Crawford School of Public Policy

TTPI

Tax and Transfer Policy Institute

Fight fire with finance: a randomized field experiment to curtail land-clearing fire in Indonesia

---

TTPI - Working Paper 7/2022
May 2022

Ryan B. Edwards
Australian National University

Walter P. Falcon
Stanford University

Gracia Hadiwidjaja
World Bank

Matthew M. Higgins
Stanford University

Rosamond L. Naylor
Stanford University

Sudarno Sumarto
National Team for the Acceleration of Poverty Reduction & SMERU Research Institute

Abstract
This paper presents a randomized evaluation of collective pay-for-performance payments for ecosystem services. We test whether community-level fiscal incentives can curtail the use of land-clearing fire, a major source of emissions and negative health externalities, in a critical low-regulation setting. The program was implemented over the 2018 fire season in Indonesia as a three-part bundle: (a) awareness raising and training on fire prevention, (b) a small capital grant to mobilize fire fighting resources, and (c) the promise of a large conditional cash transfer at the end of the year if the village does not have fire, which we monitor by satellite. While program villages increase fire prevention efforts, we find no evidence of any large or statistically significant differences in fire outcomes. The null result is likely driven by a combination of the payment not being large enough and collective action failure, and offers a cautionary tale on the importance of carefully measuring additionality when evaluating payments for environmental services and other conservation programs.

Keywords: collective action, conditional cash transfer, deforestation, forest fire, Indonesia
JEL codes: H30, H70, 010, 013, Q15, Q23, Q24, Q28, Q51, Q54. Q57, R52

** We thank the Indonesian National Team for the Acceleration of Poverty Reduction (particularly Bambang Widianto and Elan Satriawan) and the many heads of the local governments for their cooperation implementing the project; the David and Lucille Packard Foundation, the Climate and Land-use Alliance, Daemeter Consulting Co., Anne and Robert Pedrero, and John Montazee for project financing; April Zhou and Shuhao Yang for excellent research assistance; and Chris Barrett, Paul Burke, Jonah Busch, Sylvain Chabe-Ferret, Eric Edmonds, Paul Ferraro, Blane Lewis, John McCarthy, Ruth Nikijuluw, Paul Novosad, Ben Olken, Zach Raff, Chris Snyder, Frances Seymour, Daniel Suryadarma, Peter Warr, Esther Duflo, two anonymous referees, and seminar participants at the NBER Summer Institute, AERE Summer Conference, WEAI, AARES, NOVAFRICA, ANU, and Dartmouth for helpful comments and discussions. All views and any errors are our own. The trial was registered in the AEA Trial Registry at AEARCTR-0003222.*

Tax and Transfer Policy Institute

Crawford School of Public Policy

College of Asia and the Pacific

+61 2 6125 9318

tax.policy@anu.edu.au

The Australian National University

Canberra ACT 0200 Australia

www.anu.edu.au

The Tax and Transfer Policy Institute (TTPI) is an independent policy institute that was established in 2013 with seed funding from the federal government. It is supported by the Crawford School of Public Policy of the Australian National University.

TTPI contributes to public policy by improving understanding, building the evidence base, and promoting the study, discussion and debate of the economic and social impacts of the tax and transfer system.

The Crawford School of Public Policy is the Australian National University's public policy school, serving and influencing Australia, Asia and the Pacific through advanced policy research, graduate and executive education, and policy impact.

1 Introduction

Land-related greenhouse gas emissions account for a quarter of global carbon emissions, with deforestation accounting for over ten percent (IPCC, 2018). Fire is an increasingly prominent way to clear land, particularly for agriculture (Adrianto et al., 2020). In addition to being a major source of emissions (Page et al., 2002; Marlier et al., 2015), forest fires generate negative health and other externalities rarely internalized by fire-setters (Frankberg et al., 2005; Jayachandran, 2009; Reddington et al., 2014; Sheldon and Sankaran, 2017; Rosales-Rueda and Triyana, 2018; Rangel and Vogl, 2019; He et al., 2020). Curtailing land-clearing fires—which increasingly burn out of control due to land use and climate change—is arguably one of the most important environmental and social challenges of the century (Bowman et al., 2009; Fernandes et al., 2017; Gaveau et al., 2014). Yet, policy responses have typically been ineffective and unsustainable, and the tropical developing countries where fire and deforestation are often most severe face major political and governance challenges in preventing and responding to fire (Dennis, 1999; Dennis et al., 2005; Seymour and Busch, 2016). At the same time, fire is the cheapest way to prepare land for agriculture and a long-standing traditional practice (Edwards et al., 2020). How to reduce fire in these low-regulation–high-deforestation settings is unclear, yet there is an urgent need to mobilize climate finance into fire and emissions reductions on the ground (Harrison et al., 2019; Jefferson et al., 2020).

Payments for ecosystem services (PES) and conditional cash transfers (CCTs) are popular and often effective policy approaches to spur behavior change: paying people to undertake behaviors they otherwise would not (Jack et al., 2008; Fiszbein and Schady, 2009; Parker and Todd, 2017; and Molina Millan et al., 2019). Behaviors being "incentivized" usually benefit society, for example reducing deforestation with PES and increasing vaccination and school attendance with CCTs. In recent years there has been growing momentum surrounding PES, ecological fiscal transfers, and a broader suite of reduced emissions from deforestation and land degradation (REDD) initiatives that seek to engender conservation through cash or in-kind compensation, penalties, and alternative livelihoods (Angelsen et al., 2018; Busch and Mukherjee, 2017; Busch et al., 2021). The first randomized evaluation of PES found reductions in deforestation amongst private forest owners in Uganda (Jayachandran et al., 2017). By making payments to individual land owners, the trial avoided the collective action problems we attempt to address here and there remains limited evidence on the effectiveness of PES-type interventions in settings of high deforestation and limited institutional capacity (Alix-Garcia et al., 2018; Borner et al., 2017; Pattanyak et al., 2010; Wiik et al., 2019; Wiik et al., 2020). The central policy question is whether fiscal incentive schemes can still be effective amidst imperfect property rights, land-use flux, and chronic underdevelopment, features characterizing many of the world's most vulnerable landscapes. Despite the increasing prominence of fire as way to clear land, there remains limited systematic evidence on the human drivers of fire (see, e.g., Arima et al., 2007; Edwards et al., 2020; Santika et al., 2020) and even less on how to stop them.[1] Here, we report findings from a large-scale evaluation of a unique pay-for-performance program—to our knowledge the first randomized evaluation of collective PES, in a low-regulation frontier setting, and with the potential to be realistically scaled—designed to fill this gap.

The key empirical challenge when trying to understand the effects of PES and other conservation programs is understanding what would have happened anyway. A credible counterfactual level of conservation or environmental degradation is needed to discern "additionality" (i.e., actual changes resulting from the program) and avoid paying for status quo levels of conservation (Burke, 2016). A credible counterfactual is particularly important here because PES require high additionality and a low share of compensated activities that would have happened anyway. We address this challenge by conducting a large-scale randomized controlled trial, deep in the Bornean jungle of Indonesia and covering around 90,000 households, testing whether community-level conditional cash transfers reduce the use of harmful land-clearing

[1]For example, Ferraro (2011) argues "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 and to explore competing notions of effective contract design."

fire. Specifically, we estimate the effects of cash transfers to Indonesian village governments to reduce fire—offered as PES contracts to village governments, with payments made after the fire season—by randomly assigning 75 villages to the program and 200 to a comparison group. Outcomes are monitored from space with state-of-the-art remotely sensed (i.e., satellite) data. The genuinely blind comparison group reduced the scope for confounding behavioral responses, for example, from contact with researchers or a survey team. With random assignment and satellite monitoring, we obtained the most credible estimate possible of what forest fires in our treatment villages might have looked like without the program.

Our four purposively selected fire-prone districts in West Kalimantan, Indonesia, offer the ideal setting for our study. Indonesia's catastrophic 2015–16 fire season is estimated to be associated with over 100,000 premature deaths and $16 billion in economic costs (Koplitz et al., 2016; World Bank, 2016). On several days, the fires emitted more carbon emissions than the entire United States economy (Harris et al., 2015). West Kalimantan was the province where the 2015–16 fires and subsequent fire events were most concentrated and our study villages offer a diverse mosaic of large-scale agricultural development, smallholder cash crops, and traditional rural livelihoods—including swidden agriculture with slash-and-burn techniques, otherwise known as shifting cultivation—on the forest frontier. To our knowledge, our experimental evaluation is the first of a payment-by-results—otherwise known as cash-on-delivery or pay-for performance—conservation program implemented at the community level, particularly one done in partnership with government, with a view to scale, and in a setting of relatively weak governance and rapid landscape change. Ex-post payment-by-results is distinct from other approaches where payments and other in-kind support is unconditionally provided to communities ex-ante to undertake conservation activities (e.g., Wilebore et al., 2019), regardless of whether those activities achieve the desired outcomes.

The intervention was a three-part bundle of: (a) village information and instruction on fire prevention (i.e., training), (b) an up-front Rp 10 million (approximately $750) capital grant at start of the experiment to ease liquidity constraints and help with fire prevention (i.e., small unconditional cash transfer), and (c) an ex-post conditional cash transfer of Rp 150 million (approximately $10,800, equal to around 15 per cent of the average village budget) at the end of the fire season (December 31, 2018) if successful in eliminating fires. To receive their ex-post payment, villages were required to not set fire from July–December (with minor exceptions built into the contract) and promptly extinguish natural fires. Payment was conditional on performance, which we monitored by satellite data and field verification. Modeled on Indonesia's community driven-development (CDD) program (the National Program for Empowerment (PNPM), largely viewed as a success), village facilitation and fire prevention training took place in program villages before agreements were signed.

Our focus on village collective action is important. Indonesia's sweeping decentralization reforms entered a new phase in 2014 (Naylor et al., 2019). After 15 years of district-centered reform (Fitriani et al., 2005), the 2014 Village Law devolved additional fiscal and administrative responsibilities down to Indonesia's over 80,000 villages (Antlov et al 2016). In addition to informing Indonesia's decentralization reform agenda, our community-level focus fills a broader knowledge gap in important ways. Prior studies have focused on private landowners with established property rights in regions with less land-use change (Alix-Garcia et al., 2015; Grillos et al., 2019; Jayachandran et al., 2017). By contrast, Kalimantan is one of the most dramatically changing landscapes in the world. Property rights vary, with centrally managed large concessions, local land markets, and traditional regimes. Fire is particularly common amongst farmers with obscure property regimes, small plots, and a lack of formal title (Purnomo et al., 2017). The sheer number of households (over 90,000 in our study villages) make individual or plot-level contracts and monitoring infeasible and unaffordable. With village heads accountable through regular elections, our intervention instead sought to achieve change through collective action and exploit local leaders as agents of change (Martinez-Bravo, 2017).[2]

[2] Wahyudi and Wicaksono (2020) go a step further and argue that the village funds could themselves be a REDD+ PES payment, and offer suggestions on how this might work.

The results of our study send a potentially important message to other researchers studying PES-type programs and policy makers interested in fiscal incentives for conservation. The program caused villages to increase fire prevention behavior. More resources were allocated to fire prevention activities. More fire-fighting task forces were formed. Virtually all were formed after the village facilitation meetings for the program. More people were involved in fire monitoring and suppression. Villages conducting fire patrols increased the frequency of their patrols. Twenty-one of the 75 villages involved in the pay-for-performance program managed to go fire-free for the entire 2018 dry season. However, the remaining 72 percent of the program villages had fires detected over the program period. 71 percent of the control group villages also had detections. Statistically, the probability and extent of fire are not distinguishable across the treatment and control groups. The distributions of hotspot detections are also remarkably similar, and we find no discernible impacts on tree cover loss. These null results are robust to alternative satellite sources, levels of detection confidence, and methods of estimating treatment effects. We cautiously conclude that the program had no major impacts on fire-setting behavior due to high opportunity costs (i.e., the payment may not have been enough, especially for the infra-marginal fire-setters) and a collective action failure.[3] Detailed survey evidence and follow-up qualitative work together suggests that a big difference between the most successful and poorest performing villages was commitment to the program, as observed in the leadership of the village head or another prominent figure and resulting community mobilization.

Our experiment was powered to detect a sixteen percent reduction in the probability of fire from a baseline level of around seventy percent. Alternatively, we are powered to detect a 40 percent reduction in hotspot counts per village on the intensive margin. The reduction in deforestation found in Jayachandran et al., (2017) was around 50 percent, an outlier in the distribution of treatment effects from similar PES programs but a magnitude we can rule out here. However, it is much easier to not cut down a tree than it is to extinguish a fire. Although we cannot

[3] Sommerville et al. (2010) also evaluate the impacts of a community-based forest use intervention in Madagascar and similarly find changes in attitudes but no change in forest use behaviors and outcomes.

rule out very small effects—which may be policy-relevant and certainly more plausible—PES and other incentive programs require high levels of additionality. Impacts need to be large to justify expanding our pilot to other provinces or a larger scale-up. Herein lies the value of a randomized evaluation. Without a credible counterfactual comparison group, one might have concluded that the program delivered reductions in fire when the 21 successful villages are precisely as many as we would expect without the program.[4] The adoption of fire prevention practices was insufficient to deliver the fire free outcomes desired. Neither was paying explicitly for them. Disbursing the 150 million IDR incentive payment ex-ante to all 75 program villages—a common PES practice—would have cost 11,250 million IDR. By only paying to those who went fire-free, we saved 72 percent (i.e., 8,100 million IDR, or over half a million USD) and the 3,150 million IDR actually disbursed was unlikely to have reduced fire more than had no payments been made at all. Since our novel intervention was implemented as a bundle (information, ex-ante unconditional cash transfer, and ex-post conditional cash transfer) we did not test whether any element would be effective alone in isolation, but it appears unlikely. We caution that (a) economic incentives may at times be ineffective in spurring enough behavioral change to reduce externalities and conserve the environment, especially when involving difficult collective action problems or low-regulation settings, (b) programs need to measure additionality carefully, and (c) the policy and research communities should remain open to other approaches for reducing anthropogenic forest fires in Indonesia and other countries.

The article proceeds as follows. Section 2 describes the program setting, theory, and implementation. Section 3 explains the data and our approach to fire monitoring. Section 4 details the experimental design. Section 5 presents the main results, robustness checks, and impacts on tree cover loss. Section 6 concludes with our leading hypotheses why the intervention did not work as we hoped.

[4]For example, Watts et al. (2019) study a village incentive scheme without randomisation, the Fire Free Village program we aim to evaluate at scale, and argue that program villages had less fire. There are six program and six non-program villages in this study.

2 The pay-for-performance program

2.1 Setting

We targeted West Kalimantan, Indonesia (see Figure 1) for its persistent and severe fires, recent deforestation, forest stock, peat soil, recent growth in and high share of independent oil palm smallholders, and governance challenges.[5] The majority of forest fires are intentionally started by local landowners, other community members, and in a few cases, “outsiders”, as a cheap way to clear land (Purnono et al., 2019). From 2001–18, West Kalimantan lost 3.32 million hectares of tree cover, equivalent to 24% of its total tree cover and 150% the rate of Indonesia as a whole. Commodity expansion, chiefly oil palm, is an important driver of deforestation. West Kalimantan has the largest share of smallholder managed oil palm, yet the largest remaining forest area on the island (Abood et al., 2014; Austin et al., 2019; Edwards, 2019; Edwards, 2020; Sloan et al., 2017).[6] We also selected West Kalimantan due to our confidence in our field partner Sampan Kalimantan, a highly-respected and legally-credentialed local environmental NGO.

Figure 2a plots total monthly hotspot detections nationally to highlight two important facts: (a) most fires take place after July, and (b) although 2018 was not as extreme as 2015, there was not an abnormally low level of fire. Figure 2b plots annual hotspot detections in West Kalimantan, other provinces on Kalimantan, and Riau. The series are characterized by year-on-year fluctuations rather than an increase over time, and West Kalimantan had the second highest number of fires in 2018.[7]

[5]For example, Mongabay reported on a bribes-for-permits scheme in West Kalimantan in 2019 (https://news.mongabay.com/2019/12/indonesia-palm-oil-permits-bribes-corruption-kpk/) and Human Rights Watch on persistent and widespread human rights violations around planation developments (https://www.hrw.org/report/2019/09/23/when-we-lost-forest-we-lost-everything/oil-palm-plantations-and-rights-violations)

[6]West Kalimantan had fire well before oil palm expansion intensified, principally traditional small-scale fires by residents (e.g., slash-and-burn fires for swidden agriculture and using fire to attract animals when hunting). Thus, we seek to reduce not only modern land clearing fire but also these other practices which under changing land and climate conditions have been generating greater externalities over the years.

[7]Vetrita and Cochrane (2019) provide a helpful overview of Indonesian fire frequency and related land-use and land-cover change, with a focus on the peatlands.

2.2 Intervention

Our primary intervention was the offer of a comparatively large community level payment if villages eliminate (c.f., reduce) the use of land-clearing fire over the 2018 fire season. It aimed to reduce fire by making it less attractive than (a) not clearing land, (b) clearing it legally without fire at higher cost, or (c) allowing natural or spreading fires to run their course, and by activating collective action. Both forces are needed for success.

Our aggressive incentive aimed for elimination rather than reduction for practical reasons. First, prior fire fighting initiatives had aimed for and claimed to have "fire free" villages, and we sought to evaluate this popular type of initiative more rigorously. Second, focusing on going "fire-free" made success and the payment disbursement easier to explain (i.e., more feasible and tractable). [8] In this sense, our payment levels were not particularly informed by the size of the externalities (e.g., larger vs. smaller fires, different types of land generating different externalities) but the need to offer a strong enough incentive to test the underlying collective mechanism (i.e., the first-order issue).

The economic incentive (i.e., payment) should be enough to offset the lower costs and potentially greater economic benefits from clearing land with fire. For example, converting a hectare of forest for palm oil production will be orders of magnitude more profitable to landowners—yielding net present values of between \$3,835 and \$9,630—than preserving it for \$614–\$994 of carbon credits in 2009 (Butler et al., 2009). The cost of clearing by fire is estimated to be one third of mechanical clearing, at \$200 and \$595 USD per hectare (Simorangkir, 2007; Tacconi et al., 2007; Tan-Soo and Pattanayak, 2019). For the median village, the 150 million IDR incentive was around 12 percent of its 1,307 million IDR budget in 2018. Within our budget constraint, we considered this to strike an appropriate balance between the number of villages we could offer it to (i.e., the sample size) and the relative size of the payment (i.e., maximizing the

[8] In contrast, a program focused on marginal reductions would require village-specific (c.f., average treatment and control group) counterfactuals to evaluate progress and determine payments.

treatment “dosage” to test the underlying behavioral mechanism).[9] Crucially, a larger payment would be politically and practically infeasible (i.e., not scalable beyond the pilot). Figure 3 shows the 2018 village budgets, populations, budgets per capita, and incentives per capita, giving a sense of the relative size and variability of the incentive.[10]

People set fires for many reasons—reaping different benefits, internalizing different costs. Sometimes they are set by groups, outside actors, rather than individual community members. Some are accidents. Since different land users have different and often unobservable levels of willingness to accept avoiding the use of fire (let alone to fight one they did not start), policies need to cater for heterogeneity. However, the maximum potential net present value of every available hectare, assuming conversion to agriculture and excluding negative externalities, makes such payments prohibitively expensive. Although it significantly weakens the link between the individual or group decision to set or control a fire and the incentive, a community-level approach invoking collective action and leveraging social influence is our response to these challenges and one feasible way to map a single policy intervention to the decision-makers, costs, and benefits.[11] The community-level approach is also by necessity: imperfect land rights, land-use flux, and the sheer number of households make an individual-level incentive system (or, more specifically, targeting infra-marginal fire-setters) practically infeasible in terms of administration, cost, information needs, and scalability.

Villages are the key unit of economic, social, administrative, and fiscal organization in the Indonesian countryside. In addition to being the smallest administrative unit arguably best suited to manage the commons (see, e.g., Oldekorp et al. (2019) for a successful case), villages receive significant fiscal transfers and are responsible for their own budgets (Lewis, 2015). Existing fiscal infrastructure allowed us to “top up” village budgets and ensured our pilot was designed

[9] Although we did estimate the social benefit associated with the fire reductions in calculating the payments, the actual number of fire and burned area in the treatment villages were quite small, suggesting that these payments were likely in excess of what the technically correct payment level would have been setting the payment to equal the social benefit.

[10] Additional descriptive statistics on village budgets are at Appendix Table A1.

[11] See also D’Adda (2011) on motivation crowding in environmental protection, Cinner (2018) on how behavioral science might help conservation, and Gneezy et al., (2011) on when incentives might increase prosocial behavior.

for scale and realistically implemented.[12] Village heads are accountable to their communities through regular direct elections and fire has been argued to increase around elections (Purnomo et al., 2019).[13] Our pilot was thus premised on the potential of village leaders to act as agents of change when faced with salient benefits for constituents (Olken, 2010; Jack and Recalde, 2014). Project implementation closely follows PNPM, a community-driven development program involving village cash transfers, facilitations, and technical support in implementing projects of village choice.[14] One part of the program (PNPM Generasi) combined community block grants for health and education with performance bonuses. Also evaluated using a randomized controlled trial (Olken, Onishi, and Wong, 2014), the incentives improved health outcomes and spending efficiency but had no effect on education. Unlike PNPM, we did not select the most promising villages for our program and reducing the use of fire is not something people are particularly keen on (c.f., free health and education).[15]

Although different villages likely have different collective "willingness to accepts" and discriminatory payment schemes tend to be more efficient, equal payments were necessary to be considered fair (c.f., Chen et al., 2010). Payments were as high as our budget allowed to ensure a sufficient incentive for larger villages. One key limitation—which, with the benefit of hindsight, appears important—is that the Village Law transfers are relatively new and large in historical context. Although the prospect of 10–15 percent additional funding might be significant, it may feel less so if still adapting to a cash bonanza. A negative incentive, while likely more potent (e.g., due to loss aversion and social pressure), was infeasible because it (a) would have involved deductions from village funds (viewed as entitlements, promised in the 2014 election by both

[12] We opted for cash over in-kind transfers for administrative ease, because it was more flexible and less prescriptive, and because we had no strong prior that in-kind would be more effective (c.f., Grillos, 2016).

[13] Deforestation is also closely related to district elections and the world palm oil price (Cisneros, Kis-Katos, and Nuryartono, 2021).

[14] PNPM was essentially scaled up to all rural villages through the 2014 Village Law, which is lighter on facilitation and technical support.

[15] However, our qualitative autopsy of the best and worst performing villages suggests that successful villages tended to be smaller, more closely knit, better organized villages with stronger leadership. These village characteristics, capabilities, and motivations may well have been important for success, here, for PNPM, and for thinking about the Village Law in general.

presidential candidates), and (b) could have harmed poorer communities more likely to set fire (Edwards et al., 2020). The positive incentive was also important to test whether "topping up" local government budgets is a promising way to operationalize external climate finance and turn international and national REDD+ agreements into improved environmental outcomes on the ground.[16]

2.3 Implementation

This section chronologically describes the implementation of the program. After selecting the sample and randomizing villages, meetings with district heads (bupatis) and relevant district-level agencies were held to: explain the project to government officials; answer outstanding questions and receive necessary approvals; and notify treatment villages of the opportunity to participate.[17]

Village facilitations (i.e., training and information) were then conducted May–July 2018 (i.e., before the fire season) by Sampan Kalimantan. Facilitators had years of experience facilitating sustainability programs and each attended three days of training in April 2018. Each facilitation was held in a central village location over two days with three parts: (a) a facilitation with the village head and typically the entire village government staff; (b) a public facilitation for all residents, usually in the village government office; and (c) a baseline survey with the village head or secretary to better understand the program villages and improve project implementation.

The government and public facilitations covered similar material and lasted around three

[16] Our intervention, if successful, provided such a mechanism, allowing international climate finance to be channeled directly towards fire and emissions reductions in some of the most remote and at-risk parts of the developing tropics.

[17] The terms of our intervention were negotiated during the meetings and finalized in Memoranda of Understanding (MOUs) between districts and the research team. Discussions were also held with central ministries and other critical non-government stakeholders in the region and in Jakarta. No contact was made with the control villages until after the program, as our primary outcome data are all observed remotely from satellites and no baseline beyond what we have in administrative data was needed for the control group. Using the bupati offices to notify sample villages helped to legitimize the study in the eyes of local communities and increase village buy-in, as did working with Sampan Kalimantan and the Indonesian National Team for the Acceleration of Poverty Reduction (TNP2K) under the Office of the Vice-President.

hours. First, the financial incentive was explained clearly in the simplest possible terms. The village would be monitored by satellite for the presence of any fires from July 1st (or from the day of facilitation for villages that were facilitated after that date) to December 31st 2018. Villages with no hotspots detected via the National Aeronautics and Space Administration's (NASA) Moderate Resolution Imaging Spectroradiometer (MODIS) would receive a cash prize of IDR 150,000,000 (roughly $10,800 USD) into the village bank account. Facilitations also explained satellite hotspot detection with demonstrations and answered technical questions.

Next, we explained how villages could maintain traditional slash-and-burn fires without triggering a hotspot and jeopardizing success. Dayak people—for whom small-scale slash-and-burn fires are an important customary activity—account for one third of West Kalimantan residents. Working in West Kalimantan and with a stark contract (i.e., hard threshold) required us to differentiate traditional Dayak fires for subsistence farming, which are legal under Indonesian law, from larger land-clearing fires. A key concern was that small-scale legal fires would trigger hotspots, creating a "real world confound" for our experiment and adding noise to performance assessment.[18] To address this issue, villagers were required to pre-register the time, date, and location of traditional swidden fires with village governments. Such fires needed to follow customary requirements: they are not permitted to burn on peat, or for longer than 12 hours. Pre-registered fires were cross-referenced with MODIS. Matching hotspots were not counted when determining success but, importantly, since the control group was not registering swidden fires, the hotspot data used for the analysis of treatment effects was not modified on this basis.[19] Extensive information on fire-free agricultural practices were also provided, and we explained what resources were available to villages for fire prevention and suppression.

[18]To be clear, they can trigger not only hotspots but larger fires and do also have environmental and health externalities, but this concession was necessary for community and policy-maker buy-in.

[19]In practice, the confidence filters applied to the data in practice filtered out these self-reported swidden fires from the treatment group, which is helpful in that the satellite data and report data are thus consistent in the treatment and control group. More concretely, we do not believe small, controlled swidden fires to be a concern for the estimation of treatment effects because they were removed from both groups by the filter. Short-burning, small fires also will only be picked up by the satellite if burning when they pass over, which means most will by construction be missed.

Finally, we informed villages they would receive an unconditional cash transfer of IDR 10,000,000 (roughly $750) upfront to fund additional fire prevention efforts. This additional component of the treatment followed concerns raised in scoping visits regarding resource constraints and existing village funds already being committed. The goal here was to relax liquidity constraints and ensure villages had the capacity to change behavior in response to the incentive. Up-front funds were transferred to villages within one week of facilitations and ex-post discussions, surveys, and inspections of budgets indicated that transfers were spent as indicated.

After facilitation, research staff did not visit treatment villages until the end of the monitoring period.[20] Contact was again made at the end of the program, when we conducted an endline survey. In addition to our 75 program villages, we then made first contact with 75 randomly-selected control group villages to survey them as well (i.e., control villages had no contact with the research team until endline). Twenty-one of the 75 program villages had no hotspots and successfully earned their IDR 150,000,000 prize. All 75 villages were notified of the results March– April 2019 and winning villages sent representatives to celebratory meetings at bupati offices. Payments were made May–June 2019.

3 Data

3.1 MODIS hotspots

Our primary outcomes of interest are (a) whether a village had a fire, and (b) the number of fires detected per village. We measure fire as thermal hotspot detections in the NASA MODIS Active Fire Product (MCD14ML), publicly available at 1 kilometer resolution and based on over four satellite passes per day. The MODIS Active Fire Product includes the location, date, and time of detection for each hotspot detected by the Terra or Aqua MODIS sensors and, at the time of

[20] In-person visits were minimized to keep program costs as low as possible to ensure replicability and scalability. A WhatsApp group connected research staff to treatment villages and allowed follow-up questions. The group was active. Village heads asked questions and offered advice to each other on fire prevention, shared upcoming weather updates, and shared success stories.

the experiment, was generally regarded as the most accurate and complete method for detecting fire (Langner et al., 2007; Langner and Siegert, 2009; Cattau et al., 2016; Tansey et al., 2008).

We construct outcome variables by spatially joining hotspots detected from 1 July to 31 December 2018 to Indonesia's official 2016 village boundaries, applying two filters (50 confidence and a 500m buffer), and counting the hotspots in each jurisdiction. Figure A1 maps total MODIS hotspot detections, with a 50 confidence filter, across all villages in West Kalimantan in 2018. In addition to its cost and quality benefits, it is important to note that this measure does not discriminate across fire type (e.g., natural or land-clearing fire) and captures duration, scale, and intensity well by increasing (a) in the times a given fire is observed in the same space, and (b) in any multiple detections of single or related fires across pixels. It is also a key element of and positively correlated with tree cover loss (see, e.g., Figure A2) but easier to measure and monitor, a greater source of health and environmental externalities, and politically more salient.

MODIS data offer two other important advantages for our study. Since data go back to 2001, we used historical data to target the most at-risk villages and ensure the treatment and control groups had similar fire history. Additionally, remote-sensing reduced the cost and increased the quality of data collection, and allowed us to monitor the control group for the duration of the experiment without contact. The genuinely blind control group significantly reduced the risk of behavioral responses that might threaten the internal validity of the experiment, for example through John Henry or Hawthorne-type experimenter effects.[21]

3.2 Performance verification and payment

Our agreements stipulated that villages had to (a) have no hotspots detected and (b) promptly extinguish any naturally occurring or spreading (e.g., into the village from outside)

[21]One limitation of MODIS hotspots is that, since hotspots are heterogeneous in their burned area, carbon emissions, deforestation, biodiversity loss, and health impacts, they map imperfectly to social costs (See Figure A2). To spur behavioral change, however, it is only the opportunity cost of clearing to the village which should need factored in, while from a policy perspective the incentive should not be set at a level higher than the social cost. Given the small number of fires in our sample, we believe the incentive was likely much greater than the social cost in most villages.

fire to be deemed successful and receive payment. 563 hotspots were detected inside our 75 treatment villages during the monitoring period. In determining success, we were lenient in which hotspots were included in determining success—erring on the side of being over-cautious and risking paying an undeserving village, rather than accepting a false negative and failing to pay a deserving village. Hence, we eliminated hotspots with a confidence value under 50 (the standard filter in the literature), within a 500 meter buffer of village boundaries (as MODIS has a 1 kilometer spatial resolution, boundaries are in practice imperfect, and border disputes raise attribution challenges), and that matched traditional swidden fires pre-registered with us. 346 hotspots remained in the 54 unsuccessful villages. One important practical implication of filtering was how effective it was in filtering out smaller, swidden agricultural fires. All fires reported as controlled burns were filtered out from 0–50 confidence, and a number of the fires reported to us a small controlled burns did not register as hotspots either (i.e., those that did register were all significant).

For every program village with three or less hotspots (25 villages), we manually inspected aerial photography. Hotspots in residential areas (e.g., a trash fires and mosque spire reflections) or unrelated to land use change were eliminated. Exclusions were rare but ensured any villages which narrowly missed the prize were afforded the benefit of the doubt and manually inspected.[22] All treatment villages agreed with their final success-fail status. Crucially, the final hotspot data used to evaluate the program, rather than determining payments, was not modified based on any registered fires or exclusions and treated the treatment and control villages precisely the same in this regard.[23]

[22]There were two exclusions (Selampung and Pagar Mentimun). For both, the hotspot counts went from three down to two, so did not affect whether they received the payment.

[23]In practice, these data were identical in terms of the binary outcome and only very slightly different in the counts.

3.3 Baseline census and other data

We constructed a rich baseline village census to (a) check the similarity of the two study groups (i.e., verify the randomization procedure worked), and (b) provide covariates when estimating treatment effects. The baseline census brought together, as a large cross-section, the 2014 census of village heads (i.e., Potensi Desa, or PODES), the 2013 Agricultural Census, the 2015 SMERU Poverty Map (based on small-area estimation), the locations of palm oil mills on Global Forest Watch, area and peat calculated in GIS, and MODIS hotspots in the 17 preceding years.

We also collected three rounds of primary data through field visits. First, we undertook scoping visits across West Kalimantan, East Kalimantan, Riau, and with key policy makers in Jakarta, prior to the experiment, to collect information on design issues and the appetite for our project.[24] Second, we conducted an initial survey of treatment villages during facilitations (as discussed above with implementation).[25] Third, we conducted an end-line survey of all 75 treatment villages and 75 control villages (limited by budget and operational constraints) randomly selected from the main 200-village control group. The survey was conducted with the village head or secretary to gather information on the 2018 fire season, including land clearing activities, local fire response behavior, the presence of other nearby fire-prevention efforts we might need to take into account, and government spending on fire prevention and suppression. Treatment villages were also asked to account for the IDR 10,000,000 they received up front.

We complement the MODIS hotspots with two auxiliary outcome datasets. Visible Infrared Imaging Radiometer Suite (VIIRS) data offer a helpful robustness check on MODIS data, as

[24] Scoping questions included: what are the current village budgets and planned expenditures? Were village heads and farmers interested in our trial? What performance levels would they agree to, and how could we allow minor breaches and set tolerance levels? Did they trust the payments to come, and our assessment strategy? Who would be the best field partner? What challenges did participants see coming between them and getting the money? What should we be monitoring through the experiment beyond take up and compliance? Did village heads agree with out boundaries? Were there any threats to the experimental design we were missing? Were there any legal issues regarding the agreements and payments? Were there further behavioral, welfare, governance, or fiscal outcomes which might be interesting?

[25] The surveys were designed to help us better understand the villages beyond the information in our baseline census and from qualitative discussions, including land cover, fire prevention efforts, budgets, regulations, and the presence of corporations and other fire prevention programs in the village. Control villages were not surveyed, as exposure to our team is part of the treatment and could contaminate comparisons.

it comes it comes from satellites with different technology and resolution. As a proxy for deforestation, we calculate village tree cover loss from the Global Forest Change Dataset 2000–18 (Hansen et al., 2013).

4 Experiment design

4.1 Sample, randomization, and balance

We focused on four districts in West Kalimantan province: Kubu Raya, Ketapang, Sanggau, and Sintang.[26] To ease logistics and ensure our program targeted the most high-risk areas, we restricted the sample to villages (a) in the eight most fire-prone sub-districts in each district, and (b) that had hotspots in at least two of the last three years.[27] Pre-processing ensures study villages start from similar baseline levels, reduces the variance in our outcomes, allows us to target the most at risk, significantly eases field logistics, and partially alleviates the concern that some villages may be so far behind or ahead in conservation that the average differences in success (i.e., going fire-free) between treatment-control groups would be harder to discern.

75 out of 275 pre-selected villages were randomly assigned to the program, blocked by district. All villages offered to opportunity to participate in the program did. Remaining villages formed the control group. Since we were only constrained in the number of villages we could offer the program to (i.e., monitoring additional villages from space was free apart from our time), we oversampled the control group to improve statistical power.[28] We re-randomized within districts 1000 times and selected the random assignment which maximized pre-treatment

[26] The districts were selected on a similar basis to the province, targeting those with greatest need and the highest potential returns. As with provinces, we compared smallholder palm cultivation level and growth, fire history, peat soil, and intact forest across districts. Ketapang and Sintang usually have considerably more hotspots (in total, across all the villages in each) than Sanggau and Kubu Raya (see Figure 2, Panel C). None had significantly fewer fires in 2018 than in an average non-El Nino year (i.e., our study year seems broadly representative).

[27] Many villages even in these fire-prone districts were too developed or remote to be currently experiencing large-scale forest fires or land use change.

[28] The constraints were budgetary and the need to complete facilitations before the 2018 fire season began. Delaying to get financial certainty, more resources, or more time in the field was not an option. We had strict project timelines, and the Indonesian government was poised to move ahead with similar programs at scale (e.g., through their fire Grand Design). We needed to get the pilot in the field and completed before any scale-up.

similarity between the treatment and control groups (Lock and Rubin, 2012).[29] Figure 4 maps the treatment assignment.

Table 1 presents the means, standard errors, p-values from a t-test of the difference in means (conditional on district fixed effects), and pairwise normalized differences across treatment arms (where a value over 0.25 roughly indicates imbalance) for the balancing variables. The average study village had around 330 households, the whole study 90,000 household, and the program (i.e., treatment) 25,000 households. The number of households is balanced across treatment and control groups, a critical consideration as uniform village payments vary in per capita terms (see Figure 3 and Table A1). Area is also balanced, which is important as fires mechanically increase with village area.

One concern with re-randomization is that it can sometimes lead to large imbalances in other characteristics (Bruhn and McKenzie, 2009). Our baseline census captures a wide range of observables, a selection of which we present in Table 2.[30] Statistically significant differences emerge for two variables: the number of government staff, and the age of the village head. Differences are quantitatively small (no more than expected from random chance) and we show that estimates are similar when including baseline covariates (including the number of government staff and the age of the village head). Table 2 also paints a helpful descriptive picture of our study villages. The average village poverty rate was 8 percent, slightly below the national average of 11 percent. The average village had around ten staff and a 41 year old (male) leader. Sixty percent report plantation crops as their primary source of income, around twenty percent were on peat soil, more than 90 percent burned for agriculture, more than half cooked with firewood, and around 60 percent burned trash. More than half were not accessible all-year around

[29]The minimum balance to accept a given randomization was set to a joint p-value of 0.95. Balancing variables include hotspot detections in 2013, 2014, and 2015, the number of oil palms planted in the village, the education level of the village leader, whether the village road was dirt, the number of households in the village, and village land area (i.e., key correlates of fire identified in Edwards et al. (2020)). In hindsight, we suggest instead using pair-wise matching, as discussed in McKenzie and Bruhn (2009).

[30]The selection is simply that which we consider to be of descriptive interest in understanding what our study villages looked like—relevant for thinking about fiscal incentives, collective action, and the environment. We cannot rule out imbalances in unobservables which may be important here, such as behaviour and beliefs, but this threat seems unlikely.

due to the rainy season.

4.2 Estimation and inference

The null hypothesis our experiment seeks to test is that the program has no effect on fire outcomes. We estimate the following equation to test it:

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

$y_{v,d,}$ is the outcome for village v in district d. $D_{v,d}$ is a treatment indicator equal to one if a village was randomly assigned to the program. δ_d are district dummies. $X_{v,d}$ includes predetermined village characteristics, balancing variables in the preferred specification (described in the previous sub-section and in the table notes for relevant estimates). $\epsilon_{v,d}$ is a mean zero error term adjusted for arbitrary heteroskedasticity.

When $y_{v,d}$ is total number of village hotspot detections, α is the mean hotspot detections in the control group for the omitted district. $\alpha + \beta$ is the mean hotspot detections in treated communities for that district. When $y_{v,d}$ is a dichotomous indicator for whether the village had any fire, α is the probability of fire for the average village in the control group for the omitted district. β is the difference between the two groups: the treatment effect. With successful randomization, any differences between the two groups represent an unbiased estimate of the causal effect of the program. Inference is based on (a) standard t-tests on β, and (b) randomization inference p-values calculated from the full distribution of potential treatment effects using 1000 random permutations of the treatment assignment.

Mindful of our relatively small sample and high-variance outcomes, several steps were taken to improve power within budget and operational constraints. First, we collapsed our initial design with multiple treatments (i.e., different payment levels and an information-only treatment, and more provinces) down to a single treatment and increased the size of the payment (i.e., maximized the "dosage") to ensure the study had the best chance of testing the first-order issue: whether the incentive induced a large collective behavioral response. Second, we pre-screened out low-risk subdistricts and villages, randomized within districts, and balanced on key covariates of fire to make our groups as comparable as possible. Third, we oversampled our control group. Fourth, we considered binary and transformed-count outcomes in addition to the higher-variance counts. Fifth, we used our baseline census to soak up residual variation. Sixth, we construct historical monthly and annual fire panel data to check our results with more efficient, better-powered panel estimators, and explore temporal dynamics.

5 Results

5.1 Fire-related behavior

Before proceeding to the results on fire outcomes, we use the endline survey to check for causal behavioral responses to the program. Any behavioral responses would be hampered if participants did not (a) trust the implementers, (b) understand the program, or (c) have the capacity to take the actions needed to reduce fires. Working closely with representatives from the TNP2K and Sampan Kalimantan ensured villages trusted the implementing organizations.[31] Our field visits and survey data suggest understanding and interest in the program was high, and up-front unconditional cash transfers ensured villages were sufficiently resourced to buy any new fire-fighting equipment they needed to respond to the incentive.

First, Table 3 presents the responses to questions asked only of the program villages in the final survey: to check whether program villages understood the program, demonstrated interest, and undertook some of the actions suggested in facilitations. All program villages received the up-front money, as expected. Inspections of village budget documents and post-program field discussions confirm that the money was, for the most part, used how it was supposed to be (i.e., resourcing the village fire response). Virtually all villages made efforts to spread

[31] TNP2K is well-regarded and project authority through the Vice-President ensured a high level of trust. Sampan Kalimantan also has a long and successful history in our study communities.

information further after the facilitations, mostly though community and religious meetings. Village leadership more often than not at least reported taking some concrete actions to try deliver the desired collective action response. Almost all villages reported behaviour change due to the incentive, with two thirds stating they changed their agricultural practices. Most also took more precautions relative to the prior year and said that the 150 million dollar incentive was enough to change behaviour. Indeed, over a third of villages already decided how to spend the money if successful.[32]

Second, Figure 5 presents impacts on nine fire-related behavioural outcomes. The sample is the 75 program villages and 75 randomly selected villages from the control group, data are responses in the end line surveys, and estimation follows the main specification (i.e., least squares, including district fixed effects and balancing variables). A key part of the facilitation process was emphasising the formation of fire-fighting and prevention groups (i.e., task forces), which are a popular community-based (i.e., collective) approach to fire monitoring and important guarding against accidental fires, those not supported by the community at large, and those that spread from outside. Program villages were twenty percent more likely to have a fire prevention task force and most were formed after facilitations. We also see large increases in the number of task forces within villages, the number of villagers participating in them, and the frequency at which patrols are undertaken. However, we find no evidence of any effect on the probability that the local fire department outside the village (Manggala Agni) was called in. Program villages were actually less likely to report owning fire-fighting equipment and we find no statistically significant effect on the probability that the village believed it had no fire during the monitoring period. Taking the results in Table 3 and Figure 5 together, participants appear to have understood the program well and taken actions to succeed. Some program villages appeared extremely active in fire prevention activities, with others perhaps more ambivalent.

[32]Those we spoke to in follow-up qualitative work after the experiment said these decisions were made together as a group and that the plans were indeed to spend the money on things that would benefit the community as a whole, suggesting elite capture by a few might have, perhaps surprisingly, been quite unlikely.

5.2 Satellite-based fire outcomes

The main finding is that, despite the fire-related actions taken in treatment villages, there is no evidence that the pay-for-performance program reduced fire beyond what would have been expected in the absence of the program. Figure 6 shows the main null result plainly as the differences in fire outcomes between the treatment and control groups. Differences are estimated by least squares, including district fixed effects and balancing variables. The outcome in Panel A is a binary indicator for whether a village had any hotspots detected (i.e., the converse of success and going fire-free).[33] 72 percent of treatment villages had fire. The proportion was 71 percent in the control group. The 95 percent confidence interval encompasses the control group mean and the difference is not statistically significant at any conventional level.[34]

Panel B of Figure 6 considers the number of hotspots detected per village. The average for both groups, per village, is around five. The 95 percent confidence interval is larger than in Panel A but a change of two or more hotspots can be ruled out. In Panel C we transform the count using the inverse hyperbolic sine (IHS), which reduces the influence of outliers and the variance without discarding any data. The implied semi-elasticity is 0.02 and standard error 0.04 (Bellemare and Wichman, 2019).

Table 4 progressively adds covariates. Panel A considers the binary outcome and Panel B the IHS-transformed count. Column 1 is the bivariate regression on the binary treatment indicator. Column 2 adds district fixed effects, Column 3 adds the number of fires in each village in 2015, 2014, and 2013 (to hold recent fire history constant), Column 4 adds the remaining balancing variables, and Column 5 adds additional correlates of fire from the baseline census.[35] Point

[33]Recall that the outcome data used for the analysis here was not manipulated based on swidden fire self-reporting by the treatment group, rather both groups had the same two filters applied: 50 confidence, and a 500m border buffer.

[34]The range of potential treatment effects in this confidence interval is -0.10–0.14, meaning we can not rule out up to a ten percent reduction in the probability of going fire free nor a 14 percent increase. Figure A3 maps the study villages by successful-unsuccessful status, and Figure A4 plots the observed treatment effect against those obtained from 1000 random permutations of the treatment assignment.

[35]Specifically, we add as controls the distance to the nearest palm oil mill, peatland, palm oil planted area, poverty rate, poverty gap, gini index, plantation village dummy, households without electricity, village burns for agriculture, village burns trash, village had a fire disaster in the last 3 years, number of people malnourished, village accessible by land, number of marketplaces, village had an agricultural kiosk, government spending on staff, capital, and other,

estimates lie between a 1 and 3 percentage point *increase* in probability of fire. We report robust standard errors in parentheses and randomization inference-based p-values in square brackets. Point estimates are not statistically significant at any conventional level.

Rising adjusted R-squared statistics suggest additional covariates help explain hotspots. Yet, standard errors are remarkably stable. The implied minimum detectable effects (MDE) are a sixteen percentage point reduction in the probability of fire in Panel A and forty percent reduction in the average number of hotspots detected in Panel B.[36] Program impacts would need to be considerably larger to justify a scale-up. The closest study to ours (Jayachandran et al., 2017) found a 50 percent decrease in forest loss, an effect size we can confidently rule out. Our focus on the extensive margin and being fire free—although significantly more demanding for participants—also allows us to rule out smaller effects, proportionally one third the size.

Figure 7 presents the raw outcome data. The top panel is a histogram of village hotspot detections. Distributions overlap closely but higher fire counts are noticeably absent from the treatment group, weakly suggesting they may have reduced hotspots on the intensive margin.[37] Cumulative distribution functions in the second panel reveal a similar pattern.

Figure 8 plots monthly total hotspot detections in the treatment group and equal-sized control group since January 2014. The first panel looks at the monthly count in the whole area of each group, the second the share of villages with any fire (i.e., binary outcome), and the third the average village hotspot count (i.e., the continuous fire count outcome). Figure 8 reveals three important facts. First, the approximately 70 percent of villages with fire in our study is not at all atypical. Second, the behavior and levels of the two series, however the outcome is measured, map very closely to one another before, during, and after the program. Third, the 2018 fire season

---

number of government staff, and village head age.

[36] Recall that for the 5 percent significance level and 80 percent power, the MDE can be computed by multiplying the standard error by 2.8.

[37] A Kolmogorov-Smirnov test cannot reject the null hypothesis of equal distributions at the one percent level of statistical significance. This is also the case if the same figures are plotted using the restricted equal-sized control groups, suggesting the pattern is not being simply driven by increasing the sample and the chance of getting an extreme value.

was almost as bad in our study villages as in the catastrophic 2015 season, a stark contrast to other parts of Indonesia where things had calmed down.

5.3 Robustness of the main null result

One statistical issue which can arise with extreme event outcomes like fire counts is that the chance of getting an extreme value—and the variance when the data are censored at zero—increases with sample size. Table A2 addresses this issue by showing similar results using the 75 randomly selected villages for our endline survey as the control group. Statistical power is reduced, suggesting the decision to oversample the control group was appropriate.

Results are also robust to alternative approaches to the hotspot data. Tables A3 and A4 show how the probability of fire and hotspot counts change based on different confidence levels for MODIS and VIIRS data (i.e., all fires, level 50, and level 80 for MODIS; all fire, nominal and high, and high filters for VIIRS) using (a) the full village area according to official 2016 village boundaries, and (b) 500m buffers. Under all configurations, there are no major differences between the treatment and control groups.[38]

In addition to our relatively low level of statistical power, a related concern is that our relatively small sample of treatment villages and block randomization mean the 75 treatments may not be truly independent. Panel estimates leveraging pre-period data help address both issues and are presented Tables A5–A7. The last column matches on pre-treatment observables, including fire trends, and then uses difference-in-differences to remove time-invariant unobservables. These estimates are slightly more precise, but still small and not statistically distinguishable from zero.[39]

An alternative panel data approach is to estimate period-specific effects in the months

[38]Fire levels under the level 50 confidence filter and buffering are not statistically different to those with different levels of filtering and without a buffer. Together they take us from around an 80 percent chance of fire down to around a 70 percent chance using MODIS. VIIRS is more sensitive: roughly half as many villages have hotspots detected when shifting from no filter to the high confidence filter.

[39]Although panel estimates are slightly higher-powered, the pre-registered approach and power calculations were based on the simple difference in means in Equation 1, Figure 6, and Table 4.

around the treatment. Figure 9 finds a statistically significant impact of the program in July, the first month following most facilitations (a few took place in the first few days of July). Thus, the program may have had its desired impacts on the number of fires (c.f., going fire-free) immediately, but not beyond that, let alone over the full, albeit short, contract period.

One possibility here is that program villages initially tried but then gave up, not dissimilar to how one often starts but then cannot sustain a diet or exercise regime. We explore this possibility further by comparing the number of days until the first hotspot in treatment and control villages. The average and median number of days until the first hotspot was 47 and 42 in the control group, and 47 and 43 in the treatment group, which suggest this potential explanation is unlikely.

5.4 Potential heterogeneity

A remaining concern is that—notwithstanding the 2014 Village Law decentralizing significant resources and responsibilities to villages—districts are important for service delivery and regional policy differences, enacting their own laws and regulations, including on land use and the environment, and offering specific policy instructions to villages which might mediate responses across districts.[40] Table 5 offers simple cross-tabulations showing how, despite our four study districts having quite different fire levels, there were no major differences between treatment and control villages within districts. We explore potential heterogeneity based on the remaining balancing variables and theory-based covariates in Table A8–A9. For example, the incentive may have been more potent in villages where it represented a larger amount in per capita terms or per hectare of land. Although we did not the design the experiment for sub-group analysis and we are clearly underpowered for this task, combined effects are all close to zero and not statistically significant.

[40] Irawan et al. (2019), for example, argue that village environmental programs related to REDD+ should be coordinated at the district level.

5.5 Tree cover loss

Since there was no change in fire outcomes, we can rule out the possibility that villages switched from fire to non-fire (e.g., mechanical) methods of land clearing in any major way. However, null impacts on fire could still mask positive or negative impacts on deforestation. For example, the up-front cash grant and potential windfall at the end of the year could see villages increase mechanical land clearing as capital constraints are relaxed (Alix-Garcia et al., 2013). In a recent study of the Gambia's Community-Driven Development Program, deforestation appears mostly related to new agricultural projects in areas with limited market access, rather than from a strong income effect (Heb et al., 2021). Ferraro and Simorangkir (2020) show how conditional cash transfers in Indonesia substitute for deforestation as a form of insurance and consumption substitution, with market goods substituting for deforestation-sourced goods. On the other hand, with the information provided as part of the facilitations, villages may be more hesitant to convert forest for agriculture.

Table 6 reports the treatment effects on village tree cover loss, a common proxy for deforestation (Burgess et al., 2012; Garg, 2019). Panel A uses tree cover loss in hectares as the outcome and Panel B its inverse hyperbolic sine transformation. The MDE with the main specification (Column 4) is 52 hectares—around 20 new smallholder oil palm farms, or one medium-sized one. Most point estimates point to increased tree cover loss but are imprecise.

6 Discussion

6.1 Potential explanations for the null result

A first potential explanation for the null result is that 2018 may not have been a bad fire year. Perhaps top-down "stick" components of fire prevention dominated our "carrot". For example, the high penalties and additional monitoring for fires during the 18th Asian Games may have been more important, making it difficult to detect program impacts. Two related studies in Indonesia also find PES-induced changes in practices but not outcomes because the community contribution to the outcome variance was small (Leimona et al., 2015; Amaruzaman et al., 2017).[41] Three key facts help rule out this explanation here. First, our experiment compares relative differences across villages and national responses are unlikely to affect one group and not the other. Second, national efforts were clearly not that important for our study villages: over 70 percent of them still had fire. Third, 2018 was not a particularly abnormal non-El Nino fire year for West Kalimantan, our four study districts (Figure 2), or our study villages (Figure 8).

A second potential explanation is timing: six months may not be long enough to mobilize resources and change behavior, particularly when it comes to long-standing cultural practices. We cannot rule out this explanation for the main incentive component of the program, which would need to be in place longer to evaluate longer-term effects. However, if the up-front cash or facilitations had lagged effects we should see these in subsequent years. Figure 8 showed no distinguishable differences the year after the program, when there were no conditional payments and successful villages had already been paid.[42] It could also be the case that, since conversion to agriculture yields an income stream in future years but our incentive was for one (i.e., not a continuing PES or annuity), the incentive did not align closely enough with the expected benefit stream from conversion, at least in the cases where the fire is deliberately set for land clearing.

The third and fourth explanations more closely reflect the program theory in Section 2. The payment might simply not have been large enough. The value of a hectare of newly cleared land may be high, especially in a poor society (Ickowitz et al., 2017). To the extent that fires are making way for oil palm—which we know was not always the case—the present value is far in excess of the value of alternative livelihoods or that offered to maintain forest cover through carbon markets. Burning and planting is also an indirect way to garner claims on land and people likely place a high value on de facto property rights in the absence of de jure rights. Since we were

[41] An important and related point is that fires, whether ignitions or spreads, are difficult, but certainly not impossible, to control, certainly much harder than, say, cutting down a tree on your property, as was the setting in Jayachandran et al. (2016).

[42] Alpizar et al. (2019) similarly find no effects from capacity building and information workshops after two years.

not targeting individual fire setters, but rather whole villages with a low share of defectors, the relevant price is that for the average household. With around 2,400 people or 400 households per village, the incentive was around USD 25 per household (USD 4 per person). Given the the relative difficulty in fighting fire versus, for example, not cutting down a tree, there may be a significant difference between ex-post incentive offered and not only the opportunity cost of burning for the infra-marginal fire-setter, but also for other individuals who need to extinguish fire or work together for the desired collective action response (e.g., if a fire spread in from outside and was technically no one's responsibility). That said, in our follow-up survey almost all villages said that the value of the conditional cash transfer was more than enough to make them change behaviour and we did find evidence suggesting at least some behavioural change, even without the desired final outcome.

Even if payments were large enough, a failure of the village collective action mechanism underpinning the whole experiment may be the most important explanation. First, a villager might have felt that a payment to the village government would not benefit them directly, or corruption in village government (or other elite capture concerns) created disincentives to adhere to the program.[43] The private gain—for a few or even just one—would outweigh their view of the communal gain for themselves and the village. Since the program objective was no fire, one defector from the whole village (around 320 households) is all it takes to be unsuccessful. Given the different size, social cohesion, and leadership quality across villages, and that we are only powered to detect a large effect on average across all program villages, this explanation seems likely. Indeed, our qualitative follow up work suggests, quite convincingly, that one commonality across the five most successful villages (not the case in the five unsuccessful villages also studied) was that these villages tended to be smaller, more ethnically homogenous, and tighter-knit communities with strong leadership (not necessarily the village leader). This hints not only at

[43]For example, Alesina et al. (2019) highlight how the deforestation results of Burgess et al. (2012) are concentrated in more ethnically diverse districts, and Bazzi and Gudgeon (2021) highlight how redistricting along group lines reduced conflict but that increased polarization, along ethnic lines, can increase it. Our four study districts did not split.

the importance of the local institutions in mediating the collective action responses, but since smaller villages are receiving larger per capita incentives also the incentive size issue. Since villagers usually set the fires (c.f., village governments), one might also argue we incentivized the wrong units and should have attempted to focus on infra-marginal people, where we would have been more likely to meet the relevant opportunity cost.

A second related possibility is that villages did, on average, try to prevent fires, but capacity constraints prevented intentions and actions from translating into final fire outcomes. For example, villages might not have known well enough how to prevent fire (i.e., the one-shot trainings may have been insufficient), still lacked the necessary tools after the small up-front grants, not known how to use such tools properly, or had trouble mobilizing everyone, all of which were arguably necessary conditions for success here, with the target being zero fires rather than simply a reduction. More generally, collective action involves a critical mass of people adopting a collectively-dominant strategy. Where the goal is, for example, to limit off-take to below renewal rates, critical mass compliance is sufficient. A heterogeneous population will almost always have defectors, but they don't disrupt the equilibrium collective strategy until this subpopulation grows large enough for the collective action arrangements to collapse. In this sense, the behavioral responses we see are not inconsistent with our main null findings. We hypothesized the incentive might tip communities from a bad collective equilibrium to a good one. Although this shift doesn't appear to have happened on average, treatment villages are noticeably absent from the upper tail of the distribution of hotspot detections (Figure 7) and they clearly, on average, changed their behavior (Figure 5). It could be the case that the program pushed a very modest number of treated communities into a better equilibrium of a smaller number of fires, without affecting most communities.[44] Indeed, given the sheer number of households covered by our study and the observed fire outcomes, the percent of households setting fires is almost always less than one percent.

On balance, our view is that the lack of any discernible changes in fire outcomes due to a

[44] We thank Chris Barrett for this valuable insight.

combination of a collective action failure and the incentive not being strong enough, at least for the infra-marginal individuals setting fires and even though the incentive is likely larger than the social costs. Relative to prior work, our null findings are unsurprising. Collective action problems are extremely challenging in weak institutional environments, working at the community rather than individual-level was a key innovation of this trial, and it is much harder to prevent wildfire than not cut down a tree. The opportunity costs here are also high: households' alternative here is not low-productivity agriculture as in Jayachandran et al. (2017) but an extremely lucrative cash crop. Our program also differs from most conservation and environmental services programs in our efforts to implement it in a manner it would potentially be scaled, rather than at a smaller scale and carried out by a motivated non-profit organization committed to success.[45] Our qualitative follow up work and the heterogeneneity uncovered across successful and unsuccessful villages, in terms of their size and local institutional quality, suggests that this lack of selectivity and not "selecting for success" plays an important role in explaining the effectiveness of the collective action response and the null effects on average across the study villages.

6.2 Concluding remarks

This paper presents the findings from one of the first randomized evaluations of collective pay-for-performance PES. We tested whether payment-for-performance incentive contracts with local communities can help reduce land-clearing fires increasingly common across the tropics. Our trial was set in the Indonesian province of West Kalimantan, a province rich in forest, peat soil, and recent agro-industrial economic development around palm oil, where some of most severe recent fire episodes were concentrated. The treatment was a bundled program of facilitated information and training, a small up-front unconditional cash transfer, and the promise of a large cash transfer at the end of the fire season if there the village managed to go fire-free and promptly extinguish any naturally occurring fires. Villages randomly assigned to the program were more

[45] One challenge with pilot experimental studies is they can sometimes be "gold-plated", with overly positive results not representative of the average effect the intervention may have in practice or when scaled-up (see, e.g., Peters et al., 2018; Sills and Jones, 2018; Usmani et al., 2020). Here, we explicitly designed our study to reduce this concern, and ensure our pilot was representative of what the scaled intervention might look like.

likely to mobilize fire-fighting resources and do patrols, but no less likely to have fire. Our leading explanations for the null result are that the opportunity cost of not clearing land with fire may simply be too high and a failure of the village-level collective action mechanism which the intervention and much of Indonesia's rural development agenda, for example following the Village Law 2014, is premised on.

Our findings do not immediately generalize to areas with lower demand on land, more resources, and better governance, areas where the pilot may have been more likely to succeed. Since the objective was to reduce fire, it was crucial to target the program somewhere the baseline level of fire setting is high and malleable regardless of year-on-year variation. We cannot rule out whether an incentive scheme might have worked only in more carefully selected villages with stronger local institutions and commitment to the program (e.g., as was the case for PNPM). However, evidence from such areas would tell us little about what works on the front line, where solutions are most needed.

The null finding was disappointing from the perspective of finding a solution to one of the world's most intractable and critical climate challenges. Yet, against a backdrop of few rigorous causal studies on the efficacy of conservation programs, the null result offers valuable new evidence on an important and popular community-based fiscal incentive approach (Bottazzi et al., 2018; Pynegar et al., 2018; Pynegar et al., 2019; Samii et al., 2014).[46] The lack of evidence supporting this type of intervention should serve as another call for researchers and policy makers to continue to partner, innovate, and rigorously evaluate more conservation programs, particularly those seeking to activate collective action and solve difficult political economy problems (Ferraro and Pattanayak, 2006; Angelsen et al., 2018, Asquith, 2020). The need for credible evidence on how to address human-lit wildfires and related tropical deforestation, which inspired this project, very much remains.

[46]Ferraro (2018, pg. 165) notes "every program that is implemented as a good idea to be applied, rather than a good hypothesis to be evaluated, is a missed opportunity to learn. In conservation science and practice, it's been mostly missed opportunities. We can do better."

References

Abood, S. A., Lee, J.S.H., Burivalova, Z., Garcia-Ulloa, J., and Koh, L.P. (2014) “Relative contributions of the logging, fire, oil palm, and mining industries to forest loss in Indonesia”, Conservation Letters, 8:1

Adrianto, H.A., Spracklen, D.V., Arnold, S.R., Sitanggang, I.S., and Syaufina, L. (2020), “Forest and Land Fires Are Mainly Association with Deforestation in Riau Province, Indonesia”, Remote Sensing, 12:1, 3.

Alesina, A., Gennaioli, C., Lovo, S., (2019) “Public goods and ethnic diversity: evidence from deforestation in Indonesia”, Economica, 86; 341

Alix-Garcia, J., Sims, K., Orozco-Olvera, V., Costica, L., Medina, J., and Monroy, S. (2018), “Payments for environmental services supported social capital while increasing land management”, Proceedings of the National Academy of Sciences, 115:27, pp. 7016–21.

Alix-Garcia, J., Sims, K., Yanez-Pagans, P. (2015) “Only one tree from each seed? Environmental effectiveness and poverty alleviation in Mexico’s payments for ecosystem services program”, American Economic Journal: Economic Policy, 71:1, pp. 1–40.

Alix-Garcia, J., McIntosh, C., Sims, K. R. E., and Welch, J.R. (2013) “ The ecological footprint of poverty alleviation: evidence from Mexico’s opportunidades program”, The Review of Economics and Statistics, 95:2, pp. 417–435.

Alpizar, F., Bernedo del Carpio, M., Ferraro. P., and Meiselman, B.S. (2019) “The impact of a capacity building workshop in a randomized adaptation project”, Nature Climate Change, 9, pp. 587–591.

Amaruzaman, S., Leimona, B. and Rahadian, N.P. (2017) “Maintain the sustainability of PES program: Lessons learnt from PES implementation” in Sumberjaya, Way Besay Watershed, Indonesia. Co-investment in ecosystem services: global lessons from payment and incentive schemes. Nairobi: World Agroforestry Centre (ICRAF).

Angelsen, A., de Sy, V., Duchelle, A.E., Larson, A.M., and Pham, T.T., (eds.) (2018) “Transforming REDD+: Lessons and new directions”, Bogor, Indonesia: CIFOR

Antlov, H., Wetterberg, A., and Dharmawan, L. (2016), “Village Governance, Community Life, and the 2014 Village Law in Indonesia”, Bulletin of Indonesian Economic Studies, 52: 2, pp. 161–183.

Arima, E. et al. (2007) “Fire in the Brazilian Amazon: a spatially explicit model for policy impact analysis”, Journal of Regional Science, 47:3.

Asquith, N. (2020) “Large-scale randomized control trials of incentive-based conservation: what have we learned?” World Development, 127, 104785

Austin et al. (2019), “What causes deforestation in Indonesia”, Environmental Research Letters, 14: 024007

Bazzi, S., and Gudgeon, M. (2021), “The Political Boundaries of Ethnic Divisions”, American Economic Journal: Applied Economics, 13:1, pp. 235–66.

Borner, J., et al. (2017), “The effectiveness of payments for environmental services”, World Development 96, pp. 359–374.

Bowman, M.J.S. et al. (2009). “Fire in the earth system”, Science, 324: 5926, pp. 481–4

Bruhn, M. and McKenzie, D. (2009) “In Pursuit of Balance: Randomization in Practice in Development Field Experiments”, American Economic Journal: Applied Economics, 1:3, pp. 200–32.

Bellemare, M. and Wichman, C.J., (2019) “Elasticities and the Inverse Hyperbolic Sine Transformation”, Oxford Bulletin of Economics and Statistics, 82:1, pp 50–61.

Bottazzi, P., Wiik, E., Crespo, D., and Jones, J.P.G. (2018), “Payment for environmental “self-service”: exploring the links between farmers motivation and additionality in a conservation incentive programme in the Bolivian Andes”, Ecological Economics, 150, pp. 11–23.

Burgess, R., Hansen, M., Olken, B. A., Potapov, P., and Sieber, S. (2012), “The Political Economy of Deforestation in the Tropics“, Quarterly Journal of Economics, 122:1, pp. 73–117.

Burke, P.J. (2016) “Undermined by Adverse Selection: Australia’s Direct Action Abatement Subsidies”, Economic Papers, 35:3.

Busch, J., and Mukherjee, A. (2017) “Encouraging State Governments to Protect and Restore Forests Using Ecological Fiscal Transfers: India’s Tax Revenue Distribution Reform”, Conservation Letters, 11:2.

Busch, J., et al. (2021) “A global review of ecological fiscal transfers” Nature Sustainability, 23 June 2021.

Butler, R., Koh, L.P., and Ghazoul, J. (2009), “REDD in the red: palm oil could undermine carbon payment schemes”, Conservation Letters, 2:2, pp. 67–73.

Cattau, M.E., Harrison, M.E,. Shinyo, I., Tungau S, Uriarte M, DeFries R (2016) “Sources of anthropogenic fire ignitions on the peat-swamp landscape in Kalimantan, Indonesia”, Global Environmental Change, 39, pp. 205–19.

Chen, X., Lupi, F., Vina, A., Guangming, H., and Jiangou, L. (2010), “Using cost-effective targeting to enhance the efficiency of conservation investments in payments for ecosystem services”, Conservation Biology, 24:6, pp. 1469–1478.

Cinner, J. (2018) “How behavioral science can help conservation”, Science, 362: 6147, pp. 889–890.

Cisneros, E., Kis-Katos, K., Nuryartono, N. (2021) “Palm oil and the politics and deforestation in Indonesia”, Journal of Environmental Economics and Management, 108.

D’Adda, G. (2011) “Motivation crowding in environmental production: evidence from an artefactual field experiment”, Ecological Economics, 70:11, pp. 2083–2097.

Dennis, R.A. (1999), “A review of fire projects in Indonesia, 1982–1998”, CIFOR. 105p.

Dennis, R.A., Mayer, J., and Applegate, G. (2005), “Fire, people, and pixels: Linking social science and remote sensing to understand underlying causes and impacts of fires in Indonesia’, Human Ecology, 33: 4, pp. 465–504.

Edwards, R.B (2019) “Export agriculture and rural poverty: evidence from Indonesian palm oil”, working paper.

Edwards, R.B. (2020) “Spillovers from agricultural processing”, working paper.

Edwards, R.B., Falcon, W. P., Higgins, M.M., and Naylor, R. L. (2020), “Causes of Indonesia’s Forest Fires”, World Development, March.

Fernandes, K., Verchot, L., Baethgen, W., Gutierrez-Velez, V., Pinedo-Vasquez, M., and Martius, C. (2017), “Heightened fire probability in Indonesia in non-drought conditions: the effect of increasing temperatures”, Environmental Research Letters, 12, 054002.

Ferraro, P.J., and Pattanayak, S.K. (2006) “Money for nothing? A call for empirical evaluation of biodiversity conservation investments”, PLoS Biology, 4:4, pp. 482–488.

Ferraro and Simorangkir (2020) “Conditional cash transfers to alleviate poverty also reduced deforestation in Indonesia”, Science Advances, 6:24.

Ferraro, P.J. (2011) “The Future of Payments for Environmental Services”, Conservation Biology, 25:6, pp. 1135–38.

Ferraro, P. J. (2018) “Are payments for ecosystem services benefiting ecosystems and people?” In: Effective Conservation Science: Data Not Dogma. Edited by Peter Kareiva, Michelle Marvier, and Brian Silliman, Oxford University Press.

Fitriani, F., Hofman, B., and Kaiser, K. (2005), “Unity in Diversity: The creation of new local governments in a decentralising Indonesia”, Bulletin of Indonesian Economic Studies, 41:1, pp. 57–79.

Fiszbein, A., and Schady, N. (2009) “Conditional cash transfoers: reducing present and future poverty”, A World Bank Policy Research Report, The World Bank, Washington, D.C.

Frankenberg, E., McKee, D., and Thomas, D. (2005), “Health Consequences of Forest Fires in Indonesia”, Demography, 52:1, pp. 100–29.

Garg, T. (2019) “Ecosystems and human health: The local benefits of forest cover in Indonesia”, Journal of Environmental Economics and Management, 98, 102271.

Gaveau DLA, Salim MA, Hergoualch K, et al. (2014) “Major atmospheric emissions from peat fires in Southeast Asia during non-drought years: evidence from the 2013 Sumatran fires”, Scientific Reports 4(6112).

Gneezy, U., Meier, S, and Rey-Beil, P. (2011), “When and why incentives (don’t) work to modify behavior”, Journal of Economic Perspectives, 25:4, pp. 191–210.

Grillos, T. (2016) “Economic vs. Non-Material Incentives for Participation in an In-Kind Payment for Ecosystem Services Program in Bolivia”, Ecological Economics, 141, pp.178–190

Grillos, T., Bottazzi, P., Crespo, D., Asquith, N., and Jones, J.P.G. (2019) “In-kind conservation payments crowd in environmental values and increase support for government intervention: a randomized trial in Bolivia”, Ecological Economics, E166.

Harris, N., Minnemeyer, S., Stolle, F., and Payne, O., (2015), "Indonesia's Fire Outbreaks Producing More Daily Emissions than Entire US Economy", World Resources Institute, [link]

Harrison, M., et al. (2019) "Tropical forest and peatland conversion in Indonesia: Challenges and directions", People and Nature, 2:1, pp. 4–28.

Hansen, M.C., Potapov, P.V., Moore, R., Hancher, M., Turubanova, S.A., Tyukavnia, A., Thau, D., Stehman, S.V., Goetz, S.J., Loveland, T.R., Kommareddy, A., Egorov, A., Chini, L., Justice, C.O., and Townshend, J.R.G. (2013), "High resolution global maps of 21st-century forest cover change", Science, 342:6160, pp. 850–853.

He, G., Liu, T., and Zhou, M. (2020) "Straw burning, PM2.5, and death: evidence from China", Journal of Development Economics, 145

Heb, S., Jaimovich, D., Schundeln, M. (2021), "Environmental Effects of Development Programs: Experimental Evidence from West African Dryland Forests", Journal of Development Economics, 153, 102737

Ickowitz, A., Sills, E., and De Sassi (2017) "Estimate smallholder opportunity costs of REDD+ : A pantropical analysis from households to carbon and back", World Development, 95, pp. 15–26.

Irawan, S., Widiastomo, T., Tacconi, L., Watts, J.D., and Steni, B. (2019) "Exploring the design of jurisdictional REDD+: The case of Central Kalimantan, Indonesia", Forest Policy and Economics, 108, 101853.

Jack, B. K., Kousky, C., and Sims, E. (2008) "Designing payments for ecosystem services: Lessons from previous experience with incentive-based mechanisms", Proceedings of the National Academy of Sciences, 105: 28, pp. 9465–9470.

Jack, B. K., and Recalde, M.P. (2014) "Leadership and the voluntary provision of public goods: Field evidence from Bolivia", Journal of Public Economics, 122: 2015, pp. 80–93.

Jayachandran, S. (2009), "Air Quality and Early-Life Mortality Evidence from Indonesia's Wildfires", Journal of Human Resources, 44: 4, pp. 916–54.

Jayachandran, S., et al. (2017) "Cash for carbon: a randomized trial of payments for ecosystem services to reduce deforestation", Science, 357: pp. 257–273.

Jefferson, U., Carmenta, R., Daeli, W., and Phelps, J. (2020), "Characterising policy responses to complex socio-ecological problems: 60 fire management interventions in Indonesian peatlands", Global Environmental Change, 60, 102027

Koplitz, S., Mickley, L., Marlier, M., Buonocore, J., Kim, P., Liu., T., Sulprizio, M., Defries, R., and Jacob, D. (2016), "Public health impacts of the severe haze in Equatorial Asia in September–October 2015: demonstration of a new framework for informing management strategies to reduce downwind smoke exposure", Environmental Research Letters, 11: 0984923.

Langner, A., and Siegert, F., (2009), "Spatiotemperal fire occurrence in Borneo over a period of 10 years", Global Change Biology, 15, pp. 48–62.

Langner, A., Miettinen, J. and Siegert, F., (2007), "Land cover change 2005–2005 in Borneo and the role of fire derived from MODIS imagery", Global Change Biology, 13, pp. 2329-40.

Leimona, B., Lusiana, B., van Noordwijk, M., Mulyoutami, E., Ekadinata, A. and Amaruzaman, S. (2015) “Boundary work: knowledge co-production for negotiating payment for watershed services in Indonesia”, Ecosystem services, 15, pp. 45–62.

Lewis, B.D. (2015) “Decentralising to Villages in Indonesia: Money (and Other) Mistakes”, Public administration and development, 35:5, pp. 347–359

Lock and Rubin (2012), “Re-randomization to balance tiers of covariates”, Journal of the American Statistical Association, pp.1412–1421.

Marlier, M., DeFries, R., Kim, P., Koplitz, S., Jacob, D., Mickley, L., and Myers, S. (2015) “Fire emissions and regional air quality impacts from fires in oil palm, timber, and logging concessions in Indonesia”, Environmental Research Letters

Martinez-Bravo, M. (2017) “The Local Political Economy Effects of School Construction in Indonesia”, American Economic Journal: Applied Economics, 9:2

Molina Millan, T., Barham, T., Macours, K., Maluccio, J.A., and Stampini, M. (2019), “Long-term Impacts of Conditional Cash Transfers: Review of the Evidence”, World Bank Research Observer, 34:1, pp. 119–159.

Naylor, R., Higgins, M., Edwards, R.B, and Falcon, W.P. (2019), “Decentralization and the environment: Assessing Smallholder Oil Palm Development in Indonesia”, Ambio, August 2019

Oldekop., J., Sims, K.R.E., Karna., B.K., Whittingham, M.J., and Argrawal, A. (2019) “Reductions in deforestation and poverty from decentralized forest management in Nepal”, Nature Sustainability, 2, pp. 421–428.

Olken, B. (2010), “Direct Democracy and Local Public Goods: Evidence from a Field Experiment in Indonesia”, American Political Science Review, 104:2

Olken, B., Onishi, J., and Wong, S. (2014),“ Should Aid Reward Performance? Evidence from a Field Experiment on Health and Education in Indonesia”, American Economic Journal: Applied Economics, 6:4

Page SE, Siegert F, Rieley J.O., et al. (2002). “The amount of carbon released from peat and forest fires in Indonesia during 1997”. Nature, 420, pp. 61–5.

Parker, S., and Todd, P. (2017) “Conditional cash transfers: the case of Progresa/Oportunidades”, Journal of Economic Literature, 55:3, pp. 866–915.

Pattanyak, S., Wunder, S., and Ferraro, P. (2010) “Show Me the Money: Do Payments Supply Environmental Services in Developing Countries?” Review of Environmental Economics and Policy, 4:2, pp. 254–274

Peters, J., Langbein, J., and Roberts, G. (2018) “Generalization in the Tropics—Development Policy, Randomized Controlled Trials, and External Validity”, World Bank Research Observer, 33:1, pp. 33–64.

Purnomo H et al (2017) “Fire economy and actor network of forest and land fires in Indonesia”, Forest Policy and Economics, 78, pp. 21–31.

Purnomo et al., (2019) “Forest and land fires, toxic haze, and local politics in Indonesia”, International Forestry Review, 21:4., pp. 486–500.

Pynegar, E.L., Gibbons, J.M., Asquith, N.M., and Jones, J.P.G. (2019), “What role should randomized control trials play in providing the evidence base for conservation?” Oryx, pp. 1–10.

Pynegar, E.L., Jones, J.P.G., Gibbons, J.M., and Asquith, N.M. (2018), “The effectiveness of payments for ecosystem services at delivering improvements in water quality: lessons for experiments at the landscape scale”, PeerJ, 6.

Rangel, M.A. and Vogl, T.S. (2019) “Agricultural Fires and Health and Birth”, Review of Economics and Statistics, 101:4, pp 616-630.

Reddington, C.L. (2014) “Contribution of vegetation and peat fires to particulate air pollution in Southeast Asia”, Environmental Research Letters,

Rosales-Rueda, M., and Triyana, M. (2018), “The Persistent Effects of Early-Life Exposure to Air Pollution: Evidence from the Indonesian Forest Fires”, Journal of Human Resources, forthcoming

Samii, C., Lisiecki, M., Kulkarni, P., Paler, L., Charvis, L., Snilstveit, B., Vojtkova, M., and Gallagher, E. (2014), “Effects of Payment for Environmental Services (PES) on Deforestation and Poverty in Low and Middle Income Countries: A Systematic Review”, Campbell Systematic Reviews, 10:1.

Santika, T. et al. (2020) “Interannual climate variation, land type and village livelihood effects on fires in Kalimantan, Indonesia” Global Environmental Change, 64, 102129

Seymour, F., and Busch, J. (2016), “Why Forests? Why Now?: The Science, Economics, and Politics of Tropical Forests and Climate Change,” Center for Global Development, Washington, D.C.

Sheldon, T., and Sankaran, C. (2017), “The Impact of Indonesian Forest Fires on Singaporean Pollution and Health”, American Economic Review: Papers and Proceedings, 107:5, pp. 526–529.

Sills, E., and Jones, K. (2018) “Causal inference in environmental conservation: The role of institutions”, Handbook of Environmental Economics.

Simorangkir, D. (2007) “Fire use: Is it really the cheaper land preparation method for large-scale plantations?”, Mitig Adapt Strategies Glob Change 12, pp.147–164.

Sloan, S., Locatelli, B., Wooster, M.J., and Gaveau, D., (2017) “Fire activity in Borneo driven by industrial land conversion and drought during El Nino periods, 1982–2010”, Global Environmental Change, 47, pp. 95–109.

Sommerville, M., Milner-Gulland, E.J., Rahajarharison, M., and Jones, J.P.G. (2010) “Impact of a community-based payment for environmental services intervention on forest use in Menabe, Madagascar”, Conservation Biology, 24:6, pp 1488–1498.

Tacconi, L., Moore, P.F., Kaimowitz, D. (2007) “Fires in tropical forests—what is really the problem? Lessons from Indonesia”, Mitig Adapt Strategies Glob Change, 12, pp. 55–66.

Tansey, K., J. Beston, A. Hoscilo, S. E. Page, and C. U. Paredes Hernandez (2008), “Relationship between MODIS fire hot spot count and burned area in a degraded tropical peat swamp forest in Central Kalimantan, Indonesia”, Journal of Geophysical Research, 113, D23112, Tan-Soo, J., and Pattanayak, S.K (2019) "Seeking natural capital projects: Forest fires, haze, and early-life exposure in Indonesia", Proceedings of the National Academy of Sciences

Usmani, F., Jeuland, M., and Pattanayak, S. (2020) "NGOs and the effectiveness of interventions", Review of Economics and Statistics, forthcoming.

Vetrita, Y., and Cochrane, M. (2019) "Fire frequency and related land use and land cover changes in Indonesia's peatlands", Remote Sensing, 12:2, 15.

Wahyudi, R., and Wicaksono, R.L. (2020) "Village fund for REDD+ in Indonesia: Lesson learned from policy making process at subnational level", Forest Policy and Economics, 119, 102274

Watts, et al. (2019) "Incentivising compliance: evaluating the effectiveness of targeted village incentives for reducing burning in Indonesia" Forest Policy and Economics, 108, 101956

Wiik, E., D'Annunzio, R., Pynegar, E. L., Crespo, D., Asquith, N.<., and Jones, J.P.G. (2019) "Experimental evaluation of the impact of a payment for environmental services program on deforestation", Conservation Science and Practice, e8.

Wiik., E., Jones, J.P.G., Asquith, N.M., Bottazzi, P., Gibbons, J.M., Kontoleon, A., and Pynegar, E.L. (2020) "Exporing mechniams and impacts of an incentive-based conservation program with evidence from a Randomized Control Trial", Conservation Biology, registered report accepted at Stage 1.

Wilebore, B., Voors, M., Bulte, E. H., Coomes, D., and Kontoleon, A. (2019), "Unconditional transfers and tropical forest conservation: evidence from a randomized control trial in Sierra Leone", American Journal of Agricultural Economics, 101:3, pp. 894–918.

World Bank (2016) "The cost of fire: An economic analysis of the 2015 fire crisis", Indonesia Sustainable Landscapes Knowledge Note 1. World Bank , Jakarta.

Table and Figures

Table 1: Descriptive Statistics—Balancing Variables

Variable	Control (N=198) Mean/S.E.	Treated (N=75) Mean/S.E.	T-test (p-value)	Normalized difference
Area (ha)	13,600	14,200	0.799	-0.031
	[1,360]	[2,130]		
Fires in 2015 (N)	12.379	11.893	0.912	0.021
	[1.695]	[2.679]		
Fires in 2014 (N)	8.182	8.453	0.870	-0.018
	[1.099]	[1.536]		
Fires in 2013 (N)	3.970	3.240	0.273	0.163
	[0.334]	[0.434]		
Households (N)	333.020	318.387	0.429	0.066
	[15.519]	[26.226]		
Oil palm area (ha)	153	132	0.810	0.063
	[24.7]	[33.7]		
Dirt road (=1)	0.677	0.600	0.260	0.161
	[0.033]	[0.057]		

Notes: This table presents, separately for the treatment and control group, means, standard errors, and differences for the pre-determined variables used to ensure the randomization achieved balance. The t-test regression includes district fixed effects and the normalized difference is the raw normalized difference without regression adjustment. The sample is 75 treated and 198 control villages.

Table 2: Balance Test

Variable	Control (N=198) Mean/S.E.	Treated (N=75) Mean/S.E.	T-test (p-value)	Normalized difference
Distance to mill (m)	56368	58144	0.571	-0.050
	[2556]	[4110]		
Peat soil (=1)	0.177	0.213	0.773	-0.094
	[0.027]	[0.048]		
Poverty rate	0.084	0.082	0.734	0.034
	[0.003]	[0.006]		
Poverty gap index	0.014	0.014	0.873	0.005
	[0.001]	[0.001]		
Gini index	0.235	0.235	0.904	-0.004
	[0.002]	[0.004]		
Plantation village (=1)	0.662	0.613	0.597	0.101
	[0.034]	[0.057]		
Households w/out electricity (N)	134.056	137.387	0.709	-0.025
	[9.448]	[16.130]		
Cooks with firewood (=1)	0.520	0.587	0.202	-0.133
	[0.036]	[0.057]		
Burns for agriculture (=1)	0.924	0.907	0.630	0.064
	[0.019]	[0.034]		
Burns trash (=1)	0.566	0.640	0.323	-0.151
	[0.035]	[0.056]		
Fire disaster last 3 yrs (=1)	0.081	0.067	0.641	0.053
	[0.019]	[0.029]		
Malnourished (N)	0.313	0.307	0.889	0.006
	[0.068]	[0.158]		
Accessible by land (=1)	0.369	0.400	0.488	-0.064
	[0.034]	[0.057]		
Always accessible (=1)	0.338	0.360	0.673	-0.045
	[0.034]	[0.056]		
Marketplaces (N)	0.404	0.120	0.591	0.078
	[0.304]	[0.063]		
Agricultural kiosk (=1)	0.091	0.080	0.717	0.038
	[0.020]	[0.032]		
Govt staff expenditure (IDR)	96.965	72.333	0.167	0.178
	[11.438]	[2.981]		
Govt capital expenditure (IDR)	111.657	78.187	0.228	0.155
	[17.792]	[7.668]		
Govt other expenditure (IDR)	38.328	33.947	0.544	0.104
	[3.063]	[4.593]		
Govt staff (N)	9.631	11.347	0.045**	-0.261
	[0.218]	[1.324]		
KD age (years)	41.549	39.667	0.022**	0.303
	[0.443]	[0.719]		

Notes: This table presents, separately for the treatment and control group, means, standard errors, and differences, conducting a balance test over the differences for a selection of variables observed in our baseline census. The selection of variables is those we considered to be of descriptive interest and balance generally holds across our entire baseline census. The t-test regression includes district fixed effects and the normalized difference is the raw normalized difference without regression adjustment. The sample is 75 treated and 198 control villages.

Table 3: Program Outputs—Endline Survey Results

Survey question (yes = 1; no=0)	Mean	Std. Dev.
Received 10 million upfront payment	1	(0)
Efforts to spread information post-facilitation	.97	(.17)
Hosted other facilitation meetings	.94	(.23)
Hung posters about program	.35	(.48)
Distributed flyers about program	.32	(.47)
Wrote formal annoucement letter	.65	(.48)
Attended other community group meetings	.85	(.36)
Spread information through religious meetings	.72	(.44)
Contacted nearby companies about program	.49	(.50)
New village regulation was made due to the program	.14	(.35)
Taskforces recently formed due to project	.34	(.48)
Believe village was successful in preventing fires	.92	(.28)
Decisions made on how to spend 150 million	.34	(.48)
Observed behavior change due to incentive	.96	(.20)
Villagers changed how they cleared for agriculture	.63	(.49)
Less use of fire to clear land	.54	(.50)
More precautions when using fire relative to last year	.92	(.28)
More notifications when clearing than last year	.92	(.28)
150 million is enough to change village behavior	.86	(.35)
Number of treatment village respondents	71/75	

Notes: This table reports the average village responses in the endline survey to questions seeking to understand program implementation, in particular whether program villages undertook various actions to reduce fire. Control group villages were not asked these questions, and four villages from the treatment group could not be reached.

Table 4: Main Results–Effects on Fire

Column	1	2	4	5	6
Panel A 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
Panel B outcome			IHS fire count (N)		
β (treatment=1)	0.037	0.036	0.077	0.070	0.090
Robust S.E	(0.168)	(0.167)	(0.145)	(0.139)	(0.144)
R.I. p-value	[0.823]	[0.831]	[0.577]	[0.601]	[0.520]
R^2	0.000	0.022	0.281	0.368	0.449
Adjusted R^2	0.004	0.008	0.261	0.338	0.368
District FEs	N	Y	Y	Y	Y
Pre-period fire history	N	N	Y	Y	Y
Additional balancing variables	N	N	N	Y	Y
Additional covariates	N	N	N	N	Y
N villages	272	272	272	272	268

Notes: This table reports the main regression results of the experiment. Treatment is a binary indicator set equal to one if a village was randomly assigned to the program. All estimates use least squares and IHS refers to the inverse hyperbolic sine transformation of the count variable. MODIS hotspot data are filtered on 50 confidence and a 500m buffer from village boundaries. Additional balancing variables include hotspot detections in 2013, 2014, and 2015, the number of oil palms planted in the village, the education level of the village leader, whether the village road is soil, the number of households in the village, and village land area. Additional covariates are distance to the nearest palm oil mill, peatland, palm oil planted area, poverty rate, poverty gap, gini index, plantation village dummy, households without electricity, village burns for agriculture, village burns trash, village had a fire disaster in the last 3 years, number of people malnourished, village accessible by land, number of marketplaces, village had an agricultural kiosk, government spending on staff, capital, and other, number of government staff, and village head age.

Table 5: Fires by District and Treatment Status

		All groups	Treatment	Control
Panel A: 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
Panel B: Number of hotspots detected per village (N)				
All districts	Mean	4.92	4.64	5.03
	SD	8.28	6.73	8.81
	N	272	75	197
Sintang	Mean	2.82	2.62	2.9
	SD	3.7	2.84	4
	N	94	26	68
Ketapang	Mean	6.42	5.57	6.72
	SD	10.8	8.63	11.54
	N	79	21	58
Kubu Raya	Mean	8.28	7.23	8.81
	SD	12.62	9.56	14.05
	N	39	12	26
Sanggau	Mean	4.07	4.6	3.88
	SD	4.25	5.04	4
	N	60	15	45

Notes: This table reports the means for the primary outcomes and the number of village observations for treatment and control villages, by districts and for all districts. For the count outcome, the standard deviation is also reported. Hotspots are filtered on 50 confidence and a 500m buffer from village boundaries.

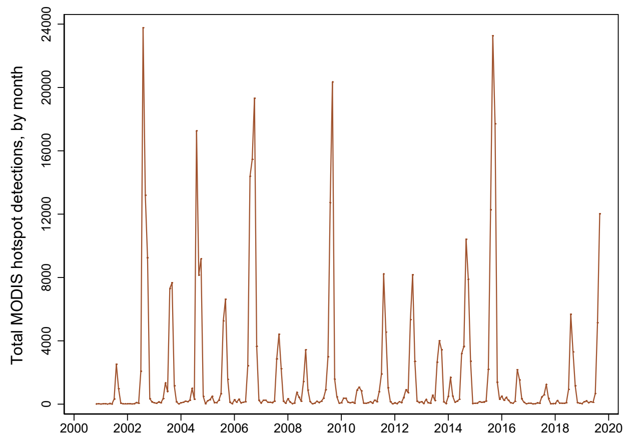
Table 6: Effects on Village Tree Cover Loss

Column	1	2	3	4	5
Panel A outcome	Village tree cover loss (ha)				
β (treatment=1)	3.999	6.501	13.947	17.026	14.253
Robust S.E.	(20.617)	(20.399)	(19.134)	(18.698)	(18.895)
R.I. p-value	[0.849]	[0.751]	[0.497]	[0.400]	[0.498]
R^2	0.000	0.028	0.155	0.218	0.298
Adjusted R^2	-0.004	0.013	0.133	0.181	0.195
Panel B 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
Additional balancing variables	N	N	N	Y	Y
Additional covariates	N	N	N	N	Y
N villages	272	272	272	272	268

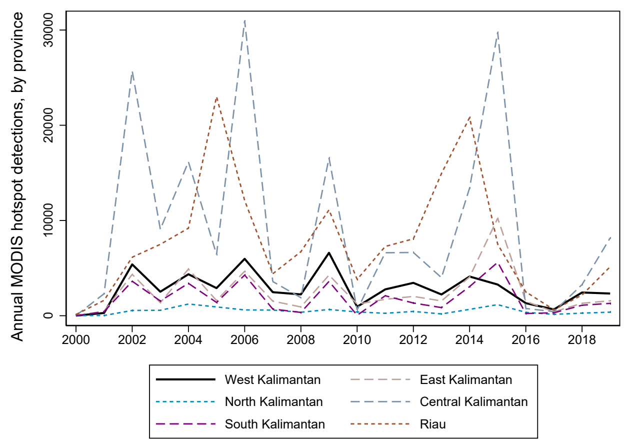
Notes: This table reports results on village tree cover loss. Treatment is a binary indicator set equal to one if a village was randomly assigned to the program. All estimates use least squares and IHS refers to the inverse hyperbolic sine transformation of the count variable. Additional balancing variables include hotspot detections in 2013, 2014, and 2015, the number of oil palms planted in the village, the education level of the village leader, whether the village road is soil, the number of households in the village, and village land area. Additional covariates are distance to the nearest palm oil mill, peatland, palm oil planted area, poverty rate, poverty gap, gini index, plantation village dummy, households without electricity, village burns for agriculture, village burns trash, village had a fire disaster in the last 3 years, number of people malnourished, village accessible by land, number of marketplaces, village had an agricultural kiosk, government spending on staff, capital, and other, number of government staff, and village head age.

Figure 1: West Kalimantan, Indonesia

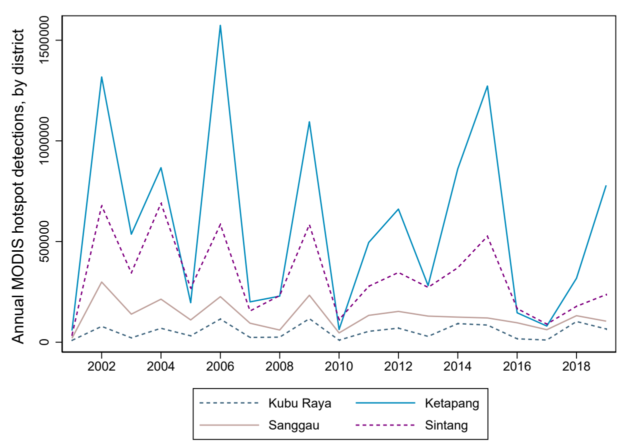
Figure 2: Hotspot Detections Over Time



(a) National-level—Monthly Data



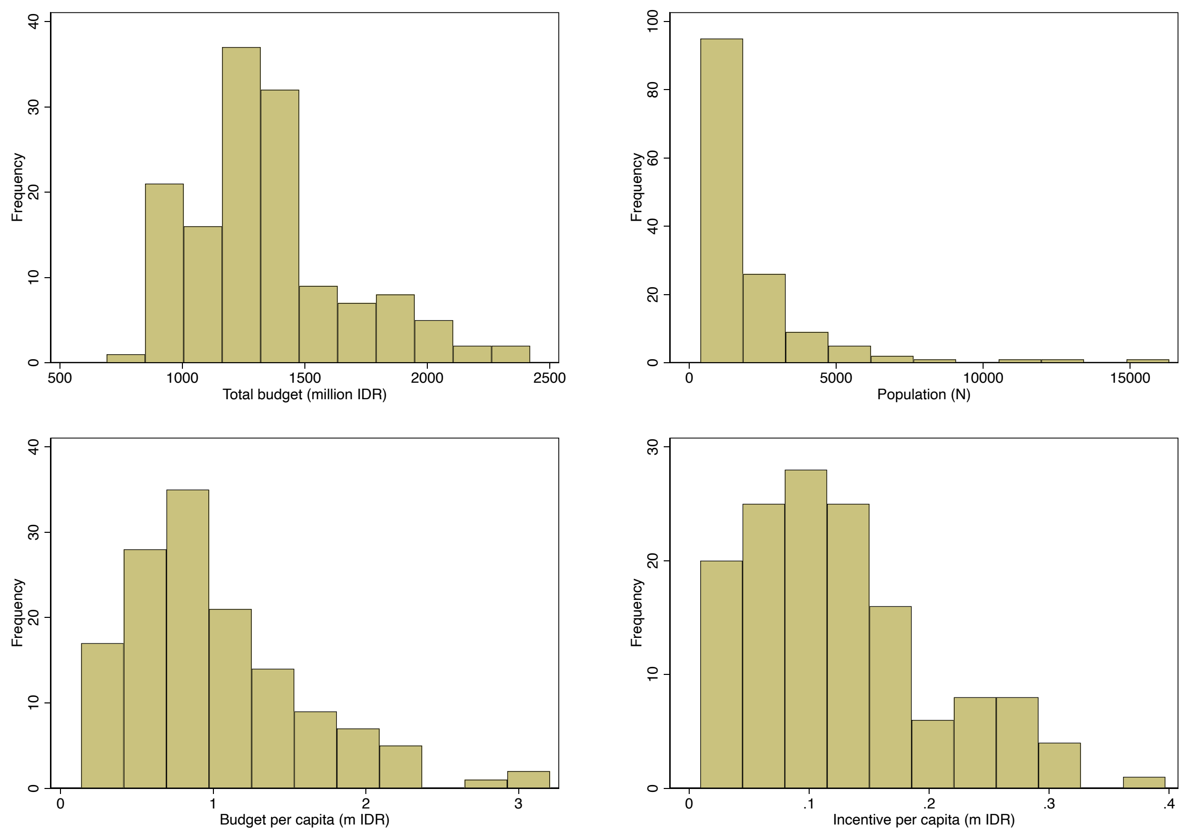
(b) Province-level—Annual Data



(c) District-level—Annual Data

Notes: Panel A plots the aggregate monthly time series for MODIS hotspot detections, with a 50 confidence filter, across all of Indonesia. Panel B plots the annual series for all provinces on the island of Kalimantan, and Riau province. Panel C plots the annual series for our four study districts in West Kalimantan province.

Figure 3: Village Budgets and Populations



Notes: These figures plot histograms of the total village budget in millions of Indonesia rupiah, village population, total village budgets in per capita terms, and our fiscal incentive in per capita terms, to give a sense of the relative size of the incentive. Village expenditure and populations are measured in our endline survey, and the sample here is the entire treatment group (75) and 75 villages selected at random from our main control group.

Figure 4: Treatment Assignment

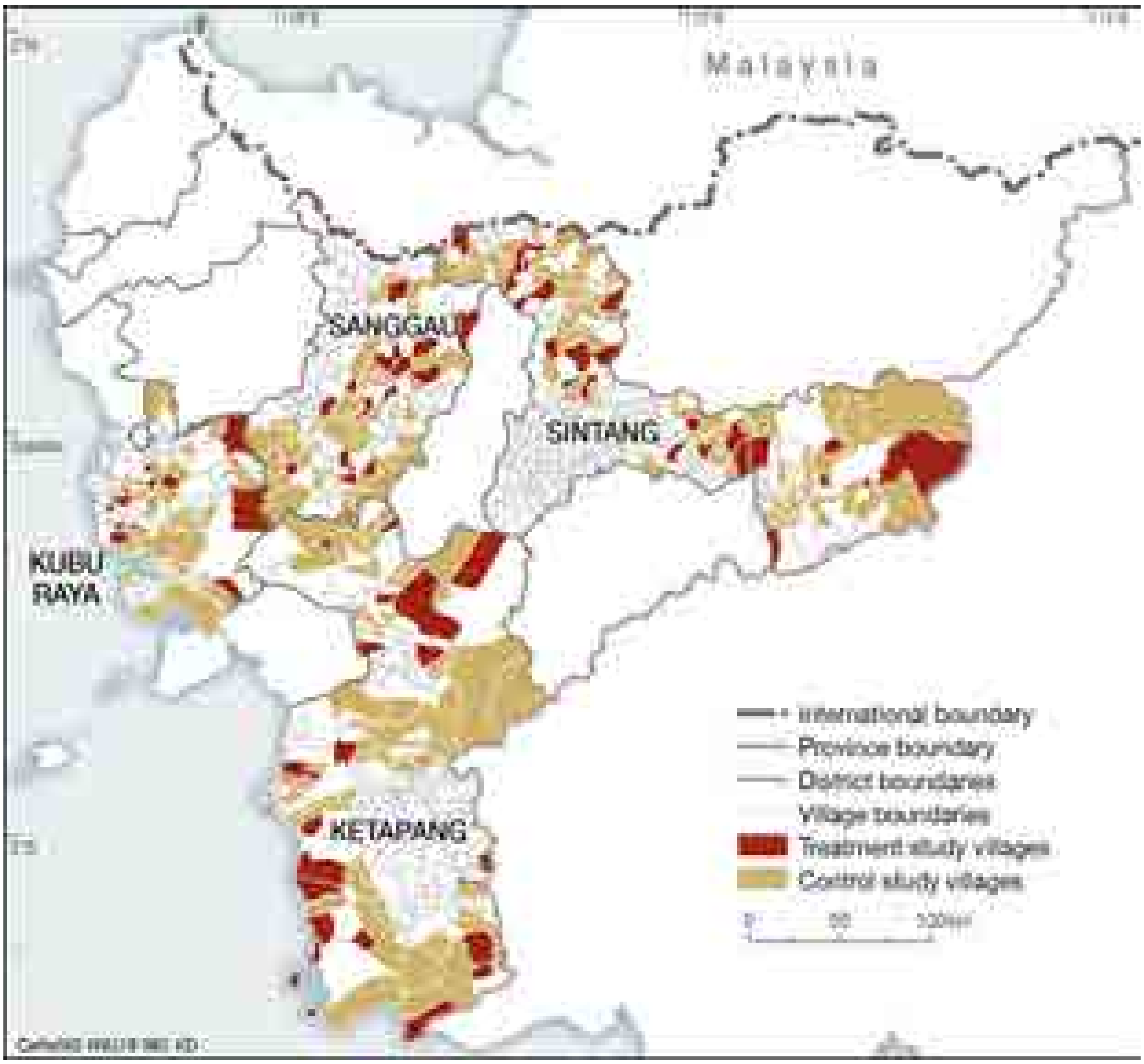
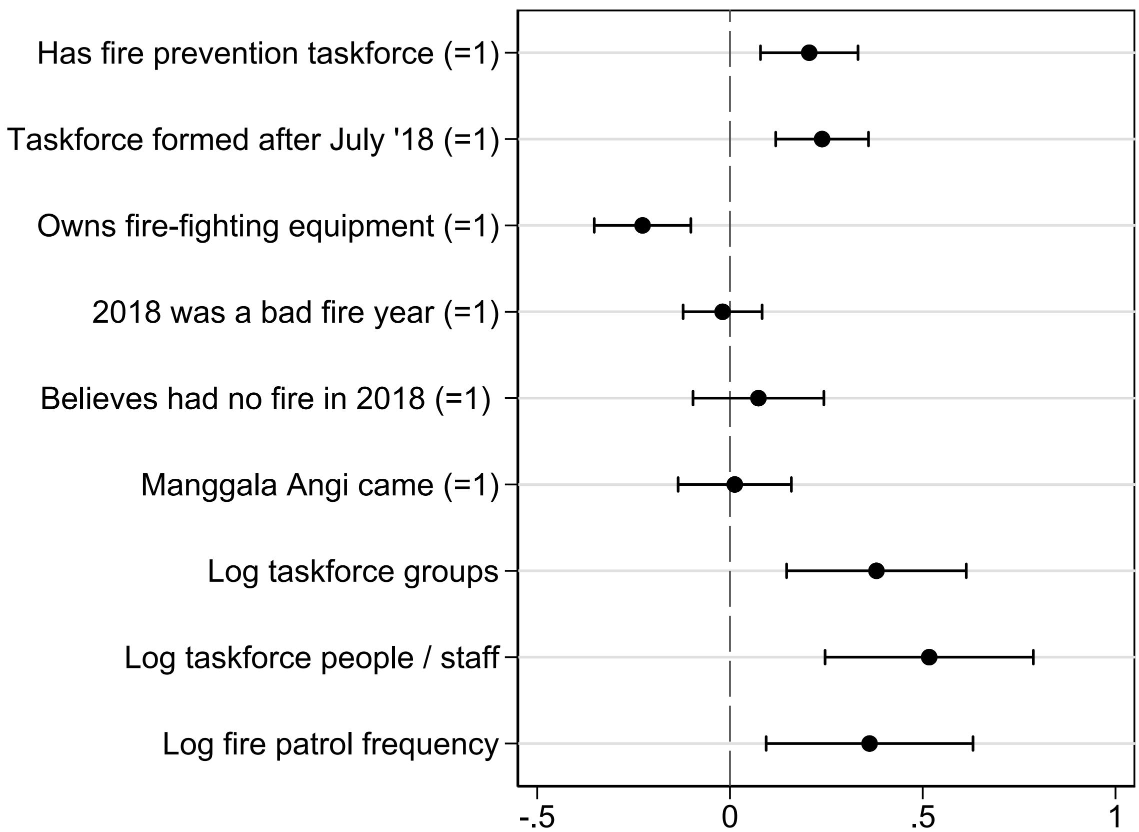
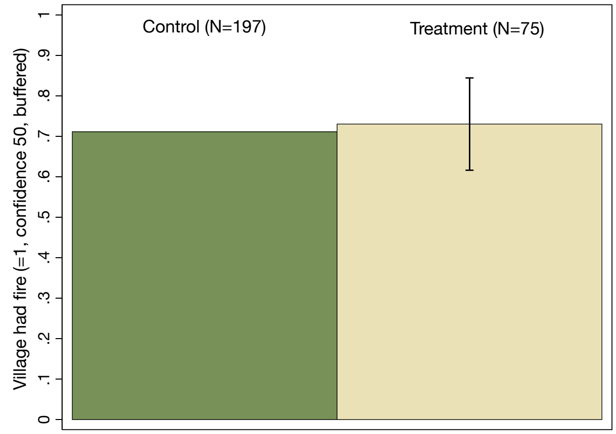


Figure 5: Effects on Village Fire-related Behaviors

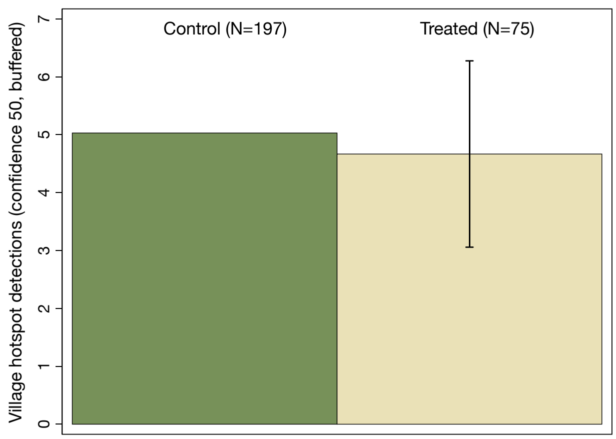


Notes: This figure plot the estimated treatment effects on intermediate outcomes capturing fire-related behavior which we measured in an endline survey. District strata fixed effects and balancing variables are included throughout and robust confidence intervals are at the 95 percent level. The sample is 75 treatment and 75 control villages.

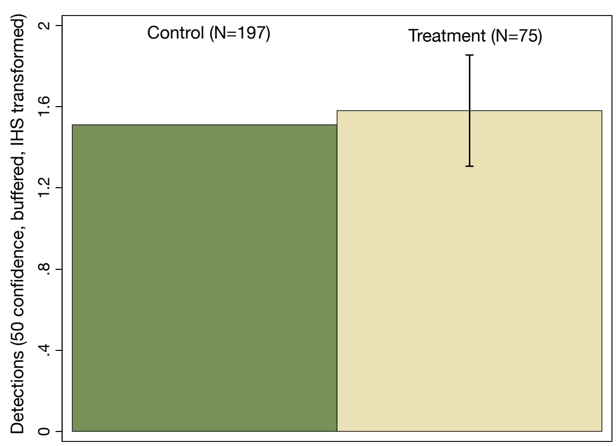
Figure 6: Effects on Village Fire Outcomes



(a) Probability of Any Fire (=1)



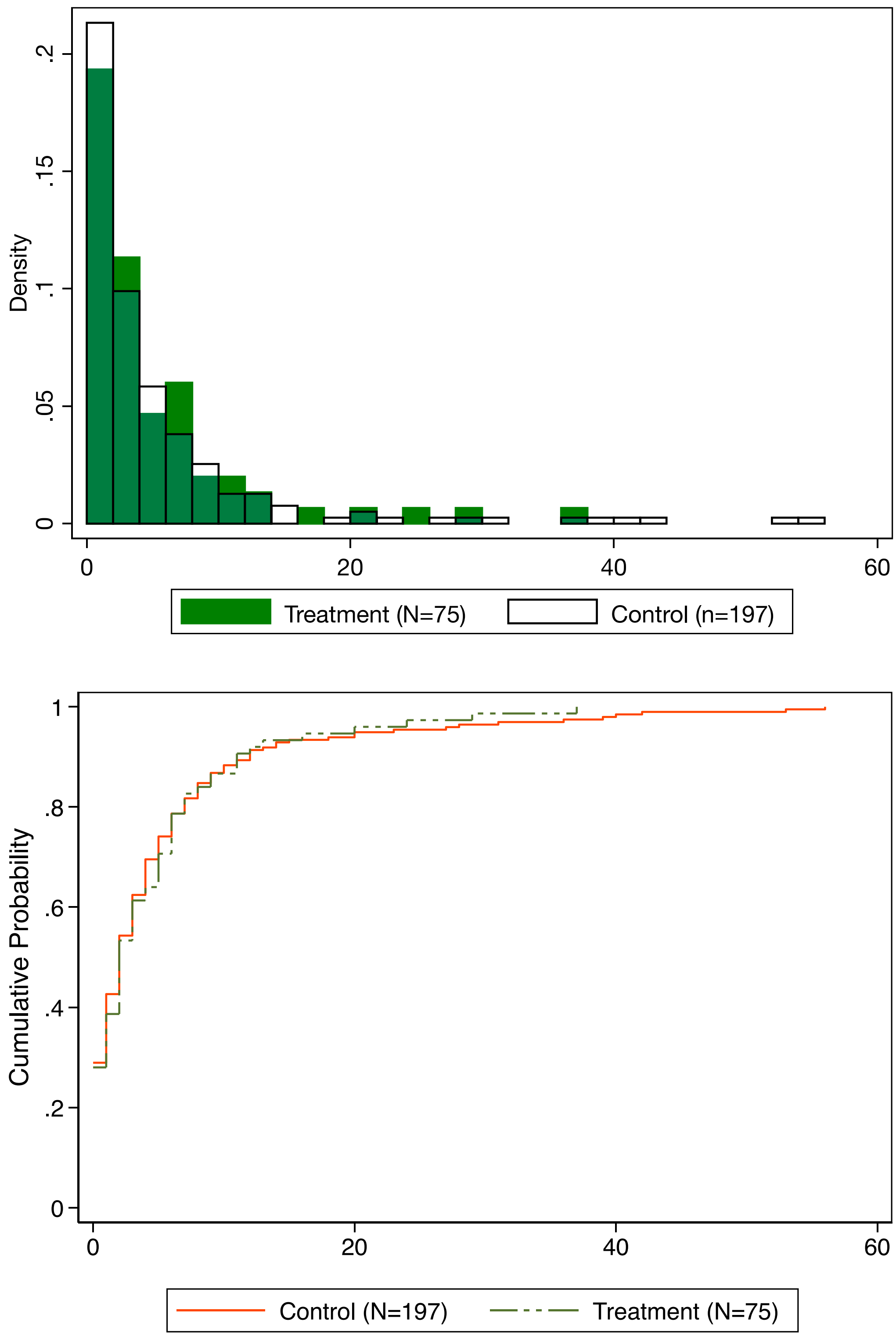
(b) Number of Detections (N)



(c) IHS Transformed Hotspots (N)

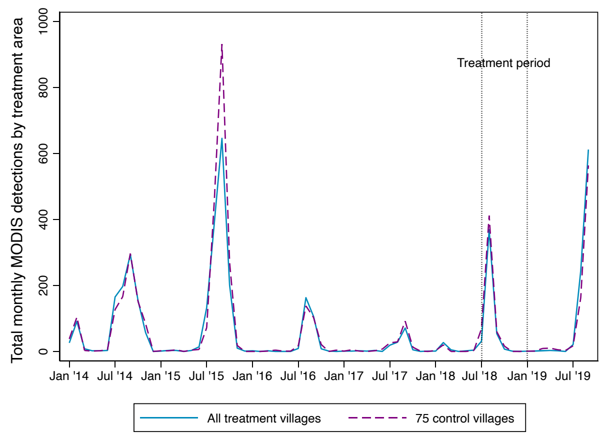
Notes: These figures plot the estimated treatment effects using (a) a binary indicator of fire (where success in having no fire is zero), (b) the count of the number of detections, and (c) the inverse hyperbolic transformation of the count as outcomes. District strata fixed effects and balancing variables are included throughout. Sample is 272 villages, 75 of which comprise the treatment group. Robust confidence intervals are at the 95 percent level.

Figure 7: Hotspot Distributions By Treatment Status

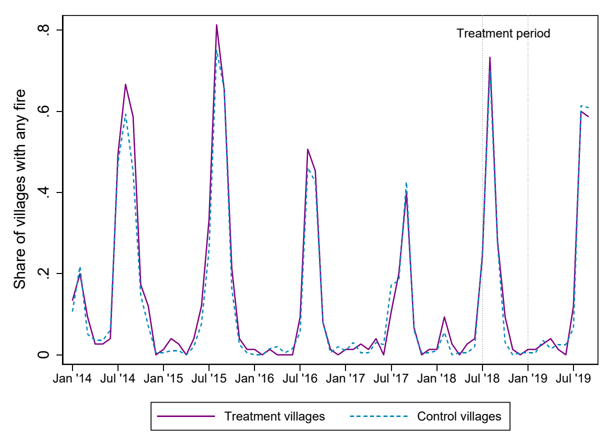


Notes: These figures plot the distribution of hotspots in the treatment and control groups. The top panel shows simple histograms and the bottom cumulative distribution functions. Hotspots are filtered to be above 50 confidence and at least 500 meters from village borders.

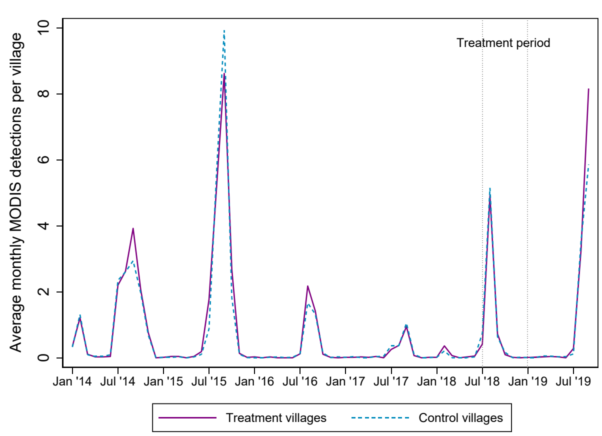
Figure 8: Monthly Hotspot Detections By Treatment Status



(a) Total Detections By Treatment Area



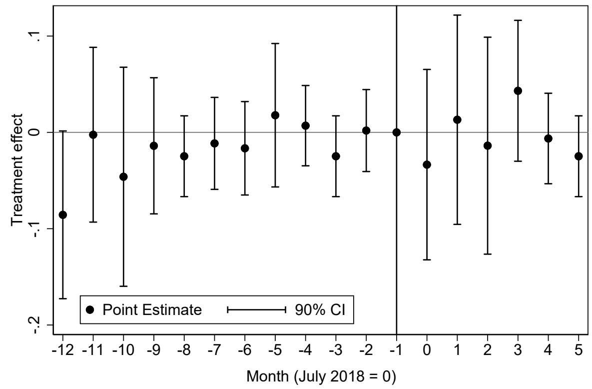
(b) Average Probability of Any Fire (=1)



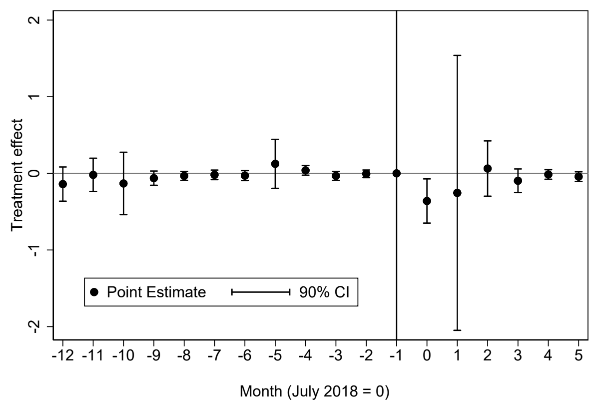
(c) Average Number of Detections (N)

Notes: These figures plot the total monthly MODIS hotspot detections in treatment and control groups, applying a 50 confidence filter. We use equal-sized groups (i.e., the smaller 75-village control group) to ease visual presentation, as the oversampled control group mechanically will have more than twice as many detections with no treatment effects and thus make proportional differences more difficult to visually discern.

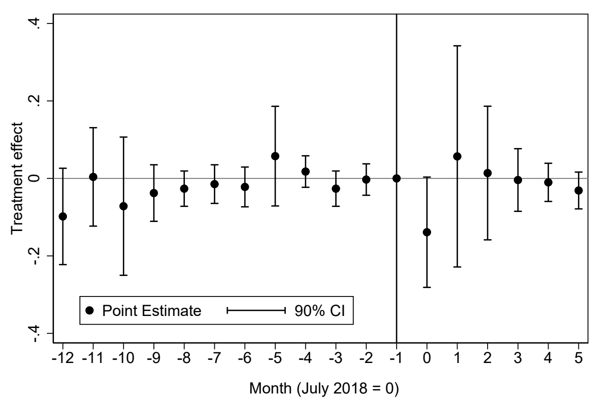
Figure 9: Dynamic Treatment Effects—Event Studies



(a) Village had any hotspot detections (=1)



(b) Number of hotspot detections (N)



(c) IHS hotspot detections (N)

Notes: These figures plot the month-specific coefficients from continuous-time difference-in-difference panel estimates using monthly village hotspot data from 2013–2018. All three models include district-by-period and village fixed effects, and additional leads are omitted from the figures for brevity.

Fight Fire with Finance
Online Appendix—Not For Publication

Supplementary Tables

Supplementary Figures

Table A1: Village Budget Descriptive Statistics

	Median	Mean	SD	Min.	Max.
Total budget (million IDR)	1305	1352	327	690	2419
Population (N)	1341	2072	2256	378	16326
Budget/capita (m IDR)	0.91	1.02	0.58	0.13	3.2
Incentive/capita (m IDR)	0.11	0.13	0.08	0.01	0.4
Households (N)	397	578	600	103	3729
Budget/household (m IDR)	3.18	3.71	2.22	0.52	12.05
Incentive/household (m IDR)	0.38	0.45	0.3	0.04	1.46
Village area (ha)	9893	18432	38683	11	367504
Incentive/ha (m IDR)	0.02	0.38	1.83	0	13.73
Village forest hectares (ha)	1000	4380	8156	7	46000
Incentive/ha of forest (m IDR)	0.15	1.48	3.77	0	21.43

Notes: This table provides summary statistics on village budgets, population, and land with data collected from our endline survey of 75 treatment and 75 randomly-selected control villages. Note that some of these figures, collected during the study, do not align closely with those in the 2014 Village Census. The fiscal incentive provided was a uniform 150 million IDR, and this table puts this incentive in context in terms of the total village budgets, in per capita terms, and in terms of village forest and total area.

Table A2: Main Results Using Equal-Sized Groups

Column	1	2	4	5	6
Panel A outcome	Village had any fire (=1)				
β (treatment=1)	0.008	0.003	0.004	-0.028	-0.024
Robust S.E.	(0.075)	(0.075)	(0.072)	(0.074)	(0.085)
R.I. p-value	[1.000]	[1.000]	[0.958]	[0.698]	[0.754]
R^2	0.000	0.026	0.134	0.205	0.363
Adjusted R^2	-0.007	-0.001	0.091	0.134	0.168
Panel B outcome	IHS fire count (N)				
β (treatment=1)	0.025	0.046	0.002	-0.046	0.053
Robust S.E.	(0.212)	(0.210)	(0.185)	(0.181)	(0.195)
R.I. p-value	[0.911]	[0.830]	[0.991]	[0.807]	[0.776]
R^2	0.000	0.052	0.308	0.393	0.529
Adjusted R^2	-0.007	0.026	0.273	0.339	0.385
District FEs	N	Y	Y	Y	Y
Pre-period fire history	N	N	Y	Y	Y
Additional balancing variables	N	N	N	Y	Y
Additional covariates	N	N	N	N	Y
N villages	148	148	148	148	146

Notes: This table reports the main regression results of the experiment, except using equal-sized treatment and control groups. Treatment is a binary indicator set equal to one if a village was randomly assigned to the program. All estimates use least squares and IHS refers to the inverse hyperbolic sine transformation of the count variable. MODIS hotspot data are filtered on 50 confidence and a 500m buffer from village boundaries. Additional balancing variables include hotspot detections in 2013, 2014, and 2015, the number of oil palms planted in the village, the education level of the village leader, whether the village road is soil, the number of households in the village, and village land area. Additional covariates are distance to the nearest palm oil mill, peatland, palm oil planted area, poverty rate, poverty gap, gini index, plantation village dummy, households without electricity, village burns for agriculture, village burns trash, village had a fire disaster in the last 3 years, number of people malnourished, village accessible by land, number of marketplaces, village had an agricultural kiosk, government spending on staff, capital, and other, number of government staff, and village head age.

Table A3: Raw Differences in the Probability of Any Fire, by Measurement Approach

Outcome variable	Control (N=197) Mean/S.E.	Treated (N=75) Mean/S.E.	T-test (p-value)	Normalized difference
MODIS				
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]		
Confidence level 50 buffered	0.711	0.720	0.880	-0.021
	[0.032]	[0.052]		
Confidence level 80 buffered	0.472	0.467	0.937	0.011
	[0.036]	[0.058]		
VIIRS				
All	0.949	0.987	0.163	-0.190
	[0.016]	[0.013]		
Nominal and high	0.949	0.987	0.163	-0.190
	[0.016]	[0.013]		
High	0.497	0.547	0.470	-0.098
	[0.036]	[0.058]		
All buffered	0.944	0.987	0.128	-0.207
	[0.016]	[0.013]		
Nominal and high buffered	0.944	0.987	0.128	-0.207
	[0.016]	[0.013]		
High buffered	0.487	0.493	0.930	-0.012
	[0.036]	[0.058]		

Notes: This table compares the probability of fire in the treatment and controls groups when the primary outcome is calculated differently: with difference confidence filters applied to the hotspot detections, and with and without a 500 meter buffer from the village border. The top panel is the main MODIS data and the bottom panel is the alternative VIIRS data, which is more sensitive and higher-resolution.

Table A4: Raw Differences in Hotspot Detections, by Measurement Approach

Outcome variable	Control (N=197) Mean/SE	Treated (N=75) Mean/SE	T-test p-value	Normalized difference
MODIS				
All	7.980	7.507	0.778	0.038
	[0.919]	[1.235]		
Confidence level 50	6.736	6.200	0.697	0.053
	[0.763]	[0.978]		
Confidence level 80	2.756	2.427	0.671	0.058
	[0.442]	[0.484]		
All buffered	6.061	5.760	0.828	0.030
	[0.763]	[0.998]		
Confidence level 50 buffered	5.030	4.640	0.729	0.047
	[0.628]	[0.778]		
Confidence level 80 buffered	1.995	1.667	0.599	0.071
	[0.357]	[0.378]		
VIIRS				
All	33.751	33.067	0.920	0.014
	[3.618]	[5.478]		
Nominal and high	32.548	31.907	0.922	0.013
	[3.509]	[5.292]		
High	1.934	1.467	0.320	0.135
	[0.270]	[0.272]		
All buffered	30.624	29.760	0.891	0.019
	[3.378]	[5.061]		
Nominal and high buffered	29.528	28.733	0.897	0.018
	[3.282]	[4.877]		
High buffered	1.787	1.307	0.287	0.145
	[0.258]	[0.265]		

Notes: This table compares number of hotspot detections observed the treatment and controls group when the primary outcomes is calculated differently: with difference confidence filters applied to the hotspot detections, and with and without a 500 meter buffer from the village border. The top panel is the main MODIS data and the bottom panel is the alternative VIIRS data, which is more sensitive and higher-resolution.

Table A5: Annual Panel Estimates, Binary Outcome

Outcome	Village had any fire (=1), annual panel						
Estimator	Pooled	Pooled	RE	RE	DD	DD	Matched-DD
Column	1	2	3	4	5	6	7
Post x treatment	0.013	0.013	0.013	0.013	0.013	0.013	0.021
	(0.055)	(0.055)	(0.055)	(0.055)	(0.055)	(0.055)	(0.056)
Post & treatment FEs	Y	Y	Y	Y	Y	Y	Y
Year FEs	N	Y	N	Y	N	Y	Y
Village FEs	N	N	N	N	Y	Y	Y
Matched	N	N	N	N	N	N	Y
Observations	1632	1632	1632	1632	1632	1632	1548

Notes: This table reports annual panel regression results where the outcome is a dichotomous indicator for whether a village had any fire. Treatment is a binary indicator set equal to one if a village was randomly assigned to the program, and post is an indicator equal to one if the period is the study year, 2018. Column 7 trims the sample based on common support from propensity scores. Robust standard errors clustered at the village level are in parentheses, and stars (*,**,***) denote statistical significance at the 10, 5, and 1 percent levels.

Table A6: Annual Panel Estimates, Transformed Count

Outcome	IHS fire count (N), annual panel						
Estimator	Pooled	Pooled	RE	RE	DD	DD	Matched-DD
Column	1	2	3	4	5	6	7
Post x treatment	-0.641	-0.641	-0.641	-0.641	-0.641	-0.641	-0.375
	(1.687)	(1.689)	(1.687)	(1.689)	(1.687)	(1.689)	(1.714)
Post & treatment FEs	Y	Y	Y	Y	Y	Y	Y
Year FEs	N	Y	N	Y	N	Y	Y
Village FEs	N	N	N	N	Y	Y	Y
Matched	N	N	N	N	N	N	Y
Observations	1632	1632	1632	1632	1632	1632	1548

Notes: This table reports annual panel regression results where the outcome is an IHS transformation of the village hotspot count. Treatment is a binary indicator set equal to one if a village was randomly assigned to the program, and post is an indicator equal to one if the period is the study year, 2018. Column 7 trims the sample based on common support from propensity scores. Robust standard errors clustered at the village level are in parentheses, and stars (*,**,***) denote statistical significance at the 10, 5, and 1 percent levels.

Table A7: Annual Panel Estimates, Count, Poisson

Outcome	Fire count (N)			
Estimator	Poisson	Poisson	PPML	PPML
Column	1	2	3	4
Post x treatment	-0.097	-0.097	-0.097	-0.064
	(0.232)	(0.232)	(0.232)	(0.234)
Treat & post FEs	Y	Y	Y	Y
Year FEs	N	Y	Y	Y
Village FEs	N	N	Y	Y
Matched	N	N	N	Y
Observations	1632	1632	1632	1548

Notes: This table reports annual panel regression results where the outcome is the village hotspot count, and estimation is by panel poisson and pseudo-poisson maximum likelihood models. Treatment is a binary indicator set equal to one if a village was randomly assigned to the program, and post is an indicator equal to one if the period is the study year, 2018. Column 4 trims the sample based on common support from propensity scores. Robust standard errors clustered at the village level are in parentheses, and stars (*,**,***) denote statistical significance at the 10, 5, and 1 percent levels.

TABLE A8: TREATMENT EFFECT HETEROGENEITY, PROBABILITY OF ANY FIRE

Outcome	Village had any fire (=1)									
Interaction	Past fire	Palm oil	KD HS	Dirt road	Population	Area	Poverty	Plantation	No forest	Forest edge
Column	1	2	3	4	5	6	7	8	9	10
β (treatment=1)	-0.051	0.038	0.694***	0.037	-0.000	0.082	0.028	-0.091	0.017	0.029
	(0.089)	(0.079)	(0.181)	(0.092)	(0.086)	(0.062)	(0.086)	(0.104)	(0.080)	(0.081)
γ (balancing variable)	0.240***	-0.181**	0.375**	-0.087	-0.010	-0.235***	0.088	-0.043	-0.036	0.003
	(0.064)	(0.083)	(0.185)	(0.067)	(0.070)	(0.063)	(0.065)	(0.069)	(0.066)	(0.066)
θ (interaction term)	0.119	-0.094	-0.694***	-0.045	0.032	-0.151	-0.014	0.171	-0.013	-0.027
	(0.113)	(0.121)	(0.191)	(0.122)	(0.122)	(0.113)	(0.122)	(0.128)	(0.125)	(0.124)
Adjusted R^2	0.099	0.050	0.029	0.023	0.012	0.110	0.020	0.019	0.013	0.012
R^2	0.119	0.072	0.050	0.045	0.034	0.130	0.042	0.040	0.035	0.034
N villages	272	272	272	272	272	272	272	272	272	272

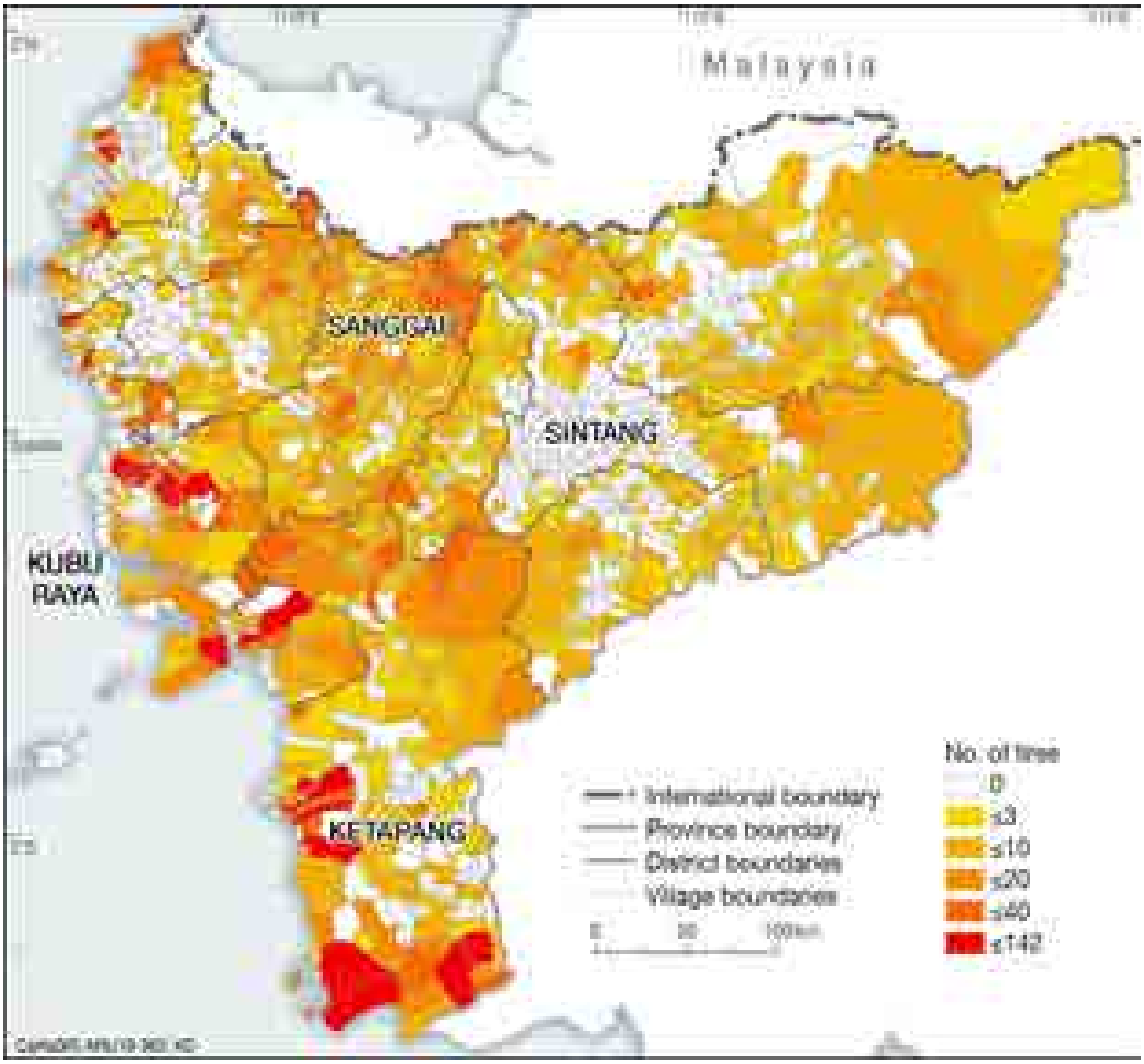
Notes: This table reports regression results exploring heterogeneous treatment effects. Although we balanced on these outcome variables, we did not set our sample size for sub-group analysis and these estimates are best viewed as exploratory. Treatment is a binary indicator set equal to one if a village was randomly assigned to the program. Balancing variables are all dichotomous, either originally in PODES (e.g., village has a dirt road, village head has high school education, plantation villages, forest edge, or no forest) or by creating a dummy variable for those with a value higher than the sample median (e.g., past fire, palm oil trees, population, area, poverty). The interaction term interact the treatment with the balancing variable in the top row. Least squares is used throughout and all estimates include district fixed effects, as in Figures 5 and 6. Stars (*,**,***) denote statistical significance at the 10, 5, and 1 percent levels.

TABLE A9: TREATMENT EFFECT HETEROGENEITY, IHS FIRE COUNT

Outcome	IHS fire count (N)									
Interaction	Past fire	Palm oil	KD HS	Dirt road	Population	Area	Poverty	Plantation	No forest	Forest edge
Column	1	2	3	4	5	6	7	8	9	10
β (treatment=1)	-0.044	0.040	1.459***	0.232	0.093	0.053	0.088	-0.178	0.079	0.112
	(0.189)	(0.216)	(0.482)	(0.264)	(0.251)	(0.215)	(0.234)	(0.308)	(0.227)	(0.229)
γ (balancing variable)	1.142***	-0.289	0.759	-0.117	-0.273	-0.882***	0.247	-0.283	-0.109	0.089
	(0.164)	(0.222)	(0.493)	(0.190)	(0.187)	(0.167)	(0.182)	(0.199)	(0.184)	(0.183)
θ (interaction term)	0.084	-0.075	-1.456***	-0.340	-0.070	-0.089	-0.083	0.332	-0.137	-0.173
	(0.297)	(0.339)	(0.510)	(0.338)	(0.334)	(0.310)	(0.334)	(0.365)	(0.335)	(0.338)
Adjusted R^2	0.201	0.011	0.010	0.011	0.012	0.130	0.008	0.008	0.004	0.002
R^2	0.219	0.033	0.031	0.032	0.034	0.149	0.030	0.030	0.026	0.024
N villages	272	272	272	272	272	272	272	272	272	272

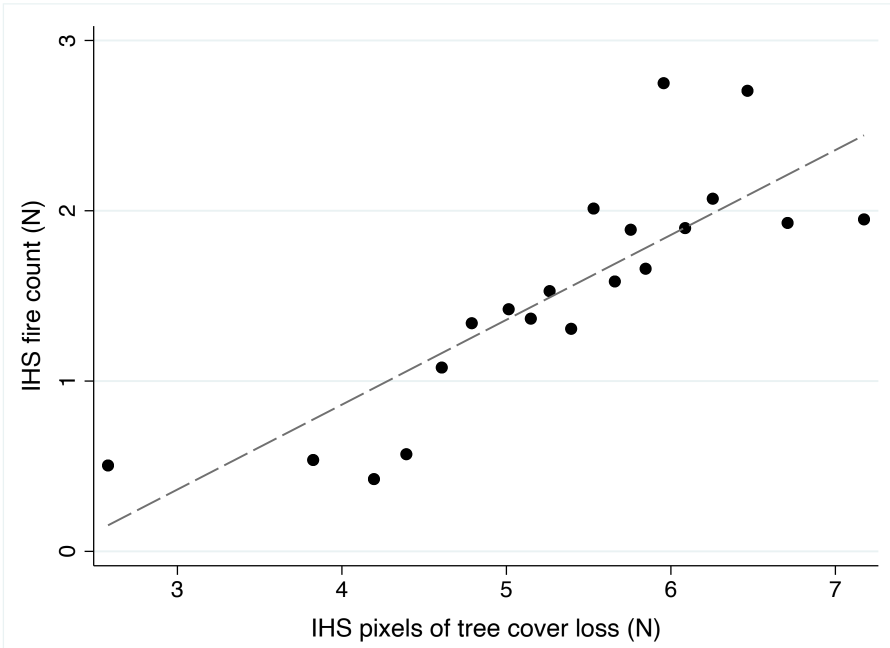
Notes: his table reports regression results exploring heterogeneous treatment effects. Although we balanced on these outcome variables, we did not set our sample size for sub-group analysis and these estimates are best viewed as exploratory. Treatment is a binary indicator set equal to one if a village was randomly assigned to the program. Balancing variables are all dichotomous, either originally in PODES (e.g., village has a dirt road, village head has high school education, plantation villages, forest edge, or no forest) or by creating a dummy variable for those with a value higher than the sample median (e.g., past fire, palm oil trees, population, area, poverty). The interaction term interact the treatment with the balancing variable in the top row. Least squares is used throughout and all estimates include district fixed effects, as in Figures 5 and 6. Stars (*,**,***) denote statistical significance at the 10, 5, and 1 percent levels.

Figure A1: Village Hotspot Detections in 2018

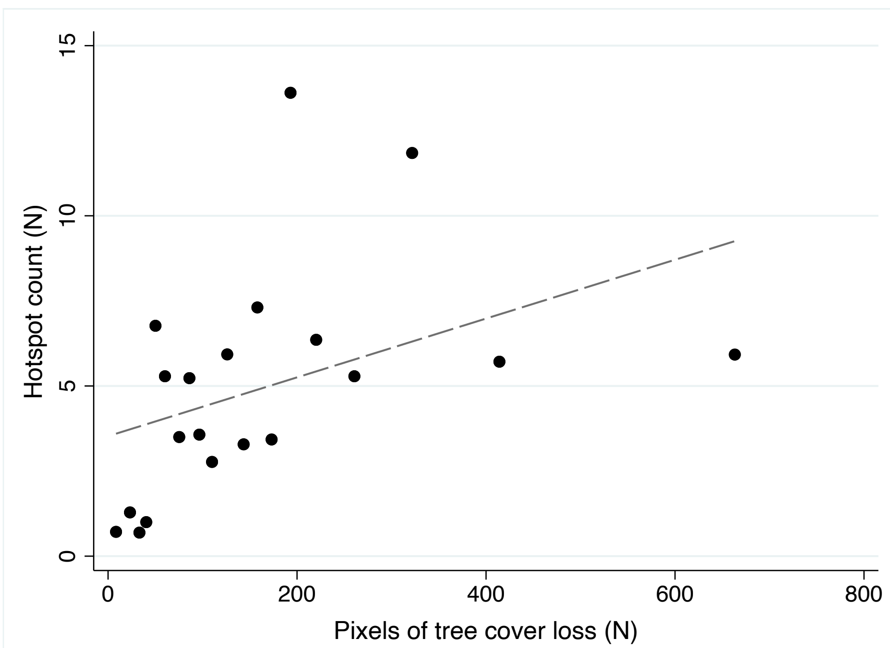


Notes: This figure maps the number of hotspots detected by MODIS in each Village in West Kalimantan after applying a 50 confidence filter. Note that this not precisely the MODIS hotspot data used in estimation and to determine success, as some further processing was required (e.g., removing swidden permitted fires, and buffering around the village boundaries).

Figure A2: MODIS Hotspots and Tree Cover Loss



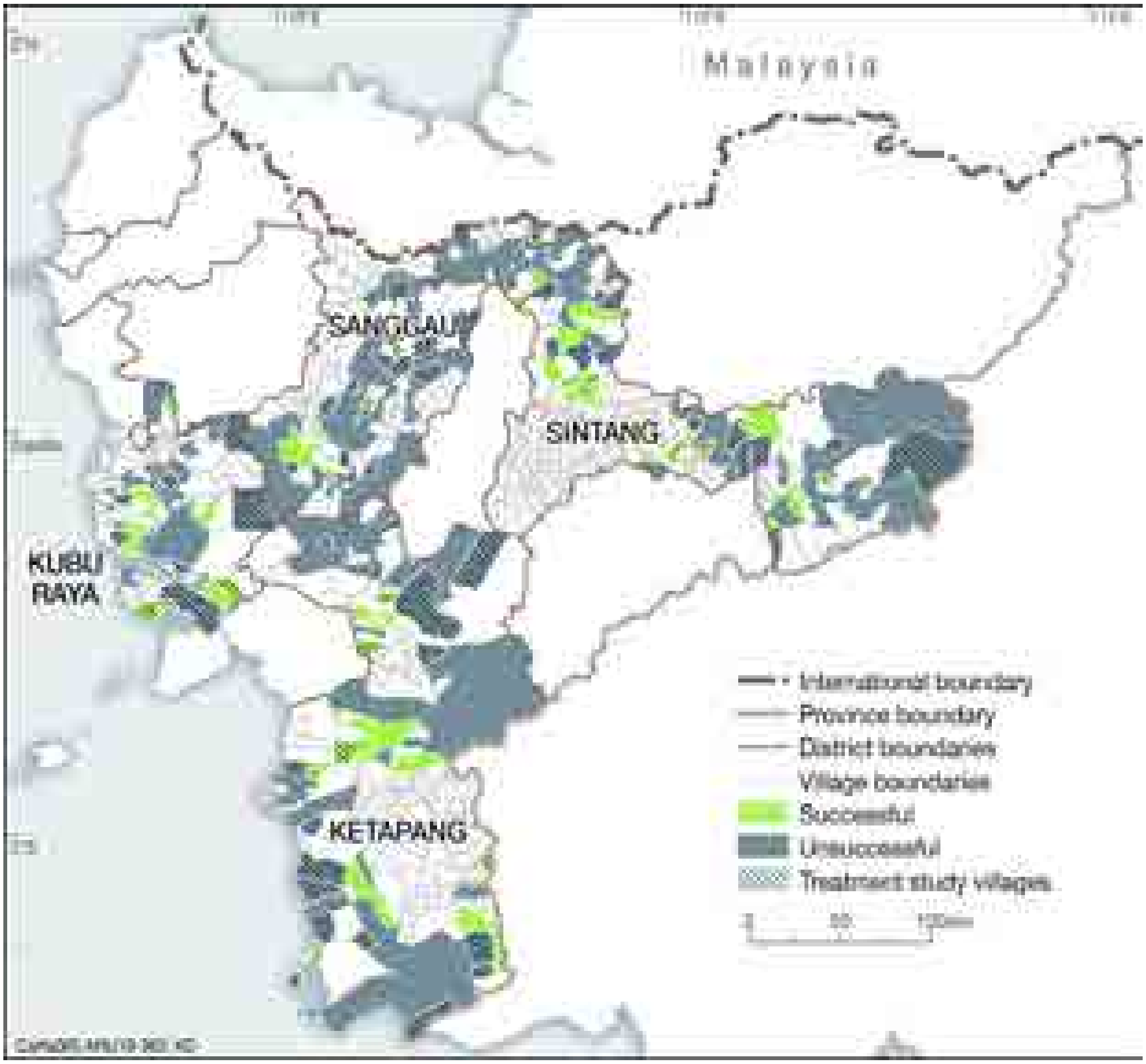
(a) IHS-IHS



(b) Linear-linear

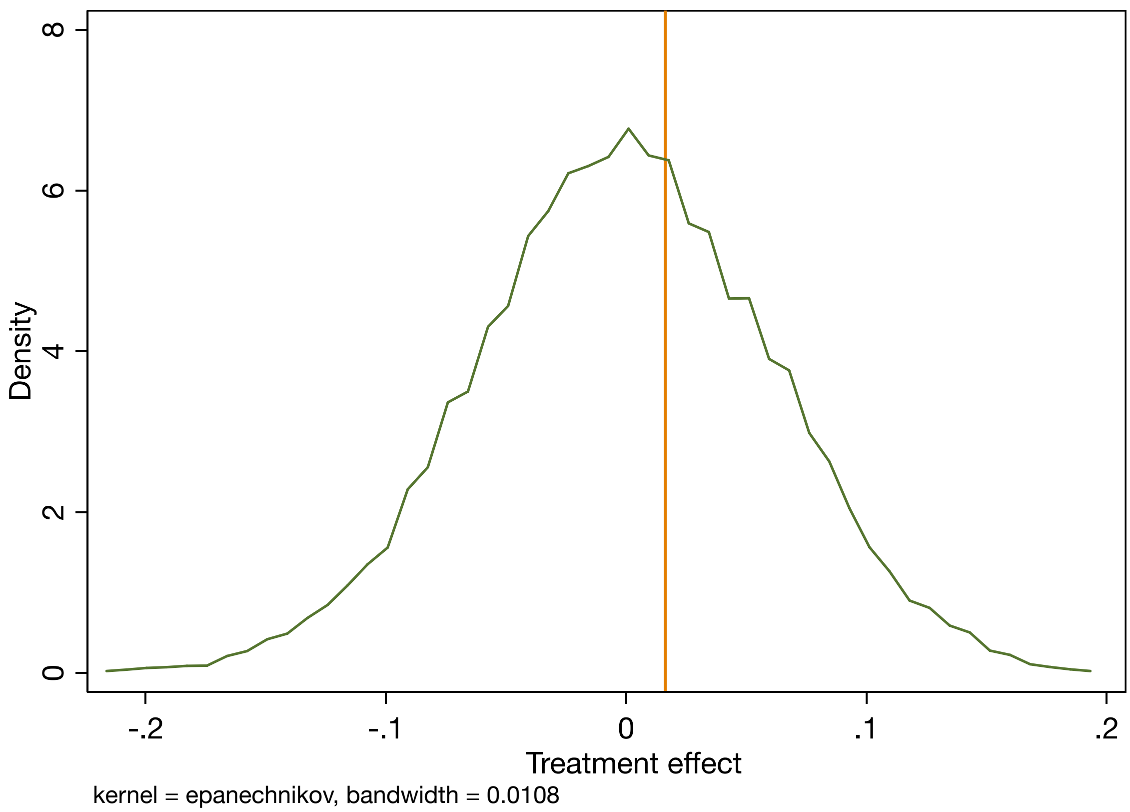
Notes: These figures present binned scatterplots plotting village hotspots against tree cover loss in 2018.

Figure A3: Fire-free Program Villages



Notes: This figure visually shows the final outcome of the pilot program, mapping the locations of the randomly-assigned treatment and control villages, and shading those which were successful in going fire-free and those which were not.

Figure A4: Randomization Inference Distribution



Notes: This figure plots the distribution of treatment effects obtained from 1000 random permutations of the treatment assignment. The outcome is the binary indicator for whether a village had any fire, and the regression model estimated throughout includes district fixed effects and balancing variables in addition to the main treatment indicator. The orange line indicates the main treatment effect point estimate.