177	0.045
	(0.258)	(0.382)	(0.207)
Find employment	0.004	0.001	0.012
	(0.063)	(0.038)	(0.109)
Job transfer	0.003	0.001	0.008
	(0.051)	(0.031)	(0.088)
Primary earner	0.013	0.017	0.004
	(0.113)	(0.128)	(0.066)
Marriage	0.042	0.150	0.002
	(0.201)	(0.357)	(0.042)
Studies	0.003	0.001	0.006
	(0.054)	(0.030)	(0.076)
Socio-political	0.000	0.001	0.001
	(0.022)	(0.023)	(0.025)
Housing	0.002	0.002	0.003
	(0.039)	(0.040)	(0.051)
No. of observations	98,334	26,925	28,400

Standard errors are in parentheses.

Table 2: Political party of winners by constituency in Rajasthan in the 1991 Indian General

Elections, and the score that the district subsequently received in 1993.

Score	District	Election Winner 1991
227	Jalore	INC
235	Barmer	INC
269	Dungarpur	INC
275	Jaisalmer	INC
281	Dholpur	BJP
304	Sawai Madhopur	BJP
346	Banswara	INC
349	Tonk	BJP
359	Churu	BJP
363	Nagaur	INC
442	Jhalawar	BJP
495	Sikar	INC
583	Bundi	BJP
593	Chittagarh	BJP
623	Bharatpur	BJP
635	Jhunjhunu	INC
775	Ganganagar	INC
783	Pali	BJP
820	Bikaner	INC
996	Udaipur	INC
1014	Jodhpur	INC
1137	Bhilwara	INC
1175	Alwar	BJP
1400	Ajmer	BJP
1437	Kott	BJP
1448	Jaipur	BJP
1650	Sirohi	INC

INC: Indian National Congress

BJP: Bharatiya Janata Party

Table 3: Difference in various district characteristics between treated and untreated districts in 1991.

Variable (log)	Score<1200	Score- all
Population	0.190	-0.0307
	(0.184)	(0.139)
Population growth	0.216	0.137
	(0.197)	(0.137)
Population density	-0.281	-0.245
	(0.404)	(0.210)
Labor force participation	-0.0265	0.0621
	(0.0758)	(0.0526)
No. of observations	85	96

All regressions include state fixed effects.

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Difference in firms and workers hired between treated and untreated districts as per the 1998 census. Outcome variables are collapsed by district and logged.

4. (A)

Manufacturing Sector	All		Female		Male	
Variable	Score<1200	Score- all	Score<1200	Score- all	Score<1200	Score- all
All workers	0.583**	0.362*	0.713**	0.461*	0.602**	0.370*
	(0.239)	(0.183)	(0.293)	(0.240)	(0.242)	(0.192)
Paid workers	0.538	0.336	0.495	0.412	0.617*	0.319
	(0.340)	(0.295)	(0.383)	(0.288)	(0.358)	(0.310)
Number of firms	0.565**	0.326*	-	-	-	-
	(0.233)	(0.168)	-	-	-	-
No. of observations	82	90	82	90	82	90

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

4. (B)

Service Sector	All		Female		Male	
Variable	Score<1200	Score- all	Score<1200	Score- all	Score<1200	Score- all
All workers	0.318*	0.203	0.353	0.373	0.327*	0.185
	(0.166)	(0.143)	(0.254)	(0.226)	(0.168)	(0.145)
Paid workers	0.151	0.0741	0.289	0.181	0.158	0.0574
	(0.196)	(0.165)	(0.317)	(0.229)	(0.194)	(0.167)
Number of firms	0.372**	0.176	-	-	-	-
	(0.168)	(0.151)	-	-	-	-
No. of observations	82	90	82	90	82	90

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Difference in migration levels from 1982 to 1988 between treated and untreated districts, in general, and for specific reasons.

5. (A)

Pre-policy	Overall migration	
Variable	Score<1200	Score- all
Migrant	0.0193	0.0158
	(0.0162)	(0.0135)
Find employment	-0.000122	0.000736
	(0.00194)	(0.00237)
Job transfer	0.00204	0.00168
	(0.00128)	(0.00113)
Primary earner	-0.00352	-0.000320
	(0.00636)	(0.00605)
No, of observations	94,471	111,578

All regressions include state fixed effects.

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

5. (B)

Pre-policy	Intra-district migration		Intra-district rural-urban migration	
Variable	Score<1200	Score- all	Score<1200	Score- all
Migrant	0.0135	0.00116	0.0000674	-0.000362
	(0.0120)	(0.00998)	(0.00242)	(0.00236)
Find employment	-0.000301	-0.000867	0.000150	-0.000108
	(0.00159)	(0.00138)	(0.000626)	(0.000589)
Job transfer	0.00116	0.000300	0.000248	0.0000248
	(0.000772)	(0.000640)	(0.000413)	(0.000331)
Primary earner	-0.00384	-0.00621*	-0.00116	-0.000861
	(0.00320)	(0.00330)	(0.00110)	(0.00125)
No. of observations	94,471	111,578	94,471	111,578

All regressions include state fixed effects.

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

5. (C)

Pre-policy	Inter-district migration		Inter-state migration	
Variable	Score<1200	Score- all	Score<1200	Score- all
Migrant	-0.000844	0.00948	0.00668	0.00515
	(0.00632)	(0.00730)	(0.00433)	(0.00311)
Find employment	-0.000836	0.000871	0.00102	0.000732
	(0.000571)	(0.00147)	(0.000632)	(0.000499)
Job transfer	0.000473	0.00108	0.000408	0.000301
	(0.000887)	(0.000932)	(0.000461)	(0.000302)
Primary earner	-0.000838	0.00422	0.00116	0.00166
	(0.00240)	(0.00333)	(0.00225)	(0.00174)
No. of observations	94,471	111,578	94,471	111,578

All regressions include state fixed effects.

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Difference in migration levels from 1994 to 2000 between treated and untreated districts- in general, and for certain reasons.

6. (A)

Post policy	Overall migration	
Variable	Score<1200	Score- all
Migrant	0.0253*	0.0421***
	(0.0129)	(0.0123)
Find employment	0.00224*	0.00517**
	(0.00115)	(0.00219)
Job transfer	0.00303**	0.00175*
	(0.00131)	(0.000908)
Primary earner	0.00674*	0.00712*
	(0.00357)	(0.00403)
No. of observations	83,256	98,334

All regressions include state fixed effects.

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

6. (B)

Post policy	Intra-district migration		Intra-district rural-urban migration	
Variable	Score<1200	Score- all	Score<1200	Score- all
Migrant	0.0218**	0.0333***	0.00455*	0.00867**
	(0.00843)	(0.0101)	(0.00263)	(0.00394)
Find employment	0.00107	0.00302**	0.000930**	0.00254*
	(0.000655)	(0.00127)	(0.000441)	(0.00133)
Job transfer	0.000947*	0.000658*	0.000600*	0.000350**
	(0.000536)	(0.000345)	(0.000346)	(0.000176)
Primary earner	0.00146	0.00295	0.00148	0.00145
	(0.00286)	(0.00258)	(0.00136)	(0.00147)
No. of observations	83,256	98,334	83,256	98,334

All regressions include state fixed effects.

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

6. (C)

Post policy	Inter-district migration		Inter-state migration	
Variable	Score<1200	Score- all	Score<1200	Score- all
Migrant	0.00269	0.00872*	0.000812	0.000104
	(0.00552)	(0.00498)	(0.00240)	(0.00253)
Find employment	0.000593	0.00166*	0.000576	0.000493*
	(0.000566)	(0.000988)	(0.000364)	(0.000286)
Job transfer	0.00183**	0.00102*	0.000247*	0.0000737
	(0.000872)	(0.000542)	(0.000133)	(0.000367)
Primary earner	0.00577***	0.00544***	-0.000494	-0.00127
	(0.00168)	(0.00174)	(0.000858)	(0.00109)
No. of observations	83,256	98,334	83,256	98,334

All regressions include state fixed effects.

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

6. (D)

Post policy	Overall migration		Inter-district migration	
Variable	Score<1200	Score- all	Score<1200	Score- all
Housing	0.00228	0.0141	0.000163	-0.000246
	(0.00185)	(0.00889)	(0.000202)	(0.000246)
Studies	-0.000383	0.00168	-0.000427	0.000120
	(0.00136)	(0.00147)	(0.000630)	(0.000865)
Marriage	0.0123	0.00974	-0.00422	-0.000156
	(0.00908)	(0.00641)	(0.00393)	(0.00324)
Health	-0.0000194	-0.0000937	0.00000304	0.0000225
	(0.000188)	(0.000155)	(0.00000901)	(0.0000185)
Socio-political	-0.000345	-0.000312	-0.000404	-0.000290
	(0.000392)	(0.000332)	(0.000268)	(0.000239)
No. of observations	83,256	98,334	83,256	98,334

All regressions include state fixed effects.

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Difference in migration levels from 1994 to 2000 between treated and untreated districts by gender and education level.

7. (A)

Overall migration	Female		Male		More educated		Less educated	
Variable	Score<1200	Score- all	Score<1200	Score- all	Score<1200	Score- all	Score<1200	Score- all
Migrant	0.0513	0.0538**	0.0260**	0.0477***	0.0618**	0.0545***	0.0270	0.0490**
	(0.0344)	(0.0250)	(0.0101)	(0.0156)	(0.0259)	(0.0191)	(0.0217)	(0.0194)
Find employment	0.000008	0.000889	0.00799**	0.0157**	0.00753*	0.00176	0.00165	0.0127
	(0.000569)	(0.000927)	(0.00400)	(0.00714)	(0.00432)	(0.00391)	(0.00127)	(0.00770)
Job transfer	0.000665	-0.000347	0.00951**	0.00595**	0.0120**	0.00598*	0.000435	0.000791**
	(0.000867)	(0.000885)	(0.00400)	(0.00257)	(0.00524)	(0.00346)	(0.000291)	(0.000325)
Primary earner	0.0112**	0.00976*	0.00422*	0.00212	0.0151**	0.0101**	0.00252	0.00334
	(0.00470)	(0.00529)	(0.00212)	(0.00195)	(0.00680)	(0.00507)	(0.00234)	(0.00289)
No. of observations	22,532	26,925	23,622	28,400	19,591	24,959	26,563	30,366

All regressions include state fixed effects.

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

7. (B)

Inter-district migration	Female		Male		More educated		Less educated	
Variable	Score<1200	Score- all	Score<1200	Score- all	Score<1200	Score- all	Score<1200	Score- all
Migrant	-0.00992	0.00464	0.0104*	0.00967**	0.0236**	0.0111	-0.0136	0.00374
	(0.0152)	(0.0129)	(0.00540)	(0.00481)	(0.0111)	(0.0102)	(0.00988)	(0.00976)
Find employment	0.00000631	0.000377	0.00212	0.00443	0.00128	-0.00177	0.000895	0.00502
	(0.000298)	(0.000369)	(0.00191)	(0.00301)	(0.00193)	(0.00213)	(0.000911)	(0.00311)
Job transfer	0.000409	0.0000439	0.00611**	0.00342**	0.00801**	0.00418*	0.000196	0.000199
	(0.000840)	(0.000643)	(0.00270)	(0.00158)	(0.00377)	(0.00231)	(0.000140)	(0.000125)
Primary earner	0.00687***	0.00557*	0.00366*	0.00293**	0.0118**	0.00832***	0.000975	0.00147
	(0.00240)	(0.00281)	(0.00199)	(0.00113)	(0.00498)	(0.00281)	(0.00109)	(0.00158)
No. of observations	22,532	26,925	23,622	28,400	19,591	24,959	26,563	30,366

All regressions include state fixed effects.

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Difference in wages and employment from 1994 to 2000 between treated and untreated districts. Outcome variables are logged.

Variable	All		Female		Male	
	Score<1200	Score- all	Score<1200	Score- all	Score<1200	Score-all
Wages	0.269*	0.139	0.203	0.254	0.259*	0.125
	(0.152)	(0.109)	(0.243)	(0.185)	(0.145)	(0.0909)
No. of observations	7,246	9,510	1,332	1,753	5,659	7,459
Employed	-0.0135	0.0110	0.0101	0.0597	-0.0469*	-0.0140
	(0.0286)	(0.0214)	(0.0922)	(0.0687)	(0.0246)	(0.0178)
No. of observations	83,256	98,334	22,532	26,925	23,622	28,400

All regressions include state fixed effects.

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 9: Difference in rents between treated and untreated districts in 2002

Variable (log)	Score<1200	Score- all
Rent	0.0615	0.128
	(0.249)	(0.182)
No. of observations	43,756	47,796
Tenant rent	0.663	0.204
	(0.496)	(0.256)
No. of observations	4,520	5,032
Homeowner imputed rent	0.061	0.133
	(0.265)	(0.187)
No. of observations	39,236	42,764

All regressions include state fixed effects.

Robust standard errors, clustered by score, are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

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