Fight fire with finance: an experiment to curtail land-clearing fire in Indonesia

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Joint with Walter Falcon, Grace Hadiwidjaja, Matthew Higgins, Rosamond Naylor, and Sudarno Sumarto (Stanford, TNP2K, and the SMERU Research Institute)

The David and Lucille Packard Foundation, the Climate and Land-use Alliance, Daemeter Consulting Co., Anne and Robert Pedrero, and John Montazee generously financed the pilot program in West Kalimantan

ACDE Seminar August 2020

Jambi Province, Indonesia—September 2019

Motivation

The problem ⇒ forest fires. In 2015-16, \$16bn economic costs, 100,000 premature deaths, some days more emissions than US economy

⇒ mostly all human-lit, an increasingly prominent way to clear land

Potential solution ⇒ payments for ecosystem services (PES) and conditional cash transfers (CCTs), two popular and often quite effective approaches to spur behavior change.

⇒ Key question: whether environmental fiscal incentives can still be effective amidst limited property rights, land use flux, underdevelopment

⇒ Empirical challenge: credible counterfactual needed to discern additionality and to avoid paying for the status quo "anyway" activities

This paper ⇒ RCT deep in the Bornean jungle, covering around 90,000 households, testing whether community-level conditional cash transfers can reduce the use of harmful land-clearing fire, as monitored from space

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Three-part payment-by-results pilot experiment

1. Village information and instruction on fire prevention
2. Up-front Rp 10 million (750 USD) capital grant to ease liquidity constraints and help with fire prevention
3. Ex-post conditional payment of Rp 150 million (around 15% of village budget) if successful in eliminating fire over the 2018 fire season (July–December)

Other salient features:

- With over 30,000 HH treated, plot-level monitoring infeasible, too costly; focus on village collective action, PNPM CDD approach
- Block-randomize 75/275 villages, from four districts in West Kalimantan, into the pilot programme
- Satellite-based outcomes ⇒ lower cost, higher quality, blind control
- Designed and trialled within existing fiscal architecture to be scaled, a mechanism to operationalize external climate finance

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Key findings

- Program caused villages to increase fire prevention behaviors: more resources allocated, more taskforces formed, more people involved in fire fighting and suppression, more patrols.
- 21/75 programme villages were successful (i.e., 72% had fire).
- 71% of control had fire. No statistically significant differences.
- Adoption of fire prevention practices was insufficient to deliver the fire free outcomes desired, and so was explicitly paying for them
- Ex-post disbursement saved 8,100 million IDR, and no evidence 3,150 million disbursed reduced fire more than no payments
- Can't rule out small potentially policy-relevant effects i.e., <16 pp on the extensive margin (to 55%), 40% on intensive
- However, impacts need to be large to justify expanding pilot or scaling up. Clearly, we find no such evidence.

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Key contribution

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Significant policy enthusiasm around PES, EFT, and REDD+ initiatives seeking to improve environmental outcomes through cash or in-kind compensation, penalties, and alternative livelihoods.

"Although it is not unusual for empirical research to lag well behind theory and policy implementation, the current state of the PES evidence base is cause for concern. There is an urgent need for PES programs to be designed at the outset with the intent to evaluate their effectiveness"

⇒ One PES experimental evaluation (Jayachandran et al., 2016)

⇒ To our knowledge, this is the **first experimental evaluation** of collective payment-by-results payment for ecosystem services

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The rest of this seminar

1. Empirical setting
2. Theoretical framework
3. Project design and implementation
4. Satellite data, performance monitoring, and other data
5. Experimental design
6. Results and robustness checks
7. Potential explanations for the main null result

Empirical setting

Programme theory

Offer comparatively large community-level payment if villages eliminate (c.f., reduce) the use of land clearing fire over the 2018 fire season

Reduce fire by ⇒ (a) making fire less attractive than not clearing land, clearing it legally without fire at higher cost, or allowing natural or spreading fires to run their course, and (b) activating collective action.

Two key assumptions:

1. The size of the payment is large enough to offset the lower costs and potentially greater benefits of fire use
   - Cannot match actual opportunity costs nor the true environmental and social benefits
   - Individual incentive scheme infeasible (cost, scale, institutions)
2. Effective village collective action (Naylor et al., 2019)
   - Instead rely on salient community benefit and social pressure
   - Leverage existing fiscal architecture, closely follow PNPM

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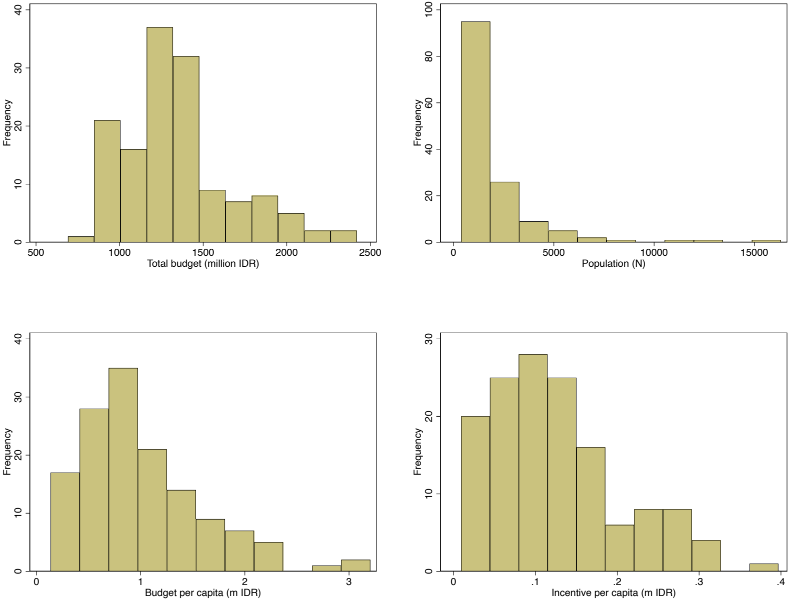
Village budgets and populations



Programme implementation

Facilitations took place May–July 2018, with 3 main parts: (a) facilitation with village head and govt staff, (b) public facilitation (both around 3 hrs), and (c) baseline survey with village head or secretary.

Government and public facilitations covered similar material:

- Explained incentive and monitoring, with demonstrations
- Provided extensive information on fire-free agricultural practices and resources available for fire prevention and suppression
- Explained how to maintain traditional slash-burn activities, through pre-registration, without jeopardizing success
- Notified about the up-front 10m IDR, provided within the week

Staff did not visiting treatment villages until end of monitoring period, when an endline survey of treatment and control villages was conducted.

21 programme villages had no fire, were notified of results in March–April 2019, and paid in May–June 2019. Celebratory meetings at bupati offices.

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Satellite outcome data and performance monitoring

MODIS Active Fire Product MCD14ML hotspots

Spatially-join thermal hotspot detections to 2016 village boundaries and create (a) binary indicator if fire, (b) fire count

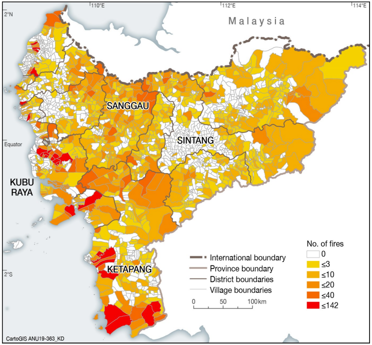
Several advantages:

⇒ lower-**cost**

⇒ higher **quality**

⇒ genuinely **blind** control

⇒ pre-period data to 2001



Assessment: err towards paying unsuccessful villages rather than not paying successes (i.e., removing detections (a) under 50 confidence, (b) within 500m of boundary, and (c) that matched pre-registered swidden fire) and manually inspected photos for villages with three or less.

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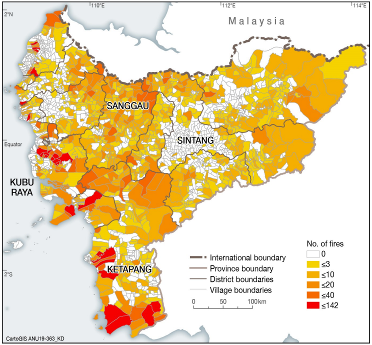

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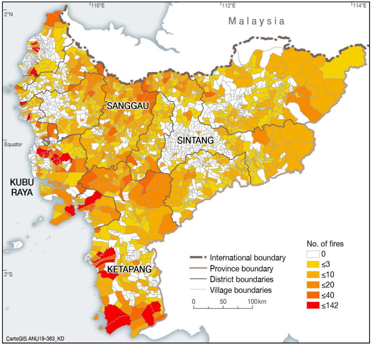

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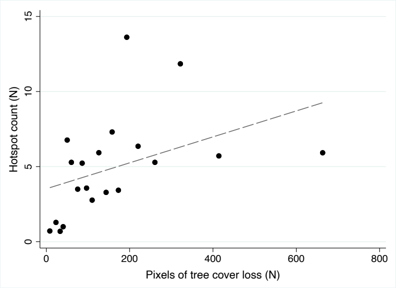
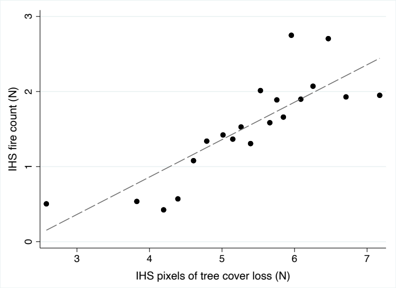
Why not incentivize less deforestation?

- Fire and deforestation are correlated; fire easier to measure, monitor
- Fire is a key channel for deforestation, but generates additional health and environmental externalities and more politically salient





Nonetheless, we also measure impacts on village tree cover change

Four other main types of data

Baseline census of villages. Giant cross-sectional dataset merging PODES 2014, Agricultural Census 2013, SMERU Poverty Map 2015, and GFW palm oil mills, village area, peat soil, and MODIS hotspots detected in previous years calculated in GIS.

Detailed village surveys which we conducted.

1. Baseline, only for the treatment group
2. Endline, for the treatment group and an randomly-selected group of 75 villages from the control group

Extensive qualitative information from pre and post field visits.

Two additional remotely-sensed datasets. Visible Infrared Imaging Radiometer Suite (VIIRS, with different technology and precision for examining fire) and Global Forest Change (to examine tree cover loss).

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Experimental design

Sample

Districts: Kubu Raya, Sanggau, Sintang, and Ketapang

Restrict sample to (a) 8 most fire-prone sub-districts in each district, and (b) villages with fire 2/3 of the last 3 years to

⇒ significantly ease field logistics

⇒ ensure target the most at-risk villages

⇒ ensure study villages start from similar baselines

⇒ reduce variance in outcomes within study sample

⇒ make differences in success easier to discern

Experimental design

Randomization

Randomize 75/275 villages from restricted sample into programme

Minimum detectable effect around 16 p.p. Δ

Block on districts (i.e., randomize within them)

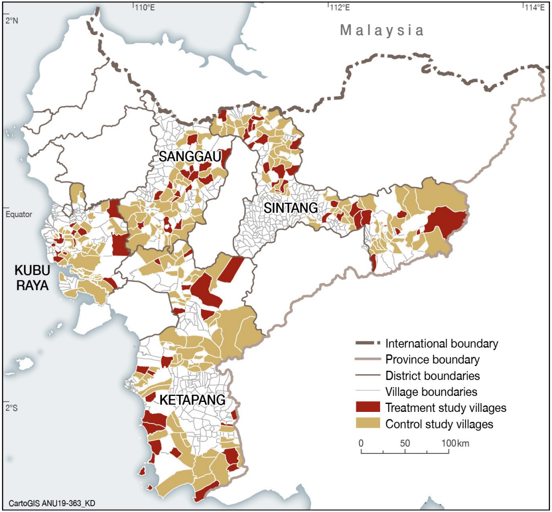
Balance on pre-treatment observable correlates of fire, including fire history (Edwards et al., 2020)

Variable*	Control μ/SE	Treated	t-test p-val	N. diff
Area (ha)	13,600 [1,360]	14,200 [2,130]	0.799	-0.031
2015 fires	12.379 [1.695]	11.893 [2.679]	0.912	0.021
2014 fires	8.182 [1.099]	8.453 [1.536]	0.870	-0.018
2013 fires	3.970 [0.334]	3.240 [0.434]	0.273	0.163
HH (N)	333.020 [15.519]	318.387 [26.226]	0.429	0.066
OP (ha)	153 [24.7]	132 [33.7]	0.810	0.063
Dirt road	0.677 [0.033]	0.600 [0.057]	0.260	0.161

* miscellaneous selection shown here
Balance checks used the complete baseline census

Experimental design

Treatment assignment



Experimental design

Estimating equation

$$y_{v,d} = \alpha + \beta D_{v,d} + \delta_d + \gamma X_{v,d} + \epsilon_{v,d} \quad (1)$$

$y_{v,d,}$ = outcome for village v in district d

$D_{v,d} = 1$ if a village was randomly assigned to the program

δ_d = district fixed effects

$X_{v,d}$ = predetermined village characteristics
(includes all the balancing variables in our main specification)

Interpretation. If $y_{v,d}$ is N hotspots, α is mean hotspots in the control group (in omitted district). $\alpha+\beta$ is mean hotspots in treated villages (in that district). If $y_{v,d}$ is the any fire binary, α is the probability of fire for the average control village in the omitted district.

β is **difference between the two groups:** the treatment effect.

Inference by (1) standard t tests on β and (2) randomization inference

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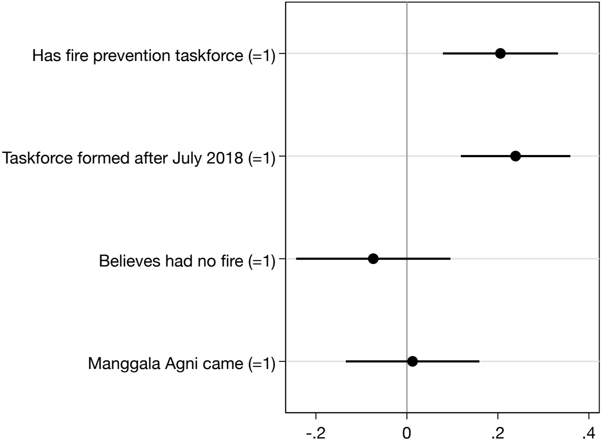
Practical steps taken to improve power...

..as needed to conduct ALL village visits before 2018 fire season

1. Collapse multiple treatment arms into single treatment with maximum payment to proximate villages
2. Pre-screen lower-risk subdistricts and villages out
3. Balance treatment and control groups on covariates of fire to make them as comparable as possible ex-ante
4. Generously oversample the control group
5. Consider binary and transformed-count outcomes
6. Use baseline census to soak up residual fire variation
7. Panel estimators with the historical outcome data, including matching on pre-trends and observables

Results—program “outputs”

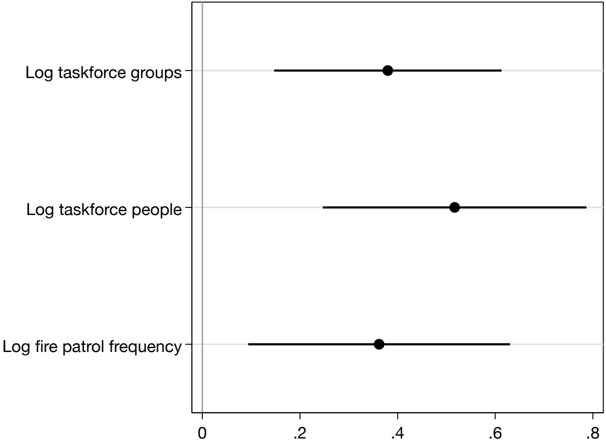
Treatment villages increased fire-related practices and behaviors (a)



Note: estimates use the endline survey of all treatment villages and 75 randomly selected control villages. District FE and balancing variables included throughout.

Results—program "outputs"

Treatment villages increased fire-related practices and behaviors (b)



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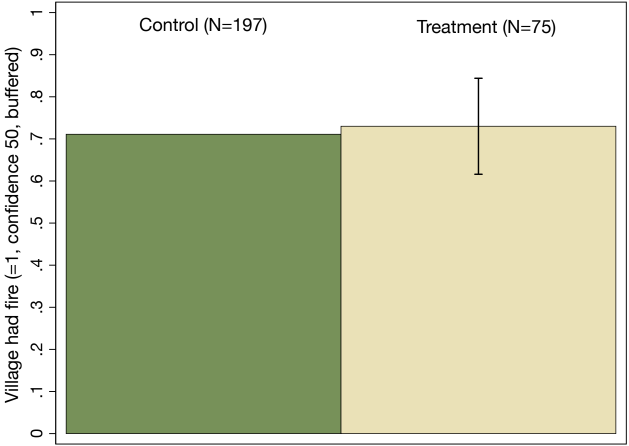
Results—program "outcomes"

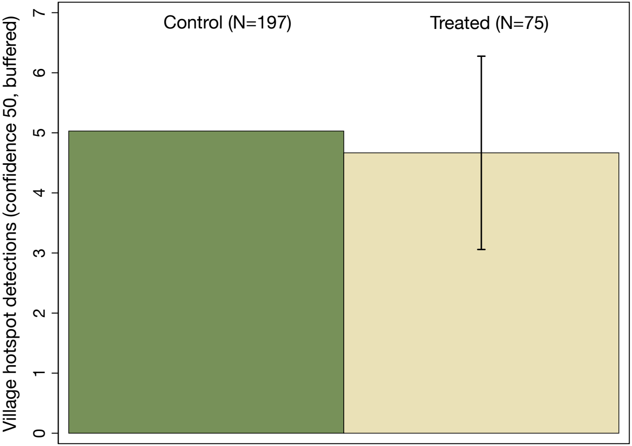
No evidence that treatment villages had less fire than controls (a)



Note: estimates estimate equation 1 using all treatment and control villages. District FE and all balancing variables included throughout.

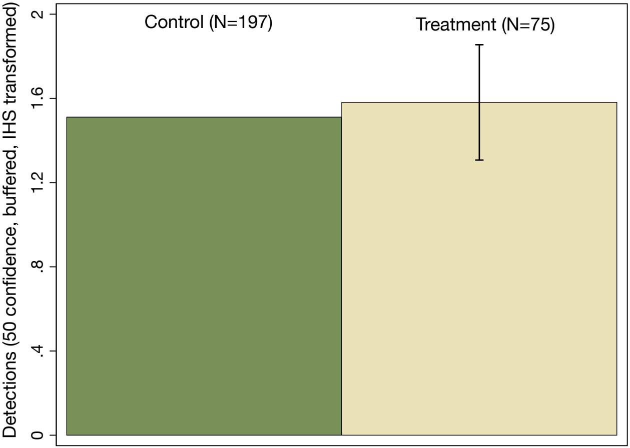
Results—program “outcomes”

No evidence that treatment villages had less fire than controls (b)



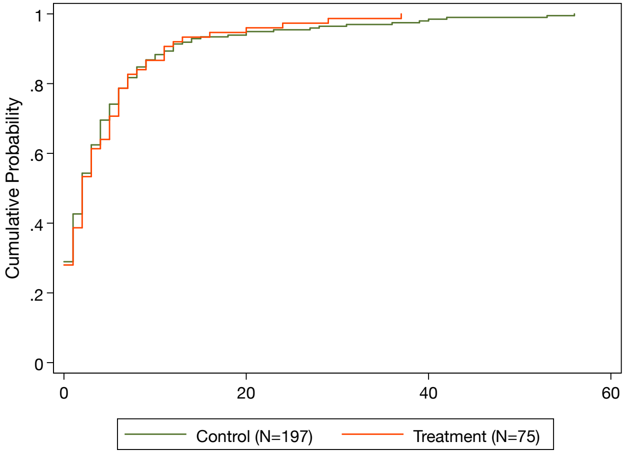
Results—program “outcomes”

No evidence that treatment villages had less fire than controls (c)



Results—program "outcomes"

Distributions of hotspots are across groups are quite similar



Note: a Kolmogorov-Smirnov test can't reject that the two distributions are equal.

Results—program “outcomes”

Tabulated regression results

Outcome	Village had any fire (=1)				
β (treatment=1)	0.009	0.016	0.030	0.019	0.003
Robust S.E	(0.061)	(0.061)	(0.057)	(0.058)	(0.061)
R.I. p-value	[0.874]	[0.874]	[0.600]	[0.740]	[0.967]
R^2	0.000	0.034	0.129	0.194	0.289
Adjusted R^2	0.004	0.019	0.106	0.157	0.186
District FEs	N	Y	Y	Y	Y
Pre-period fire history	N	N	Y	Y	Y
Other balancing vars	N	N	N	Y	Y
Additional covariates	N	N	N	N	Y
N villages	272	272	272	272	268

Results—program “outcomes”

Additional robustness checks

1. Binary, count, and count-transformed outcomes
2. Equal-sized treatment and control groups
3. Different confidence levels on the hotspot detections
4. Using full village area instead of buffering border zones
5. Using alternative VIIRS hotspot data, buffered and not, and with different confidence levels
6. Using the historical fires data to estimate panel models: random effects, difference-in-differences, matched difference-in-differences, higher-frequency event studies

Results—program "outcomes"

Raw differences in p (any fire) by measurement approach

Outcome variable	Control Mean/S.E.	Treated Mean/S.E.	T-test (p-value)	Normalized difference
All	0.817	0.827	0.857	-0.024
	[0.028]	[0.044]		
Confidence level 50	0.787	0.813	0.630	-0.065
	[0.029]	[0.045]		
Confidence level 80	0.563	0.560	0.959	0.007
	[0.035]	[0.058]		
All buffered	0.741	0.760	0.750	-0.043
	[0.031]	[0.050]		
CL 50 buffered	0.711	0.720	0.880	-0.021
	[0.032]	[0.052]		
CL 80 buffered	0.472	0.467	0.937	0.011
	[0.036]	[0.058]		

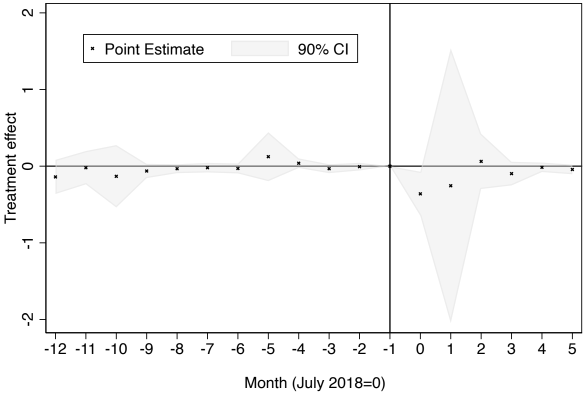
Results—program “outcomes”

Annual panel estimates, binary (any fire=1) outcome

Outcome	Village had any fire (=1), annual panel						
Estimator	Pool	Pool	RE	RE	DD	DD	PSM-DD
Column	1	2	3	4	5	6	7
Post*treat	0.013 (0.055)	0.013 (0.055)	0.013 (0.055)	0.013 (0.055)	0.013 (0.055)	0.013 (0.055)	0.021 (0.056)
P & T FE	Y	Y	Y	Y	Y	Y	Y
Year FE	N	Y	N	Y	N	Y	Y
Village FE	N	N	N	N	Y	Y	Y
Matched	N	N	N	N	N	N	Y
Obs	1632	1632	1632	1632	1632	1632	1548

Results—program "outcomes"

Month-specific treatment effects on N detections



Note: District-by-month and village FE. Data from 2012.
No discernible differences in days to first fire or average fire duration across groups.

No evidence of treatment effect heterogeneity

However, the study was not designed for sub-group analysis

		All groups	Treatment	Control
Village had any fire (=1)				
All districts	Mean	0.71	0.72	0.71
	N	272	75	197
Sintang	Mean	0.68	0.69	0.68
	N	94	26	68
Ketapang	Mean	0.67	0.67	0.67
	N	79	21	58
Kubu Raya	Mean	0.64	0.69	0.62
	N	39	12	26
Sanggau	Mean	0.86	0.87	0.87
	N	60	15	45

n.b. intensive margin differences (N hotspots) are smaller

Or by:

1. Past fire
2. OP SH
3. KD HS
4. Dirt road
5. Population
6. Area
7. Poverty
8. Plantation
9. No forest
10. Forest edge

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Impacts on tree cover loss

- No change in fire outcomes lets us rule out that villages switched from fire to non-fire methods in any major way
- Null impacts could still mask impacts on deforestation: relaxed capital constraints to mechanical clearing (Alix-Garcia et al., 2013), or standard income effects (Ferraro and Simorangkir, 2020).

Outcome	IHS-transformed village tree cover loss (ha)				
β (treatment=1)	-0.013	0.023	0.066	0.092	0.054
Robust S.E.	(0.152)	(0.146)	(0.136)	(0.128)	(0.122)
R.I. p-value	[0.935]	[0.875]	[0.621]	[0.450]	[0.674]
R^2	0.000	0.096	0.217	0.349	0.433
Adjusted R^2	-0.004	0.082	0.196	0.319	0.351
District FEs	N	Y	Y	Y	Y
Pre-period fire history	N	N	Y	Y	Y
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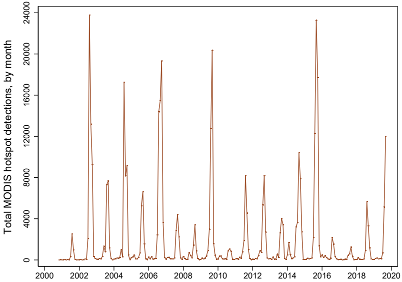
Potential explanations for the main null result

1. Not a bad fire year, or sticks more important than carrots?

Three key facts help rule out this potential explanation:

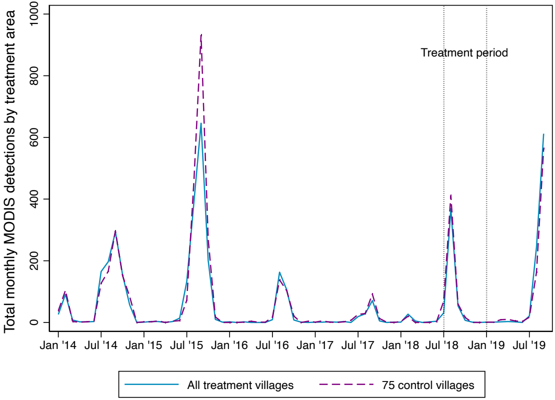
1. We compare relative differences across villages; national responses unlikely to affect only one of our groups
2. 70 percent of our study villages still had fire, so national responses couldn't have been that important
3. 2018 was not a particularly abnormal fire year



Potential explanations for the main null result

2. 6 months might not be enough to mobilize resources and change behavior

- Cannot rule this out for main incentive treatment
- Appears unlikely for the other two components
- One-off payment vs. continued benefit stream issue



Potential explanations for the main null result

3. Incentive payment might not be large enough

- Fire is by far the cheapest way to clear land (200 vs 600 USD/ha)
- In some study villages, literally no other way
- Value of a hectare of newly cleared land may be high
- NPV of ha of palm (3,800–9,600) far in excess of alternative livelihoods or that offered to maintain forest cover through carbon markets (600—1,000 in 2009)
- Burning and planting are indirect ways to claim land; people likely place a high value on de facto property rights absent de jure rights
- We could not meet the true opportunity costs here, and had to break the link between the individual setting fires and the incentive

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4. Collective action failure

Did we incentivize the wrong unit (i.e., villages rather than villagers)? And was it unrealistic to focus on the extensive margin?

Two main ways to think about it:

View one. Villager might have felt payment to village government would not benefit them directly, or that corruption in government created disincentives to adhere. Here, the private gain may outweigh the view of the communal gain. We estimate less than 1% of HH set fire.

Only need one defector from around 320 HH. Different size, social cohesion, and leadership quality across villages, and we were only powered to detect a large effect on average across all villages, seems likely.

View two. Villages tried, but still lacked capacity, tools to succeed fully.

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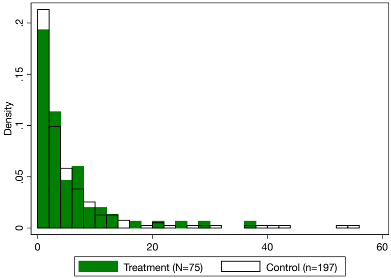
4. Collective action failure

Collective action requires critical mass adopting collectively dominant strategy and heterogeneous populations always have defectors, but they don't disrupt equilibrium until large enough. H: bad eq ⇒ good one.

Treatment villages are noticeably absent from tail, and clearly changed behavior. Did program push a very modest number of villages to a slightly better equilibrium without affecting most? Possibly!



Our interpretation

Explanations three and four—consistent with our theoretical framework and with the benefit of hindsight and extensive follow-up qualitative fieldwork—appear most important.

In Hadiwadjaja et al. (2020), we report findings from follow-up qualitative fieldwork in five of the most successful villages and five of the least, where we attempted to understand the experiences of these villages with the program.

Crucially, collective action problems in weak institutional environments are hard. We did not "gold plate" anything, and the opportunity costs matter—not low-productivity agriculture here, but a lucrative cash crop.

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