

# FIGHT FIRE WITH FINANCE: A RANDOMIZED FIELD EXPERIMENT TO CURTAIL LAND-CLEARING FIRE IN INDONESIA

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**JULY 2020**



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**TNP2K Working Paper 55-e/2020  
July 2020**

The TNP2K Working Paper Series disseminates the findings of work in progress to encourage discussion and exchange of ideas on poverty, social protection and development issues.

Support to this publication is provided by the Australian Government through the MAHKOTA Program.

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Suggested citation: Edwards, R.B., Falcon, W.P., Hadiwidjaja, G., Higgins, M.M., Naylor, R.L., Sumarto, S. Fight Fire with Finance: A Randomized Field Experiment to Curtail Land-Clearing Fire in Indonesia. TNP2K Working Paper 55-e/2020. Jakarta, Indonesia.

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**THE NATIONAL TEAM FOR THE ACCELERATION OF POVERTY REDUCTION**

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

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July 25, 2020

## Abstract

This paper presents one of the first randomized evaluations 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. The program was implemented over the 2018 fire season in Indonesia with three parts: (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. Our results appear to be driven by a combination of the payment not being large enough and a failure of collective action, and offer a cautionary tale on the importance of measuring additionality when evaluating payments for environmental services and other conservation programs.

**Keywords:** collective action, conditional cash transfer, deforestation, forest fire, payments for ecosystem services, pay-for-performance, Indonesia

**JEL codes:** H30, H70, 010, 013, Q15, Q23, Q24, Q28, Q51, Q54, Q57, R52

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\*Edwards: Australian National University; Falcon, Higgins, Naylor: Stanford University; Hadiwidjaja: Independent Consultant; Sumarto: National Team for the Acceleration of Poverty Reduction (TNP2K) and SMERU Research Institute. We thank the Indonesian National Team for the Acceleration of Poverty Reduction (particularly Bambang Widiyanto 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, Chris Snyder, Frances Seymour, Daniel Suryadarma, Peter Warr, and seminar participants at the NBER Summer Institute, the ANU, and Dartmouth College for invaluable comments and discussions. All views and any errors, however, are our own. The trial was registered in the AEA Trial Registry at [AEARCTR-0003222](#).

# 1 Introduction

Land-related greenhouse gas emissions account for approximately 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 to date have typically been ineffective and unsustainable (Dennis, 1999; Dennis et al., 2005), and the tropical developing countries where fire and deforestation are often the most severe face major political and governance challenges in preventing and responding to fire (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 the presence of these constraints remains 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). 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). A central policy question is whether fiscal incentive schemes can still be effective amidst imperfect property rights, land-use flux, and chronic underdevelopment, features which characterize many of the world’s most critical 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) and even less on how to stop them.<sup>1</sup> 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, 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 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.

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<sup>1</sup>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.”

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 associated with over 100,000 premature deaths and \$16 billion in economic costs (Kopplitz 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 had three components: (a) village information and instruction on fire prevention, (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, and (c) an ex-post conditional payment 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 the 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, administrative, service delivery, and developmental responsibilities down to Indonesia’s over 80,000 villages (Antlov et al 2016). In addition to informing crucial aspects of 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 where land-use change has been less dynamic (see, e.g., Alix-Garcia et al., 2015; Grillos et al., 2019). 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), and 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 sought to instead achieve change through collective action and exploit local leaders as agents of change.

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 was not enough) and a collective action failure.<sup>2</sup> Follow-up qualitative work suggests that, consistent with our program theory, 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 approximately 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. Although we cannot rule out very small effects—which may be policy-relevant and

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<sup>2</sup>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.

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. 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. We caution that economic incentives may at times be ineffective in spurring enough behavioral change to reduce harmful externalities and conserve the environment (especially when involving difficult collective action problems), that programs need to measure additionality carefully, and that the policy and research communities should remain open to other approaches for reducing anthropogenic forest fires in Indonesia and other countries.

## **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. The majority of forest fires are intentionally started by local landowners, other community members, and in some 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).<sup>3</sup> 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.<sup>4</sup>

## 2.2 Theory

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 are necessary for success.

The size of the payment must be large 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

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<sup>3</sup>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).

<sup>4</sup>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.

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., the treatment “dosage”). 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. Additional descriptive statistics on village budgets are at Appendix Table A1.

People set fires for many reasons—reaping different benefits, internalizing different costs. Since different land users have different and often unobservable levels of willingness to accept (WTA) avoiding the use of fire, policies need to cater for individual heterogeneity. However, the net present value of every available hectare (excluding negative externalities) makes such payments prohibitively expensive. Although it weakens the direct link between individual behaviour and the incentive, a community-level approach invoking collective action and leveraging social influence is our response to these challenges.<sup>5</sup> The community-level approach is also by necessity: imperfect land rights, land-use flux, and the sheer number of households in our study villages make an individual-level incentive system 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 for scale and realistically implemented.<sup>6</sup> Villages heads are accountable to their communities through regular direct elections. Fire has been argued to increase around elections (Purnomo et al., 2019).

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<sup>5</sup>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.

<sup>6</sup>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).

Our pilot was thus also 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, Indonesia’s community-driven development program involving village cash transfers, facilitations, and technical support in implementing projects of village choice.<sup>7</sup> 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.

Although different villages likely have different collective “willingness to accept” 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 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 potentially 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 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. 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.

## 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.<sup>8</sup>

Village facilitations were then conducted May–July 2018 (i.e., before the fire season) by **Sampan Kalimantan**. Facilitators had years of experience facilitating sustainability programs. Each attended a three-day training seminar in Pontianak in April 2018, which we led. Each facilitation was held in a central village location over two days, consisting of three main 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-only and public facilitations covered similar material and lasted around three 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) deposited into the village bank account. Facilitations also explained satellite hotspot detection with demonstrations and answered technical questions.

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<sup>8</sup>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.

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 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. 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. Extensive information on fire-free agricultural practices were also provided, and we explained what resources were available to villages for fire prevention and suppression.

Finally, we informed villages they would receive IDR 10,000,000 (roughly \$750) upfront to fund additional fire prevention efforts not included in the preexisting budget. 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.

After facilitation, research staff did not visit treatment villages until the end of the monitoring period.<sup>9</sup> 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

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<sup>9</sup>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.



hotspots and successfully earned their IDR 150,000,000 prize. All 75 villages were notified of the results in March and April 2019. 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 is 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).

To construct our outcome variables, we spatially joined hotspots detected from 1 July to 31 December 2018 to Indonesia’s official 2016 village boundaries, applied two filters (confidence and a 500m buffer, as we explain in the next sub-section) and counted the number of hotspots in each jurisdiction. Figure A1 maps total MODIS hotspot detections, with a 50 confidence filter, across all villages in West Kalimantan in 2018. 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.

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.<sup>10</sup>

### **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) fire to be deemed successful and receive payment. 563 hotspots were detected inside our 75 treatment villages during the monitoring period. 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. Field discussions and careful inspection of land cover maps suggest around 70 percent not excluded were unregistered swidden fires, 20 percent related to land clearing for palm, and 10 percent accidental. 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 as small controlled burns did not register as hotspots either.

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.<sup>11</sup> All treatment villages agreed with their final success-fail status.

### 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 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 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.<sup>12</sup> Second, we conducted an initial survey of treatment villages during facilitations.<sup>13</sup> 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

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<sup>11</sup>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.

<sup>12</sup>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?

<sup>13</sup>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.

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 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 2000–2018 (Hansen et al., 2013).

## 4 Experiment design

### 4.1 Sample, randomization, and balance

We focused on four districts in West Kalimantan: Kubu Raya, Ketapang, Sanggau, and Sintang.<sup>14</sup> 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.<sup>15</sup> Pre-processing ensures study villages start from similar baseline levels, reduces the variance in our outcomes, and partially alleviates the concern that some villages may be so far behind or ahead of others in conservation activities that the average differences in success (i.e., going fire-free) between treatment-control groups would be harder to discern.

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<sup>14</sup>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).

<sup>15</sup>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.

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 to which we could offer the program (i.e., monitoring additional villages from space was free apart from our time), we oversampled the control group to improve statistical power.<sup>16</sup> Following Lock and Rubin (2012), we re-randomized within districts 1000 times and selected the random assignment which maximized pre-treatment similarity between the treatment and control groups.<sup>17</sup> 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, and the whole study covered around 90,000 households (i.e., hundreds of thousands of people) and the program (i.e., treatment) around 25,000 households. The number of households is balanced across treatment and control groups, a critical consideration as in West Kalimantan our uniform village payments vary significantly in per capita terms (see Figure 3 and Table A1). Area is also balanced, important as, all else equal, fires mechanically increase in village area with the opportunity cost of no fire.

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<sup>16</sup>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.

<sup>17</sup>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)).

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.<sup>18</sup> 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 in the main results 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 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 leader. Sixty percent of villages report plantation crops as their primary source of income. Around twenty percent of villages were on peat soil. More than 90 percent of villages burned for agriculture, more than half cooked with firewood, and around 60 percent burned trash. More than half of the study villages were not accessible all-year around due to the rainy season.

Figure 4 maps the treatment assignment. Large areas are excluded from the study (e.g., in the centres of Sintang and Ketapang districts) as a result of pre-processing and treatment villages are randomly dispersed across the map.

## 4.2 Estimation and inference

The null hypothesis our experiment seeks to test is that the program has no effect on fire outcomes. A simple difference in means allows us to test this hypothesis, so we estimate the following equation:

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

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<sup>18</sup>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. All variables are available in the replication files.

where  $y_{v,d}$  is the outcome for village  $v$  in district  $d$ .  $D_{v,d}$  is a dichotomous treatment indicator equal to one if a village was randomly assigned to the program.  $\delta_d$  are district fixed effects.  $X_{v,d}$  includes predetermined village characteristics, balancing variables in the preferred specification.  $\epsilon_{v,d}$  is a mean zero error term adjusted for arbitrary heteroskedasticity. When  $y_{v,d}$  is total number of village hotspot detections,  $\alpha$  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,  $\alpha$  is the probability of fire for the average village in the control group for the omitted district.  $\beta$  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  $\beta$ , 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 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.<sup>19</sup> Our field visits and survey data suggest understanding and interest in the program was high, and up-front capital grants ensured villages could buy any new fire-fighting equipment they needed to respond to the incentive.

Figure 5 presents impacts on seven fire-related behavior outcomes. A key part of the facilitation process was emphasizing the formation of fire-fighting and prevention groups (i.e., task forces). Program villages were twenty percent more likely to have a fire prevention task force. Most were formed after facilitations. We also see large increases in the number of task force groups within villages, the number of villagers participating, and the frequency at which patrols are undertaken, and no impact on the probability that the local fire department outside the village (Manggala Agni) was called in. Taking the results together, participants appear to have understood the program well. Some program villages appeared to be extremely active in fire prevention activities, with others perhaps more ambivalent. Consistent with the main findings to follow, we find no statistically significant effect on the probability that the village believed it had no fire during the monitoring period.

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<sup>19</sup>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.



## 5.2 Satellite-based fire outcomes

The main finding from our experiment is that, despite the reasonably strong fire-related actions undertaken 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). 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.<sup>20</sup>

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 3 presents the main results in tabulated form and 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. Column 5 adds a host of additional correlates of fire from our baseline village census.<sup>21</sup> Point estimates lie between a 1 and

<sup>20</sup>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.

<sup>21</sup>Specifically, 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

3 percentage point *increase* in probability of fire. We report robust standard errors in parentheses and randomization inference-based p-values in square brackets below each point estimate. None of the point estimates are statistically significant at any conventional level.

Rising adjusted R-squared statistics suggest additional covariates help explain hotspots. Yet, the standard errors are remarkably stable. The implied minimum detectable effects (MDE) are around 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.<sup>22</sup> 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.

We now turn to the raw outcome data. The top panel of Figure 7 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.<sup>23</sup> Cumulative distribution functions plotted in the second panel reveal a similar pattern.

Figure 8 plots monthly total hotspot detections across all villages in the treatment group and the equal-sized control group since January 2014, revealing two important facts. First, the behavior and levels of the two series map closely to one another before, during, and after the program. Second, the 2018 fire season was almost as bad in our study villages as in 2015, a stark contrast to other parts of Indonesia.

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of marketplaces, village had an agricultural kiosk, government spending on staff, capital, and other, number of government staff, and village head age.

<sup>22</sup>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.

<sup>23</sup>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.

### 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.<sup>24</sup>

In addition to our relatively low level of statistical power, a related concern is that our relatively small sample of treatment communities and block randomization mean our 75 treatments may not be truly independent. Panel estimates leveraging pre-period data help address both issues. Tables A5–A7 present omnibus panel results, from random effects and difference-in-difference estimators, on both outcomes. 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 more precise but still small and not statistically distinguishable from zero.

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<sup>24</sup>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.

An alternative approach is to estimate period-specific effects in the months following the treatment. Figure A4 finds evidence of 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). The program appears to have had its desired impacts immediately, but not beyond that. 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, providing no further empirical support for this explanation.

## 5.4 Potential heterogeneity

One remaining concern is that—withstanding the 2014 Village Law decentralizing significant resources and responsibilities to villages—districts remain an important subnational government responsible for the majority of service delivery and regional policy differences. Districts enact their own laws and regulations, including on land use and the environment, and often offer specific policy instructions to villages, potentially mediating village responses across districts. Irawan et al. (2019), for example, argue that village environmental programs related to REDD+ should be coordinated at the district level. Table 4 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, combined effects are close to zero and not statistically significant throughout.

## 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., 2019). 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 5 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 of the point estimates point towards 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). 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 of our groups and not the other. Second, national efforts were clearly not that important for our study villages because around 70 percent of them still had fire. Third, 2018 was not a particularly abnormal 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. 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 7 showed no distinguishable differences the year after the program, when there were no conditional payments and successful villages had already been paid. Alpizar et al. (2019) similarly find no effects from capacity building and information workshops after two years. 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 to the expected benefit stream from conversion.

The third and fourth explanations reflect the 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, 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.

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 created disincentives to adhere to the program.<sup>25</sup> 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. Since villagers usually set the fires (c.f., village governments), one might argue we incentivized the wrong units and should have attempted to focus on infra-marginal people.

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) and still lacked the necessary tools after the small up-front grants. 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

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<sup>25</sup>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 (2020) highlight how redistricting along group lines reduced conflict but that increased polarization, along ethnic lines, can increase it. Our for study districts did not split.

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.<sup>26</sup> Indeed, even if the fires are all set by humans, 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 is likely due to a combination of a collective action failure and the incentive not being strong enough. Relative to prior work, our null findings are unsurprising. Collective action problems are extremely challenging in weak institutional environments, and working at the community rather than individual-level was a key innovation of this trial. The potential opportunity costs here are also quite high, as households' alternative here is not low-productivity agriculture but a 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.<sup>27</sup>

## 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 the harmful 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, a small up-front cash grant, 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

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<sup>26</sup>We are particularly thankful to Chris Barrett for this valuable insight.

<sup>27</sup>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.



naturally occurring fires. Villages randomly assigned to the program were more 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 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).<sup>28</sup> 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.

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<sup>28</sup>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."

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## 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 [1,360]	14,200 [2,130]	0.799	-0.031
Fires in 2015 (N)	12.379 [1.695]	11.893 [2.679]	0.912	0.021
Fires in 2014 (N)	8.182 [1.099]	8.453 [1.536]	0.870	-0.018
Fires in 2013 (N)	3.970 [0.334]	3.240 [0.434]	0.273	0.163
Households (N)	333.020 [15.519]	318.387 [26.226]	0.429	0.066
Oil palm area (ha)	153 [24.7]	132 [33.7]	0.810	0.063
Dirt road (=1)	0.677 [0.033]	0.600 [0.057]	0.260	0.161

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

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: MAIN RESULTS—EFFECTS ON FIRE

Column	1	2	4	5	6
Panel A outcome	Village had any fire (=1)				
$\beta$ (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)				
$\beta$ (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 4: 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 5: EFFECTS ON VILLAGE TREE COVER LOSS

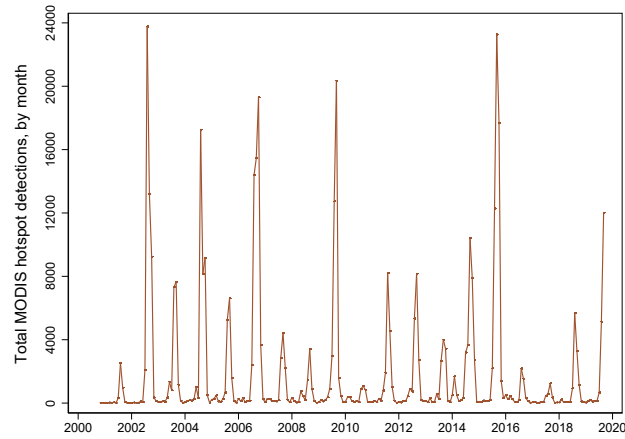
Column	1	2	3	4	5
Panel A outcome	Village tree cover loss (ha)				
$\beta$ (treatment=1)	3.999	6.501	13.947	17.026	14.253
Robust S.E.	(20.617)	(20.399)	(19.134)	(18.698)	(18.895)
R.I. p-value	[0.849]	[0.751]	[0.497]	[0.400]	[0.498]
$R^2$	0.000	0.028	0.155	0.218	0.298
Adjusted $R^2$	-0.004	0.013	0.133	0.181	0.195
Panel B outcome	IHS-transformed village tree cover loss (ha)				
$\beta$ (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.

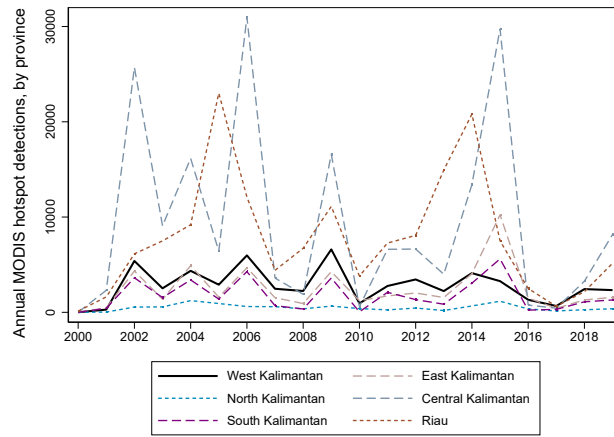
FIGURE 1: WEST KALIMANTAN, INDONESIA



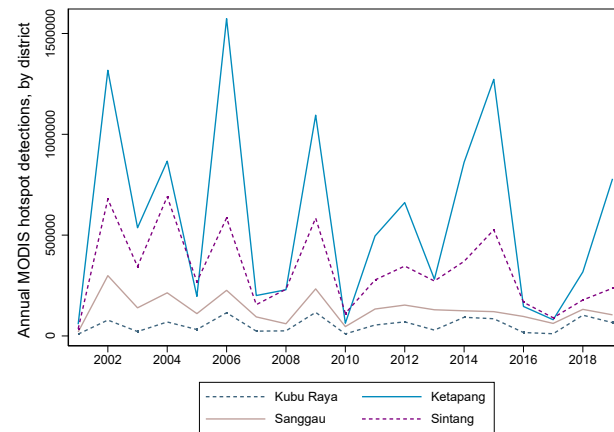
FIGURE 2: HOTSPOT DETECTIONS OVER TIME



(A) NATIONAL-LEVEL—MONTHLY DATA



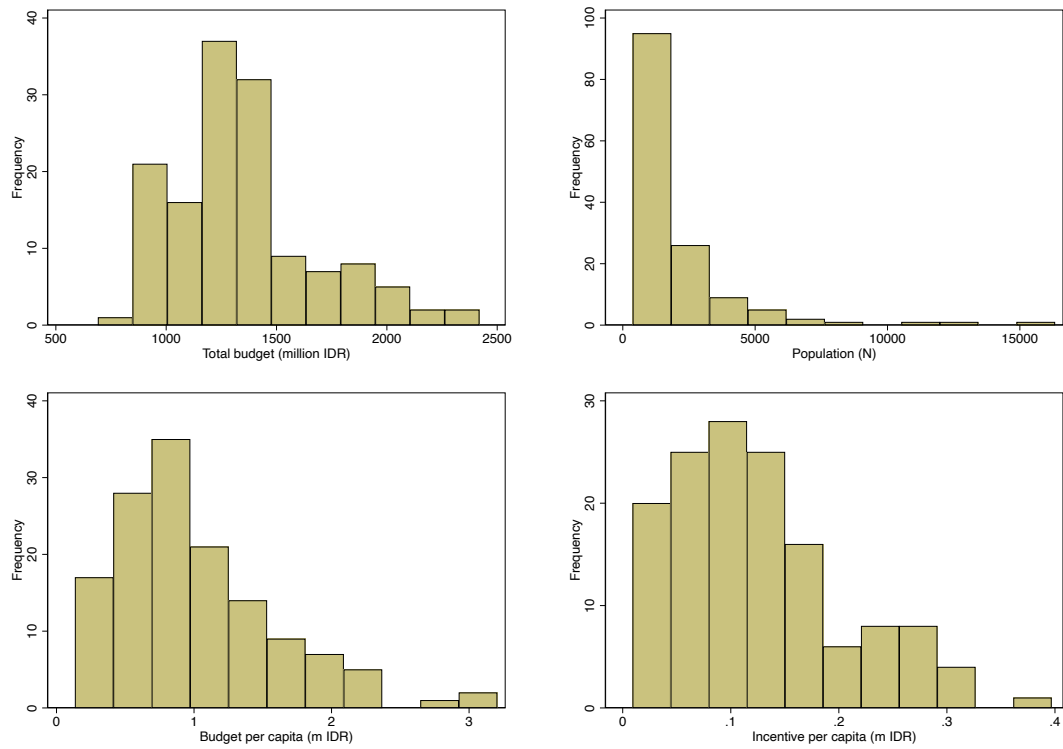
(B) PROVINCE-LEVEL—ANNUAL DATA



(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

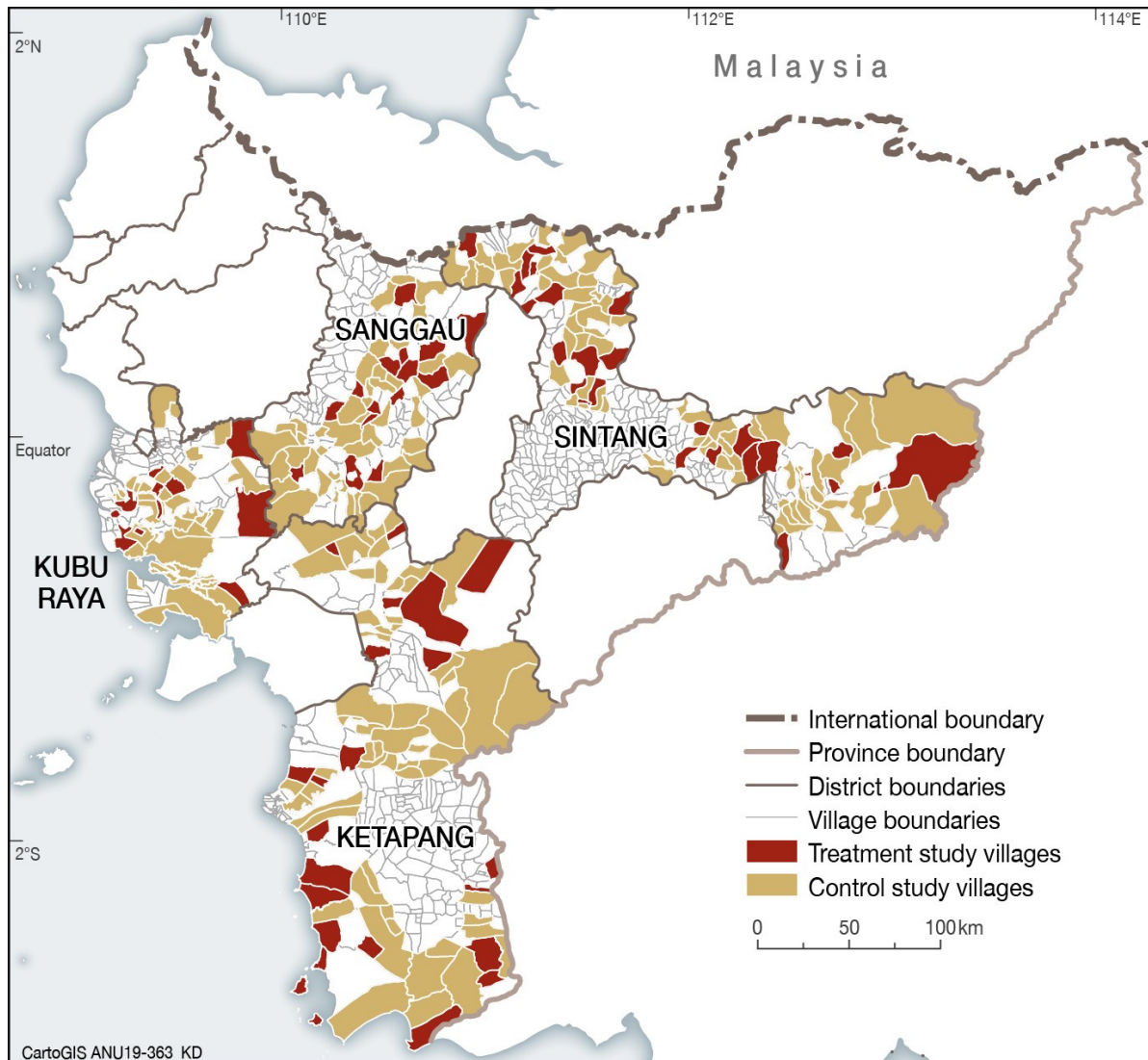
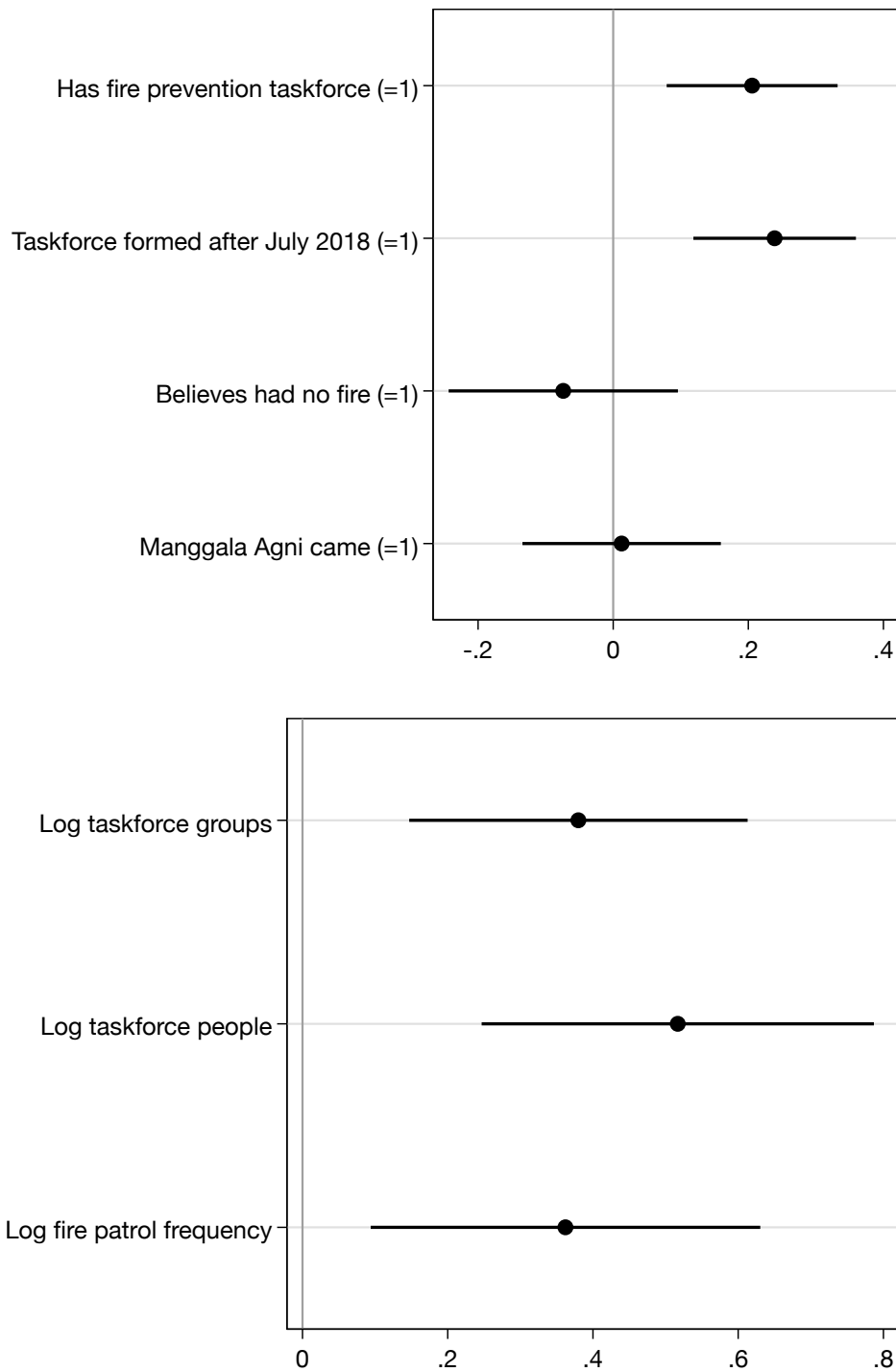
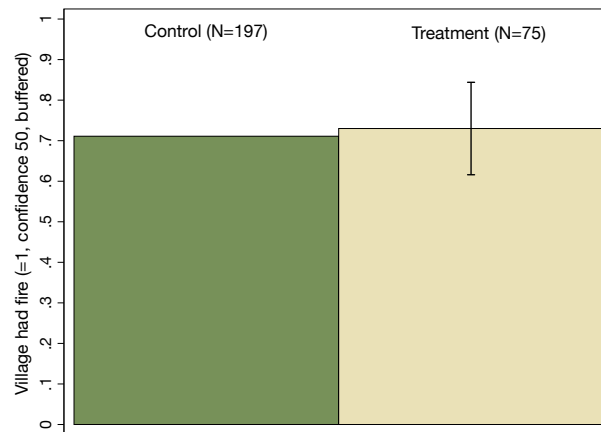


FIGURE 5: EFFECTS ON VILLAGE FIRE BEHAVIOR

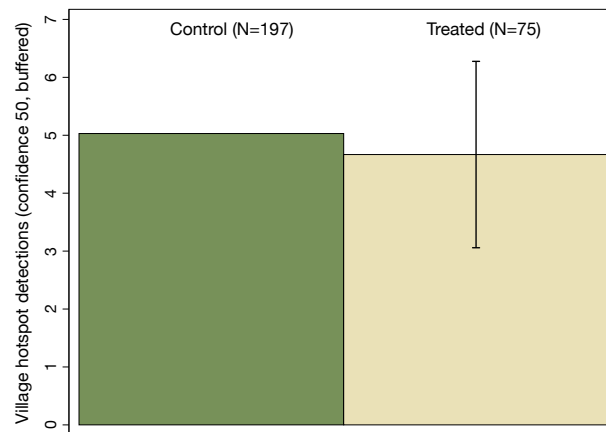


*Notes:* These figures plot the estimated treatment effects on intermediate outcomes capturing fire-related behavior which we measured in an endline survey. District strata fixed effects and balancing variables are included throughout and robust confidence intervals are at the 95 percent level. The sample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

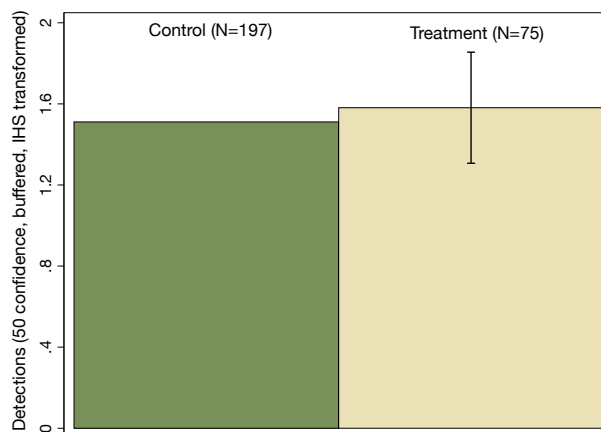
FIGURE 6: EFFECTS ON VILLAGE FIRE OUTCOMES



(A) PROBABILITY OF ANY FIRE (=1)



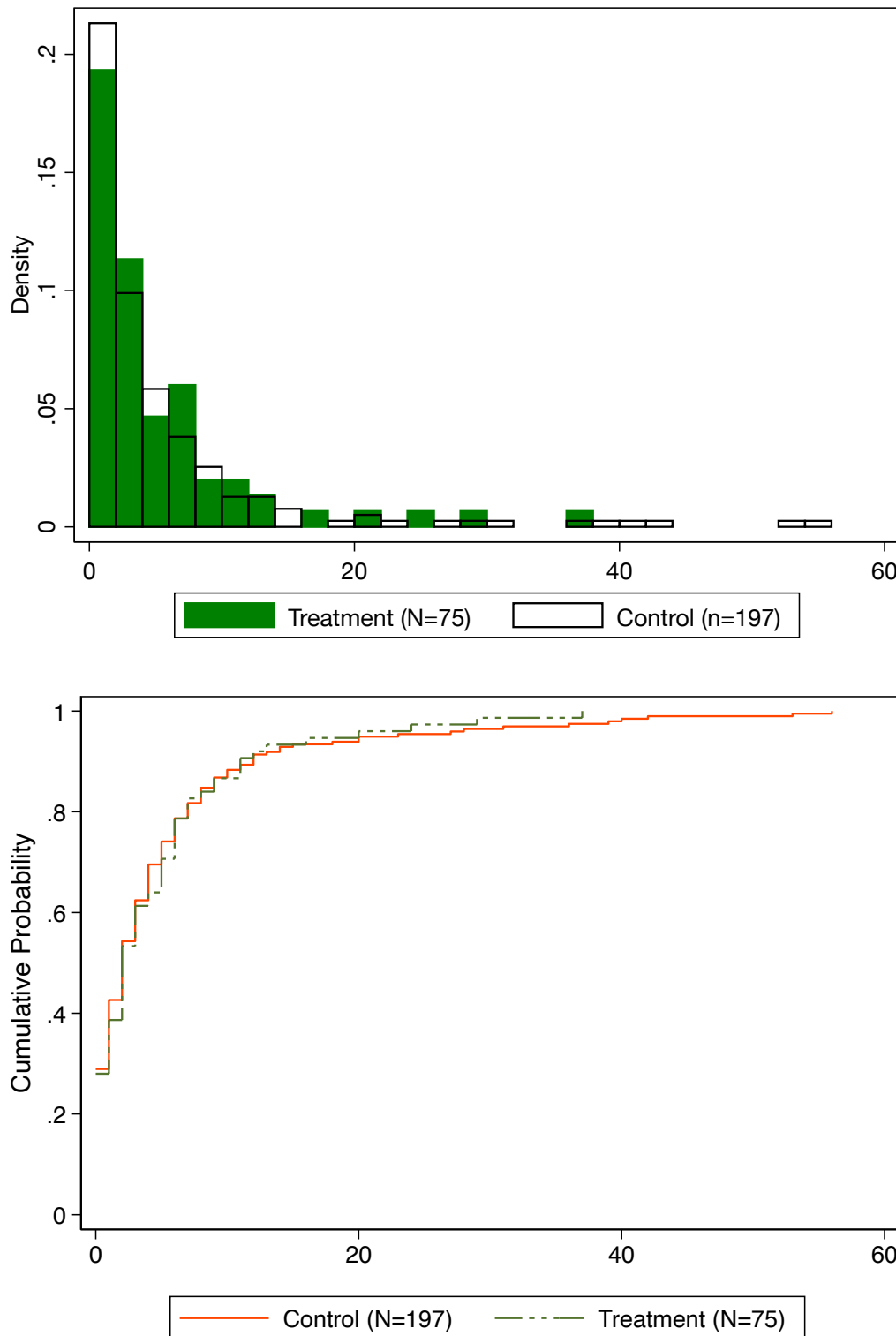
(B) NUMBER OF DETECTIONS (N)



(C) IHS TRANSFORMED HOTSPOTS (N)

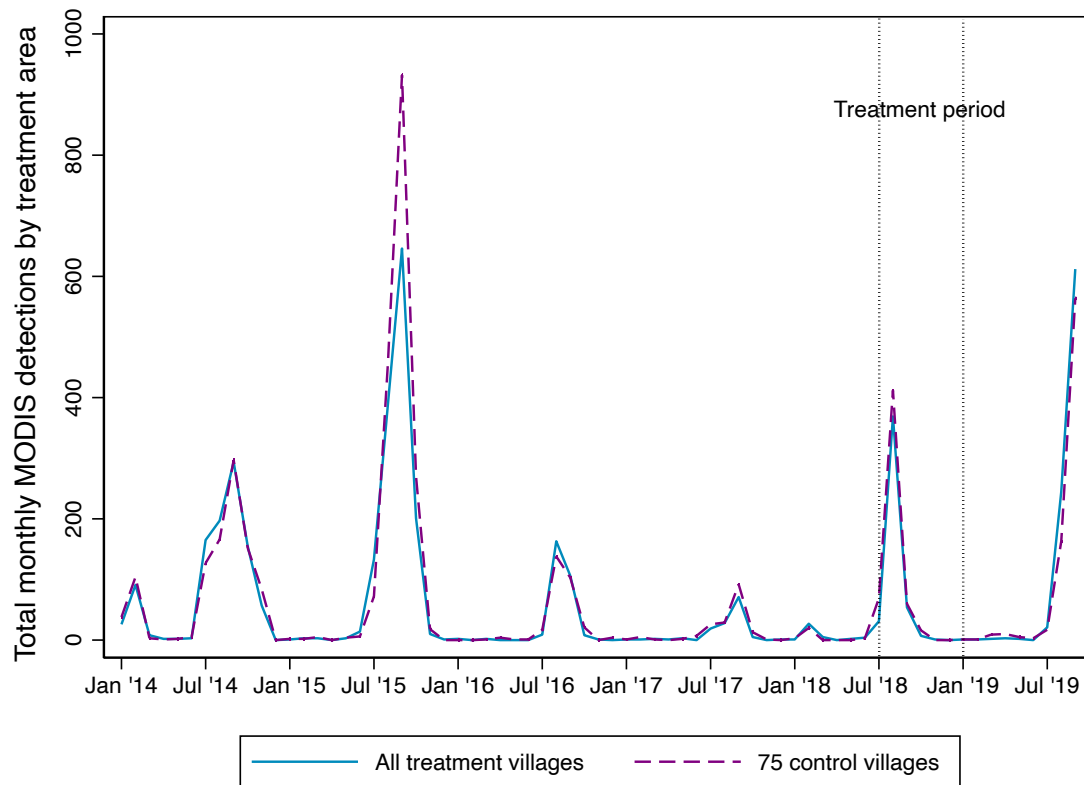
*Notes:* These figures plot the estimated treatment effects using (a) a binary indicator of fire (where success in having no fire is zero), (b) the count of the number of detections, and (c) the inverse hyperbolic transformation of the count as outcomes. District strata fixed effects and balancing variables 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: TOTAL MONTHLY HOTSPOT DETECTIONS BY TREATMENT STATUS



*Notes:* This figure plots the total monthly MODIS hotspot detections over our whole 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.

## **Fight Fire with Finance**

### **Online Appendix—Not For Publication**

#### **Supplementary Tables**

Table A1—Village Budget Descriptive Statistics

Table A2—Main Results Using Equal-Sized Groups

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

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

Table A5—Annual Panel Estimates, Probability of Any Fire (LPM)

Table A6—Annual Panel Estimates, Hotspot detections (OLS)

Table A7—Annual Panel Estimates, Hotspot detections (Poisson)

Table A8—Heterogeneous Treatment Effects, Probability of Any Fire (LPM)

Table A9—Heterogeneous Treatment Effects, Hotspot Detections (OLS)

#### **Supplementary Figures**

Figure A1—Village Hotspot Detections in 2018

Figure A2—Hotspots and Tree Cover Loss

Figure A3—Fire-free Program Villages

Figure A4—Dynamic Treatment Effects, By Month

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)				
$\beta$ (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)				
$\beta$ (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
<i>MODIS</i>				
All	0.817 [0.028]	0.827 [0.044]	0.857	-0.024
Confidence level 50	0.787 [0.029]	0.813 [0.045]	0.630	-0.065
Confidence level 80	0.563 [0.035]	0.560 [0.058]	0.959	0.007
All buffered	0.741 [0.031]	0.760 [0.050]	0.750	-0.043
Confidence level 50 buffered	0.711 [0.032]	0.720 [0.052]	0.880	-0.021
Confidence level 80 buffered	0.472 [0.036]	0.467 [0.058]	0.937	0.011
<i>VIIRS</i>				
All	0.949 [0.016]	0.987 [0.013]	0.163	-0.190
Nominal and high	0.949 [0.016]	0.987 [0.013]	0.163	-0.190
High	0.497 [0.036]	0.547 [0.058]	0.470	-0.098
All buffered	0.944 [0.016]	0.987 [0.013]	0.128	-0.207
Nominal and high buffered	0.944 [0.016]	0.987 [0.013]	0.128	-0.207
High buffered	0.487 [0.036]	0.493 [0.058]	0.930	-0.012

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
<i>MODIS</i>				
All	7.980 [0.919]	7.507 [1.235]	0.778	0.038
Confidence level 50	6.736 [0.763]	6.200 [0.978]	0.697	0.053
Confidence level 80	2.756 [0.442]	2.427 [0.484]	0.671	0.058
All buffered	6.061 [0.763]	5.760 [0.998]	0.828	0.030
Confidence level 50 buffered	5.030 [0.628]	4.640 [0.778]	0.729	0.047
Confidence level 80 buffered	1.995 [0.357]	1.667 [0.378]	0.599	0.071
<i>VIIRS</i>				
All	33.751 [3.618]	33.067 [5.478]	0.920	0.014
Nominal and high	32.548 [3.509]	31.907 [5.292]	0.922	0.013
High	1.934 [0.270]	1.467 [0.272]	0.320	0.135
All buffered	30.624 [3.378]	29.760 [5.061]	0.891	0.019
Nominal and high buffered	29.528 [3.282]	28.733 [4.877]	0.897	0.018
High buffered	1.787 [0.258]	1.307 [0.265]	0.287	0.145

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 (0.055)	0.013 (0.055)	0.013 (0.055)	0.013 (0.055)	0.013 (0.055)	0.013 (0.055)	0.021 (0.056)
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 (1.687)	-0.641 (1.689)	-0.641 (1.687)	-0.641 (1.689)	-0.641 (1.687)	-0.641 (1.689)	-0.375 (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 Estimator Column	Fire count (N)			
	Poisson	Poisson	PPML	PPML
	1	2	3	4
Post x treatment	-0.097 (0.232)	-0.097 (0.232)	-0.097 (0.232)	-0.064 (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)									
	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
$\beta$ (treatment=1)	-0.051 (0.089)	0.038 (0.079)	0.694*** (0.181)	0.037 (0.092)	-0.000 (0.086)	0.082 (0.062)	0.028 (0.086)	-0.091 (0.104)	0.017 (0.080)	0.029 (0.081)
$\gamma$ (balancing variable)	0.240*** (0.064)	-0.181** (0.083)	0.375** (0.185)	-0.087 (0.067)	-0.010 (0.070)	-0.235*** (0.063)	0.088 (0.065)	-0.043 (0.069)	-0.036 (0.066)	0.003 (0.066)
$\theta$ (interaction term)	0.119 (0.113)	-0.094 (0.121)	-0.694*** (0.191)	-0.045 (0.122)	0.032 (0.122)	-0.151 (0.113)	-0.014 (0.122)	0.171 (0.128)	-0.013 (0.125)	-0.027 (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

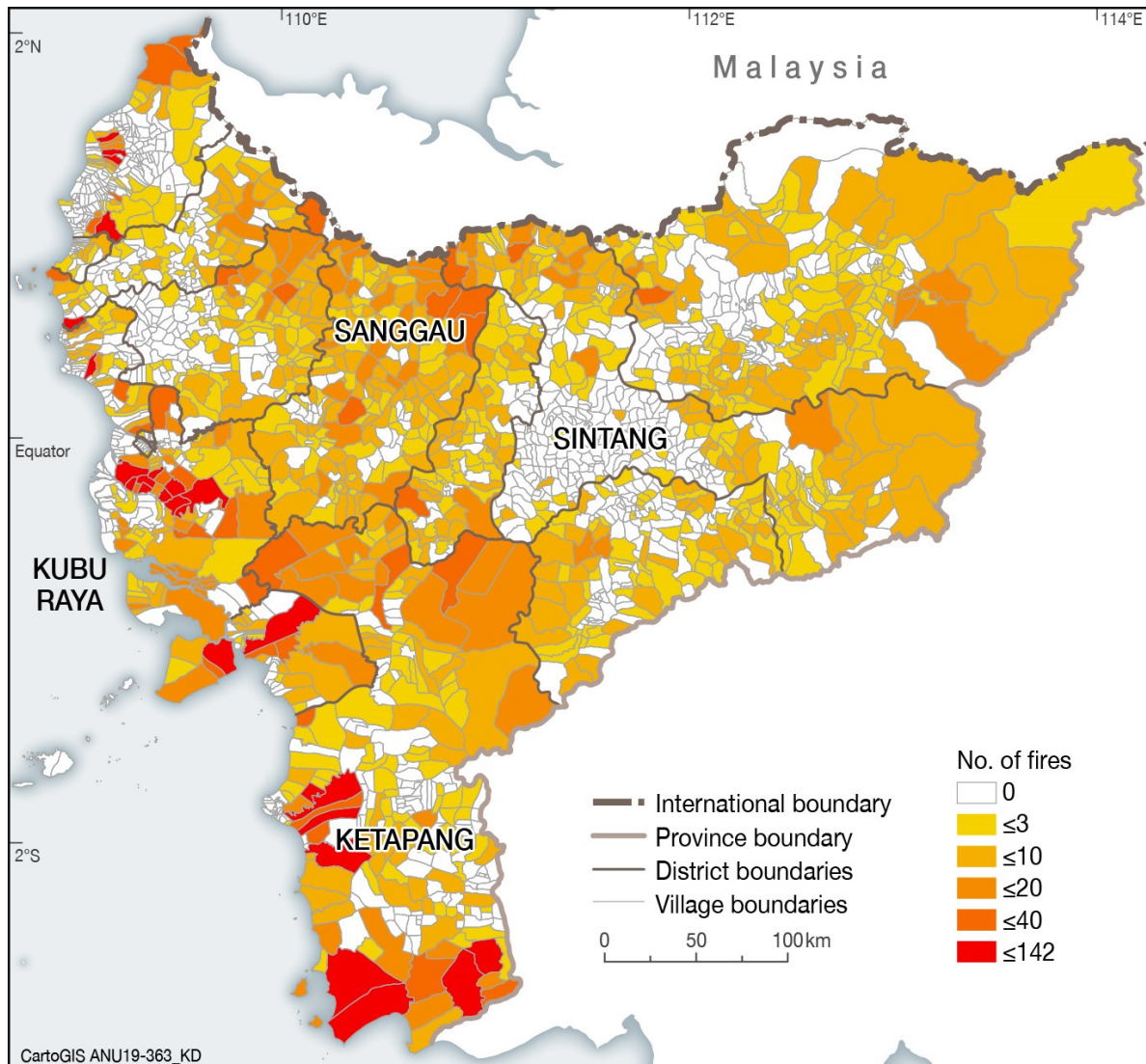
Notes: This table reports regression results exploring heterogeneous treatment effects. Although we balanced on these outcome variables, we did not set our sample size for sub-group analysis and these estimates are best viewed as exploratory. Treatment is a binary indicator set equal to one if a village 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)									
	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
$\beta$ (treatment=1)	-0.044 (0.189)	0.040 (0.216)	1.459*** (0.482)	0.232 (0.264)	0.093 (0.251)	0.053 (0.215)	0.088 (0.234)	-0.178 (0.308)	0.079 (0.227)	0.112 (0.229)
$\gamma$ (balancing variable)	1.142*** (0.164)	-0.289 (0.222)	0.759 (0.493)	-0.117 (0.190)	-0.273 (0.187)	-0.882*** (0.167)	0.247 (0.182)	-0.283 (0.199)	-0.109 (0.184)	0.089 (0.183)
$\theta$ (interaction term)	0.084 (0.297)	-0.075 (0.339)	-1.456*** (0.510)	-0.340 (0.338)	-0.070 (0.334)	-0.089 (0.310)	-0.083 (0.334)	0.332 (0.365)	-0.137 (0.335)	-0.173 (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

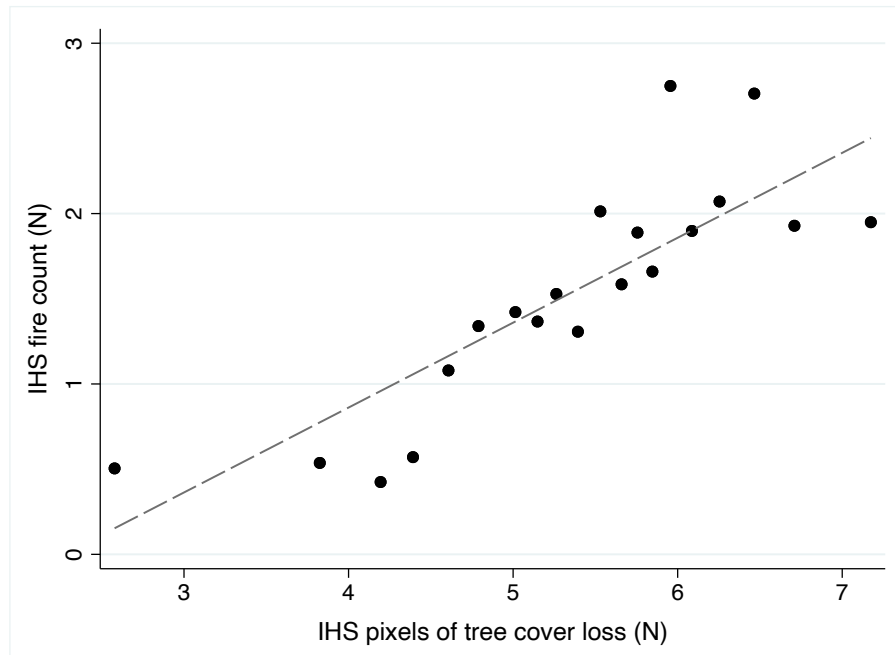
Notes: this table reports regression results exploring heterogeneous treatment effects. Although we balanced on these outcome variables, we did not set our sample size for sub-group analysis and these estimates are best viewed as exploratory. Treatment is a binary indicator set equal to one if a village 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.

FIGURE A1: VILLAGE HOTSPOT DETECTIONS IN 2018

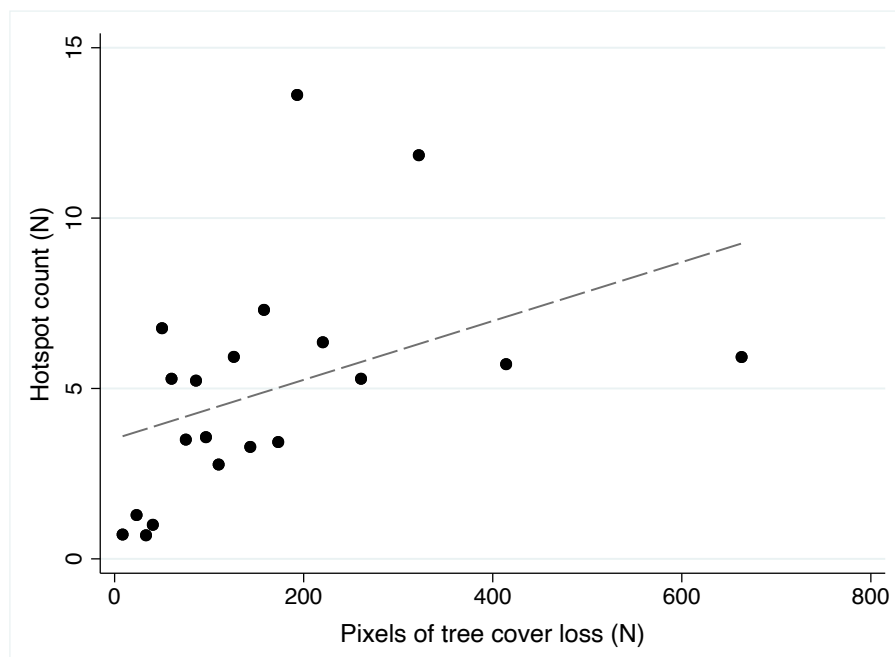


*Notes:* This figure maps the number of hotspots detected by MODIS in each Village in West Kalimantan after applying a 50 confidence filter. Note that this not precisely the MODIS hotspot data used in estimation and to determine success, as some further processing was required (e.g., removing swidden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

FIGURE A2: MODIS HOTSPOTS AND TREE COVER LOSS



(A) IHS-IHS

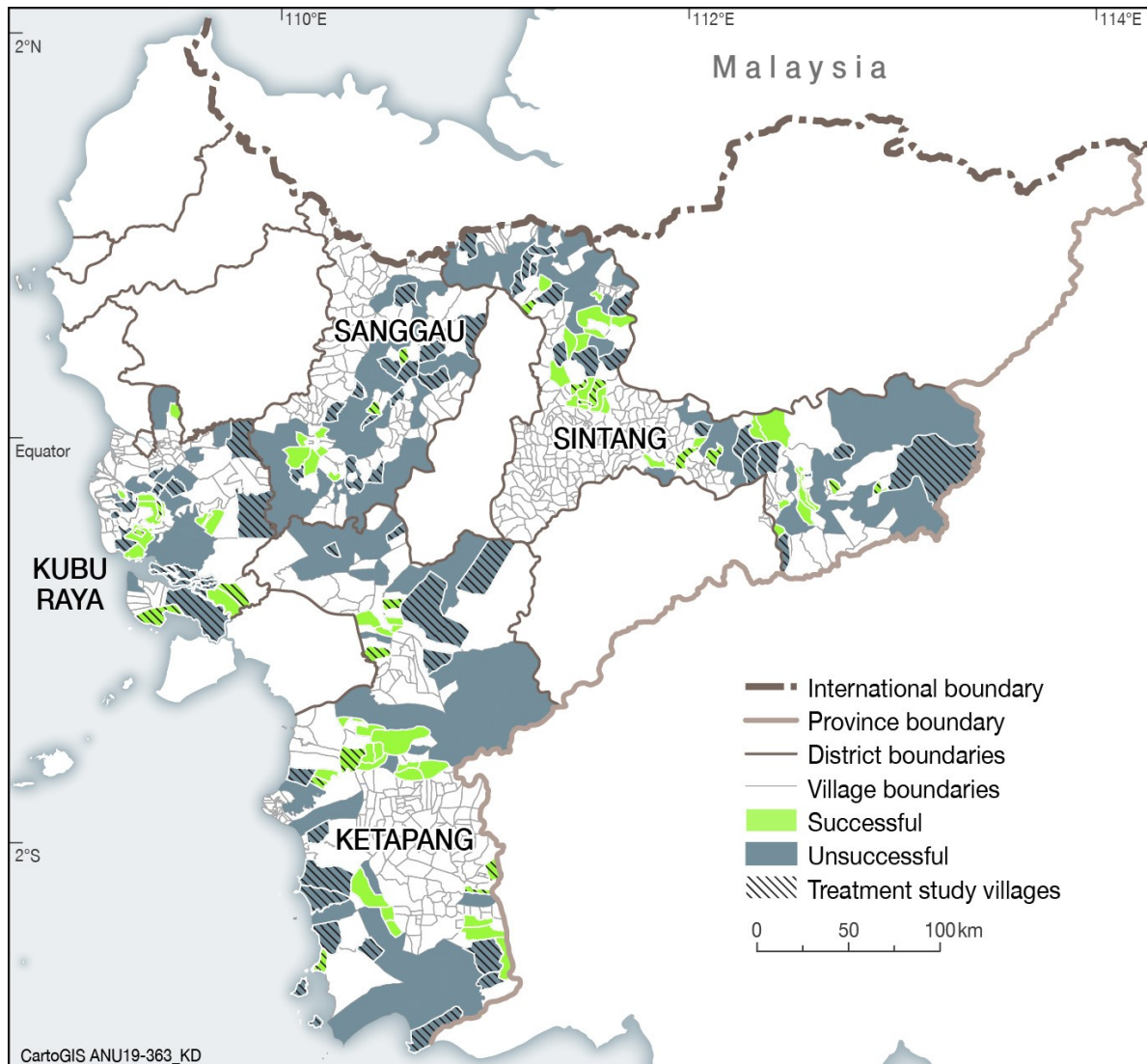


(B) LINEAR-LINEAR

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

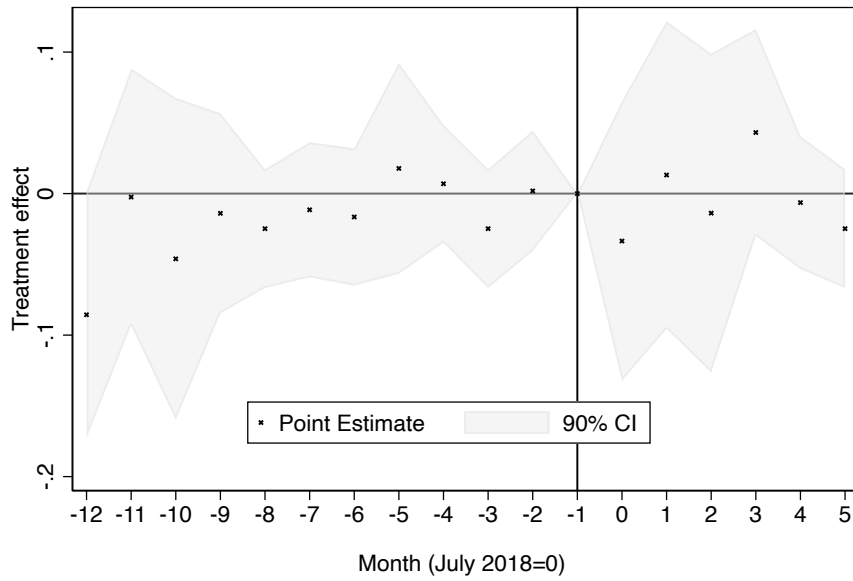


FIGURE A3: FIRE-FREE PROGRAM VILLAGES

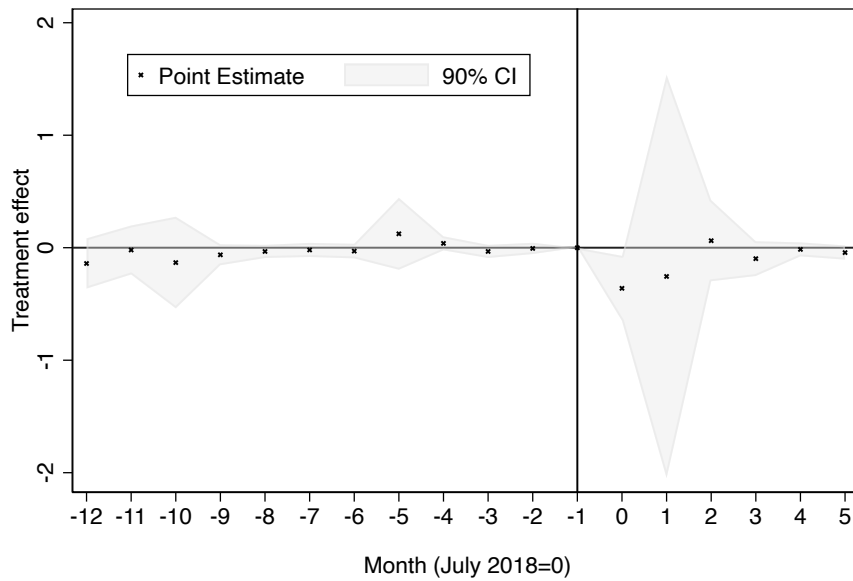


*Notes:* This figure visually shows the final outcome of the pilot program, mapping the locations of the randomly-assigned treatment and control villages, and shading those which were successful in going fire-free and those which were not.

FIGURE A4: DYNAMIC TREATMENT EFFECTS—EVENT STUDIES



(A) VILLAGE HAD ANY HOTSPOT DETECTIONS (N=1)



(B) NUMBER OF 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. Both models include district-by-period and village fixed effects, and additional leads are omitted from the figures for brevity.



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