Reducing Hunger with Payments for Environmental Services (PES): Experimental Evidence from Burkina Faso

Guigonan S. Adjognon, Daan van Soest, and Jonas Guthoff

Both environmental protection and poverty alleviation are high on the international policy agenda for developing countries. We examine whether conditional environmental cash transfer programs contribute to the social protection of the beneficiaries, using data from a randomized controlled trial on reforestation, implemented in cooperation with the government of Burkina Faso. Randomly selected farmers were invited to undertake maintenance activities to increase the survival rate of trees that were planted on degraded forest lands. Compensation, in the form of monetary payments, varied with the number of trees still alive nine months after the tree planting. The timing of the conditional payments coincided with the lean season, when most farmers needed cash for food consumption and agricultural inputs. Six months after the transfers, the recipient households reported 12% higher food consumption expenditures compared to the control group, as well as a 35 and 60% reduction in, respectively, moderate and severe food insecurity. Our data indicate that the transfers received during the lean season were not only used to address immediate consumption needs but also to invest in crop and livestock production. The investments resulted in subsequent increases in agricultural outputs and income. We thus conclude that conditional environmental cash transfers, when paid in the lean season, can not only support consumption smoothing in the short run but can also improve livelihoods in the longer run.

Key words: Burkina Faso, food security, payments for environmental services, REDD+, reforestation.

JEL codes: I38, Q18, Q23, Q56.

Hunger and malnutrition are still pervasive in the developing world. Food insecurity is disproportionately concentrated in Sub-Saharan Africa (SSA). This is especially true in the Sahelian drylands, where a large share of the population is moderately or even severely food insecure (FAO et al. 2019). The dependence on increasingly unpredictable rainfed food production systems and the limited access to funds to support consumption smoothing result in a vicious poverty cycle. Poor households engage in negative coping strategies during the hungry season with adverse consequences for both agricultural production and post-harvest welfare that, in turn, trap a substantial share of the rural population in poverty and food insecurity (Christian and Dillon 2018; Fink, Jack, and Masiye 2018).

Food insecurity is one important issue in the Sahelian drylands of Sub-Saharan Africa; the battle against desertification is another. Payments for environmental services (PES)

Guigonan Serge Adjognon is an agricultural economist in the Development Impact Evaluation (DIME) Department at the World Bank. Daan van Soest is professor of environmental economics (Department of Economics and CentER) at Tilburg University, and academic director of the Tilburg Sustainability Center. Jonas Guthoff is a research assistant in the Development Impact Evaluation (DIME) Department at the World Bank. This work was made possible by the financial support from the Climate Investment Funds and thanks to the fruitful collaboration with Burkina Faso's Programme d'Investissement Forestier (PIF) in the implementation of this research. We thank colleagues from DIME for helpful feedback on initial drafts of this paper. We also thank the editor Marc Bellemare and three anonymous reviewers for their constructive comments and suggestions. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank and its affiliated organizations, or those of the executive directors of the World Bank Group or the governments they represent. All remaining errors are our own.
Correspondence to be sent to: gadjognon@worldbank.org

Amer. J. Agr. Econ. 103(3): 831–857; doi:10.1111/ajae.12150
Published online October 12, 2020

schemes offer communities and/or individual landowners financial compensation in return for the provision of environmental services, such as reduced deforestation or increased reforestation (Engel, Pagiola, and Wunder 2008). By offering financial compensation conditional on improved environmental service provision, PES schemes address the fundamental cause of environmental degradation—the fact that the costs of conservation are incurred locally, whereas a substantial share (if not all) of the conservation benefits accrue globally. Although Jayachandran et al. (2017) show that PES schemes are effective in promoting conservation, recent overview papers document a critical dearth of empirical evidence on their socio-economic impacts (Samii et al. 2014; Borner et al. 2017).

In this paper, we leverage a randomized controlled trial (RCT) to estimate the welfare impacts of a government-led PES scheme aimed at reforesting degraded forest lands in arid Burkina Faso. As part of this intervention, inhabitants of the communities in close proximity to the project forests were invited to participate in tree planting campaigns in well-defined areas in the forests. The campaigns started with, in total, about 33,500 new trees being planted on sixty-six reforestation sites across eleven protected forests in August 2017. Subsequently, community members of villages surrounding these project forests were randomly selected, from a pool of volunteers, to receive a PES contract for the maintenance of the newly planted trees. Those who were selected for the maintenance activities were grouped into teams of five and made responsible for the survival of the trees planted on their assigned reforestation site. Contracts were signed between each participant in the maintenance group and the implementing government agency. The contracts guaranteed that each group would earn about $0.60 for every tree still alive at verification almost a year after the planting campaign (in June 2018). It stated further that the amount received would be divided equally among all five group members. We ensured that this was the case by transferring all money via private mobile money accounts.

We investigate the impacts of the Burkina Faso PES scheme, using primary data collected at the start of the tree maintenance project as well as six months after the completion of the transfers—a time period of fourteen months. At baseline about 90% of the individuals in our sample were farmers whose families were at risk of food insecurity, facing significant liquidity constraints because of the (imminent) exhaustion of their food stocks and earnings from the previous harvest period. The timing of the PES payments coincided with this "lean period." The transfers are thus expected to alleviate short-run food deficits. But if the transfers also allowed households to invest more in productive activities, or avoid negative coping strategies to tackle immediate consumption needs, their impacts may actually be larger and long lasting.

We find that participation in the PES scheme induced an increase in households' food consumption expenditures by 12% and a reduction of the prevalence of moderate and severe food insecurity by 35 to 60%. We also document positive effects on agricultural as well as livelihood outcomes in the post-harvest period. We investigate the channels of the PES payment impacts and find that the treated participants cultivated more land, invested more in improved seeds and pesticides, and obtained higher agricultural outputs than their control peers. Although crop production is the main economic activity for over 90% of our participants, PES recipients were significantly more likely to engage in a secondary occupation at endline (especially livestock production) and reported an overall higher level of income. We thus find that participation in the PES program (and the associated receipt of cash payments) did not just reduce food insecurity directly by allowing for more food purchases but also by increasing the participants' income-generating capacity. In fact, the average increase in food consumption expenditures in the month prior to the endline survey is about 25% of the average amount of PES transfer received. The impacts of participation in the PES scheme on livelihoods may thus extend well beyond the agricultural cycle in which the payments were disbursed.

Our results contribute to three main strands of the development economics literature: (*a*) PES as a dual environmental conservation and poverty reduction tool; (*b*) a cash for work (C4W) program as a social protection tool for vulnerable communities in the drylands; and last, (*c*) the impacts of seasonal liquidity constraints on rural households.

Accounting for the socio-economic impacts of environmental programs is important to fully appreciate the overall benefits of those programs relative to their costs and in order to make appropriate policy decisions about whether and how to implement them. Yet the literature on the socio-economic impacts of

PES schemes is still very thin, geographically skewed, and so far, suggests limited impacts (Samii et al. 2014; Puri et al. 2016; Borner et al. 2017; Liu and Kontoleon 2018). For example, Alix-Garcia, Sims, and Yañez-Pagans (2015), in their evaluation of a federal PES program in Mexico, find substantial effects on land cover outcomes but only small positive effects on poverty reduction. The authors rely on matching techniques to identify treatment effects while our study takes advantage of an experimental design. Our study brings evidence from the Sahel, a region affected by multiple climate and poverty-related challenges, and yet critically underrepresented in the available literature on the question. The closest study to ours is by Jack and Santos (2017) who use an experimental design to evaluate the welfare and environmental impacts of a PES program in Malawi. They conclude that the program led to significant labor shortage among the participants without generating a significant impact on any socio-economic outcome. Their program was, similarly to our study, a reforestation intervention where participants were paid based on tree survival rates. However, one main difference with our case, in addition to the geography, is that their scheme was based on private lands, which changes the nature of the trade-off involved in participation. In our case, the scheme was based on protected forest lands, which makes it more comparable to a C4W intervention. Another related study is by Alix et al. (2018), who find significant effects of PES on social capital in Mexico. Our study is the first to report strong, significant, and positive effects of a PES scheme on participants' welfare outcomes, based on an experimental design. It complements the evolving literature on this issue, with evidence from a data-poor context such as the Sahel region of SSA.

There is a large and still evolving literature across the developing world showing that cash transfers[1] to individuals can relieve capital constraints and lead to positive welfare effects (De Mel, McKenzie, and Woodruff 2008; Angelucci and De Giorgi 2009). For example, Haushofer and Shapiro (2016) find strong improvements in consumption and large increases in psychological well-being of poor households in rural Kenya as a result of a purely unconditional cash transfer intervention. These results are corroborated by Bedoya et al. (2019) in Afghanistan. However, very little of the evidence base on cash transfers focuses on C4W per se. One of the few exceptions is Beegle, Galasso, and Goldberg (2017), who find no evidence that a labor-intensive public work program implemented by the government of Malawi resulted in higher lean season food security among participants. In contrast, Rosas and Sabarwal (2016) find positive effects of a C4W program in Sierra Leone on food, medical, and asset spending. Our study complements this literature by linking it to the one on environmental policies and by bringing in evidence from the Sahel region.

Finally, by providing the cash transfers to the PES participants, specifically during the lean season, our study also contributes to the literature on the impacts of seasonal liquidity constraints. The lack of access to capital, to support immediate consumption needs at critical times of the year, pushes households to make suboptimal choices, which can affect their income and keep them in poverty. For example, Burke, Bergquist, and Miguel (2019) use a field experiment in Kenya to show that liquidity constraints at harvest time prevents farmers from taking advantage of intertemporal arbitrage opportunities in grain markets. Providing timely access to credit allowed farmers to delay sales to a later point in time, when prices are high, thereby increasing their revenues. In a related study, Fink, Jack, and Masiye (2018) show experimentally that liquidity constraints in the lean season push small-scale farm households in Zambia to oversell labor off farm. They find that lowering the costs of access to liquidity during the lean season mitigates labor oversupply and improves consumption and income for the more liquidity-constrained farms, with positive implications for inequality reduction in the community. Our study is the first experimental study of the impacts of an environmental C4W program, which also addresses seasonal liquidity constraints for rural farm households, specifically in the drylands of SSA. Thus, our study seeks to inform social protection and environmental policies in the Sahel and other regions with similar agro-ecological conditions.

The next section of this paper describes the study context and the experimental design. The empirical strategy for estimating the

[1]Here, we refer broadly to all types of programs that involve handing cash to people. We recognize that the term cash transfers may indicate more specific types of interventions in the development economics literature.

treatment effects, as well as the data sources, are detailed in the subsequent section. The following section presents the results and related discussions, including the mechanisms of impacts. The last section concludes and makes recommendations for future research.

Program Description and Experimental Design

The notion that forests represent a cost effective source of carbon sequestration while providing economic, environmental, and socio-cultural benefits (Canadell and Raupach 2008; Busch et al. 2019) has received renewed impetus with the 2015 Paris Agreement, as evidenced by the key role the agreement attributed to forest conservation. In this spirit, the government of Burkina Faso has initiated a Forest Investment Program (FIP) with joint technical assistance from the African Development Bank and the World Bank Group, and with financial support from the Climate Investment Fund. One of the main goals of Burkina Faso's FIP is to improve the carbon sequestration capacity of protected forests while contributing to poverty reduction in forest-dependent communities.

The Intervention

Burkina Faso is a landlocked country in the Sahel region of Africa and is characterized by dry forests with sparse tree coverage. As a result, landscape restoration through large scale reforestation is one of the most common forest interventions in this country (Adjognon, Rivera-Ballesteros, and van Soest 2018).[2] One of the FIP's key interventions is a reforestation project involving eleven of the country's seventy-seven protected forests, in which local communities are engaged in the planting of indigenous trees on degraded areas of targeted forests.[3] Assistance to these activities is provided by the FIP's project implementation team with the support of local institutions called forest management committees (Comité de Gestion Forestière, or CGF).

The reforestation campaign has two distinct phases. The first phase, which is not part of this study, is the tree planting phase, whereby the local communities are involved in planting seedlings on well-defined reforestation plots. The tree planting phase for the 2017 reforestation campaign happened in July/August 2017, during which about 33,500 seedlings were planted across sixty-six reforestation plots defined within the eleven protected forests of intervention. Note that this phase was not part of the RCT. Although quite a few of the individuals who participated in the tree planting scheme were also interested in participating in the subsequent phase of tree maintenance, prior engagement in the tree planting activity did not give prioritized access to the tree maintenance phase.

The second phase is thus the tree maintenance phase, which seeks to keep as many sapling trees alive as possible. Growing conditions in Burkina Faso are harsh, and survival rates can be improved not only by watering plants but also by maintaining the holes in which saplings are planted, by removing dead organic material in the plant's vicinity, by protecting the plants from being eaten by wildlife or livestock, and also by putting up fire breaks. These activities require time and effort, and hence the FIP decided to enroll community members into a PES scheme that will compensate participants based on the survival rate of the newly planted trees under their care. Our RCT is based on the random allocation of individuals, who were interested in participating in this tree maintenance scheme, to a treatment and a control group.

Experimental Design

We harness the outcomes of the above-described RCT to evaluate the welfare impacts of the environmental cash transfer intervention aimed at increasing the planted trees' survival rates. The study's implementation required a close partnership between the research team and the FIP, in order to embed key RCT-related activities in the project implementation. Figure 1 summarizes the timeline of the different activities related to this study.

As described in the previous subsection, the RCT is built on the tree maintenance phase; this phase is completely orthogonal to the prior tree planting phase. Having completed the tree planting phase, the project team

---

[2]For example, the African Landscape Restoration Initiative (AFR100) is a large scale multi-country initiative to bring 100 million hectares of African land into restoration by 2030 (https://afr100.org/).

[3]The choice of indigenous tree species is seen as important for both carbon sequestration and biodiversity conservation.

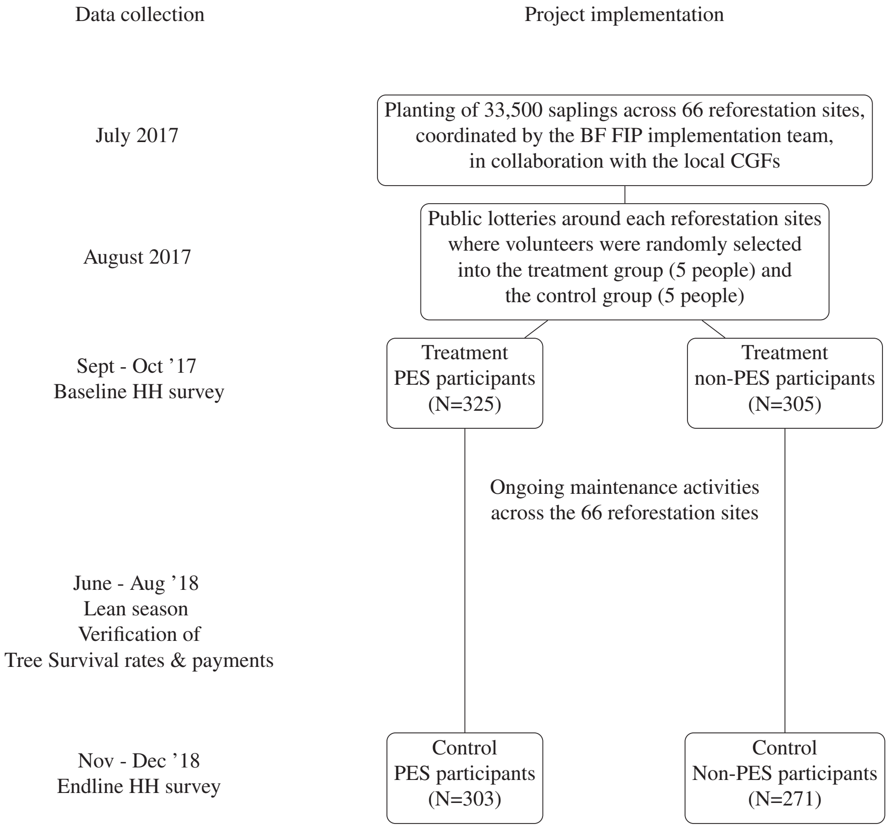

Figure 1. Timeline of the randomized controlled trial

Note: The flowchart above depicts the full implementation timeline of the PES intervention and data collection activities for this study.

invited local community members to apply to participate in the tree maintenance phase. Many of the volunteers previously participated in the tree planting phase, but this did not mean that they automatically participated in the tree maintenance project. All local community members interested in participating in the tree maintenance activity were informed about the details of the PES schemes that had been designed to incentivize tree maintenance. This entailed the information that the size of the monetary payments to participants would depend on the number of trees still alive on their reforestation plot nine months later. For each of the sixty-six reforestation plots, we intended to select ten people: five of whom would participate in the PES scheme and five of whom would end up in the control group. Interest was such that, having heard the details of the contract, the number of eligible individuals[4] was always strictly larger than ten.

Our program was thus oversubscribed, and hence the selection of the participants was implemented in two stages. First, we used a public lottery to select ten people from the pool of interested and eligible individuals. Second, we used again a public lottery to allocate five of the ten to the treatment and the remaining five to the control group. Those in the treatment group were offered a PES contract; those in the control group were not included in the PES scheme but were visited for all survey rounds.[5] The lottery approach was considered the fairest way to enroll people and to preserve the trust of the community members (Gueron 2017). Field agents representing the research team were present and helped the project teams implement these public lotteries. The lottery is also central to our identification strategy to evaluate the impacts of the

[4]The only two eligibility criteria were being 18 years or older and a deemed fit to do the work. Eligibility was confirmed on the spot by the project's field agents.

[5]Contrary to Bertrand et al. (2007), we did not offer any monetary incentives to induce participation, especially from the control group, in our survey. Doing so would have potentially affected our estimates of the impacts of the PES cash transfers.

PES intervention on participants, because randomly rejecting some of the interested individuals from each community implies that the treatment and control groups are likely to be identical in all respects.

Tree Survival Outcome and PES Payments

Each team of the five participants employed in the PES contract was informed that they could collectively earn money based on the survival rate of the newly planted trees on their assigned reforestation plot. PES contracts assured the groups that they would receive about US$0.60 for every tree still alive at verification (about nine months after the start of the maintenance project), and that every group member would receive an equal share (20%) of the total amount. All team members received instructions about the activities in which they could increase the survival rate. About 500 trees were planted on each plot, and hence the group earnings could be anything between zero and $300.

The contract signing was followed by the baseline survey, which was rolled out in September and October 2017, and during which 630 respondents were surveyed successfully—325 in the treatment group and 305 in the control group. The enumerators were unable to locate and interview the remaining thirty respondents, mostly due to migration reasons.

About nine months later, in June and July 2018 (just before the start of the next rainy season), all reforestation areas were visited by independent monitors and consecutively participants received their payments based on the number of trees still alive on the reforestation site. The average survival rate was around 37%, with some variation across species and regions.[6] Although these survival rates may seem low, this outcome is actually very good considering the arid conditions in the Sahel (Carey 2020).[7] Given low survival rates of trees in the Sahel, it is important for policy makers to also appreciate the co-benefits of these interventions, including the livelihood outcomes for the affected communities, in order to best decide among alternative policy instruments.

The RCT's endline survey was implemented in November/December 2018, about five months after the payments had taken place. We were able to survey a total of 574 study participants—303 in the treatment group and 271 in the control group. From the 303 treatment respondents surveyed, 291 or 96.04% confirmed having received a PES transfer. The individual payments received ranged from about 840 FCFA ($1.50) to 25,620 FCFA (about $45.60). The average payment was 8,300 FCFA ($14.75)—the value of about one week of food consumption for the median rural household in Burkina Faso, or 25–30 kilos of fertilizer (NPK, or Urea).[8] Figure A1 in the online supplementary appendix shows the distribution of the payment amounts received by the participants in the treatment group.

From a participant's private welfare perspective, the PES transfers are the only short-time benefits from participation in the program. The saplings planted were sufficiently small so that they were not able to provide any local private benefits (in the form of food or fruits, or as building materials or fuelwood) during the study time frame. Furthermore, the reforestation was implemented on degraded land within protected forests. Planting trees therefore does not establish property rights (neither formally nor informally), and hence the land with the newly planted trees cannot be used as collateral for accessing credit (Abdulai, Owusu, and Goetz 2011; Deininger, Ali, and Alemu 2011; Fenske 2011).

The timing of the PES payments is of particular importance. Figure 2 presents data on food insecurity, as experienced by the households in the control group, over the period October 2017–September 2018—the agricultural season before the payments and two months into the new agricultural season. This figure is thus reflective of the levels of food insecurity experienced by farmer households in the region, absent any intervention. As is clear from this figure, the lean period spans March to September and is the time when food insecurity is most prevalent among non-

[6]The most resistant species were *Acacia Senegal* (acacia) and *Adansonia Digitata* (Baobab), with survival rates of 54% and 49%, respectively. The least resistant were *Parkia Biglobosa* (Nere) and *Khaya Senegalensis* (Cailcedrat), with survival rates of 24 and 20%, respectively.

[7]For example, an experiment conducted as part of the pan-African Great Green Wall for the Sahara and the Sahel Initiative (GGW) reported an average survival rate of about 20%, with variations across species, which are consistent with our findings in Burkina Faso (Wade et al. 2018).

[8]Authors' calculations from the BF 2014 Living Standards Measurement Survey (LSMS) data estimate the median rural household's daily consumption expenditures at about 1580 FCFA ($3). In July 2018, 1,000 FCFA = US$1.78.

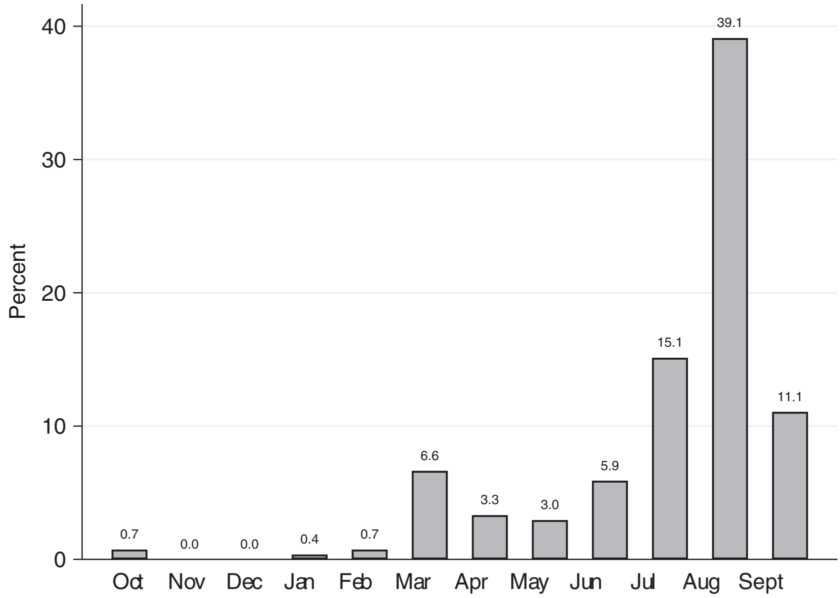

Figure 2. Seasonality of food insecurity (October 2017 to September 2018)

Note: This figure uses data from our control group only, and summarizes the percentage of households with insufficient food for each of the twelve months prior to the endline. In the post-harvest months (Oct-Feb), hunger rates are the lowest. But from March through September, food insecurity rises because most people run out of food reserves at some point during this period.

participants due to dwindling stocks from the previous harvest. As households' marginal utility of consumption increases during this period, they may trade off future consumption for present consumption. For example, they may borrow money from local money lenders at a high interest rate and pay back after harvest (Hoff and Stiglitz 1990). They may also temporarily sell off too much household labor off farm, which may affect the household's production and earnings for the season (Fink, Jack, and Masiye 2018). In both cases, the amount of resources available for the household in the post-harvest period could be reduced because part of it is used (directly or indirectly) to support immediate consumption needs. At the time of the PES transfers distribution, which coincided with the lean season, most of the participants were already out of food stock from the previous harvest, which was almost a year ago by that time.

Regarding the costs of participation, the opportunity costs of the time invested in tree maintenance is quite low in our context. The distance between the plots and the participants' homestead was reported to be 7.8 km on average and considered relatively far by the majority participants. However, the vast majority had access to some means of transportation, typically a bicycle or a moped. Most of the maintenance labor needed to be supplied during the dry season—the six-month period after harvest is completed and until the next rainy season will start. So demand for agricultural labor (or the time needed to work the land) is negligible during the period in which most of the tree maintenance activities needed to be implemented. Also off-farm employment opportunities are typically scarce. This means that households that actively contributed to keeping the trees alive can only be worse off in terms of having worked at times where others were idling—but not in terms of reduced land or labor available for, for example, agricultural activities. That also means that if our PES design invited free riding, this is unlikely to have resulted in biased financial consequences. We ensured that each member received the 20% share of the group payments they were entitled to (by sending the money they were entitled directly into mobile money account), and the scarcity of alternative employment opportunities in the dry season makes it unlikely that free riders were able to earn additional incomes during the time others were engaged in the tree maintenance activities.[9]

The PES payments may have affected behavior and outcomes from the moment they were disbursed—from June/July 2018 onwards. However, the timing of the payments was clearly communicated at the start of the project (as well as the payment conditions), and hence they may also have affected behavior in the period *prior* to their disbursement. They may thus also have acted as safety nets for the recipient households by supporting their consumption smoothing capacities and avoiding negative coping strategies. The payments may also have allowed the recipients households to invest in productive activities to increase their earnings for the season. We focus in the remainder of this paper to investigate whether these mechanisms were present.

Empirical Framework

We seek to estimate the average treatment effect of participation in the PES scheme on households' welfare, mainly captured by food security. Our identification strategy exploits the random assignment of individual members of the communities into a treatment and a control group.

The treatment group received contracts, which guaranteed a monetary compensation based on the number of trees still alive on their assigned reforestation area at verification. The control group did not receive any contract and hence received no cash transfers from the project. We estimate an intention-to-treat (ITT) effect of the intervention on households' food security, using the following ANCOVA model:

$$(1) \quad \mathbf{Y}_{ij1} = \alpha + \beta \mathbf{Y}_{ij0} + \gamma \mathbf{X}_{ij} + \delta \mathbf{T}_{ij} + \boldsymbol{\mu}_j + \boldsymbol{\epsilon}_{ij}.$$

Here, $\mathbf{Y}_{ij1}$ and $\mathbf{Y}_{ij0}$, respectively, represent the endline and baseline values of the outcome variable of interest for household i in location j. For outcome indicators collected only at endline, a constrained version of equation (1) is used, which does not include the baseline outcome value. Vector $\mathbf{X}_i$ captures participant characteristics (such as age, gender and level of education) as well as household characteristics (such as household size, land holdings, and the homestead's distance to the reforestation site). Treatment status is captured by $\mathbf{T}_{ij}$, taking value one if a household was offered the opportunity to participate in the PES scheme on reforestation site j, and zero otherwise. Randomization into treatment and control was at the level of the individual reforestation site, and hence we also include reforestation site fixed effects, as represented by $\boldsymbol{\mu}_j$. Finally, $\boldsymbol{\epsilon}_{ij}$ is the mean-zero error term, clustered at the maintenance group level.[10]

We rely on the ANCOVA approach because it can increase the statistical power of the test compared to a difference-in-difference approach, especially when the outcome variables have low autocorrelation (McKenzie 2012). It also presents an advantage when the baseline and follow up data are collected in different seasons, so that Y_{ij0} and Y_{ij1} can differ even if nothing had changed. For example, in our case, the baseline data were collected in September/October 2017, before harvest, whereas the endline data were collected in November/December 2018, after harvest.

In equation (1), δ is the main parameter of interest, which under exogeneity and stable unit treatment value (SUTVA) assumptions identifies the ITT effects on beneficiaries' food security outcomes. The exogeneity assumption is a direct implication of the randomized selection of beneficiaries among eligible candidates. As for the SUTVA assumption, we cannot fully rule out the possibility of spillover effects affecting our estimates. In fact, 90% of our sample respondents live in villages where there is least one representative from each treatment group. The SUTVA assumption might be violated if treatment group participants share their transfers with other members of the community, including the members of the control group (Boltz, Marazyan, and Villar 2019). We do not find any evidence of such social redistribution. More specifically, we find no statistically significant differences in food expenditures of control group households that reside in villages with

[9]The reforestation project's endline survey probed respondents regarding the prevalence of free riding. More than 70% of the treatment respondents reported that all four of their peers contributed at about equally to the common task. For another 12% of the treatment sample, three out of the four peers contributed at least as much as the respondent interviewed. That means that free riding seemed not to have been very prevalent.

[10]Although the treatment assignment was at the individual level, we cluster the standard errors at maintenance group level to account for the correlation of outcomes within treatment groups—if any. Outcomes may be correlated because participants in the same maintenance group had to work together towards a common goal, and also received the same amount of PES payments. For consistency, we also apply clustering to the members of the control group for each of the reforestation plots, even though no special ties (are likely to) exist between them. In any case, clustering does not affect our results and are available upon request.

fellow villagers participating in the PES project compared to those without; see table A2 in the online supplementary appendix for details. Most of the mechanisms we can think of (ranging from payment sharing within villages to sabotage) would result in our measured treatment effects to be a lower bound estimate of the true impacts of participation in the PES scheme. But of course we cannot fully rule out other potential sources of spillover that might result in an upward or downward bias of the treatment estimates.

We estimate equation (1) using ordinary least squares (OLS) and implement several robustness checks. We first use randomization inference as an alternative to *t*-test inferences based on sampling variation. The randomization inference approach, still not very widespread in economic research, has been strongly recommended for the analysis of randomized experiments such as ours (Imbens and Wooldridge 2009; Athey and Imbens 2017). To calculate the *p*-value associated with the estimated treatment effect, the randomized inference approach reassigns the treatment status by keeping the number of individuals in each group fixed and calculating the corresponding difference in means. This process is repeated many times and produces a distribution of "fake" treatment effects under the null hypothesis. The *p*-value is then simply the proportion of cases in which the "fake" treatment effect estimated is at least as large in absolute value as the estimate we seek to test. The method is attractive in part because it relies on fewer modeling assumptions that are (implicitly) used in a regression framework and leads to inferences that are robust to any clustering bias (Athey and Imbens 2017). Additional robustness tests include corrections for multiple hypothesis testing to account for the increasing likelihood of rejecting null hypotheses as the number of tests increases (Kling, Liebman, and Katz 2007; Anderson 2008), as well as Lee bounds, to show that our results are robust to potential attrition bias (Lee 2009).

Data and Descriptive Statistics

As shown in figure 1, this study relies on two rounds of primary household survey data: (*a*) a baseline survey implemented just after participants had been allocated to either of the two treatment arms, and (*b*) an endline survey implemented 5–6 after the PES transfers were distributed.

Baseline Characteristics

In September to October 2017, right after the PES contracts were signed with participants, we administered the baseline household survey.[11] The baseline survey instrument captured households' socio-demographic characteristics, assets ownership, agricultural production, non-farm economic activities, as well as food consumption expenditures.

Table 1 summarizes the baseline characteristics of our survey participants (and their households) in both the treatment and control group, as well as the tests comparing the two groups. This table shows that the participants in our RCT come from the country's poorest and most vulnerable groups. About 90% of them cited farming as their primary source of income, from which they had earned an average of $30 over the last thirty days prior to the survey to support a family of, on average, twelve members.

Balancing Tests

Table 1 presents the characteristics of our participants and their households at baseline. The table suggests that the treatment and control groups are well-balanced with respect to most characteristics. More importantly, there is no significant difference, at conventional statistical thresholds, between treatment and control, at baseline, for our key food security and food consumption outcome variables. The covariates showing an imbalance at baseline are the indicator variables for the respondent being head of her household, whether the homestead is considered far from the reforestation site in the forest, as well as the age, gender, and marital status of the respondent. Although those differences appear statistically

[11]Unfortunately, logistical challenges and financial constraints prevented us from implementing the baseline survey prior to the treatment allocation. That means that treatment assignment was completely random and not stratified on observable characteristics thought to be of key importance for the outcomes of interest. This is not of great concern as stratification is not very effective if the number of stratification variables is relatively large compared to the number of units to be allocated at the same time (as the number of singletons then becomes quite large), and the non-stratified approach (just a simple lottery) is also more transparent to the participants. That treatment allocation may have resulted in biased survey answers is potentially a greater concern, but then the balance tests (see table 1) do not really show important imbalances between the treatment and control group either.

Table 1. Baseline Characteristics and Balance Tests

Variable	(1) PES N	(1) PES Mean/SE	(2) Non-PES N	(2) Non-PES Mean/SE	(3) Total N	(3) Total Mean/SE	T-test difference (1)–(2)	Normalized difference (1)–(2)
Age of respondent	325	39.871	305	38.433	630	39.175	1.438*	0.133
		(0.597)		(0.621)		(0.431)		
Female respondent (1/0)	325	0.123	305	0.193	630	0.157	−0.070**	−0.193
		(0.018)		(0.023)		(0.015)		
Respondent is the head of the household (1/0)	325	0.680	305	0.577	630	0.630	0.103***	0.213
		(0.026)		(0.028)		(0.019)		
Agriculture as primary activity (1/0)	325	0.908	305	0.885	630	0.897	0.022	0.074
		(0.016)		(0.018)		(0.012)		
Annual revenue from primary activity (LOG)	325	3.057	305	3.064	630	3.061	−0.007	−0.001
		(0.411)		(0.415)		(0.292)		
Annual total revenue (LOG)	325	9.893	305	9.927	630	9.909	−0.035	−0.004
		(0.526)		(0.531)		(0.374)		
Secondary activity at baseline (1/0)	325	0.514	305	0.489	630	0.502	0.025	0.051
		(0.028)		(0.029)		(0.020)		
Married (1/0)	325	0.905	305	0.862	630	0.884	0.042*	0.132
		(0.016)		(0.020)		(0.013)		
Household size	325	13.286	305	12.666	630	12.986	0.621	0.080
		(0.405)		(0.467)		(0.308)		
Some formal schooling (1/0)	325	0.182	305	0.190	630	0.186	−0.009	−0.022
		(0.021)		(0.023)		(0.016)		
Member of a forest management group (1/0)	325	0.588	305	0.538	630	0.563	0.050	0.101
		(0.027)		(0.029)		(0.020)		
Homestead far from the reforestation site (1/0)	325	0.877	305	0.944	630	0.910	−0.067***	−0.235
		(0.018)		(0.013)		(0.011)		
Asset Index (PCA)	325	0.129	305	−0.137	630	0.000	0.266	0.111
		(0.136)		(0.134)		(0.096)		
Household landholdings (ha)	325	5.022	305	4.910	630	4.967	0.112	0.021
		(0.304)		(0.293)		(0.211)		
Land area cultivated (ha)	325	3.928	305	3.899	630	3.914	0.029	0.008
		(0.201)		(0.206)		(0.144)		
Total (nominal) food consumption expenditures in FCFA (past 7 days)	325	11258.837	305	11602.666	630	11425.294	−343.829	−0.028
		(639.395)		(729.457)		(482.895)		
	311	8.908	300	8.875	611	8.891	0.033	0.029

(Continues)

Table 1. Continued

	(1) PES		(2) Non-PES		(3) Total		T-test	Normalized
Variable	N	Mean/SE	N	Mean/SE	N	Mean/SE	difference (1)–(2)	difference (1)–(2)
(LOG) Total (nominal) food consumption expenditures in FCFA (past 7 days)		(0.060)		(0.068)		(0.045)		
Household dietary diversity score (HDDS)	325	4.425	305	4.466	630	4.444	−0.041	−0.029
		(0.080)		(0.082)		(0.057)		
F-test of joint significance (*p*-value)							0.326	
F-test, number of observations							611	

Notes: The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are p-values. ***, **, and * indicate significance at the 1, 5, and 10% critical level.

significant based on *t*-tests, the actual differences are quite small. The normalized differences, generally preferred to *t*-tests because they provide a scale-free comparison (Imbens and Wooldridge 2009; Abadie and Imbens 2011; Imbens and Rubin 2015), are all below 0.25. Moreover, the *F*-test for joint orthogonality of the covariates, reported at the bottom of the balancing table, fails to reject the null hypothesis of no significant difference between treatment and control groups ($p = 0.33$). This joint test is also preferred to the individual *t*-tests for ensuring the validity of the randomization process Bruhn and McKenzie (2009). Finally, we also ran an alternative balance test by using a probit model to regress treatment status on all baseline characteristics (again see Bruhn and McKenzie (2009)). The relevant Chi2 test yields a *p*-value 0.33 (see table A3 in online supplementary appendix).

Although we find no robust evidence of selection bias (or, for that matter, for strategic misrepresentation bias; see footnote 11), our econometric analysis includes regression specifications with additional covariates, including the few covariates that failed the balance tests. We follow Bruhn and McKenzie (2009) and choose to add all available covariates that are prognostic of our outcomes of interest, regardless of whether they show a significant difference, at baseline, between treatment and control. The list of covariates deemed relevant is added at the bottom of each regression table and consists of almost all the variables in our balancing table (table 1). We find generally that our results are robust to the addition of covariates.

Effort and Attrition

The endline survey was administered in November/December 2018, five to six months after the monetary transfers were disbursed to the PES participants, and about one to two months after the new harvest. This follow-up survey tracked the same households interviewed at baseline, using a very similar instrument with an identical survey module for our key outcome variables.

During the endline survey, we were able to successfully interview 574 out of the 630 baseline respondents. The remaining ones could not be interviewed mostly due to (seasonal) migration reasons. This corresponds to an overall attrition rate of 9%, fairly modest considering the challenging context in Burkina Faso.[12] However, the difference in attrition rates between treatment and control groups is 4.4 percentage points, and this difference is statistically significant at the 10% level (see table A4 in the online supplementary appendix for details).

As argued by Ghanem, Hirshleifer, and Ortiz Becerra (2019), differential attrition is not the main threat to internal validity; it is selective attrition. We therefore test for selective attrition in our sample. We do so by evaluating the extent to which observable baseline characteristics differ between treatment and control non-attritors (the endline respondents), and between treatment and control attritors. Table A5 in the online supplementary appendix summarizes these pairwise comparisons. The *F*-test of joint significance of the pairwise differences yields a *p*-value of 0.29 for the comparison of treatment and control non-attritors, suggesting no significant difference between the two groups. This is rather comforting as it implies that the remaining sample, after attrition, is still fairly balanced. Similarly, using an *F*-test to compare treatment and control attritors yields a *p*-value of 0.33, suggesting the two groups are also fairly balanced.

We conclude that selective attrition is not likely to be an issue for the validity of our treatment effects estimates. Nevertheless, we include Lee bounds (Lee 2009) as a robustness check in our analysis to ensure our estimation results are not affected by potential attrition bias.

Main Outcomes of Interest

Our primary outcome of interest is food security. According to the 1996 World Food Summit, food security exists when all people, at all times, have physical and economic access to sufficient, safe, and nutritious food to meet their dietary needs and food preferences for an active and healthy life (FAO 1996). Food security is thus a multidimensional concept involving the availability, access, utilization, and stability of food consumption. No single indicator exists that captures all four dimensions of food security. Therefore, a combination of indicators is often recommended for assessing food security in any given context (Carletto, Zezza, and Banerjee 2013; Maxwell, Vaitla, and Coates 2014).

In this study, we focus mostly on the food access dimension, which is, arguably, the most important dimension for food security measurement at the household or individual level and also incorporates the availability dimension. We use the following common household level food security indicators, which capture broadly dietary diversity and household's behaviors or experiences, widely considered to be symptomatic of insecure food access (Carletto, Zezza, and Banerjee 2013; Maxwell, Vaitla, and Coates 2014). These are: (*a*) household food consumption expenditures, including both home-produced and purchased food items in the past seven days before the survey; (*b*) household dietary diversity score (HDDS), captured the number of food groups consumed by the household in the seven-day period before the survey; as well as (*c*) household food insecurity access scale (HFIAS) and (*d*) household hunger scale (HHS). The HFIAS and the HHS are both covering self-reported occurrences and frequencies of events over a last thirty-day recall period, which are indicative of limited food access and diversity for the household of the respondent. We provide a more detailed description of how we compute each of those indicators in the online supplementary appendix A. For a comparative assessment on the relative strengths and weaknesses of the most common food security indicators, see Carletto, Zezza, and Banerjee (2013).

We also explore the mechanisms underlying the observed impacts on food security. For this purpose, we focus on agricultural production indicators such as land cultivated (in hectares), inputs use (including seeds, fertilizers, pesticides, and hired labor), and the value of the agricultural production. We extend the study of mechanisms to household income from the participant's primary economic activity, overall income, as well as the likelihood of her engaging in a secondary economic activity.[13]

[12] More than 50% of the top journal papers reviewed by Ghanem, Hirshleifer, and Ortiz Becerra (2019) report an overall attrition above 10%.

[13] The indicators included in the treatment effects estimation do not include environmental outcomes such as tree survival rates. Due to small sample concern and the reluctance of the government team to have reforestation plots without maintenance contracts, we could not set aside pure control reforestation plots where no PES contracts would have been signed. For that reason, we could not credibly capture the impacts of the PES on tree survival rates. It is also true that the environmental impacts of PES schemes are fairly established in the available literature (Borner et al. 2017; Jayachandran et al. 2017); and that it is unlikely that people would have traveled to the forests and invested time and effort in keeping trees alive on degraded protected forest land without any financial incentives.

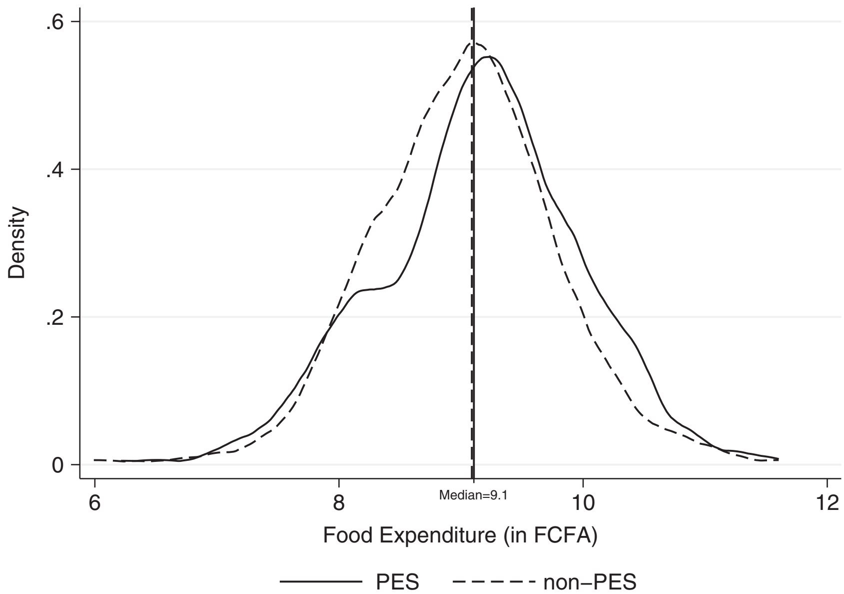

Figure 3. (Log) food consumption expenditures during the past seven days

Note: The (log) food consumption expenditure covers the household's expenditures on cereal and tubers, pulses, vegetables, fruits, meat and fish, milk, oil, sugar as well as expenses for other items such as tobacco, alcohol and condiments, during the last seven days prior to the survey.

The Food Security Impacts of PES Participation

In this section, we present our estimation results for each outcome of interest. We begin by presenting non-parametric evidence of the relationship between the participation in the PES scheme and food security outcomes. Given exogenous variation in treatment assignment, these non-parametric estimates are unbiased for the intention-to-treat effects of interest and not expected to be influenced by confounding factors. Nevertheless, our preferred specification remains the parametric estimation, including the respective baseline outcome as a control variable, to improve the precision of our estimates.

Food Consumption Expenditures

Figure 3 presents the kernel density distribution of the reported household food consumption expenditures (in logarithms) during the last seven days prior to the survey for individuals in treatment and control group respectively. Although food is not typically scarce in absolute terms in the period in which the endline survey is implemented (one or two months after the end of the harvesting period, see figure 2), food expenditures are still reflective of differences in welfare outcomes. We find that the treatment affected positively food consumption expenditures; the distribution of the weekly food consumption expenditures for the treatment group is shifted to the right compared to the control group. The point estimates of the treatment effects from equation (1) are summarized in table 2. The results indicate an increase in weekly household food consumption expenditures of 12.8% or 1798 FCFA ($p < 0.05$). These results are robust to the inclusion/exclusion of additional covariates as well as baseline outcomes. However, when we run the same model using per capita household consumption expenditures, the average treatment effect drops to 6.4% or 132.4 FCFA, and this impact is not significantly different from zero (see table A6 in the online supplementary appendix).

Household Dietary Diversity

Did the Burkina Faso reforestation PES scheme improve the dietary diversity of participants' households? Figure 4 presents a bar graph of the seven-day recall food consumption expenditure values for each food group, separately for the participants and nonparticipants in the PES scheme. The graph suggests that, in general, cereals, roots and

Table 2. The Impact of PES Participation on Food Consumption Expenditures

	Food expenditures			log(Food expenditures)		
	(1)	(2)	(3)	(4)	(5)	(6)
PES	1780.8**	1798.1**	1558.8*	0.119**	0.128**	0.107**
	(806.9)	(815.6)	(876.0)	(0.0485)	(0.0507)	(0.0519)
Baseline outcome		0.0317	0.0226		0.0446	0.0492
		(0.0359)	(0.0410)		(0.0344)	(0.0327)
Constant	7126.2***	6856.1***	2290.8	8.619***	8.231***	8.109***
	(1493.8)	(1574.0)	(6393.0)	(0.167)	(0.341)	(0.565)
Observations	574	574	574	574	556	556
R^2	0.231	0.232	0.246	0.341	0.336	0.359
Adjusted R^2	0.183	0.182	0.174	0.299	0.291	0.296
Covariates included	No	No	Yes	No	No	Yes
Reforestation site fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Control mean			11097.08			9.02

Notes: Standard errors in parentheses.
We cluster standard errors at group level.
The baseline outcomes represent the food expenditure and log-food expenditure at baseline.
The covariates included are the age and gender of the respondent, the marital status, the role of the respondent in the household, whether the respondent has a formal level of schooling, the household size, the primary economic activity, the annual revenue from the primary activity, the total annual revenue, whether the respondent has a secondary occupation, the landholdings, the land area cultivated, assets, whether the respondent is a member of forest management group, and whether the distance from the homestead to the reforestation site is far.
All covariates are baseline values.
*$p < 0.10$.
**$p < 0.05$.
***$p < 0.01$.

tubers account for more than 50% of a household's food expenditures for both the treatment and the control group. This is consistent with the relatively low HDDS observed at baseline for both groups (HDDS = 4.4 at baseline).

The diversity in food consumed is thus fairly low. We also find that there is no significant impact of the PES payments on the HDDS. This is already implied by the treatment differences presented in figure 4. Expenditures on each food group appear higher in the treatment group than in the control group, but the distribution is not really different. This conclusion is confirmed by figure A7 in the online supplementary appendix, which presents the distribution of HDDS in the treatment and control groups. The treatment differences are negligible, and the *p*-value for the Kolmogorov–Smirnov test of equality of the two distributions is 0.99. Participation in the PES scheme allowed for higher food consumption expenditures, but the income elasticities of the various food groups are not sufficiently different to generate a more diverse consumption pattern. These conclusions are corroborated by the regression results in table 3 showing no evidence of a significant treatment effect on HDDS, in all three regression specifications.

We thus find that although there is a positive and significant effect of the PES transfers on households' overall food consumption expenditures, the effect did not translate into an improved dietary diversity. To assess whether these results are contradictory, we investigate the main food items for which the consumption expenditures were affected, by comparing the mean expenditures on each food group, between treatment and control. Based on *p*-values from both *t*-tests and randomization inference tests, we conclude that the additional income received through the PES transfers significantly increased households' expenditures on starchy foods (cereals and tubers), pulses, vegetables, as well as meat and fish, albeit only at the intensive margin (see table A8 in the online supplementary appendix). We find no evidence of PES participation effecting the extensive margin—the likelihood of households' starting to consume food from a food group they would otherwise not consume. This could indicate a limited availability of a diverse range of food items in local markets, a limited diversity in own production by primarily subsistence farmers, a limited knowledge of recommended nutrition practices, or a combination of all three (Hirvonen et al. 2017; Hirvonen and Hoddinott 2017; Dillon, Arsenault, and Olney 2019; Headey et al. 2019). This issue is the main motivation behind most integrated agriculture and nutrition programs.

Household Food Insecurity Access Scale and Household Hunger Scale

Table 4 summarizes the OLS estimates of the treatment effects on food security captured by the Household Food Insecurity Access Scale (HFIAS), with a 30-day reference period. The table reports the results for three alternative transformations of the HFIAS: the overall score in columns 1 and 2; the binary indicator for being food insecure at any level (mild, moderate, or severe) in columns 3 and 4; and the binary indicator for being severely food insecure in columns 5 and 6. The conclusions are consistent across all those regressions.

The results in the table consistently indicate that the Burkina Faso PES scheme helped to significantly reduce households' food insecurity. The overall HFIA score was reduced by about 0.6 points—a 40% reduction relative to the control group. In columns 3 and 4, we see a 14 to 15 percentage points decrease in the likelihood of being food insecure at any level. In columns 5 and 6, we observe a 9 percentage point reduction in the likelihood of being severely food insecure. All reported impacts are significant at the 1% level, or better. These results are illustrated graphically in figure 5. Relative to the control group, these estimates amount to a 35 and 60% reduction in food insecurity and severe food insecurity, respectively.

Moreover, the estimation results, using HHS as outcome variable, are very consistent with the impacts measured on HFIAS. They suggest that participation in the PES scheme led to a 66% reduction in the likelihood of being food insecure at the moderate or severe levels (see table A9, in the online supplementary appendix, for further details).

Overall, these strong results on food security five to six months after the payments transfers, and even during the post-harvest period, suggest that the PES transfers had lasting effects on the recipient households. These results are even more striking given that the figure 2 suggests a quasi-absence of hunger during the post-harvest period, based on self-reported food insufficiency.[14] Nevertheless, based on the HFIAS, we find the rates of food insecurity and severe food insecurity, to be 40% and 15%, respectively, in the control group. This stresses the fact that agricultural households still can, and often do, experience food insecurity even during the post-harvest

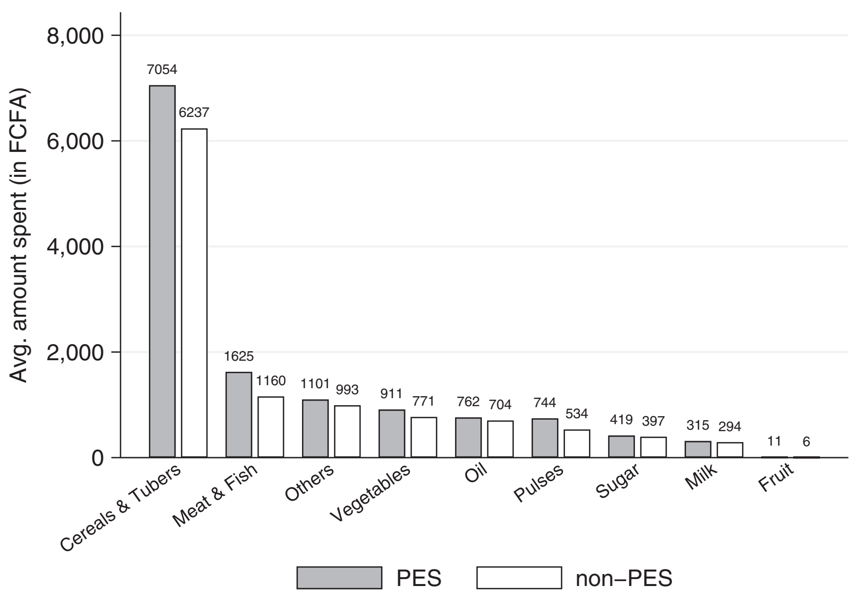

Figure 4. Average food expenditure by food groups (reported at endline)

Note: We display the average food expenditures (seven-day recall) by food group and treatment status.

Table 3. The Impact of PES Participation on the Household Dietary Diversity Score (HDDS)

	HDDS		
	(1)	(2)	(3)
PES	0.0282	0.0289	0.00989
	(0.0826)	(0.0830)	(0.0819)
HDDS at baseline		0.0120	0.00564
		(0.0433)	(0.0470)
Constant	2.005***	1.954***	2.333***
	(0.162)	(0.247)	(0.608)
Observations	574	574	574
R^2	0.550	0.550	0.565
Adjusted R^2	0.522	0.521	0.524
Covariates included	No	No	Yes
Reforestation site fixed effects	Yes	Yes	Yes
Estimation approach	OLS	OLS	OLS
Control mean			3.79

Notes: Standard errors in parentheses.
We cluster standard errors at group level.
The covariates included are the age and gender of the respondent, the maritial status, the role of the respondent in the household, whether the respondent has a formal level of schooling, the household size, the primary economic activity, the annual revenue from the primary activity, the total annual revenue, whether the respondent has a secondary occupation, the landholdings, the land area cultivated, assets, whether the respondent is a member of forest management group, and whether the distance from the homestead to the reforestation site is far.
All covariates are baseline values.
$^{***}p < 0.01$.

period, especially if pre-harvest constraints push them to make choices that end up reducing their production. The fact that the figure 5 and the figure 2 seem contradictory with respect to the prevalence of food insecurity in the months of November and December also confirms the point made by Carletto, Zezza, and Banerjee (2013) that alternative food security indicators do not always agree. It is possible that the direct self-reported measures would underreport food insufficiency due to potential stigma attached to being hungry (Moffitt 1983; Stuber and Kronebusch 2004). These considerations also suggest that our measured treatment effects might have been larger if they were they measured in the lean season when one would expect higher prevalence of food insecurity.

Impact Mechanisms

Having documented the sizeable treatment effects of participating in the PES program (and the payments received as a result), we now investigate the mechanisms of such food security impacts. The results described above suggest that post-harvest households' consumption is affected by binding liquidity constraints during the lean season, which were, at least, partly alleviated by the timely PES transfers received. We investigate the potential channels of impacts from various angles of our data. Overall, we find suggestive evidence that the transfers received were allocated in part to immediate consumption. In addition, we also find that the recipients were able to cultivate more land and invest more in agricultural inputs. This in return led to higher agricultural outputs, higher income and therefore improved food security of the beneficiary households in the post-harvest period. Moreover, we find some evidence that a few recipients (4%) had saved part of the payments, up until the time of the endline survey.

Self-Reported Use of the PES Transfers

First, at the time of the payments, recipients were asked to report the three main intended uses for the PES transfers. An overview of the responses (see table 5, column 1) shows food consumption (mentioned by 39% of the respondents) at the top but also includes the purchase of agricultural inputs (29%) and investments

[14] Bellemare and Novak (2017) defines the hungry season as the months during which the members of the household go without three meals per day. The post-harvest period is not typically considered a hungry period because most farmers are expected to still have a food reserve from the previous harvest.

Table 4. The Impact of PES Participation on the Household Food Insecurity Access Scale (HFIAS)

	Overall scale $\in [0,4]$		Food insecure(0/1)		Severely food insecure (0/1)	
	(1)	(2)	(3)	(4)	(5)	(6)
PES	-0.607^{***}	-0.567^{***}	-0.148^{***}	-0.136^{***}	-0.0934^{***}	-0.0894^{***}
	(0.192)	(0.194)	(0.0424)	(0.0441)	(0.0312)	(0.0307)
Constant	0.800^{***}	0.197	0.405^{**}	0.396	0.0625^{*}	-0.140
	(0.263)	(1.278)	(0.168)	(0.355)	(0.0350)	(0.128)
Observations	574	574	574	574	574	574
R^2	0.184	0.201	0.089	0.099	0.283	0.304
Adjusted R^2	0.132	0.126	0.032	0.014	0.238	0.239
Covariates included	No	Yes	No	Yes	No	Yes
Reforestation site fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Control mean		1.517		0.406		0.151

Notes: Standard errors in parentheses.
We cluster standard errors at group level.
The covariates included are the age and gender of the respondent, the marital status, the role of the respondent in the household, whether the respondent has a formal level of schooling, the household size, the primary economic activity, the annual revenue from the primary activity, the total annual revenue, whether the respondent has a secondary occupation, the landholdings, the land area cultivated, assets, whether the respondent is a member of the forest management group, and whether the distance from the homestead to the reforestation site is far.
All covariates are baseline values.
$^{*}p < 0.10$.
$^{**}p < 0.05$.
$^{***}p < 0.01$.

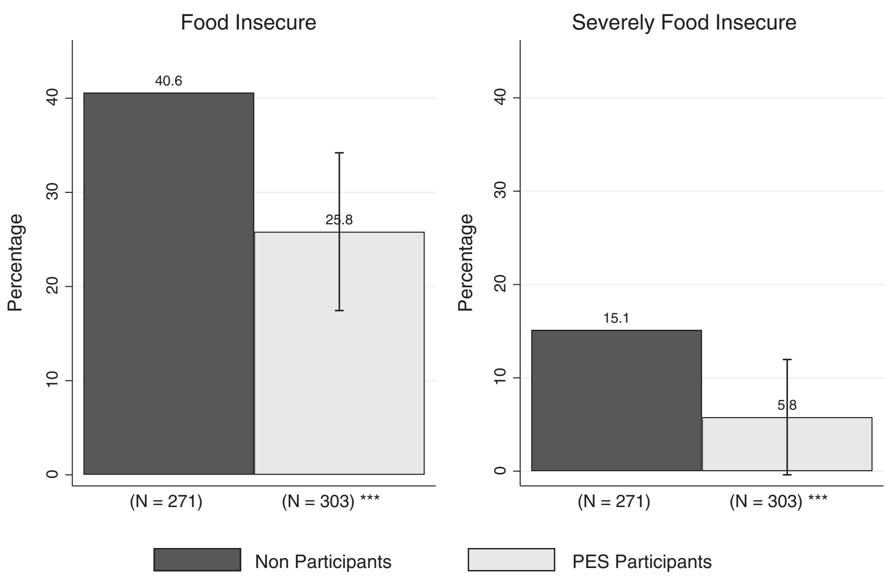

Figure 5. PES and food security

Note: The figure presents, on the left, the share of PES participants and non-participants that are food insecure at any level (mild, moderate, and severe) at endline; and, on the right, the share of those that are severely food insecure, taking into account the treatment effect. Although 40% of the non-participants are food insecure, only about 26% of the PES participants are so too. Furthermore, 15% of the non-participants are severely food insecure, compared to about 6% of the participants. This means that we observe treatment effect sizes of 35–60% in the reduction of the prevalence of mild to severe food insecurity.

into livestock production (16%), among the main intended uses reported by the respondents.

At endline, we asked recipients how they actually spent the money received and if they still had any left. Table 5, column 2 shows that the money was used by a large share of respondents as intended at baseline: 37% stating that they spent some of the money on food. In

Table 5. Intended (ex ante) and Reported (ex post) Usage of the PES Payments

	(1) Intended use	(2) Reported use	(3) Shares spent
Food	0.39	0.37	0.28
Other family expenses	0.31	0.09	0.07
Agricultural inputs	0.29	0.16	0.12
Livestock production	0.16	0.22	0.13
Investments in transport/mobility	0.15	0.10	0.05
Clothing	0.06	0.09	0.05
Cosmetic products	0.05	0.01	0.00
Medication	0.03	0.06	0.02
School fees	0.02	0.14	0.11
Savings			0.04
Observations	330	303	289

Note: In column (1) we summarize the share of PES participants who stated at the time of the disbursement to intend to spend the transfers on the respective items. Participants were able to enumerate up to three items. We display the actual use reported by the participants at endline, as the proportion of respondents mentioning the item (column (2)) and as share of the transfer spent on it (column (3)).

addition, also 22% and 16% reported to have used part of the money for investments in agricultural inputs and livestock production. Investments in transport mobility were mentioned by 10% of the respondents.

A similar pattern emerges from the amount spent on each item as a reported share of the total PES transfer. The responses, summarized in table 5, column 3, indicate that the largest share of the amount received was spent on direct food consumption (28% of the payment). But investments in transport, agricultural inputs, and livestock production also took up substantial shares of the payment received. It is also noticeable that, about 4% of the transfer amount was on average still reported as unspent savings by the respondent.

We confirm these self-reported usages with a set of econometric analyses.

PES Impacts on Agricultural Production

We explore the effects of the PES payments on agricultural production using the same econometric model as in equation (1). Here, the variables and parameters are defined as earlier, except that the outcome variables Y_{ij1} capture households' agricultural production at endline (land cultivated, value of production) and investments in inputs (improved seeds, fertilizers, pesticides, hired labour). For the effects on land cultivated and the value of agricultural production, we use the logarithmic transformation of the outcome variables on the left-hand side and estimate the regressions' parameters using OLS. For the treatment effects on the household's expenditures in each agricultural input, we estimate a linear probability model (using standard OLS) for the binary input use response function. We use tobit regressions to estimate the treatment effects on the actual expenditures in agricultural inputs (in FCFA) conditional on purchasing a positive quantity.[15] In each regression model, we control for baseline outcomes and adjust standard errors for clustering at the group level. We report the results both with and without additional covariates, just as we did in the main results on food security and consumption expenditures outcomes.

The results are summarized in table 6. The first panel of the table shows the results on investments in improved seeds. The first two columns of the panel report the results of the estimation of the linear probability model of the use of improved seeds, estimated with OLS, with and without additional covariates. The last two columns of the panel show the marginal effect estimates of a tobit regression model for the actual expenditures on improved seeds given non-zero improved seeds use, again with and without additional covariates. The second, third, and fourth panels of table 6 present the results for fertilizers, pesticides, and hired labor, using the same structure as the first panel. The two

[15]Tobit regressions are appropriate for corner solutions responses. See Wooldridge (2010) for further details on tobit models. In our cases, zero is a corner solution for variables capturing the expenditures in each agricultural input.

columns of the fifth panel of the table present the OLS estimation results of the linear model of land cultivated (log transformed), with and without additional covariates. Finally, the two columns of the sixth and last panel present the same results, but for agricultural output, captured by the total estimated value of the agricultural production during the ongoing season (log transformed).

The results suggest that participation in the PES scheme had positive and significant impacts on investments in improved seeds, on both the extensive and intensive margins. The PES treatment increased the likelihood of using improved seeds by about six percentage points (significant at the 10% level). The treatment also increased the conditional expenditures on improved seeds by about 885 FCFA (significant at the 5% level)—a 48% increase relative to the control group. As for pesticides, we fail to detect a significant treatment effect on the likelihood of their usage—the extensive margin. However, we do find that the PES transfers led to a significant increase in pesticide investments of about 2310 FCFA, conditional on purchase. This represents about 15% increase relative to the control group. Next, we find a 17% increase in the total area cultivated (significant at 1%) and also a 17% increase in the value of agricultural production—albeit that the significance of the latter impact is attenuated when controlling for baseline covariates. We find no effect on investments in chemical fertilizers nor hired labor.

PES and Household Income

Next, we explore the (direct and indirect) effects of the PES treatment on household income. We do so by estimating equation (1) using OLS, but with the following outcome variables: (*a*) households' estimated primary income for the past twelve months (FCFA), (*b*) households' estimated total income for the past twelve months (FCFA), and (*c*) a binary indicator for whether the respondent had a secondary income source. For the two income variables, we use the inverse hyperbolic sine (IHS) transformation on the left-hand side of the relevant regressions (Bellemare and Wichman 2020).[16] Again, all regression models control for baseline outcome values, and their standard errors are adjusted for clustering.

The results are summarized in table 7. The first panel contains two columns, which present the estimates of the treatment effects on the primary income, with and without covariates inclusion. Similarly, the second panel presents the results for total household income. The last panel of the table presents the results for the likelihood of having a secondary occupation.

The results suggest that participation in the PES scheme led to 34% increase in household's primary income and a 26% increase in household's total income for the past twelve months. The results are robust to the addition of additional covariates, and the estimated coefficients are significant at 1% or better in all cases. It is worth noting that agriculture is the primary income source for more than 90% of the respondent in our sample.

The last panel of table 7 suggests that the increase in income does not come only from increase in agricultural revenues but also from livelihood diversification. Participation in the PES scheme led to a seven percentage points increase in the likelihood of the respondent's having a secondary occupation. The most common secondary occupation reported by respondents is livestock production.

The econometric results and the self-reported uses of the PES transfers by the recipients are highly consistent with each other. Taken together, these results suggest, first, that participation in the PES scheme did not affect negatively other productive activities, as found by Jack and Santos (2017). This may be explained by the fact that the maintenance work of the trees was during the dry season and therefore did not compete for labor with the main income generating activities (agriculture and livestock), which require labor investments in the rainy season. Secondly, participation in the PES scheme, and the transfers received, seem to have released financial constraints for the respondents at the right time, allowing them to not only satisfy immediate consumption needs during the lean season but also to invest in productive activities (agriculture and livestock), which led to increased income. Although our data do not allow us to explicitly test this, it is likely that, by being able to meet urgent consumption needs, the recipients were also able to avoid negative coping strategies in the lean season.

[16]The IHS is commonly used to approximate the natural logarithm of a variable while retaining zero-valued observations.

Table 6. The Impact of PES Participation on Agricultural Investments

	Improved seeds				Chemical fertilizer				Pesticides				Agricultural labor hired				Land cultivated		Value ag production	
	Used (0/1)		Amt. invested (FCFA)		Used (0/1)		Amt. invested (FCFA)		Used (0/1)		Amt. invested (FCFA)		Used (0/1)		Amt. invested (FCFA)		(log transformed)		(log transformed)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
PES	0.0645^{*}	0.0580^{+}	885.2^{**}	849.0^{**}	−0.0331	−0.0452	786.5	216.4	0.0343	0.0227	2310.7^{*}	2077.1^{*}	0.0000168	−0.00604	991.5	507.9	0.191^{***}	0.135^{**}	0.169^{**}	0.104
	(0.0355)	(0.0363)	(427.3)	(429.3)	(0.0406)	(0.0403)	(1605.9)	(1610.7)	(0.0404)	(0.0408)	(1262.1)	(1237.8)	(0.0358)	(0.0354)	(1707.7)	(1713.1)	(0.0592)	(0.0595)	(0.0739)	(0.0755)
Constant	0.620^{***}	0.219			0.737^{***}	0.386			0.781^{***}	0.557^{*}			0.159^{*}	0.165			0.951^{***}	−0.122	12.71^{***}	11.75^{***}
	(0.0519)	(0.338)			(0.0854)	(0.390)			(0.147)	(0.313)			(0.0948)	(0.321)			(0.142)	(0.560)	(0.224)	(0.579)
Observations	473	473	473	473	473	473	473	473	473	473	473	473	473	473	473	473	473	473	556	556
Covariates included	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Reforestation site fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline outcome included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Model specification	OLS	OLS	Tobit	Tobit	OLS	OLS	Tobit	Tobit	OLS	OLS	Tobit	Tobit	OLS	OLS	Tobit	Tobit	OLS	OLS	OLS	OLS
Control mean		0.28		1859.28		0.59		16390.15		0.64		15374.43		0.41		10791.29		1.06		12.58

Notes: Standard errors in parentheses.
We cluster standard errors at group level.
The covariates included are the age and gender of the respondent, the marital status, the role of the respondent in the household, whether the respondent has a formal level of schooling, the household size, the primary economic activity, the annual revenue from the primary activity, the total annual revenue, whether the respondent has a secondary occupation, the landholdings, the land area cultivated, assets, whether the respondent is a member of forest management group, and whether the distance from the homestead to the reforestation site is far. All covariates are baseline values.
$^{+}p < 0.15$.
$^{*}p < 0.10$.
$^{**}p < 0.05$.
$^{***}p < 0.01$.

Table 7. The Impact of PES Participation on Income and Livelihood Diversification

	Primary income last 12 months (IHS transformed)		Total income last 12 months (IHS transformed)		Respondent has a secondary occupation (1/0)	
	(1)	(2)	(3)	(4)	(5)	(6)
PES	0.344***	0.284***	0.262***	0.212**	0.0718**	0.0762**
	(0.0992)	(0.0891)	(0.0962)	(0.0882)	(0.0356)	(0.0364)
Constant	12.62***	16.25***	13.52***	16.98***	0.612***	0.603*
	(0.156)	(2.451)	(0.173)	(1.408)	(0.107)	(0.307)
Observations	574	574	574	574	574	574
Covariates included	No	Yes	No	Yes	No	Yes
Reforestation site fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Baseline outcome included	Yes	Yes	Yes	Yes	Yes	Yes
Control mean		12.54		13.24		0.56

Notes: Standard errors in parentheses.
We cluster standard errors at group level.
The covariates included are the age and gender of the respondent, the marital status, the role of the respondent in the household, whether the respondent has a formal level of schooling, the household size, the primary economic activity, the annual revenue from the primary activity, the total annual revenue, whether the respondent has a secondary occupation, the landholdings, the land area cultivated, assets, whether the respondent is a member of forest management group, and whether the distance from the homestead to the reforestation site is far. All covariates are baseline values.
*$p < 0.10$.
**$p < 0.05$.
***$p < 0.01$.

Robustness Analyses

The results presented above suggest significant and positive impacts of Burkina Faso's FIP PES scheme on households' food security outcomes and trace the mechanisms of impacts to increased investments and higher production. In this section we perform a series of robustness checks to ensure that those results do not suffer biases related to our analytical procedures.

Randomized Inference Tests

The results presented so far are derived from regression analyses, with inferences based on asymptotic theory and distributional assumptions. In this subsection, we use a randomized inference approach to ensure that our main results are robust to relaxing those assumptions (Imbens and Wooldridge 2009; Athey and Imbens 2017). In table A10 in the online supplementary appendix, we summarize the p-values for each of our outcome variables from the randomized inference tests alongside the p-value of the t-tests used in the main analysis. The t-tests and the randomized inference p-values in the last two columns of the table are very similar, which supports our main results. Based on traditional significance thresholds, the randomization inference p-values lead to the same conclusion as the t-tests—that the PES intervention led to significant improvements in all food security measures, except for the HDDS variable.

Table A11 in the online supplementary appendix reports analogous results for agricultural production and investments, as well as income and livelihood diversification outcomes. Again, the randomized inference results support our conclusion that the PES treatment led to significant and positive effects on agricultural investments in most inputs, agricultural production and livelihood diversification. Even better, although the regression results in table 6 suggest no statistically significant treatment effects on investments in chemical fertilizers, nor hired labor, the randomized inference results suggest significant effects on both—albeit only at the intensive margins.

Multiple Inference Adjustments

The probability of incorrectly finding significant effects increases with the number of hypotheses being tested (Anderson 2008; Athey and Imbens 2017). Failing to correct for multiple inference can lead to misleading policy advice. Given the number of indicators under consideration, in this study, might it be possible that the results obtained were the product of chance?

Overall, it is not very likely that our analysis suffers from (severe) multiple hypothesis bias. In our main analysis, we have tested seven food security outcomes that are likely to be reasonably well-correlated with each other and expected to be affected in the same direction by the treatment. We were able to reject the null hypothesis for all but one food security outcome. Nevertheless, we follow Anderson (2008) and correct for multiple hypothesis testing in two main ways.

First, we apply Bonferroni's adjustment procedure, which estimates the probability that one or more of our outcome "families" of multiple tests is false—the so-called family wise error rate (FWER). It does so by upwardly adjusting the p-value for each of the tests within the family (Savin 1984; List, Shaikh, and Xu 2019). The standardized results, summarized in figure 6, are fully consistent with our main results. All food security indicators (Family 1 or F1) have confidence intervals excluding zero and on the expected side of the zero line, except the HDDS.

Figure 6 also summarizes the same Bonferroni adjustment results for the agricultural investments and production variables (Family 2 or F2) as well as the income and livelihood diversification outcomes (Family 3 or F3), used to explore the mechanisms of impacts. The results, for both families of outcomes, are broadly robust to the Bonferroni adjustments. Only the two variables capturing investments in improved seeds have changed from being significant to nonsignificant.

The drawback of the above approach is that, by adjusting p-values upwards, it effectively reduces the power of any given test. As an alternative, we use the summary index tests (O'Brien 1984), which pools all outcomes into a single test and thereby reduces overtesting, increases power, and provides a test on the general effect of the treatment on the outcomes of interest. We implement Kling, Liebman, and Katz's (2007) approach by combining all our outcomes from each family into one weighted standardized index and estimate the mean standardized effect size. To align all variables in the same direction (capturing food insecurity), we multiply the consumption expenditure variables as well as the HDDS variable by -1; and we subtract the food secure status binary variables from 1. We obtain a point estimate of -0.14 standard deviation and a p-value of 0.006 for the summary index with the food security outcomes only. This confirms that the Burkina Faso FIP PES scheme has reduced food insecurity for the beneficiary households. For the agricultural outcomes, we find a point estimate of 0.14 (p = 0.26). For the income and livelihood diversification outcomes, we get a point estimate of 0.20 (p = 0.005). The summary index results also confirm positive treatment effects on those two classes of outcomes used to explore the mechanisms of impacts on food security.

Dealing with Potential Attrition Bias

To ensure that potential attrition bias did not influence our results, we implement Lee (2009) bounds estimates for our main outcomes of interest.[17] Table A12 in the online supplementary appendix presents the Lee bounds estimation results for the food security outcomes, whereas table A13 and table A14 in the same online supplementary appendix present the Lee bounds estimates for the agricultural outcomes and income variables.

The estimated bounds for the food consumption expenditures indicate positive treatment effects in the range of 7 to 23%, although the lower bound estimate is not significantly different from zero. For the HDDS, neither the lower bounds nor the upper bound is significant, confirming the lack of a treatment effect of the PES scheme on dietary diversity. Finally, all three transformations of the HFIA variables show consistently significant lower and upper bounds treatment effects. This confirms that the impacts of the PES transfers on food security in our sample is quite robust to potential attrition bias.

As for the agricultural production outcomes, the upper bounds broadly agree with our treatment effects results, but the lower bounds effects are generally not significant (table A13). This is also the case for the income and livelihood diversification outcomes (table A14, online supplementary appendix). That means that we cannot rule

[17]The Lee bounds estimator is a non-parametric approach to dealing with attrition. The treatment effect bounds are estimated by trimming the sample, from above or below, so that the share of observed individuals is equal in both the treatment and the control groups. It is similar to—but produces tighter bounds than – Horowitz and Manski (2000)'s bounds. The approach is also preferred to Heckman (1976, 1979)'s parametric selection correction estimator and to the semi-parametric approaches proposed by Ichimura and Lee (1991) and Ahn and Powell (1993), mainly because it relies on fewer assumptions. The main underlying assumptions for Lee bounds estimation are random assignment and monotonicity (Tauchmann 2014).

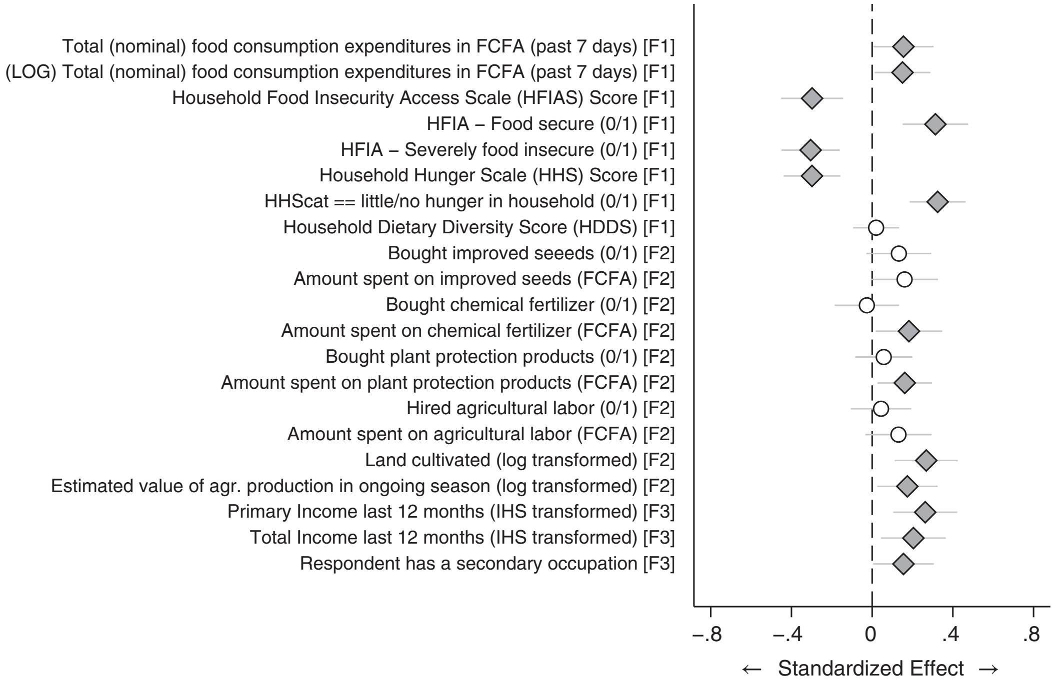

Figure 6. Bonferroni multiple hypothesis adjustment

out that selection bias may have affected our outcomes at least to some extent. However, given that table A5 in the online supplementary appendix suggests that selective attrition is not really an issue, we remain confident in our primary results on the mechanisms of impacts.

Conclusion

We use data on a sample of households across thirty-two communes in Burkina Faso and an experimental design to investigate the causal impacts of participation in a so-called payments for environmental services (PES) scheme on a variety of food security outcomes. The specific intervention of interest is a reforestation campaign in which interested community members were invited to participate; those who ended up in the treatment group were offered PES contracts according to which they would receive monetary payments based on the number of trees still alive at verification—nine months after the implementation. The contracts were allocated to a subset of volunteers via a public lottery. Considering several different food security indicators, we consistently find significant and positive effects of the PES scheme on food security. Participants' households reported 12% higher food consumption expenditures, and as high as a 60% reduction in the likelihood of being severely food insecure (as measured on the HFIA scale). The mechanisms of impacts involve higher investments in productive inputs, leading to higher production and income. For example, we find a 17% increase in the total area cultivated, a six percentage point higher likelihood of using improved seeds, and a 15% increase in pesticide use. These results suggest potentially high poverty reduction benefits of conservation payments, especially in contexts where seasonal liquidity constraints are keeping vulnerable households into poverty. These co-benefits are not always considered in the costs-benefits analyses of environmental policies. In the drylands, policy makers face an ostensibly clear trade-off—whether to invest limited development funds in natural resource management or focus on fighting contemporaneous poverty and hunger issues that are often considered more pressing issues. Studies like this, which shed the light on the co-benefits of environmental interventions, allow policy makers to make more informed decisions.

The fact that we find positive and significant net effects of the PES schemes on the participants in our study suggests that the benefits of participating (mostly through the payments received) outweighed the costs of participation (the tree maintenance on state-owned forest lands required only some labor during the dry season where very few people were occupied in other income generating activities). However, in more humid agro-ecological conditions, or in areas where irrigation water is available, or for PES schemes that require land use changes on private lands, the opportunity costs of participation might be higher and thus outweigh the benefits due to potentially stiffer competition for labor between agricultural-related activities and environmental work. That means that external validity may be limited to nature restoration projects in arid regions (with just one agricultural season per year) that targets protected or heavily degraded lands.

Designing multifaceted interventions presents substantial challenges, which are further complicated by the diversity of the context across which such interventions might be relevant. To better understand the impacts of such multifaceted interventions, more insights are needed in the heterogeneity of the treatment effects across subgroups and space. Moreover, it would also be important to consider equilibrium effects, in the case of scale-up (Angelucci and De Giorgi 2009). Our study is not able to address these questions, as the estimation of these effects would require larger scale RCTs. Finally, our study focused on the medium-term impacts of the PES intervention in Burkina Faso. To fully appreciate the poverty reduction potential of PES schemes, it would be valuable to test the long-run effects on food security and production outcomes. We leave these questions for future research.

Supplementary Material

Supplementary material are available at *American Journal of Agricultural Economics* online.

References

Abadie, Alberto, and Guido W Imbens. 2011. Bias-Corrected Matching Estimators for Average Treatment Effects. *Journal of Business & Economic Statistics* 29: 1–11.

Abdulai, Awudu, Victor Owusu, and Renan Goetz. 2011. Land Tenure Differences and Investment in Land Improvement Measures: Theoretical and Empirical Analyses. *Journal of Development Economics* 96: 66–78.

Adjognon, Guigonan S, Alexis Rivera-Ballesteros, and Daan van Soest. 2018. Satellite-based Tree Cover Mapping for Forest Conservation in the Drylands of Sub Saharan Africa (SSA): Application to Burkina Faso Gazetted Forests. *Development Engineering* 4 100039.

Ahn, Hyungtaik, and James L Powell. 1993. Semiparametric Estimation of Censored Selection Models with a Nonparametric Selection Mechanism. *Journal of Econometrics* 58: 3–29.

Alix-Garcia, Jennifer M, Katharine R Sims, Victor H Orozco-Olvera, Laura E Costica, Jorge DF Medina, and Sofia R Monroy. 2018. Payments for Environmental Services Supported Social Capital while Increasing Land Management. *Proceedings of the National Academy of Sciences* 115: 7016–21.

Alix-Garcia, Jennifer M, Katharine R Sims, and Patricia Yañez-Pagans. 2015. Only One Tree from Each Seed? Environmental Effectiveness and Poverty Alleviation in Mexico's Payments for Ecosystem Services Program. *American Economic Journal: Economic Policy* 7: 1–40.

Anderson, Michael L. 2008. Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American Statistical Association* 103: 1481–95.

Angelucci, Manuela, and Giacomo De Giorgi. 2009. Indirect Effects of an Aid Program: How Do Cash Transfers Affect Ineligibles' Consumption? *American Economic Review* 99: 486–508.

Athey, Susan, and Guido W Imbens. 2017. The State of Applied Econometrics: Causality and Policy Evaluation. *Journal of Economic Perspectives* 31(2): 3–32.

Bedoya, Guadalupe, Aidan Coville, Johannes Haushofer, Mohammad Razaq Isaqzadeh, and Jeremy Shapiro. No household left behind: Afghanistan targeting the ultra poor impact evaluation. The World Bank, 2019.

Beegle, Kathleen, Emanuela Galasso, and Jessica Goldberg. 2017. Direct and

Indirect Effects of Malawi's Public Works Program on Food Security. *Journal of Development Economics* 128: 1–23.

Bellemare, Marc F, and Lindsey Novak. 2017. Contract Farming and Food Security. *American Journal of Agricultural Economics* 99: 357–78.

Bellemare, Marc F, and Casey J Wichman. 2020. Elasticities and the Inverse Hyperbolic Sine Transformation. *Oxford Bulletin of Economics and Statistics* 82: 50–61.

Bertrand, Marianne, Simeon Djankov, Rema Hanna, and Sendhil Mullainathan. 2007. Obtaining a Driver's License in India: An Experimental Approach to Studying Corruption*. *Quarterly Journal of Economics* 122: 1639–76.

Boltz, Marie, Karine Marazyan, and Paola Villar. 2019. Income Hiding and Informal Redistribution: A Lab-in-the-field Experiment in Senegal. *Journal of Development Economics* 137: 78–92.

Borner, Jan, Kathy Baylis, Esteve Corbera, Driss de Ezzine-de-Blas, Jodi Honey-Rosés, U. Martin Persson, and Sven Wunder. 2017. The Effectiveness of Payments for Environmental Services. *World Development* 96: 359–74.

Bruhn, Miriam, and David McKenzie. 2009. In Pursuit of Balance: Randomization in Practice in Development Field Experiments. *American Economic Journal: Applied Economics* 1: 200–32.

Burke, Marshall, Lauren F Bergquist, and Edward Miguel. 2019. Sell Low and Buy High: Arbitrage and Local Price Effects in Kenyan Markets. *Quarterly Journal of Economics* 134: 785–842.

Busch, Jonah, Jens Engelmann, Susan C Cook-Patton, Bronson W Griscom, Timm Kroeger, Hugh Possingham, and Priya Shyamsundar. 2019. Potential for Low-cost Carbon dioxide Removal Through Tropical Reforestation. *Nature Climate Change* 9(6): 463.

Cafiero, Carlo, Sara Viviani, and Mark Nord. 2018. Food Security Measurement in a Global Context: The Food Insecurity Experience Scale. *Measurement* 116: 146–52.

Canadell, Josep G, and Michael R Raupach. 2008. Managing Forests for Climate Change Mitigation. *Science* 320: 1456–7.

Carey, John. 2020. News Feature: The Best Strategy for Using Trees to Improve Climate and Ecosystems? Go Natural. *Proceedings of the National Academy of Sciences* 117: 4434–8.

Carletto, Calogero, Alberto Zezza, and Raka Banerjee. 2013. Towards Better Measurement of Household Food Security: Harmonizing Indicators and the Role of Household Surveys. *Global Food Security* 2: 30–40.

Christian, Paul, and Brian Dillon. 2018. Growing and Learning when Consumption Is Seasonal: Long-Term Evidence from Tanzania. *Demography* 55: 1091–118.

Coates, Jennifer, Anne Swindale, and Paula Bilinsky. 2007. *Food and Nutrition Technical Assistance Project (FANTA): Household Food Insecurity Access Scale (HFIAS) for Measurement of Food Access: Indicator Guide (v. 3)*. Washington DC: Food and Nutrition Technical Assistance Project.

De Mel, Suresh, David McKenzie, and Christopher Woodruff. 2008. Returns to Capital: Results from a Randomized Experiment. *Quarterly Journal of Economics* 123: 1329–72.

Deininger, Klaus, Daniel A Ali, and Tekie Alemu. 2011. Impacts of Land Certification on Tenure Security, Investment, and Land Market Participation: Evidence from Ethiopia. *Land Economics* 87: 312–34.

Deitchler, Megan, Terri Ballard, Anne Swindale, and Jennifer Coates. 2010. *Validation of a Measure of Household Hunger for Cross-cultural Use*. Washington DC: Food and Nurtrition Technical Assistance II Project (FANTA-2), Academy for Educational Development.

Dillon, Andrew, Joanne Arsenault, and Deanna Olney. 2019. Nutrient Production and Micronutrient Gaps: Evidence from an Agriculture-Nutrition Randomized Control Trial. *American Journal of Agricultural Economics* 101: 732–52.

Engel, Stefanie, Stefano Pagiola, and Sven Wunder. 2008. Designing Payments for Environmental Services in Theory and Practice: An Overview of the Issues. *Ecological Economics* 65: 663–74.

FAO. 1996. Rome Declaration on World Food Security and World Food Summit Plan of Action: World Food Summit 13–17 November 1996. Rome, Italy: FAO.

FAO, IFAD, UNICEF, WFP, and WHO. 2019. The State of Food Security and Nutrition in the World 2019. Safeguarding against Economic Slowdowns and Downturns. Rome, Italy: FAO. http://www.fao.org/3/ca5162en/ca5162en.pdf

Fenske, James. 2011. Land Tenure and Investment Incentives: Evidence from West Africa. *Journal of Development Economics* 95: 137–56.

Fink, Günther, B. Kelsey Jack, and Felix Masiye. 2018. Seasonal Liquidity, Rural Labor Markets and Agricultural Production. Working Paper No. 24564, National Bureau of Economic Research, April.

Ghanem, Dalia, Sarojini Hirshleifer, and Karen Ortiz Becerra. 2019. Testing Attrition Bias in Field Experiments.

Gueron, Judith M. 2017. The Politics and Practice of Social Experiments: Seeds of a Revolution. In *Handbook of Economic Field Experiments*, D Esther and A Banerjee, Vol 1 27–69. Amsterdam: Elsevier.

Haushofer, Johannes, and Jeremy Shapiro. 2016. The Short-term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya. *Quarterly Journal of Economics* 131: 1973–2042.

Headey, Derek, Kalle Hirvonen, John Hoddinott, and David Stifel. 2019. Rural Food Markets and Child Nutrition. *American Journal of Agricultural Economics* 101: 1311–27.

Heckman, James J. 1976. The Common Structure of Statistical Models of Truncation, Sample Selection and Limited Dependent Variables and a Simple Estimator for Such Models. *Annals of Economic and Social Measurement* 5(4): 475–92.

Heckman, James. 1979. Sample Selection Bias as a Specification Error. *Econometrica* 47: 153161.

Hirvonen, Kalle, and John Hoddinott. 2017. Agricultural Production and Children's Diets: Evidence from Rural Ethiopia. *Agricultural Economics* 48: 469–80.

Hirvonen, Kalle, John Hoddinott, Bart Minten, and David Stifel. 2017. Children's Diets, Nutrition Knowledge, and Access to Markets. *World Development* 95: 303–15.

Hoff, Karla, and Joseph E Stiglitz. 1990. Introduction: Imperfect Information and Rural Credit Markets: Puzzles and Policy Perspectives. *World Bank Economic Review* 4: 235–50.

Horowitz, Joel L, and Charles F Manski. 2000. Nonparametric Analysis of Randomized Experiments with Missing Covariate and Outcome Data. *Journal of the American Statistical Association* 95: 77–84.

Ichimura, Hidehiko, and Lung-Fei Lee. 1991. Semiparametric Least Squares Estimation of Multiple Index Models: Single Equation Estimation. in: William A. Barnett, James L. Powell, and George E. Tauchen, eds., *Nonparametric and Semiparametric Methods in Econometrics and Statistics*, (pp 3–49). New York, NY: Cambridge University Press.

Imbens, Guido W, and Donald B Rubin. 2015. *Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction*. Cambridge, UK: Cambridge University Press.

Imbens, Guido W, and Jeffrey M Wooldridge. 2009. Recent Developments in the Econometrics of Program Evaluation. *Journal of Economic Literature* 47: 5–86.

Jack, B Kelsey, and Elsa Cardona Santos. 2017. The Leakage and Livelihood Impacts of PES Contracts: A Targeting Experiment in Malawi. *Land Use Policy* 63: 645–658.

Jayachandran, Seema, Joost de Laat, Eric Lambin, and Charlotte Stanton. 2017. Cash for Carbon: A Randomized Trial of Payments for Ecosystem Services to Reduce Deforestation. *Science* 357: 267–73.

Kennedy, Gina, Terri Ballard, and Marie C Dop. 2011. *Guidelines for Measuring Household and Individual Dietary Diversity*. Rome: Food and Agriculture Organization of the United Nations.

Kling, Jeffrey R, Jeffrey B Liebman, and Lawrence F Katz. 2007. Experimental Analysis of Neighborhood Effects. *Econometrica* 75: 83–119.

Lee, David S. 2009. Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects. *Review of Economic Studies* 76: 1071–102.

List, John A, Azeem M Shaikh, and Yang Xu. 2019. Multiple Hypothesis Testing in Experimental Economics. *Experimental Economics* 22: 773–93.

Liu, Zhaoyang, and Andreas Kontoleon. 2018. Meta-Analysis of Livelihood Impacts of Payments for Environmental Services Programmes in Developing Countries. *Ecological Economics* 149: 48–61.

Maxwell, Daniel G. 1996. Measuring Food Insecurity: The Frequency and Severity of Coping Strategies. *Food Policy* 21: 291–303.

Maxwell, Daniel, Clement Ahiadeke, Carol Levin, Margaret Armar-Klemesu, Sawudatu Zakariah, and Grace Mary Lamptey. 1999. Alternative Food-security Indicators: Revisiting the Frequency and

Severity of Coping Strategies. *Food Policy* 24(4): 411–429.
Maxwell, Daniel, Bapu Vaitla, and Jennifer Coates. 2014. How Do Indicators of Household Food Insecurity Measure Up? An Empirical Comparison from Ethiopia. *Food Policy* 47: 107–16.
McKenzie, David. 2012. Beyond Baseline and Follow-up: The Case for More T in Experiments. *Journal of Development Economics* 99: 210–21.
Moffitt, Robert. 1983. An Economic Model of Welfare Stigma. *American Economic Review* 73: 1023–35.
O'Brien, Peter C. 1984. Procedures for Comparing Samples with Multiple Endpoints. *Biometrics* 40 1079–87.
Puri, Jyotsna, Megha Nath, Raag Bhatia, and Louise Glew. 2016. Examining the Evidence Base for Forest Conservation Interventions. International Initiative for Impact Evalutation (3ie). *Evidence Gap Map Report* 4: 1–70.
Rosas, Nina, and Shwetlena Sabarwal. 2016. Can You Work It? Evidence on the Productive Potential of Public Works from a Youth Employment Program in Sierra Leone. World Bank Policy Research Working Paper 320.
Samii, Cyrus, Matthew Lisiecki, Parashar Kulkarni, Laura Paler, Larry Chavis, Birte Snilstveit, Martina Vojtkova, and Emma Gallagher. 2014. Effects of Payment for Environmental Services (PES) on Deforestation and Poverty in Low and Middle Income Countries: A Systematic Review. *Campbell Systematic Reviews* 10(1): 1–95.
Savin, N Eugene. 1984. Multiple Hypothesis Testing. *Handbook of Econometrics* 2: 827–79.
Stuber, Jennifer, and Karl Kronebusch. 2004. Stigma and Other Determinants of Participation in TANF and Medicaid. *Journal of Policy Analysis and Management* 23: 509–30.
Tauchmann, Harald. 2014. Lee (2009) Treatment-Effect Bounds for Nonrandom Sample Selection. *Stata Journal* 14: 884–94.
Upton, Joanna B, Jennifer D Cissé, and Christopher B Barrett. 2016. Food Security as Resilience: Reconciling Definition and Measurement. *Agricultural Economics* 47: 135–47.
Wade, Thierno Ibrahima, Ousmane Ndiaye, Margaux Mauclaire, Babacar Mbaye, Maurice Sagna, Aliou Guissé, and Deborah Goffner. 2018. Biodiversity Field Trials to Inform Reforestation and Natural Resource Management Strategies Along the African Great Green Wall in Senegal. *New Forests* 49(3): 341–62.
Wooldridge, Jeffrey M. 2010. *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA: MIT Press.