Political Decentralisation and Forest Conservation: Evidence from India's Scheduled Areas

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Abstract

Natural resource management decision-making has been decentralised significantly in the developing world, despite theoretical ambiguity in whether such a move would improve preservation or accelerate depletion. Using a difference-in-differences design, we study the effects of the staggered implementation of a 1996 law that implemented elected local government councils in Scheduled Areas in 8 states in India on deforestation rates as measured using remote-sensing data. We find that the extension caused a large decrease in the rate of deforestation, and find suggestive evidence that these effects are concentrated in areas with greater mining potential, and where ethnic groups with strong resource-preservation norms are an electoral plurality. Our findings suggest that devolution of political power to local communities that can also enforce sustainable resource-use norms can appreciably aid the preservation of forest ecosystems, even in areas where state capacity is weak.

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1 Introduction

The future trajectory of the earth's climate likely depends, to a large extent, on our ability to preserve forest ecosystems. In addition to limiting our capacity to stem the rate of change of the earth's climate, deforestation also leads to loss of biodiversity (Bonan 2008), and accounts for 12%-20% of annual greenhouse gas emissions, which is more than all transport combined (Solomon et al. 2007). While this point has been emphasised extensively in the global policy agenda (N. H. Stern 2006), it presents an unresolved global and local collective action problem: logging needs to be kept at or below a sustainable level, but it a source of livelihood for those living in close proximity. This problem is especially pronounced in developing countries, where the rate of deforestation has been most rapid and conservation efforts are limited by weak state capacity in the forests, which are de-facto hinterlands. This paper explores whether the design of political institutions can help allay the problem. Specifically, we study if giving rights to forest produce to local communities has the power to stem resource-overuse, and find that the introduction of elected local-government institutions to areas with 'scheduled tribes' substantially decreased the rate of deforestation.

There are strong reasons to be suspect a weak or null effect of state policy on environmental and natural resource outcomes, especially in the developing world. Forests are de-facto hinterlands even in the developed world, and there is scant empirical evidence suggesting natural resource policies made by the government is consequential for resource extraction. Burgess, Costa, and Olken (2019) provide some of the first credible empirical evidence that state policies do affect even the remote wilderness by examining variation in forest density at the Brazil's international borders in the Amazon over time and documenting a large slowdown in deforestation in Brazilian pixels following a national policy in 2006 as well as a large increase in 2014 onwards following an economic crisis and laxer regulation, which only deteriorated since. Even if we believe that state policy matters for environmental outcomes, the comparison and design of optimal institutions and delegation for such policies isn't well studied. One of the ways that states choose to implement such policies is by delegating resource preservation to lower-tiers of government, sometimes as small as community-level institutions. Decentralisation drives, in the form of community forestry and the like, have been championed by NGOs and development agencies throughout the developing world with seemingly scant credible evidence of their effectiveness in curbing over-utilisation.

Theoretically, the effects of decentralisation on resource use are ambiguous. On the one hand, many environmental studies scholars show considerable enthusiasm in favour of decentralisation, citing its benefits as arising from the congruence between the unit affected by resource-use decisions and those making them. This position is concisely articulated by Lemos and Agrawal (2006) argue that decentralisation may facilitate preservation through three channels: "It can produce greater efficiencies because of competition among sub-national units; bring decision making closer to those affected by governance, thereby promoting higher participation and accountability; and it can help decision makers take advantage of more precise time- and place-specific knowledge about natural resources." On the other hand, increasing the number of 'sellers' in a market through decentralisation could potentially exacerbate the 'tragedy of the commons' problem: in a Cournot setup, an increase in the number of firms leads to lower prices of forest produce and increase quantity produced (that is, increase deforestation) (Burgess, M. Hansen, et al. 2012). The sign of the net effect, therefore, is theoretically ambiguous yet important for public policies that aim to improve conservation.

The effects of political changes on environmental outcomes are relatively under-studied because of both the paucity of data and credible designs. Early work in this vein, such as sections of seminal books by Ostrom (1990) and Ellickson (1991), relied on detailed case-studies and fieldwork in small communities, which gave us valuable insights into mechanisms but had unclear general implications. Reliable data with global coverage on deforestation and similar environmental measures have only become available to social scientists recently following the publicly available remote-sensing data from satellites such as LANDSAT, Sentinel, DMSP, and so on. We introduce the use of one such dataset and invite researchers to make more use of large scale, high throughput datasets produced by environmental scientists and geographers. On the research design front, the difficulty of estimating causal effects arises from the fact that environmental decentralisation drives are either selectively implemented in areas that are often trending differently from the rest of the country (which often don't have comparable resource bases, thereby making it difficult to construct a suitable counterfactual), or implemented simultaneously country-wide, which leaves one with no 'control' units. As a result, cross-sectional regression/matching based approaches are unlikely to recover a credible causal effect of decentralisation.

The inferential challenges mean that we must rely on natural experiments that either create new administrative units or devolve political power to existing ones. In an influential paper, Burgess, M. Hansen, et al. (2012) use district splits in Indonesia between 2000 and 2008 to study the effects of decentralisation on deforestation (as measured through NDVI, an early LANDSAT derived measure of deforestation) and find that adding one more district increased deforestation rate in the province by 8.2%. We follow Burgess et al in the use of remote sensing imagery to measure deforestation and use a novel source of institutional variation - the institution of village governments in India's 'scheduled' (tribal) areas - to evaluate its effects on deforestation.

India is an important context to study the effects of decentralisation on deforestation, since its massive population is predominantly rural,[1] and a large share of it, especially from the scheduled tribes, depends on forests for their livelihood.[2] The rate of deforestation in India has also been rapid

[1] Approximately 66% (*World Bank* 2017)

[2] Globally, about 447 million people may depend on forests in India, Indonesia, Nepal, the Philippines, Sri Lanka, and Thailand alone (Lynch and Talbott 1995).

thanks to population growth and industrialisation; forest cover has decreased from roughly 40% in the 1850s to less than 20% in the 1990s (Lynch and Talbott 1995).

There also exists substantial devolution of power to states, districts within them, and the sub-districts (tehsils) in India. The 73rd amendment to the Indian Constitution (1993) extended this decentralisation further by devolving a variety of powers over "the preparation of economic development plans and social justice ... implementation in relation to 29 subjects [3] listed in the 11th schedule of the constitution, and the ability to levy and collect appropriate taxes, to the panchayats" (*Constitution of India, Seventy-Third Amendment* 1992). The Panchayats have been extensively studied by scholars of political selection because of the randomisation of gender and demographic quotas, which was used to study the effects of female leadership by a string of papers beginning with Chattopadhyay and Duflo (2004). However, their extension to scheduled areas, which followed a more protracted process , has received scant attention.

The provisions of the 74rd amendment were nominally extended to places where the vast majority of tribal populations (officially known as *Scheduled Tribes*; 11% of the Indian Population (Census 2011)) in India reside – Scheduled Areas, as listed in 5th schedule[4] – by the Panchayat Extension to Scheduled Areas Act of 1996 (Sudipta 2015), but the implementation was left up to the respective states. The key clauses of the PESA act for our purposes are that of introducing direct election to Gram Panchayat (village councils) and the reservation of seats in said panchayat based on proportion of scheduled tribes[5] in 5th Scheduled Areas. These reforms were adopted in a gradual fashion by the nine states because of legal battles, notably for the newly created state of Jharkhand (which was carved out of Bihar in 2000), where a protracted legal battle resulted in the first panchayat elections in scheduled areas to be delayed to 2010 (Jharkhand SC, 2010).

The decentralisation movement that gives rise to the natural experiment we analyze is, therefore, comprises: (i) political decentralisation through extension of the 73rd 'Panchayati Raj' amendment (Singh 1994) that introduced local government to scheduled areas where there was none, and (ii) political reservations for Scheduled Tribes (henceforth STs) in 5th schedule areas in accordance with PESA when this local government was introduced. These 'treatments' are perfectly correlated: scheduled areas are defined by, among other things, presence of tribal population, and therefore villages that received PESA extensions all elected members of STs to the *panchayats*. The control units within

[3]The 29 listed subjects in the original extension included forest related subjects, such as social forestry and farm forestry, and minor forest produce, which gave the village council substantial powers over forests. While this was of little import to the original set of villages, they are highly salient in scheduled areas.

[4]The fifth schedule was based on the following criteria: preponderance of tribal population; compactness and reasonable size of the area; under-developed nature of the area; and marked disparity in economic standard of the people. (Sudipta 2015)

[5]"The reservation of seats in the Section Scheduled Areas at every Panchayat shall be in proportion to the population of the communities in that Panchayat for whom reservation is sought to be given under Part IX of the Constitution; Provided that the reservation for the Scheduled Tribes shall not be less than one-half of the total number of seats" (Rath 2013)

states (our preferred specification include state-year fixed effects), villages in nearby non-scheduled areas, already had local-elections through the original 73rd amendment, which gives us an estimate of the reservations. In summary, we will estimate combined effect of political decentralisation (the first year of gram panchayat elections) and reservations for Scheduled Tribes (scheduled areas) on deforestation, and posit that the joint effect is of interest.

We leverage this state-level staggered adoption of the gram-panchayat election system for a difference-in-differences design that identifies the causal effect of the PESA gram-sabha election on deforestation based on the comparison of within-village variation in the rate of deforestation over time across scheduled and non-scheduled areas. To our knowledge, this is the first paper to estimate the effects of decentralisation on natural resource preservation in the developing world using quasi-experimental variation in local government policy. We find effects in the order of an 8% decline in the rate of deforestation in our preferred specification (column 4 in table 3), which is a statistically and economically meaningful decrease in the rate of deforestation.

The rest of the paper is organised as follows: 2 outlines a conceptual framework for the problem, section 3 provides further institutional details, section 4 describes the various data sources and the construction of our analysis sample, section 5 describes our empirical strategy, section 6 reports our results and contains a brief discussion, section 7 reports some extensions and robustness checks that we perform to validate our results, and section 8 concludes with future extensions for this project.

2 Conceptual Framework

In this section, we discuss various strands of the theoretical literature in environmental governance and economics that discuss the potential effects of decentralisation on resource conservation.

A strong argument for why decentralisation may worsen resource preservation, both theoretically and empirically, is provided in Burgess, M. Hansen, et al. (2012). The authors model the market for illegal timber as a Cournot oligopoly, where districts compete on the amount of logging permits to issue to firms, which they decide on independently, taking other districts' output as given. As a result, an increase in the number of districts increases the quantity of wood felled and decreases equilibrium prices in wood markets, as predicted by the canonical Cournot model. The intuition here is similar to the tragedy of the commons, with the externality component embedded in price declines that firms face as a consequence of the increase in quantity. A key assumption here, which the authors emphasise, is that "firms need permission from only one district to conduct illegal logging. If, alternatively, firms needed to bribe multiple districts to log, the predictions would be quite different ... if firms needed to bribe every district between the source of the logs and the destination in order to transport logs, then increasing the number of districts could increase total bribes and decrease logging, as in Olken and Barron (2009)".

There are also some broad reasons why decentralisation may improve resource preservation. Many environmental governance scholars (Dasgupta 1995; Lemos and Agrawal 2006; Dietz, Ostrom, and P. C. Stern 2003) posit that one plausible mechanism for sustaining moderate resource use in the face of the tragedy of the commons problem is through internalization of social norms leading to more effective enforcement - "maintain frequent face-to-face communication and dense social networks ... that increase the potential for trust, allow people to express and see emotional reactions to distrust, and lower the cost of monitoring behaviour and inducing rule compliance" (Dietz, Ostrom, and P. C. Stern 2003). The intuition behind this prediction is broadly that the costs of monitoring go down / efficacy of monitoring goes up when the political unit is congruent with the community unit at which social norms are enforced. Alternatively, under the assumption that ex-ante deforestation was largely driven by corruption / collusion between illegal loggers and the district government, the devolution of power to the villages greatly increases the number of administrations the loggers need to buy-off. This additional friction may push out some sellers, thereby reducing overall deforestation. Note, however, that this explanation relies on the new administrators themselves being passive agents instead of being active producers of illegal timber, which is the key assumption in Burgess, M. Hansen, et al. (2012)'s model that yields the opposite prediction. Empirical evidence in this direction has been scant; for example, Agrawal and Ostrom (2001) assert that "the cases in which there has been most effective decentralisation in property rights are also the ones in which forest conditions have improved more", but are unable to muster credible evidence in favour of the claim, and spend the majority of the article qualitatively discussing decentralisation as the dependent variable and discuss potential determinants of the precise forms of decentralisation in four decentralisation waves in India and Nepal.

We believe this difference in approach is justified by the second aspect of the PESA act implemented in the first PESA elections - that of the scheduled tribe reservations in the village councils. Political reservations are intended to incorporate the priorities of a hitherto marginalised group, but their effects on overall governance are ambiguous; both small positive[6] and negative[7] are reported in the literature. To tackle this problem, Anderson and Francois (2017) embed reservations (the random allocation of the paramount leadership of a village to a particular group) into a now canonical politics-of-fear model (Padró i Miquel 2007) with polarised electorates. Their model generates a "conditional predicted pattern of effect that is non-monotonic in the size of the group from which the village leader is drawn when leadership is reserved. If the group is small, so small that it ordinarily would not be able to contest for power, then reservations have no effect. If it is large enough to contest power, but not so large as to be guaranteed it, then reserving the leadership position for the group generates a positive impact on overall governance". The key mechanism by which this counter-intuitive prediction is generated in the model is that reservations improve the quality of the in-group challenger, since the

[6] surveyed in footnote 1 in Anderson and Francois (2017)

[7] surveyed in footnote 2 in Anderson and Francois (2017)

reserved group does not then need to rally behind a poorly governing incumbent leader (who gets kleptocrat rents, as in Padró i Miquel (2007)) who will raise their chances of being in power, as power is assured. Thus, reservations may, in certain conditions, improve governance quality (which, in our case, is a lower level of forest depletion). These dynamics may apply in a context where village-level leadership is being contested openly for the first time, and the poorly governing incumbent (at the larger sub-district administrative level) is challenged by within-group competition. This also yields predictions about heterogeneous treatment effects by population shares of STs that we intend to test.

In summary, there are reasonable accounts that predict both increases and decreases in deforestation as a result of decentralisation. We believe this is mediated by the particulars of the permit regime, as well as the presence, or lack thereof, of political reservations for groups that have norms in favour of resource preservation.

3 Context: Identity and Quotas in India

The Indian government has instituted numerous forms of political quotas since Independence. These quotas reserve particular positions among elected officials, within political parties, in civil service, and for higher education for specific identity groups. We define a political quota as a rule that requires that a government body represent individuals from a particular identity category. In the Indian context these individuals are typically from the categories of Scheduled Tribes (ST), Scheduled Castes (SC), Other Backward Classes (or Other Backward Castes, OBC), and/or women. Following Indian Independence, the newly written constitution provided dramatic guaranteed representation for individuals from these identity categories for candidates for political office - including in the national parliament (*Lok Sabha*), state legislatures (*Vidhan Sabha*), and from 1993 in the country's three-tier system of local government councils, called *Panchayat Raj*, at the village-cluster, block and district levels.

We focus in this paper on India's *Scheduled Areas*, a system of political quota for tribal populations that has not yet been subject to systematic quantitative analysis. Scheduled Areas exist in nine Indian states – Andhra Pradesh, Chhattisgarh, Gujarat, Himachal Pradesh, Maharashtra, Madhya Pradesh, Jharkhand, Odisha, and Rajasthan. Collectively these states represent roughly half of India's territory and population. Scheduled Areas represent 41% of the territory and 45% of the local population within these states, such that more than a hundred million people live in Scheduled Areas.

3.1 Tribes and Scheduled Areas in India

India's 'tribal' identity category was first codified, with corresponding separate administrative areas specified, during the British Colonial period. Scholars have identified these 'tribal' groups (or *adivasi*) by (a) their descent from particular lineages (Sundar 2009), (b) pre-colonial systems of administration, and/or (c) well-defined land arrangements and rights (Gupta 2011a; Gupta 2011b). Despite regular mention of these factors, scholars agree that there has been little clear definition or criteria as to what constitutes a 'tribe' (Béteille 1974; Béteille 1986; Dhebar 1962; Corbridge, Jewitt, and Kumar 2004; Corbridge 2002; Galanter 1984).

Encountering these communities, British administrators defined and enumerated what they viewed as 'tribal' populations. British authorities first provided a list of 'Aboriginal Tribes' and 'Semi-Hinduised Aboriginal Tribes' in the Census of 1872 (Corbridge 2002, p. 64) and implemented special institutions targeting tribes based on this census with the Scheduled Districts Act of 1874. Following Independence in 1947, the new Indian state identified in the Fifth Schedule of the Constitution its own 'Scheduled Areas,' with few differences from the British Scheduled Districts Act. The Indian government justified Scheduled Areas specifically as a means to improve representation and welfare for Scheduled Tribes (ST).

The geographic boundaries of the Scheduled Areas have changed little over time. Per the Constitution, the President of India has the right to Schedule or De-schedule Areas and does so in consultation with State Governors. In 1962, the Dhebar Commission proposed that an area should be eligible to become a Scheduled Area according to the following four, relatively vague, criteria.

1. Preponderance of tribals in the population
2. Compact and reasonable size
3. Under-developed nature of the area
4. Marked disparity in economic standards of the people

In practice there has been no exact formula for updating or adjusting the previous notification or de-notification of Scheduled Areas in India, and these Areas have remained remarkably stable since they were demarcated by the Dhebar Commission.

Scheduled Areas matter today in particular because more recent legislation has implemented political quotas using Scheduled Areas as the key reference. The Panchayats Extension to Scheduled Areas Act of 1996 (PESA) mandated that *all* chairperson positions at the three levels of local government, and at least 50% of all seats on these councils, be reserved for ST individuals. Hence, when local elections were next held – as early as 2000 for Rajasthan and as late as 2010 for Jharkhand (due to a Supreme Court case) – these reforms gave a tremendous positive shock to the local political representation of Scheduled Tribes in India.

3.2 The Forest Rights Movement and PESA

Despite having been inhabitants of forests in central and Eastern India for a long time, members of scheduled tribes had no property rights over their land in Colonial India. The British implemented legislation to prevent over-extraction in 1878 the Indian Forest Act in 1927, which sought to consolidate areas as 'Reserved Forest', where timber extraction was banned and levies were implemented (Rao 2017). Patnaik (2007, p3) contends that the 1927 law remains India's central forest legislation describing it as follows:

> the Government can constitute any forest land or wasteland which is the property of the Government or over which the Government has proprietary rights, as reserved forest, by issuing a notification to this effect. This Act enabled the colonial Government to declare more and more land as reserve forests, without ascertaining the rights of the tribals and other forest dwellers.

Patnaik also contends that the rights situation of the scheduled tribes worsened post-independence when large tracts of land were declared "forest" by the zamindars, princely states and private owners and transferred to the government through a blanket notification. These tracts included the traditional homeland of scheduled tribes who were subsequently declared as encroachers. The eminent domain of the government was challenged by activists and human rights campaigners through a sustained campaign in the 1990s, which culminated in the PESA act. The act aimed to 'decentralise existing approaches to forest governmance by bringing the *Gram Sabha* (village council) center stage and recognised the traditional rights of tribals over "community resources" - meaning land, water, and forests' (Patnaik 2007, p 5).

The nine states that contained Fifth Schedule Areas: Andhra Pradesh, Chhattisgarh, Gujarat, Himachal Pradesh, Jharkhand, Maharashtra, Madhya Pradesh, Orissa and Rajasthan, were to enact suitable laws within one year of the coming into force of PESA (Bijoy 2012). The variation in timing of PESA implementation arises from the fact that Panchayat councils serve a 5 year term, and different states held Panchayat elections for the first time in different years following the initial implementation of the 73rd amendment in late 1992[8].

4 Data

4.1 Deforestation Data

The deforestation data we use comes from the Global Forest Cover dataset produced by M. C. Hansen et al. (2013), who use LANDSAT 7 images (30 $\times$ 30 m grid) to construct forest cover measures in 2000 (average tree cover canopy of the Landsat pixels) as well as deforestation indicators for entire planet between 2001 and 2017. An example of the data for our study region is reported in figure 1,

[8]Some villages in Maharashtra, West Bengal, and Rajasthan had village elections going back to the 1960s. Following the 73rd amendment in 1992, Madhya Pradesh was the first state to hold local elections, in 1994, and has held them every 5 years since (Commonwealth Human Rights Initiative 2006); it held elections in scheduled areas in 2004. Similarly, Orissa held its first panchayat elections in 1997, and held them in scheduled areas in 2002. Through legal complications, the first election in PESA areas were delayed to after 2000 in most states.

which reports forest loss and gain cells in southern Jharhand over the study period. In addition to indicators for whether a cell underwent forest- loss or gain over the sample period, Hansen et al also construct a dataset that tags a cell by the year in which it was deforested, which allows us to construct a rate of deforestation for aggregated administrative units, e.g. villages (discussed in detail below).

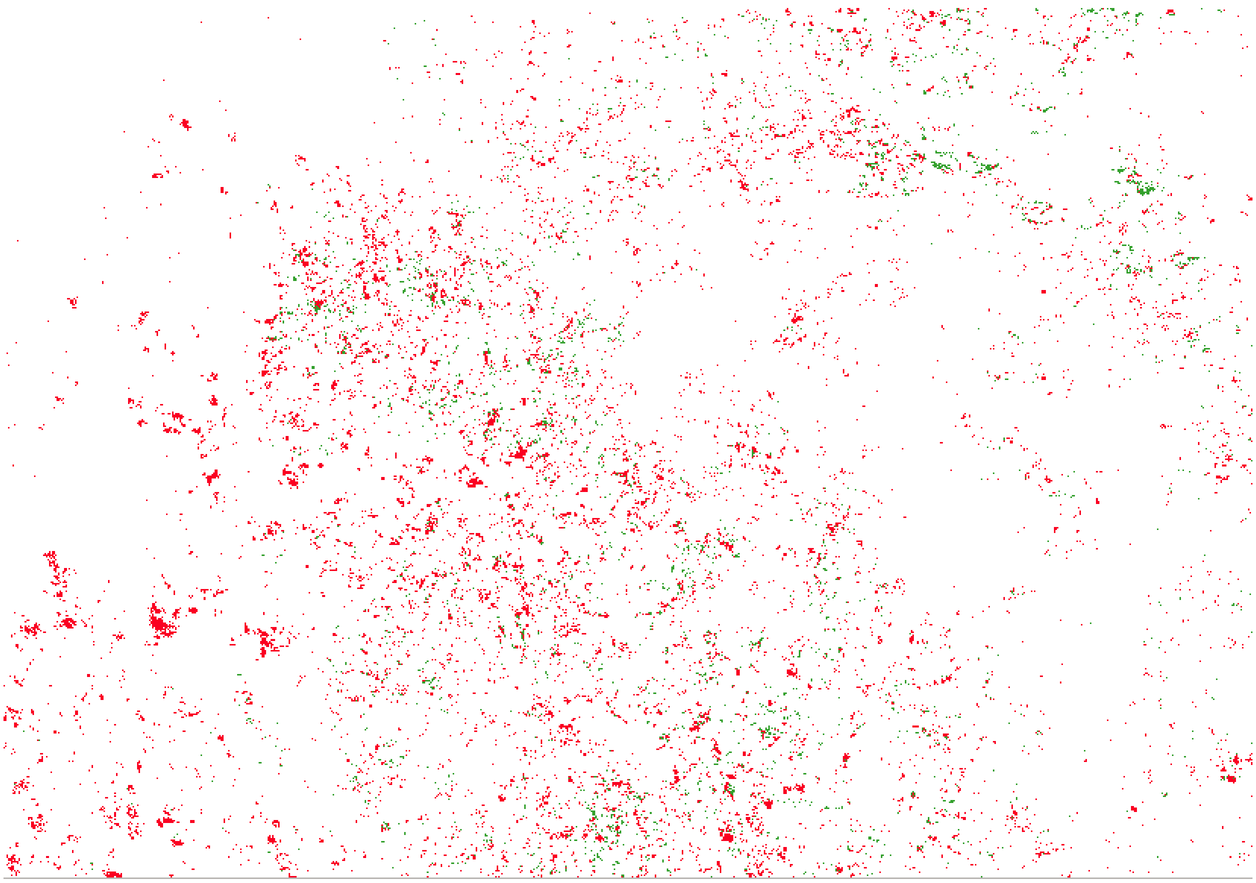

Figure 1: Forest loss (red) and gain (green) over the entire sample period (2001-2017) in southern Jharkhand, India. Forest loss is far more common than forest gain in the regions under consideration, although India reportedly experienced net forest-gain in the decades preceding the analysis (Foster and Rosenzweig 2003)

4.2 Administrative and Political Data

We use the Infomap (2001) geocoded village-level Indian census to get village boundaries, and aggregate the deforestation data described above to the village level [9] in a GIS system to construct our panel.

We use district-block or district level designations of scheduled areas (which varies by state) to designate treated regions (as illustrated in 3). We then spatially merge the block level treatment

[9] i.e. if 25 cells labelled 2006 fall in a particular village, the rate of deforestation in said village is the $log(x+1)$ of this quantity

indicators to the village level to generate a village-level treatment indicator for scheduled area. We construct a switching indicator for scheduled areas in each state based on archival research of the first *gram-panchayat* election in scheduled areas in each state in accordance with PESA (as illustrated by the treatment timing figure 2). This restricts us to estimating treatment effects for 6 of the 9 treatment states because the remaining three implemented PESA before the first year of forest data availability. We then merge these village level treatment timing data with the deforestation panel to finally arrive at our analysis sample, which is a balanced panel of roughly 300,000 villages over 17 years. Not all of these villages have any forest cover to begin with, which is why we subset to those with above-median levels of forest cover in 2000 in our preferred specification. We further analyse the sensitivity of our estimates to this cutoff in section 7.

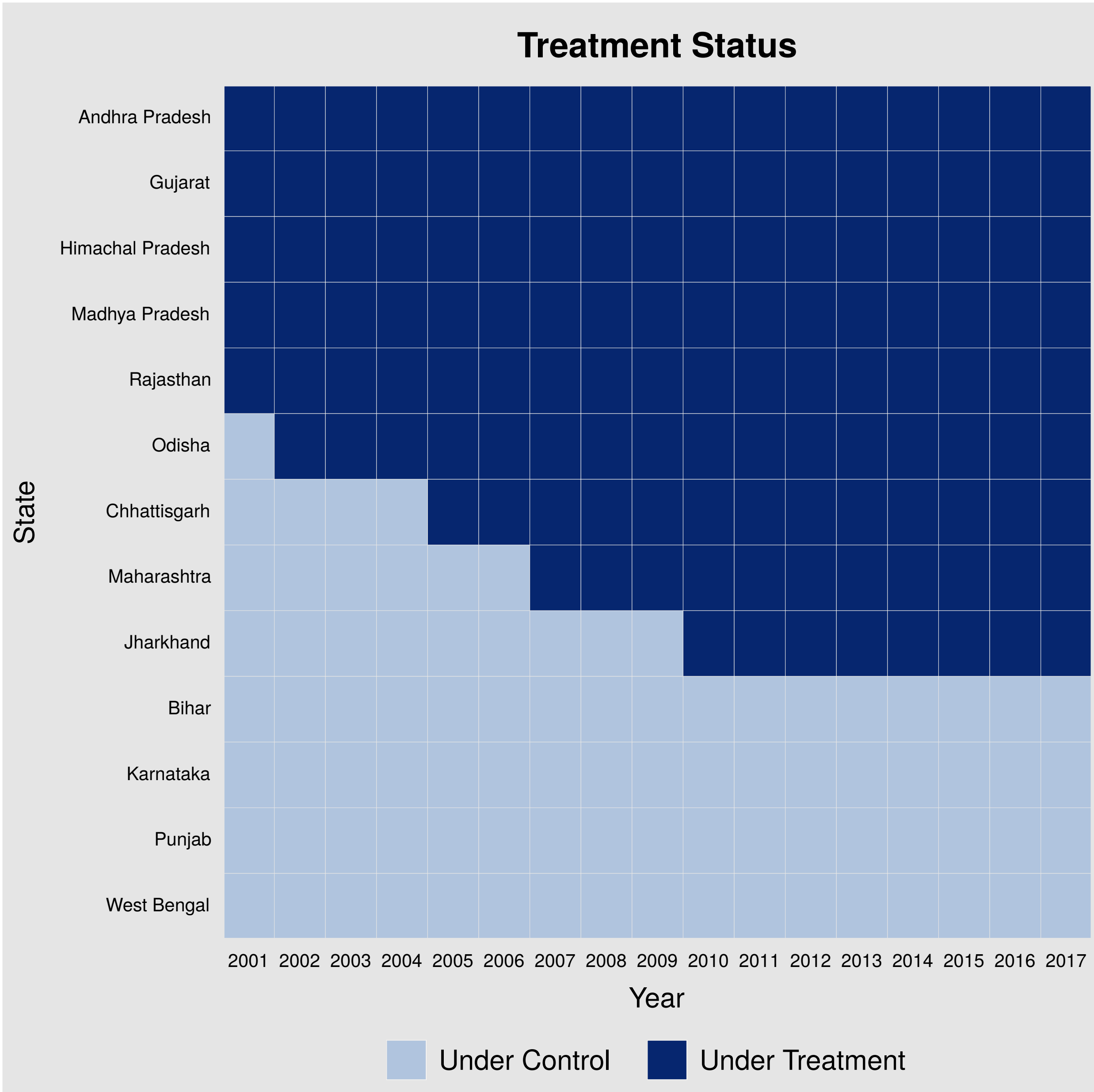

Figure 2: PESA implementation timing for 9 states (plus neighbouring states close to border)

5 Empirical Strategy

Since we have a panel dataset with time-varying entry into the treatment, we use a standard generalised difference-in-differences design of the form

$$Y_{ist} = \tau \text{Scheduled Area}_{is} \times \text{PESA Election Year}_{ist} + \delta_i + \gamma_t + \epsilon_{ist} \tag{5.1}$$

where i indexes villages, s indexes state, and t indexes years. Y_{ist} is the total area (in Hectares)

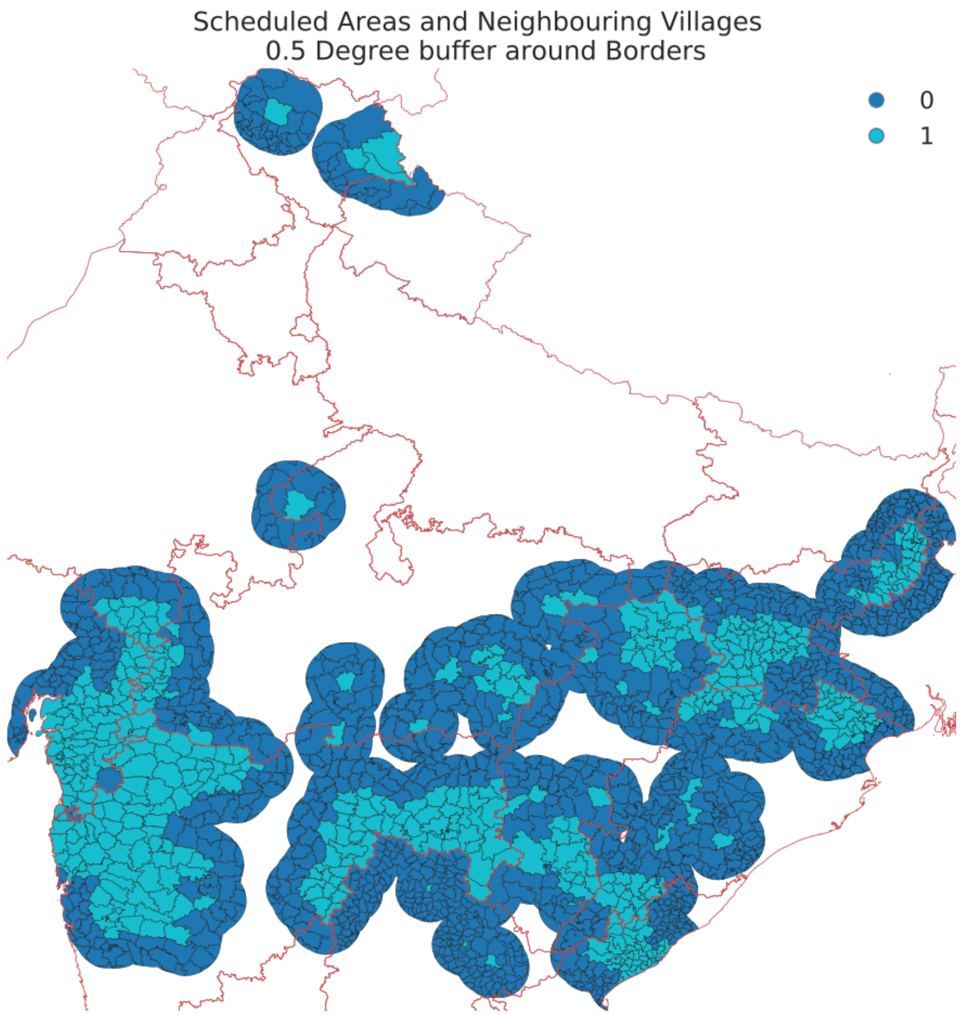

Figure 3: Sample Villages: Scheduled Areas under the 5th Schedule of the Indian Constitution and neighbouring villages ($\leq$ 50 km away)

deforested in village i in year t [10], Scheduled Area $\times$ PESA Election Year$_{it}$ is a dummy that for villages in scheduled areas in the year the first election where PESA was implemented, δ_i is a village fixed effect (which nests the base term for Scheduled Area, since it is time invariant), and γ_t is a year fixed-effect. We cluster standard errors by village in the main tables and examine robustness to more aggregated clustering in the robustness section.

[10] $:= \#$Deforested Cells $\times\ 0.09)_{ist}$, since the area of a LANDSAT cell is known to be 900 m^2

To make the parallel trends assumption more credible, we estimate more flexible specifications of the following form:

$$Y_{ist} = \tau \text{Scheduled Area} \times \text{PESA Election Year}_{ist} + \delta_i + \gamma_t + \delta_i t + \delta_i t^2 + \epsilon_{ist} \tag{5.2}$$

where the additional terms $\delta_i t$ and $\delta_i t^2$ are village-specific linear and quadratic time trends, which account for secular trends in deforestation by village that would be mis-attributed to the policy if take-up was indeed non-random and related to the underlying village trend of deforestation.

In order to further isolate 'clean' variation, we also estimate regressions of the form

$$Y_{ist} = \tau \text{Scheduled Area} \times \text{PESA Election Year}_{ist} + \delta_i + \gamma_t + \xi_{st} + \epsilon_{ist} \tag{5.3}$$

where ξ_{st} is a vector of state-year fixed effects. This restricts the comparison to scheduled and non-scheduled areas *within* each state. This way, we can control for unobservables that vary by state for each year (for example, particular upticks in state political activity on forest conservation, and the like), and therefore isolate the effect of PESA by comparing scheduled areas and non-scheduled areas within each state. This results in the specification estimating a multiple standard differences-in-differences estimates (wherein the treatment and control units are within each state), and doesn't rely on the staggered adoption of the policy for identification. This is, to our mind, the most credible comparison wherein parallel trends might hold.

6 Results

6.1 Primary Specification

Our main regression findings are reported in table 3 below. Columns 1 and 4 columns report estimates of τ from 5.1, columns 2 and 5 ones report the estimates from 5.2, and columns 3 and 6 report estimates from 5.3.

The estimated treatment effects are consistently large and negative. In our preferred specification with state-year fixed effects, the implied reduction in the rate of deforestation is approximately 0.10 hectares, 30% on the control mean of 0.33. The difference in the estimated treatment effect between the two-way fixed-effect estimate (column 4), the specification that includes additional time-trends (column 5), and state-year fixed effects (column 6) suggests that there was plausibly negative selection into treatment. The coefficients from the two specifications are in the same order of magnitude and consistently have the same sign, which gives us confidence that the time trends are absorbing confounding in selection into treatment.

Table 1: Village Level Regressions

	(1) Full Sample	(2) Full Sample	(3) Full Sample	(4) Forested	(5) Forested	(6) Forested
Scheduled X PESA	-0.00893***	-0.0137***	-0.0165***	-0.119***	-0.249***	-0.101***
	(0.00183)	(0.00330)	(0.00193)	(0.0174)	(0.0283)	(0.0182)
N X T	4501515	4501515	4501515	447389	447389	447389
Villages (N)	264755	264755	264755	26316	26316	26316
Dep Var Mean	.05	.05	.05	.33	.33	.33
Village FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Village Time Trends	no	yes	no	no	yes	no
State-Year FE	no	no	yes	no	no	yes

Standard errors in parentheses

Cluster-Robust SEs by code_2011

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6.2 Dynamic Treatment Effects

We also estimate distributed lag models which include leads and lags of the treatment dummy to decompose the treatment effect by each year preceding and following the switch. This is an intuitive method of performing a Granger Causality test, wherein the leads should be relatively insignificant and moderate in size, while the contemporaneous and lagged effects ought to be large if the policy is indeed the cause of the change. We report the results from this model graphically in fig 4. This strengthens our confidence in our results because the leads are insignificant and the treatment effect appears precisely in the election year and then stabilises to a lower rate in subsequent years.

$$Y_{ist} = \delta_i + \gamma_t + \delta_i t + \delta_i t^2 + \sum_{\phi=0}^{m} \tau_{-\phi} D_{i,t-\phi} + \sum_{\phi=1}^{q} \tau_{+\phi} D_{i,t+\phi} + \epsilon_{ist}$$

6.2.1 Mechanisms / Effect Heterogeneity

One of the hypothesised mechanisms for the reduction in deforestation is that ST politicians are able to gain political power in gram panchayat elections and subsequently enact policies to reduce deforestation. As such, it follows that we should see larger treatment effects in areas that have a ST plurality. To test this mechanism, we define a plurality indicator for each village as follows

$$\text{ST Plurality}_i = \mathbb{1}\left(\text{ST Population}_i > \max(\text{SC Population}_i, \text{Non SC/ST Population}_i)\right)$$

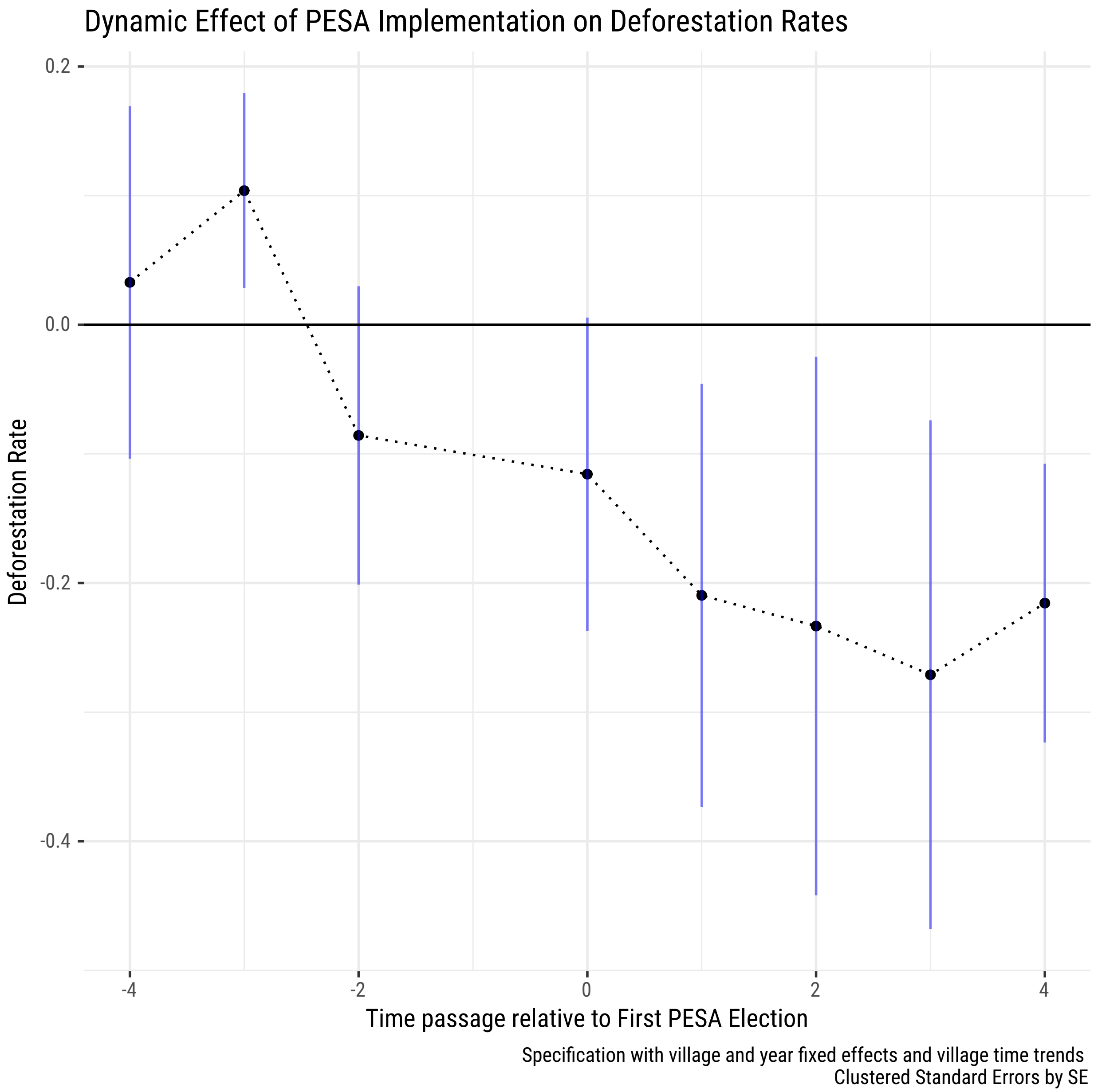

Figure 4: Dynamic Treatment Effects of PESA Adoption

We then estimate specifications 5.1, 5.2, and 5.3 including an interaction with an indicator for ST Plurality, which serves as a test for heterogeneous treatment effects by ST share. The coefficient for Scheduled × PESA × ST Plurality is consistently negative and statistically significant across specifications, which suggests that the effect of the policy is stronger in villages where STs are a large enough share of the electorate to constitute a strong electoral bloc in village government.

The same is true for treatment effects by ST share quintile, as illustrated in fig 5. The treatment effects are effectively zero for the first three quintiles (because they STs don't constitute a plurality;

see distribution in fig 7), and appear when STs are in the plurality. This also serves as an indirect test of the hypothesis of inverted-U-shaped effect of quota effectiveness implied by the model in Anderson and Francois (2017). In our case, however, we find no inverted-U shape of the net effect as a function of ST shape; the treatment effect is increasing in ST share (albeit estimated noisly). It should be emphasised, however, that we are estimating the effect of quotas on a particular policy - that of forest preservation - while Anderson and Francois (2017)'s model predicts net effects on overall governance, and as such our estimates do not directly test and falsify the model's predictions.

These results suggest that the observed decrease in the rate of deforestation as a result of PESA is concentrated in villages where STs are an electoral plurality, and therefore can potentially shape village agendas to reduce deforestation. This strongly suggests electoral control is a potential mechanism for the observed effect.

Table 2: Regressions with ST Plurality Interaction

	(1) def_ha	(2) def_ha	(3) def_ha
Scheduled X PESA	-0.000800	-0.179^{***}	0.0138
	(0.0466)	(0.0542)	(0.0465)
Scheduled X PESA X ST Plurality	-0.132^{***}	-0.0802	-0.129^{***}
	(0.0491)	(0.0621)	(0.0489)
Village FE	yes	yes	yes
Year FE	yes	yes	yes
Village Time Trends	no	yes	no
State-Year FE	no	no	yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Another hypothesised mechanism, closely related to the model proposed in Burgess, M. Hansen, et al. (2012), is that villages rich in natural resources were previously unable to regulate resource extraction because of lack of political power are now able to do so following the implementation of village democracy and ST quotas. To test this, we use data from the Indian Mining Census, compiled by Asher and Novosad (2019). The Mining Census lists the total production and value of various minerals extracted in each district for every year since the 1981, and we use pre-treatment production for each district found in the mineral database for the 10 highest value minerals [11] to classify districts as either high or low mining potential. We then estimate the three specifications above including an interaction with an indicator for mining potential in the district. The coefficient for Scheduled $\times$ PESA $\times$ Mining

[11] These include gold, coal, bauxite, copper and so on. Subsetting by value omits some seemingly less relevant minerals, such as clay. Using the presence of any of the entire of the original extensive list of minerals labels well over 90% villages as 'mining potential', which leaves us with little variation to work with. The results are qualitatively similar when employing slightly more restrictive (presence of 5 most high value minerals) and inclusive definitions of mining potential

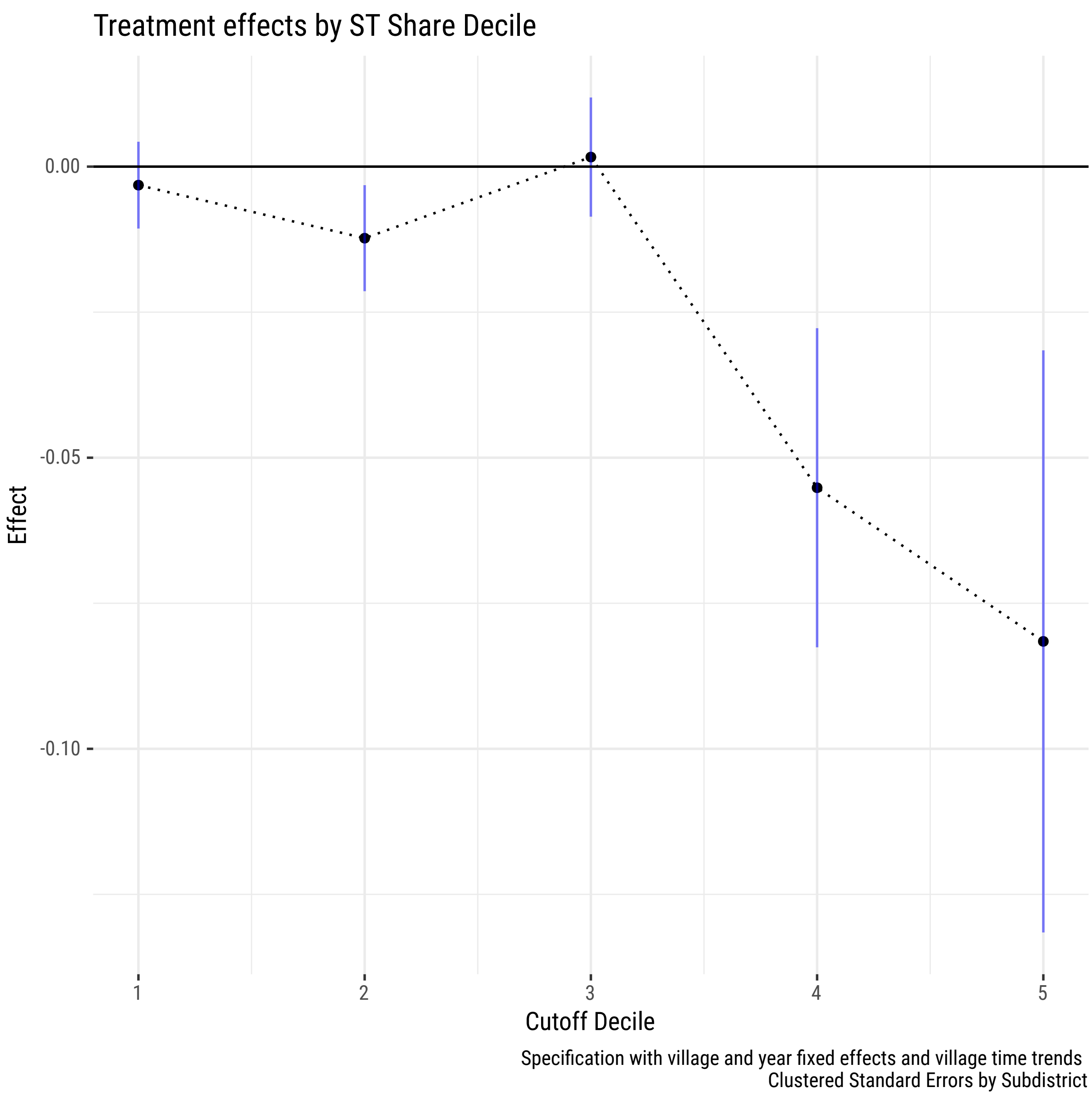

Figure 5: Treatment effects by ST Share Quintile

Potential is consistently negative and statistically significant across specifications, which suggests that the effect of the policy is stronger in villages where mining was previously widespread and presumably a policy concern of the local population. This suggests that reduced mining, potentially through village council oversight over mining in the forests, might be a possible mechanism for the observed decrease in deforestation.

Table 3: Regressions with Mining Potential Interaction

	(1)	(2)	(3)
	def_ha	def_ha	def_ha
Scheduled X PESA	-0.0909***	-0.143***	-0.0692***
	(0.0186)	(0.0353)	(0.0202)
Scheduled X PESA X Mining Potential	-0.0841**	-0.248***	-0.0822**
	(0.0375)	(0.0628)	(0.0386)
Village FE	yes	yes	yes
Year FE	yes	yes	yes
Village Time Trends	no	yes	no
State-Year FE	no	no	yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

7 Robustness Checks

In table 5, we report results from the main specifications clustered at the sub-district to account for both spatially correlated errors within Subdistricts (also known as Tehsils, administrative units comprised of multiple blocks, which in turn are comprised of multiple villages), and serial autocorrelation in errors. These units are sufficiently large as to cover the entire bandwidth (50 Km) around the border, and therefore are equivalent (and more conservative) than estimating Conley standard errors with the largest admissable bandwidth.

As discussed above, it makes little sense to include villages that had no forests to begin with in our sample, since their deforestation rates are mechanically zero throughout the sample. In our preferred specification in table 3 (columns 4, 5, and 6), we use only villages with above-10% forest cover (averaged over all cells in the village) ex-ante(in 2000).

Here, we go a step further and vary the ex-ante vegetation index cutoff for entry into our estimation sample and estimate 5.2 on the subset sample to test for whether the coefficient is sensitive to the choice of this cutoff, as illustrated in fig 6.

We also estimate the same specifications as in section 5 using outcome data derived from the Vegetation Continuous Fields data aggregated by Asher, Lunt, et al. (2019) which provides annual tree cover from 2000-2014 in the form of the percentage of each pixel under forest cover (Sexton et al. 2013). This allows us to estimate the intensive margin effects in addition to the extensive margin that we report in the previous section. The results are reported in section A.2, and yield substantively identical conclusions. Since the outcome in these regressions is the vegetation index, a positive coefficient indicates a net positive effect on forest levels. A major drawback of this data source is that it is derived from a different satellite with more coarse resolution ($250 \times 250m$ cells)

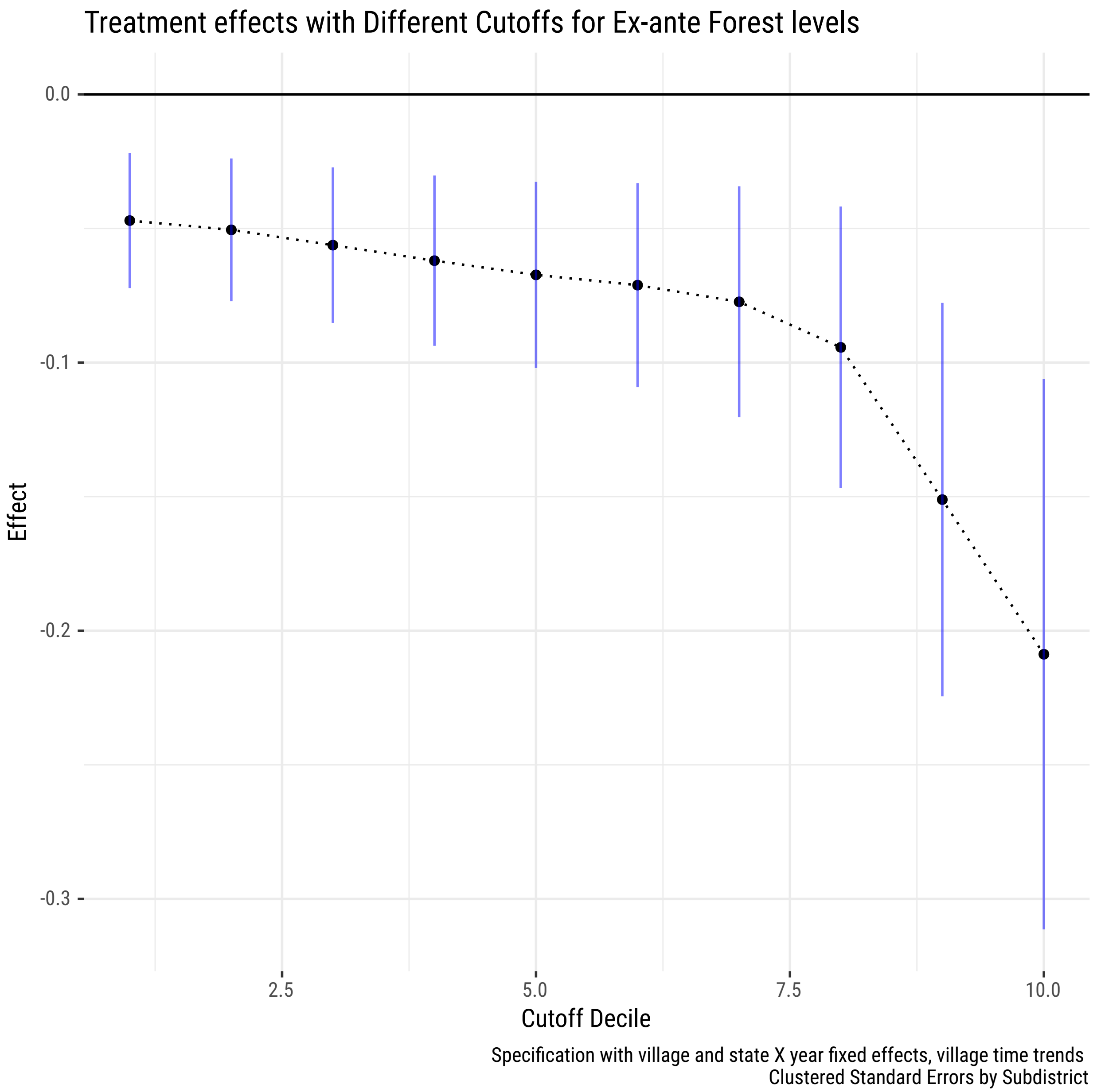

Figure 6: Treatment Effects as a function of ex-ante cutoffs

and therefore some villages' indices are derived from a small handful of measurements. Nevertheless, the results serve as a useful robustness check and validate our main findings.

8 Conclusion

In summary, we find that the a policy that introduced local government and quotas for most-affected groups substantially reduced the rate of deforestation in Indian villages. We provide suggestive evidence in favour of an electoral control mechanism – that PESA reservations resulted in ST politicians gaining electoral control and shaping local policy –, and a resource extraction slowdown mechanism for the observed effect. We hypothesise that the election of ST leaders to the panchayat improved enforcement, thereby reflecting the strong norms of preservation among the scheduled tribes in village policy. The general conclusion to draw from the PESA expansion episode is that endowing political and bureaucratic power at administrative units congruent with networks at which informal norms of preservation exist may improve the effectiveness of environmental preservation by lowering enforcement costs and adding administrative frictions that render illegal overuse unprofitable.

This paper finds strong evidence in favour of local governments' effectiveness in promoting environmental protection, especially when key stakeholders, such as STs in India, are included pro-actively in local government. This finding runs counter to the argument that administrative-unit proliferation and decentralisation create incentives for over-utilisation by exacerbating the collective action problem. The collective action problem of resource preservation may plausibly have been ameliorated in this context through the presence of strong informal institutions present in ethnic networks that enforce norms of sustainable use.

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A Additional Tables

A.1 Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
# households	263,140	317.495	1,656.512	1	76	310	248,162
Population	263,140	1,484.041	7,730.440	10	356	1,458	977,771
Scheduled	263,140	0.206	0.405	0	0	0	1
Ex-Ante Forest Max	263,140	30.693	26.865	0	12	45	100
Ex-ante Forest Mean	263,140	3.370	9.423	0.000	0.019	0.825	89.098
Number of Cells	263,140	5,855.365	8,111.402	1	1,668	7,285	1,097,168
ST Share	263,140	0.310	0.372	0.000	0.000	0.628	1.000
SC Share	263,140	0.145	0.194	0	0	0.2	1

A.2 VCF Estimates

	Outcome: Log(Forest Index + 1)			
	All		Above Median Pre-2000 Forests	
	(1)	(2)	(3)	(4)
Scheduled Village X PESA Implementation	-0.02792^{***}	0.01639^{***}	-0.02785^{***}	0.01616^{***}
	(0.00097)	(0.00122)	(0.00129)	(0.00163)
Village FEs	X	X	X	X
Year FEs	X	X	X	X
Village (Lin, Quad) Time Trends		X		X
Number of Villages	278909	139454	278909	139454
Observations	4,183,680	4,183,680	2,091,855	2,091,855
R^2	0.82390	0.84320	0.79770	0.81690

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$
Standard Errors clustered by Village

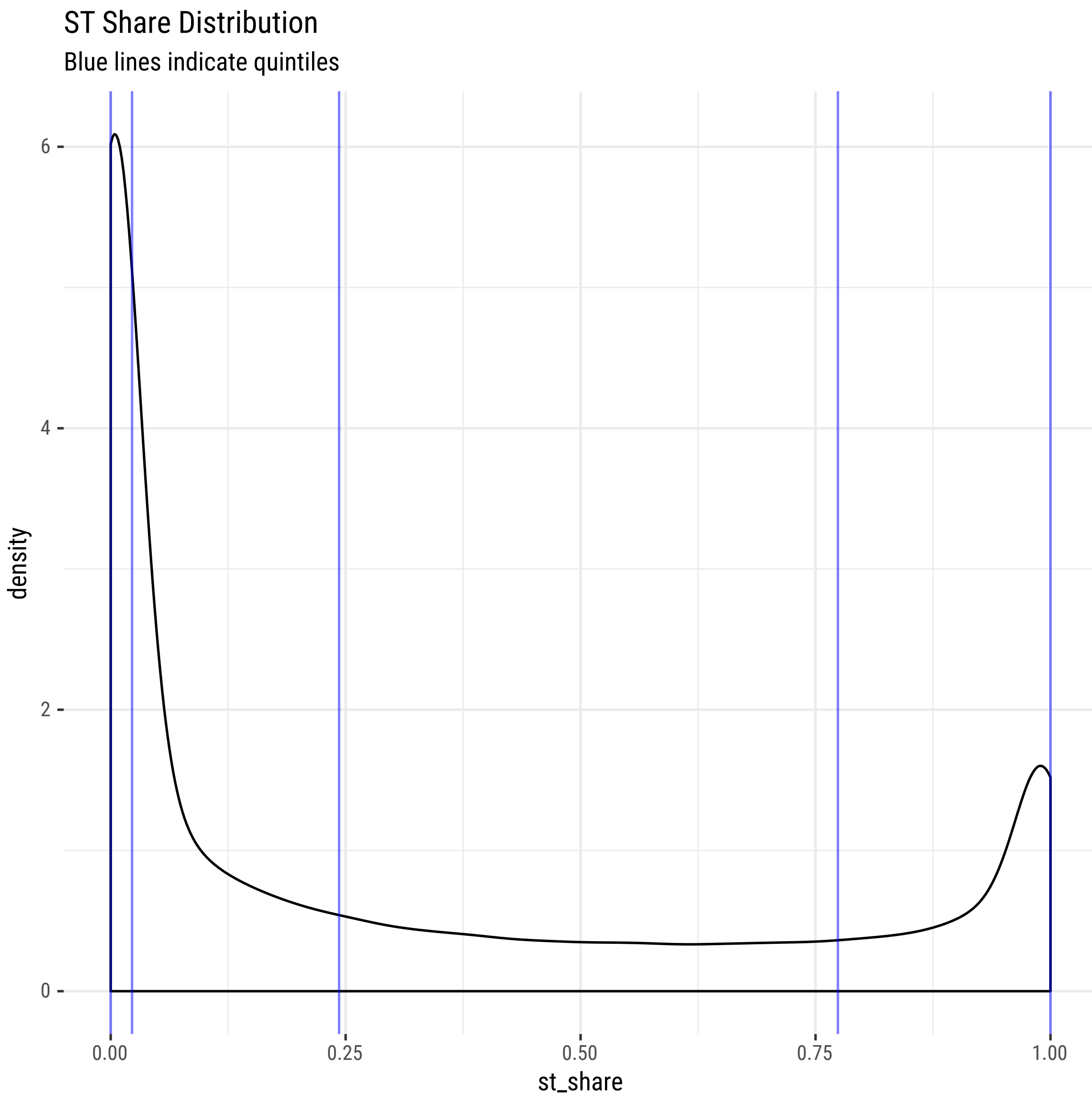

Figure 7: ST share distribution

Table 4: Varying Cutoff for Ex-ante Forest Levels for Sample

	Cutoff Percentile	Cutoff Vegetation Index	Number of Obs	Number of Villages	coef	se
1	10	0	4,865,026	285,091	-0.045	0.004
2	20	0.010	3,892,031	227,898	-0.053	0.004
3	30	0.028	3,405,525	199,299	-0.058	0.005
4	40	0.056	2,919,019	170,702	-0.067	0.005
5	50	0.105	2,432,513	142,118	-0.077	0.005
6	60	0.205	1,946,024	113,540	-0.096	0.004
7	70	0.486	1,459,518	84,984	-0.126	0.005
8	80	1.933	973,012	56,491	-0.196	0.008
9	90	10.871	486,506	28,134	-0.278	0.045

A.3 Clustering Standard Errors

Table 5: Village Level Regressions clustered at sub-district level

	(1) Full Sample	(2) Full Sample	(3) Full Sample	(4) Forested	(5) Forested	(6) Forested
Scheduled X PESA	-0.00893	-0.0137	-0.0165**	-0.119***	-0.249***	-0.101***
	(0.00661)	(0.0127)	(0.00682)	(0.0398)	(0.0827)	(0.0341)
N X T	4501515	4501515	4501515	447389	447389	447389
Villages (N)	264755	264755	264755	26316	26316	26316
Dep Var Mean	.05	.05	.05	.33	.33	.33
Village FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Village Time Trends	no	yes	no	no	yes	no
State-Year FE	no	no	yes	no	no	yes

Standard errors in parentheses

Cluster-Robust SEs by sub_dist

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$