Replication Exercise

“Representation and Forest Conservation: Evidence from India’s Scheduled Areas”

Sachet Bangia, Byron Cohen,
Chengyu Fu and Kartik Srivastava

Outline

Summary

Replication

Suggested Robustness Checks and Extensions

Summary

Research Question:

- How does the political empowerment of indigenous groups affect observed levels of environmental protection versus natural resource exploitation, as manifested by levels of deforestation?

Empirical Approach

- In 2000, India began implementing the Panchayat Extension to Scheduled Areas (PESA) Act of 1996, staggered by state
- PESA extended local government to Scheduled Areas and implemented electoral quotas that required all chairpersons of local government councils (across three levels) and at least half of the council seats be reserved for ST members
- This paper uses the staggered implementation (by state) of elections with quotas for ST for local government councils resulting from the PESA law for a DID analysis to identify the law's effect on village-level deforestation rates.

Summary

Main findings:

- The implementation of elections with quotas for Scheduled Tribes reduced deforestation by 30% compared with the overall trend.
- The timing of the emergence of effects coincides with the implementation of PESA elections, supporting the relevance of electoral politics as a mechanism for influencing the pace of deforestation.
- The introduction of PESA narrows the gap in deforestation rates between villages closer to mines compared to villages farther from mines, suggesting that improved representation helps local communities resist mining interests.

Contributions to the Literature

Theoretical:

- Prior literature provides conflicting theoretical grounds for assessing the likely impact of decentralization on decisions regarding an environmental protection / resource extraction tradeoff
    - Some scholars argued that decentralization could worsen the tragedy of the commons due to stronger incentives for short-termism at local than central levels
    - Other scholars argue that empowering local leadership might strengthen environmental protection, since such communities rely heavily on the land, internalizing the externalities from natural resource extraction
- This paper provides evidence that one reason that the evidence on the efficacy of local political control on natural resource sustainability is mixed is because the impact of decentralization is mediated by the resulting representative institutions, including electoral processes, which may or may not empower traditionally marginalized groups.

Contributions to the Literature

Empirical:

- Achieves better causal inference than many previous studies by combining a high-resolution forest outcome dataset with a longitudinal, quasi-experimental research design.

Policy:

- In policy debates conservation and socio-economic development are frequently characterized as being at odds with one another.
- This paper presents evidence that overarching political institutions that empower marginalized indigenous groups can contribute to advancement along both desiderata simultaneously.
- This may suggest that such desiderata might in some contexts also be complements rather than substitutes.

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Replication: High-Level Feedback

- Using all of our computers, as a team we were able to run all sections of the R-files to replicate the overall analysis, but not every person’s computer was able to run every section of the code
- Some packages seemed to not run on macs while other packages seemed to not run on PCs
- The very large volume of data slowed down the replication process
  - Not a true push-button replication from scratch; we skipped the python scripts.
- A more detailed read_me file might be helpful to future reviewers

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Effective sample in spec 3 and 4

State-year FE reduces the sample to 4 states, best to make it obvious in table:

Table 1: Main Effects (Difference in Differences)

	Annual Deforestation in Hectares			
	(1)	(2)	(3)	(4)
Scheduled X PESA	-0.088^{***}	-0.032^{***}	-0.016^{**}	-0.067^{***}
	(0.008)	(0.010)	(0.007)	(0.012)
Village FE	✓	✓	✓	✓
Year FE	✓	✓		
Village TT		✓		✓
State X Year FE			✓	✓
Dep. Var. Mean	0.22	0.22	0.22	0.22
N. Villages	52776	52776	52776	52776
N	897,192	897,192	897,192	897,192

Original table

Main Effects (Difference in Differences)

Annual Deforestation in Hectares			
(1)	(2)	(3)	(4)
-0.088^{***}	-0.032^{***}	-0.016^{**}	-0.067^{***}
(0.008)	(0.010)	(0.007)	(0.012)
x	x	x	x
x	x		
	x		x
		x	x
0.22	0.22	0.19	0.19
52776	52776	31601	31601
897,192	897,192	537,217	537,217

Suggested change

Treatment effect by state (spec 4):

(Possibly useful addition to the appendix)

Table 2: Main Effects by State

	Annual Deforestation in Hectares			
	Chhattisgarh	Jharkhand	Maharashtra	Odisha
	(1)	(2)	(3)	(4)
Scheduled X PESA	0.022	-0.082^{***}	-0.088^{**}	-0.094^{***}
	(0.029)	(0.013)	(0.041)	(0.018)
Dep. Var. Mean	0.2	0.08	0.12	0.27
N. Villages	4440	5015	7783	14363
N	75,480	85,255	132,311	244,171

$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$
Cluster-Robust Standard Errors (by village)

Intensive vs extensive margin

Some more summary stats would really help. Level of zeroes in the dependent var in text:

```
gen def_0 = deforestation == 0
```

def_0	Freq.	Percent	Cum.
0	129,863	14.47	14.47
1	767,329	85.53	100.00
Total	897,192	100.00	

Count of zero deforestation years at village level:

```
egen mean_zero = mean(def_0), by(village)
tab mean_zero if tag_vill
```

mean_zero	Freq.	Percent	Cum.
0	207	0.39	0.39
.0588235	262	0.50	0.89
.1176471	295	0.56	1.45
.1764706	385	0.73	2.18
.2352941	488	0.92	3.10
.2941177	572	1.08	4.19
.3529412	692	1.31	5.50
.4117647	804	1.52	7.02
.4705882	954	1.81	8.83
.5294118	1,146	2.17	11.00
.5882353	1,365	2.59	13.59
.6470588	1,621	3.07	16.66
.7058824	1,824	3.46	20.11
.7647059	2,381	4.51	24.62
.8235294	3,274	6.20	30.83
.8823529	4,845	9.18	40.01
.9411765	8,286	15.70	55.71
1	23,375	44.29	100.00
Total	52,776	100.00	

Main results table with dummy outcome instead:

Table 3: Main Effects (Difference in Differences)

	Indicator for no deforestation			
	(1)	(2)	(3)	(4)
Scheduled X PESA	0.008***	−0.029***	0.009***	0.016***
	(0.002)	(0.003)	(0.003)	(0.004)
Village FE	x	x	x	x
Year FE	x	x		
Village TT		x		x
State X Year FE			x	x
Dep. Var. Mean	0.86	0.86	0.83	0.83
N. Villages	52776	52776	31601	31601

Under the hood: Table 4: Main Effects by State

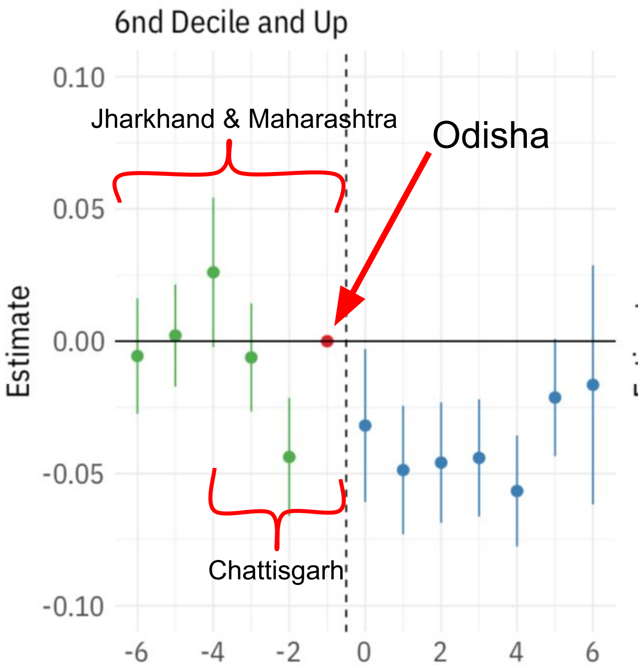
	Indicator for no deforestation			
	Chhattisgarh	Jharkhand	Maharashtra	Odisha
	(1)	(2)	(3)	(4)
Scheduled X PESA	-0.062^{***}	0.071^{***}	-0.023^{***}	0.037^{***}
	(0.010)	(0.008)	(0.009)	(0.007)
Dep. Var. Mean	0.81	0.88	0.9	0.78
N. Villages	4440	5015	7783	14363
N	75,480	85,255	132,311	244,171

Dominance of Odisha gives the overall positive coeff. Maharashtra interesting on the extensive/intensive margin split result:

Scheduled X PESA	0.022	-0.082^{***}	-0.088^{**}	-0.094^{***}
	(0.029)	(0.013)	(0.041)	(0.018)

Figure 6: composition of sample differs across time

Disclaimer: wasn't able to run thi[s] file on mac bc of R package issues



Best to split these up by state as well?

(coming later)

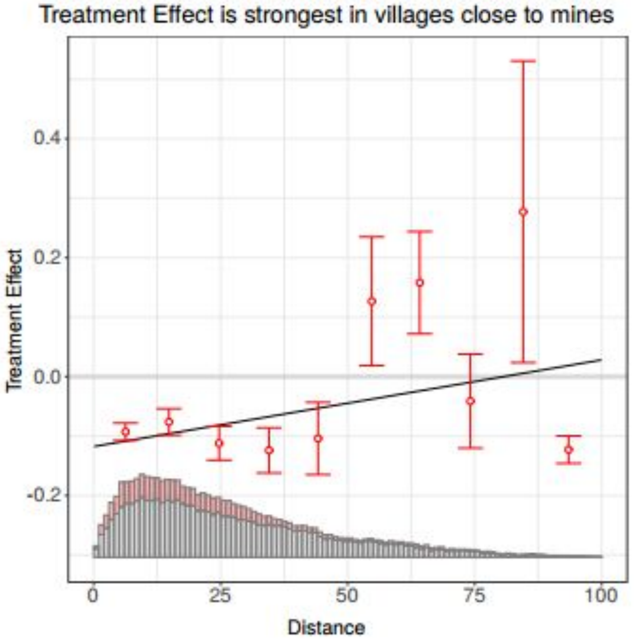
Treatment Effect and Distances to Mining

Table 2: Treatment Effects on Annual Deforestation by Distance to Nearest Mine

	Annual Deforestation in Hectares			
	(1)	(2)	(3)	(4)
Scheduled X PESA X 1st Tercile	-0.093^{***}	-0.043^{***}	-0.025^{**}	-0.079^{***}
	(0.010)	(0.014)	(0.010)	(0.015)
Scheduled X PESA X 2nd Tercile	-0.103^{***}	-0.034^{**}	-0.026^{**}	-0.077^{***}
	(0.012)	(0.016)	(0.011)	(0.016)
Scheduled X PESA X 3rd Tercile	-0.052^{***}	-0.004	0.018	-0.031
	(0.017)	(0.026)	(0.017)	(0.025)
Village FE	✓	✓	✓	✓
Year FE	✓	✓		
Village TT		✓		✓
State X Year FE			✓	✓
Dep. Var. Mean	0.22	0.22	0.22	0.22
N. Villages	52776	52776	52776	52776
N	897,192	897,192	897,192	897,192

$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$
Cluster-Robust Standard Errors (by village)

***Panel B**: Treatment effects on Annual Deforestation by Distance to Mines*



Censoring Cases by Forest Rates: Is it Random?

- “We subset ... to villages with an ex-ante forest cover index of 2 or more out of a possible 100 ... which we label as ‘forested’”
- Is previous forest coverage independent from distances to mining (and the deforestation rates)?

- Possibly, a closer distance to mines correlates with higher foresting rates
- Censoring cases by a cutoff equals selecting cases by a covariate, eliminating observations with close distances to mines (or other variables, like previous forest rates). The estimation of treatment effects may thus be biased (MNAR)

Censoring Cases by Forest Rates (Cont.)

```
Call:
lm(formula = pref_mean ~ min_dist_to_mine, data = df)

Residuals:
   Min     1Q Median     3Q    Max 
-3.915 -3.479 -3.025 -1.774 85.362 

Coefficients:
                  Estimate Std. Error t value Pr(>|t|)    
(Intercept)       3.916465   0.005845   670.0   <2e-16 ***
min_dist_to_mine -2.144498   0.012217  -175.5   <2e-16 ***
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 9.248 on 4877468 degrees of freedom
Multiple R-squared:  0.006278,	Adjusted R-squared:  0.006278 
F-statistic: 3.081e+04 on 1 and 4877468 DF,  p-value: < 2.2e-16
```

- This problem exists whenever selecting cases based on forest rates, regardless of the cutoff

Censoring Cases by Forest Rates (Cont.)

- The treatment effect may change across previous forest rates
- Does it harm internal validity of the analysis?
- Heterogeneous treatment effect by distance or previous forest rates?
- Consistent estimations?

Table 8: Treatment Effects on Annual Deforestation by Previous Forest Rates

	Annual Deforestation in Hectares			
	(1)	(2)	(3)	(4)
Treatment	-0.119^{***}	0.008	-0.038^{***}	-0.037^{***}
	(0.008)	(0.012)	(0.008)	(0.013)
Treatment*Prev Forest Rate	0.003^{***}	-0.003^{***}	0.002^{***}	-0.002^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes		
Village TT		Yes		Yes
State X Year FE			Yes	Yes
Dep. Var. Mean	0.22	0.22	0.22	0.22
N. Villages	52776	52776	52776	52776
N	897,192	897,192	897,192	897,192

*p < .1; **p < .05; ***p < .01

Cluster-Robust Standard Errors (by village)

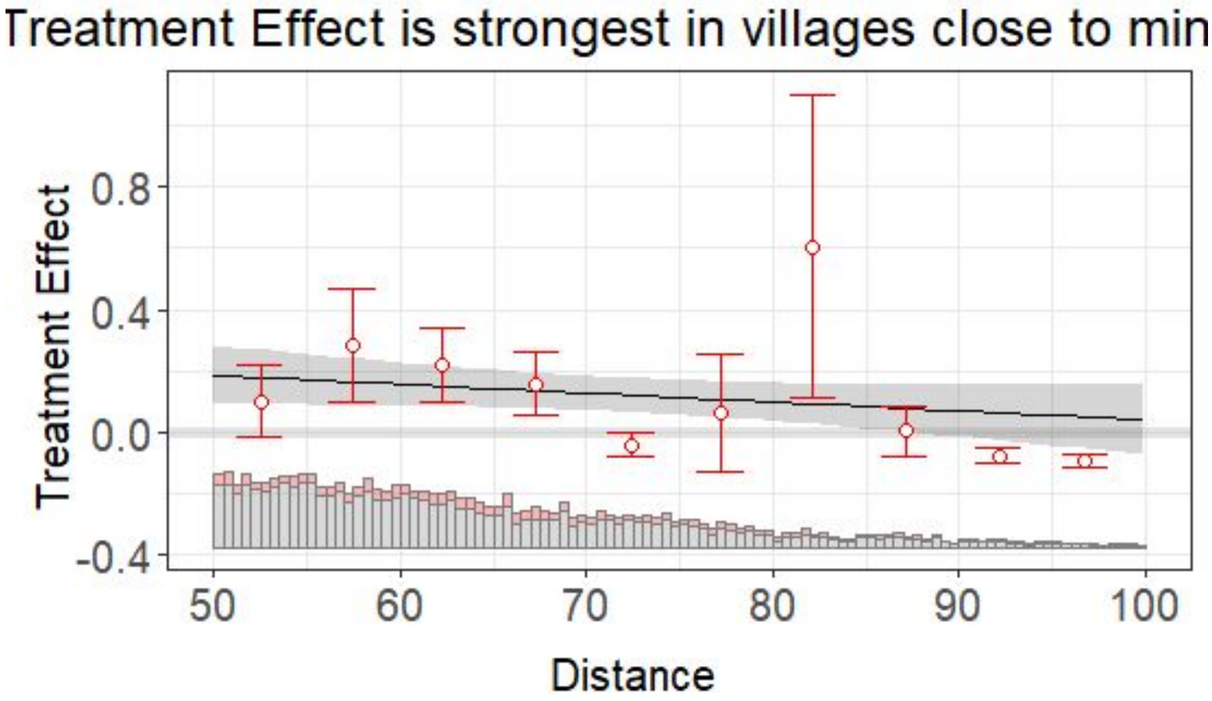
Cutting Distances into Two Ranges

- The Wald test, as is suggested by Hainmueller et al. (2018), has a significant result. This outcome indicates that the interaction effect is non-linear

- It looks like cases from 0-50km are hugely different from those in 50-100km

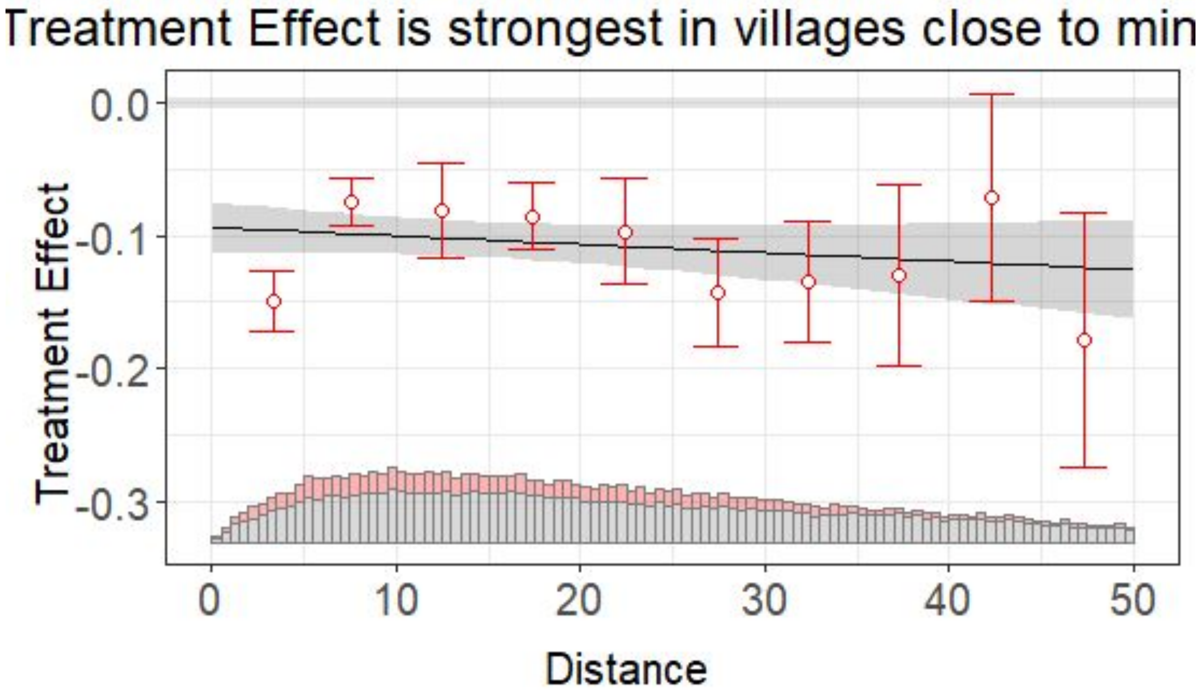
- Does that difference drive the interaction effect? I.e., no or weak effect for both 0-50km and 50-100km, while aggregating them leads to significance

Cutting Distances into Two Ranges (Cont.)



- Positive (and significant) treatment effects?
- Inconsistent results across 50-100km

Cutting Distances into Two Ranges (Cont.)



- Negative but improving treatment effects
- The 2nd to 10th bins are not significantly different from each other

Cutting Distances into Two Ranges (Cont.)

- Interaction coefficients are generally insignificant for 5-50km cases, which corresponds to the interaction plot

Table 6: Treatment Effects on Annual Deforestation by Distance to Nearest Mine (5km-50km)

	Annual Deforestation in Hectares			
	(1)	(2)	(3)	(4)
Treatment	-0.058^{***}	-0.063^{***}	-0.001	-0.077^{***}
	(0.015)	(0.021)	(0.016)	(0.022)
Treatment*Distance	-0.002^{***}	0.001	-0.001^{*}	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes		
Village TT		Yes		Yes
State X Year FE			Yes	Yes
Dep. Var. Mean	0.22	0.22	0.22	0.22
N. Villages	52776	52776	52776	52776
N	715,309	715,309	715,309	715,309

$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$
Cluster-Robust Standard Errors (by village)

Cutting Distances into Two Ranges (Cont.)

- Interacting Treatment with a binary variable: distances to mines below or over 5km
- Significant interaction coefficients
- It is the gap between 0-5km and 5-50km that drives the negative interaction effects

Table 7: Treatment Effects on Annual Deforestation by Distance to Nearest Mine (5km below or above)

	Annual Deforestation in Hectares			
	(1)	(2)	(3)	(4)
Treatment	-0.154^{***}	-0.083^{***}	-0.091^{***}	-0.107^{***}
	(0.014)	(0.021)	(0.014)	(0.021)
Treatment*Binary_5km	0.057^{***}	0.051^{**}	0.065^{***}	0.059^{***}
	(0.015)	(0.023)	(0.014)	(0.021)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes		
Village TT		Yes		Yes
State X Year FE			Yes	Yes
Dep. Var. Mean	0.22	0.22	0.22	0.22
N. Villages	52776	52776	52776	52776
N	776,118	776,118	776,118	776,118

$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Cluster-Robust Standard Errors (by village)

Relaxing the Linearity Assumption

- Using distance assumes a linear interaction effect, which is unlikely to hold
- The inverse of distance

Table 3: Treatment Effects on Annual Deforestation by Proximity to Nearest Mine

	Annual Deforestation in Hectares			
	(1)	(2)	(3)	(4)
Treatment	-0.078^{***}	-0.022^{*}	-0.006	-0.057^{***}
	(0.009)	(0.012)	(0.008)	(0.013)
Treatment*Inverse_Distance	-0.001^{***}	-0.001^{*}	-0.001^{***}	-0.001^{*}
	(0.0003)	(0.0005)	(0.0003)	(0.0005)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes		
Village TT		Yes		Yes
State X Year FE			Yes	Yes
Dep. Var. Mean	0.22	0.22	0.22	0.22
N. Villages	52776	52776	52776	52776
N	897,192	897,192	897,192	897,192

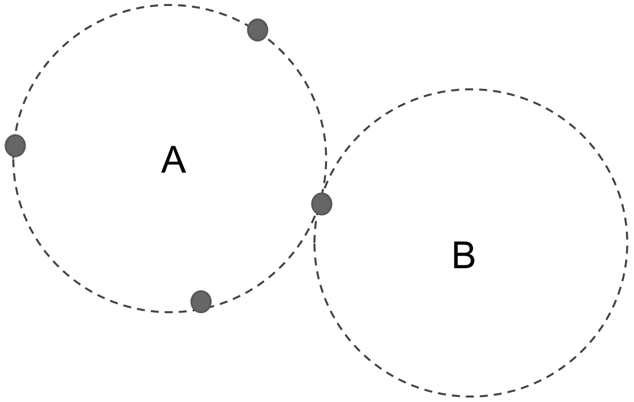
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$
Cluster-Robust Standard Errors (by village)

Alternative measure of proximity to mines

The current measure (distance to nearest mine) is instructive and naturally relevant, but might suppress potentially useful heterogeneity that can further tease out the underlying mechanism we are interested in.

A simple and reductive example is shown on the right: the existing strategy will encode both villages A and B to have the same measure for proximity to mines, but the market-induced pressures will be substantially higher in A than in B.

Instead, we propose an alternative measure: the number of mines within bands of X km.



Villages A and B
Bands of radii X km
Grey dots represent mines

Alternative measure of proximity to mines

We ran the same specification as in Table 2, using a version of our alternative measure for proximity instead.

Due to time and data capacity constraints, we could only count the number of mines of different minerals, not all mines in total. The trends here should be expected to only be reinforced with the full dataset.

	Annual Deforestation in Hectares			
	(1)	(2)	(3)	(4)
Scheduled X PESA X 1st Tercile	-0.093^{***}	-0.043^{***}	-0.025^{**}	-0.079^{***}
	(0.011)	(0.014)	(0.011)	(0.016)
Scheduled X PESA X 2nd Tercile	-0.103^{***}	-0.034^{**}	-0.026^{**}	-0.077^{***}
	(0.013)	(0.017)	(0.011)	(0.017)
Scheduled X PESA X 3rd Tercile	-0.052^{***}	-0.004	0.018	-0.031
	(0.018)	(0.027)	(0.017)	(0.027)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes		
Village TT		Yes		Yes
State-Year FE			Yes	Yes
Dep. Var. Mean	0.22	0.22	0.22	0.22
N Villages	52776	52776	52776	52776
N	897,192	897,192	897,192	897,192

Existing version of Table 2

Alternative measure of proximity to mines

We ran the same specification as in Table 2, using a version of our alternative measure for proximity instead.

Due to time and data capacity constraints, we could only count the number of mines of different minerals, not all mines in total. The trends here should be expected to only be reinforced with the full dataset.

	Annual Deforestation in Hectares			
	(1)	(2)	(3)	(4)
Scheduled X PESA X 1st Tercile	−0.093***	−0.043***	−0.025**	−0.079***
	(0.011)	(0.014)	(0.011)	(0.016)
Scheduled X PESA X 2nd Tercile	−0.103***	−0.034**	−0.026**	−0.077***
	(0.013)	(0.017)	(0.011)	(0.017)
Scheduled X PESA X 3rd Tercile	−0.052***	−0.004	0.018	−0.031
	(0.018)	(0.027)	(0.017)	(0.027)
Scheduled X PESA X 1st Tercile	−0.042**	−0.194***	0.008	−0.150***
	(0.021)	(0.041)	(0.021)	(0.040)
Scheduled X PESA X 2nd Tercile	−0.058***	0.022	0.014	−0.0003
	(0.009)	(0.015)	(0.009)	(0.017)
Scheduled X PESA X 3rd Tercile	−0.130***	−0.050***	−0.066***	−0.107***
	(0.013)	(0.016)	(0.012)	(0.018)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes		
Village TT		Yes		Yes
State-Year FE			Yes	Yes
Dep. Var. Mean	0.22	0.22	0.22	0.22
N Villages	52776	52776	52776	52776
N	897,192	897,192	897,192	897,192

Distance to closest mine

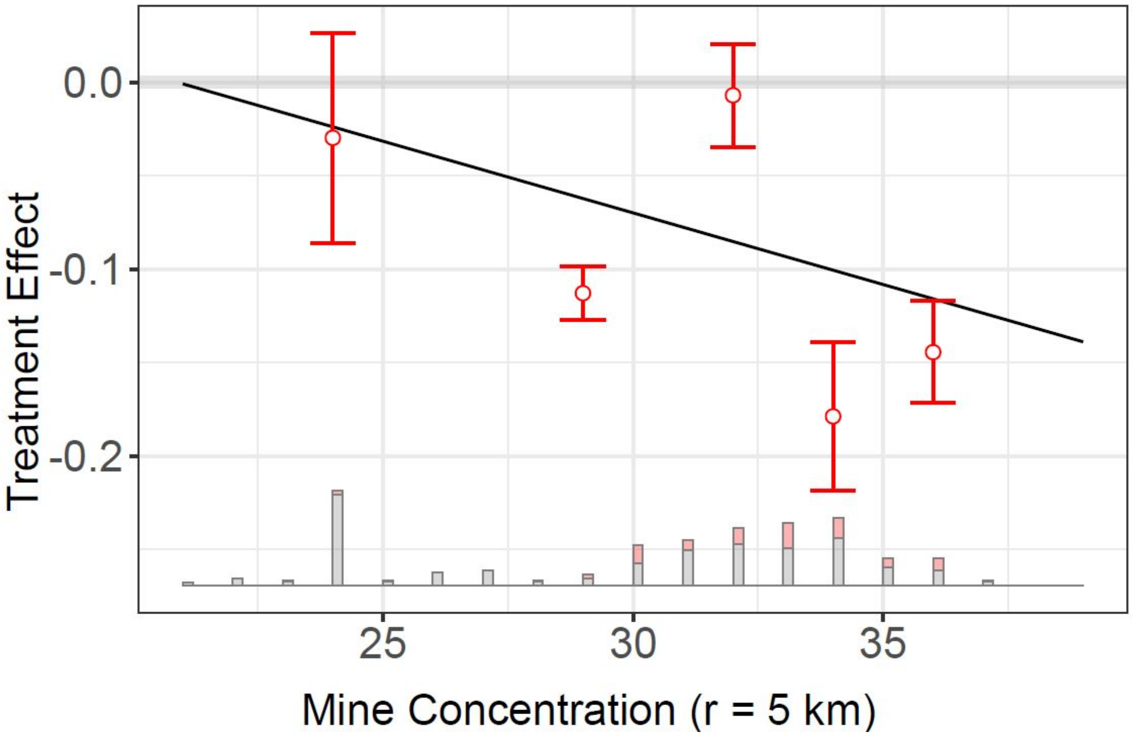
Mine concentration (Tercile 1 is least concentrated)

Proposed alternative measure

Alternative measure of proximity to mines

An additional benefit could be more mass across the distribution of villages, allowing us to better model the functional form of the relationship.

These results don't seem to be sensitive to the choice of the radius (we've checked for robustness at 3, 5, 10, 15, 20 and 25 km), but the variation does decrease at larger radii.



Does attenuation vary across early vs. late adopters?

The event study model lends evidence to the mechanism that effects start to attenuate around the 5 year mark, which indicates that village councils are rendered ineffective towards the end of their first terms.

It might be helpful to compare early and late adopters, using the fact that we observe more election cycles for certain states.

This kind of comparison is not meant to be robust, given the small number of states we can do this for, but should still be helpfully indicative.

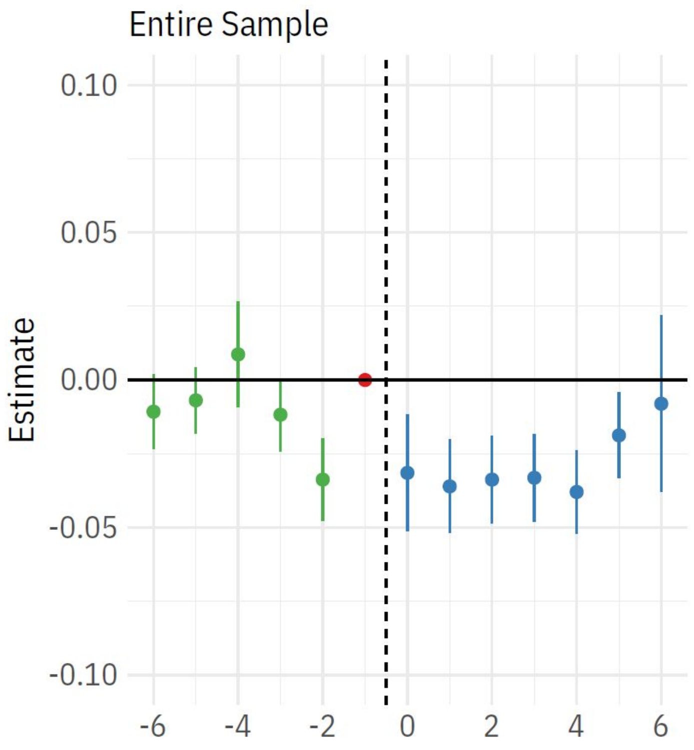
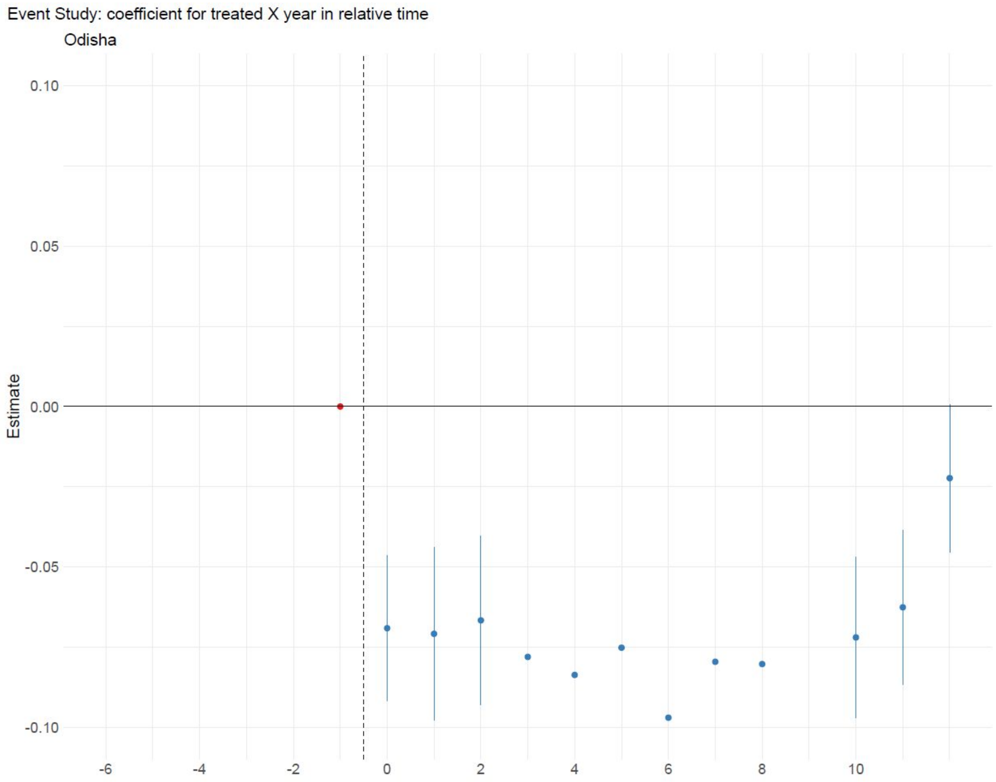


Figure 6 from paper

Does attenuation vary across early vs. late adopters?

We do not see similar patterns of attenuation in Odisha, which is the earliest adopter among the time-varying states in the sample.

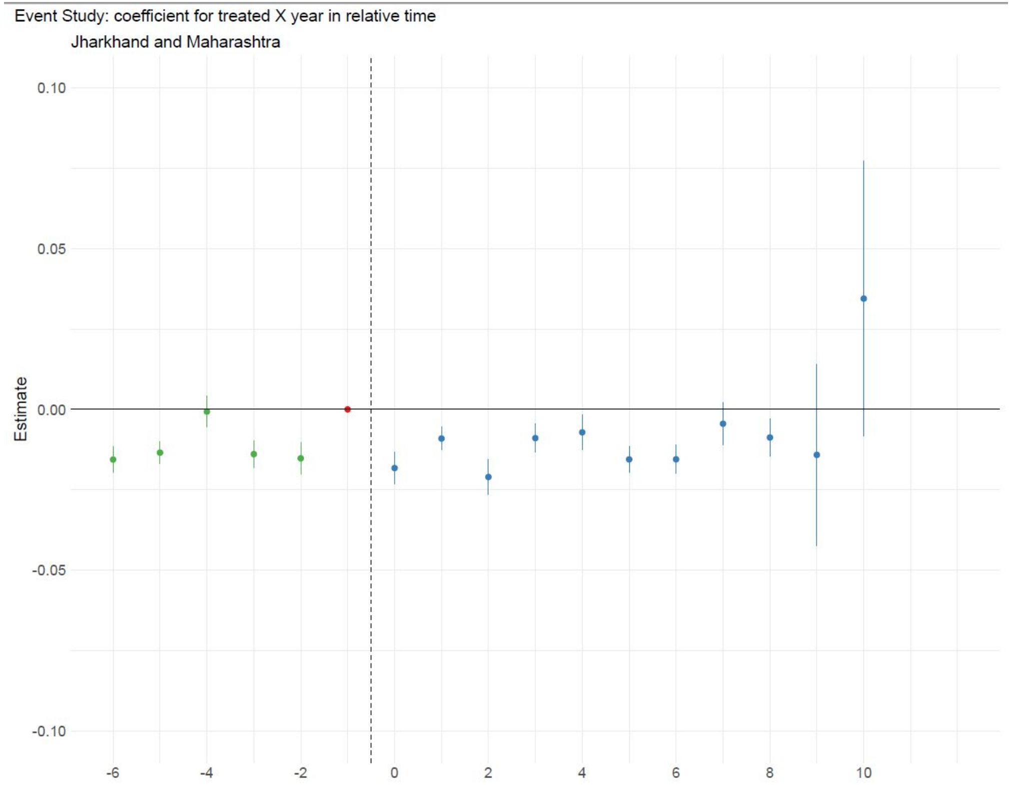
There are slight movements towards and away from 0 before and after the elections, but the dominant trend more likely speaks to the floor effects explanation given in the text.



Does attenuation vary across early vs. late adopters?

The patterns in the overall results tend to be driven by Jharkhand and Maharashtra, where we do observe this attenuation around the 5-year mark.

Aggregate effects in these states are lower than in Odisha - the floor effects hypothesis is difficult to delineate with the current setup.

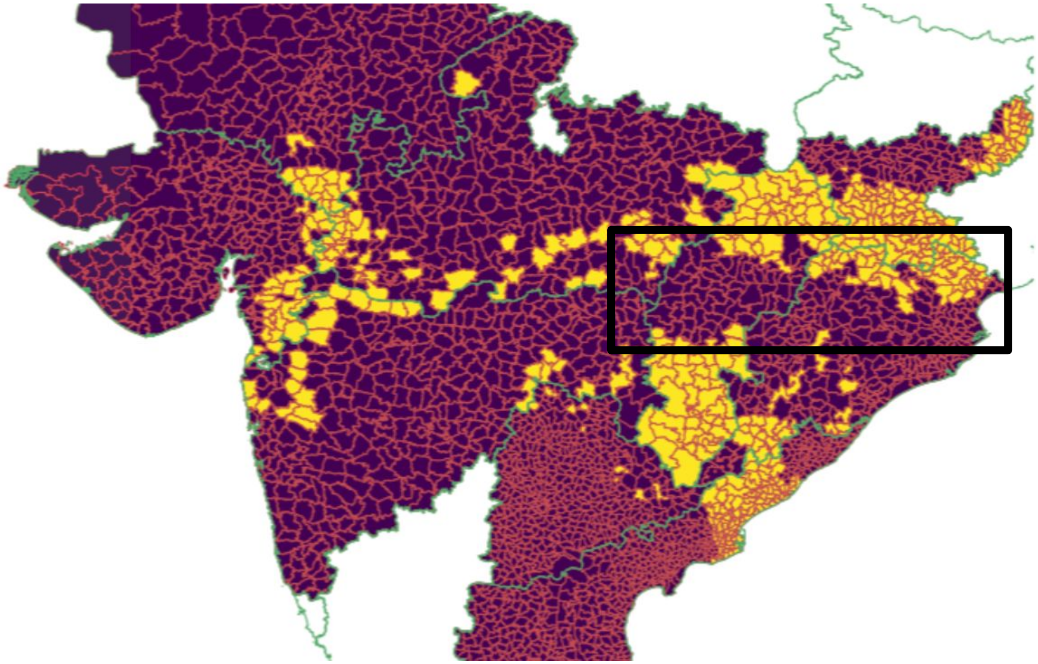


Does attenuation vary across early vs. late adopters?

One proposal to establish the electoral co-option mechanism might be to look at **learning across state borders**: Jharkhand and Odisha seem like they are good candidate states.

Elites with interests in village councils where PESA hasn't been implemented yet (Jharkhand) should presumably learn from neighboring states' village councils where PESA has been implemented (Odisha).

We haven't been able to implement this given our constraints but wanted to suggest and discuss it nevertheless.



Treatment allocation: two discussion points

(1). Given that the *de facto* treatment status has been allocated at the block level, even though villages are the *de jure* units of program implementation, how have you thought about the level at which to cluster?

a. There are competing considerations, including spatial correlation (Conley errors), and the fact that the village is the unit of policy and theoretical interest

b. However, our understanding of the methods literature on diff-in-diff is that clustering is implemented at the stable unit of treatment assignment (Bertrand et al 2004, might be helpful to add a footnote explaining the rationale for deviation from this)

Treatment allocation: two discussion points

(2). Your point about biasing effects towards zero by allocating treatment at the block level and thus being conservative on effect size seems extremely credible, but it would still be extremely useful to be able to see what's happening within the blocks.

a. It will be helpful to see to what extent commercial interests direct their focus towards non-scheduled villages with substantial forest resources, and whether there is heterogeneity in blocks that are saturated by scheduled areas to different extents, i.e. is long-term conservation more prevalent in blocks that are 50% scheduled vs. 10% scheduled?

b. To this end, it might be incredibly instructive to see this heterogeneity in an extremely small sub-sample in a given state. There will be constraints to generalizability out of this small sub-sample, but it will help nail down the overall picture a lot better.

Does Level of Clustering of Standard Error Matter?

Table 4: Treatment Effects on Annual Deforestation by Distance to Nearest Mine (Block-level SE)

	Annual Deforestation in Hectares			
	(1)	(2)	(3)	(4)
D_mine_1	-0.093^{***}	-0.043^{*}	-0.025	-0.079^{***}
	(0.023)	(0.023)	(0.021)	(0.030)
D_mine_2	-0.103^{***}	-0.034	-0.026	-0.077^{**}
	(0.028)	(0.035)	(0.024)	(0.038)
D_mine_3	-0.052	-0.004	0.018	-0.031
	(0.041)	(0.068)	(0.039)	(0.065)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes		
Village TT		Yes		Yes
State X Year FE			Yes	Yes
Dep. Var. Mean	0.22	0.22	0.22	0.22
N. Villages	52776	52776	52776	52776
N	897,192	897,192	897,192	897,192

$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Does Level of Clustering of Standard Error Matter? (Cont.)

Table 5: Treatment Effects on Annual Deforestation by Proximity to Nearest Mine (Block-level SE)

	Annual Deforestation in Hectares			
	(1)	(2)	(3)	(4)
D	-0.078^{***}	-0.022	-0.006	-0.057^{*}
	(0.021)	(0.031)	(0.019)	(0.034)
D:mine_dist_inv	-0.001	-0.001	-0.001^{*}	-0.001
	(0.0005)	(0.001)	(0.0004)	(0.001)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes		
Village TT		Yes		Yes
State X Year FE			Yes	Yes
Dep. Var. Mean	0.22	0.22	0.22	0.22
N. Villages	52776	52776	52776	52776
N	897,192	897,192	897,192	897,192

$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Minor points on context and theory (1/2)
(that might be beyond the scope of the paper)

- "Development and conservation need not be substitutes." How should we think about the counterfactual for this policy change?
- How generalizable is this particular kind of "development" to other contexts?
  - This is probably discussed more in the other paper that focuses on economic and development outcomes, but would this sort of institutional change work in a context where the state is not available to provide workfare guarantees and absorb labor that would otherwise go to the private sector?
  - I would be curious to know how much of the local political economy is shaped by increasing costs to mining operations: are improvements in NREGS simply a by-product of more demand among workers (and increased institutional pressures), stemming from simultaneous falls in mining jobs?
  - What are the effects on in and out migration among communities that have historically depended on forest resources?
- Given the large degree of heterogeneity in ST population shares, and the fact that even in scheduled areas ST tend to make less than 50% of the population, additional outcomes on ST's political engagement would be extremely interesting.
  - Is it possible to draw lines from here to statewide party coalitions between national and regional parties, or the changing rates of tribal candidates being fielded in non-tribal state and federal constituencies?

Minor points on context and theory (2/2)
(that might be beyond the scope of the paper)

- It'll be helpful to have some understanding of how the village councils operationally interact with the state/commercial interests that are pushing towards deforestation. Do councils effectively have veto power?

- How should we pick the right unit of analysis in empirical work where the outcome is necessarily tied to geographical units?
    - Villages/blocks are non-uniform real world units, while grids are uniform (in area at least) but do not correspond to anything "real".

- Is there a way to test for possible spillover effects from scheduled areas to non-scheduled areas?

Minor points to look into later

No need to discuss, I also could be wrong on these

- Doesn’t seem like quadratic time trends are included in the regression?
- Numbers in text on top of page 18 don’t appear to be consistent with figure 3
- Claim about the percentile cutoff on bottom of page 18 seems inconsistent with the data

Appendix: Granular comments on replication

- Packages “Stargazer2” and “Interflex” in “LalRUtils” may not be available for R 4.0.0; have to replace with “Stargazer” and the github version of “Interflex”
- Formula 5.4 on page 22: it seems that the time fixed effects (gamma_t) is a typo and might not be included in the regressions
- The notes of Figure 5 on page 25: it seems that the figure estimates Specification 5.4 rather than 5.3, so it looks like a typo
- Tables A1 and A2 on page 42: the estimates are very slightly different from the paper. For instance, the mean for “# households” in Table A1 is 169.134 in replication, not 169.100 in the original table
- Figure A2 on page 46: the legend overlaps with the lower right part of the graph
- 7_Estimation R file: Line 232: #create Intermediate subfolder in "imp" folder before following lines, & copy this file into the intermediate subfolder from the tmp folder: df = import(file.path(data, 'Intermediate/est_clean2.rds')) %>% setDT
- 8_Dyn_effects: for mac users, fect package wouldn’t install from install.packages nor for install_github
- 8_Summary_Plots: for mac users, ggstatsplot doesn’t seem to work