The Ecological Impact of Place-Based Economic Policies*

Teevrat Garg
UC San Diego

Ajay Shenoy
UC Santa Cruz

Abstract

Does economic development have an unavoidable ecological cost? We examine the impacts on forest cover of one of India's signature place-based economic policies involving massive tax benefits for new industrial and infrastructure development following the creation of the new state of Uttarakhand. Using a spatial difference-in-discontinuities design, we show that the policy, which explicitly excluded environmentally damaging industries, resulted in no meaningful change in local forest cover. Our results suggest that even in settings with low levels of enforcement, place-based economic policies that deliver transformative economic expansion can be implemented with minimal ecological costs.

Keywords: place-based economic policies, agglomeration, deforestation

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*Garg: Assistant Professor of Economics, School of Global Policy and Strategy, University of California – San Diego, La Jolla, CA 92093 (email: teevrat@ucsd.edu); Shenoy: Assistant Professor, Department of Economics, University of California – Santa Cruz, Santa Cruz, CA 95064 (email: azshenoy@ucsc.edu).We are grateful Sam Asher and Paul Novosad for generously sharing data. We thank the editor, Amy Ando and three anonymous reviewers, Jonah Busch, Francisco Costa, Francisco Garfias, Gaurav Khanna, Kate Sims, Gregor Singer, Dimitri Szerman, seminar participants at UCSD and conference participants at the 2019 AERE Summer Conference for feedback and comments on earlier versions of this paper. All errors are our own.

The central challenge of sustainable development is bridging the gap between rich and poor regions without lasting damage to the environment that could in turn undermine the goal of poverty alleviation (United Nations 2015). Indeed, there has been a long-standing debate in both the conservation and economics literature on the effects of economic development and policies that encourage such development on the environment (Arrow et al. 1995; Grossman and Krueger 1995; Stern, Common and Barbier 1996; Andreoni and Levinson 2001; Foster and Rosenzweig 2003; Dasgupta 2007; Alix-Garcia et al. 2013; Asher, Garg and Novosad 2020). Increasingly, governments around the world are using place-based policies – policies that target tax breaks or infrastructure development to an underdeveloped region – as a means to close the rising gaps between regions within their borders (Felkner and Townsend 2011; Busso, Gregory and Kline 2013; Kline and Moretti 2014; Shenoy 2018). Yet even as these policies become ubiquitous, relatively little is known about their environmental impacts, particularly in developing countries (Greenstone and Jack 2015).

We focus on a principal concern about such targeted development, the risk that forests will be cleared in the wake of infrastructure investments (Asher, Garg and Novosad 2020) and rising incomes (Alix-Garcia et al. 2013). In the context of place-based economic policies, such land-use change is particularly relevant since these policies often target remote and previously underdeveloped regions with native vegetation. Furthermore, forest cover loss is an urgent concern, generating global greenhouse emissions (IPCC 2014; Jayachandran et al. 2017), local health externalities (Bauch et al. 2015; Garg 2019; Masuda et al. 2019) deteriorating ecosystem services (Masuda et al. 2020) and resulting in loss of biodiversity (Gibson et al. 2011). The most recent report by the Intergovernmental Panel on Climate Change (IPCC) suggests that restoring and protecting forests could yield almost a sixth of the emissions mitigation required to prevent runaway climate change by 2030 (IPCC 2019).

We exploit a spatial discontinuity in the introduction of one of the world's most generous place-based policies. In 2002, the Government of India provided tax breaks and infrastructure investments worth nearly $34 billion to the recently formed state of Uttarakhand. The policy had an important additional feature, an explicit environmental rider that excluded certain environmentally detrimental industries from receiving any subsidies or tax-exemptions while favoring industries generally considered environmentally friendly.[1] Our setting is particularly important because Uttarakhand contains one of the only large contiguous tracts of forest in Northern India, with over 63% of the area in the state under forest cover. Furthermore, Uttarakhand's forests are important due to their rich biodiversity (Gaur 2007). The region has also historically identified with the environmental conservation movement as the birthplace of the *Chipko* Movement that encouraged local residents to hug trees in order to dissuade logging efforts (Shiva and Bandyopadhyay 1986).

The introduction of large scale regional investment in infrastructure and production subsidies can have ambiguous effects on forest cover. Forested land may be cleared for the development of industrial estates and associated physical infrastructure. Additionally, timber demand can increase either because rising incomes induce demand for land-intensive goods (Alix-Garcia et al. 2013) or highways and other infrastructure expand the scope for wood-using industry (Asher, Garg and Novosad 2020). At the same time, increased industrial activity could be associated with exits from agriculture that could lower demands on forested land from the agricultural sector (Assunção et al. 2017; Abman and Carney 2020) or poverty reduction could reduce demand for forest goods (Ferraro and Simorangkir 2020). Yet, other interventions such as alternative energy sources, even while ex-ante promising, have failed to reduce forest loss except when accompanied by complementary policies (Meeks, Sims and Thompson 2019). Overall, the effect of directed, geographically concentrated economic growth on forest cover is ambiguous.

While a broad literature has documented the relationship between economic development and environmental quality – often characterized as the "Environmental Kuznets Curve" – to the best of our knowledge, none have considered the effects of place-based economic policies on forest cover.[2] Unlike other development policies, place-based economic policies target an underdeveloped region rather than a segment of the population (e.g. the rural poor). One aim of these policies is to concentrate development in a region to generate a new center of agglomeration. These agglomerations could damage the environment by fostering industries that clear land and consume timber, or they could preserve it by concentrating people and economic activity within a few cities while leaving forests to regenerate. And by targeting firms rather than individuals, a place-based policy has the potential to shift production away from environmentally-intensive industries. The environmental damage done in developing countries by rapid industrialization continues to be a major source of controversy and therefore it is crucial to understand whether a carefully designed place-based policy can achieve major economic development without causing major forest loss.

Using a difference-in-discontinuities design, we find that the introduction of these subsidies had a small, statistically insignificant effect on forest cover, even 10 years after the introduction of the policy. By contrast, the same policy increased economic activity by at least 70% and as much as 300% (Shenoy 2018). We find no evidence to suggest that the null effects are driven by spillovers across the border or within-borders. Ten years after the introduction of the policy, we show that the absolute increase in employment in wood-using firms is modest relative to the overall expansion in employment. Given the cost of transportation and general uncommonness of cross-state investment, any potential spillover would most likely be near the border within the control state. But we find no evidence of such—if anything we find a general increase in forest cover on both sides of the border. Together, our results demonstrate that at least in terms of forest cover, place-based economic policies that explicitly exclude "worst offender" industries can achieve transformative economic expansion with relatively minimal environmental costs.[3]

Background and Data

The Policy

In 2002, the federal government initiated of a series of separate initiatives targeting the state of Uttarakhand (Shenoy 2018).[4] These included spending for new infrastructure, better access to existing infrastructure, and business tax exemptions. Though some of these funds were available ever since the state was formed in late 2000, it was only in 2002 that it began concentrating the funds in a handful of industrial estates along the border between Uttarakhand and the state of Uttar Pradesh to the south. These estates play a key role in the raft of tax exemptions that were specifically designed to spur growth without harming the environment.

These exemptions, titled the "Special Package Scheme for Himachal Pradesh and Uttarakhand," were first announced in March of 2002 with an effective date of 2003.[5]

The most generous include 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 corporate tax rate of 36.75 percent and an excise tax of 16 percent.[6]

Firms can only exploit the investment subsidy and excise tax exemption if they build and produce within Uttarakhand, giving firms an incentive to move factories rather than just their nominal headquarters. Figure A.1, which shows the change in the number of factories, makes it clear that 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 tax exemptions were designed to attract certain industries at the expense of others. The government published a "positive" list of industries that it considered "environmentally friendly" (Government of India 2003). These include floriculture, honey, and goods related to tourism (especially "eco-tourism"). Unlike most firms, which got tax exemptions at establishments within approved industrial estates, firms in the positive industries were eligible throughout the state. There was likewise a "negative" list of industries denied any tax benefits regardless of their location. The negative list includes coal and oil-based power plants, wood pulp, and most paper products. The complete positive and negative lists are provided in the Appendix.

The explicit environmental focus of the policy is in part a consequence of Uttarakhand's history. The movement that ultimately led to its creation had its roots in environmentalist protests triggered by timber concessions many decades ago (Tillin 2013). The policy was a calibrated attempt by the central government to win political support in the new state by promoting economic development without alienating the still-potent environmentalist movement.

The firms ultimately attracted to the industrial estates produce goods across all industries. Aside from information technology firms specifically courted by the IT Park at Dehradun's estate, nearly all registrants at the estates are in manufacturing. They produce everything from processed food to processed metals, Ayurvedic medicine to automobile parts, plastics to pharmaceuticals. Though paper products are supposedly excluded from the tax subsidies, there are still a non-trivial number of firms that produce boxes and packaging (possibly to supply the other firms). Given their presence it is not a foregone conclusion that the program caused little deforestation. That is an empirical question to which we devote the rest of the paper.

Data

We rely on three main types of data - satellite-imagery based forest cover, data on sector-specific firm composition and geographic boundaries.

Forest Cover: Detailed and reliable administrative records on forest cover and deforestation rarely exist, especially in developing countries. Instead, we obtain high resolution time series estimates of forest cover using a standardized publicly-available satellite-based dataset. Vegetation Continuous Fields (VCF) is available at 250m resolution and provides annual tree cover from 2000–2014 in the form of the percentage of each pixel under forest cover (Townshend et al. 2011).[7] For our primary specification we define forest cover as the average percentage of forest cover in a pixel. We define 0.1x0.1 degree latitude and longitude cells, and calculate the average pixel value within each cell. These cells are the unit of analysis in the specifications below.[8]

Firm Level Data: We obtain data on firms and employment from the the 1998 and 2013 Economic Census.[9] These data were merged to the Socioeconomic High-resolution Rural-Urban Geographic Dataset on India (SHRUG) and collapsed to a SHRUG location, which is the lowest identifiable census unit, either village or town (Asher et al. 2019). Our regressions thus give the impact on employment in the average census location.

Borders: We measure the discontinuous change in outcomes at the state boundaries by linking the forest cover and firm-level data to shapefiles of administrative boundaries created by ML Infomap - a commercial mapping firm[10]. These data give the border between Uttar Pradesh (control state) and Uttarakhand (treated state) as well as subdistricts, which we use as clusters in calculating standard errors.[11]

Research Design

Forest Cover

Our design closely matches that of Shenoy (2018), which is based on the assumption that there are parallel trends at the border in all unobservable factors that could affect economic growth for reasons unrelated to the place-based policy. Our assumption is that any unobservable factors (for example, geography, climate, local demand etc.) that might affect forest cover or employment are likewise growing in parallel at the border. This assumption is weaker than the assumption behind difference-in-differences, a more common approach in this literature, which requires parallel trends throughout the treated and control states. The stronger assumption is suspect in this context because governments target place-based policies to regions precisely because they are growing more slowly than other areas.

Shenoy (2018) shows that the assumption needed for difference-in-differences fails while that needed for difference-in-discontinuities appears to hold. There are clear differential trends in several variables between Uttarakhand (the treated state) and Uttar Pradesh (the control state), but these differences become statistically and economically insignificant at the border (see Appendix Table A.1 for a reproduction of these results).[12]

We measure the impact of the policy on deforestation and other outcomes using three difference-in-discontinuities specifications. Though all three estimate the discontinuous change in the growth (or loss) of forest cover from 2000 to each subsequent year through 2014, they differ in the approach they take to control for the unobservables that are assumed to vary smoothly around the border.

The spatial polynomial specification assumes that any unobservable correlates of deforestation vary smoothly over the surface of the earth and can thus be absorbed into a time-varying third-order polynomial in latitude and longitude. This polynomial acts like a two-dimensional version of the running variable in a traditional regression discontinuity design. It will remove from the estimated treatment coefficient any changes in deforestation that are smooth in space. Let i index cells and t index years, and let P^3 be a third-order polynomial in the latitude and longitude of the centroid of each cell.[13]

We estimate

$$
\begin{aligned}
[Tree\ Cover]_{i,t} = [Fixed\ Effect]_i + & \sum_{t=2001}^{2014} \kappa_t [Year\ Dummy]_t \\
& + \sum_{t=2001}^{2014} [Year\ Dummy]_t \times P_t^3([Lat]_i, [Lon]_i) \\
& + \sum_{t=2001}^{2014} \beta_t^S [Year\ Dummy]_t \times [Targeted]_i + [Error]_{i,t}
\end{aligned}
\tag{1}
$$

where $[Targeted]$ is an indicator for whether the cell is inside the targeted region. The direct term for the polynomial $P^3(\cdot)$ and the dummy $[Targeted]$ are absorbed into the fixed-effect. The terms $\{\beta_t^S\}$ measure the impact on forest cover in t relative to the level in 2000.

The distance to border specification estimates a local linear regression, as is standard for a univariate regression discontinuity design. We calculate the distance from each cell's centroid to the new border, and assume that any unobservable covariate of deforestation will be controlled for by the term $L_t([Distance]_i, [Targeted]_i) = \omega_{1,t}[Distance]_i + \omega_{2,t}[Distance]_i \times [Targeted]_i$. This specification assumes that the unobservables vary smoothly and (approximately) linearly with distance to the border, though the slope of that line may differ on the treated versus control side of the border and the entire relationship may change from year to year. We estimate

$$
\begin{aligned}
[Tree\ Cover]_{i,t} = [Fixed\ Effect]_i + & \sum_{t=2001}^{2014} \kappa_t [Year\ Dummy]_t \\
& + \sum_{t=2001}^{2014} [Year\ Dummy]_t \times L_t([Distance]_i, [Targeted]_i) \\
& + \sum_{t=2001}^{2014} \beta_t^D [Year\ Dummy]_t \times [Targeted]_i + [Error]_{i,t}
\end{aligned}
\tag{2}
$$

The coefficients $\{\beta_t^D\}$ measure the impact of the border on the change in forest cover.

The comparison of means specification simply calculates the difference in forest cover (relative to 2000) in a 4-kilometer neighborhood around the border. We estimate

$$\begin{aligned}[Tree\ Cover]_{i,t} = [Fixed\ Effect]_i + \sum_{t=2001}^{2014} \kappa_t [Year\ Dummy]_t \\ + \sum_{t=2001}^{2014} \beta_t^C [Year\ Dummy]_t \times [Targeted]_i + [Error]_{i,t}\end{aligned} \tag{3}$$

Finally, we estimate average program impacts by pooling pre- and post-program years in all three specifications:

$$\begin{aligned}[Tree\ Cover]_{i,t} = [Fixed\ Effect]_i + \sum_{t=2001}^{2014} \kappa_t [Year\ Dummy]_t \\ + [Post]_t \times P_t^3([Lat]_i, [Lon]_i) + \beta^S [Post]_t \times [Targeted]_i + [Error]_{i,t}\end{aligned} \tag{4}$$

$$\begin{aligned}[Tree\ Cover]_{i,t} = [Fixed\ Effect]_i + \sum_{t=2001}^{2014} \kappa_t [Year\ Dummy]_t \\ + [Post]_t \times L_t([Distance]_i, [Targeted]_i) \\ + \beta^D [Post]_t \times [Targeted]_i + [Error]_{i,t}\end{aligned} \tag{5}$$

$$\begin{aligned}[Tree\ Cover]_{i,t} = [Fixed\ Effect]_i + \sum_{t=2001}^{2014} \kappa_t [Year\ Dummy]_t \\ + \beta^C [Post]_t \times [Targeted]_i + [Error]_{i,t}\end{aligned} \tag{6}$$

We follow Shenoy (2018) in setting the bandwidth of the first two specifications at 30 kilometers and using a rectangular kernel.[14] As noted above, the comparison of means specification is restricted to a bandwidth of 4 kilometers. We deliberately choose specifications as similar as possible to those of Shenoy (2018) to let us compare the estimated impact on forest cover to the estimated impact on aggregate output.

Since our research design exploits variation across both time and space, our inference must correct for arbitrary correlation in the error terms along those dimensions. We take a nonparametric approach by clustering the standard errors within geographic administrative units. Though there are several levels of administrative unit that might serve, we follow Shenoy (2018), who shows that clustering by sub-district gives standard errors that reject a placebo null hypothesis at roughly the correct rate. Since the number of sub-districts available for the comparison of means specification is relatively small, we also confirm in Appendix Table A.3 that bootstrapped standard errors yield similar results to asymptotic errors for this specification.

Employment and Firm Growth

Since the 2005 Economic Census did not separate logging and tree-felling from other forestry industries (e.g. forest conservation), we must rely on only the 1998 and 2013 rounds. Since there are only two periods (pre and post), the specifications in the empirical section are not identified. We instead take the location-level change from 1998 to 2013 and run a local linear regression with a triangular kernel.[15] Since the difference-in-discontinuities is now essentially a standard regression discontinuity design (but taking a difference as the outcome), we apply the optimal bandwidth selection method of Calonico, Cattaneo and Titiunik (2014).][16] We estimate

$$\Delta[Outcome]_i = \pi_0 + \pi_1[Distance]_i + \pi_2[Distance]_i \times [Targeted]_i + \omega[Targeted]_i + [Error]_i \quad (7)$$

again clustering by sub-district.

Results

We report two principal findings in this paper. First, across a number of specifications and robustness checks we find that the policy had a small and statistically insignificant effect on forest cover. The effect is especially small relative to the expansion of economic activity. Second, we find a precisely estimated impact on employment in logging and wood-using firms that, though positive, is small relative to the overall expansion of employment. Finally, we discuss potential threats to our research design, most notably the risk that forest loss is displaced from treatment to control areas.

Effect on Forest Cover: Figure 1 compares raw average night time luminosity (left panel) to average forest cover (right panel) within 10 kilometers on either side of the discontinuity.[17] While average night time luminosity between treatment and control areas diverges substantially within a few years of the introduction of the policy (2002), average forest cover in treatment areas tracks closely with average forest cover in control areas showing no divergence in trends.

Figure 2 shows the discontinuity at the border in average forest cover in the years 2000 (left panel) and 2014 (right panel). Even 12 years after the introduction of the policy, and four years after the end of the policy, there is no discernible difference in forest cover at the border.

Figure 3 shows the year-by-year estimates corresponding to each of the Equations 1—3. In all three figures, each estimate provides the discontinuous change in tree cover at the boundary relative to the discontinuity in the year 2000. The red line indicates the year 2002 when the policy came into effect. Across all three specifications, we observe a small negative effect of the policy on forest cover.

We formally estimate the effect of the policy on tree cover and report the aggregate results of our difference-in-discontinuities design in Table 1. In Column (1) we employ a spatial polynomial estimator, in Column (2) we use a distance to border approach and in Column (3) we calculate a simple difference of means. Across all three specifications, we find that the shift in the estimate at the border before and after the implementation of policy was small and statistically insignificant at conventional levels. These null effects are unlikely to be the result of a lack of statistical power; indeed our results on employment reported subsequently show that our design has statistical power to pick up even small changes in forest cover/employment if they exist. Using our preferred specification in Column (1) we find a mean reduction of 0.49 percentage points or 2.98% of forest cover. Based on a 95% confidence interval, we can reject forest loss in excess of 1.37 percentage points or 8.3% of the mean at baseline.[18] We are able to reject similar increases using alternative specifications (Columns 2 - 3). To alleviate concerns that forest cover in levels may be too noisy , we show that our results are robust to using an inverse hyperbolic sine transformation of the dependent variable (Appendix Figure A.4, Appendix Table A.2).[19]

Effect on Employment: Table 2 shows the effects of the policy on employment in all firms and specifically the subset of firms in the logging industry and more generally in industries where the primary input is raw lumber. We find there is a marked increase in overall employment. In Column (1) we show that employment increased by 84.26 persons in each census location and the effect is significant at the 5% level. Compared to a baseline treatment group mean of 64 employed persons per census location, this translates to a 32.2% increase in overall employment. By contrast, we see a precise but modest increase in employment in logging firms. The average census location saw an increase of 0.56 workers in this category (Column 2, Table 2). There was virtually no employment in this sector on either side of the discontinuity before the implementation of the policy. Logging firms represent 0.66% of total change in employment as a result of policy. When considering wood-using firms (Column 3, Table 2), we find that the policy increased employment in this category by nearly 6.35 workers per census location, or 7.5% of overall increase in employment.

Does displacement explain the null-result? One reason for our null-estimate could be that the effect of the policy led to increased forest loss in not only the treatment area but also the control area.[20] While it is not possible to test for displacement explicitly, Figure 1 shows that forest cover in the treatment area closely tracks forest cover in the control areas, before, during and after the policy is in effect. There is also no evidence of an overall decline in control areas. Appendix Figure A.2 shows maps of forest cover in 2000 and 2014 confirm no evidence of systematic change in the control region (south of the border) after the implementation of the policy. In 2013, employment in logging is 0.07% of total employment near the border of the control region—not much of an increase from 0% in 1998. Taken together these results suggest local displacement is unlikely to be the source of our null-finding.

Is it possible that deforestation was displaced to regions far from the border in the control state or to states entirely outside our study region? Although we cannot categorically rule out this kind of displacement, it is unlikely because shipping logs over long distances across India's relatively slow infrastructure is costly. The least costly option is to source wood from the closest point in the control state. The absence of any detectable decline in the border region of the control state makes it unlikely that such displacement is driving our result.[21]

Discussion and Conclusion

The rising concern of increasing, geographically-concentrated economic divisions within national borders has spurred the growth of place-based economic policies. These policies provide incentives for industrial development and infrastructure through subsidies and tax-breaks and typically target remote areas that are more likely to have native vegetation. While concern has been expressed over the short- and long-run ecological footprint of such rapid development, the policy we study showed no such ramifications. Exploiting a spatial discontinuity in the policy, even ten years after its introduction and four years after its end we find no effect on forest cover. By contrast, the expansion of economic activity was massive. Finally, we find no evidence for spillovers across the border from the treatment to the control region.

One possible reason for this win-win result is that the policy had an explicit environmental rider that excluded tax-breaks to certain environmentally detrimental industries such as pulp, paper and mining while explicitly promoting others such as food, pharmaceuticals and non-timber forest-based products. In effect, the policy increased the relative costs of setting up environmentally detrimental industries. The policy also created strong incentives to locate production inside a set of compact industrial areas located within existing cities. By increasing the density of industrial production the policy may have minimized the deforestation that arises from sprawling new construction.

An important caveat for our findings is that we focus on one measure of environmental quality - forest cover. Economic development can also affect air and water quality; however, the lack of detailed data during the relevant time period in our study region precludes us from estimating these effects. A second caveat to our study is that we are unable to uncover the exact mechanisms that led to minor forest loss from the policy. Future research should address other such potential external costs of policy-driven, geographically-concentrated economic development to fully uncover their welfare effects. Moreover, research on similar policies such as Special Economic Zones (SEZs) in other contexts is urgently needed to understand the external validity of our findings.

Notes

[1]In the Appendix, we provide both the "positive" or encouraged environmentally friendly list and the "negative" or environmentally unfriendly list of industries.

[2]Other papers have considered cash transfers (Alix-Garcia et al. 2013; Wilebore et al. 2019), rural credit (Assunção et al. 2019), agriculture (Assunção et al. 2017; Abman and Carney 2020) and trade (Antweiler, Copeland and Taylor 2001; Copeland and Taylor 2004). There is also an extensive literature documenting the relationship between economic development and the environment. For a non-exhaustive list, see: Den Butter and Verbruggen (1994); Arrow et al. (1995); Grossman and Krueger (1995); Stern, Common and Barbier (1996); Andreoni and Levinson (2001); Dasgupta et al. (2002); Foster and Rosenzweig (2003); Stern (2004). For a through review on drivers of deforestation, see Busch and Ferretti-Gallon (2017).

[3]It is worth noting that we are unable to attribute our findings to any particular feature of this place-based policy but only to the policy as a whole. As such we provide an existence result that large economic expansion can be coupled with relatively minor ecological footprint.

[4]Uttarakhand was previously a set of administrative districts that formed the northeast corner of Uttar Pradesh. The new state was formed along the boundaries of several existing districts. Since it followed these existing administrative divisions the new state border was not completely arbitrary, but it also was not custom designed to include an underdeveloped area (and in some areas it actually bisected existing urban agglomerations). Although districts can play a role in administering state and central government programs, they do not have leeway to pass laws or set policy.

[5]Uttarakhand and Himachal Pradesh received this favorable treatment because the Indian government considers their mountainous terrain a significant barrier to development. India's Planning Commission has long allocated extra funds to rugged and sparsely populated areas. The tax exemptions, which would later be extended to Jammu and Kashmir (a Himalayan state in northern India), were at least on paper intended as extensions of this long-standing practice.

[6]As explained in Shenoy (2018), the effective rate is somewhat lower but still far from trivial.

[7]Some previous studies have used Global Forest Cover (GFC) dataset that describes baseline forest cover in the year 2000, and a binary indicator for the year of deforestation for each 30mX30m pixel. As noted in Asher, Garg and Novosad (2020), GFC is less useful for the study of forest cover in India because GFC does not capture forest gains in areas with positive baseline forest cover or partial forest loss. While GFC is an excellent source for other contexts such as Brazil and Indonesia, it is less suitable in the Indian context which saw overall increases in forest cover during our study period (for example, see Figure 1). For more information on the comparability of different forest cover datasets in India, see Asher, Garg and Novosad (2020).

[8]Any cell that intersects the border is split in half along the border, with halves assigned to the appropriate state.

[9]While there was an economic census conducted in 2005, employment figures for logging firms were combined with those engaged in afforestation practices and hence are unsuitable for the analysis in this

paper.

[10]See: https://www.mlinfomap.com/

[11]Sub-districts are the third tier of administrative divisions in India, preceded by states and districts.

[12]One difference in trends (population) remains marginally significant. Given the number of variables tested (and the fact that we do not adjust for multiple inference in this table), the difference is likely to have arisen by chance. Regardless, the point estimate implies there was slightly faster population growth at the border, which if anything would bias the results towards finding greater deforestation in the treated state.

[13]This polynomial is simply the variables that arise from a triple interaction of latitude and longitude with themselves and one another.

[14]The bandwidth of a regression discontinuity design is the set of observations given positive weight in the estimation—in this case, those within 30 kilometers of the border.

[15]A triangular kernel puts heavier weight on observations close to the discontinuity, making it less sensitive than a rectangular kernel to small changes in bandwidth. This property is especially useful when dealing with a relatively noisy outcome like the change in employment.

[16]The optimal bandwidth selection method minimizes weighted mean-squared error.

[17]Nighttime luminosity has been used a proxy for economic activity (Cook and Shah 2020) and development outcomes such as electrification (Baskaran, Min and Uppal 2015; Mahadevan 2020). As in (Shenoy 2018) we use nighttime lights as a proxy for local economic development and activity.

[18]Our results compare to the effects the national rural roads program that had no discernible effect of forest cover and contrast with a national highway construction program reduced forest cover by 17% (Asher, Garg and Novosad 2020).

[19]We prefer inverse hyperbolic sine transformation over a log transformation to account for zero values in forest cover data.

[20]There is also the possibility of displacement from the border to locations in the treated state further away from the border. But the policy contains stricter environmental protections outside the industrial estates along the border, making it unlikely that forest cover loss was displaced from the border to the interior. Moreover, reasonable alterations in the bandwidth of our discontinuity design do not overturn our result suggesting that there is no reason to suspect spillovers to neighboring regions away from the border.

[21]We also cannot categorically rule out that in the absence of the policy, firms would counterfactually would have sited their factories elsewhere and workers might have been drawn out of environmentally unfriendly industries.

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Figures

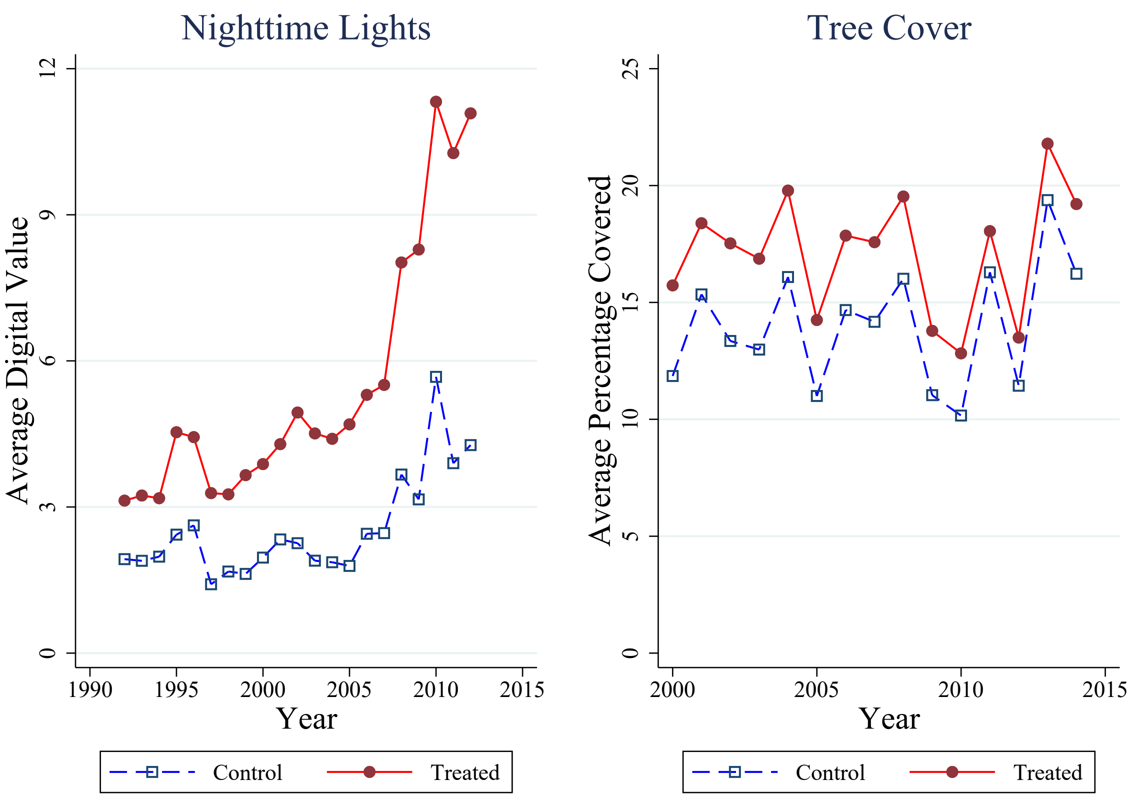

Figure 1: Comparison of Nighttime Luminosity and Deforestation Within 10KM of Border

We plot the mean of each outcome for cells that lie within 10 kilometers of the border.

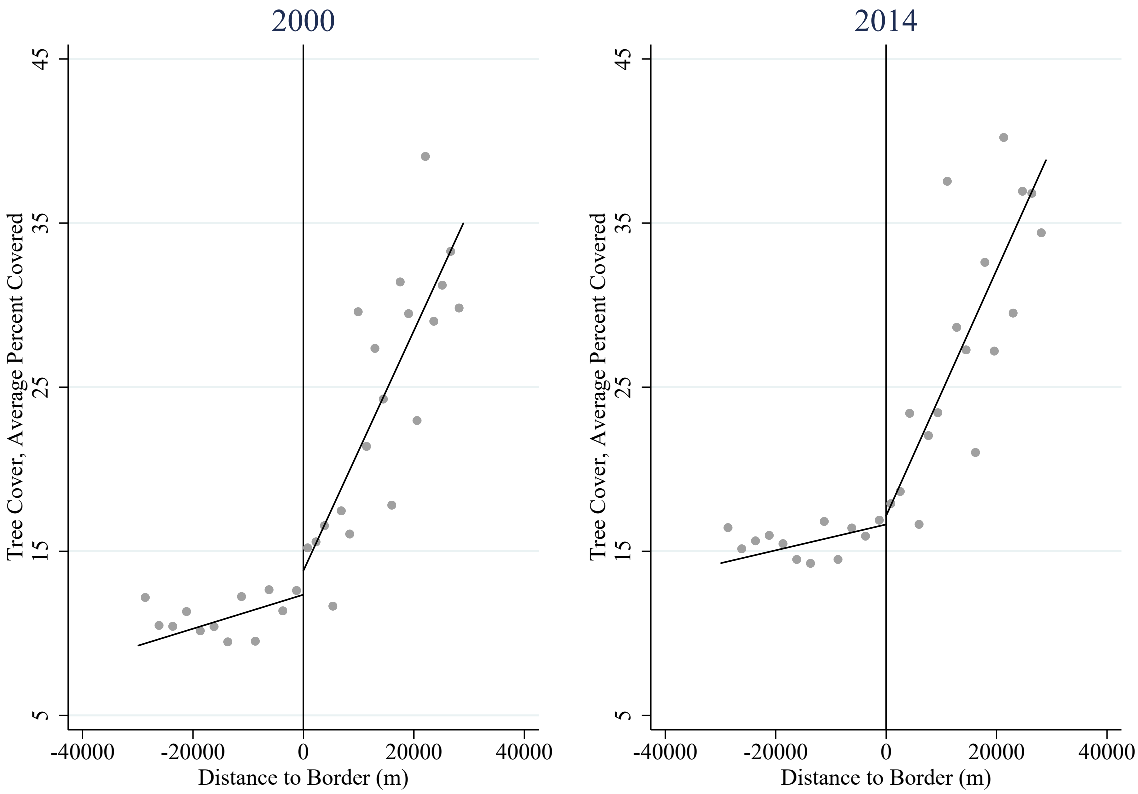

Figure 2: Regression Discontinuity at the Border in 2000 and 2014

We plot average tree cover against distance to the boundary (positive values are in the targeted state). Each dot shows average tree cover within a bin, where the bins are chosen by the variance evenly-spaced method estimated using code from Calonico, Cattaneo and Titiunik (2014).

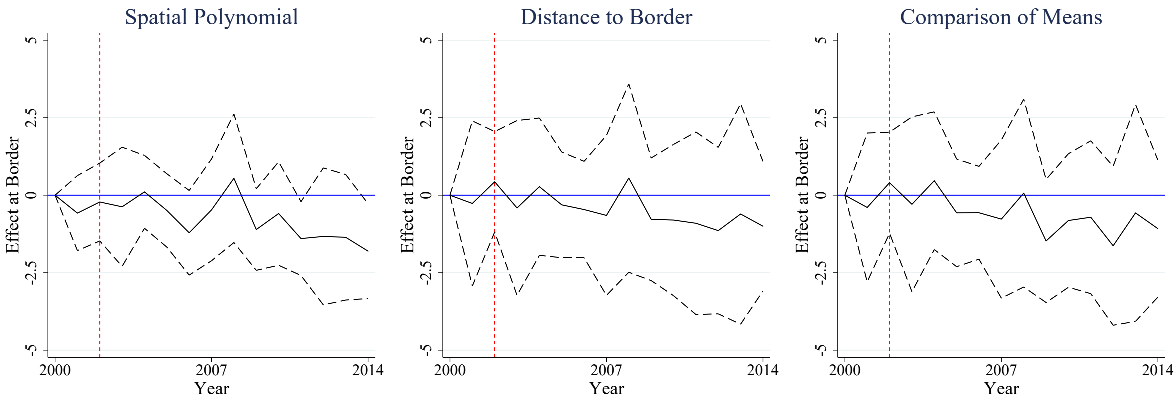

Figure 3: Difference-in-Discontinuities Estimate of Effect of Place-Based Policy on Deforesation

We plot the estimates $\{\hat{\beta}_t^S\}$, $\{\hat{\beta}_t^D\}$, and $\{\hat{\beta}_t^C\}$ from estimating Equations 1—3. Each estimate gives discontinuous change in tree cover at the boundary relative to the discontinuity in the year 2000. The red dashed line shows the first year of the policy.

Tables

Table 1: Difference-in-Discontinuities Estimate of Place-Based Policies on Tree Cover

	Spatial Polynomial	Distance to Border	Comparison of Means
Post-PBP	-0.49	-0.31	-0.38
	(0.45)	(0.54)	(0.53)
Cell-Years	4320	4320	1350
Cells	288	288	90
Sub-districts	38	38	26
Mean at Baseline	16.4	16.4	16.4

Estimates of $\hat{\beta}^S, \hat{\beta}^D, \hat{\beta}^C$ from Equations 4—6. The outcome is the average tree cover within each cell. Standard errors are clustered by sub-district.
Significance levels denoted at conventional levels *** p<0.01, ** p<0.05, * p<0.1

Table 2: Regression Discontinuity Estimate of Place-Based Policy on Employment and Firms

	Employment		
	All	Logging	Wood-Using
Post-PBP	84.26**	0.56**	6.35**
	(33.81)	(0.23)	(2.48)
Observations	19236	19236	19236
Sub-districts	29	45	29
Optimal BW	12.9	30.9	15.3
Control Mean, 1998	64.0	0.0	1.2
Treated Mean, 1998	80.0	0.0	3.2

We estimate Equation 7 for employment and the number of firms within each of the given industries ("all" is all employment measured in the Economic Census). The unit of observation is a census location (either a town or a village). Standard errors are clustered by sub-district.
Significance levels denoted at conventional levels *** $p<0.01$, ** $p<0.05$, * $p<0.1$