Impact of Rural Employment Guarantee on Deforestation: Evidence from India

Martin Kosík

22nd January 2021

Abstract

-

1 Introduction

Mitigating deforestation in developing countries is considered to be one of the most cost-effective way to reduce CO2 emissions since the costs of decreasing deforestation (e.g., intensifying agriculture or adopting different construction materials) tend to be low compared to technology changes such as necessary in high-income countries (Jayachandran et al., 2017; Stern, 2007, chapter 25). While the reduction of extreme poverty in developing countries has been the priority of many international organizations and governments, there has been discussion of how programs to alleviate poverty affect environmental quality and deforestation (World Bank, 2007; Gilliland et al., 2019). However, the existing empirical evidence on this topic is relatively scarce. Moreover, the existing studies are rather and limited in their scope with almost exclusive focus on the conditional cash transfers (Alix-Garcia et al., 2013; Ferraro and Simorangkir, 2020).

The goal of this paper of is to estimate the effect of the flagship workfare program of India, the National Rural Employment Guarantee Act (NREGA), on deforestation. The staggered rollout of the program across the Indian districts enables me to apply the difference-in-differences design. The data

The direction of this effect is theoretically ambiguous since there are several ways by which a rural job guarantee could influence deforestation. On the one hand, the demand for deforestation could rise due to increase in income. On the other hand, if the farmers deforest in response to negative income shocks, the employment guarantee could reduce deforestation by in effect providing a form of insurance.

2 Literature review and Contribution

This paper relates to two important topics within development economics.

First, there is large literature on the causes of deforestation and evaluation of policies that might mitigate it. Jayachandran et al. (2017) conducted a RCT to estimate the impact of ecosystem protection payments (whereby owners of the forest are paid if they do not cut down their trees) and find a negative effect of the payment on deforestation. Moreover, there is no evidence that deforestation spills over to neighboring forests. Souza-Rodrigues (2019) instruments regional differences in transportation costs to recover the demand of farmers in Brazilian Amazon for deforestation. Using this estimated demand from a structural model, he then infers the effects of different counterfactual policies (including carbon tax and ecosystem protection payments) on deforestation in the Amazon.

However, the impact of anti-poverty programs on deforestation in developing countries have received relatively little attention and have focused almost exclusively on conditional cash transfers (CCT). While Alix-Garcia et al. (2013) find that Mexico's CCT program increased deforestation (which was mainly driven by increase in consumption of land-intensive goods), the results of Ferraro and Simorangkir (2020) show the opposite effect in case of Indonesian CCT program. Ferraro and Simorangkir (2020) explain it by consumption smoothing effect of CCTs

Nevertheless, these contradictory results suggest that further evidence from other types of programs and from other contexts is need to better understand under what conditions should we expect poverty alleviation programs to reduce deforestation. Moreover, I have available much larger dataset consisting of a balanced panel of more than 500,000 villages for a 20-year time span, which enables me to apply more demanding specifications in terms of degrees of freedom (e.g., to include village-specific linear time trends).

Second, this paper also contributes to literature on the effects of Indian anti-poverty program National Rural Employment Guarantee Act (NREGA). Imbert and Papp (2015) examine the effects of NREGA on the local labor markets. They find evidence that NREGA crowded out private sector work and increased private sector wages. Cook and Shah (2020) use night time light intensity and bank deposit data to estimate the effect the program on the aggregate output. Their results imply 1 to 2 % increase in per capita output although there is substantial heterogeneity with the poorer districts experiencing no significant gain in output due to NREGA. Morten (2019) finds that NREGA reduced migration and risk sharing. Moreover, if these effects are ignored, the welfare gain from NREGA are substantially overstated. Fetzer (2020) shows that by effectively providing insurance to poor farmers, the NREGA reduced the impact of negative weather shocks on conflict. A strand of the literature studies which factors influenced the effectiveness of NREGA implementation (Gulzar and Pasquale, 2017; Banerjee et al., 2020).

3 Context and data

The National Rural Employment Guarantee Act passed in 2005 with the aim to help poor farmers. It provided a guarantee to every rural household of 100 days of manual labor at the stipulated state-level minimum wage. The local village governments (Gram Panchayats) are responsible for choosing the infrastructure projects and organizing the work. The program was introduced in three phases. The 200 phase I districts received the NREGA in February 2006, the additional 288 districts in phase II followed in April 2007, and the rest of rural India received the program in April 2008 (Desai et al., 2015).[1]

The assignment of districts to phases was not random. In fact, the poorer districts were more likely to receive NREGA earlier (Khanna and Zimmermann, 2017). Therefore the selection on observable design would clearly be inappropriate in this case. Instead, my identification strategy will rely on the parallel trends assumption, which is more plausible in this context (I will describe this in greater detail in the following section).

I will use forest cover panel data on a town and village level annually from 2000 to 2019 provided by Asher et al. (2020). The percentage of forest cover for each 250×250 meters pixel is estimated from high-resolution satellite imagery using thermal signature of the reflected light. The pixel-level data are then aggregated to village-level, by summing all the values of the pixels within given village, adding one, and taking natural logarithm of the result, which will be my

[1] The table with assigned phases for every district can be downloaded from `https://nrega.nic.in/MNREGA_Dist.pdf`

main measure of forest cover. Nevertheless, I also test the robustness to using average value of pixels for each village.

4 Identification strategy

4.1 Difference-in-differences

My identification strategy uses a difference-in-differences (DiD) type design that exploits the staggered rollout of NREGA across the districts.

$$Y_{idt} = \alpha_i + \delta_t + \tau W_{dt} + \epsilon_{idt} \tag{1}$$

where Y_{idt} is an aggregated forest cover for village i, in district d, in year t, α_i and δ_t are village and time fixed effects, and W_{dt} is an indicator variable that equals one if district d was eligible for NREGA at year t and zero otherwise. As was described above due to the staggered nature of the rollout, the W_{dt} can be written as $\mathbb{1}\{t \geq 2006\}$ for districts in phase I, as $1\mathbb{1}\{t \geq 2007\}$ for districts in phase II, and $\mathbb{1}\{t \geq 2008\}$ for the rest. Since our treatment varies only on the district level, I will cluster standard errors by districts as recommended by Cameron and Miller (2015).

The identifying assumption is that in the absence of treatment (introduction of NREGA) the trends in the logarithm of total forest cover for villages in phase I would have evolved in parallel to those in phases II and III (and phase II would have evolved in parallel to those in phase III).

Although we cannot directly test this assumption, we can assess its plausibility by testing if the trends prior to treatment were parallel. This is usually done by adding leads of the treatment dummy into the main regression. However, Borusyak and Jaravel (2017) advises against it since it decreases efficiency of the treatment effect estimation by combining validation and estimation stage of the design. Moreover, it induces correlation between treatment effect and pre-trend estimators which might lead to bias if treatment effect estimates are trusted only if pre.treatment trends are not significant. Instead, Borusyak and Jaravel (2017) recommends to run the following regression on the untreated observations only

$$Y_{idt} = \alpha_i + \delta_t + \sum_{k=1}^{K} \gamma_k \mathbb{1}\{t = E_d + k\} + \epsilon_{idt} \tag{2}$$

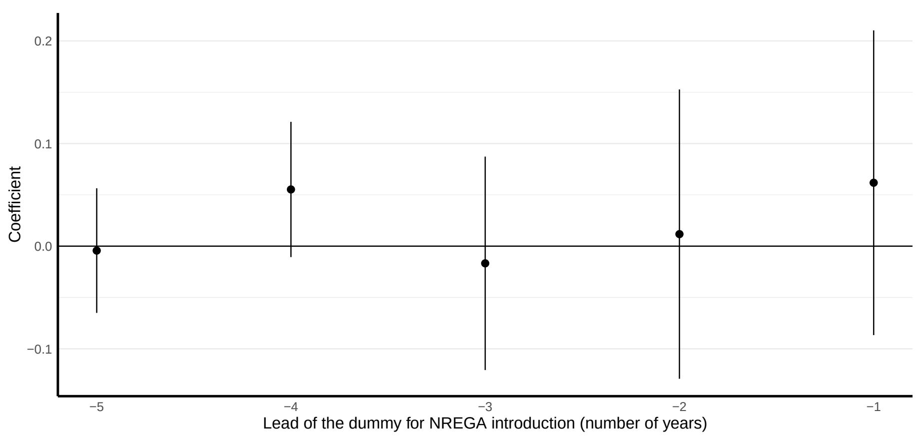
and to test the significance of the coefficients γ_k, where E_d denotes the year when the district d was first treated (i.e. 2006 for those in phase I etc.).

5 Results

5.1 Pre-trends test

The results of the specification (2) for different number of leads (K) are provided in table A1 in the appendix. The regressions with higher number of leads included enable us to test trends further back in time but tend to give less precise estimates (due to smaller degrees of freedom). Overall, the coefficients on leading effects are relatively small and almost all are insignificant at the conventional 5% level.[2] There also do not seem to be any noticeable monotonic trends in the pre-treatment coefficients as can be seen from figure 1, which plots them. These results do not give us strong reason to doubt the validity of the parallel trends assumption. Nevertheless, I still later perform additional robustness checks to probe the sensitivity of the main results this paper.

Figure 1: The Coefficients on Pre-trends (γ_k)



Note: The error bars show the 95% confidence intervals. The standard errors are clustered on the district level.

[2]The only exception is the coefficient on the fourth lead in the model with $K = 4$

5.2 Main results and robustness checks

The estimates of the treatment effect of NREGA for the baseline specification (1) are provided in table 1. The results suggest that NREGA did not have substantial impact on forest cover of an average village. The point estimate of the baseline specification are very small (implying 1.7% increase in forest cover in response to NREGA) and are not significantly different from zero.

Table 1 also shows the results of additional robustness checks. First, the column (2) augments the baseline model with village-specific linear time trends (in other words the terms $\omega_i \cdot t$ are added to the baseline specification). This is demanding in terms of the degrees of freedom, but the upside is that it relaxes the parallel trends assumption to some extent since the trends have to be parallel only conditional on the village-specific linear time trends. Moreover the large number of pre-treatment periods in our data (5 years) hopefully partly allviate the concerns about the degrees of freedom. The treatment effect change little after addition of these village-specific linear time trends.

One potential concern might be that various state-level policies implemented in 2006, 2007, and, 2008 might confound the results since the number of districts assigned to different phases is not balanced across states. To address this issue, I include the state-year fixed effects to the baseline specification. As the results in column (3) of table 1 show, the treatment effect estimates are very similar to the baseline specification. Finally, our main results also do not depend on the particular measure of forest cover. Specifically, using average forest cover (in percent) for a village instead of the logarithm of the total cover leads us to essentially the same conclusions.

6 Heterogeneity and Mechanisms

The presented evidence suggests that on average NREGA did not have significant impact on deforestation. However, it is possible that this is caused by averaging out the heterogeneous effects across villages. One possible source of heterogeneity is effectiveness in NREGA implementation. As many studies documented (Gulzar and Pasquale, 2017; Banerjee et al., 2020), there were large differences in prevalence of corruption and administrative capacity across villages. Interacting some indicator of the program implementation effectiveness with the treatment dummy could then test if program leakage can explain the lack of large effect we observed. Nonetheless, it might be challenging to find an

Table 1: Main Results

Dependent Variables:	Log of total forest cover			Avg. forest cover (%)
Model:	(1)	(2)	(3)	(4)
Variables				
NREGA	0.0170	0.0009	0.0116	0.0518
	(0.0131)	(0.0141)	(0.0122)	(0.1229)
Fixed-effects				
Village (515,120)	Yes	Yes	Yes	Yes
Year (20)	Yes	Yes		Yes
State × Year (680)			Yes	
Linear time trends				
Village-specific		Yes		
Fit statistics				
Observations	10,302,400	10,302,400	10,302,400	10,302,400
R^2	0.908876	0.92083	0.923461	0.919245
Within R^2	4.07×10^{-5}	1.141×10^{-7}	1.834×10^{-5}	6.806×10^{-6}

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The standard errors in parentheses are clustered on a district level.

village-level nation-wide measure of NREGA implementation effectiveness that would not confound our specification.

In the future research I would conduct several additional tests to better understand the mechanisms behind the results. In the Indonesian context, Ferraro and Simorangkir (2020) argue that farmers tend to deforest more when they anticipate inadequate harvest due to low rainfall. It would be useful to test whether such behavior occurs also in the Indian context. This could be done simply by running a reduced-form regression of forest cover on amount of rainfall in the cultivation season. An obvious concern is that the amount of rainfall can have direct effect on forest cover and can be correlated with other factors influencing forest cover. Therefore it may be better to exploit variation in the price of futures of agricultural commodities instead (while controlling for the local whether conditions). These price changes induce shocks in the expected income of the farmers since they change the expected value of the future harvest. Nevetheless, there might be other problems with this approach (the production in India might influence the world price of certain crops, the Indian agricultural markets are heavily regulated).

7 Conclusion

This paper estimated the ecological impact of the National Rural Employment Guarantee Act (NREGA), a nation-wide workfare program in India, using a difference-in-differences. I find no significant effect of the program on forest cover for the average village that received the program in the first two phases. These results might, to some extent, address concerns about the possible adverse effects of poverty alleviation on environmental quality (empirically documented e.g., by Alix-Garcia et al. (2013) in the case of a conditional cash transfer program in Mexico). According to the estimates of Cook and Shah (2020), the NREGA increased the per-capita output by 1 to 2%, it is therefore encouraging that this rise in economic activity does not seem to be accompanied by higher deforestation.

Nevertheless, these results are only preliminary and additional research is needed to better understand the mechanisms behind the main results and the heterogeneity in the effect.

References

Alix-Garcia, Jennifer, McIntosh, Craig, Sims, Katharine R. E. and Welch, Jarrod R. (2013), 'The Ecological Footprint of Poverty Alleviation: Evidence from Mexico's Oportunidades Program', *The Review of Economics and Statistics* 95 (2), pp. 417–435.

Asher, Sam, Garg, Teevrat and Novosad, Paul (2020), 'The Ecological Impact of Transportation Infrastructure', *The Economic Journal* 130 (629), pp. 1173–1199, DOI: `10.1093/ej/ueaa013`.

Banerjee, Abhijit, Duflo, Esther, Imbert, Clement, Mathew, Santhosh and Pande, Rohini (2020), 'E-governance, accountability, and leakage in public programs: Experimental evidence from a financial management reform in india', *American Economic Journal: Applied Economics* 12 (4), pp. 39–72.

Borusyak, Kirill and Jaravel, Xavier (2017), *Revisiting Event Study Designs*, SSRN Scholarly Paper ID 2826228, Rochester, NY: Social Science Research Network, DOI: `10.2139/ssrn.2826228`.

Cameron, A. Colin and Miller, Douglas L. (2015), 'A Practitioner's Guide to Cluster-Robust Inference', *Journal of Human Resources* 50 (2), Publisher: University of Wisconsin Press, pp. 317–372, DOI: `10.3368/jhr.50.2.317`.

Cook, C. Justin and Shah, Manisha (2020), 'Aggregate Effects from Public Works: Evidence from India', *Review of Economics and Statistics*, pp. 1–38.

Desai, Sonalde, Vashishtha, Prem and Omkar, Joshi (2015), *Mahatma Gandhi National Rural Employment Guarantee Act: A Catalyst for Rural Transformation*, tech. rep., New Delhi: National Council of Applied Economic Research.

Ferraro, Paul J. and Simorangkir, Rhita (2020), 'Conditional cash transfers to alleviate poverty also reduced deforestation in Indonesia', *Science Advances* 6 (24), DOI: `10.1126/sciadv.aaz1298`.

Fetzer, Thiemo (2020), 'Can Workfare Programs Moderate Conflict? Evidence from India', *Journal of the European Economic Association* 18 (6), pp. 3337–3375, DOI: `10.1093/jeea/jvz062`.

Gilliland, Ted E., Sanchirico, James N. and Taylor, J. Edward (2019), 'An integrated bioeconomic local economy-wide assessment of the environmental impacts of poverty programs', *Proceedings of the National Academy of Sciences* 116 (14), pp. 6737–6742, DOI: 10.1073/pnas.1816093116.

Gulzar, Saad and Pasquale, Benjamin J. (2017), 'Politicians, bureaucrats, and development: Evidence from India', *American Political Science Review* 111 (1), pp. 162–183.

Imbert, Clément and Papp, John (2015), 'Labor Market Effects of Social Programs: Evidence from India's Employment Guarantee', *American Economic Journal: Applied Economics* 7 (2), Publisher: American Economic Association, pp. 233–263.

Jayachandran, Seema, Laat, Joost de, Lambin, Eric F., Stanton, Charlotte Y., Audy, Robin and Thomas, Nancy E. (2017), 'Cash for carbon: A randomized trial of payments for ecosystem services to reduce deforestation', *Science* 357 (6348), pp. 267–273, DOI: 10.1126/science.aan0568.

Khanna, Gaurav and Zimmermann, Laura (2017), 'Guns and butter? Fighting violence with the promise of development', *Journal of Development Economics* 124, pp. 120–141, DOI: 10.1016/j.jdeveco.2016.09.006.

Morten, Melanie (2019), 'Temporary migration and endogenous risk sharing in village india', *Journal of Political Economy* 127 (1), pp. 1–46.

Souza-Rodrigues, Eduardo (2019), 'Deforestation in the Amazon: A Unified Framework for Estimation and Policy Analysis', *The Review of Economic Studies* 86 (6), pp. 2713–2744, DOI: 10.1093/restud/rdy070.

Stern, Nicholas (2007), *The Economics of Climate Change: The Stern Review*, Cambridge: Cambridge University Press, DOI: 10.1017/CBO9780511817434.

World Bank (2007), *Poverty and the Environment: Understanding Linkages at the Household Level*, Washington, DC: World Bank Publications.

A Appendix

Table A1: Pre-trends regressions

Dependent Variable:	Log of total forest cover		
Model:	(1)	(2)	(3)
Variables			
$\mathbb{1}\{t = E_d + 1\}$	0.1492	0.0618	0.0712
	(0.1734)	(0.0757)	(0.0600)
$\mathbb{1}\{t = E_d + 2\}$	0.0859	0.0117	0.0197
	(0.1413)	(0.0719)	(0.0444)
$\mathbb{1}\{t = E_d + 3\}$	0.0437	-0.0167	-0.0101
	(0.1240)	(0.0530)	(0.0354)
$\mathbb{1}\{t = E_d + 4\}$	0.1025	0.0553	0.0601**
	(0.0942)	(0.0336)	(0.0305)
$\mathbb{1}\{t = E_d + 5\}$	0.0285	-0.0043	
	(0.0574)	(0.0310)	
$\mathbb{1}\{t = E_d + 6\}$	0.0221		
	(0.0410)		
Fixed-effects			
Village (515,120)	Yes	Yes	Yes
Year (8)	Yes	Yes	Yes
Fit statistics			
Observations	3,568,475	3,568,475	3,568,475
R^2	0.923404	0.923401	0.9234
Within R^2	0.003896	0.003856	0.003851

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The standard errors in parentheses are clustered on a district level.