



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.

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.¹ 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 or infrastructure development to 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 to 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.² This largess was meant to compensate 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 fund-

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¹ Recent examples include Ravallion and Jalan (1999); Ravallion and Chen (2007); Acemoglu and Dell (2010); Bruhn and Gallego (2012); Musacchio et al. (2014).

² 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 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.

ing 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 exploiting a spatial regression discontinuity. 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 of 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 years after the program began. By my most conservative estimate, output is 28 percent higher 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 benefited 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 popula-

tion 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 India in general and Uttarakhand specifically, this place-based program successfully created economic 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](#)).³ 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. To be clear, my results are only informative about local development and cannot answer whether the policies targeted at Uttarakhand brought net benefits to the country as a whole. Given that the policy did little to create agglomeration externalities, it is possible there were no net benefits. But if the government's objective was regional development—for example, to reduce regional inequality—the results suggest the policy may have been effective.

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.⁴ I show in Section 3 that the difference-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.⁵ 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. The long lead of pre-program data 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.

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

³ 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.

⁴ [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 arguably a stronger assumption than parallel trends.

⁵ 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 a German place-based policy. Like much of the literature, they focus on developed rather than developing countries.

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