Combatting forest fires in the drylands of Sub-Saharan Africa: Quasi-experimental evidence from Burkina Faso*

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Abstract

Forest fires are among the main drivers of deforestation and forest degradation in the drylands of Sub-Saharan Africa. We use remote sensing data on forest fires and remaining tree cover to estimate the effectiveness of a project targeted at reducing fire incidences in twelve protected forests in arid Burkina Faso. The project consisted of two components that were implemented in the villages surrounding the target forests: a campaign aimed at raising community awareness about the detrimental effects of forest fires, and a program to support establishing and maintaining forest fire prevention infrastructures. Using the Synthetic Control Method we find that the project resulted in an overall reduction of 35% in the number of days on which an average forest grid cell was detected to be on fire in the period of the year when fires tend to be most prevalent – at the very end of the agricultural season. This impact is, however, short-lived (as the reduction only occurred in the first four years of the program), and the overall reduction in forest fire occurrences was not sufficiently large to result in a detectable increase in vegetation cover. We then try to uncover the underlying mechanisms to shed light on which of the project's components were effective, to also learn how the program can be improved.

Keywords: Forest conservation, forest fires, deforestation, synthetic control method.
JEL Codes: Q23, Q56, Q28, O13.

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1 Introduction

Forest fires rank high among the key causes of global forest degradation and forest loss. They affect about 2% of the world's forested area every year (van Lierop *et al.*, 2015; Tyukavina *et al.*, 2022), and are thus important contributors to both climate change and biodiversity loss (Moritz *et al.*, 2014; Oreskes, 2004; Kelly and Brotons, 2017; Le Quéré *et al.*, 2018). Forest fires are especially harmful in Sub-Saharan Africa where they are responsible for 90% of the continent's forest loss as well as for 50% of the world's fire-related carbon emissions (van Lierop *et al.*, 2015; Andela and van der Werf, 2014). Fires occur because of natural causes, but the bulk of the forest fires in Sub-Saharan Africa are the result of economic activity (Le Page *et al.*, 2010; CIFOR, 2016). Fire is used to clear agricultural land, produce ashes to fertilize the soil, drive out wildlife for hunters, stimulate the growth of young shoots as feed for cattle, and to produce charcoal as fuel (Savadogo *et al.*, 2007; Sawadogo, 2009; Potapov *et al.*, 2012; Sow *et al.*, 2013; Curtis *et al.*, 2018). While forest conservation is recognized as a key strategy to mitigate climate change (as evidenced by the United Nation's REDD and REDD+ programs), relatively little is known about how effective forest conservation policies are in reducing forest fires, especially so in the dryland forests of Sub-Saharan Africa.

In this paper we evaluate the impact of a policy targeted at reducing both the number and the geographical spread of forest fires in twelve of Burkina Faso's 77 protected forests. Because of the country's arid climate, forest fires are especially damaging as the combination of limited annual rainfall and frequent fires prevent tree cover regeneration. Fires thus increase forest fragmentation in forests where tree canopy cover is sparse already (Cochrane, 2003; Hoffmann *et al.*, 2009; Staver *et al.*, 2011; Dwomoh *et al.*, 2019). The program we evaluate can be characterized as a participatory forest management project aimed at increasing community interest and involvement in forest conservation in line with, for example, Agrawal and Ostrom (2001). It was designed by the government of Burkina Faso as part of its Forest Investment Program (FIP), and it was cofunded by the World Bank and the African Development Bank.

The program was launched in October 2014, and consisted of two main parts. One, the program aimed to sensitize communities living in the vicinity of each of the project forests to the detrimental effects of forest fires. Two, the program also aimed to actively engage community members in local conservation activities using a combination of technical support (by experts from the regional and national authorities) and improved coordination of management activities between adjacent communities. Communities were encouraged to participate in setting up fire barriers within forests to compartmentalize wildfires, in establishing forest management infrastructures and in protecting and monitoring the forests. Taken together, these efforts were expected to reduce both the frequency with which forest fires were started as well as their spread, and especially so at the end of the agricultural season (in November and December) when most of the forest fires take place. We use the Synthetic Control Method (Abadie and Gardeazabal, 2003; Abadie, 2021) to estimate the impact of the policy intervention over the period 2014-2019, the first six years after its inception, using remote sensing data.

Overall, we find that the project was not very effective in reducing fire-induced forest degradation. Forest fires occurrences were reduced in the project forests, but only in the month of November (the month in which most of the post-harvest forest fires take place), and also in just the first four years after the start of the program. The program did not reduce forest fires occurrences in any of the other months in the dry season. And even though the project managed to decrease the November forest fires by on average 35% in the first four years, we detect neither a decrease in annual forest fire occurrences nor an increase in overall vegetation cover.

While the impact of the project was thus limited at best, we perform additional analyses to gain insight into the mechanisms via which the forest fire occurrences were achieved in this November month. First, the timing of the impact – the first month after harvest – suggests that the effect is driven by farmers; additional support for this hypothesis comes from the fact that most of the reduction of forest fire occurred on the forest fringe, where agriculture is the main economic activity. Second, we find that improved forest fire containment is likely to have been the key driver in reducing forest fire occurrences (although we cannot rule out that the program may have also been effective in reducing the number of forest fires started). Combined, these results suggest that to make the policy more effective, more attention should be paid to the behavioral aspects of forest fire prevention – among farmers, but especially also among hunters and livestock herders. Third, we also analyze how estimated treatment effect on November fires is moderated by a number of characteristics of the local communities surrounding the forests. Before the intervention, forest fires were more prevalent around communities with lower average income, where agriculture was relatively intensive (as evidenced by the use of chemical fertilizers and pesticides), and with better access to regional markets (as measured by the proximity to the local road network). Regarding the program's effectiveness, we find that, in absolute terms, the impact on the average number of days in a month on which a grid cell was detected to have been on fire is largest in areas where forest fires were more prevalent before the program. We also find that the impact is larger the smaller the distance to the neighboring villages, but otherwise the impact is by and large independent of all other community characteristics. Our study thus predicts that when rolled out to other forests (either within Burkina Faso, or elsewhere in the region), the intervention will be most effective in those areas where forest fires are most frequent, independent of their cause.

This paper contributes to two strands of literature. First, it contributes to the literature on the effectiveness of forest conservation policies that target forest fires in the developing world. Even though (i) improved conservation in developing countries is widely recognized to be a key component of the global effort to combat climate change and (ii) the effectiveness of forest conservation policies in reducing deforestation has been widely studied (Agrawal *et al.*, 2011; Börner *et al.*, 2020), little attention has been devoted to exploring the effectiveness of forest policies in reducing forest fires. One exception is Nelson and Chomitz (2011), who used exact- and nearest-neighbor matching to show that protected areas that strictly limit access to forests by communities can reduce forest fires in the tropical forests of Latin America, Asia, and Africa. However, policymakers concerned about rural livelihood are less inclined to implement (strict) protected areas and call for alternative fire reduction policies (Andam *et al.*, 2010; Sims and Alix-Garcia, 2017). Paudel (2021) provides evidence on the differences between community-managed forests and non-community managed forests during the COVID-19 pandemic in Nepal. The study finds that the restrictions on movement imposed by the government was effective in reducing the overall forest fire occurrences in non-community managed forests, but not in that were managed by the nearby communities. The study by (Edwards *et al.*, 2020a) is most closely related to our, as they focus on assessing the effectiveness of a policy aimed at reducing forest fire incidence. More specifically, they use a field experiment to study the effectiveness of financial incentives to reduce forest fire damages in humid Indonesia. They find that offering conditional payments to communities resulted in increased forest management activities to prevent forest fires, but they do not find an impact on forest fire occurrence, the number of fires started usage nor forest cover.[1] Contrary to the findings of Edwards *et al.* (2020a), we find that a community forest management project improved forest fire containment in the most fire-prone month in the dry forests of West Africa, where the vegetation is more flammable than in the tropical forests of Indonesia. Our results are complementary to those of Edwards *et al.* (2020a) in that this is effect was just temporary (both within the season and across years), and also that it was too small to effectively increase tree cover.

Second, we contribute to the understanding of the anthropogenic sources of forest fires in the dry forests of Africa. While ecologists have studied the environmental consequences of forest fires in many different biomes (Cochrane, 2001, 2003; Muñoz-Rojas *et al.*, 2016), understanding of the socio-economic drivers of these fires is still limited, especially in developing countries (Balboni *et al.*, 2021). Our work is closely linked to that of Edwards *et al.* (2020b) who show that fires in the rainforests of Indonesia are more prevalent in the proximity of villages that are either very poor, or that have a long history of using fire to clear land. We confirm the role of these socio-economic factors in the context of Sub-Saharan Africa, where socio-economic institutions and climatic conditions are markedly

[1]Although not motivated by forest conservation, Jack *et al.* (2022) implemented a field experiment to study the impact of payment for ecosystem services conditional on not using fires for crop residue burning at the end of harvest in India. They find that conditional payments offered to individual farmers reduced the use of fires for crop residue burning. However, the context of their study does not allow them to evaluate the impact of payments on forest fires.

different. Our results also support earlier findings from Latin-America that forest fires are prevalent around forests that are accessible by roads (Nepstad *et al.*, 2001; Joppa and Pfaff, 2010). We show that access to forests is a key factor for consideration for forest management policies as it mitigates the effectiveness of these policies in reducing forest fires and where the drivers of deforestation are common between countries (Rudel, 2013).

The remainder of this paper is organized as follows. Section 2 provides an overview of the role of forest fires in forest degradation and deforestation in Burkina Faso, as well as the details of the program the Burkinabé government implemented to reduce forest fires. Section 3 presents the empirical approach we use to evaluate the impact of the program and Section 4 presents the data we use in the analysis. Section 5 presents the results on the FIP program's impact on both forest fires and vegetation cover, and Section 6 explores the role of the characteristics of forest communities surrounding the targeted forests in moderating the overall impact. Section 7 concludes.

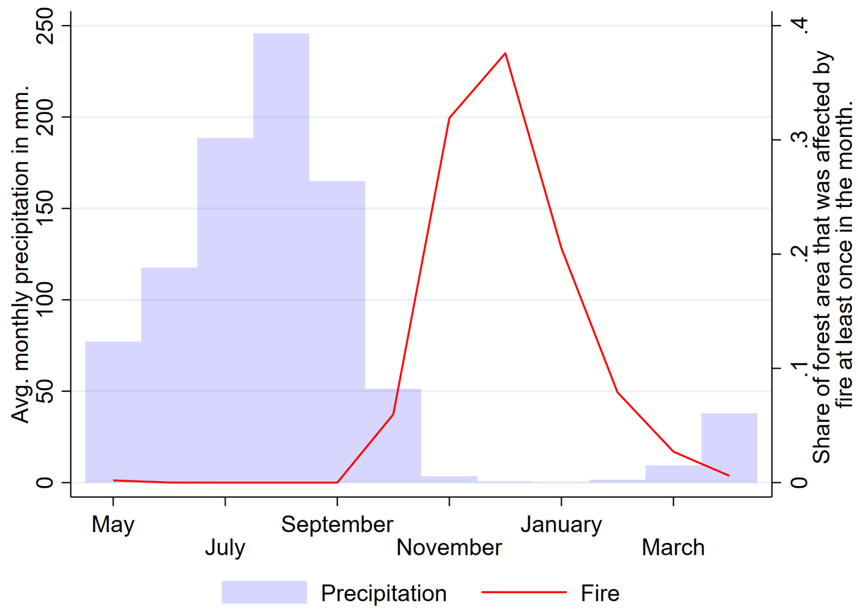
2 Description of the study context

2.1 The role of fires in forest degradation

Despite the Burkinabé government's efforts to mitigate deforestation and forest degradation, the annual rate of deforestation is considerable. In the period between 1990 and 2010, deforestation rates were, on average, about 1.1% per annum (FAO, 2014). This rate of forest loss is especially worrisome as natural regeneration is hampered by the country's very low levels of rainfall (on average about 600–1000 mm per year, concentrated on between 50 and 100 days in the rainy season; MECV (2014)); see Figure 1 (and then especially the grey bars therein). The rate of natural regeneration is thus low, and hence Burkina Faso's forests are threatened by forest fragmentation as well as by a loss of resilience to extreme climate conditions (Miles *et al.*, 2006).

The main proximate causes of forest cover loss in Burkina Faso are land conversion (especially for agriculture and cattle herding), logging (especially for the production of firewood and charcoal), and bush and forest fires; see Pouliot *et al.* (2012) and CIFOR

Figure 1: Average precipitation per month of the year (left axis and grey bars), and the average share of Burkina Faso's protected forest areas that are affected by fire (right axis and solid line), in the period between 2003 and 2013.



Note: The average amount of precipitation (measured in cubic millimeters) received by the 77 protected forests in Burkina Faso in each of the calendar months in the period 2003-2013, as well as the share of forest cover having been affected at least once by forest fires in each month in that same period.

(2016). Expansion of agricultural and pastoral activities is driven mainly by population growth (Pouliot *et al.*, 2012; Ouedraogo *et al.*, 2009) combined with very limited improvements in land productivity (Goldstein and Udry, 2008; Etongo *et al.*, 2015). Population growth is also the main driver of the increasing demand for firewood and charcoal, the two most affordable energy sources for low-income, rural households (Ouedraogo, 2006; Ouedraogo *et al.*, 2011; Bensch *et al.*, 2015).

Forest fires, the third cause of forest degradation, can occur because of natural causes, but the vast majority of fires in Burkina Faso are man-made (Menaut *et al.*, 1991; MECV, 2007; Devineau *et al.*, 2010; Le Page *et al.*, 2010; CIFOR, 2016). This is the case even though starting fires in forested areas has been declared illegal from 1997 onward. As shown by the solid line in Figure 1, the bulk of these fires occur in the dry season (between November and February) when rainfall is low (as reflected by the grey bars), implying that the vegetation is dry and highly combustible. Farmers use fire to remove crop residue that remains after harvest and to restore soil fertility on previously cultivated land (in

October/ November), or to clear new land (in March/ April). These fires may run out of control and spread from cultivated lands to neighboring forest lands (Savadogo *et al.*, 2007; Sow *et al.*, 2013). Fire is also used by cattle herders to stimulate regrowth of young sprouts as feed for cattle, and by hunters (or poachers) to spot and drive out game (Sawadogo, 2009). Fires originating from all these different types of economic activity are detrimental to forests as they do not only damage the canopy of developed trees, but they also hamper the development of seeds (Zida *et al.*, 2007). Forest fires in Burkina Faso thus result in an impoverished and fragmented forest biome, and possibly even in the degradation of forests to savanna grasslands (Sawadogo, 2009; Devineau *et al.*, 2010; Sow *et al.*, 2013). Mitigating the damages from forest fires can be achieved both by reducing the number of forest fires started as well as by better forest fire containment.

2.2 Burkina Faso's Forest Investment Project

As part of the country's effort to reduce deforestation and forest degradation and to improve carbon sequestration, Burkina Faso's government implemented the Forest Investment Program (FIP) with financial support from the World Bank, the African Development Bank, and the Climate Investment Fund. Twelve forests were selected to be included in this pilot program aimed at reducing forest fires by a combination of participatory forest management and technical forest fire containment measures.

The main axis of the intervention was the establishment of Forest Management Committees (FMC) in each of the twelve project forests. These FMCs consisted of inhabitants of the communities surrounding the forest, and were tasked to disseminate knowledge on forest management in their communities and to coordinate conservation efforts at the forest level. Most importantly, they were to raise community awareness about the adverse consequences of forest degradation and hence about the importance of reducing the number of forest fires started, as well as explain the different methods with which fires can be contained. As a coordinating body, the FMCs were to share tools and equipment (such as vehicles and communication devices) with the so-called Forest Management Groups

(FMGs)[2] who implement the activities at the community level, and to also organize the FMGs' forest management efforts. These efforts included constructing firebreaks (strips of cleared land in the forest to keep fires from spreading), managing the amount of combustible vegetation in the forest at the beginning of the dry season, and delimiting the forest borders (with signposts and cleared forest strips around forest borders). These forest management efforts primarily sought to improve the monitoring and containment of fires burning in the forests. The FIP program was scheduled to run from October 2014 to 2019, but in many of the project forests the program's rollout did not start before early 2015. As announcement effects may affect behavior (think of the possibility of setting more fires now to avoid the risk of not being able to use fire later), we retain October 2014 – the month in which the program was announced – as the program's starting date, even though the actual starting dates may differ between forests.

Of Burkina Faso's 77 forests with protected forest status, only twelve were to be included in the FIP program, for two reasons. First and foremost, the available budget was not sufficient to include all forests, but the government also explicitly viewed the intervention as a pilot of which the effectiveness was to be assessed. To determine which forests to enroll in the program, the government ranked all 77 protected forests based on criteria regarding forest characteristics as well as the perceived urgency of conservation, such as the frequency of forest fires, deforestation rates, carbon sequestration capacity, forest size, the agro-climatic zone they are located in, and the (perceived) availability of non-timber forest products (for the full list of selection criteria, see Appendix A). The government then selected twelve forests that ranked high on these criteria (see Figure 2). Because of the limited budget and the intention to learn, no large-scale interventions were implemented in the 65 non-project forests.

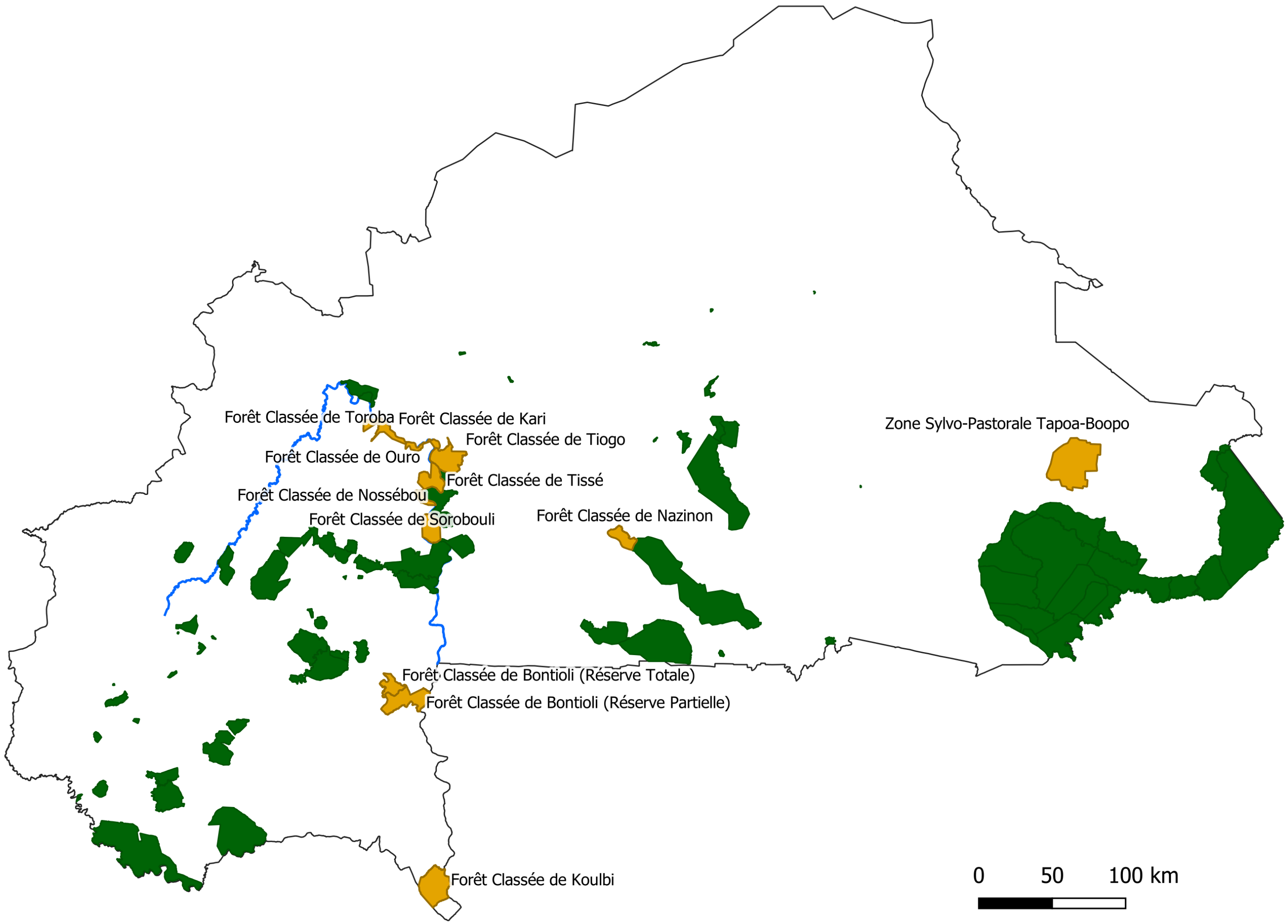
[2]Forest Management Groups (FMGs) were established in the reform of 1986 (Coulibaly-Lingani *et al.*, 2011) to improve the sustainable management of Burkina Faso's forest areas. Protected forests were partitioned into Forest Management Units (FMU) of between 20 and 40 km^2, each of which was to be managed by an FMG consisting of representatives of the nearby communities, including local leaders and volunteers. As evidenced by the still high rate of forest loss, these FMGs were not very effective in fostering forest conservation due to their inability to formulate effective plans and to protect forests because of poor organization at the community level, lack of knowledge of and/or of resources for forest management, and limited authority (Kalame *et al.*, 2009; Bouda *et al.*, 2011). The FMCs were thus installed by the government to address these issues.

The FIP program officially ran from 2014 to 2018, and it envisaged to reduce the incidence of forest fires by the establishment of clear(er) demarcation of the 12 protected forests (covering 284,000 hectares), paying local communities to construct 3500 km of fire breaks, organizing the formation of the FMCs and training the FMGs to improve their forest management skills (including the construction and maintenance of fire breaks and monitoring the forest for fires), and on sensitizing the local communities towards the importance of reducing the number of fires set and of better fire containment. The bulk of the budget (73% of $8.5 mln; AFDB (2013)) was to be spent on the construction of physical infrastructures (especially the fire breaks) and for establishment of the FMCs. The rest of the budget was to be spent on capacity building among the FMGs (22%) and on sensitization of the communities surrounding the forests (5%). However, the intensity of the program implementation was not constant over the 2014-2018 period. The bulk of the money was scheduled to be disbursed in 2015 (about 60%); because of the start-up problems, actual spending was largest in 2016, and the last disbursements took place 2019. The project was designed on the premise that wasteful forest fires (unnecessary fires started, but also "useful ones" ones that were started but were not kept under control) destroy an asset that is valuable for the local communities (also because they have usufruct, see Kambiré *et al.* (2016)), and hence the bulk of the investments were scheduled to be made in the early years of the project.

3 Empirical approach

We estimate the causal impact of the intervention on forest fire occurrences using the Synthetic Control Method (SCM) as developed by Abadie and Gardeazabal (2003) and Abadie *et al.* (2010). This method estimates the counterfactual outcome for each of the intervention units in the intervention period (i.e., each project forest's outcome had the intervention not been implemented) using a convex combination of the units that had not received the intervention (the remaining 65 of Burkina Faso's 77 protected forests); the estimate of the actual treatment effect is then the difference between the treated

Figure 2: Burkina Faso's 77 protected forests, and their status in the selection process.



Note: The orange forests are the twelve forests selected for the FIP program. The green forests were not selected, and hence form the so-called "donor forest pool" for the Synthetic Control Method. The river marked in blue in the western part of the country is the Mouhoun river.

unit's actual and counterfactual outcomes. More specifically, the estimated Treatment on the Treated effect for project forest i in intervention period t is $\alpha_{i,t>T_0} = Y_{i,t>T_0} - \sum_{j\in C} w_{i,j} Y_{j,t>T_0}$, where T_0 denotes the last period before the start of the intervention, $Y_{i,t>T_0}$ is the outcome of interest of forest i in period $t > T_0$, C is the set of non-treated forests, and $w_{i,j}$ (with $0 \leq w_{i,j} \leq 1$ and $\sum_{j\in C} w_{i,j} = 1$) is the weight assigned to the outcome of each of the non-treated units ($Y_{j,t>T_0};\ j \in C$).

The main challenge is thus find the set of weights $w_{i,j}$ that minimizes the differences between treatment unit i's actual and counterfactual outcomes in the pre-intervention period in terms of both variables that are predicted to affect the outcome variable of interest (in our case, forest size and forest precipitation rates affecting forest fire occurrences) and pre-intervention values of the outcome variable (in our case, forest fire occurrences); see Appendix B for a more detailed explanation of the process. Intuitively, if weights can be found such that the synthetic control (or the predicted counterfactual outcome, $\sum_{j\in C} w_{i,j} Y_{j,t}$) closely traces the outcomes of the treated unit ($Y_{i,t}$) in the pre-intervention period ($t \leq T_0$), it is also likely to provide an accurate estimate of the treated unit's counterfactual outcome in the post-intervention period.[3] Having estimated α_{it} for each of the project forests and using K to denote the set of project forests, the average treatment effect then equals $\alpha_{t>T_0} = \frac{1}{|K|} \sum_{i\in K} \alpha_{i,t>T_0}$.

We evaluate the likelihood of any results being false positives by using the placebo test approach proposed by Abadie *et al.* (2010) and Cavallo *et al.* (2013). Intuitively, the smaller the share of placebo estimates exceeding the estimated treatment effects, the more likely it is that the treatment was indeed effective, and hence this share p^{signif} can serve as the estimated treatment effect's pseudo-significance level. Because poor pre-intervention fits may result in inflated treatment estimates, we follow Abadie (2021) and scale the treatment estimates by the goodness of fit of their synthetic control in

[3] By not just fitting on pre-treatment forest fire outcomes but also on observable pre-treatment characteristics thought to be predictive of forest fire outcomes, the method reduces the likelihood that unobservable time-varying characteristics cause outcomes of the synthetic control unit to differ from those of the treated unit in the intervention periods. In other words, fitting on both pre-treatment outcomes and observable characteristics helps ensure that the estimated treatment effect is not affected by unobservables even if they systematically differ between treated and non-treated units before the start of the intervention.

the pre-intervention period; see equation (9) in Appendix B.[4] We calculate the pseudo-significance levels of our estimates using placebo tests on 5 million combinations of twelve of Burkina Faso's 65 non-project forests.

The Synthetic Control Method is better suited to estimate causal impacts for this particular study than other, more standard, methods, like difference-in-difference models with matching. Matching of the project forests to non-project forests is not feasible because neither the scores on each of the criteria nor the weights attached to each of these criteria are available; see Appendix A. And because selection into treatment was non-random, the parallel trend assumption needed for difference-in-difference methods is very likely to be violated.[5] Finally, the SCM has the added benefit that the method is able to estimate the per-period treatment impacts even if the number of project forests is relatively small. As such, the method is ideally suited to evaluate not just the average effectiveness of the intervention, but also the dynamics of the treatment effect.

4 Data and estimation procedure

We use satellite data from NASA's MODIS's Active Fire Product (Giglio *et al.*, 2016) (publicly accessible via Google Earth Engine; see Gorelick *et al.* (2017)) to construct a panel of monthly grid-cell data on forest fires for each of Burkina Faso's 77 protected forests in the period from January 2003 to December 2019. On each day, the MODIS collection identifies pixels that contain at least one actively burning fire based on thermal hotspot detections (Giglio *et al.*, 2016, 2018b). The collection has a resolution of 1 km^2

[4]For the SCM to work well, the evaluated forests (project, and placebo) need to lie within the convex hull of the set of all control forests. If not, the requirements that $0 \leq w_{i,j} \leq 1$ and $\sum_{j \in C} w_{i,j} = 1$ result in a poor fit in the pre-intervention period, and not correcting for the quality of pre-intervention fit would result in a higher rate of false positives. This is especially relevant for the placebo tests because, by definition, some of the placebo forests will lie outside the convex hull. We address this by scaling all treatment estimates by each synthetic control's Root Mean Squared Prediction Error (RMSPE) for the pre-intervention period (Abadie *et al.*, 2010; Abadie, 2021; Galiani and Quistorff, 2017). Intuitively, a poorer pre-treatment fit of the synthetic unit leads to a large RMSPE, which in turn increases the share of scaled placebo estimates that are larger than that of the treated unit (all else equal). See Appendix B for a more detailed explanation.

[5]As shown in Figure C1 in Appendix C, the average number of days a forest grid cell was detected to be on fire is similar in the project and non-project forests in the pre-intervention years, and so is their co-movement. However, these averages hide substantial differences between forests, so that a difference-in-differences approach is not feasible.

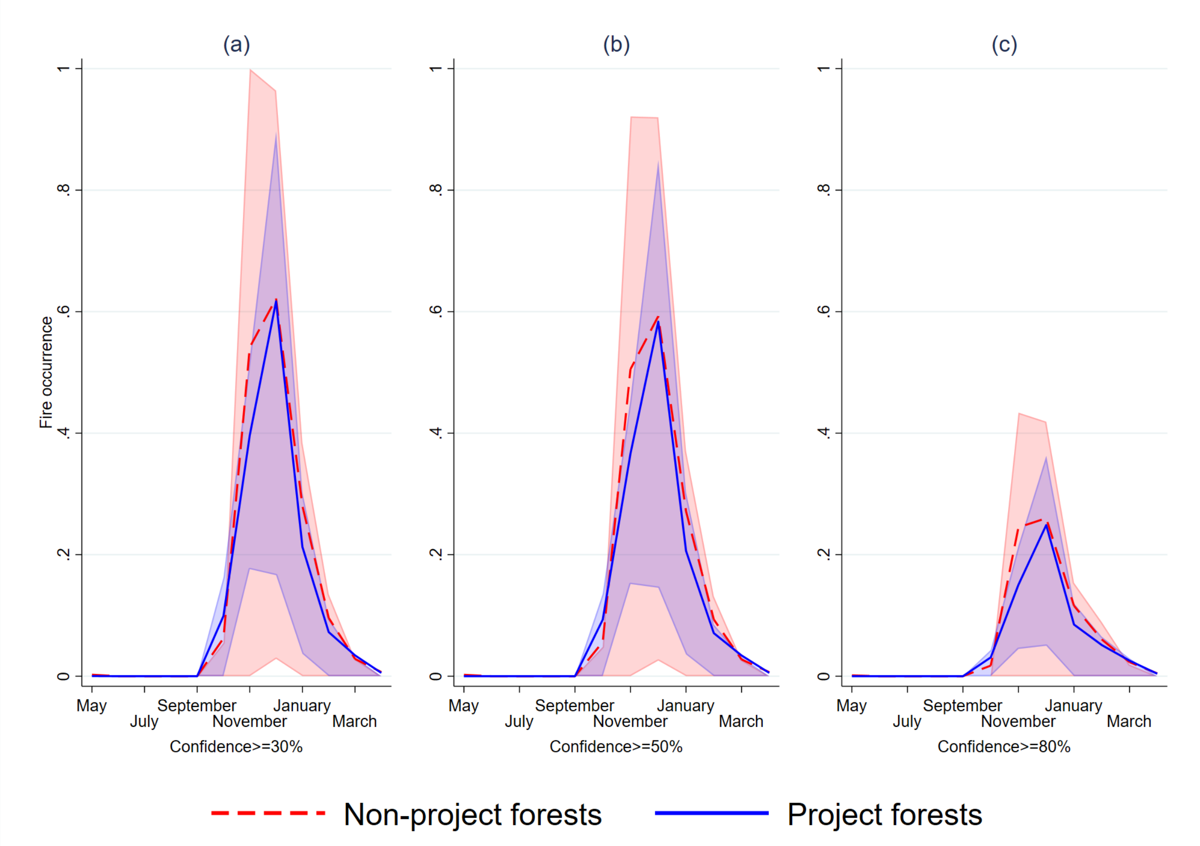
and provides global coverage every one or two days. Our main measure of forest fire occurrences is the number of days in a month on which a hotspot was detected on a forest grid cell, averaged over all grid cells in a forest. This measure thus simultaneously captures two dimensions of forest fires – the number of grid cells that were on fire in a month, but also the number of days each of the grid cells was on fire in that month. Following Hantson *et al.* (2013) We will refer to this measure as "forest fire occurrences", which is thus forest- and month-specific.

MODIS reports the data on hotspots detected with different levels of confidence (between 0 and 100%) for each detected fire. Figure 3 presents the pre-intervention fire occurrences, averaged over the pre-intervention period 2003-2013, for each month of the calendar year, using three different confidence thresholds – 30%, 50% and 80%. Consistent with Figure 1, the three panels of Figure 3 show that fires occur mostly in the dry season (November-February), and especially so in November and December, the first two post-harvest months. Forest fires are close to absent in May and June, at the beginning of the new agricultural season; fires thus seem not to be started very frequently to clear land. This is consistent with the observation by Kambiré *et al.* (2016, p. 5) that most of the land clearing occurs in the Sudanian zone of the country, whereas most of our treatment forests are located in the Sudano-Sahellian part of the country (Kambiré *et al.*, 2016, p.2)).

Regarding possible systematic differences between the project and non-project forests, the panels in Figure 3 also show that the mean fire occurrences are similar in the (then yet to be) treated project forests and in the 65 other non-project forests (depicted by the continuous and dashed lines, respectively). Also note that the variance around these means is quite substantial (as indicated by the blue- and red-shaded areas, representing the 25% and 75% percentiles). We focus our analyses on those fires that were detected with a minimum confidence of 30% – the threshold that MODIS identified as the cut-off for nominal-confidence levels. We test whether our results are robust to using the 50% confidence criterion, but we do not do so for the 80% level. The program was designed to not just improve forest fire containment but also reduce the number of forest fires started.

According to Liu *et al.* (2018) the 80% confidence level is too strict to pick up smaller fires in Sub-Saharan Africa – the ones that are started, but are subsequently well-contained; see also Devineau *et al.* (2010) and Schroeder *et al.* (2014).

Figure 3: Monthly fire occurrences in the project and non-project forests, averaged over the period 2003-2013, and for different levels of detection confidence.



Note: Average forest fire occurrences are depicted by the blue solid and red dashed lines. The top and bottom of the blue and red shaded areas capture the 25$^{\text{th}}$ and 75$^{\text{th}}$ percentile of fire occurrences between 2003 and 2013 for the project and non-project forests. The threshold confidence levels for detection are 30%, 50%, and 80% for panels (a), (b), and (c) respectively.

We decided to use MODIS's Active Fire Product, and not its Burned Area Product, for two reasons. First, the intervention that we study aimed to reduce the area burned via two mechanisms: (i) reducing the number of forest fires started (e.g., by raising awareness about the problems associated with forest fires), and (ii) preventing fires from running out of control (again by raising awareness, but also by constructing fire breaks). The burned area product is well able to detect the fires that ran out of control, but it is less able to detect forest fires that were started but subsequently properly contained (Giglio *et al.*, 2018a; Boschetti *et al.*, 2019; Liu *et al.*, 2018). Using the burned area product MODIS data base thus would thus not allow us to provide insight into why the intervention may have been effective – was it successful because it managed to reduce the number of forest fires started, or because it managed to induce the local population to better contain the fires they set? Second, the pattern of the shares of forest area affected in each of the months is very similar in both the Active Fire Product and in the Burned Area Product (albeit that the share is lower in the latter). That means that absent any systematic biases in either of these measurement instruments between project and non-project forests, the percentage difference between the outcomes in the project forests and in the non-project forests (as represented in the synthetic control) is likely to be the same for the area burned as for the hotspots. We also decided to not use the fire brightness in MODIS's Active Fire Product. This product is interesting, as the amount of damage a forest fire gives rise to is increasing in the intensity of the fire (Johnston *et al.*, 2017). The year-to-year brightness outcomes were similar between the project and non-project forests, both in terms of levels and in changes in over time. That means that, conditional on the number of fires started, the intensity is unlikely to have been affected.

Forest fire occurrences are thus our main indicator variable, but it should be noted that this measure may hide important differences. A forest fire occurrence of 0.2 days per month may mean that two of every ten grid cells in a forest experienced a forest fire on one day of the month, or that fire was detected on one out of ten grid cells on two days in that month. Which of the two is the correct interpretation of the observed forest fire occurrence is important because fires in the Sahelian savannah zone degrade forests primarily by damaging the seeds and destroying the canopy of young saplings (Zida *et al.*, 2007). That means that fires taking place on two different grid cells may be more damaging than a single fire affecting the same grid cell on two consecutive days. It is thus important to know not just (the changes in) forest fire occurrences, but also (the changes in) the share of forest grid cells that experienced one or more fires in a specific month. And if the program proves to have been successful, it would also be useful to know the mechanism via which the program managed to reduce forest fire occurrences. Was it effective because it resulted in a decrease in the number of fires started, or was it effective because were the fires better contained?

Table 1: Descriptive statistics of the averages over the pre-intervention period of the share of forest areas affected by fire, the number of fires started, and the number of grid cells burned per fire, in the two months of the year in which forest fires are most frequent – November and December.

Variable	(1) All forests Mean (SD)	(2) Project forests Mean (SD)	(3) Non-project forests Mean (SD)
November			
Share of unique forest grid cells affected by fire	0.303	0.262	0.311
	(0.305)	(0.179)	(0.322)
# forest fires started	11.172	8.568	11.653
	(19.296)	(7.558)	(20.717)
Number of square kilometers affected per fire	5.659	5.961	5.604
	(5.567)	(2.885)	(5.930)
December			
Share of unique forest grid cells affected by fire	0.365	0.330	0.371
	(0.302)	(0.263)	(0.308)
# forest fires started	11.682	12.492	11.533
	(16.102)	(15.212)	(16.266)
Number of square kilometers affected per fire	5.410	6.083	5.286
	(4.423)	(3.105)	(4.616)
N	847	132	715

Note: Means and standard deviations of the three additional forest fire measures of all forests are presented in Column (1); those for the program and non-program forests are presented in Columns (2) and (3), respectively. The first three rows show the summary statistics for fires in November, while the second three rows correspond to December fires. N indicates the total number of pre-treatment forest-year observations in the respective (sub)groups.

To answer these questions, we calculate three additional forest fire indicators that are also specific to each forest and for each month of the year. The first is the share of grid cells in a forest on which at least one forest fire was detected in the month under consideration. Our second additional indicator is the number of fires that were started in a month. We define a fire event as a contiguous set of grid cells on which fires were detected on one day of the month, whereas none of these grid cells were on fire on the previous day. The latter criterion makes sure that a forest fire that lasted for more than one day is coded as one fire event, and the contiguity criterion implies that we assume that fires on contiguous grid cells emanate from the same initial incident. And we can then also measure the spatial spread of the forest fires, our third additional indicator. We do so by calculating the average number of contiguous grid cells on which a hotspot was detected on the current but not on the previous day. This thus links the area affected to unique forest fire events, and hence avoids double counting. Table 1 shows that in November and December, when most fires were observed, fires were detected on 25-36%

of the forest grids average, the average number of fires ignited was about 11, and the average size of the fires was 5-6 km^2 before the program.

We use the Synthetic Control Method to estimate the FIP program's impact on each of the four forest fire measures: forest fire occurrences, the share of a forest grid cells having been on fire at least once, the number of forest fires started, and the extent to which forest fires spread out.[6] To apply the SCM, we use the `synth_runner` STATA package implemented by Galiani and Quistorff (2017). As forest characteristics that predict fire occurrences, we use the annual panel of 2006-2013 pre-treatment outcomes,the average annual precipitation in the forest before the treatment, and the size of the forest (measured at baseline, in 2013) to construct the synthetic control forest for each of the treatment forests. Although the relationship between each of these two forest characteristics and the actual forest fire occurrences is ex-ante ambiguous[7], including them in the weighting process will improve the fit, independent of the exact relationship.[8]

We have monthly data on forest fires, and this relatively high frequency poses a challenge to the SCM approach for two reasons. First, applying the SCM to high frequency and highly varying data may yield a synthetic control that gives relatively more weight to idiosyncratic shocks (i.e., to the number of forest fires in the pre-treatment period)

[6]While it would also be interesting to have information on the duration of forest fires, the MODIS data set does not contain the necessary information (Balboni *et al.*, 2021). We can approximate it, however, by taking the ratio of forest fire occurrences and the share of forest grid cells that were affected at least once during the month. This would provide an upper-bound estimate of the average duration of fires because it does not take into account whether grid cells were on fire on consecutive days or not. An analysis of this fifth (and imprecise) indicator would not yield any new insights because we already analyze the numerator and denominator separately.

[7]Forest size is expected to affect forest fire occurrences because of two reasons. First, man-made fires are more likely to be started on the forest fringe than in the interior (especially if agriculture is the main activity causing forest fires), so that larger forests may have, on average, fewer forest fires per grid cell. Alternatively, larger forests may have more fire occurrences because they are more difficult to monitor. And also the relationship between precipitation and forest fires is also ambiguous ex ante. In drier forests the vegetation is more prone to catching fire and the fire is likely to spread wider too. But then it may also be the case that agricultural activity is higher in areas with more precipitation, so that forest fires are more pronounced in forests with higher precipitation rates. Whichever of the two opposing mechanism is dominant for each of these two variables, including precipitation rates and forest size in the SCM's weighting process will improve the accuracy of the synthetic control.

[8]Ideally, one could also include socio-economic characteristics of the forest communities as predictors to improve the credibility of the synthetic control units. However, the 2014 Living Standard Measurement Survey we use was collected in a sample of communities in the country and it was not feasible for us to appropriately aggregate the responses to the forest level. We thus do no include these characteristics to the estimation of the program's impact but we will use them to study the robustness of the main impact and the heterogeneity of the program's impact at the subforest-level in Section 6.

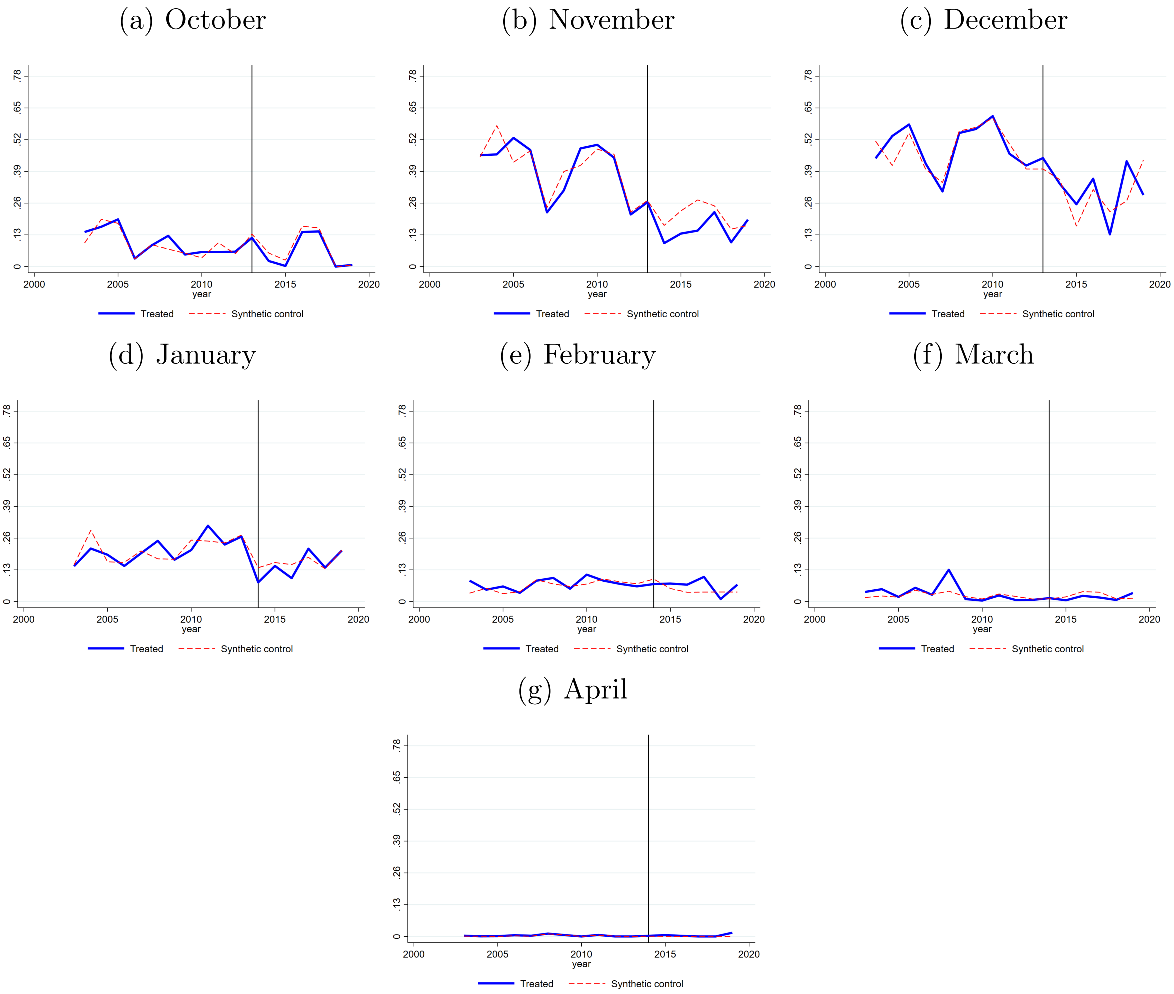
and relatively little weight to the predictors of the outcome variable (precipitation and forest size), leading to biased estimates of the counterfactual (Dube and Zipperer, 2015; Robbins *et al.*, 2017). Second, high variation in the data prevent the detection of small or even moderate effect sizes as these effects would be similar in size to the prediction errors in the pre-treatment outcomes due to imperfect fit (Abadie, 2021). We address these concerns by applying the SCM to the forest fire indicators for each of the seven months in the dry season (October-April), those months of the year in which forest fires (may) occur; see Figure 1. For instance, to estimate the impact of the forest fire prevention program on forest fires in November, we construct synthetic controls by using forest fire occurrences in the month of November as well as by using time-invariant forest characteristics (forest size and precipitation).

5 Overall impact of the FIP

5.1 Impact on monthly forest fire occurrences

We first test whether the FIP program was effective in reducing the number of days the average forest grid cell was on fire, for each of the seven months of the dry season (October through April). We present the results on forest fires with a detection confidence of 30% or better in Figure 4. In each of the seven panels of the figure the solid line represents the average of fire occurrences observed in the project forests, the dashed line reflects those in the corresponding synthetic controls, and the grey vertical line indicates the last observation prior to the start of the intervention (in October 2014). As is clear from this figure, the synthetic controls closely match the observed pre-intervention outcomes (i.e., in 2003-2013/2014) of the treatment forests in each of the seven months, and especially so in the last two to three years of the pre-intervention period. Indeed, the root mean squared prediction errors (RMSPEs) in the pre-intervention period are below 0.06 days per month in November, December, and January (when average fire occurrences are higher; see Figure 1), and below 0.03 days in March, April, May, and October (when fire occurrences tend to be less frequent); see also Table C1 in Appendix C. The synthetic controls we

Figure 4: Fire occurrences in the project forests in each of the seven dry months of the year, and the counterfactual outcome as derived using the Synthetic Control Method.



Note: The panels in this figure present the average number of days a fire was detected on a grid cell. The continuous blue lines depict the observed fire occurrences in project forests with at least 30% fire detection confidence, whereas the red dashed lines show the counterfactual outcomes as derived from the Synthetic Control Method. The last observation before the start of the program is indicated by the black vertical line. As the program was launched in October 2014, the last pre-intervention observation is for 2013 for the months of October to December, and for 2014 for January-April.

constructed do not only properly reproduce the pre-treatment outcomes averaged over all twelve treatment forests, but also those for each of the treatment forests separately (see Figure C2 in Appendix C). And this does not only hold for the case of forest fires that are detected with a 30% confidence level, but also when using the 50% criterion; see Figure C3 in Appendix C. Our results are thus robust to changes in forest fire detection confidence levels.

Regarding the effectiveness of the intervention in reducing the number of forest fire occurrences, Figure 4 shows that the post-intervention differences between the treatment forests and their synthetic controls tend to be small in all months except for, possibly, November – and then only for the first three or four years. This is confirmed by the SCM treatment estimates $\left(\alpha_t = \frac{1}{|K|}\sum_{i \in K} \alpha_{it}\right)$ presented in Table 2. Columns (1) and (2) of this table present the estimated impact on fire occurrences with a minimum detection confidence of 30% and 50%, respectively. Note that the associated p-values are presented in square brackets, and that those with $p < 0.10$ are printed in bold.

As already suggested by the graphical evidence presented in Figure 4, Table 2 documents that the FIP's intervention only managed to significantly reduce forest fires in November, the post-harvest month. In the first year of the intervention, the number of days the average grid cell was on fire in that month decreased by 0.073 compared to the synthetic control; see Column (1). While this effect is sizeable (as it is equal to a 43% reduction in forest fire occurrences), treatment effects were too divergent between the twelve treatment forests for this reduction to be significant. In the subsequent years the treatment effect increased from 0.073 (in 2014) to 0.126 (in 2016), after which it started to decline. And while the effects were still considerable in 2018 (with the average grid cell in the treatment forest experiencing 0.054 fewer days on fire than the synthetic control unit), they are also measured with less precision, and the effect is fully extinguished by 2019. A similar pattern emerges when using the 50% confidence criterion (see Column (2) of Table 2).

There are no discernible effects in any of the other months of the year except for March, where the program seems to have have resulted in a *reduction* in forest fire occurrences in 2017 and in an *increase* thereof in 2019. However, when using the 30% confidence criterion these impact are small (a decrease or increase of less than 0.025 days of fire on the average grid cell; see Column (1)), and virtually zero (and highly insignificant) when using the 50% criterion (in Column (2)). This suggests that the March effects were driven by smaller fires (that are detectable at the 30% confidence level, but not at the 50% level), but this is at odds with the general observation of land clearing fires tending to be large (to clear land for agriculture, or to make room for young shoots to be used as fodder for

Table 2: Estimated impacts of the forest fire prevention program on forest fire occurrences per calendar month, for the period 2014-2019.

		(1)	(2)
		Fire occurrences	
Month	Year	C30	C50
Oct	2014	–0.033	–0.027
		[0.116]	[0.178]
	2015	–0.024	–0.013
		[0.184]	[0.218]
	2016	–0.024	–0.035
		[0.776]	[0.727]
	2017	–0.014	–0.039
		[0.102]	[0.461]
	2018	0.000	–0.000
		[1.000]	[0.856]
	2019	0.001	–0.002
		[0.322]	[0.859]
Nov	2014	–0.073	–0.040
		[0.284]	[0.706]
	2015	-0.093***	-0.098***
		[0.002]	[0.004]
	2016	-0.126***	-0.115***
		[0.000]	[0.006]
	2017	-0.026***	-0.103**
		[0.003]	[0.020]
	2018	–0.054	–0.075
		[0.459]	[0.636]
	2019	0.023	0.015
		[0.212]	[0.298]
Dec	2014	–0.017	0.028
		[0.901]	[0.401]
	2015	0.090	0.067
		[0.165]	[0.199]
	2016	0.044	0.017
		[0.856]	[0.947]
	2017	–0.093	–0.085
		[0.241]	[0.277]
	2018	0.162	0.127
		[0.202]	[0.139]
	2019	–0.143	–0.121
		[0.384]	[0.370]
Jan	2015	–0.013	–0.016
		[0.994]	[0.878]
	2016	–0.056	–0.047
		[0.957]	[0.882]
	2017	0.036	0.035
		[0.641]	[0.612]
	2018	0.005	–0.000
		[0.731]	[0.837]
	2019	–0.001	–0.010
		[0.621]	[0.813]
Feb	2015	0.020	0.031
		[0.728]	[0.568]
	2016	0.031	0.027
		[0.568]	[0.690]
	2017	0.062	0.062
		[0.162]	[0.169]
	2018	–0.029	–0.044
		[0.239]	[0.201]
	2019	0.031	0.022
		[0.121]	[0.293]
Mar	2015	–0.014	–0.016
		[0.304]	[0.247]
	2016	–0.017	–0.012
		[0.442]	[0.881]
	2017	-0.022***	–0.002
		[0.000]	[0.854]
	2018	–0.005	–0.006
		[0.920]	[0.724]
	2019	0.021***	0.020
		[0.000]	[0.702]
Apr	2015	0.005	0.005
		[0.540]	[0.834]
	2016	0.002	0.002
		[0.707]	[0.238]
	2017	–0.001	–0.001
		[0.864]	[0.838]
	2018	–0.001	–0.001
		[0.586]	[0.254]
	2019	0.015	0.015
		[0.159]	[0.497]

Note: This table presents the average decrease in the number of days a forest grid cell is on fire, as a result of the FIP program. The effects of the program are estimated separately for each month on an annual-panel of forest level fire occurrence in the given month. We dropped two project forests in the estimation of December effects and one project forest in the estimation of April effects because pre-treatment fire occurrence in these forests fell outside the convex hull. Variables in the SCM process include past fire occurrences from 2006 onwards, the size of the forests, and the amount of annual precipitation before the FIP program. The p-values of the impact estimates (as derived from the inference tests) are presented in square brackets; those that are not "significant" at the 10% level have been grayed out. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

cattle; see Kambiré *et al.* (2016)). In any case, the March results are economically very small, and hence November is the only month in which the FIP program managed to change forest fire occurrences.

Based on Figure 4 and Table 2, we draw two conclusions. First, the program was not effective in reducing forest fire occurrences throughout the year; it only managed to reduce forest fire occurrences in the first month after the harvest has been completed – November. Second, even for the November fires the program's impact was short-lived, with the impact having vanished within four years.

We verify the robustness of the above conclusions using five different tests; see Table C2 and Figure C4 in Appendix C. First, even though the inference method we use already corrects for less-than-perfect fit between the project forests' outcomes and their synthetic controls in the pre-intervention period (see Appendix B), we re-estimate the project's impact using the Augmented SCM method Ben-Michael *et al.* (2021) to see whether our estimates are biased by less-than-perfect fits. The ASCM differs from the standard Synthetic Control Method as it allows extrapolation (i.e., it does not impose the constraint that $0 \leq w_{i,j} \leq 1$ (see equation (5) in Appendix B). Comparing the pre-intervention fit of the ASCM (see Figure C4b in Appendix C) to that of the Synthetic Control Method (see Figure C4a), we see that the ASCM produces a near-perfect fit in the pre-intervention period, but also that it yields a very similar estimate of the overall impact. This is confirmed by column (2) of Table C2 in Appendix C, which yields November impact estimates that are very similar to those presented in column (1).[9] Second, we implement the backdating test (Abadie, 2021) to test how well the SCM is able to "predict out of sample". The essence of the method is to reduce the length of the training period (the number of observations that is used to construct the synthetic control) by omitting some of the last pre-intervention years (in our case, 2013, or 2012 and 2013), and see whether the synthetic controls thus produced are able to accurately predict the observed outcomes of the project forest in these last years prior to the start of the intervention.

[9] This holds for the average treatment effects, but also those for each individual forests – even for those with the worst pre-treatment fits; Nazinon, Bontioli Reserve Totale and Bontioli Reserve Partielle). Results are available upon request.

Rather than using 2005-2014 as the training period, we construct synthetic controls using 2005-2012 and also 2005-2011. As is clear from Figure C4c and Figure C4d in Appendix C, the synthetic controls constructed using the shorter training periods closely predict the actual observed project forests' outcomes in the last pre-intervention years (2013 in Figure C4c and 2012 and 2013 in Figure C4d). The synthetic control estimator is thus able to reproduce outcomes for the treated unit in the absence of the intervention; it also implies that there is little evidence for any biases due to anticipation effects. Also note that the impact estimates for the intervention period itself (2014-2019) are very similar independent of the length of the training period. This holds not just for the impact estimates in the early years (2014 and 2015), but also for the later years (2018 and 2019), independent of whether the SCM is tasked to predict 5-6 years into the future (as in the core result) or 7-8 years into the future (if we reduce the training period by two years, thus extending the prediction period by two years). Third, we test whether our treatment estimates are affected by spillovers between project and non-project forests. We do so by re-estimating the SCM impacts while excluding all non-treatment forests that are contiguous to treatment forests. As shown in column (5) of Table C2 and Figure C4e in Appendix C. The results are very similar, and hence the SUTVA assumption is not likely to have been violated (Pearl, 2009).

Fourth, we test whether the results may be driven by the inclusion or exclusion of specific control forests; see also Abadie (2021). If the size of the treatment estimates is sensitive to the exclusion of a non-project forest from the set of donor forests, the results may be less reliable than previously thought – as they may have been driven by large idiosyncratic shocks on the outcome of the excluded donor forest. We thus exclude, one by one, each of the non-project forests that received a positive weight in our main analysis , and subsequently re-estimate the treatment estimates using the synthetic controls thus derived. As shown in Figure C4f and column (6) of Table C2 in Appendix C, we find that treatment estimates are again very similar to those obtained using the full set of non-project forests. The treatment estimates thus remain by and large unaffected in each of these three robustness tests. Fifth, we test whether our results are robust to also use socio-economic determinants in the SCM's fitting process. This robustness test is not uncontestable, because the sampling process of the data that we use (the Living Standard Measurement Survey data set; see the list in Section 6) was designed for the surveyed villages to be representative for the country – not for the villages surrounding each of the forests. When using the average of each of the socio-economic characteristics in the villages surrounding each forest, the results are again very similar; see Figure C4g and also Column (7) in Table C2 in Appendix C.

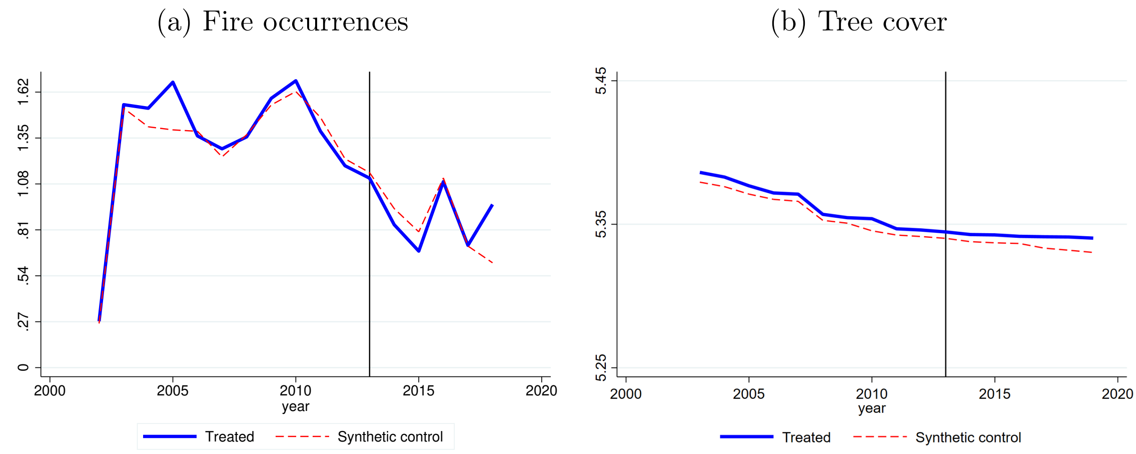
5.2 Impact on annual tree cover and forest fire occurrences in the agricultural year

Having confirmed the robustness of the project's impact on November fire occurrences, we now study whether the sizeable yet relatively short-lived impact on November forest fires resulted in overall improved forest conservation outcomes. We do so in two steps. First, we use the SCM to test whether the decrease in the November forest fire occurrences resulted in a reduction in annual forest fire occurrences. Second, we test whether there is an impact on tree cover – again using the SCM.

Regarding measuring the impact of the project on annual forest fire occurrences, we counted the total number of days in an agricultural season (June to April) on which the average forest grid was detected to be on fire. To estimate the impact on tree cover, we use the annual forest level tree cover data from the Global Forest Change dataset of Hansen *et al.* (2013), which combines Landsat images and MODIS data with ground-truth verification (Hansen *et al.*, 2010). We use this dataset to calculate the share of forest area covered by tree canopy in each forest-year, a standard measure of tree coverage (Sims and Alix-Garcia, 2017; Balboni *et al.*, 2021). Net deforestation decreases this measure, while net forest growth increases it. The results of the associated SCM analyses are presented in Figure 5 and in Table 3.

As can be inferred from Panel (a) in Figure 5, the project reduced the year-by-year forest fire occurrences, but the impact is small and declining. This is confirmed by the results presented in Column (1) of Table 3. Although the point estimates start out negative, they gradually revert to zero, and none of the estimates are significant ($p > 0.192$ for all years). This (insignificant and) "vanishing" impact is consistent with the temporal pattern of the budget expenditures that was characterized by strong "frontloading", but the results also make clear that the reduction in forest fire occurrences in November (as observed in the period 2014-2017) was not sufficiently large to result in a significant decrease in overall forest fire occurrences. And the shrinking treatment effect also implies that there is no evidence for possible delayed effects (such as investments needing time to become fully effective).

Figure 5: Fire occurrences in the agricultural season and tree coverage in the project forests, and the counterfactual outcomes as derived from the Synthetic Control Method.



Note: The left panel in the figure presents the total fire occurrences in the agricultural season, while the right panel presents tree coverage. In both panels, the blue continuous lines depict the observed outcomes in project forests, whereas the red dashed lines show the counterfactual outcome as derived from the Synthetic Control Method. The last observation before the start of the program is indicated by the black vertical line. As the program was launched in October 2014, the last pre-intervention observation is for 2013.

The outcomes of the analysis of the project's impact on vegetation cover are presented in Panel (b) of Figure 5 and in Column (2) of Table 3. Two features of these outcomes are most notable. First, while average tree cover in the treatment forests is systematically larger than that in their synthetic controls, the differences are small in the pre-intervention period (about 0.01 percentage points on average), and the synthetic control also closely traces the observed tree cover in the treatment forests over time. Second, the post-intervention difference continues to be small, although the figure also seems to suggest that the reduction in tree cover is slowed down (if not stopped) in the treated forests

Table 3: Estimated average treatment effect on fire occurrences in the agricultural season and tree coverage.

	(1) Fire occurrence in the agricultural season	(2) Tree coverage
2014	-0.095	0.005
	[0.710]	[0.939]
2015	-0.113	0.006
	[0.442]	[0.940]
2016	-0.002	0.005
	[0.954]	[0.941]
2017	-0.004	0.008
	[0.720]	[0.922]
2018	0.343	0.009
	[0.192]	[0.922]
2019		0.010
		[0.930]

Note: Estimates are based on the synthetic control method. Column (1) presents the estimated impact of program on total fire occurrences, whereas Column (2) presents the impact on tree coverage. The last pre-treatment year in annual outcomes is 2014. We do not consider 2014 to be in the treatment period in these analyses because there are only three months from October 2014. P-values from the placebo analyses, in which treatment effects are standardized by pre-treatment RMSPE, are in brackets. Average fire spread is measured in km^2. *** p<.01, ** p<.05, * p<.1

whereas the decline in the synthetic control seems to continue. Indeed, Column (2) of Table 3 shows that while none of the point estimates are significantly different, the differences in tree cover between the treated forests and their synthetic controls gradually increases over time, from about 0.1 percentage points in 2015 to about 0.2 percentage points in 2020.[10] While the slow-down in the loss of forest is interesting, it is too small to be of economic importance.

The results in Figure 5 and Table 3 thus confirm that the reduction in the November forest fire occurrences was not sufficiently large for the overall decrease to be significant. Even though the project was designed on the premise that wasteful forest fires would destroy an asset that is valuable to the local communities (as discussed in Section 2.2),

[10] As is clear from Figure 5b forest cover decreased not just in the project forests, but also in the non-project forests (and then especially so over the period 2004–2010. In the period 2014–2019 the difference in the share of vegetation cover is somewhat larger, but remains small in absolute terms. As already stated on Page 24, this is not likely due to technical and/or informational spillovers having to non-project forests attenuated the project's impact.

strengthening the local institutions and infrastructural investments did not result in a permanent reduction in forest fires.

5.3 Mechanism behind the reduction in November forest fire occurrences

Overall, the forest fire prevention program was thus not very effective. It managed to reduced fire occurrences in just one month of the dry season (November, at the end of the agricultural cycle) and for just a limited number of years (between 2014 and 2017), and the program also did not result in an increase in forest cover. Still, uncovering the mechanisms via which the November forest fires occurrences were reduced is of interest. Has the reduction in November forestthat farmers were the main agents of change), or in the forests' interior (suggesting it was fires been the result of a reduction in the number of fires started (e.g., due to improved community awareness), or because of improved forest fire containment? And was the largest reduction achieved in the forest fringe (suggesting that farmers were the main agents of change), or in the forests' interior (suggesting it was the livestock herders and hunters that changed their use of forest fires)? We now turn to addressing these issues.

The timing of the impact – in November, when the bulk of the harvesting just has been completed – already suggests that the intervention was particularly effective in reducing agricultural fires. Farmers typically burn the crops residues on agricultural plots, so we would expect large reductions in fire occurrences at the forest fringe. This is corroborated by Table 4, which shows that most of the November forest fire reductions took place within a 1 to 2 kilometer band away from the forest fringe. Comparing the size of the impact estimates in that table to those in Column (1) of Table 2, we can infer that the reduction has been largest on the forest fringe, and that the reductions in the forest interior are smaller and less likely to be significant; see also Figure C5 in Appendix C. So while a change in agricultural practices is likely to have been the most important driver of the decrease in the November forest fire occurrences, other economic activities seem to have been affected (much) less.

Table 4: Estimated average treatment effect on fire occurrences excluding the forest fringe

Month	Year	(1) No thresh.	(2) Fire occurrences 1 km thresh.	(3) 2 km thresh.
	2014	–0.073	–0.053	–0.010
		[0.284]	[0.285]	[0.971]
	2015	–0.093***	–0.174***	–0.081
		[0.002]	[0.001]	[0.116]
	2016	–0.126***	–0.158**	–0.106
		[0.000]	[0.023]	[0.139]
Nov.	2017	–0.026***	–0.025	–0.039
		[0.003]	[0.424]	[0.474]
	2018	–0.054	–0.118	–0.060
		[0.459]	[0.318]	[0.698]
	2019	0.023	–0.011	0.048
		[0.212]	[0.276]	[0.955]

Note: Estimates are based on the synthetic control method using forest-month level data. We use fires with 30% or better confidence to construct the outcomes. Treatment start from October 2014. P-values from the placebo analyses, in which treatment effects are standardized by pre-treatment RMSPE, are in brackets. Average fire spread is measured in km^2. *** p<.01, ** p<.05, * p<.1

In Table 5 we present the program's estimated impact on the share of grid cells that were burned at least once in November (see Column 1 of Table 5), and we also separately estimate the intervention's impact on the number of forest fires started, and their geographical spread (see Columns 2 and 3 of that table, respectively); see Figure C6 in C for graphical illustrations of these treatment effects. Comparing the Column (1) results in Tables 2 and 5, the FIP intervention was about as effective in reducing the total share of grid cells affected by fire in the post-harvesting month in the period 2015-2017 as it was in reducing overall forest fire occurrences. Indeed, the percentage-point reduction in the share of grid cells affected by fire in the 2015-2017 November months ranged between 8.2% and 10.1%. From Columns (2) and (3) of Table 5 we can infer that the main reason why the intervention was effective. As shown in Column (2) of Table 5, fewer fires were started throughout the post-intervention period, but these reductions are not significant in any of the post-intervention years except for 2016. Column (3), however, shows that the intervention resulted in a reduced spread of forest fires over neighboring grid cells in both 2016 and 2017: the 2.10 km^2 and 1.83 km^2 decreases in the size of fire events translate into a 38% and 29% decrease, respectively. We thus find that improved forest fire containment was a key in reductions documented in Table 2.

The program may have also been effective in reducing the number of fires started, but we cannot statistically distinguish the impacts from zero. Regarding 2018, note that the estimated impact on the number of fires started (Column (2)) is negative and sizeable, but it is not statistically significant. This impact is counteracted by a significant rebound in the geographical spread of the fires in the same year (Column (3)). Together these two opposing effects lead to a loss in the program's effectiveness in reducing November fire occurrences in the same year (see November month in Table 2).

Table 5: Impact of the intervention on the share of forests affected by fire in November in each of the post-intervention years, as well as on the number and size of the forest fires.

Month	Year	(1) Share of fire affected grids in November	(2) # of ignitions	(3) Avg. fire spread
	2014	–0.060	–0.867	–0.295
		[0.470]	[0.745]	[0.678]
	2015	–0.082**	–0.471	0.331
		[0.035]	[0.416]	[0.971]
	2016	–0.086**	–1.324**	–2.108**
		[0.014]	[0.043]	[0.032]
Nov.	2017	–0.101*	–0.445	–1.829***
		[0.071]	[0.136]	[0.000]
	2018	–0.059	–1.237	1.730***
		[0.373]	[0.268]	[0.002]
	2019	–0.006	–0.410	–0.481
		[0.305]	[0.452]	[0.364]

Note: Estimates obtained using the Synthetic Control Method, using the 30% confidence criterion; p-values are presented in parentheses. or better confidence to construct the outcomes. Treatment start from October 2014. P-values from the placebo analyses, in which treatment effects are standardized by pre-treatment RMSPe, are in brackets. Average fire spread is measured in km^2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Together our results on better fire containment and no reduction in the number of forest fires started complement the findings of Edwards *et al.* (2020a). These authors evaluate the effect of a payment for ecosystem service program in Indonesia, in which villages were offered a large cash transfer if they did not set any fires within their villages' boundaries. Although payments induced villagers to better monitor their peers and to prevent them from setting fires, Edwards *et al.* (2020a) find that payments did not decrease the number of villages with one or more fires, and tree coverage did not increase either. Our findings suggest that forest conservation programs can be effective in increasing villagers' fire prevention effort and in containing fires, but not so much in preventing fire setting and limiting the subsequent damages. Even so, it is yet to be understood why better fire containment may not improve tree cover neither in Indonesia or Burkina Faso.

6 The role of socio-economic characteristics in driving (the reduction in) forest fire occurrences

Section 5 showed that the forest fire prevention program managed to reduced fire occurrences in just one month of the dry season and for just a limited number of years, and also that the program did not result in an increase in forest cover. These overall effects may hide important differences between (sub-) forests – see Figure C2 in Appendix C. Having established that the FIP program was effective in reducing end-of-the-agricultural cycle forest fires, we test whether the program was equally effective in all forest areas, or whether there are marked differences in the response to the program by the communities surrounding the forests. Differences in the number of inhabitants in the forest communities may determine the number of volunteers in the local Forest Management Groups who construct firebreaks and contain burning fires. Wealthier communities may also be more willing to forego income from charcoal production and to abstain from burning forest, and they may also be better prepared to contain agricultural clearing fires on their plots. Also, the program may be more effective in preventing agricultural fires from spreading to remote forest areas because Forest Management Groups can better extinguish and/or contain these fires before they reach the forests.

We study differences in the program's impact using responses from the 2014 Living Standards Measurement Study (LSMS) to characterize villages surrounding the forests. Survey responses were elicited between January and March of 2014 before the program was implemented. In order to link the fire occurrences to the survey responses, we assign each forest grid cell to the "sphere of influence" of the geographically most proximate village in the survey (see Appendix D for details about the matching process) and define

Table 6: Characteristics of the villages surrounding project and non-project subforests.

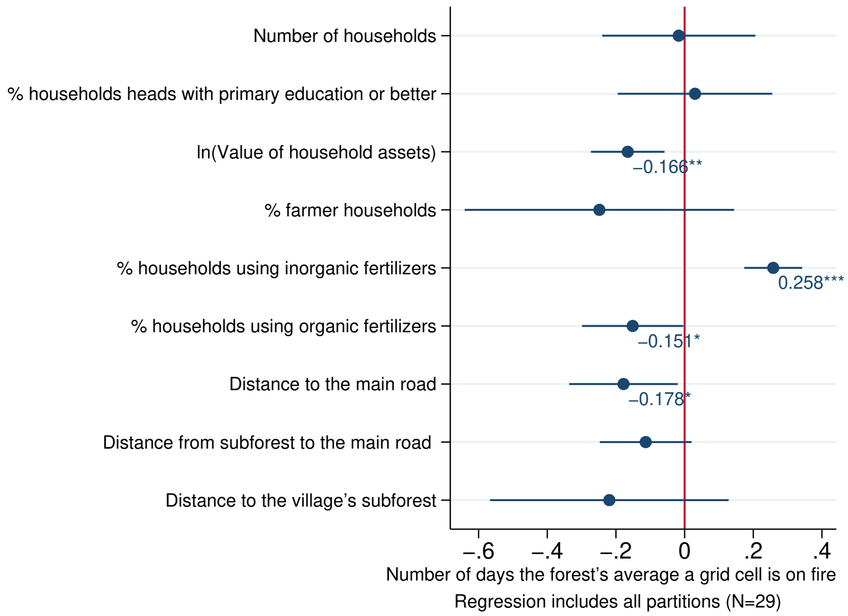
Variable	(1) Overall village characteristics Mean (SD)	(2) Villages surrounding project forests Mean (SD)	(3) Villages surrounding non-project forests Mean (SD)
Number of households	173.413	177.517	172.430
	(83.996)	(63.819)	(88.346)
Share of farmer households	0.980	0.981	0.979
	(0.072)	(0.045)	(0.077)
Share of households using organic fertilizers	0.552	0.479	0.569
	(0.269)	(0.301)	(0.259)
Share of households using inorganic fertilizers	0.582	0.595	0.579
	(0.297)	(0.316)	(0.294)
Share of households heads with primary education or better	0.149	0.118	0.156
	(0.150)	(0.101)	(0.159)
Value of household assets (FCFA, in natural logarithms)	12.445	12.060	12.537
	(0.987)	(1.014)	(0.962)
Distance to the main road (in km)	2.792	3.422	2.641
	(1.171)	(1.280)	(1.096)
Distance to the village's subforest (in km)	1.573	1.055	1.697
	(1.422)	(0.640)	(1.528)
Distance from village's subforest to the main road (in km)	8.977	9.286	8.903
	(6.293)	(3.867)	(6.756)
N	150	29	121

the set of grid cells in the same sphere as a "subforest". By doing so, we assume that economic activities in a specific forest location are undertaken by inhabitants of the nearest village. We then calculate fire occurrences at the subforest level and relate the observed fire occurrences to the characteristics of the associated village.[11]

Table 6 presents the socio-economic characteristics of the villages surrounding all 77 protected forests in Burkina Faso, as well as those of the subsamples of project and non-project forests. As shown in Column (1), the average village consists of about 175 households, nearly all households engage in agriculture, about half of them use inorganic fertilizers and the same holds for the use of organic fertilizers. The level of education is fairly low, household assets amount to FCFA 270,000 (or about US$ 400), the average distance from the village to the nearest protected forest is about 1.5 kilometers, and the

[11] As already stated in regard to the fifth robustness test in Section 5.1, aggregating the village level characteristics to the (sub-) forest level is not straightforward. The LSMS stratification process was designed to make sure the outcomes were representative for Burkina Faso; not all hamlets and villages surrounding the forests are included in the 2014 data set. And while we know the weight the sample locations receive in the national analysis, we have no information on how representative they are of the villages surrounding each forest. Not having this information is less problematic at the subforest level than at the forest level, because measurement errors are more likely to cancel with 29 units than with 12.

Figure 6: The relationship between the average rate of November fire occurrences in subforests and the socio-economic characteristics in the neighboring villages.



Note: This figure presents the coefficients from regressing average pre-treatment November forest fire occurrences in the treatment forests' 29 subforests on the neighboring communities' standardized socio-economic characteristics, in the pre-intervention period. The dots represent the point estimates, and the horizontal lines represent the 90% confidence intervals. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

average distance from a village to the main road is about 3 kilometers. More importantly, comparing Columns (2) and (3) of Table 6 we see that differences between the project and non-project villages tend to be small.

We implement two types of village-level analyses. First, we explore whether the characteristics listed in Table 6 are correlated with the subforest-level November forest fire occurrences before 2014. We do so by regressing the average of November fire occurrences before the treatment on a set of village characteristics. The results of the cross-sectional OLS regression are shown in Figure 6. We find that forest fires tend to occur more frequently the poorer the village, the more intensively it makes use of chemical fertilizers and the less it relies on organic fertilizers. And we also find that forest fires tend to occur more frequently in subforests of villages that are better connected to the road network. These results are in line with the findings of Bandiera *et al.* (2017), Balboni *et al.* (2021) and Oliveira *et al.* (2007), who also document the relevance of agricultural dependence and access to markets as drivers of forest fires.

Second, we test for heterogeneous treatment effects, to explore whether the FIP intervention proved to be more effective in changing the behavior of some villages than of others. We use the SCM to estimate $\alpha_{srt}^{\text{November}}$, the subforest-specific treatment effect for subforest s in region r in post-intervention year t ($t > T_0$). We include all sub-forest treatment impacts in the analysis, independent of whether they are significant.[12] To construct synthetic controls for each of the treated subforest we not only fitted the weights on pre-intervention outcomes, forest size and precipitation data (as we did in Section 5), but also on all village characteristics as presented in Table 6. After having estimated the treatment effects for all 29 project subforests between 2014 and 2019, we regress $\alpha_{srt}^{\text{November}}$ on all village characteristics from the LSMS survey and pre-treatment average of November fire occurrences using the following random effects panel model:

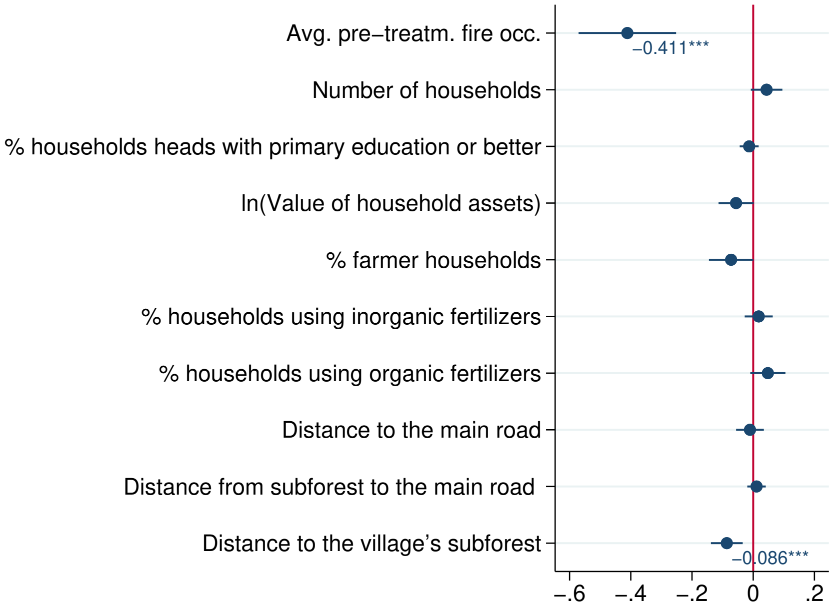
$$\hat{\alpha}_{srt}^{\text{November}} = \mu + \zeta \bar{Y_{sr}}^{Nov,t\leq T_0} + H_s'\Gamma + \nu_s + \delta_r + \delta_t + \varepsilon_{srt}. \tag{1}$$

Here, $\bar{Y_{sr}}^{Nov,t\leq T_0}$ is the average pre-intervention level of November forest fire occurrences in subforest s in region r, H_s denotes the set of normalized (and time-invariant) characteristics of the village matched with subforest s, ν_s is the subforest random effect, δ_r and δ_t are the region and time fixed effects, and ε_{srt} is the idiosyncratic error term clustered at the forest level. The results of this analysis are presented in Figure 7.

As shown in Figure 7, we find a stronger response to the intervention in subforests that tend to have more frequent forest fires at baseline. The heterogeneous treatment estimate of -0.411 implies that, all else equal, subforests that faced 0.1 more days of pre-treatment fire occurrences per month experience an additional treatment-induced reduction in forest fires of 0.04 days for the average grid cell. Although this effect may seem small, it is relatively large compared to the average treatment estimate of between -0.08 to -0.14 days per month; see Table 2. We also find that larger distances between the subforest and the corresponding village is associated with stronger reductions in forest

[12] Insignificant impacts may be taken at face-value, they can be interpreted as (and hence reset equal to) zero, or they can be dropped from the analysis. Because the last two options are sensitive to the choice of the significance threshold, we decided to do the first – we included all impacts in the analysis, even those which are insignificant. After all, insignificant results tend to be smaller, and hence including them (potentially) provides useful information about the size of the impact.

Figure 7: Estimates of the heterogeneous treatment effects on November forest fire occurrences for the various socio-economic characteristics of each of the subforests' neighboring villages.



Note: This figure presents the heterogeneous treatment effects, estimated at the subforest level, for each of the socio-economic characteristics of the adjacent village. Village characteristics in the regression are standardized by their means and standard deviations; the regression controls for the pre-intervention subforest fire occurrences as well as for region and year fixed effects. The dots in this figure represent the point estimates, with point estimate values depicted adjacently for those coefficients that are significant at the 10 percent, or better; the horizontal bars depict the 90% confidence intervals. Standard errors are clustered at the forest level. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

fire occurrences. The treatment-induced reduction in the number of forest fire occurrences is 0.08 days larger for villages that are one standard deviation (about 6 km) farther away from their subforest. Apart from baseline levels of forest fires and the distance between subforests and villages, none of the other covariates are found to affect the intervention's overall effectiveness.

The inference tests we used for the regression analyses presented in Figure 7 are based on standard z-tests. Because we cluster the standard error at the forest level, we only have 12clusters, and hence the assumption about the distribution's asymptotic properties may lead to standard errors that are artificially small. Calculating the significance levels of baseline forest fires and of the distance between subforests and villages using the Wild Cluster Bootstrap method (see MacKinnon *et al.* (2023)) increases the p-values from 0.001 to 0.20 percent and from 0.06 to 0.10, respectively. Our results on these heterogeneous impact effects are thus not robust to this alternative inference method, and hence they should be taken with caution. However, we also think that they are sufficiently large to be of economic interest, and hence are an important topic for future work.

7 Conclusions

In this paper we evaluated the impact of a forest fire prevention program in twelve of Burkina Faso's protected forests. Forest fires cause environmental damages in the form of both the increased emissions of greenhouse gasses and biodiversity loss. Forest fires are particularly damaging in arid regions because low precipitation results in fires spreading wider and vegetation regeneration being slower, resulting in forest fragmentation and degradation. Most of the forest fires in Burkina Faso are anthropogenic, with fire being used in economic activities such as agriculture (with the ashes acting as a natural fertilizer), cattle herding (to produce new shoots for grazing), hunting (to drive out game), or charcoal production. Reducing the frequency and size of man-made forest fires is thus essential for sustainable development, but evidence on effective fire reducing forest conservation policies is scarce.

The program we evaluate aimed to raise community awareness about the importance of preventing and mitigating forest fires, and to provide both the tools and knowledge necessary for forest fire containment. We use satellite images to monitor forest fire occurrences in each of Burkina Faso's 77 protected forests, for the period 2004-2019. We thus observe the frequency of forest fires in the intervention forests, and we use the Synthetic Control Method (Abadie *et al.*, 2010) to estimate each treatment forest's counterfactual outcome by assigning proper weights to each of the non-treatment forests.

We show that overall the program was not very effective in reducing forest fires. We find that the program reduced forest fires at the end of the agricultural season (in November) – after harvest, when farmers tend to use fire to burn the crop residue on their land. We do not detect any effects for any of the other months in the dry season, and also the vegetation cover was not significantly improved either. Analyzing the forest fires' geographical spread suggests that the program was effective in reducing forest fires especially by means of better fire containment. The number of days a fire is detected on the average grid cell of treatment forests was reduced by 35% between 2014 and 2017, and we also find that the project's effectiveness tends to be stronger the more prevalent were the forest fires before the beginning of the program. Despite the initial success, the project's impact became statistically insignificant from 2018 onwards – even though the new institutions aimed at improving forest management, like the Forest Management Committees, were still in place. The project's impact was therefore quite temporary, and it also failed to significantly reduce the number of forest fires in any of the other months in the dry season. We also find that the decrease in November forest fire occurrences did not result in improved overall forest conservation outcomes – neither in terms of annual forest fire occurrences, nor in terms of an improvement of tree cover.

We thus conclude that just mitigating the geographical spread of post-harvest fires is not enough to substantially improve forest conservation, and that additional measures are needed to reduce not only the number of agriculture-related forest fires, but also the frequency and geographical spread of the fires emanating from other non-agricultural economic activities. These insights are of obvious importance for the design of forest conservation policies in Burkina Faso, but probably for other countries and regions as well. The project itself if quite standard for forest conservation, as it mainly consisted of technical assistance and local community involvement; as such, it is a typical Community Forest Management approach (for an overview, see for example Pelletier *et al.* (2016). The impact of these programs typically differ depending on the region it is implemented – because of differences in climatological circumstances, but also because of differences in the type and number of actors involved; see Burivalova *et al.* (2019); Di Girolami *et al.* (2023). Still, deforestation is a major concern in arid Sub-Saharan Africa (especially because of (the threat of) desertification), and the causes of forest fires are very similar throughout the region (Rudel, 2013). As such, the external validity is likely to be high for arid Sub-Saharan Africa, and maybe for other more arid regions as well (e.g. Northern India; see Jack *et al.* (2022)).

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A Selection criteria for the forests to be included in the program

The selection process of forests into the FIP program consisted of two main steps. In the first, the government narrowed down the number of forests from 77 to 23 following the seven selection criteria on the perceived urgency of conservation below:

- Capacity in terms of carbon sequestration of the forest in relation to the productivity
- Level of CO_2 emission by wildfire
- Current level of destocking or export of carbon (forest clearing, excessive cutting of firewood, etc.)
- Level of the ecosystems degradation/anthropisation
- Opportunities to take stock of anterior interventions in the forests
- Security level (elimination criterion)
- Main factor of deforestation/degradation

The second step of the selection process consisted of the government narrowing down the number of forests from 23 to 12 on the basis of a set of eight criteria:

- Forest must have or must be designing a development and management plan
- Opportunity to further develop existing resources (e.g. non-timber products of vegetal and animal origins)
- Spatial span (large forest must be privileged)
- Level of the ecosystems degradation/anthropisation
- How management of common forest areas is allocated (inter-communities and inter-regions)
- Whether the forest is representative in terms deforestation dynamics
- Presence and activity of professional organizations
- Risk level of activating safeguard policies when interventions are done in the forest.

B Estimation of the synthetic control outcomes

The Synthetic Control Method can be summarized as follows. Let us use K to denote the set of treated units that were selected into the intervention, C to denote the set of non-treated units, and $|C|$ and $|K|$ to denote the number of units in each. Suppose further that we observe all units for T periods (indexed $t = 1, \ldots, T$), and that the intervention started in period T_0+1 ($1 \leq T_0 < T$). Finally, let us denote the observed outcome of unit i in period t by $Y_{i,t}$, and the value of an observable characteristic of the same unit that is expected to affect this outcome by z_{li} (indexed $l = 1, \ldots, L$). Then the SCM constructs a synthetic control unit for each treated units $k \in K$, represented by the weighting vector $\boldsymbol{W_k} = (w_{1,k}, \ldots, w_{|C|,k})'$ (where $w_{ik} \geq 0 \ \ \forall\ i \in C$ and $\sum_{i \in C} w_{ik} = 1$), such that the set of following conditions are met:

$$\sum_{i \in C} w_{i,k} Y_{it} = Y_{kt}, \quad \forall\ 1 \leq t \leq T_0, \text{ and} \tag{2}$$

$$\sum_{i \in C} w_{i,k} z_{li} = z_{lk}, \quad \forall\ 1 \leq l \leq L. \tag{3}$$

Abadie *et al.* (2010) show that if equations (2) and (3) hold for all $t \leq T_0$ and for all L baseline variables, the synthetic control unit's predicted outcome $\sum_{i \in C} w_{i,k} Y_{it}$ is an unbiased estimate for the counterfactual of treated unit k in treatment period $t > T_0$. Ideally, treated unit k's synthetic control is thus the one with a vector of weights $\boldsymbol{W_k}$ that exactly replicates k's pre-intervention outcomes (see equation (2)) as well as each of its observable baseline characteristics (see equation (3)). Fitting on both pre-treatment outcomes and observable characteristics helps ensure that the estimated treatment effect is not affected by differences in unobservables even if they systematically differ between treated and non-treated units before the start of the intervention.

The process via which $\boldsymbol{W_k}$ is derived consists of two steps. In the first step the SCM assigns a randomly selected set of "relevance weights" $v_m \geq 0$ to each of the $M = T_0 + L$ constraints (see equations (2) and (3)) and collects them in an $(M \times M)$ symmetric, diagonal, and positive semi-definite matrix ($\boldsymbol{V}_k$). The SCM subsequently finds the set of control units' weights $\boldsymbol{W}_k(\boldsymbol{V}_k)$ that minimizes the distance between the $(M \times 1)$ vector of attributes of treated unit k that need to be matched and the corresponding $(M \times |C|)$ matrix containing the attributes of the non-treated units for the given relevance vector. Denoting the former by $\boldsymbol{X}_k = (z_{1k}, ..., z_{Lk}, Y_{k1}, ..., Y_{kT_0})'$ and the latter by $\boldsymbol{X}_0$, the distance function to be minimized in the first step is thus:

$$\min_{\boldsymbol{W_k}} \ \parallel \boldsymbol{X}_k - \boldsymbol{X}_0 \boldsymbol{W}_k \parallel_{\boldsymbol{V}_k} = \sqrt{(\boldsymbol{X}_k - \boldsymbol{X}_0 \boldsymbol{W}_k)' \, \boldsymbol{V}_k \, (\boldsymbol{X}_k - \boldsymbol{X}_0 \boldsymbol{W}_k)}, \tag{4}$$

$$\text{s.t.} \ \ w_{ik} \geq 0 \ \ \forall \ i \in C \ \ \wedge \sum_{i \in C} w_{ik} = 1. \tag{5}$$

Having repeated the first step for a large number of randomly selected relevance vectors $\boldsymbol{V}_k$ (each resulting in a specific $\boldsymbol{W}_k(\boldsymbol{V}_k)$), the second step is to find the $\boldsymbol{V}_k$ that results in the best fit in the pre-intervention period. Collecting the pre-intervention outcomes in a $(T_0 \times 1)$ vector denoted by $\boldsymbol{Y_k^{\leq T_0}}$ for treated unit k and the pre-intervention outcomes of non-treated units in a $(T_0 \times |C|)$ matrix denoted by $\boldsymbol{Y_0^{\leq T_0}}$, Doudchenko and Imbens (2017) propose to choose the set of relevance weights $\boldsymbol{V_k^*}$ that minimizes the root mean squared prediction error on the pre-intervention outcomes of the treated unit:

$$\min_{\boldsymbol{V}_k} \ \ \text{RMSPE}_{\boldsymbol{k}} = \sqrt{\frac{1}{T_0} \left(\boldsymbol{Y_k^{\leq T_0}} - \boldsymbol{Y_0^{\leq T_0}} \boldsymbol{W_k(V_k)} \right)' \left(\boldsymbol{Y_k^{\leq T_0}} - \boldsymbol{Y_0^{\leq T_0}} \boldsymbol{W_k(V_k)} \right)} \tag{6}$$

$$\text{s.t.} \ \ \ v_{mm} \geq 0 \ \forall \ m \ \ \wedge \ \ \sum_{m=1}^{M} v_{mm} = 1. \tag{7}$$

Having determined $\boldsymbol{V_k^*}$, the resulting treatment effect estimate of this unit in each period $t > T_0$ is equal to the difference between treated unit k's observed outcome and its counterfactual outcome as generated by its synthetic control:

$$\alpha_{k,t} = Y_{k,t} - \sum_{j \in C} w_{j,k}(\boldsymbol{V_k^*}) Y_{j,t} \quad \ \forall \ t > T_0. \tag{8}$$

We can then take the unweighted average of the treatment effects in period $t > T_0$ to calculate the average treatment on the treated effect of the intervention in the period $\left(\alpha_t = \frac{1}{|K|} \sum_{i \in K} \alpha_{it} \right)$.

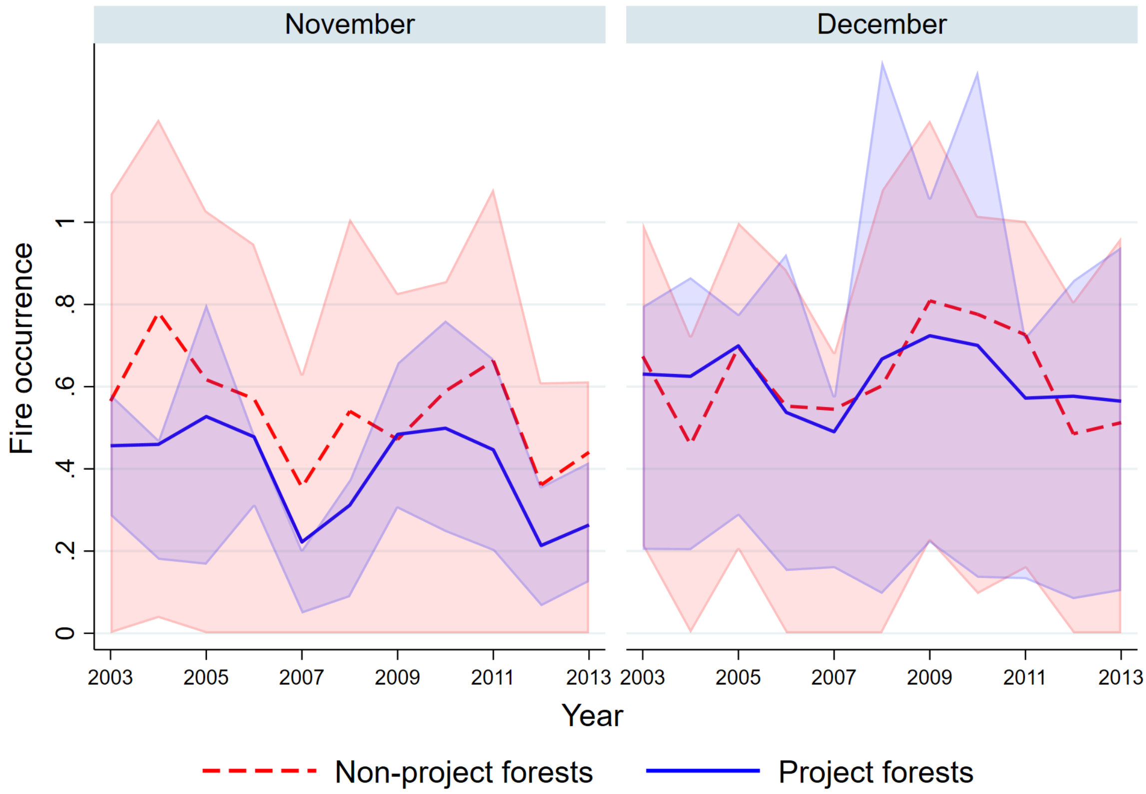
To test the "significance" of the estimates, we follow the placebo-test type approach proposed by Abadie *et al.* (2010) and Cavallo *et al.* (2013). We apply the SCM estimator to obtain placebo effect estimates on non-treated units and calculate the share of placebo test results that are larger than the actual impact estimates. This method, similar in spirit to randomization inference (Fisher, 1935; Rosenbaum, 2002), consists of selecting $|K|$ non-treated units from the donor pool (C) and using the SCM to estimate the "treatment effect" for each of these "placebo-treated" units ($\alpha_{it}^{\text{Placebo}}$) with synthetic controls that are obtained from the (properly weighed) remaining $|C| - |K|$ non-treated units; the average placebo treatment effect is then equal to $\alpha_t^{Placebo} = \frac{1}{|\mathcal{K}|}\sum_{i\in\mathcal{K}} \alpha_{it}^{Placebo}$, where $\mathcal{K}$ denotes the set of placebo-treated units ($|\mathcal{K}| = |K|$). For each average placebo effect estimate, the method also calculates the corresponding root mean squared prediction error ($\text{RMSPE}^{Placebo}$; see equation (6)) to capture the quality of the synthetic controls based on the pre-treatment fit on the observed outcomes (see also section 3). These steps are repeated N^{Placebo} times (with N^{Placebo} sufficiently large), and significance is then measured as the share of (normalized) average placebo impact effects that are larger than the actual (normalized) average treatment effect (α_t/RMSPE):

$$p_t^{\text{signif}} = \frac{\sum_{g=1}^{N^{\text{Placebo}}} I\left(\left|\frac{\alpha_t^{\text{Placebo},g}}{\text{RMSPE}^{\text{Placebo},g}}\right| > \left|\frac{\alpha_t}{\text{RMSPE}}\right|\right)}{N^{\text{Placebo}}}. \tag{9}$$

We scale the absolute treatment effect by RMSPE $\left(\left|\frac{\alpha_t}{\text{RMSPE}}\right|\right)$ as the test statistic instead of using the absolute treatment effect ($|\alpha_t|$) to control for the quality of the pre-treatment fit of the synthetic unit (see Abadie (2021)). Using the absolute treatment effect as the test, if the pre-treatment fit of the synthetic control is imperfect, estimated effects tend to be larger and the likelihood of false inference increases. Thus to penalize for this inflation of the treatment effect, Abadie (2021) proposes to scale the (placebo) treatment estimates with their pre-intervention RMSPEs. The equation above shows that all else equal, if the synthetic control unit of the treated unit fits poorly to the pre-treatment outcomes of the treated unit, then the significance level increases. This is because a poor pre-treatment fit results in large RMSPEs, which in turn decreases the test statistic of the treated unit (absolute of the effect normalized by the RMSPE, $\left(\left|\frac{\alpha_t}{\text{RMSPE}}\right|\right)$). All else equal, this results in a higher share of placebo test statistics to be larger than that of the treated unit and thus yields higher significance level.

C Supplementary figures and results

Figure C1: The evolution of fire occurrences in project and non-project forests over the period 2003-2013.



Note: Average November fire occurrences over the 2003-2013 period in the month are presented in the left panel, and while average December fire occurrences in the same period are presented in the right panel. The blue solid and red dashed lines depict fire occurrences in project and non-project forests, respectively. The top and bottom of the blue and red shaded areas capture the 25$^{\text{th}}$ and 75$^{\text{th}}$ percentile of fire occurrences.

Figure C2: November forest fire occurrences in the project forests and in the corresponding synthetic controls.

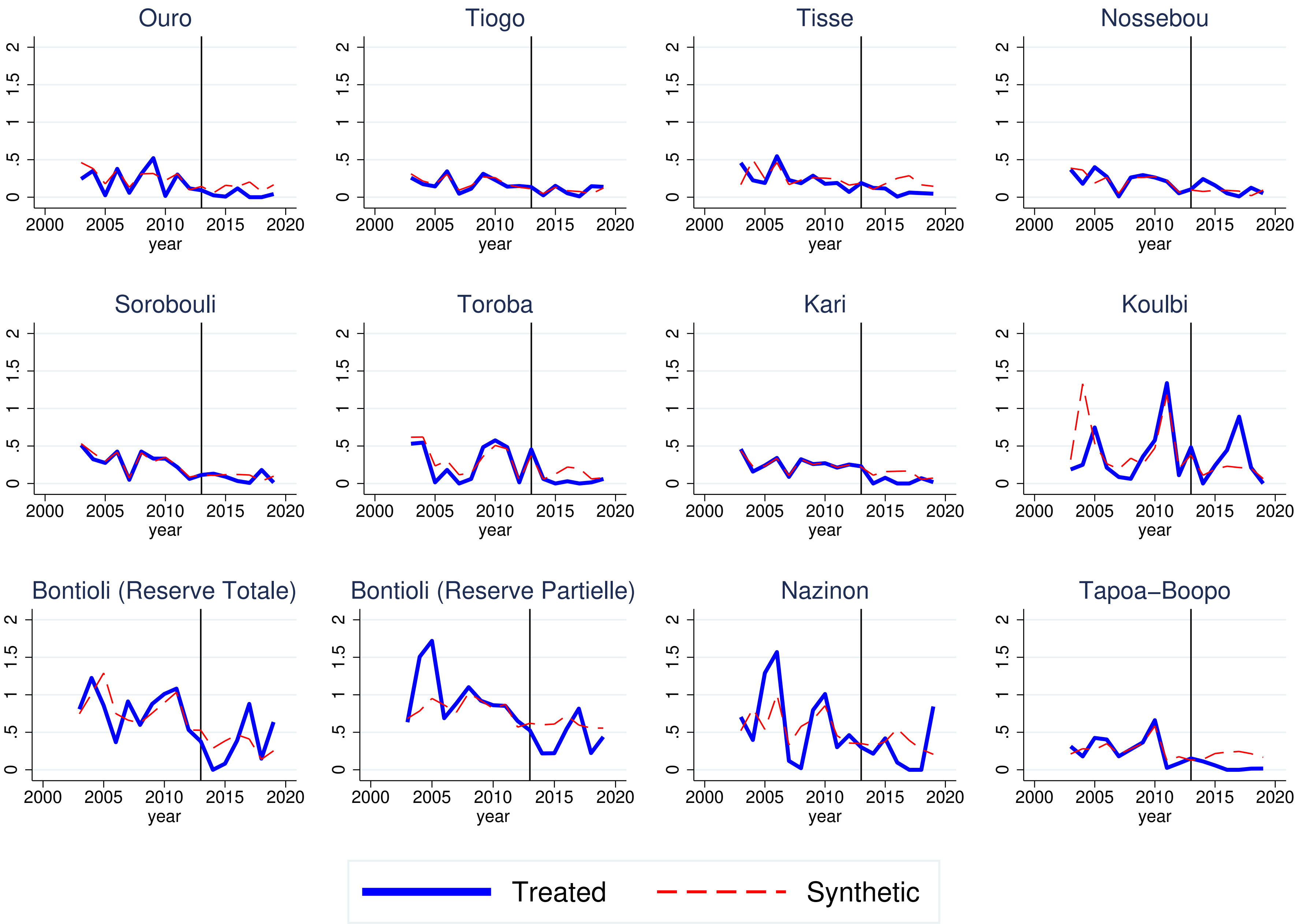
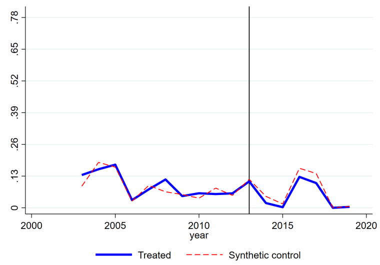
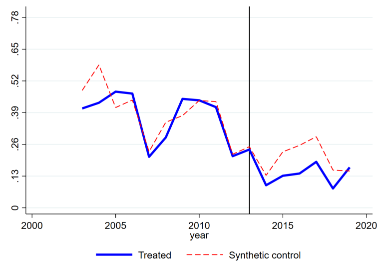


Figure C3: Fire occurrences with a detection confidence of 50% or better in the project forests and in their synthetic controls.

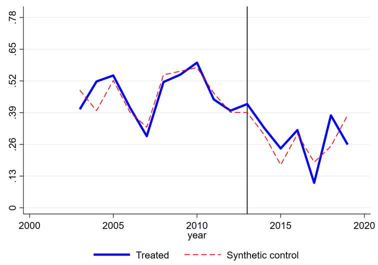
(a) October



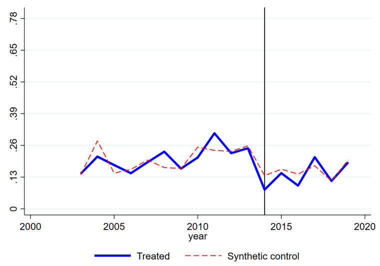
(b) November



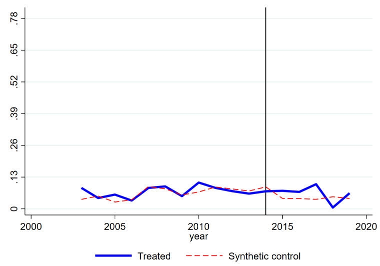
(c) December



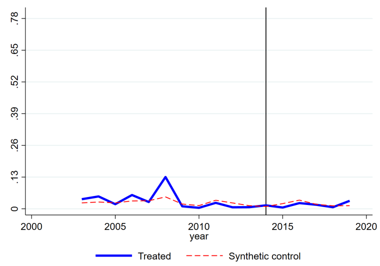
(d) January



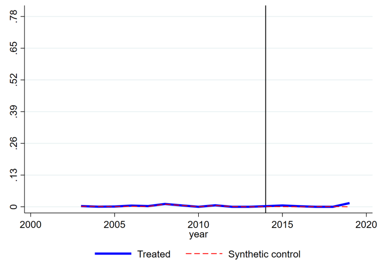
(e) February



(f) March



(g) April



Note: These figures present fire occurrences in the project forests measured in days per month per grid. The blue continuous lines show the observed fire occurrences in project forests with at least 50% fire detection confidence, whereas the red dashed line show the estimated counterfactual outcomes in the absence of the FIP program. The synthetic control units, that build into the counterfactual outcomes, are estimated separately for each months. The year before the beginning of the program (October 2014) is is indicated with the black vertical line.

Table C1: Pre-treatment Root Mean Square Errors of the synthetic controls.

	(1)	(2)	(3)	(4)
	Synthetic Control RMSPE		Simple average non-project forest RMSPE	
Month	RMSPE	RMSPE relative to avg. pre-treat. Y	RMSPE	RMSPE relative to avg. pre-treat. Y
1	.0424	.2104	.0972	.4821
2	.023	.3178	.0558	.7697
3	.0282	.8645	.0351	1.079
4	.0012	.3688	.0051	1.5831
10	.0281	.2829	.0495	.4989
11	.0571	.144	.1678	.4232
12	.0493	.1015	.2268	.4673

t

Note: The table shows the pre-treatment Root Mean Squared Error of the counterfactual outcomes derived form the SCM and of the unweighted averages of all donor forests as predictors of the observed outcomes of the program forests. Columns (1) and (3) show the absolute RMSPE measured in the number of days a grid was detected to be on fire, while columns (2) and (4) shows the RMSPE relative to the pre-treatment average of fire occurrences. The table shows that pre-treatment Root Mean Squared Errors of the synthetic control units are small in absolute terms (between 0.001 days to 0.057 days per month, see Column (1)), It also shows that the synthetic control units predict much better the pre-treatment outcomes of the program forests than the simple average of non-program forests (columns (1) vs (3). Column (2) of the table also shows that the differences between the synthetic units and the observed outcomes of the program forests are small in the most fire prone months (October, November, December, January) relative to the pre-treatment average of those outcomes ($RMSPE/\bar{Y}_{t<T_0}$): 10 to 29 percent of the pre-treatment outcomes.

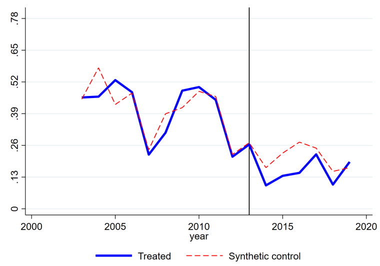
Table C2: Estimated impacts of the the program on November fire occurrences from the robustness checks.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Original estimates	ASCM estimates	Estimates from the backdating test		Estimates testing robustness to spillovers	Estimates from the leave-one-out test	Estimates including socio-economic variables for fitting
Training period:	2006-2013	2006-2013	2006-2012	2006-2011	2006-2013		
2012				–0.050			
				[0.699]			
2013			–0.001*	–0.029			
			[0.094]	[0.459]			
2014	–0.073	–0.049	–0.079	–0.087	–0.043	–0.076	–0.076
	[0.284]	[0.2093]	[0.131]	[0.232]	[0.890]	[0.735]	[0.746]
2015	–0.093***	-0.140***	–0.094***	–0.106**	–0.104**	-0.103**	–0.104**
	[0.002]	[0.0095]	[0.001]	[0.015]	[0.019]	[0.019]	[0.017]
2016	–0.126***	0.106**	–0.094***	–0.094***	–0.109**	-0.128***	–0.131***
	[0.000]	[0.040]	[0.000]	[0.002]	[0.048]	[0.002]	[0.002]
2017	–0.026***	–0.015	–0.029***	–0.035*	–0.129**	-0.038**	–0.035**
	[0.003]	[0.626]	[0.002]	[0.094]	[0.026]	[0.0166]	[0.015]
2018	–0.054	-0.072	–0.064	–0.071	–0.103*	-0.060	–0.059
	[0.459]	[0.485]	[0.825]	[0.869]	[0.091]	[0.327]	[0.200]
2019	0.023	0.049	0.021	0.007	0.000	0.012	0.013
	[0.213]	[0.302]	[0.154]	[0.229]	[0.329]	[0.250]	[0.237]

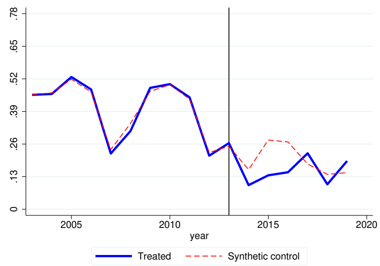
Note: The table presents the estimated average impact of the program on fire occurrences in November for various robustness tests. For ease of comparison, column (1) presents the original results (i.e., the November results presented in Table 2); these were obtained using 2006-2013 as the SCM training period (as the program started in 2014). Column (2) presents the estimates using the Augmented Synthetic Control Method of Ben-Michael *et al.* (2021) instead of the standard SCM method and 2006-2013 as the training period. Columns (3) and (4) present the estimates using 2006-2012 and 2006-2011 as training periods with the standard SCM method, respectively. Column (5) presents estimated impacts using the 2006-2013 as the training period but excluding non-project forests adjacent to project forests from the donor pool. Column (6) presents the average point estimates over the 34 estimations we obtained by excluding each non-project forests that received a positive weight in our main analysis one at a time and applying the SCM on the remaining non-project forests. Column (7) presents the estimates using the whole pre-treatment period for training, but also including the socioeconomic variables (constructed by simple averaging at the forest level) as fitting variables. P-values from the inference tests are presented in square brackets. Point estimates with $p < 0.10$ are presented in black. * p < 0.10, ** p < 0.05, *** p < 0.01.

Figure C4: Fire occurrences in November and the counterfactual outcomes from the robustness checks.

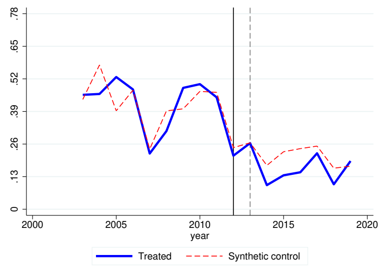
(a) Main estimation



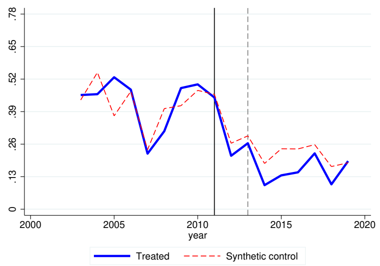
(b) ASCM estimates



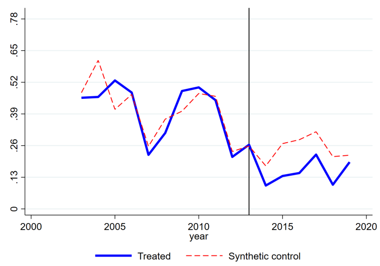
(c) Backdating (2006-2012)



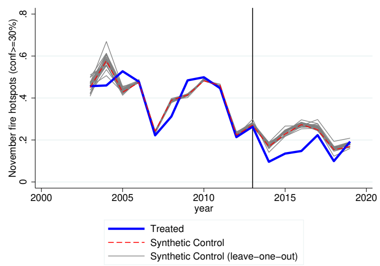
(d) Backdating (2006-2013)



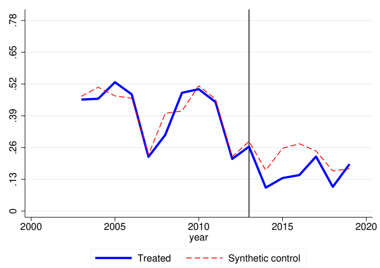
(e) Robustness to spillover



(f) Leave-one-out estimates



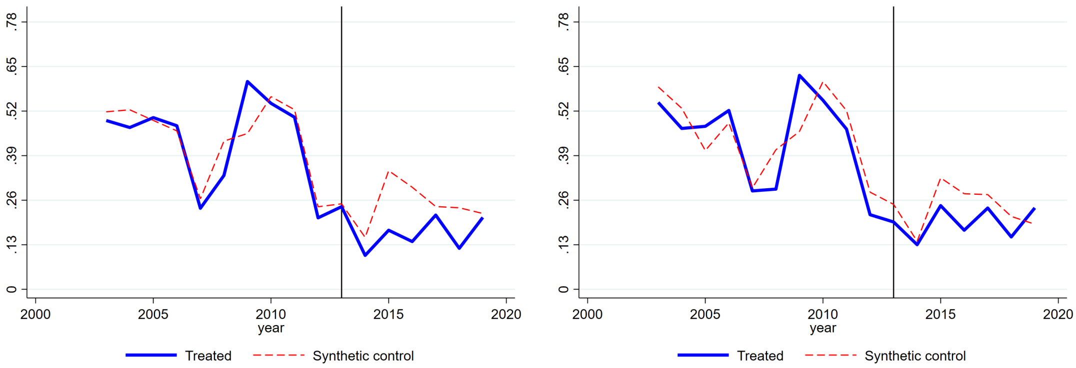
(g) SCM estimates fitting on socio-economic variables



Note: These figures present fire occurrences in project forests (blue continuous lines) in November and the estimated counterfactuals (red dashed liens). Panel (a) shows the main results (see Figure 4b). Panel (b) shows the estimations from the Augment Synthetic Control Method of Ben-Michael *et al.* (2021). Panel (c) and (d) shows the results from the backdating method (with the standard SCM) which uses 2006-2012 and 2006-2011 as the training period, respectively. Panel (e) shows the estimations using the standard SCM method and the years from 2006 to 2013 period as the training period, but excluding non-project forests adjacent to project forests from the estimation. Panel (f) shows the estimation results from the leave-one-out tests where each gray continuous line corresponds to a synthetic control units estimated using the SCM and excluding one of the donor forests that received positive weights in the main analysis. Panel (g) shows the results using the standard SCM and including socio-economic variables in the fitting process.

Figure C5: Observed fire occurrences in November in the interior of the forest.

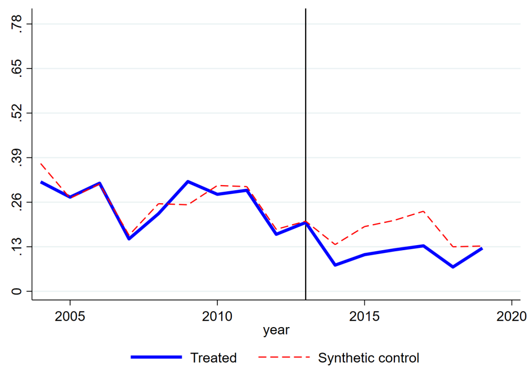
(a) Fire occurrence using a 1km threshold (b) Fire occurrence using a 2km threshold



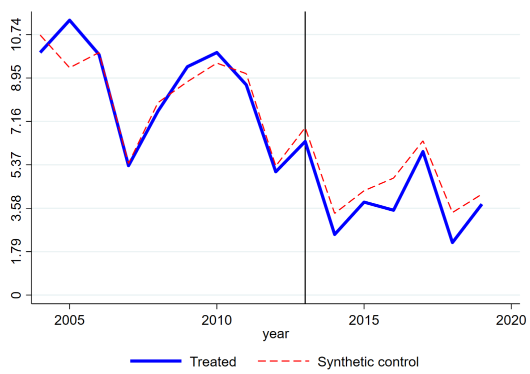
Note: These figures present fire occurrences within the interior of the project forests in November. In Panel (a) we consider fire occurrences on grid cells that are at least 1 km from the forest border. In Panel (b) we calculate the same fire occurrence using 2 km as the threshold. The blue continuous lines show the observed outcomes in project forests (taking fires with 30% or better confidence), whereas the red dashed line show the estimated counterfactual outcomes in the absence of the FIP program. The beginning of the program is is indicated with the black vertical line and it corresponds to October 2014.

Figure C6: Observed number of fires that were started, their average size, and the estimated counterfactuals in project forests in November.

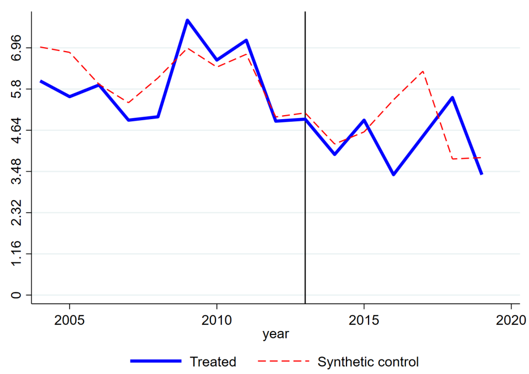
(a) Share of fire affected grids in November



(b) # of ignitions



(c) Average spread of fires



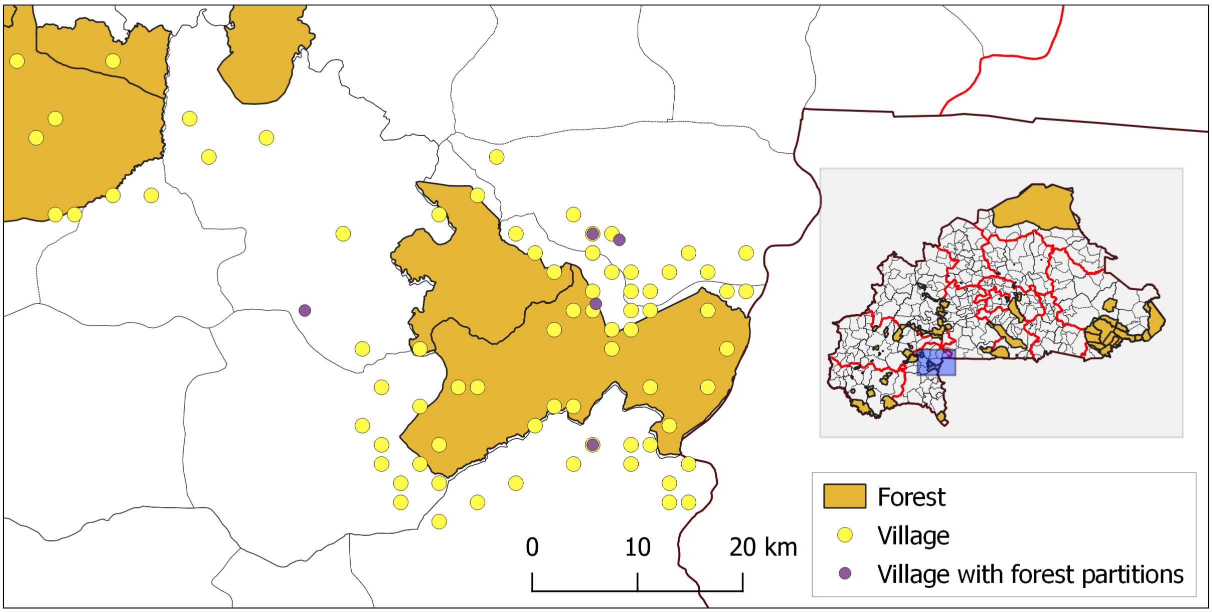
Note: These figures present the number of fire ignitions and the average size of fires (measured in km^2) in the project forests in November. The blue continuous lines show the observed outcomes in project forests (taking fires with 30% or better confidence), whereas the red dashed line show the estimated counterfactual outcomes in the absence of the FIP program. The beginning of the program is is indicated with the black vertical line and it corresponds to October 2014.

D Sub-forest level analysis with the LSMS survey

The LSMS is a nationally representative household level survey that collected information on household composition, households' economic and employment status, housing, agricultural production, health, education, and food security. The surveys were implemented by the National Institute of Statistics and Demography of Burkina Faso. We construct our community level variables based on the responses from the first round collected between January and March of 2014.

An important limitation of the LSMS is that compared to the ten thousand settlements in the country, the survey sample only consists of 905 enumeration areas (normally a village, a small group of villages, or a district in a city) and only 150 could be linked to forest partitions. More precisely, 189 forest partition can be formed, but 39 of these forest partitions (3 in project forests and 36 in non-project forests) are small, less than 10 grid size. Although our results are not sensitive to the inclusion of these forest grid cells in our analysis, we omit these outliers from the sample to avoid the mis-characterization of small forest partitions. The remaining forest partitions consist of 10 to 600 forest grid cells. This means that villages assigned to forest partitions are not necessarily the closest to the forest among all villages (see Figure D 7) and the assignment of survey villages to forest partitions inherently assume that the survey village is similar to other villages between the survey forest and the forest. This concern may be relevant, as the average Euclidean distance between the forest partition and the corresponding LSMS village around project forests is about 10.3 km (see Table 6) – a considerable distance indeed because of sparsity of roads which are oftentimes also of poor quality. Therefore analyses in this section may be subject to bias from measurement error in community characteristics to the degree that villages surveyed in the LSMS are different from those closer to the forest.

Figure D 7: Visualization of the LSMS villages around the Bontioli Reserve.



Note: The map shows the communities surrounding the Bontioli Reserve, which lies in the South-South-West of Burkina Faso (highlighted in the small blue box on the right). The Bontioli Reserve is highlighted by the orange area in the center of the map. The villages within 10 kilometres of the forest border are represented by the yellow dots. Villages in the sample of the LSMS survey are depicted by the purple dots.