Regional Development through Place-Based Policies: Evidence from a Spatial Discontinuity

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Abstract

In 2002 the Indian government targeted the new state of Uttarakhand with massive improvements in infrastructure, a generous investment subsidy, and a complete exemption from corporate and excise taxes. I estimate the causal effect of this policy on economic development by exploiting the spatial discontinuity created by the new state border. Nighttime light emissions rise sharply in the targeted state, implying a 28 percent increase in output. Village public goods, farm employment, and proxies for household welfare rise in tandem. I rule out that the effect is driven by decentralization of policy, improvements in business regulations, or differential trends at the border.

Keywords: Place-based policy, development, infrastructure, tax incentives
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1 Introduction

Though the gap between rich and poor countries has long held the attention of economists, recent work has shown that the gap between rich and poor regions within a country can be nearly as wide (Ravallion and Jalan, 1999; Ravallion and Chen, 2007; Acemoglu and Dell, 2010). For example, the median household in India's wealthiest district earns 16 times as much as its counterpart in the poorest district—more than half the gap between the median Indian and American household. Such gaps may arise and persist because underdevelopment is self-reinforcing, as would happen if there are agglomeration economies in production. The divergence is especially stark in developing countries, whose explosive economic growth has been concentrated in the most productive regions (Felkner and Townsend, 2011). For reasons both cultural and political, governments in these countries are unable or unwilling to encourage migration to productive regions. Instead they have sought to close the gaps between regions.

Place-based policies—policies that target tax breaks and infrastructure development at an underdeveloped region—have been an especially common response. Such policies are often justified on the grounds that temporarily making a region attractive may convince firms to move, creating a new center of agglomeration that remains productive after the policies end (Kline and Moretti, 2014b). But both theory and evidence is mixed on whether place-based policies have even short-term effects, leading some economists to question their value (Glaeser and Gottlieb, 2008). One challenge to identifying any effect is that areas targeted with such policies are not only poorer but growing more slowly, potentially confounding difference-in-differences estimates. Another challenge is that many programs previously studied were modest—perhaps too modest revitalize an economic backwater.

This paper measures the impact of one of the world's most generous place-based policies. In 2002 the Indian government targeted the newly created state of Uttarakhand with a tenfold increase in infrastructure spending, better access to existing power plants, a complete exemption from corporate and excise taxes, and a generous investment subsidy.[1] This largess was meant to compen-

[1]A rough calculation puts the cost of the grants and tax exemptions from 2001 to 2012 at roughly 34 billion U.S. dollars (at 2005 purchasing power parity, as are all dollar amounts that

sate for the Himalayan state's geographic disadvantages, which couple a rugged terrain that is costly to develop with a population too small to form a viable tax base. By funding industrial estates and offering tax incentives, the government hoped to spur enterprise that would cause rapid and sustained economic development.

I test whether these efforts succeeded by running a series of spatial regression discontinuities. I estimate how the border discontinuity changes over time in each year for ten years before and after the policy—a differences-in-discontinuities design. I measure economic activity using the nighttime lights data that Henderson et al. (2012) link to economic growth. I compute the growth in light emissions within small cells on either side the border. This yields a measure of economic activity granular enough to estimate the discontinuity at the border.

My design avoids several problems that would normally make it implausible to equate the difference in outcomes across a state border with the effect of a policy targeted within that border. For example, one might expect pre-existing differences in state laws would drive firms into the state with easier regulations, creating differential trends in economic growth. But in my design the treated state is formed from the control state, ensuring regulations on either side of the new border were identical at the time of the split. This leaves only differences in geographic advantages and social trends. Assuming both are smooth across space they will not differ in areas just across the border. Although there are differential trends in the targeted state as a whole, I show that these trends shrink to insignificance near the border.

I find a sharp increase in light emissions on the targeted side of the border in the first year of the policy. When rescaled by the correlation between light and output, the increase in light implies output rose by 12 percent. The size of the effect only grows, reaching its highest point when my sample ends 10

follow). By comparison, the Tennessee Valley Authority program cost roughly 20 billion dollars (Kline and Moretti, 2014a), the first round of the U.S. Urban Economic Zone roughly 466 million dollars (Busso et al., 2013), the California enterprise zone project roughly 76 million dollars in 1995 and 1996 (O'Keefe, 2004), and the French Enterprise Zones between 289 and 547 million dollars per year (less than 4.9 billion dollars total from 1998 to 2006, according to Briant et al., 2015). Though total regional transfers in the U.S. and the E.U. are larger than these figures (Von Ehrlich and Seidel, 2015), to my knowledge the only single program that is larger is China's Leading Group for Economic Development in Poor Areas, which spent 65 billion USD from 1986 to 1997 (Park et al., 2002). But this spending is spread over nearly 10 times as many people as India's program, making the per capita spending of India's program somewhat more generous.

years after the program began. By my most conservative estimate, output is 28 percent higher than than at baseline. Using census data on living conditions in towns, I confirm that the change in nighttime lights is mirrored by a change in directly measured household welfare.

I rule out that the effect is driven by any of several confounders. The first of these is that forming a new state decentralizes political power, which may itself have an effect. To address this concern I test for a similar discontinuity in two other newly created states. Though all three states were formed in November of 2000, only Uttarakhand got the place-based policy. Neither of the two other states show significant effects in the year that they formed, at the start of the policy, or even for years afterwards. I also show that areas within Uttarakhand that were far from the new state capital had effects similar to those close to the capital. Together these two tests make it unlikely the effects are driven by decentralization.

Another potential confounder is that the new state may have reaped its gains by improving business regulations. I show that whereas the ease of doing business in the two other new states improved or remained similar, it actually *worsened* in Uttarakhand. A third confounder would be if the effects are caused not by improvements in the treated region but damage to control regions—regions just outside the treated area. I show that, if anything, control regions benefitted from their proximity to the targeted state. Finally, I show that most of the increase in light emissions happened at the sites of major industrial estates created through the program—suggestive evidence that the place-based policy is the major change affecting firms in the targeted region. Taken together these tests make it unlikely that such confounders drive the results.

One may worry that the benefits of this policy—new public goods and better economic opportunities—accrued only to towns. Yet I find that villages at the border also reaped substantial benefits. By 2011, villages in Uttarakhand were more likely to have primary schools and health centers. Migrants from regions further from the industrial estates arrived in border villages to take up new jobs. These jobs were largely in farming, suggesting that, perhaps through its effect on aggregate demand, the policy stimulated production even outside manufacturing.

Finally, I test for whether the program succeeded in creating new centers

of agglomeration. I measure the effect of the policy on population density, a common measure of agglomeration. Even under generous assumptions, population agglomeration raised productivity by only 3.2 percent. I find no effect on human capital agglomeration. This suggests the bulk of the change in output is the direct effect of improved infrastructure and tax incentives.

The key contribution of this paper is to show that, even under the relatively weak institutions that govern a developing country, place-based policies can cause growth. A place-based program is in essence external aid to a region within a country, but much of the literature finds that external aid has had little or even negative effects (Djankov et al., 2008; Rajan and Subramanian, 2008; Kraay and Raddatz, 2007; Zhang and Zou, 1998). Some studies suggest aid is only effective in countries or regions with efficient governments and sound economic policy (Isham and Kaufmann, 1999; Burnside and Dollar, 2000; Becker et al., 2013).[2] It is not clear that India, which in 2014 the World Bank ranked 132nd in the world for ease of doing business, or Uttarakhand, ranked 23rd out of 32 states within India, meets these criteria.

Nevertheless I find immediate and large effects that persist for a decade. A rough cost-benefit calculation suggests that every dollar spent created between 0.65 and 1.55 dollars of benefit. Even the low end of this range is impressive when compared to other large transfer programs within developing countries (e.g. Reinikka and Svensson, 2004; Olken, 2006, 2007), which lost vast sums to corruption alone. A place-based policy—especially one in which much of the "expenditure" was through tax exemptions that cannot be stolen or misplaced—may be a cost-effective way to transfer income between regions in developing countries.

My second contribution is to estimate these effects using a design that requires weaker assumptions than much of the prior literature, which is based on difference-in-differences estimators.[3] I show in Section 3 that the difference-

[2]Both Boone (1996) and Easterly et al. (2004) have disputed this conclusion. Their findings may be consistent with my result that the program has large effects despite policy imperfections.

[3]Grembi et al. (2014) show formally that the difference-in-discontinuities estimator is valid under weaker assumptions than the difference-in-differences estimator. The other methods used commonly in the literature make similar or stronger assumptions. Dynamic panel estimators may control for mean reversion but are still biased by differential trends. Propensity score matching requires that treatment be as good as random conditional on observables, which is

in-discontinuities design is necessary because, at least in my setting, trends away from the border are not parallel. These differences in trends would bias difference-in-differences.[4] One caveat is that the effect is local to the border; but since most of Uttarakhand's population lives near the border, the border effect may be a reasonable estimate of the average effect.

Another benefit of the design is that I observe nighttime lights yearly for ten years before and after the policy starts. This lets me confirm there are parallel trends at the border before the policy. Unlike earlier work that relies on relatively infrequent censuses or surveys, this paper can confirm that effects appear precisely in the first year of the policy. The timing makes it more likely that these effects are caused by the policy. The difference in design may explain why I find larger and more positive effects than other papers that study place-based policies or external aid, especially those that focus on developing countries. My results suggest earlier pessimism about place-based policies and external aid may need to be reconsidered.

1.1 Related Literature

This paper most directly extends the literature on place-based policies, which has drawn mixed conclusions about their success. Studies of the French Urban Zones program have found it had at best modest and heterogeneous effects on employment (Gobillon et al., 2012; Givord et al., 2013; Briant et al., 2015). O'Keefe (2004) and Neumark and Kolko (2010) both study California's enterprise zone program but draw opposite conclusions. Bondonio and Engberg (2000), who study the State Enterprise Zones, find no effect, whereas Ham et al. (2011), who study this same program as well as the federal Empowerment Zones and Enterprise Communities programs, find large effects. Busso et al. (2013) also studies the Empowerment Zones program and finds positive effects. Kline and Moretti (2014a) find that the Tennessee Valley Authority's infrastructure program had large aggregate effects. To my knowledge only two papers have studied place-based programs outside the developed world. Both Jalan and

arguably a stronger assumption than parallel trends.

[4]As I note in Section 1.1, the recent working paper by Von Ehrlich and Seidel (2015) uses a credible regression discontinuity to estimate the effects of German place-based policy. Like much of the literature, they focus on developed rather than developing countries.

Ravallion (1998) and Park et al. (2002) find that Chinese place-based programs raised household welfare. As mentioned earlier, much of this literature measures impacts using either difference-in-differences, dynamic panel estimators, or propensity score matching. These methods require assumptions about parallel trends or unconfoundedness not necessary for the approach used in this paper. More recent work (e.g. Von Ehrlich and Seidel, 2015) exploits a spatial discontinuity like that studied here, but like much of this literature studies a developed country (in their case, Germany).

This paper also builds on the extensive work on how firms respond to taxes. Several papers (e.g. Gentry and Hubbard, 2000; Appelbaum and Katz, 1996) have shown that business taxes fall especially hard on new entrants, suggesting that cutting taxes may stimulate entry. Other firm-level studies show that tax cuts induce informal firms to become formal, which makes them more profitable (Fajnzylber et al., 2011; McKenzie and Sakho, 2010). Several studies have used cross-country regressions to show that higher taxes predict lower investment and firm entry (for example, Cummins et al., 1996; Djankov et al., 2010; Da Rin et al., 2011). I add to this work by estimating the causal effect on aggregate output of a policy that includes large tax reductions. Another branch of literature shows that temporary tax cuts can act as temporary stimulus (House and Shapiro, 2006; Romer et al., 2010; Mian and Sufi, 2012). I extend this literature by showing that tax cuts may also stimulate medium-run economic development—that is, effects on output that last at least 10 years.

Finally, this paper extends a vast literature on the effect of infrastructure on economic development in poor countries. Though it may seem obvious that a massive investment in infrastructure would increase output, in fact the literature has found mixed evidence. Aggarwal (2014), Asher and Novosad (2015), and Donaldson (Forthcoming) find that massive road and railroad construction programs in India improved welfare and occupational choice. However, Banerjee et al. (2012) and Duflo and Pande (2007) find less hopeful effects of roads in China and dams in India. My results suggest improvements to infrastructure can cause large increases in output. A more micro-focused literature has found that access to power has positive effects on firms in India (Allcott et al., 2014; Abeberese, 2012), households in India (Chakravorty et al., 2014), and rural employment in South Africa (Dinkelman, 2011). My work complements this

literature by measuring the effect of a big improvement in power infrastructure on aggregate output.

2 The Policy

What I have thus far called "the policy" is actually the combination of several measures all meant to counter Uttarakhand's geographic disadvantages. Starting in 2002, these measures targeted spending for new infrastructure, better access to existing infrastructure, and tax exemptions for firms at a handful of industrial estates. The first measure was the special treatment accorded Uttarakhand by India's Planning Commission. India is a fiscal union in which states pay for their spending with both their own tax revenue and transfers from the central government (Bagchi, 1997). The size of each tranfer and what fraction should be granted versus loaned is decided by India's Planning Commission. The Commission usually grants funds proportionately with population and past success in meeting development goals. However, it believes mountainous and sparsely populated states like Uttarakhand need special assistance. Figure 1.A shows the value of grants from the central government made to Uttarakhand and Uttar Pradesh, as scaled by 2001 population.[5] The difference is stark; from the outset Uttarakhand received over ten times as much support per person as Uttar Pradesh, and in later years the gap widened. [6] Grants for both states fall drastically in 2012, roughly coinciding with the end of my sample period.[7]

Though in principle these funds were available as soon as the new state formed, in practice they were only channeled towards building industrial estates when in 2002 the government of Uttarakhand created the State Infrastructure and Industrial Development Corporation of Uttarakhand Limited (SIID-

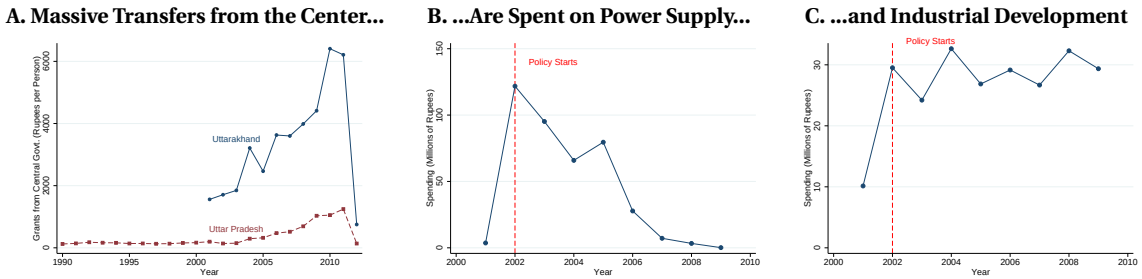
[5]Source: Reserve Bank of India. I exclude the data on the new state's budget for 2000-2001 (as the state exists for only part of the fiscal year). In all discussion of budget numbers I refer to the fiscal year by its initial year—for example, the 2001-2002 fiscal year as 2001. Since India's fiscal year begins in on 1 April this seems the most accurate description of the calendar year of the economic activity.

[6]Uttarakhand was awarded Special Category Status, which brings an even larger transfer, in 2010. But it is clear from Figure 1 that it had been receiving favorable treatment long before.

[7]The drop in grants may have been a result of the central government's shift towards directly devolving tax revenues to the states rather than dispensing it as grants.

Figure 1

Uttarakhand Receives and Spends Funds from the Center



CUL). Endowed with share capital of 500 million rupees (USD 37.3 million in purchasing power parity) and up-front capital of 200 million rupees (USD 14.9 million), SIIDCUL focused its efforts on building government-run industrial estates and encouraging private industrial estates. These industrial estates were built with dedicated sources of power and water. For example, the Integrated Industrial Estate of Haridwar offers a large substation of 220 kilovolts, 4 smaller stations of 33 kilovolts, and 11 kilovolt stations "as required" (Government of Uttarakhand, 2016).

Panels B and C of 1 show that Uttarkhand's spending on power supplies rose thirty-fold and its spending on industrial development tripled in 2002. Not only was this the year that SIIDCUL was formed, but Uttarakhand's public power corporation reports in its balance sheets that the size of grants and subsidies it received from the government more than tripled from 2001 to 2002. Finally, this was also the first year that Uttarakhand was allocated full control over its own power resources, including rights to draw from power plants run by the central government. According to data compiled by Indiastat, Uttarakhand's drawal rights from central power rose from zero to over 1000 gigawatt hours. As I show in Section 4, this spike in spending and access coincides with a large increase in nighttime lights.

The creation of SIIDCUL roughly coincided with the announcement in March of 2002 of a package of tax exemptions for Uttarakhand and two other mountainous states. These exemptions, which became the "Special Package Scheme

for Himachal Pradesh and Uttarakhand," took effect in early 2003. The law includes a raft of tax incentives, the most generous of which are a complete exemption from federal income taxes for the first 5 years of production (and a 30 percent reduction for the next 5 years); a complete exemption from excise taxes for 10 years; and a 15 percent investment subsidy for new or expanded factories. For comparison, in 2003 the two exemptions bought relief from a statutory coporate tax rate of 36.75 percent and an excise tax of 16 percent.[8] These exemptions are targeted at SIIDCUL's industrial estates, meaning all firms within those estates can claim the exemptions.[9] Crucially, firms can only exploit the investment subsidy and excise tax exemption if they build and produce within Uttarakhand. It is not enough to simply move their nominal headquarters.

Firms moved quickly to build factories in the new industrial estates. As of this writing, 680 firms had requested space in the Integrated Industrial Estate of Haridwar, just one of the seven government-run estates. Figure 2.A shows the number of factories registered and producting in Uttarakhand by fiscal year. The number started to increase in 2004, which suggests firms started building their factories in 2002 and 2003 after the industrial estates were created and the tax policy was announced. Whereas there were roughly 750 factories in 2001, the year before the program, by 2012 the number had risen to nearly 3000. Figure 2.B, which shows the change in the number of factories, makes it clear firms were responding in part to the tax incentives. Only factories registered by 2010 could claim the excise tax exemption. After the deadline the rate of new registrations drops sharply, suggesting that firms pushed forward their investment to exploit the policy.

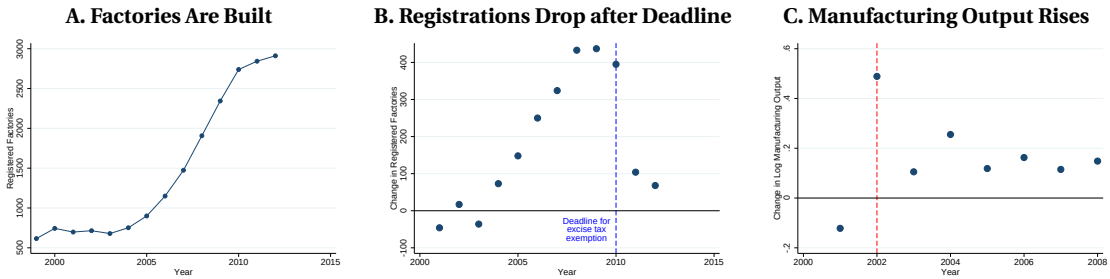
The effect of these measures does not seem restricted to the extensive margin. Figure 2.C shows the year-on-year change in the log of manufacturing output in the formal sector.[10] Whereas in a typical year output rises by about 0.15 log points, in the first year of the program it rises by nearly 0.5 log points. Given that Figure 2.B shows no similar sharp increase in the number of factories in 2002, existing factories must have increased their production. The regression

[8]As noted in Appendix C, the effective rate is somewhat lower but still far from trivial.

[9]The policy also indicated several "thrust" industries, typically in agriculture or tourism, that could be claimed from anywhere in the state.

[10]The data for this figure comes from the nominal state-wise gross domestic state product tables provided by the Reserve Bank of India.

Figure 2
Firms Respond to the Policy



discontinuity estimates of Section 4 confirm that this sharp increase in production coincides with a sharp increase in nighttime light emissions.

3 Research Design

3.1 Data

I measure economic output using the Nighttime Lights Time Series from 1992 to 2012. The National Geophysical Data Center created this series using data from Defense Meteorological Satellite Program. The series divides the earth into a grid with cells of 30 arc seconds on each side. For each year the series gives an index of the average intensity of light emitted from each cell after correcting for cloud cover and natural sources of light (e.g. forest fires).

To measure the effect of each new state I link the nighttime lights to administrative boundaries created by ML Infomap. The data give the boundaries of Uttarakhand and Uttar Pradesh as well as the other two states that split. These boundaries have not changed since 2000.

I break each state into a grid of 0.1x0.1 degree cells.[11] For each cell and for each year I compute the average light intensity, making the cell-year my unit of observation. Henderson et al. (2012) measure light intensity with log of the average "digital value," meaning the value of the index created for the Nighttime

[11] I ensure the cells do not cross the border. That is, after drawing square cells I split any cells crossed by the border and assign them to the old or new state.

Lights series. Though their measure works well for entire countries, it creates problems for small cells because some are completely dark or very faint. Instead I measure intensity with $\log(1 + Digital\ Value)$. Adding 1 ensures the measure is defined for dark cells, and that the measure does not take extreme negative values for cells that are very faint. I show in Section 4.3 that despite the minor change the measure still predicts both output and household welfare. (Indeed, I show in Appendix 1.1 that the modified measure is a better predictor of welfare than the unmodified measure.) I also show in the appendix that the results hold when I instead define light intensity as the level of the digital value.

One problem with using nighttime lights to measure growth is that different satellites are used to measure light in different years. If a satellite is better able to detect light from some parts of the world the measure of growth will be distorted. But my design implicitly controls for this problem. The regression discontinuity compares directly adjacent areas within a year. Any distortion in measurement will be similar on both sides of the border and thus absorbed by the time-varying polynomial in latitude and longitude.

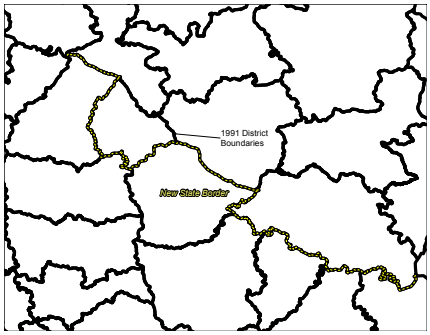
I translate increases in light to increases in output using data on state domestic product from the Reserve Bank of India. I translate light to welfare by constructing a panel of subdistricts for the years 2001 and 2011, to which I link night lights and data from the corresponding rounds of the Indian census.[12] The census records the fraction of households with access to power, latrines, and a home with a solid roof, as well as the fraction that are urban.[13] I also measure household welfare directly using these same measures of welfare at the level of the town.[14] Finally, I use the village directory from the 1991, 2001, and 2011

[12]The panel includes only subdistricts whose boundaries have not changed, as forming a panel for subdistricts that have split, merged, or been amalgamated is not straightforward. I construct the panel by matching each 2011 subdistrict to the 2001 subdistrict that contains its centroid. I compare the area in 2011 to the area in 2001 and discard any matches for which the area changes by more than 1 percent. This effectively keeps only subdistricts with stable borders.

[13]I define a "solid roof" as one made of tile, slate, metal sheet, brick, stone, or concrete.

[14]The Census defines a settlement as a town if it meets a population threshold, the density of that population meets a threshold, and most of that population is employed outside agriculture. I geocoded the towns in 2001 using the India Place Finder (http://india.csis.u-tokyo.ac.jp/). Using the reclink Stata command I fuzzy match the 2001 towns to the 2011 towns by name. I drop any town that has a match probability below 0.9 or appears to have moved too far, as that suggests an incorrect match. This yields a panel of 750 towns.

Figure 3
The New State Border Follows Existing Districts



Indian Census to measure whether village outcomes.[15]

3.2 Background: The Three New States

By 1997, the legislative assemblies of Uttar Pradesh, Madhya Pradesh, and Bihar had all passed resolutions calling the formation of the three new states. India's national parliament, which holds sole authority to create new states, passed laws to create all three states in November of 2000. Chhattisgarh split from Madhya Pradesh on 1 November; Uttarakhand from Uttar Pradesh on 8 November; and Jharkhand from Bihar on 15 November. The reasons for the splits were complex and varied, but all three areas were considered underdeveloped.[16] It was expected that making new states of these three regions would help them develop (Tillin, 2013). But only Uttarakhand was deemed too mountainous to survive without special support from the center.

The borders of the new states were drawn to keep existing districts intact,

[15]Very special thanks to Sam Asher and Paul Novosad for sharing these data.

[16]Uttarkhand's path may be the most contorted (Tillin, 2013). Unrest began in Uttar Pradesh's Himalayan region when environmentalists protested timber concessions. These quickly turned to anti-environmentalist protests after the Union government under Indira Gandhi banned the felling of tall trees, stifling economic development. But these protests only became serious calls for a new state with the rise in the 1990s of the Bharatya Janata Party. When the state government in Lucknow imposed a quota on university seats for low caste students, Uttarakhand erupted in discontent. The Bharatya Janata Party capitalized on this discontent, channeling it into a new drive for secession. By creating a new state the Party hoped to create a new support base in northern India.

minimizing concerns over an endogenous border. The three "Reorganization" acts simply name the districts that will form the new state. For example, Statement 3 of the Bihar Reorganization Act states "there shall be formed a new State to be known as the State of Jharkhand comprising the following territories of the existing State of Bihar, namely: Bokaro, Chatra, Deogarh, ... and thereupon the said territories shall cease to form part of the existing State of Bihar." Though the boundaries of districts sometimes change, they did not change at any point between when the decision to form new states was made and when the new states actually formed. Figure 3 shows the 1991 districts boundaries overlaid with the new border between Uttarakhand and Uttar Pradesh. The new border simply follows the old district boundaries.

It is still possible that the districts chosen differed in some key respect. The research design I propose in Section 3.4 effectively assumes any such differences are fixed over time. This is similar to assuming there are parallel trends at the border. As I show in the next section, this assumption seems plausible. Differences in trends shrink to insignificance near the new boundary.

3.3 Differential Trends Shrink to Insignificance at the Border

As noted in the introduction, difference-in-differences—the estimator of choice in most studies of place-based policies—may be biased by differential trends in the targeted and untargeted areas. A simple placebo test—an estimate of the effect of a policy that never happened—can show whether differential trends are a problem. Using only pre-policy data I estimate the difference-in-differences effect of a fake policy chosen to begin in 1996 (halfway between the start of my sample and the year before the policy).

The top panel of Table 1 shows that the fake policy has a large and statistically significant "effect" on nighttime lights. Each column shows the estimate when I restrict the test to observations within a given distance of what in 2001 becomes the new border. The largest effect—a 0.24 log point increase—appears when the test uses observations within 100 kilometers of the border. The effect grows smaller when using only observations within 20 kilometers of the border, but remains large and significant. For reference, districts along this border are between 30 and 80 kilometers wide, suggesting that even estimates made using

Table 1

Difference-in-Differences Estimates are Biased by Pre-Trends

	Fake 1996 Policy		
	Distance < 100	Distance < 20	Distance < 4
Placebo Policy	0.240***	0.146**	-0.022
	(0.028)	(0.069)	(0.090)
Subdistricts	98	29	26
Cells	781	217	90
Observations	7810	2170	900
	Differential Trends		
	Distance < 100	Distance < 20	Distance < 4
Pre-Trend	0.045***	0.026**	-0.005
in Uttarakhand	(0.005)	(0.012)	(0.015)
Subdistricts	98	29	26
Cells	781	217	90
Observations	7810	2170	900

Note: The top panel gives the difference-in-differences estimate of the "effect" on log light intensity of a placebo policy enacted in Uttarakhand in 1996, 6 years before the actual policy. The bottom panel reports the differential trend in the targeted area. All standard errors are clustered by subdistrict. The regressions use only data from before the actual program began.

only observations in border districts may be biased.[17]

But the bias vanishes when the distance is restricted to within 4 kilometers of the border. Within 4 kilometers the point estimate is less than one-tenth the estimate at 100 kilometers, and it is statistically insignificant. The bottom panel of Table 1 shows why. Again using only pre-policy data, I estimate the differential linear trend in the targeted area. There is a clear trend among observations within 100 and 20 kilometers. Targeted areas are actually growing 4.5 or 2.6 percent faster. This trend shrinks to a statistically insignificant -0.5 percent within 4 kilometers.

The same pattern holds for trends in other variables. Table 2 shows the differential change from 1991 to 2001 in each of several variables drawn from the village table. There is strong evidence of differential trends within 40 kilometers of the border, but these trends become small and insignificant when restricted to within 4 kilometers.[18]

[17]Chaurey (2013), who runs difference-in-differences on firms in districts on either side of the Uttarakhand-Uttar Pradesh border, argues that adjacent districts are similar enough to have parallel trends. Tables 1 and 2 suggest that, at least for the outcomes I study, adjacent districts do not have parallel trends.

[18]The marginally significant trend on population could be a sign that there was some migra-

Table 2

Pre-Trends in Village Outcomes Shrink Near the Border

Distance < 40

	Population	Workers	Under 7	Work in Farming	Literacy	Health Center	School	Paved Road
Pre-Trend	-191.93***	-17.90***	-37.99***	-0.82	-0.03***	0.00	-0.06***	-0.11***
in Uttarakhand	(42.78)	(6.36)	(7.88)	(6.12)	(0.01)	(0.02)	(0.02)	(0.03)
Villages	10596	10596	10596	10596	10596	10596	10596	10596
Sub-districts	54	54	54	54	54	54	54	54

Distance < 4

	Population	Workers	Under 7	Work in Farming	Literacy	Health Center	School	Paved Road
Pre-Trend	69.52*	3.05	15.53	-6.14	-0.01	-0.04	-0.01	-0.03
in Uttarakhand	(35.62)	(8.97)	(9.16)	(10.86)	(0.01)	(0.03)	(0.04)	(0.08)
Villages	1012	1012	1012	1012	1012	1012	1012	1012
Sub-districts	23	23	23	23	23	23	23	23
Control Mean in 1991	959.27	281.64	204.05	240.31	0.31	0.11	0.56	0.60

Note: Each column estimates the average change in the outcome from 1991 to 2001 among villages in the targeted area relative to the change in the control area. The regressions are restricted to villages less than the indicated distance from the border between Uttarakhand and Uttar Pradesh. "Workers" is the number of people who worked for at least 6 months. "Under 7" is the number of children under 7. "Work in Farming" is the number of people working in farming. "School," "Health Center," and "Paved Road" are indicators for whether the village has a primary school, a primary health center or subcenter, and are approached by a paved road.

3.4 Estimation

The previous section shows that any differential trends between the targeted and untargeted states shrink to insignificance near the border. To exploit this fact I use a series of estimators that compare the difference in the discontinuity at the border across years—a difference-in-discontinuities estimator.

Since a discontinuity in space does not have a single running variable, a spatial regression discontinuity may be run using any of several specifications. To my knowledge there is no clear consensus on which is best. Rather than choose one I compare the results across three specifications.

The first specification uses a spatial polynomial in latitude and longitude to control for bias. I follow Dell (2010), who uses a third-order polynomial in the latitude and longitude of each observation as a control function. This control function absorbs all smooth variation in the outcome. The effect is measured by the coefficient on an indicator for being in the targeted state, which captures the discontinuous change at the border. Recall from Section 3.1 that the night-time lights are averaged within cells on a grid. Let i index each cell, let t be the

tion in anticipation of the formation of the new state. If so, it is very little migration—roughly 0.7 percent average annual population growth within the border region, far too little to drive the post-policy results. But it is also quite possible it is caused by sampling variability. Even if all the coefficients in the bottom panel are in truth zero, there is a 57 percent chance that one of the eight independent tests will reject at the 10 percent level.

year of observation, and let P^3 be a third-order polynomial in the latitude and longitude of the centroid of each cell. I estimate

$$\begin{aligned}[Light]_{i,t} = [Fixed\,Effect]_i &+ \sum_{t=1993}^{2012} \kappa_t [Year\ Dummy]_t \\ &+ \sum_{t=1993}^{2012} [Year\ Dummy]_t \times P_t^3([Lat]_i, [Lon]_i) \\ &+ \sum_{t=1993}^{2012} \beta_t^S [Year\ Dummy]_t \times [Targeted]_i + [Error]_{i,t}\end{aligned} \tag{1}$$

where $[Targeted]$ is an indicator for whether the cell is inside the targeted region. There is no direct term for the polynomial $P^3(\cdot)$ or the dummy $[Targeted]$ because they are absorbed into the fixed-effect. The coefficients $\{\beta_t^S\}$ measure the effect at the new border, relative to its effect in 1992, in each year before and after the policy.

The second approach uses the distance to the new border as a univariate running variable. Let $L_t([Distance]_i, [Targeted]_i) = \omega_{1,t}[Distance]_i + \omega_{2,t}[Distance]_i \times [Targeted]_i$. Following Imbens and Lemieux (2008) and Lee and Lemieux (2010) I estimate a local linear regression of the form

$$\begin{aligned}[Light]_{i,t} = [Fixed\,Effect]_i &+ \sum_{t=1993}^{2012} \kappa_t [Year\ Dummy]_t \\ &+ \sum_{t=1993}^{2012} [Year\ Dummy]_t \times L_t([Distance]_i, [Targeted]_i) \\ &+ \sum_{t=1993}^{2012} \beta_t^D [Year\ Dummy]_t \times [Targeted]_i + [Error]_{i,t}\end{aligned} \tag{2}$$

Similar to the first specification, the coefficients $\{\beta_t^D\}$ measure the effect at the new border.

The third specification is the simplest: a comparison of means very close to

the border. Using only observations within 4 kilometers of the border I estimate

$$[Light]_{i,t} = [Fixed\ Effect]_i + \sum_{t=1993}^{2012} \kappa_t [Year\ Dummy]_t + \sum_{t=1993}^{2012} \beta_t^C [Year\ Dummy]_t \times [Targeted]_i + [Error]_{i,t} \tag{3}$$

to yield estimates $\{\beta_t^C\}$.

In all three specifications I cluster standard errors by sub-district to account for arbitrary correlation in the error term across time and space. Since the number of clusters in the third specification is small, I show in Appendix 1.1 that bootstrapped standard errors yield similar results to asymptotic errors.[19] I use a bandwidth of 30 kilometers to estimate the first two specifications, and a bandwidth of 4 kilometers for the third. I show that the qualitative results are robust to the choice of bandwidth.

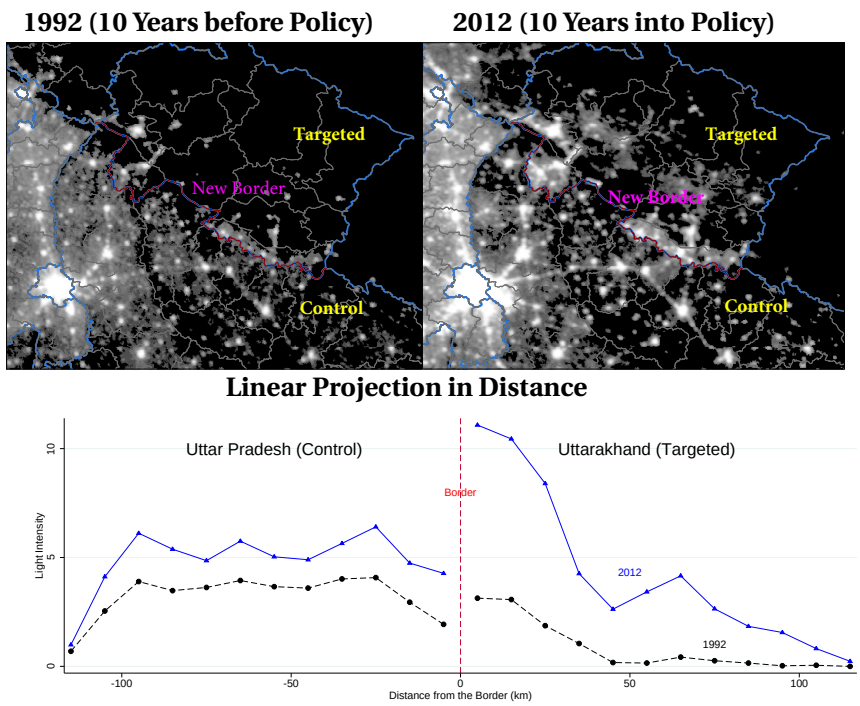
4 Results

4.1 Border Effects

Just 10 years after the start of the policy, the effect is literally visible. Figure 4 shows the raw nighttime lights data. The purple line marks the new border between Uttarakhand, the targeted state, and Uttar Pradesh, the control. In 1992 the new border did not yet exist and the policy was 10 years from its start. There is no evidence of a difference in the intensity of light on either side of the border. Several clusters of light, which roughly mark urban outgrowths, are split by the future border. But 10 years after the program began it is clear that the parts of these outgrowths on the northeast side of the boundary have grown

[19] Using the approach proposed by Conley (1999), as implemented by code written by Hsiang (2010), produces much smaller standard errors. Indeed, one of the coefficients has an implausible p-value of 8×10^{-65}. A Monte Carlo simulation confirms that, at least for a difference-in-discontinuities design, the spatial HAC suffers from size distortions. Bertrand et al. (2004) find that non-spatial HAC standard errors, when applied to difference-in-differences estimators, are often too small. A similar problem likely applies to a difference-in-discontinuities estimator. I avoid it by taking the more conservative approach of allowing arbitrary within-cluster correlation. My simulations suggest clustering solves the size distortion. See Online Appendix B for the details of the simulation.

Figure 4
Raw Light Emissions



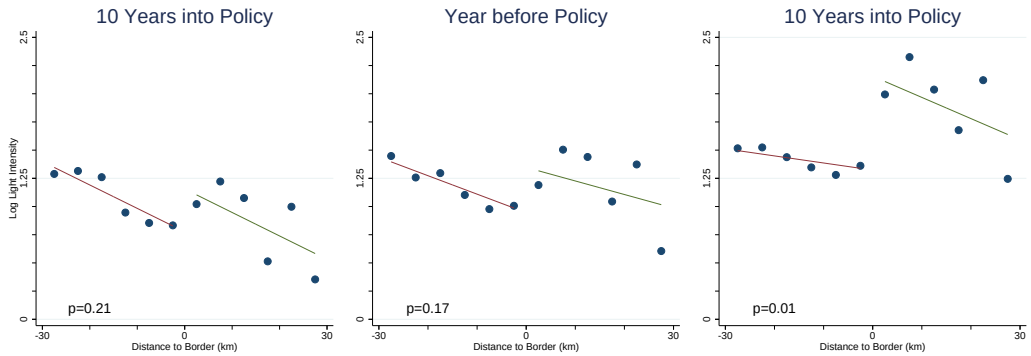
Note: Top panel: raw images from NOAA (see Section 3.1). Bottom panel: digital values averaged within cell and averaged within bins by distance from the border.

brighter than those in the southwest. The bottom panel collapses the raw data to one dimension—distance from the new border—to make the change even clearer. There is little or no change at the border in 1992, but a large change in 2012.

To better understand the timing of this change I estimate the difference-in-discontinuities. Figure 5 illustrates how the estimator works in the case of Specification 2, though the intuition is similar for the other specifications. I group cells into bins by their distance to the new border. For 1992, 2001, and 2012 I plot the average light emitted from each bin and fit lines to these averages.[20] The graphs show that there may be a break at the border in 1992, but it is statistically insignificant and may be sampling error. More important is that the size of the break is almost unchanged in the year of the before the policy—that

[20]The p-values are computed from regressions on the cell-level data, as is standard.

Figure 5
Illustrating the Difference-in-Discontinuities Estimator



Note: The diagrams show a cross-sectional representation of the distance-to-border specification for the years 1992, 2001, and 2012. The p-value gives the significance of the border effect in the cross-sectional regression. Each dot represents the average of the log digital value within a 5 kilometer bin.

is, there are parallel trends at the border. By 2012, however, there is a large and clear discontinuity. The difference between the 2012 discontinuity and the 1992 discontinuity is the difference-in-discontinuities estimate for 2012.

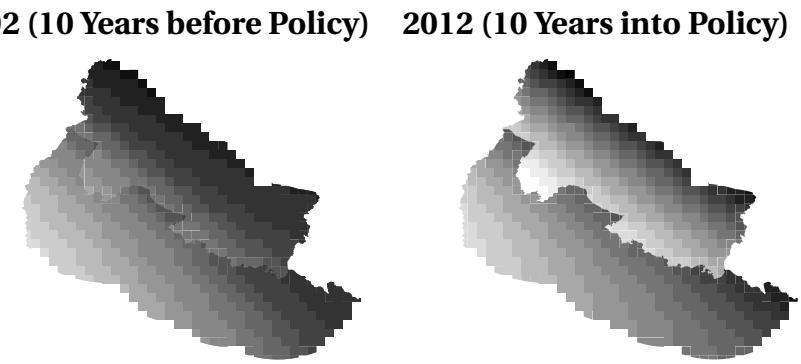
Figure 6 shows the spatial approach. It plots predicted values from a regression similar to Equation 1.[21] In 1992, light radiates out from the southwest (the metropolitan area around Delhi). There is no effect at the as yet undrawn border. Twenty years later the effect is sharp enough to delineate the border.

Figure 7 plots the difference-in-discontinuities estimates by year using each of the three specifications. The dashed line shows the first year of the policy. In no specification is there a statistically significant effect before the policy began. In particular, there is no effect in 2001, the year that the new state formed.

But there is a large increase in light in the first year of the policy—that is, the year in which the tax incentives were announced, the state infrastructure corporation started work, spending on local power and industrial development rose sharply, and Uttarakhand gained access to central power. There is roughly a 22 percent increase in light emissions. Light emissions continue to grow after

[21] The predicted values from Equation 1 for 1992 are uniformly zero by construction. To avoid that uninformative prediction I replace the fixed effect with a dummy for $[New\ State]$, which avoids stripping out all the variation in the predicted value for 1992. I also expand the bandwidth to 50 kilometers to make a larger map.

Figure 6
Predicted Values for Light Emissions: Spatial Polynomial



Note: The map plots predicted values from a variant of Equation 1. The variant controls for a dummy for the new state instead of controlling for cell fixed-effects. This allows me to predict non-zero values for 1992. I also use a wider bandwidth (50 km) to make a larger map.

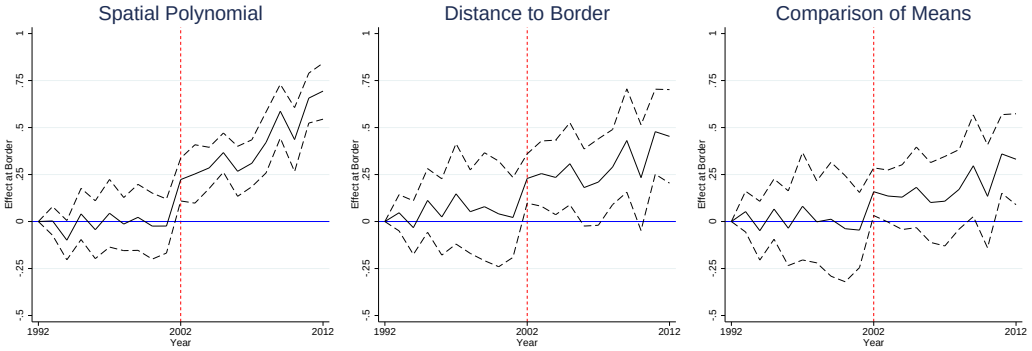
2002, reaching their highest point at the end of the sample.

Table 3 reports the estimates from all three specifications for 2002 and 2012. The effect in 2002 is broadly similar across specifications—a roughly 0.2 log point or 22 percent increase in light intensity. There is more variation in the estimated effect in 2012. The spatial polynomial estimates an increase over baseline of 0.69, the comparison of means an increase of 0.33, with the distance to border specification somewhere in between. But in all three cases the effect is large and significant at the 1 percent level.

In Appendix 1.1 I show that this pattern of results is unchanged when I vary the bandwidth of each specification. I also show that the result is not sensitive to the measure of light intensity used here (the adjusted log of the average digital value of light intensity). The result holds when I use the level of the average digital value.

Is there any evidence that the sharp increase in light emissions in 2002 was caused by an increase in production? Recall from Figure 2 in Section 2.C that the estimated increase in light emissions coincides with a 44 percent increase in Uttarakhand's output in manufacturing. The increase in manufacturing was measured not with light emissions but by standard factor price accounting. The same source suggests there is also a doubling of output in the power, gas, and

Figure 7
Difference-in-Discontinuities Estimates



Note: Plots of the difference-in-discontinuities estimates and 90 percent confidence intervals from Equations 1, 2, and 3. See Section 3.4 for details. The dashed vertical line shows the first year of the policy. The state formed in November of 2000 (1 to 2 years prior).

Table 3
Border Effect by Year

	Spatial Polynomial	Distance to Border	Comparison of Means
2002	0.22***	0.23***	0.16**
	(0.07)	(0.08)	(0.07)
2012	0.69***	0.45***	0.33**
	(0.09)	(0.15)	(0.14)
Cell-Years	6048	6048	1890
Cells	288	288	90
Sub-districts	38	38	26

Note: The table reports the coefficient and standard error of the difference-in-discontinuities estimates for the years 2002 (first year of policy) and 2012 (10 years into policy). These are simply the estimates plotted for those years in Figure 7

water sector. It is impossible to prove that these measures did not all increase in 2002 by coincidence. But at the least, an independent source of data suggests two sectors of Uttarakhand's economy saw sharp increases in output in the same year as light emissions increased.

What could have caused such a sudden increase in production? One possibility is that the increase in 2002 of Uttarakhand's drawal rights—its access to power from central government power plants—triggered an increase manufacturing output. Both Allcott et al. (2014) and Abeberese (2012) find that better access to power raises output and investment in Indian firms. Access to central government power plants cannot, however, explain the sustained growth in the border effects that came after 2002. This growth had to have been some combination of the infrastructure spending and the tax exemptions. Another possibility is that the sudden increase in development spending had a direct effect—say, through construction—as fiscal stimulus generally does (see Ramey, 2011, for one example). The resulting infrastructure may then have enabled the sustained growth of later years.

4.2 Is it Really the Effect of the Place-Based Fiscal Policy?

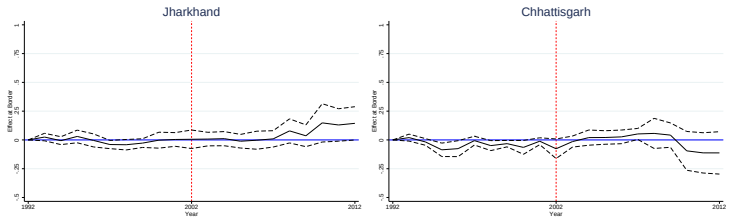
The results of Section 4.1 demonstrate that there are no border effects before the policy and large border effects afterwards. Is it safe to interpret these effects as being caused by the place-based policy?

The most obvious objection is that the creation of the state may itself have had an effect. For example, a smaller state may set policies better suited to local conditions. Though the literature suggests decentralization need not always have benefits, that does not mean it cannot have brought benefits in this case.[22] Figure 7 suggests there is no effect in the year of secession and that effects appear only in the year that the place-based policy began in earnest. Could that simply be a delayed effect of secession?

To test for the effect of secession in the absence of the policy I study the other two states formed in November of 2000. If secession itself has an effect, it should appear in the difference-in-discontinuities estimates of those two states.

[22]See Bardhan and Mookherjee (2006) and Galiani et al. (2008) for evidence that decentralization hurts the interest of the poor, and Faguet (2004) for an example of the opposite result.

Figure 8
No Evidence of a Response in Other New States



Note: The plot is formed analogously to the first panel of Figure 7.

I estimate Equation 1 using Jharkhand and Chhattisgarh. I focus on the spatial polynomial because it gave the largest effects in Section 4.1. It is most likely to detect any effect of secession.

Figure 8 plots the estimates. Like Uttarakhand, neither state shows any effect in the year of secession. But unlike Uttarakhand, neither state shows any effect in the year that the policy starts or even for many years afterwards. The estimates turn positive in Jharkhand many years after the new state forms (though their 90 percent confidence intervals contain zero); but they actually turn negative in Chhattisgarh. In neither state is there a sharp increase like that shown in Figure 7.

Since decentralization is expected to work by bringing government closer to the governed, its effect may also be measured by testing whether areas closer to the new state capital benefitted more. It happens that Dehradun, the new capital, is located on the northwest part of Uttarakhand's border. Since the capital is on the border, this test effectively compares parts of the border close to the capital to those more distant. I run each specification again, but now estimate an interaction between $[Year\ Dummy]_t \times [Targeted]_i$ and a dummy for being far from the new capital. I also control for an interaction between the year dummies and the far dummy; otherwise the estimate will sweep up effects on areas in the control state. I define "far" as being in the top quartile of distance from

Table 4

No Evidence that Distance to Government Drives the Results

	Spatial Polynomial	Distance to Border	Comparison of Means
2002	0.31***	0.21**	0.18*
	(0.09)	(0.08)	(0.09)
2012	0.58***	0.42***	0.30*
	(0.14)	(0.15)	(0.17)
2002, Far	0.02	0.11	-0.03
	(0.14)	(0.13)	(0.12)
2012, Far	0.04	0.19	0.24
	(0.23)	(0.21)	(0.18)
Cell-Years	6048	6048	1890
Cells	288	288	90
Sub-districts	38	38	26

Note: I define "far" as being in the top quartile of distance from the capital of Uttarakhand.

the capital.[23] If decentralization has large effects the coefficient on the interaction between the border effect and the far dummy should be negative and significant, meaning that after the split areas far from the new capital received less benefit than those nearby. Table 4 shows no such pattern. In no case is the interaction statistically significant, and in many cases it is actually positive. There is little to suggest that being closer to the new government brought any benefit. Though this test alone is not definitive, given that there is no effect on the other new states (see Figure 8) it suggests decentralization was not the key factor behind the sharp increase in light emissions in 2002.

Even if decentralization itself cannot account for the effect, could a simple divergence of regulatory policy explain the effects? If the ease of doing business in Uttarakhand greatly improved after it broke away from Uttar Pradesh, it might encourage new business and raise output. I check whether such improvements might be driving the results by consulting the state-level Ease of Doing Business scores calculated by the World Bank (2015).[24] The World Bank, together with representatives of the state governments and the Make In India

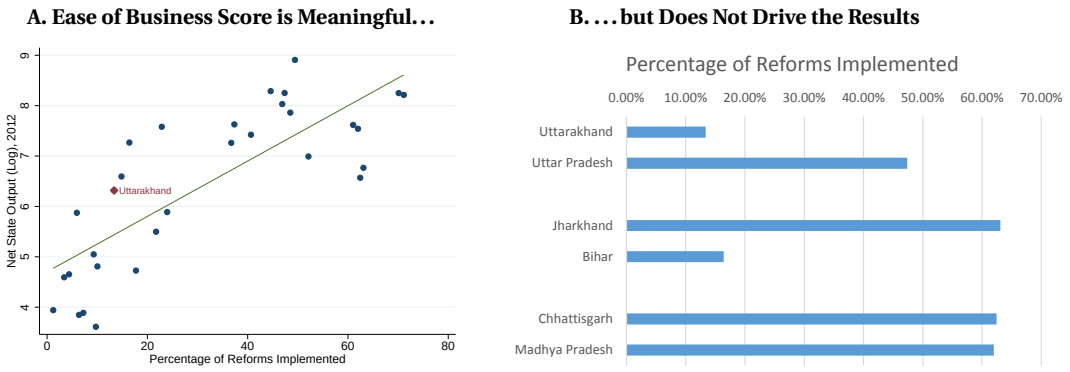
[23]For example, the comparison of means estimator is

$$[Light]_{i,t} = [Fixed\,Effect]_i + \sum_{t=1993}^{2012} \kappa_t [Year\,Dummy]_t + \sum_{t=1993}^{2012} \kappa_t [Year\,Dummy]_t \times [Far]_i$$
$$+ \sum_{t=1993}^{2012} \beta_t^C [Year\,Dummy]_t \times [Targeted]_i + \sum_{t=1993}^{2012} \beta_t^C [Year\,Dummy]_t \times [Targeted]_i \times [Far]_i + [Error]_{i,t}$$

.

[24]See Besley (2015) for a recent review of studies that use the country-level Doing Business indicators.

Figure 9
Firms Respond to the Policy



initiative, agreed to 98 reforms that would make it easier to do business. For each state the World Bank calculated what percentage of these reforms had been made. Figure 9.A shows that these scores are correlated with output, suggesting they are not meaningless.

Figure 9.B shows these percentages for each of the three new states alongside the state from which each was formed. The figure shows that the ease of doing business actually worsened in Uttarakhand relative to Uttar Pradesh. By contrast, Jharkhand improved its score over Bihar and Chhattisgarh remained on par with Madhya Pradesh. Jharkhand's improvement may explain the small increases in output depicted in Figure 8. If so, Uttarakhand's decline in policy suggests Figure 7 actually underestimates the effect of the place-based policy.

Another simple test is to see whether the increase in light is concentrated in areas that most affected by the policy. As noted in Section 2, the state's industrial corporation channeled its spending and restricted the tax benefits to several public and private integrated industrial estates. Figure 10 maps the change in light intensity from 1992 to 2012 and marks the locations of the biggest industrial estates. Most of the increases are concentrated in areas around these estates. Though this does not prove that the increases in light were caused by increased production in the industrial estates, it is consistent with that interpretation.

Even though the increase in output was concentrated in industrial estates,

Figure 10
Light Emissions in Uttarakhand Increased
at the Sites of Major Industrial Estates

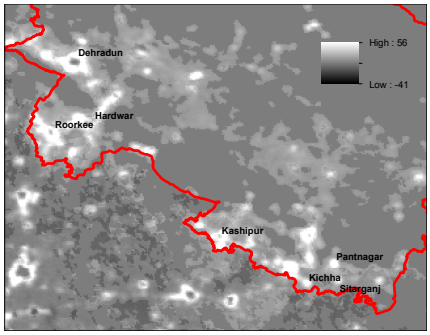
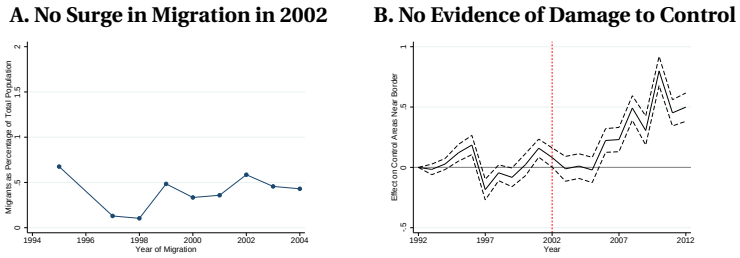


Figure 11
Ruling out Other Explanations



there remain two other potential confounders. The first is migration. By itself migration is not a problem; a simple Roy model predicts that there should be migration to the targeted state after the policy increases labor productivity. I show evidence of such migration in Section 4.3. The concern is that people migrated in 2002 for reasons unrelated to the policy, and that their presence increased output. Panel A of Figure 11 uses the Integrated Household Development Survey to measure the percentage of the population living outside its place of birth by year of arrival. The figure shows that migration in 2002 accounted for 0.6 percent of the population, as compared to 0.4 percent in 2001. It is hard to imagine such a small number of migrants could drive the large effect shown in Figure 7.

The second question is whether the policy raised output in the targeted state or simply lowered output in the untargeted state. The difference-in-discontinuities estimates give only the relative increase in light. If tax breaks and new infrastructure drive firms to shift their production across the border, the regions on the other side of the border may be hurt. In other words, one may worry that the Stable Unit Treatment Value Assumption is violated. One could argue that since the aim of the program is to reduce regional inequality the relative difference is all that matters. But since regions on either side of the border are similar, damaging control regions may actually increase regional inequality.

Panel B of Figure 11 plots light intensity in regions of Uttar Pradesh, the untargeted state, within 10 kilometers of the border. Far from being damaged, the figure suggests these regions have grown rapidly since the start of the policy. This result is not driven by overglow from the treated state, as the figure looks identical if I exclude areas directly adjacent to the border (see Appendix 1.3). Finally, I show in Appendix 1.3 that there is no evidence of a decrease in the number of factories in the control state in the wake of the policy.

The absence of harm is not entirely surprising. According to the 2005 World Bank Enterprise survey, 80 percent of surveyed firms reported that they produce in their chosen state because the owners are from that state. Many of the new factories in Uttarakhand may have been registered by local entrepreneurs who would otherwise have never entered or would have kept their firms informal. To the extent that firms shifted production, they would have shifted away from major centers like Mumbai or Ahmedabad rather than the underdeveloped parts of Uttar Pradesh near its border with Uttarakhand. Since there is trade across state borders the rise in production in targeted areas may actually have helped untargeted areas. Though it is impossible to say for certain what would have happened in the absence of the policy, the figure makes it seem unlikely that the results of Figure 7 are driven by a failure of the Stable Unit Treatment Value Assumption.[25]

[25]One other concern is that the increase in light was largely political. Baskaran et al. (2015) argue that electricity is used by state governments just before elections to sway votes. Could the sharp increase in 2002 really be caused by the electoral cycle rather than actual investment in power generation? Though Uttarakhand did have a state assembly election in 2002, the election was in February. Given that the election was so early in the year it seems more likely the political effects on power would have appeared the previous year. Moreover, a surge in power driven by elections would have petered out after the election. Figure 7 suggests the increase in lights was

4.3 Impact on Economic Development

To quantify the benefit caused by the change in policy I rescale light intensity to predict changes in output and household welfare. To compute the rescaling factor I regress changes in each outcome on changes in light intensity in separate datasets where both are measured.

The first outcome is output. Though Henderson et al. (2012) estimate the correlation between night lights and output in a cross-country dataset, to make more accurate predictions I measure a sub-national India-specific correlation in a dataset of Indian states. According to Henderson et al. (2012), night lights are a valid measure of changes in output, not levels of output. Since the difference-in-discontinties estimator controls for fixed effects, it effectively gives changes in light caused by secession. I estimate the correlation between changes in light and changes in net state product relative their means—that is, controlling for state fixed effects.[26]

The left panel of Figure 12 shows a scatter plot of output against light intensity after controlling for state fixed effects. A 1 percent increase in light intensity predicts a 0.76 percent increase in output. Night lights explain nearly half of the residual variation in output.

But a strong correlation between output and light at the level of the state may break down at the level of the cell, which is the unit of observation in the spatial regression discontinuity. Since at finer levels I cannot directly measure output, I instead study several Census proxies for welfare at the level of the subdistrict.[27] Using a panel of subdistricts I measure the correlation between changes in light and four proxies for welfare: the fraction of households with power, the fraction with latrines, the fraction living in homes with a solid roof, and the fraction urban.[28] I regress the change in each of these proxies on the

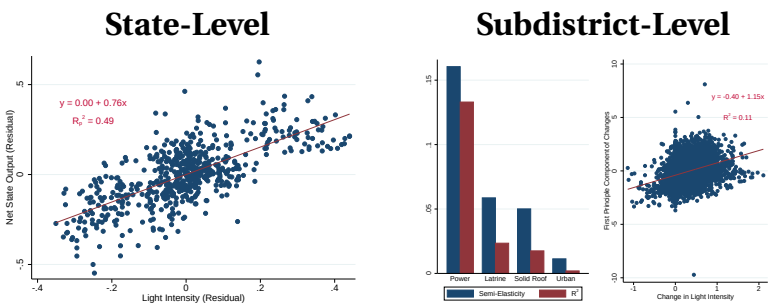
permanent and only increased over time.

[26]The Indian Ministry of Statistics and Programme Implementation periodically changes how it computes output and to my knowledge does not or cannot update earlier figures to be consistent. The Ministry uses data from the Indian Census and the National Survey Sample to estimate both national accounts and gross state product. The different "base years" reflect not only changes in the price index but changes in the Ministry's estimates of the labor force and of employment in each industry. Rather than do arbitrary adjustments to make the numbers consistent I include dummies to control for changes in the base years, allowing the dummies to differ by state. The regression is $[Output]_{st} = \alpha_s + \sum^s \sum^b \gamma_{bs}[Base\,Year]_b[State]_s + \eta \log(DV+1) + \varepsilon$.

[27]In Uttar Pradesh the average sub-district contains roughly 8 cells.

[28]A solid roof—which I define as one made of tile, slate, metal sheet, brick, stone, or

Figure 12
Light Predicts Output and Development



change in light intensity.

The right panel of Figure 12 shows the results. For each proxy the bar graph shows the coefficient and the R^2 of the regression. The coefficients can be interpretted as the percentage point change predicted by a 1 percent increase in light intensity. In all cases the coefficient is significant at the 1 percent level. (See Table 9 in Appendix 1.1 for the estimates.) It is no surprise that light is most strongly correlated with the fraction of households who have power. It is important to note, however, that this does not simply mean households who had power are using more. The measure is extensive. Households were actually connected to the grid, suggesting new infrastructure was built.

Beyond electricity, an increase in light also predicts an increase in the fraction of households who have latrines and solid roofs. There is a small (but precisely estimated) correlation between urbanization and light. However, the estimate makes it clear that light is not simply a proxy for cities. This result alleviates the concern that night lights are a poor measure of welfare in rural areas. Indeed, when I re-estimate the correlation between light and the other variables among subdistricts with no urban population the correlations become even stronger (see Appendix 1.1). I summarize these correlations by extracting the first principle component of the changes in all four measures. The scatter plot in Figure 12 shows that this summary measure is strongly correlated with changes in light.

concrete—is one measure of wealth in poor countries.

Table 5
Increase in Output and Welfare

	Spatial Polynomial		Distance to Border		Comparison of Means	
	2002	2012	2002	2012	2002	2012
Output	18.2%	68.9%	19.1%	40.8%	12.9%	28.5%
Percentage of households with						
Power	3.61	11.75	3.77	7.51	2.61	5.46
Latrines	1.31	4.16	1.37	2.69	0.95	1.97
Solid roofs	1.11	3.51	1.16	2.28	0.80	1.66
Urban residence	0.24	0.76	0.25	0.50	0.18	0.36

Note: Effects on output are the percent increase. All other effects are the percentage point increase.

Table 5 uses these correlations to rescale Table 3 into predicted effects on several proxies for household welfare. I multiply each estimate by the correlation between light and output. I make a similar calculation using the correlation between light and the four proxies for household welfare.[29] Depending on which estimate is used, the immediate effect of the program was to raise output by 13 to 19 percent. By 2012, the program had raised output by between 28.5 and 69 percent. The percentage of households with access to power rose by between 5.5 and 12 percentage points; the percentage with a latrine and a solid roof rose by between 1.5 and 4 percentage points.

The estimates in Table 5 are indirect, in that they use the effect on light to infer the effect on welfare. Can they be trusted? To validate them I study the growth in access to power, latrines, and a solid roof from 2001 to 2011 within towns, the finest level at which they are reported in the Indian Census. Since there are so few towns I must expand the bandwidth of the spatial polynomial and the distance to border specifications to 50 kilometers to have any statistical power.

Table 6 shows that the estimates are broadly similar. If anything the direct estimates are larger than the indirect estimates, especially those for the percentage of households with access to power. This may be because the correlation between light and access to power is higher in Uttarakhand than in the rest of India; or it may be that the benefits of the policy accrued disproportionately to

[29]For the effects on output I also convert the log point effect to a percentage effect by exponentiating and subtracting 1.

Table 6
Directly Measured Household
Welfare Improves in Towns

	Spatial Polynomial			Distance to Border			Comparison of Means		
	(1) Power	(2) Latrine	(3) Solid Roof	(4) Power	(5) Latrine	(6) Solid Roof	(7) Power	(8) Latrine	(9) Solid Roof
Estimate	0.15***	0.05**	0.04**	0.17***	0.05**	0.05*	0.20***	0.03	0.05
	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.05)	(0.03)	(0.05)
Towns	129	129	129	129	129	129	14	14	14
Sub-districts	46	46	46	46	46	46	7	7	7
Control Mean	-0.00	0.03	0.08	-0.00	0.03	0.08	-0.00	0.03	0.08

Note: These regressions use each of the three specifications to measure the change from 2001 to 2011 in the fractions of households in each town that have power, a latrine, and a solid roof. The bandwidth for the spatial polynomial and the distance to border is 50 kilometers. The bandwidth for the comparison of means is 4 kilometers. The row labeled "control mean" gives the average change from 2001 to 2011 in towns within 50 kilometers of the border in Uttar Pradesh.

towns. Of course these estimates should be taken with some caution, as they are made using very few observations (especially the comparison of means). Nevertheless they are an independent confirmation of the inferred welfare effects reported in Table 5.

Though I have argued nighttime lights are not merely a measure of power and not merely a measure of urban welfare, the reader may still wonder if other measures of welfare improved in rural areas. To measure rural welfare I turn to the village tables from the Indian Census. The village data raise two challenges. The first is that villages tend naturally to cluster according to the terrain. This makes it hard to capture smooth variation in trends with the spatial polynomial and the linear distance to the border. Rather than try to find the right functional form I focus on the comparison of means estimator, which is relatively conservative and does not require any assumption about the shape of the smooth variation. I show in Appendix 1.2 that the other estimators yield similar results. The other challenge is that, unlike nighttime lights, the village outcomes are observed only in the years of the census. Thus I can only report how the growth of these outcomes from 2001 to 2011 changes at the border. There is no way to tell whether the effects appear immediately after the policy starts, only that they appear in its wake.

These caveats aside, Table 7 shows the comparison of means estimates for

Table 7
Village Outcomes

	(1) School	(2) Health Center	(3) Workers	(4) Emp. Rate	(5) Population	(6) Agr. Share	(7) Farm Laborer	(8) Cultivators
Estimate	0.15**	0.16***	91.29**	0.03	184.91***	0.03***	0.01**	0.02**
	(0.06)	(0.04)	(35.28)	(0.02)	(54.41)	(0.01)	(0.00)	(0.01)
Villages	1007	1007	1007	1007	1007	1007	1007	1007
Sub-districts	23	23	23	23	23	23	23	23
Control Mean	0.02	0.00	65.42	-0.01	213.42	-0.02	0.01	-0.03

Note: These regressions use the comparison of means estimator to measure the change from 2001 to 2011 in the level of each of several outcomes in villages. "School" and "Health Center" are indicators for whether the village has a primary school and a primary health center or subcenter. "Workers" is the number of people who worked for at least 6 months. "Emp. Rate" is the number of workers divided by total population. "Agr. Share" is the number main workers in agriculture divided by total population. Columns 7 and 8 break the share working in agriculture into the share working as cultivators (farm owners) versus farm laborers.

several measures of economic development. The bottom row labeled "Control Mean" shows the average change from 2001 to 2011 in villages within 4 kilometers of the border in the untargeted state. Columns 1 and 2 show that whereas the change in the fraction of villages that have a primary school or health center was effectively flat in control regions, it rose by roughly 15 percentage points in treated villages. It is hard to tell whether the construction was financed directly from the fiscal program or if the new industrial activity indirectly raised state tax revenue. But either way it shows the government did not ignore rural development.

Columns 3–8 measure how employment and the structure of the rural economy changed in treated villages. Columns 4 and 5 show that though the number of workers increased, the employment rate did not (or at least the increase was too noisy to detect). This may be because the best measure of employment available from the Census—number of "main" workers (those who spend most of the year working) divided by total population—cannot measure whether the underemployed are now fully employed. It may also mean that the new workers are migrants. Column 5 shows that the population in treated villages rose by nearly twice as much as in control villages. I show in Appendix 1.3 that this influx coincides with a depopulation of villages further to the interior of Uttarakhand, suggesting people have moved to villages closer to the new industrial estates.

Yet it does not appear that workers are moving into industry. Column 6

shows that the fraction of the population employed in agriculture actually rose relative to control areas (or rather, stayed constant while the share fell in untreated areas). This may be because the tax exemptions granted to Uttarakhand gave special priority to certain forms of agriculture. Column 7 shows a rise in the fraction of the population working as agricultural laborers (those who work on other people's farms).

But there is also a rise in the fraction of cultivators, defined as those who farm their own land. Given that there is a decrease in control areas it seems that either cultivators moved from control to treated villages, or people who would have left farming in the absence of the policy were induced to stay or were replaced by migrants from the interior of the state. Since these cultivators likely run farms too small to have paid any tax, the change is more likely a general equilibrium effect. The rise in output in the industrial estates created more demand for food, which induced farmers to expand their fields and hire workers to till them. This last result should be treated with caution, however, as the effect on the fraction of cultivators is not robust across specifications (see Appendix 1.1).

4.4 Cost-Benefit Calculation

Taking as given the rescaled impact on output, was the program cost effective? A rough estimate puts the cost of the program at about 33 billion dollars at 2005 purchasing power parity.[30] The total benefit from 2002 through 2012 depends on the estimator, ranging from 52.3 billion (spatial polynomial) to 36.6 billion (distance to border) to 21.9 billion (comparison of means). This suggests that every dollar spent yielded between 0.65 and 1.55 dollars of benefit.

On first glance, the low end of these estimates may make the program seem a failure. For example, Kline and Moretti (2014a) estimate that each dollar spent on the Tennessee Valley Authority program in the U.S. brought 1.38 dollars of benefit. And though Busso et al. (2013) are unable to reject that the federal urban Empowerment Zone program had no benefit, their point estimates imply 1.88 dollars of benefit for each dollar spent.

[30] It is not trivial to calculate the cost of the program, largely because differences in statutory and actual corporate taxes make it hard to estimate how much tax revenue the central government. I give the details of my calculations in Online Appendix C.

But these programs from the developed world are arguably not the right comparison. Rather, the program should be compared to transfer programs in developing countries, which are often riddled with corruption and mismanagement. Reinikka and Svensson (2004) find that for every dollar of grants made from the Ugandan government to schools, only 13 cents reached their recipients. Olken (2007) finds for every dollar spent in Indonesia on road construction, 24 cents are lost to corruption alone. The figure for Indonesia's rice subsidy program is 18 cents (Olken, 2006). One strength of India's program is that most of the "expenditure" was through tax exemptions, which cannot be stolen.

Moreover, the figures for the Ugandan and Indonesian programs reflect only the amount lost to corruption. The actual impact on welfare or income may be even lower. For example, Park et al. (2002) estimate that every dollar spent on China's massive regional development program raised income by between 11 and 16 cents. By comparison, even the most conservative estimates for India's program suggest it was a success.

4.5 Impact on Agglomeration

As noted in the introduction, one aim of place-based policies—especially temporary tax breaks—is to create new centers of agglomeration. If the concentration of people directly raises the productivity of firms, as in Ciccone and Hall (1996) or Combes et al. (2012), then the migration triggered by such policies might raise population density and thus productivity. Most of the literature on agglomeration has assumed productivity is isoelastic in population density, and has aimed to measure the elasticity (Rosenthal and Strange, 2004). To leverage these elasticities I estimate the increase in log population density caused by the program.

I apply the comparison of means estimator to a pooled sample of towns and villages, weighting each town or village by its 2001 population. The coefficient may be interpreted as the population density experienced by the average person, which can then be scaled to the agglomeration effect on the average person.[31] Colum 1 of Table 8 shows that the program raised population density by

[31]To be precise, suppose there is a set of settlements (towns or villages) $i = 1, \ldots, I$. Each settlement has population L_i and labor-augmenting productivity A_i. If the aggregate capital

Table 8
Agglomeration: Population vs. Human Capital

	(1) Density	(2) Literacy
Estimate	0.29***	-0.02
	(0.08)	(0.01)
Towns/Villages	1010	1010
Sub-districts	23	23
Control Mean	0.09	0.13

Note: Regressions use the comparison of means specification, pooling village and town data. Observations are weighted by population. The density is in logs and literacy is in percentage points of the total population.

an extra 0.29 log points.

According to the literature, a doubling of population density raises productivity by between 3 and 8 percent (Rosenthal and Strange, 2004). Even using the highest of these numbers, agglomeration raised the productivity of the average person by just 3.2 percent. Migration also directly increased the labor force of these productive border regions. The population of treated subdistricts along the border rose by an extra 5.5 percent relative to control subdistricts across the border. Assuming aggregate output is a Cobb-Douglas function with labor-augmenting productivity, the new equilibrium of a Solow economy would be 8.7 percent higher after capital adjusts. Though not trivial, this increase accounts for less than 31 percent of the total increase in output measured by the most conservative specification of Section 4.3. Though these are only back-of-the-envelope calculations, they suggest the program did not create huge gains from population agglomeration.

Other work—for example, Glaeser et al. (2004),Moretti (2004), and Shapiro (2006)—suggest it is not simply the concentration of people but educated people that creates agglomeration economies. But Column 2 of Table 8, which

stock is K, output is $Y = K^{\alpha}(\sum_i A_i L_i)^{1-\alpha}$. Define aggregate labor $L = \sum_i L_i$ and aggregate productivity as $A = \sum_i \frac{L_i}{L} A_i$. Note that aggregate productivity is simply the weighted average of each settlement's productivity, where the weights are precisely the population weights used in the weighted regression. Then $Y = K^{\alpha}(AL)^{1-\alpha}$. Note that $\log A \approx \sum_i \frac{L_i}{L} \log A_i$. Then the change in this weighted sum at the border is a sufficient statistic for the change in aggregate productivity caused by agglomeration. This change is the coefficient estimated by the weighted regression.

measures the effect of the program on the literacy rate, suggests that if anything the program concentrated less educated workers in treated areas. This is not surprising, as much of the new work created was in low skill manufacturing or—as shown in Section 4.3—farming. Though there may be other forms of agglomeration that raised productivity, these two—population and human capital agglomeration—have received much of the attention in the literature. Even when counting the increase in total labor, these two only account for a small part of the rise in output. This result is consistent with what Kline and Moretti (2014a) find in areas affected by the Tennessee Valley Authority. Their structural model suggests the bulk of the impact is the direct effect of better infrastructure.

Another branch of theory proposes that agglomeration works by increasing local demand rather than local productivity. In the models of Murphy and Shleifer (1989) and Krugman (1991), a one-time push towards industrialization expands the market for manufactured goods, which in turn spurs further industrialization. For these models to apply it must be that firms sell their products locally, either to households or to one another. But according to the 2014 World Bank Enterprise Survey, it takes more than 6 days for the average Uttarakhandi firm to ship its goods to the final customer, roughly the same as the average Indian firm. That makes it unlikely firms are selling their goods in the town where it is produced, and thus unlikely that growing the local market would affect a firm's decision to invest. Based on this evidence it seems unlikely that theories of a big push can explain the success of the program.

5 Conclusion

Using a spatial difference-in-discontinuities estimator I show that a generous place-based program had large effects on regional output and proxies for household welfare. I find that only a small part of the effect is explained by agglomeration economies, suggesting much of the effect comes directly from the tax breaks and improvements in infrastructure.

One caveat to this result is that, by design, the results are only informative about regional development. If the government's objective is to reduce regional

inequality the results suggest a place-based program may be effective. It is less clear whether place-based programs bring net benefits to the country as a whole. Though I show that the results are not driven by damage to control regions—regions just outside the targeted state—that does not mean there was no damage to regions outside the study sample. For example, the program may have convinced firms to open factories in Uttarakhand rather than Mumbai. Measuring the net cost or benefit of such programs—and more broadly, answering whether targeting benefits at one region is an effective way to grow the entire country—is a project left to future research.

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A Empirical Appendix (For Online Publication)

1.1 Additional Checks

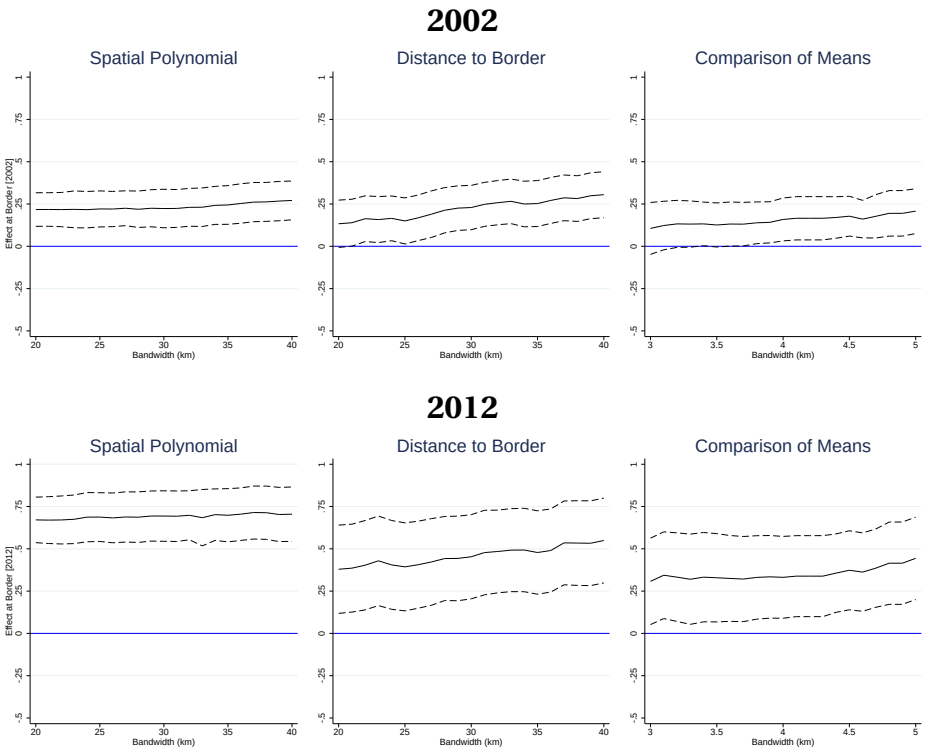
This appendix shows additional checks referenced in the text.

Figure 13 shows the effect at the border for both 2002 and 2012 for different choices of bandwidth. The estimates of the spatial polynomial approach are almost unchanged at all bandwidths, suggesting the third-order polynomial is able to capture the shape of the data. The comparison of means estimate for 2002 gets marginally smaller at narrower bandwidths, but the difference is small. The distance to border estimate becomes smaller close to the border, suggesting there may be some nonlinearity in the relation between distance and light intensity. Nevertheless it remains significant except at the very narrowest bandwidths in 2002, and at all bandwidths in 2012. Nearly all of the estimates for 2002 lie within the 90 percent confidence intervals of one another, suggesting that all specifications and all bandwidths tell a similar story. In 2012 the spatial polynomial estimator gives consistently higher results, exactly as it does in the main text. In no case does the choice of bandwidth change the qualitative results.

Figure 14 shows that the pattern of results—no effect before 2002 and an immediate effect in 2002 that continues to grow—holds when the outcome is the level of the average digital value (light intensity) rather than $\log(DV + 1)$.

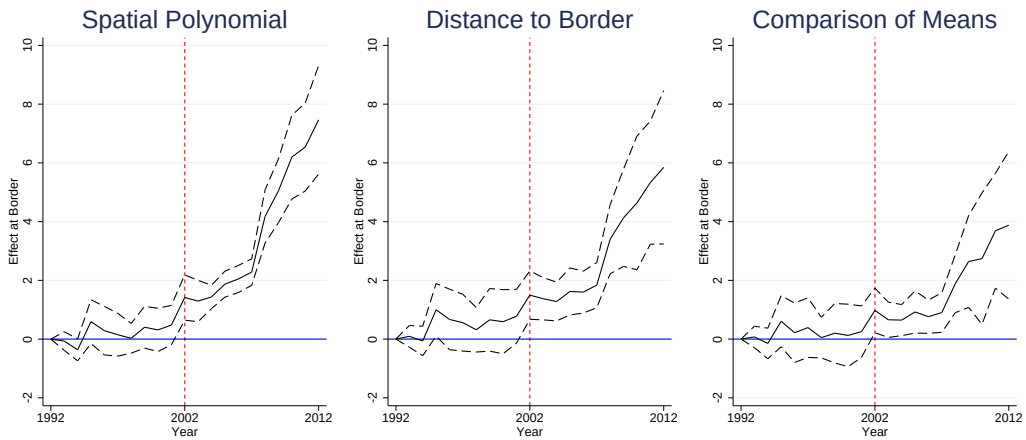
Table 9 shows the correlation between changes in light intensity and changes

Figure 13
Robustness: Varying the Bandwidth



Note: Each point gives the estimate and 90 percent confidence interval for either 2002 or 2012 using the specified bandwidth. The coefficients are estimated using Equation 1, 2, and 3.

Figure 14
Difference-in-Discontinuities Estimates



in several measures of welfare. The unit of observation is the sub-district. I restrict the sample to sub-districts whose boundaries did not change from 2001 to 2011.

The top panel uses all sub-districts that fit these criterial. The middle panel uses only sub-districts with no rural population. This middle panel shows that changes in light intensity predict improved welfare even in sub-districts without cities. The bottom panel compares two measures of light intensity, one that adds 1 before taking logs and one that does not. The measure that adds 1 is the measure I use for my analysis in the main text. The results show that the measure that adds 1 actually wins the "horse race" of regressing the outcomes on both measures together. This suggests it is safe to use that as my measure of output and welfare.

Tables 10, 11, and 12 show the village-level regressions of Section 4.3 using all three specifications. The results are broadly similar across all three specifications. The one exception is the change in the fraction of the population working as cultivators, which is statistically significant only in the comparison of means.

Table 9
Additional Regressions: Light and Welfare

	$\Delta Power$	$\Delta Latrine$	$\Delta Urban$	$\Delta Solid\ Roof$	$\Delta Combined$
Full Sample:					
$\Delta \log(DV+1)$	0.161***	0.059***	0.011***	0.050***	1.155***
	(0.006)	(0.006)	(0.003)	(0.006)	(0.056)
Subdistricts	4544	4544	4544	4544	4544
R^2	0.13	0.02	0.00	0.02	0.11
Rural Sample:					
	$\Delta Power$	$\Delta Latrine$	$\Delta Urban$	$\Delta Solid\ Roof$	$\Delta Combined$
$\Delta \log(DV+1)$	0.171***	0.090***	0.000	0.067***	1.336***
	(0.011)	(0.010)	(.)	(0.010)	(0.089)
Subdistricts	1906	1906	1906	1906	1906
R^2	0.12	0.05	.	0.03	0.13
Horse Race:					
	$\Delta Power$	$\Delta Latrine$	$\Delta Urban$	$\Delta Solid\ Roof$	$\Delta Combined$
$\Delta \log(DV+1)$	0.200***	0.107***	0.030***	0.043***	1.464***
	(0.010)	(0.010)	(0.006)	(0.011)	(0.090)
$\Delta \log(DV)$	-0.021***	-0.037***	-0.012***	0.007	-0.177***
	(0.005)	(0.005)	(0.002)	(0.005)	(0.041)
Subdistricts	4107	4107	4107	4107	4107
R^2	0.15	0.03	0.01	0.02	0.12

Note: Effects are in percentage points. Standard errors are robust to heteroskedasticity.

Table 10
Public Goods are Built in Villages

	Spatial Polynomial		Distance to Border		Comparison of Means	
	(1) School	(2) Health Center	(3) School	(4) Health Center	(5) School	(6) Health Center
Estimate	0.14***	0.11*	0.19***	0.09	0.15**	0.16***
	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.04)
Towns	7628	7628	7628	7628	1007	1007
Sub-districts	44	44	44	44	23	23
Control Mean	-0.03	-0.01	-0.03	-0.01	-0.03	-0.01

Note: See Table 7 in the main text.

Table 11
Jobs are Created; Migrants Move in to Fill Them

	Spatial Polynomial			Distance to Border			Comparison of Means		
	(1) Workers	(2) Emp. Rate	(3) Population	(4) Workers	(5) Emp. Rate	(6) Population	(7) Workers	(8) Emp. Rate	(9) Population
Estimate	91.89***	0.01	235.90***	123.17***	0.04	240.41***	91.29**	0.03	184.91***
	(27.24)	(0.02)	(48.20)	(37.66)	(0.03)	(62.45)	(35.28)	(0.02)	(54.41)
Towns	7628	7628	7628	7628	7628	7628	1007	1007	1007
Sub-districts	44	44	44	44	44	44	23	23	23
Control Mean	84.17	0.00	269.09	84.17	0.00	269.09	84.17	0.00	269.09

Note: See Table 7 in the main text.

Table 12
Villages Became *Less* Industrialized

	Spatial Polynomial			Distance to Border			Comparison of Means		
	(1) Agr. Share	(2) Farm Laborer	(3) Cultivators	(4) Agr. Share	(5) Farm Laborer	(6) Cultivators	(7) Agr. Share	(8) Farm Laborer	(9) Cultivators
Estimate	0.02*	0.01**	0.00	0.02**	0.01*	0.01	0.03***	0.01**	0.02**
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Towns	7628	7628	7628	7628	7628	7628	1007	1007	1007
Sub-districts	44	44	44	44	44	44	23	23	23
Control Mean	-0.01	0.01	-0.03	-0.01	0.01	-0.03	-0.01	0.01	-0.03

Note: See Table 7 in the main text.

Table 13
Difference-in-Differences Estimates are Biased by Pre-Trends

	Fake 1996 Policy		
	Distance < 100	Distance < 20	Distance < 4
Placebo Split	0.240***	0.146**	-0.022
	(0.029)	(0.065)	(0.093)
Subdistricts	98	29	26
Cells	781	217	90
Observations	7810	2170	900
	Differential Trends		
	Distance < 100	Distance < 20	Distance < 4
Pre-Trend	0.045***	0.026**	-0.005
	(0.005)	(0.011)	(0.015)
Subdistricts	98	29	26
Cells	781	217	90
Observations	7810	2170	900

See Table 1

1.2 Bootstrapped Standard Errors: Comparison of Means

This appendix replicates the most important tables of the main text using the nonparametric bootstrap to compute standard errors for the comparison of means estimator. The bootstrap resamples at the level of the subdistrict.

1.3 Additional Figures

This appendix shows additional figures referenced in the main text. Figure 15 is similar to Figure **??** in the main text except it shows averages for regions between 6 and 12 kilometers from the border. Discarding control regions too close to the

Table 14
Pre-Trends in Village Outcomes Shrink Near the Border

Distance < 40

	Population	Workers	Under 7	Work in Farming	Literacy	Health Center	School	Paved Road
In Uttarkhand	-191.93***	-17.90***	-37.99***	-0.82	-0.03***	0.00	-0.06***	-0.11***
	(42.94)	(6.50)	(7.58)	(6.21)	(0.01)	(0.02)	(0.02)	(0.04)
Villages	10596	10596	10596	10596	10596	10596	10596	10596
Sub-districts	54	54	54	54	54	54	54	54
Control Mean in 1991	1132.95	330.57	247.38	275.41	0.26	0.11	0.63	0.60

Distance < 4

	Population	Workers	Under 7	Work in Farming	Literacy	Health Center	School	Paved Road
In Uttarkhand	69.52*	3.05	15.53	-6.14	-0.01	-0.04	-0.01	-0.03
	(37.84)	(9.19)	(9.90)	(11.47)	(0.02)	(0.03)	(0.05)	(0.09)
Villages	1012	1012	1012	1012	1012	1012	1012	1012
Sub-districts	23	23	23	23	23	23	23	23
Control Mean in 1991	959.27	281.64	204.05	240.31	0.31	0.11	0.56	0.60

Note: See Table 2

Table 15
Border Effect by Year

	Spatial Polynomial	Distance to Border	Comparison of Means
2002	0.22***	0.23***	0.16**
	(0.07)	(0.08)	(0.08)
2012	0.69***	0.45***	0.33**
	(0.09)	(0.15)	(0.14)
Cell-Years	6048	6048	1890
Cells	288	288	90
Sub-districts	38	38	26

Note: See Table 3

Table 16
Directly Measured Household Welfare Improves in Towns

	Spatial Polynomial			Distance to Border			Comparison of Means		
	(1) Power	(2) Latrine	(3) Solid Roof	(4) Power	(5) Latrine	(6) Solid Roof	(7) Power	(8) Latrine	(9) Solid Roof
Estimate	0.15***	0.05**	0.04**	0.17***	0.05**	0.05*	0.20***	0.03	0.05
	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.06)	(0.03)	(0.05)
Towns	129	129	129	129	129	129	14	14	14
Sub-districts	46	46	46	46	46	46	7	7	7
Control Mean	-0.00	0.03	0.08	-0.00	0.03	0.08	-0.00	0.03	0.08

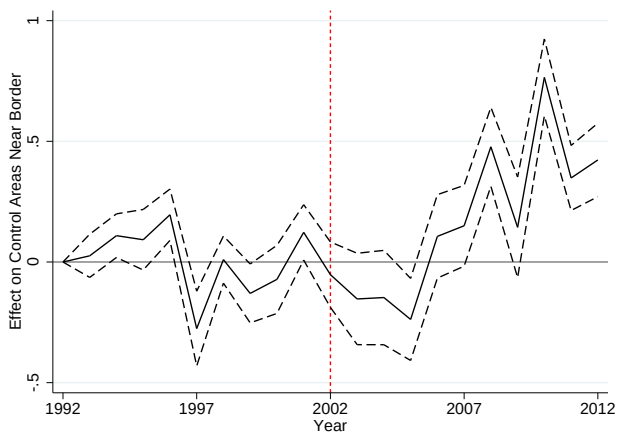
Note: See Table 6

Table 17
Village Outcomes

	(1) School	(2) Health Center	(3) Workers	(4) Emp. Rate	(5) Population	(6) Agr. Share	(7) Farm Laborer	(8) Cultivators
Estimate	0.15**	0.16***	91.29**	0.03	184.91***	0.03***	0.01**	0.02**
	(0.06)	(0.04)	(38.34)	(0.03)	(59.43)	(0.01)	(0.01)	(0.01)
Villages	1007	1007	1007	1007	1007	1007	1007	1007
Sub-districts	23	23	23	23	23	23	23	23
Control Mean	0.02	0.00	65.42	-0.01	213.42	-0.02	0.01	-0.03

Note: See Table 7

Figure 15
The Policy Did Not Harm Control Regions

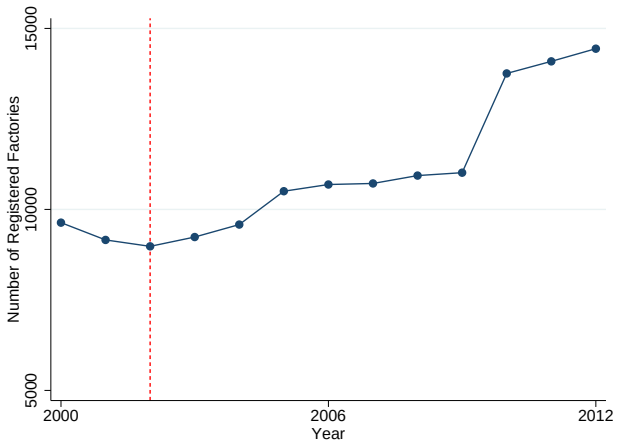


border ensures the estimates are not driven by overglow from the treated state. Even excluding areas just adjacent to the border, light emissions in the control state increased. There is no evidence that the control state was harmed by the policy.

Figure 16 shows the number of registered factories in the control state. There is no evidence that the number of factories decreased because of the policy.

Figure 17 shows villages that experienced positive versus negative population growth (relative to the average). The darker regions are in Uttar Pradesh; the brighter regions are in Uttarakhand. Villages marked in blue had relative increases in population; villages in red had relative decreases. The size of the

Figure 16
The Policy Did Not Reduce the Number of Factories in the Control State



marker is proportional to the absolute change in population, meaning a larger village had a bigger increase or decrease. The figure shows that villages away from the border (and thus away from the industrial estates) were depopulated even as places closer to the border saw influxes of people. (The empty areas are either cities or national parks.) This graph confirms there was an influx of migrants into the border region of the treated state. But it shows that though there may have been some migration from the control state into the treatment state, the major source of migrants was the interior of the treated state. People moved from villages in the mountains to villages close to the new industrial estates.

B Inference in the Difference-in-Discontinuities Estimator (For Online Publication)

This appendix describes the simulation used to establish that clustering standard errors at the sub-district gives the most accurate standard errors. I construct a dataset of cell-level nighttime lights for the Indian state of Maharashtra, which has had stable borders for the entire sample period. Since Maharashtra is

Figure 17
The Policy Triggered Migration towards the Industrial Estates

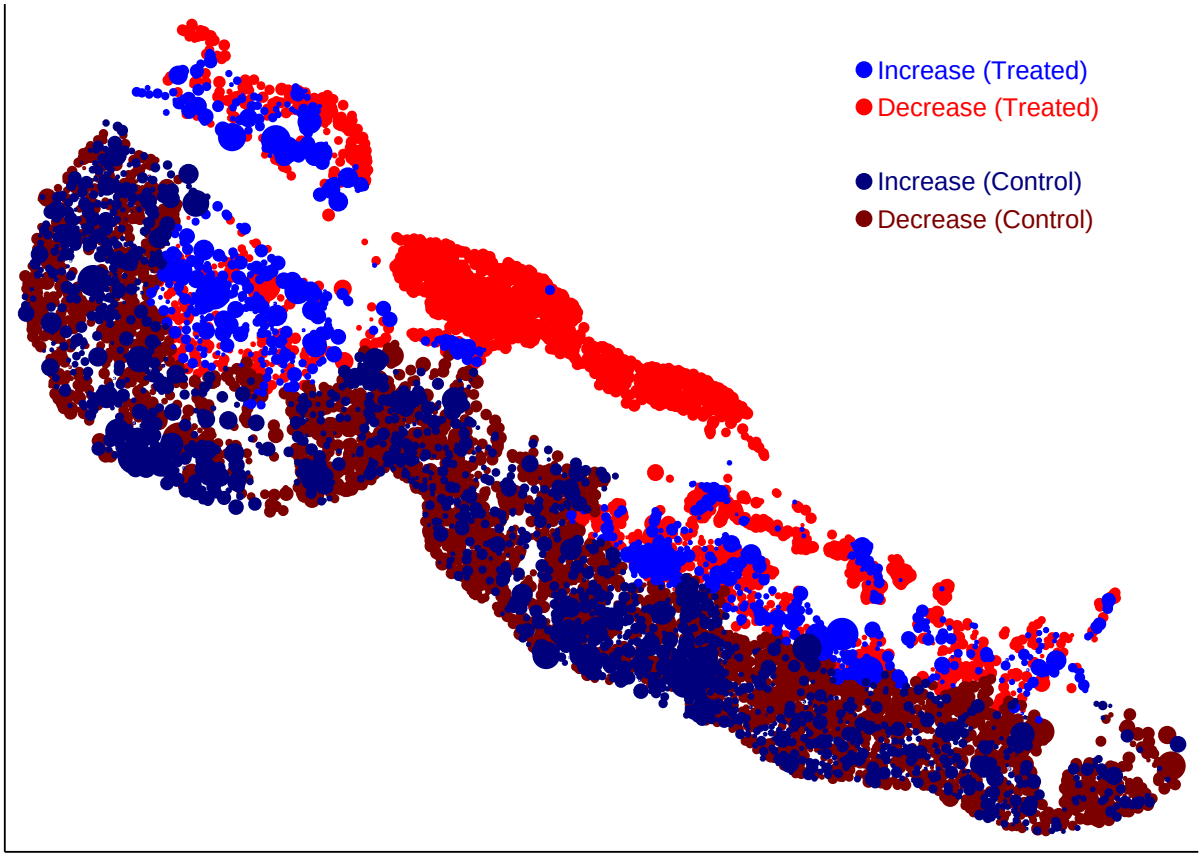
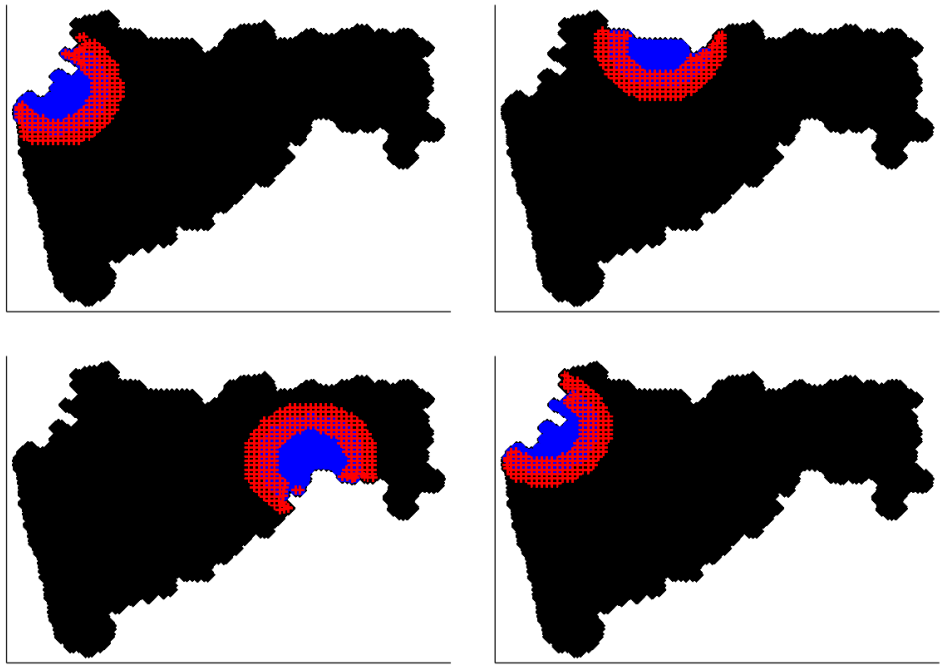


Figure 18
Example "States" Generated by the Simulation



Note: Each panel shows an example of the treated and control region of a single simulation. The black region shows the control region. The blue circle is the "state" that is generated and marked as having received the treatment. The red cross hatch shows the study area, which is all parts of the treatment and control state within 30 kilometers of the fake border.

a single state, there cannot be any state-level intervention within Maharashtra.

The simulation draws random borders within Maharashtra and tests for a difference-in-discontinuities after a hypothetical "intervention." For each replication I choose an arbitrary point along Maharashtra's actual border. Around that point I draw a circle of radius distributed uniformly between 95 and 105 kilometers. The regions of this circle that intersect with Maharashtra are marked as the "treated state." All other parts of Maharashtra is marked as the control state. I keep all cells within a bandwidth of 30 kilometers of the "border" between the treated state and the control state. Figure 18 shows several examples of fake states. I also draw a random year in which the "program" starts. I draw it from a discrete uniform distribution over the set $\{1998, 1999, \ldots, 2006\}$.

After constructing a fake state and a fake study area, I run a spatial polynomial regression similar to Equation 1. (Since this specification has the largest point estimates, it provides the most stringent test for any method of inference.) Rather than estimate a border effect for each year, I estimate a single average effect for all years after the fake program begins. This collapses the treatment effect into a single coefficient whose significance I can test. I estimate

$$\begin{aligned}[Light]_{i,t} = {} & [Fixed\ Effect]_i + P_t^3([Lat]_i, [Lon]_i) + [Post\ Program]_t \times P_t^3([Lat]_i, [Lon]_i) \\ & + \beta[Post\ Program]_t \times [Targeted]_i + [Error]_{i,t}\end{aligned} \tag{4}$$

where $[Post\ Program]_t$ is a dummy that switches on after the fake program begins. The coefficient β measures the average effect at the fake border after the start of the fake program.

Given that there has not been any intervention, a valid set of standard errors should reject the hypothesis $\hat{\beta} = 0$ at the 5 percent level roughly 5 percent of the time. In each simulation I estimate the standard error of $\hat{\beta}$ using 5 methods:

1. Assuming iid error terms
2. Using the spatial/time HAC standard errors proposed in Conley (1999), as implemented by code written by Hsiang (2010)
3. Clustering standard errors by cell
4. Clustering standard errors by sub-district (the method used in the main text)
5. Clustering standard errors by district

I run 100 simulations and report the rejection rates in Table 18. Not surprisingly, iid errors reject too much. More surprising is that the spatial/time HAC standard errors perform very poorly. Errors clustered by cell perform much better, likely because there is a lot of autocorrelation in light intensity. However, this method still rejects too much because it does not account for spatial correlation.

Table 18
Results of the Monte Carlo Simulation

	(1) Rejection Rate
iid Error	.28
Conley Spatial HAC	.71
Clustered by Cell	.09
Clustered by Subdistrict	.06
Clustered by District	.08
Simulations	100

Note: I estimate Equation 4, calculating standard errors using each of five methods. This table reports the rejection rate for the hypothesis $\hat{\beta} = 0$ at the 5 percent level. They null hypothesis is true, so the rejection rate should be .05. See text for the definition of each method.

The standard errors clustered by subdistrict perform best, rejecting only 6 percent of the time. (The 1 percent excess rejection may be because the procedure for defining the treatment, though fairly arbitrary, may occasionally coincide with an actual within-state program or policy). The standard errors clustered by district do slightly worse, likely because districts are too large; there are too few clusters within the study area for valid inference.

Given these results, I conclude that standard errors clustered by subdistrict have the best coverage and are least likely to over-reject a false null.

C Cost-Benefit Calculation (For Online Publication)

This appendix explains how I estimated the program's total cost to the central government and the total benefit. The total benefit is straightforward. For each of the three sets of estimates in Figure 7 I added up all the coefficients from 2002 onwards. These three sums give the total output created according to the spatial polynomial specification, the distance to border specification, and the comparison of means specification. I treated 2001 nominal GDP as baseline GDP (counterfactual GDP in the absence of the program), which is a conservative estimate. I take the product of baseline GDP and the sum of coefficients,

which I deflate to 2010 rupees using the consumer price index.[32]

Calculating the cost is less straightforward. To calculate the cost of all grants I rescale by 2001 population the yearly grants to both Uttarakhand and Uttar Pradesh. I subtract the per capita grants to Uttar Pradesh from the per capita grants to Uttarakhand to measure the excess grants given to Uttarakhand during the program. I then scale excess per capita grants back to an aggregate figure using the 2001 population.

To approximate the cost of the tax revenue foregone I turn to the Annual Survey of Industry. This survey covers formal firms in the industrial sector, which are precisely the firms covered by the tax concessions. Unfortunately, the firm-level data contain missing values and anomalies that make it difficult to replicate the Indian government's aggregate statistics. This makes the ideal approach—calculating each firm's tax burden and scaling up—untrustworthy. Instead I use the Indian government's aggregate statistics as follows. Since the corporate and excise tax exemptions depend on the age of an establishment, I first calculate the fraction of output accounted for by establishments in each of three bins: 0-4 years old, 5-9 years old, and 10 years or older. I calculate these fractions for each year.

I estimate the excise tax owed as total production in each year times times the excise tax rate. In theory, the excise tax rate in 2012 is 12 percent. In practice, there are so many exemptions that the average rate paid, according to the ASI firm-level data, is a tenth of that. Rather than use the largely invalid statutory rate, I calculate the average excise rate paid by firms in the ASI for each year in all states except Uttarakhand, Himachal Pradesh, and Jammu and Kashmir (which all received some tax concessions in this period). Since the exemption applied only to establishments younger than 10 years, I multiply this total tax owed by the estimated fraction of output generated by establishments less than 10 years old.

Like the excise tax, the corporate tax has both a statutory rate and a (far lower) effective rate. Unfortunately, firms do not report in the ASI how much corporate tax they pay. Instead, I compile the statutory rate for the years 2003-2012 from the annual budgets of the Ministry of Finance. I then calculate the

[32]In all calculations I deflate nominal variables using the CPI because India's method of GDP accounting creates artificial discontinuities in the GDP deflator.

average ratio of effective to statutory rate for the years from 1980 to 1999 using tables reported in Guha (2007), which is roughly 0.54. I take the product of this and the statutory rate for 2003-2012 to estimate the effective rate during the program.[33] For each year I multiply aggregate profits by the proportion of output that falls into each of the three age bins. Since the government granted establishments in the youngest bin a 100 percent exemption, I calculate the tax foregone on these firms as their share of profit times the effective tax rate. Since the second age bin had only a 30 percent exemption, I compute their tax foregone as their share of profit times the effective tax rate times 0.3. The oldest group received no exemption.

Finally, I estimate the cost of the investment subsidy as aggregate net investment times the subsidy rate of 15 percent. I add up the cost of grants and foregone taxes for each year after 2003 (when the tax exemptions officially took effect). I deflate the annual totals to 2010 rupees using the CPI. I then sum up the total cost for all years.

[33]Since 2008 the statutory rate has been higher for firms that earn more than 100 million rupees in profit. Since the ASI suggests these firms earn nearly all the profit in Uttarakhand, I assume all firms pay the higher rate. In practice this assumption matters little because the difference in rates is tiny. For example, in 2012 small firms paid a statutory rate of 30 percent while large firms paid 31.5 percent.