Smart subsidies for sustainable soils: Evidence from a randomized controlled trial in southern Malawi*

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Abstract

Conventional agricultural practices – especially conventional tillage – are a major driver of soil erosion globally. While soil may not frequently be considered a vulnerable natural resource, the erosion and degradation of soils poses a serious threat to food production and the production of numerous other*in situ* and*ex situ* ecosystem services. This study provides some of the first evidence on the effectiveness of a payments for ecosystem services (PES) program to encourage the adoption of soil conservation practices, specifically conservation agriculture (CA). Through minimized soil disturbance, permanent soil cover, and diversified crop mix, CA is believed to enhance soil fertility and rehabilitate soil structure, with the resulting preservation of ecosystem service flows. By providing calibrated financial incentives, we demonstrate that it is possible to substantially increase the extent and intensity of CA adoption. What's more, we show that a novel incentive mechanisms that leverages social networks for the consolidation of fragmented land may be more effective at bringing more land under conservation objectives, even if some of the additional land does not officially fall under the purview of the PES program. We also demonstrate that some of the supposed weaknesses hindering the adoption of CA – lower yields in the short-run and higher expenditures on weed control – were not necessarily obstacles in our study area, perhaps suggesting that the provision of subsidies need not continue into perpetuity, but may only be needed to overcome short-term transition costs.

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1 Introduction

Soil erosion threatens the sustained production of several important ecosystem services. Healthy soil ecosystems provide the conditions for growing food, and perhaps the most obvious impact of soil erosion is its effect on food production. Even when erosion has not completely stripped away the topsoil, erosion generally diminishes soil fertility (Montgomery, 2007b; Amundson et al., 2015), though this effect can be at least partially offset by the addition of organic and inorganic fertilizers and other soil amendments, such as lime. Over time, however, it is feared that erosion will dramatically reduce soil's ability to produce sizable harvests to feed growing populations. In addition to food provisioning services, soils also directly provide several other valuable *in situ* ecosystem services, such as those related to providing habitats for various biota, enhancing biodiversity, carbon sequestration, regulating the flow of water, and providing the conditions under which solid and water-borne wastes can be broken down and eliminated (Van Oost et al., 2000). Soil erosion and degradation threaten the production of many of these important ecosystem services through their effects on the physical and chemical properties of the soil (Odelman et al., 1991; Amundson et al., 2015).

Soil erosion also poses a threat to *ex situ* ecosystem services; that is, ecosystem services that benefit recipients potentially far removed from where they are being produced – or, more appropriately, where soil erosion threatens their production. Because eroded soil often ultimately ends up in waterways, there are frequently adverse impacts to a number of aquatic ecosystems, therein inhibiting the production of a number of aquatic ecosystem services (Amundson et al., 2015), including fish habitats and hydroelectric power generation.

Scientists and policymakers are increasingly appreciating the challenges associated with soil erosion. One policy instrument that has been developed and employed to address this type of agri-environmental problem is the Payments for Ecosystem Services (PES) approach. PES is a type of direct payment approach to environmental conservation in which a well-defined ecosystem serivce (or land use believed to provide an ecosystem service) is supplied by an individual in exchange for financial compensation provided by the beneficiary of the ecosystem services, conditional on the actual provision of the ecocystem service or the undertaking of the desired land use (Engel et al., 2008; Wunder, 2005). The logic of PES programs is quite simple, especially in the case of soil erosion. By undertaking certain agricultural practices that produce or accelerate soil erosion, a farmer's production results in both intertemporal (*in situ*) and spatial (*ex situ*) externalities. Farmers could, of course, undertake agricultural practices that conserve the soil, reduce or slow the pace of soil erosion, and eliminate these externalities – but at a private cost to the farmer. Over time, farmers would reap many of the*in situ* ecological benefits of soil conservation (such as enhanced soil fertility), but by their very nature, many of the *ex situ* benefits of soil conservation accrue to external beneficiaries. With a PES, some of those who bear the costs of the spatial external damages provide the farmer with a direct payment to compensate him for these private costs, and so long as the payment amount is (a) no greater than the external damges they incur and (b) no less than the farmer's private costs from conservation, such a negotiated outcome has the potential to produce social welfare gains.

A promising approach to achieving the *ex situ* benefits of soil conservation while preserving agency, livelihoods, and *in situ* benefits for farmers is conservation agriculture (CA). CA has been widely promoted as a means for reducing soil erosion and sedimentation (Derpsch et al., 2010), while at the same time enhancing soil fertility, increasing farmers' profitability, and contributing to achieving food security objectives (Corbeels et al., 2014). Yet while the *ex situ* benefits of reduced soil erosion and sedimentation may be realized immediately, some of the *in situ* benefits may only occur gradually. Further, much evidence suggests there is a cost associated with the transition from conventional agriculture to CA, largely borne out in lower yields, higher costs associated with labor for weeding, or increased expenditures on herbicides. If these private costs are overshadowed by the social value of the ecosystem benefits that would accrue under CA, there is a misalignment of interests between the private farmer and society at large.

The present study evaluates a novel PES program in the Shire River Basin in southern Malawi encouraging the adoption of CA. The Shire River Basin provides an interesting context in which to test the effectiveness of a PES program for encouraging soil conservation, primarily because it is just such an environment in which the private costs and social benefits of soil conservation may be imbalanced. In the evaluation of this PES pilot program, villages were randomly selected for inclusion in a program in which farmers could voluntarily register and agree to practice CA in exchange for direct compensation. This pilot program entailed two alternative compensation modalities: a conventional subsidy and an agglomeration payment. Both modalities provide a direct subsidy payment to adopting farmers, but the agglomeration payment provides bonus compensation for each of the farmers' neighbors that also practiced CA, thereby encouraging positive network externalities in expediting the adoption of soil conservation practices. Using a series of difference-in-differences regressions, the present study demonstrates that direct compensation has the potential to increase the number of farmers practicing CA, as well as both the extent and intensity of CA in the Shire River Basin. Further, we find evidence that the agglomeration payment modality may engender a peer-policing mechanism that may reduce the need for costly monitoring by program implementers.

We address several important knowledge gaps related to the use of PES programs in encouraging CA. First, the pilot PES program analyzed here is one of the first PES programs that has been implemented among small-scale farmers. Although there have been evaluations of past efforts using financial incentives to promote forest conservation and the sustained generation of beneficial ecosystem services (e.g., Jayachandran et al., 2017; Alix-Garcia et al., 2018), the conventional wisdom has largely held that the transaction costs associated with providing conditional payments to small-scale farmers would be simply too expensive.

Second, this study provides one of the first rigorous evaluations of the effects of direct financial incentives in encouraging the adoption of CA. Although challenges associated with the widespread adoption of CA have been konwn for some time, there have been few efforts to our knowledge that have provided compensation to farmers to offset some of the short- to medium-term adjustment costs, perhaps due to the time horizon that might be required. In a recent framed field experiment in Ghana, Ambler et al. (2020) find that financial incentives have a significant effect on encouraging adoption of CA, and further that they were less likely to abandon CA once they had initially adopted. Although several previous studies in Malawi (e.g., Marenya et al., 2014; Ward et al., 2016) have found that farmers would very likely be responsive to such direct cash payments, these studies have relied on stated preference methods rather than observing farmers' revealed preferences in a real-world setting.

Third, assuming that financial incentives are successful in encouraging adoption of CA, we assess the relative effectiveness of the conventional subsidy and agglomeration payment modalities. While there is much evidence on the effectiveness of subsidies in encouraging behaviors to offset externalities, this represents the first attempt at implementing an agglomeration payment mechanism in a real world setting. The agglomeration payment first proposed in Smith and Shogren (2002) and Parkhurst et al. (2002) provides program participants with bonus payments for contiguous parcels of land that also adhere to the conservation program requirements. To date, much of the evidence on the effectiveness of agglomeration payment mechanisms at achieving conservation objectives have come from laboratory settings with university students as the experimental subjects (Parkhurst et al., 2002; Parkhurst and Shogren, 2007, 2008; Panchalingam et al., 2019). One notable exception is the recent study by Liu et al. (2019), who conduct a framed field experiment with farmers from rural areas in China. Although initially envisioned as a method for aggregating land parcels voluntarily set aside for habitat conservation (specifically for endangered species), the present study demonstrates how it can also be applied to merge fragmented parcels of land under a unified conservation objective under the presumption that the generation of ecosystem services exhibits increasing returns to scale, as is the case with soil conservation. To our knowledge, the present study is the first attempt at implementing an agglomeration payment mechanism in a real-world setting. Specifically, the agglomeration payment used in this study combined a conventional subsidy with a bonus payment tied to neighbors' adoption of CA. It is hypothesized that this structure would effectively leverage social networks and farmer-to-farmer extension efforts to aggregate larger areas under conservation. Fourth, we assess the effectiveness of varying levels of farmer monitoring, including in conjunction with the two alternative payment modalities. In most cases, it is not possible to condition payments on the basis of the actual ecosystem services that are provided, so the most frequent approach for providing PES payments is to condition payments on the adoption of a particular practices (such as CA) that is *believed* to contribute to the provision of ecosystem services. Consequently, it is almost always the case that the PES program administrators must somehow monitor participants' actions to ensure that they are compliant with the program requirements. This has proven to be a major hurdle with many PES programs, since one of the main challenges to the success of many PES programs has been monitoring and assessing participants' compliance with program requirements.

Finally, we are able to assess the impacts of CA directly in farmers' fields rather than under controlled conditions like agricultural test plots. Although there have been previous attempts to estimate the economics of CA, many have been susceptible to selection bias since the decision to practice CA is very likely conditioned by a number of individual and farm-level characteristics that might also be correlated with economic outcomes like yield and profit. By incorporating individual/farm and time fixed effects, we are effectively able to control for unobservable factors that could condition the decision to practice CA, thus yielding an unbiased estimate of the causal effect of practicing CA on important agricultural economic outcomes like yield and labor demand. Being able to clearly demonstrate that CA is economically remunerative for farmers vis-á-vis conventional tillage and related practices would be a boon for efforts encouraging CA adoption.

The remainder of this article is structured as follows. In Section 2, we provide some background on the broader problems of soil erosion and the promotion of CA to address this problem. In 3, we introduce the cluster randomized controlled trial that we implemented in the Shire River Basin in Southern Malawi to test the effectiveness of competing payment modalities in encouraging the spread of CA. In Section 4, we introduce the data that are used in addressing the knowledge gaps identified above. In Section 5, we discuss the results of our experiment. Finally, in Section 6, we offer some concluding remarks.

2 Background on Conservation Agriculture

Soil conservation efforts emerged after concerns were raised about the role of tillage in contributing to the devastation experienced during the "Dust Bowl" that struck the midwest and southern Great Plains of the United States during the 1930s (Egan, 2006). For good reason, the Dust Bowl has frequently been referred to as one of the worst man-made agricultural and ecological disasters in history (Montgomery, 2007a). Although drought conditions at various points throughout the decade no doubt contributed to the disaster, these impacts were exacerbated by the widespread use of inappropriate agricultural practices, namely the "introduction of the plow into the vast, semiarid, wind-swept grasslands" (Hillel, 1991). Experiences in the Dust Bowl vividly illustrate the potential calamities that can arise from the combination of ignorance about the nature and functioning of ecosystems, the improper or excessive use of technology (namely the plow), and the blind quest for maximum short-term profits at the expense of long-term sustainability.

There have been many agricultural technologies and practices that have been developed and promoted to help conserve soil and enhance soil productivity (including enhanced drainage, organic fertilization with compost and manure, and the use of soil amendments such as gypsum and lime; cf. Hudson, 1971; Hillel, 1991). The suite of practices now known as CA emerged in the early 1990s as a widely applicable set of relatively simple practices that could be adapted to local agroecological conditions (Friedrich et al., 2012). Simply put, CA is a suite of land management practices consisting of (a) minimum soil disturbance through reducing or eliminating tillage; (b) permanent soil cover through the retention and mulching of crop residues; and (c) crop rotations or intercropping with different plant species (FAO, 2001; Thierfelder et al., 2013) that aim to conserve and improve soil structure, reduce soil erosion, and enhance the soil fertility. Through reducing or eliminating tillage, soil is protected against wind and water erosion and the organic matter in the soil is sustained or rehabilitated. In addition, reducing or eliminating tillage reduces farmers' fuel, time, and labor costs associated with land preparation. Providing a permanent soil cover also helps to protect the soil from wind and water erosion while also recycling nutrients into the soil. Intercropping or rotating (almost always through integrating cereals with legumes) improves the utilization of soil moisture, reduces pressures from pests and diseases, and can improve soil fertility through nitrogen fixation.

CA spread widely during the 1990s, especially in North and South America, accelerated in part due to the involvement of international agricultural development and research organizations such as FAO, CIRAD, CIMMYT, and others. Although early studies suggested that CA could result in higher yields (Lal, 1991), several studies have pointed to some challenges for the broad adoption of CA. Rusinamhodzi et al. (2011), for example, find that maize yields increase over time under CA, especially in low rainfall environments. In high rainfall environments, however, mulching crop residues can actually reduce crop yields due to waterlogging. The yield gains also appear to be highly dependent on the application of other inputs, especially nitrogen fertilizer. Rusinamhodzi et al. (2011) also found little evidence of increasing yield stability (i.e., reducing yield variability). A recent meta-analysis (Pittelkow et al., 2015) finds that CA actually introduces a yield penalty, though the extent of the yield penalty varies over time and across the spectrum of agroecological conditions. In particular, Pittelkow et al. (2015) find that CA increases yields in rainfed dry climates, most likely due to increased water infiltration and soil moisture conservation. Further, over time, the yield penalty tends to diminish, such that after 10 or more years under a CA system, yields are virtually indistinguishable from those under conventional agriculture. Michler et al. (2019) find a similar result in their study of the effects of CA on crop yields in Zimbabwe, finding no effect – or perhaps a slight decline – in yields during periods of normal rainfall, but a positive effect on yields during rainfall deficiencies. Importantly, yields are higher when all three practices are implemented, rather than when only zero tillage is practiced, which is one reason why some advocates argue for a holistic definition of CA that encompasses adoption of the full set of practices, rather than just a subset of practices (Derpsch et al., 2014).

Despite the potential benefits of CA, actual adoption remains relatively low worldwide. Friedrich et al. (2012) have estimated that only about 9 percent of the world's cropped area was under CA, with much of this area concentrated in North and South America. There have been a number of studies that have attempted to understand the factors that drive farmers' decision to adopt CA. Knowler and Bradshaw (2007) and Knowler (2015) provide comprehensive reviews of the literature on CA adoption and the specific factors that are frequently included as potential determinants of adoption. The studies that are reviewed vary over several dimensions, including the specific CA practices that are analyzed, the potential determinants of CA adoption that are included as explanatory variables in the analysis, the methods that are employed, the geographic region of focus, and the quality of the publication (specifically, peer-reviewed publications versus so-called "grey literature": publications that have not been subjected to rigorous peer review). While there are some factors that demonstrate consistent and precisely measured impacts on CA adoption when they are included in the analysis (namely awareness of environmental threats and productivity of the soil), but these are included quite infrequently across the studies reviewed, so it is not possible to draw robust conclusions about their efficacy in explaining CA adoption beyond the small set of studies in which they are concluded.

This conclusion is consistent with the conclusions drawn by other researchers, particularly related to the importance of socio-economic considerations that are likely to vary considerably from place to place, based on heterogeneity both between and within regions (Pannell et al., 2006; Giller et al., 2011; Erenstein et al., 2012; Pannell et al., 2014). Pannell et al. (2014) reviewed much of the existing literature on the farm-level economics of CA adoption in various geographical contexts, further concluding that, although the constituent practices may have economic benefits, these benefits are far from universal. For example, one of the arguments often put forward for the economic benefits of zero tillage is the reduction in land preparation costs, whether due to reduced demand for labor, reduced expenditures on renting machinery, or reduced expenditures on fuel for operating machinery. Yet these cost reductions may simply be transferred to later in the season, when farmers have to increase demand for labor and/or increase expenditures on herbicides to combat increased weed pressure. In simulating the potential economic benefits of transitioning from conventional agriculture to CA, Pannell et al. (2014) demonstrate that the economic benefits of CA involve numerous factors, and suggest that the net economic gains from transitioning from conventional farming to CA would likely be realized among larger and wealthier farmers. Michler et al. (2019) conclude that the lower yields during years with normal rainfall may be one of the factors inhibiting widespread adoption of CA.

3 Empirical methodology

To study the potential impacts of direct payment approaches to encouraging the adoption of CA, we introduced a cluster randomized controlled trial of a PES program in conjunction with the National Smallholder Farmers Association of Malawi (NASFAM) and the Department of Land Resources Conservation (DLRC) in the Shire River Basin in southern Malawi starting in June/July 2014, prior to the 2014-2015 rainy season (the primary growing season in Malawi). As mentioned above, the Shire River Basin provides an important context in which to test the effectiveness of a PES program for encouraging soil conservation because the private costs and social benefits of soil conservation are frequently imbalanced. In the catchment areas of the Shire River, an important*ex situ* ecosystem service that is impacted by soil erosion is the generation of hydroelectric power. Power generation, transmission, and distribution in Malawi is effectively operated by two state-run monopolies, with the Electricity Generation Company (Egenco) in charge of electricity generation, and the Electricity Supply Commission of Malawi (ESCOM) managing the bulk purchase, transmission, and distribution of electricity in the country.[1] In particular, hydroelectric power generation in three catchments in the Shire Basin (Nkula, Tedzani, and Kapichira) have been estimated to produce as much as 90 percent of the total power generated in Malawi, serving more than 150,000 consumers and generating over 1,100 Gwh of power annually MCC Malawi (2011). But due to increasing populations and increased clearing of land for farming, these three catchments have endured significant increases in siltation, including into the Shire River. In 2008-09, these three power stations lost 2,712 hours of power generating potential due to excessive siltation, and incurred costs of over $500,000 for dredging, not to mention expenditures on repairing mechanical damage due to silt entering plant equipment MCC Malawi (2011). Reducing the extent of soil erosion in these catchments would dramatically reduce the extent of siltation and ease the impacts on hydropower generation, which in turn would result in an increased power supply to much of the country, potentially enhancing the livelihoods of millions of Malawians.

Our focal area for this study is the three furthermost upstream districts adjacent to the Shire River, namely Balaka, Machinga, and Zomba. A total of 60 villages were ultimately selected from these districts

[1]Until 2017, ESCOM was in charge of all aspects of the electricity supply in Malawi. Egenco was spun-off from ESCOM on January 1, 2017.

for participation in the trial (see Figure 1). The approach used to select the villages included in the trial entailed initially drawing a large number (10^6) of simple random samples of 60 villages from five Extension Planning Areas (EPAs) in these three districts adjacent to the Shire River, with the resulting village selection being the one that maximized the minimum distance between participating villages.[2] This sample of villages was then randomly allocated to treatment (48 villages) and comparison (12 villages) groups, with the treatment group further randomly allocated to one of two different payment treatments (conventional voucher or agglomeration payment) and one of three varying levels of monitoring intensity (no monitoring, partial monitoring, and full monitoring). Because there is a considerable amount of background promotion of CA throughout Malawi, including in the Shire River Basin, our experimental design does not permit the analysis of the effects of our alternative incentive regimes vis-á-vis a pure control (i.e., a counterfactual in which farmers have never previously been exposed to informational campaigns about the benefits of CA nor received incentives to practice CA). Rather, our experimental design permits us to glean insights about the additive benefits of these incentive regimes relative to a status quo with some existing background level of CA promotion.

The conventional subsidy was a fixed amount coupon that was given to program participants who complied with the program requirements, set at approximately USD 3 per tenth of an acre of land, up to a total of one acre (that is, for a maximum subsidy valued at approximately USD 30). The agglomeration payment consisted of a comparatively smaller conventional voucher given to compliant program participants, with an additional bonus voucher for each of his contiguous neighbors that also adopt CA. In particular, the agglomeration payment treatment was calibrated so that participants would receive USD 2.5 per tenth of an acre for practicing CA on their own plots (up to a total of one acre), plus an additional bonus of USD 0.25 per tenth of an acre (up to one acre, and pro-rated against the registrant farmer's own level of adoption) for each neighbor that also practiced CA, up to a total of four neighbors. With the registrant farmer fully adopting CA on at least an acre of his own land, and with four neighbors each fully adopting CA on at least an acre of their land, the agglomeration payment has a slightly higher maximum value (at USD 35 per acre, compared to USD 30 per acre for conventional voucher). Further, since the marginal return on neighbors' adoption is conditional on the registrant farmer's own level of adoption, this incentive should encourage registrant farmers to maximize their compliance with the program requirements up to at least one acre. The agglomeration payment structure also embeds uncertainty in the total payment amount, as it is dependent on the willingness of neighbors to practice CA on their plots. Thus there is an incentive for registrant farmers

[2]While this design selects for samples with more remote villages, it has the benefit of reducing the probability of spillover effects which could contaminate the experimental design and bias the estimated treatment effects.

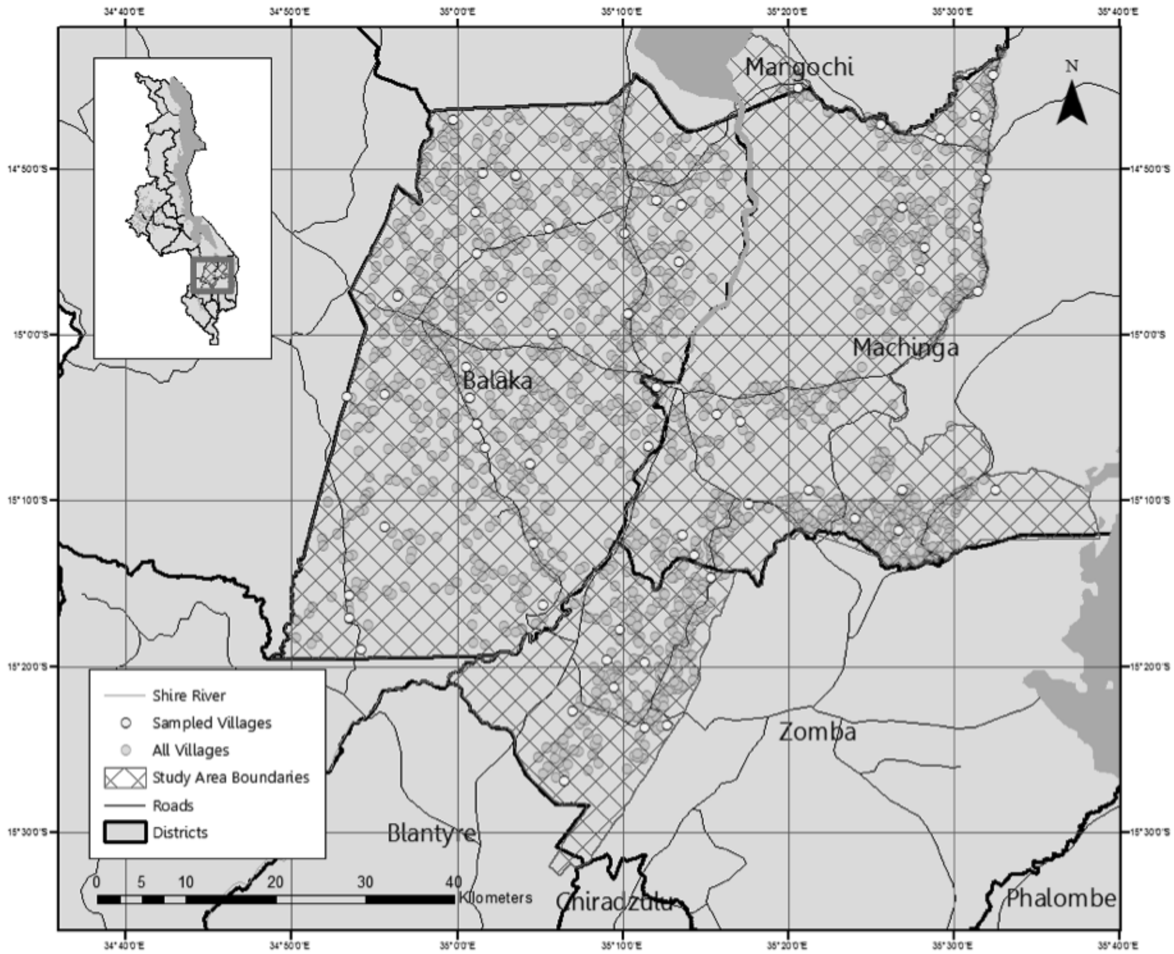

Figure 1: Sample area: Shire River Basin, Malawi
Source: The authors.

to increase their engagement with neighboring farmers to maximize the probability that they also adopt CA, thus maximizing the registrant farmer's agglomeration bonus. Under both payment modalities, the vouchers that were received could be redeemed at a local input supplier (Agora, Ltd.) for agricultural tools or inputs (e.g., shovel, hoe, seed, fertilizers, pesticides, etc.) or other household items (e.g., soap, lotion, cooking oil, etc.).

The program required participants to practice all three constituent CA practices in order to be eligible for payment. Consequently, the program operated very much like a standard payments for ecosystem services (PES) program, in which compensation is conditional upon undertaking land management practices that result in – or at least are plausibly believed to result in – the provision of socially beneficial ecosystem services (e.g., reduced soil erosion, siltation, etc.). One of the principal challenges to the successful implementation of

PES programs has historically been the effective (and efficient) monitoring of PES participants' compliance with the terms of the PES contract. In the case of this experiment, compliance with program requirements was assessed through visual inspection of registrants' plots. In order to assess the effectiveness of monitoring activities in ensuring compliance with program requirements, as well as whether the effectiveness of monitoring varied over payment structure, we randomly introduced variation in the intensity of monitoring as part of the experiment. Within the agglomeration payment group in particular, it is thought that social pressures and social interactions could substitute for more traditional forms of compliance monitoring, thereby reducing monitoring costs and making the overall program more cost effective. The monitoring factor levels were of increasing intensity or likelihood of monitoring: no monitoring (compliance with the program's required land management practices would be determined based solely on farmer's self-reported practices elicited through in-person interviews); partial monitoring (a random selection of 50 percent of registered farmers would have follow-up visits to their registered plots to verify land management practices, with compliance for the remaining 50 percent would be determined based on self-reported practices elicited through in-person interviews); and full monitoring (all registered farmers would have follow-up visit to their registered plots to verify land management practices).

Sensitization and registration efforts were led by representatives of NASFAM and DLRC to inform potential registrants of the details of the program. In particular, potential registrants were informed of the mode of payment that was being offered in their village, what amount was being offered per tenth of an acre registered for the program, what bonus would be offered for neighbors' adoption (if relevant), how to obtain their voucher, and whether there would be any monitoring visits registrants' plots to verify compliance with program requirements in order to qualify to receive the voucher. In the treatment villages in which the conventional voucher was offered, there was no mention of the agglomeration bonus. Similarly, in villages in which there was to be no monitoring of farmers' registered plots, there was no mention of visits to registrants' plots for visual verification of program compliance. In all villages, payment schedules were presented in tabular form so that potential registrants could estimate their total potential compensation for participating in the program, based upon the amount of the farmers' own land that was registered in the program, and how many of their neighbors also practiced CA on their plots. During the sensitization and registration visits, villagers in the treatment villages were informed that participation would be limited due to availability of funds, such that payment could be guaranteed only up to the first 80 registrants. Additional interested parties would be added to a waiting list, and would be invited to participate pending availability of funds.

4 Data

The data used in this paper come from several sources. For the purposes of evaluating the impact of the PES program in the Shire River Basin, our primary data come from a series of household surveys conducted in treatment and comparison villages over the lifespan of the project. The first household survey (which represents the project baseline) commenced in June and July 2014, while the follow-up survey (which represents the project endline) was conducted in September and October 2016. Nearly all households that were interviewed during the baseline survey were also interviewed during the follow-up survey (97 percent). These household survey data were collected from a random sample of households in the randomly allocated treatment and comparison villages, prior to the initiation of sensitization activities and farmers' subsequent registration with the program. Consequently, it is possible that the survey participants were never actually participants in the PES program itself. Thus our top-level estimation for the impacts of the programs on adoption of CA will be intention-to-treat (ITT) effects, rather than the impacts of actual program participation on adoption of CA, which would be more akin to average treatment effects (ATE).

Basic summary statistics for the households in our sample are reported in Table 1. As can be seen from this table, self-reported adoption of CA practices at the time of the project baseline were generally quite low.[3] By far, the most widely adopted of the three constituent practices at baseline was intercropping or crop rotations, with about 80 percent of farmers in our sample reporting intercropping or practicing crop rotations, and those farmers doing so on almost all (95 percent) of their total cultivated area. Zero-tillage and retention and mulching of crop residues were considerably less widely practiced at baseline, though those who had adopted these practices at the time of the project baseline were doing so quite intensively. Only about 15 percent of farmers reported practicing zero tillage at the time of the project baseline, but those farmers practiced zero tillage on about 75 percent of their total cultivated area. Similarly, when it comes to retaining and mulching crop residues, only about 20 percent of farmers reported adopting this practice at the time of the project baseline, but did so on about 80 percent of their total cultivated area. Incidentally, farmers from villages randomly selected to receive the agglomeration payment demonstrated higher baseline levels of practicing the three constituent practices than farmers from comparison villages. While the actual allocation of villages to receive treatment was randomized, the associated p-values for each of these differences is less than or equal to 0.10, suggesting that differences in farmers' practicing each of the three constituent practices

[3]As indicated here, agricultural practices are self-reported by the respondents. As will be discussed in greater detail below when discussing the registration and monitoring data, self-reporting of agricultural practices runs the risk of participating households to over- or mis-state their adoption of CA. In the operation of the PES program, households may an incentive to do so in order to be deemed as complying with the PES program requirements and thus to be eligible for the conditional compensation. We are less concerned about this in the baseline and endline surveys because the respondents have no incentive to over- or mis-state their agricultural practices.

at the time of the project baseline could introduce an important source of selection bias when it comes to estimating the effects of the PES program. In particular, the difference-in-differences identification strategy that we employ to estimate the program effects relies on the assumption that differences in CA adoption would have followed parallel trajectories in the absence of the program. Violation of this assumption would threaten to invalidate our interpretation of causal effects. In Section 5, we maintain the assumption that these differences in observed patterns of soil conservation measures across treatment and comparison villages is a statistical anomoly, and consequently, we will control for these baseline levels of adoption in our treatment effects regressions. We revisit this assumption and demonstrate the robustness of our primary results in Section 5.3.

We also observe differences in the number of agricultural plots cultivated and maize yields at the time of the project baseline. Though perhaps not meaningful in any physical sense, we note that households from villages randomly selected to receive the conventional voucher cultivated roughly 0.10 more plots on average than households from comparison villages ($p = 0.19$), while households from villages randomly selected to receive the agglomeration payment cultivated 0.15 more plots on average than households from comparison villages ($p = 0.04$). When it comes to maize yields, we observe that farmers in the villages randomly selected to receive the conventional voucher achieved maize yields of roughly 530 kg/acre, approximately 70 kg/ac more than farmers in the comparison villages ($p = 0.03$), and farmers in the villages randomly selected to receive the agglomeration payment obtained maize yields of 555 kg/acre, about 100 kg/ac more than farmers in the comparison villages ($p = 0.002$) at the time of project baseline. Because of these differences – despite the random allocation of villages to these different groups – and because it seems plausible that the number of plots and maize yields could also be correlated with adoption of CA or other various agricultural outcomes, we control for these baseline levels in our treatment effects regressions.

There are some additional observed differences worth briefly mentioning, though not reported in Table 1. In particular, there are differences in the languages that households speak and the religious traditions that households adhere to, with these differences coming between villages randomly selected to receive the conventional voucher and those in the comparison group. Because these characteristics are most likely the consequence of cultural sorting and the formation of ethnic enclaves and manifest themselves in our sample due to the clustered nature of our intervention and data collection, and because these characteristics seem quite unlikely to result in differential outcomes, we do not control for these factors in the treatment effects estimation, but rather adjust standard errors to take account of the clustered nature of the sample.[4]

[4] As has been frequently observed in literature, land tenure is an important determinant of investments in soil conservation. Recent research by Lovo (2016) has noted that different ethnic groups within Malawi have different systems of intergenerational land transfers (e.g., matrilineal vs. patrilineal), and these traditional systems of intergenerational

Table 1: Baseline characteristics of households in randomly allocated treatment and comparison villages

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample	Comparison group	Voucher treatment arm	Agglomeration payment treatment arm	Voucher - comparison difference	Agglomeration payment - comparison difference
Area under intercropping (ac)	1.301	1.195	1.268	1.383	0.073	0.188**
	(0.028)	(0.062)	(0.043)	(0.046)	(0.074)	(0.079)
Area under zero tillage (ac)	0.230	0.152	0.197	0.299	0.045	0.147***
	(0.016)	(0.031)	(0.022)	(0.029)	(0.038)	(0.047)
Area with mulched residues (ac)	0.257	0.232	0.207	0.314	−0.025	0.082*
	(0.017)	(0.037)	(0.023)	(0.029)	(0.041)	(0.049)
Sex of household head (male = 1)	0.722	0.742	0.698	0.734	−0.043	−0.008
	(0.012)	(0.025)	(0.019)	(0.018)	(0.032)	(0.031)
Age of household head (yrs)	45.924	46.034	45.658	46.115	−0.376	0.082
	(0.422)	(0.942)	(0.684)	(0.653)	(1.165)	(1.145)
Household size (# members)	5.110	5.188	5.055	5.123	−0.133	−0.065
	(0.050)	(0.118)	(0.080)	(0.077)	(0.140)	(0.138)
Number of agricultural plots	1.966	1.869	1.961	2.016	0.092	0.147**
	(0.026)	(0.050)	(0.041)	(0.043)	(0.067)	(0.070)
Total area cultivated	2.275	2.293	2.240	2.299	−0.054	0.006
	(0.038)	(0.084)	(0.061)	(0.058)	(0.104)	(0.102)
Male family agric. labor (person-days)	95.355	94.745	93.196	97.638	−1.549	2.893
	(2.665)	(5.145)	(4.181)	(4.483)	(6.869)	(7.373)
Female family agric. labor (person-days)	117.334	118.225	119.245	115.143	1.020	−3.082
	(2.860)	(4.965)	(5.727)	(3.709)	(8.682)	(6.353)
Male hired agric. labor (person-days)	16.921	14.634	19.143	15.982	4.509	1.348
	(1.478)	(2.638)	(2.808)	(2.079)	(4.321)	(3.508)
Female hired agric. labor (person-days)	14.939	3.480	14.173	21.187	10.693	17.707
	(6.819)	(0.732)	(7.159)	(15.011)	(9.891)	(21.595)
Total expenditures on pesticides (MKW)	945.925	1010.738	956.534	904.805	−54.204	−105.933
	(78.245)	(228.878)	(132.964)	(90.561)	(247.306)	(205.609)
Maize yields (kg/ac)	525.420	456.011	528.322	555.499	72.311**	99.488***
	(11.532)	(24.507)	(18.532)	(18.236)	(32.765)	(32.719)
Neighbors: Received assistance last season	0.080	0.077	0.088	0.073	0.011	−0.004
	(0.007)	(0.015)	(0.012)	(0.010)	(0.020)	(0.019)
Neighbors: Received advice last season	0.253	0.252	0.270	0.239	0.018	−0.013
	(0.011)	(0.025)	(0.019)	(0.017)	(0.032)	(0.030)
# Observations	1481	298	567	616	865	914

Source: The authors.
Note: * 0.05 < p-value ≤ 0.10; ** 0.01 < p-value ≤ 0.05; *** p-value ≤ 0.01. In columns (1)-(4), standard deviations are reported in parentheses below the sample or sub-sample means. In columns(5)-(6), standard errors (not adjusted for clustering) are reported in parentheses below the estimated differences. The differences in columns (5) and (6) are based on coefficient esitmates from linear regressions of the form $x_{ij} = \alpha + \beta T_i + \varepsilon_{ij}$, where x_{ij} is the characteristic over which balance is being tested (i.e., the variable described in the row header) and T_i is a binary indicator equal to 1 if the household was in a village assigned the treatment described in the respective column header. The p-values for these differences (not reported, though indicated by *, **, and *** symbols where applicable) are based on a t-test of the estimated coefficient for each household characteristic for each treatment arm.

land transfer have implications for the security of land tenure and investments in soil conservation. In the context of the present study, the dominant ethnic groups in our sample (and, indeed in Southern Malawi more broadly) are the Chewa and Yao ethnic groups, both of which adhere to a matrilineal system. Consequently, we do not have sufficient

Since this project aimed at promoting soil conservation as a means of reducing soil erosion, we also test whether there are differences in various soil or land characteristics between farmers in treatment and comparison villages that could potentially pose a threat to identification. However, because of the homogeneity of soil and land characteristics on a geographically -localized (e.g., intra-village) scale, there is a relatively high likelihood that there could be significant differences in, e.g., the percentage of total cultivated land with a specific soil type, in spite of random assignment. Consequently, we are less concerned with whether there might be differences in specific soil types, land slopes, etc., but whether these *classes* of soil or land characteristics might differ between the treatment and comparison groups. In Table 2, we report summary statistics for various soil and land characteristics (proportion of farmers' land), as well as the p-value associated with a joint hypothesis test that a class of soil and land characteristics differs between the two treatment groups and the comparison group.[5] As is seen in column (6), there is a difference between the agglomeration payment treatment group and the comparison group over the class of soil types, with this driven by households in the agglomeration payment treatment group having a higher proportion of their land with clay soils. As with the other variables mentioned above which demonstrate some baseline imbalance, we will control for soil types below when estimating the effect of the program on adoption of CA.

We also utilize data collected in the course of participants' registering their plots to participate in the program, as well as subsequent visits at the end of the season to assess registrants' compliance with the program. The registration data contain some basic information on the household and the characteristics of the plots that are being registered for participation in the program, while the monitoring data assess the extent of compliance with the program requirements, whether through visual inspection or merely through farmers' self-reporting. These data are used to summarize some of the operational aspects of the program (e.g., numbers of registrants, verified compliance, etc.), though because these data do not represent a random sample with a counterfactual, they are of limited practical use in evaluating the impacts of the PES program on adoption of CA and the effects of practicing CA on agricultural productivity and profitability.

ethnic variation in our sample with which to test for these differences. When we regress baseline adoption of CA against binary variables for the Chichewa and Yao languages and against binary variables for Christianity and Islam, there are no explanatory variables with a p-value less than 0.29.

[5]In other words, we run a set of simple least squares regressions on piecewise sub-samples of the full dataset consisting of observations from the comparison group and one of the two treatment arms, with $T_i^j = \alpha + \beta_1 x_{i1} + \beta_2 x_{i2} + ... + \beta_k x_{ik} + \varepsilon_i$, where T_i^j is a binary variable associated with the j^{th} treatment arm included in the sub-sample, and x_k pertains to the k^{th} level of a class of land or soil characteristic X, and we test the hypothesis that $\beta_1 = \beta_2 = ... = \beta_k = 0$ and report the $p - values$ for these tests in columns (4) and (5) in Table 2. This is basically a joint orthogonality test, but rather than focusing on the joint orthogonality of all characteristics, we focus on one class of land or soil characteristics at a time.

Table 2: Soil and land characteristics of households in randomly allocated treatment and comparison villages

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample	Comparison group	Voucher treatment arm	Agglomeration payment treatment arm	p-value of F-statistic (a)	p-value of F-statistic (b)
Soil type: sandy	0.131	0.142	0.139	0.118		
Soil type: sandy-clay	0.631	0.664	0.633	0.613	0.480	0.009
Soil type: clay	0.209	0.160	0.198	0.243		
Soil type: other	0.029	0.033	0.031	0.026		
Soil quality: good	0.471	0.484	0.452	0.483		
Soil quality: fair	0.361	0.364	0.365	0.356	0.402	0.932
Soil quality: poor	0.168	0.152	0.183	0.161		
Land slope: flat	0.570	0.559	0.565	0.579		
Land slope: slight	0.323	0.340	0.340	0.300		
Land slope: moderate	0.081	0.084	0.072	0.088	0.432	0.190
Land slope: steep	0.022	0.010	0.021	0.029		
Land slope: hilly	0.004	0.007	0.002	0.004		
Soil erosion: none	0.600	0.576	0.626	0.587		
Soil erosion: low	0.253	0.271	0.239	0.258	0.272	0.471
Soil erosion: moderate	0.095	0.111	0.086	0.094		
Soil erosion: high	0.052	0.042	0.049	0.060		

Source: The authors.
Note: p-values in columns (5) and (6) based on F-tests associated with joint hypotheses that associated levels are all equivalent to zero, based on a linear regressions of the form $T_{ij}^{k} = \alpha + \beta_1 x_{i1} + \beta_2 x_{i2} + ... + \beta_k x_{ik} + \varepsilon_{ij}$, where x_{ij} pertains to the k^{th} level of the class of land or soil characteristic X (i.e., soil type, soil quality, slope of the land, and the severity of erosion). In these regressions, T_i is a binary indicator equal to 1 if the household was in a village assigned to (a) the conventional voucher treatment or (b) the agglomeration payment, and 0 if the household was from the comparison group. All soil or land characteristics are the percentage of farmers' total cultivated area with a specific level of the soil or land characteristic, based on self-reported classifications. The standard errors used in these tests are OLS standard errors and have not been adjusted for clustering.

5 Results and discussion

5.1 Overview of program registration and compliance with program requirements

During the first year of the program, there were just over 900 farmers who registered to participate in the program, for a total of 420 acres of land. Rates of program registration at the village level was varied, ranging from 3 to about 60, with an average of about 20 individuals per village who registered for the program during this first year. While it is difficult to know for certain the total populations of these villages, a rough approximation is that there are about 200 households per village. Assuming each registrant represents a different household, this suggests that roughly 10 percent of households in the selected villages registered for the program during the first year.

Registration was higher in the villages in which the agglomeration payment was offered than in villages in which the conventional voucher was offered (518 registered participants in villages in which the agglomeration payment was offered compared with 385 registered participants in villages in which the conventional voucher was offered). With the larger number of registrant farmers, there was also a larger area registered under the program in villages being offered the agglomeration payment (244 acres, compared with 176 acres from villages being offered the conventional voucher), though the average amount of land per farmer that was registered for the program across the two payment modalities were quite similar (roughly 0.5 acres).

Farmer participation in the program increased quite markedly in the second year of the program, with registrations increasing more than threefold, to more than 2800 registrants. In this second year, there was an average of approximately 60 individuals per village who registered for the program, representing roughly 30 percent of households in the selected villages. The area registered under the program increased significantly as well, with more than 3000 acres being registered. In this second year, there were again more registered participants in villages in which the agglomeration payment was offered (1566) compared to villages in which the conventional voucher was offered (1263), though the year-over-year increase in participation rates in the latter group was slightly higher (roughly 330 percent compared to about 300 percent).

The real benefit of the program, of course, is not so much in registering to participate in the program, but in complying with its requirements. Over the two years of the program, approximately 40 percent of program registrants followed through and complied with the program's requirements, though the rate of compliance increased from 33 percent in the first year of the program to 43 percent in the second year. There were not sizable differences in compliance rates across the two payment modes. In the first year of the program, the compliance rate was 31 percent among registered participants in villages being offered the conventional voucher, and 35 percent among registered participants in villages being offered the agglomeration payment. In the second year, the compliance rates across both types of payment modes increased to 43 percent of registered participants.

Compliance rates differed quite a bit between villages in which compliance was directly monitored and verified and those in which compliance was based solely on farmers' self-reported practices. Across the two years of the program, compliance rates in villages where program compliance was monitored (either fully or partially) was 37 percent (among registered farmers), compared with 47 percent (among registered farmers) in villages that were not subject to any monitoring. Interestingly, although the rate of verified compliance remained steady over the two years of the project (at 37 percent of registered participants), the rate of self-reported compliance nearly doubled from the first year of the program to the second year of the program, from 28 percent during the first year of the program to 55 during the second year of the program. The rather dramatic increase in the rate of self-reported compliance compared to the unchanged rate of monitored compliance may indicate a tendency for inaccurate self-reporting of program compliance, perhaps due to moral hazard. Clearly, since these farmers' actual practices were never actually observed, we cannot confirm this supposition. Arguably, at a superficial level this supports the general consensus that program compliance in a traditional PES program must be monitored to ensure that payments are indeed conditioned on farmers undertaking the desired behaviors.

The potential impacts of moral hazard in the absence of program monitoring may be somewhat negated under an agglomeration payment mode of PES, since the structure of the payment mechanism imbues peer-policing that may increase program compliance even in the absence of external monitoring and enforcement. Alternatively, since the total amount of compensation under an agglomeration payment is a function of both own- and peer-participation in the program, and in the absence of monitoring both of these participation rates could be artificially inflated, there is the potential that collusion between registrants and neighbors could exacerbate the impacts of moral hazard. During the follow-up interviews, at which time plots were examined to determine whether CA practices had been properly implemented, if required by the program structure at the village level, we asked farmers to identify which of their neighbors had also registered for the program (in the case of villages in which adoption was subject to visual inspection and verification), or to simply report how many of their neighbors had registered for the program (in the case of villages in which adoption was based solely on self-reporting). In the former case, monitors would visually inspect the neighbors' plots to confirm that the neighbors had complied with the program requirements and practiced CA. In the latter case, enumerators would simply take the participant's word for it. In the first year of the program, participants in villages that were subject to some visual inspection had virtually the same number of (verified) neighbors registering in the program as the (self-reported) neighbor participation in villages not subject to any monitoring for program compliance (roughly 0.8 in both cases). In the second year of the program, a wide disparity emerged between villages with at least some monitoring of program compliance and those without any monitoring of program compliance. In the former, registrant farmers had an average of 0.55 neighbors that were verified as compliant participants in the program, while in the latter, farmers reported that they had more than one neighbor that had registered in the program. While we do not have observations on whether these neighbors did in fact register for the program, let alone comply with the program requirements, this large disparity in the levels of neighbor participation is consistent with the supposition that farmers that are not subject to monitoring may have a tendency to inflate or misstate the extent of their neighbors' participation in the program and adoption of CA, though participants may only learn of this exploitable loophole after some experience.

An important consideration in the design of PES programs is the extent to which there is "additionality" in the adoption of the desired practices; in other words, is the program able to bring additional land under the desired practices, or is the program merely compensating participants to undertake practices that – arguably – they would have done otherwise, even in the absence of direct compensation. In the course of registration, we asked interested participants whether they had mulched crop residues, intercropped or diversified crop production, or practiced zero tillage on their registered plots in the previous season. During the first year of the program, only about 15 percent of registered farmers indicated that they had practiced all three CA practices in the year preceding the initiation of the project, suggesting that roughly 85 percent of registrant farmers were potentially "additional" adopters of CA.

5.2 Effectiveness of financial incentives in encouraging CA adoption

To address our first research question, we estimate a series of difference-in-differences regression models to isolate the intention-to-treat (ITT) effect, which captures the effect of the program (offering one of the two types of incentives) on farmers' subsequent decision to practice CA, regardless of whether or not they actually registered to participate in the program (i.e., regardless of whether they were actually "treated" by the intervention by receiving subsidies to practice CA). Such effects estimates provide broad insight on the potential economy-wide impacts that might be realized if this program would be introduced at scale. The difference-in-differences estimator used in the subsequent empirical analysis can be operationalized using least squares by estimating the regression:

$$CA_{i,t} = \alpha + \beta T_i + \phi Post_t + \delta T_i \times Post_t + \sum_{j=1}^{J} \gamma_j x_{ij,0} + \varepsilon_{i,t}, \tag{1}$$

where $CA_{i,t}$ is the given outcome of interest (i.e., the various definitions of "practicing CA" that we consider; discussed in greater detail below) for farmer i in period t; $\mathbf{x}_{i,0} = \langle x_{i1,0}, x_{i2,0}, ..., x_{iJ,0} \rangle$ is a vector of covariates to control for differences in household and soil or land characteristics between households in treatment and control villages at baseline; T_i is a time-invariant binary treatment indicator equal to unity if the household resided in a village that was randomly assigned to be offered either of the two types of PES subsidy, and zero otherwise; $Post_t$ is the time varying binary indicator equal to unity if the observation is from the endline survey period, and zero if the observation is from the baseline survey period; and $\varepsilon_{i,t}$ is an idiosyncratic error term. The interaction term $T_i \times Post_t$ reflects the effect of having resided in a village randomly assigned to be in a treatment group through the period of program implementation. The α, β, ϕ, γ, and δ terms are parameters to be estimated. Specifically, δ is an estimate of the impact of the PES program on the adoption of CA in program villages. Given that exposure to subsidy treatments will be similar among members from a particular village, we relax the assumption that error terms are independently and identically distributed, and instead allow for error terms to be independent across villages but correlated within villages and adjust the standard errors for the clustered nature our design.

Table 3 reports the results from estimating the effect of the PES program in encouraging farmers to practice CA based on least squares estimation of equation (1). We consider three distinct definitions of "practicing CA". First, we consider simply the *proportion* of farmers who practice all three CA practices using a linear probability model with a binary dependent variable. Second, we consider the *extent* of and *intensity* with which they have practiced all of these three practices, considering as dependent variables both the acreage under CA (extent) as well as the area under CA as a proportion of their total cultivated area (intensity). When we consider simply the binary decision to practice CA in columns (1) and (2) in Table 3, we see that the financial incentives (regardless of the nature of the incentive) led to a nearly 10 ($p = 0.01$) percentage point increase in the likelihood that households would practice the full suite of CA practices, and this result is robust to the inclusion of additional controls for baseline imbalances. Approximately 30 percent of households in the treatment villages had adopted the full suite of CA practices at the time of the endline survey, compared with only 17 percent of households in the comparison villages.

Because this may be a relatively new and unfamiliar set of practices for those who adopted CA primarily as a result of the program, they may choose only to practice CA on a relatively small portion of their cultivated area, perhaps preferring to experiment and observe the performance under these practices before undertaking these practices on a larger scale. To test this assertion, we consider both the extent and intensity of CA. In considering the extent of CA in columns (3) and (4) of Table 3, we see that the incentive programs increased the area under CA among farmers in treatment villages by roughly 0.1 acres per farmer ($p = 0.02$) compared to farmers in the comparison villages. This may not seem like much, but given that farmers in the comparison group only practiced CA on about 0.25 acres at the time of our endline survey, providing incentives to practice CA increased the area under CA by roughly 45 percent. We also see that, not only do the incentives increase the extent of CA, but the incentives also increase the intensity with which farmers practice CA, as reported in columns (5) and (6) of Table 3. Farmers in the treatment villages, on average, practiced CA on roughly 7.2 ($p = 0.01$) percent more of their cultivated area than their counterparts in the

Table 3: Treatment effects estimation: effect of random allocation to treatment on adoption of CA

Dependent variable	Practiced CA (0/1)		Area (acres)		Area (pct. of total)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.041	0.016	0.042	−0.002	0.022	0.005
	(0.026)	(0.015)	(0.035)	(0.016)	(0.016)	(0.009)
Post	0.109	0.109	0.136	0.142	0.085	0.087
	(0.026)	(0.027)	(0.033)	(0.037)	(0.019)	(0.021)
Treatment × Post	0.091	0.091	0.119	0.107	0.075	0.072
	(0.033)	(0.034)	(0.045)	(0.047)	(0.026)	(0.027)
Additional controls	No	Yes	No	Yes	No	Yes
Observations	2751	2507	2751	2507	2751	2507
R^2	0.065	0.276	0.043	0.399	0.069	0.271
Mean for comparison group at endline	0.173		0.234		0.125	

Source: The authors.
Note: Standard errors adjusted for clustering at the village level in parentheses. Additional controls (where indicated) include area under the three constituent practices of CA, number of plots cultivated, maize yields, and soil type at baseline.

comparison villages.

5.3 Robustness of CA adoption effects

It was previously noted that there were some differences between treatment sub-groups and the comparison group that may have arisen in the population due to factors other than chance. In particular, we noted that the area under zero tillage, mulching of crop residues, and intercropping was considerably higher in the sub-sample randomly allocated to be offered the agglomeration payment PES program than in the comparison group. Since the research team was directly involved in the selection of participating villages, we can be certain that this was not the result of any sort of intentional targeting for this specific program, though we cannot be certain that these villages had not been intentionally targeted by previous agricultural development programs, including other programs that might have been promoting CA. We control for these baseline imbalances in columns (2), (4), and (6) of Table 3, but this approach will only produce unbiased estimates of the treatment effect if the functional form specified (linear) fits the underlying data generating process. Further, these differences could still pose a threat to the identification of causal effects if there were some underlying unobservable characteristics of this sub-sample that could be correlated with post-intervention outcomes. Suppose, for example, by coincidence our randomization procedure happened to select for inclusion in the treatment arms villages that had previously been intensely targeted for promotion of soil conservation. And further, suppose that there is some unobservable reason that led to these villages previously being so intensely targeted. This could have resulted in the higher levels of observed adoption of the three soil conservation practices at baseline *and* could have resulted in increased adoption of CA even in the absence of the PES intervention. This would violate the assumption that CA adoption between treatment and comparison sub-groups would have evolved along parallel trajectories in the absence of the intervention.

We do not have additional pre-intervention data (i.e., prior to the project baseline) with which to confirm that patterns of CA adoption had evolved along parallel trajectories in the pre-intervention histories. But we are able to test whether there is any evidence that certain villages in the treatment group might have been exposed to higher levels of CA promotion activities prior to our intervention. To do so, we examine patterns of adoption at the village level by aggregating the data from all sample farmers in each village and comparing these village aggregates across experimental groups. The first four rows of Table 4 report p-values from t-tests and Kolmogorov-Smirnov (KS) tests of sub-sample areas of CA adoption between treatment and comparison villages. Only the difference in the area under zero-tillage has a p-value less than 0.10. When we dig deeper into the levels of zero-tillage adoption at baseline among villages that subsequently were randomly selected to participate in the PES program, we observe one village randomly assigned to the agglomeration payment treatment that had more than 30 acres under zero tillage prior to the intervention, which may suggest that this village was previously targeted for prior soil conservation promotional activities. If we were to drop this village from the sample, the p-value associated with the t-test comparing treatment and comparison village mean levels of zero tillage adoption increases to 0.11. Within this village, however, individual adoption of zero tillage is not uniformly high, as one might expect if this village had been successfully targeted for promoting soil conservation. Indeed, 41 percent of respondents from this village reported *no* adoption of CA prior to the intervention, and an additional 26 percent reported less than 2 acres under CA prior to the intervention. There is one farmer in this village that reported more than 5 acres under CA prior to the PES program intervention, which incidently is the maximum area observed in the entire sample at the time of project baseline. Consequently, therefore, there is not much evidence that the observed differences in baseline adoption are due to any sort of explicit pre-intervention targeting of villages that were subsequently randomized into our experiment, but rather seems to be largely driven by one apparently zealous advocate of zero tillage.

We also test the stability of the treatment effects estimates under different combinations of control

Table 4: Comparison of village-level adoption of CA and constituent practices (area, ac) and average maize yields (kg/ac) at baseline

	Both treatment vs. comparison		Conventional voucher vs. comparison		Agglomeration payment vs. comparison	
	t-test	KS test	t-test	KS test	t-test	KS test
CA	0.25	1.00	0.61	1.00	0.18	0.91
Zero-tillage	0.09	0.49	0.39	0.54	0.06	0.58
Mulching residues	0.53	1.00	0.76	0.79	0.23	0.90
Intercropping (or rotation)	0.35	0.83	0.49	0.74	0.32	0.90
Maize yields (kg/acre)	0.16	0.07	0.22	0.08	0.17	0.09

Source: The authors.
Note: Figures represent p-values from statistical tests of village-level rates of CA adoption and maize yields between treatment and comparison villages. Data are aggregated from the individual farm household level to village totals among surveyed (in the case of the area under CA and constituent practices) or village mean (in the case of maize yields).

variables, including not only controls for baseline imbalances, but *all* household and agricultural production characteristics summarized in Table 1 and land and soil characteristics summarized in Table 2. The estimated treatment effects pertaining to the binary adoption decision, the extent of CA adoption, and the intensity of CA adoption under different combinations of these controls are illustrated in Figures A1, A2, and A3, respectively in Appendix A. In each of these figures, we control for (1) whether the household was in a village randomly assigned to be a part of the control group (Treated) and (2) whether the period was post-intervention in all regressions (Post), we (3) adjust the standard errors for the clustered nature of the intervention (at the village level), and (4) order the estimated treatment effects in ascending order.[6]

There are several aspects of these plots of regression specifications that perhaps merit special attention. The first thing to notice, of course, is that these figures indicate that the treatment effect – regardless of the outcome – is remarkably stable to the inclusion or exclusion of different controls. This is not surprising, since the selection of treatment villages was random, despite there being some imbalances in characteristics of farmers across the different experimental arms. Generally speaking, coefficient stability is not a sufficient condition to demonstrate unbiasedness, but the lack of coefficient movement over such a large number of widely varying permutations of coefficients is consistent with an unbiased treatment effect.

A second but related point worth mentioning, however, is that the treatment effect illustrated in Figure

[6]The specification charts in Figures A1, A2, and A3 were prepared using the `starbility` package (Rao, 2021).

A2 illustrates some minor sensitivity to the inclusion of some different control variables. In particular, there are two controls that produce apparently discrete shifts in the estimated treatment effects, namely prior experience with CA (area under each of three constituent practices at baseline) and baseline maize yields. The latter is particularly stark, since there is a clear demarcation in that the largest half of estimated treatment effects across all specifications are in models that *do not* include controls for baseline maize yields. Although the estimated treatment effects remain robust to the inclusion of controls for maize yields, the shift in the estimated treatment effect and the reduction in the precision with which this effect is estimated suggests the importance of controlling for maize yields in estimating this effect.

As mentioned previously, there were differences in baseline maize yields between the treatment and comparison sub-samples, and this imbalance could potentially threaten our identification of treatment effects if, for example, these differences in baseline yields reflect different levels of agricultural suitability. We first consider whether there is any evidence of potential differences in agricultural suitability by comparing village-level average maize yields at the time of project baseline. In the final row of Table 4, we report p-values from t-tests and KS-tests comparing baseline maize yields from treatment and comparison villages. There is rather compelling evidence that the average maize yields in villages randomly assigned to both the conventional voucher and the agglomeration payment treatment were higher than the average maize yields in villages randomly assigned to the comparison group, with p-values less than 0.09.

Could these differences in baseline maize yields reflect differences in underlying agricultural suitability, and could we attribute the estimated treatment effects to these differences in agricultural suitability rather than the PES program? That leads to the final point worth noting pertaining to figures A1, A2, and A3 and the regressions underlying them. Among the controls included in these myriad regression specifications are a comprehensive set of variables that might reflect agricultural suitability, such as soil quality, land slope, and the extent of soil erosion. As previously mentioned, the treatment effects coefficients are remarkably stable, which is consistent with what we would expect if decisions pertaining to the practice of CA as a result of the PES program were independent of agricultural suitability.

Appendix A documents some other steps that we have taken to demonstrate the robustness of our main results. In particular, we conducted a Monte Carlo placebo test across 10,000 simulations to verify that the observed treatment effects are due to the program intervention and not due to other unrelated features (Figure A4 in Appendix A). If the treatment effect reported in Table 3 was due to anything other than random allocation to participation in the PES program, we would expect to a nontrivial share of these placebo effects to be nonzero. Instead, we observe a near-universal null placebo effect. Indeed, in only about 8 percent of the simulated cases is the p-value on the the placebo effect less than or equal to 0.05.

Additionally, we implement a Monte Carlo sensitivity analysis to test whether the observed results in Table 3 are specific to the sample used in the primary analysis, and specifically whether a small percentage of our sample has been excessively influential in determining our results. Across 10,000 simulations, we randomly select 10 percent of observations, exclude them from the sample, and re-estimate the effect of the PES program (Figure A5 in Appendix A). The estimated treatment effects are highly robust to alternative samples, and under virtually every simulated sample, we can be 95 percent confident that the actual treatment effect is greater than zero. When we consider the effect of treatment on the proportion of farmers in treatment villages that practiced CA, the average treatment effect across the 10,000 simulations is roughly 0.09, with point estimates ranging from 0.07 to 0.11. This is virtually identical to the estimated effect from our full sample, confirming that the effect of the program on adoption of CA is very insensitive to the specific sample under which the effects are being analyzed.

We also re-estimated equation (1) restricting the sample to only those participants who had *not* practiced CA prior to the intervention. These results are reported in columns (1)-(3) in Table A1 in Appendix A. Rather than finding an attenuated treatment effect among the newly exposed farmers, we actually observe that the estimated treatment effects are larger (or at least no less) among this restricted sample. Finally, we re-estimated equation 1 incorporating both individual-/farm- and time fixed effects to further control for unobserved heterogeneity that could confound the estimated treatment effect. These results are reported in columns (4)-(6) in Table A1 in Appendix A. Although the treatment effects after controlling for unobserved heterogeneity through two-way fixed effects are slightly attenuated relative to the difference-in-differences estimates reported in Table 3, the attenuation is very slight, and does not detract from the conclusions drawn based on the main results reported in Table 3.

In sum, the above tests are highly suggestive that the treatment effects observed in this pilot and reported in Table 3 are reliable estimates of program impacts, and are robust to varying assumptions of specific sample and alternative explanations of observed adoption of CA. Consequently, in what follows, we will maintain our original full sample and maintain the assumption that any estimated effects from participating in the PES represent true and unbiased program effects, and similarly robust.

5.4 Differential effects by PES payment modes and monitoring intensity

We next consider whether the different payment modalities (conventional voucher or agglomeration payment) result in different levels of CA relative to the comparison villages. To do so, we slightly modify equation (1) to allow for more than one binary treatment indicator. Specifically, we estimate the following regression:

$$CA_{i,t} = \alpha + \sum_{j=1}^{J} \gamma_j x_{ij,0} + \beta_1 T_{i,C} + \beta_2 T_{i,AP} + \phi Post_t + \delta_1 T_{i,C} \times Post_t + \delta_2 T_{i,AP} \times Post_t + \varepsilon_{i,t} \tag{2}$$

where $CA_{i,t}$; $\mathbf{x}_{i,0}$; $\varepsilon_{i,t}$; and α, β, ϕ, and γ terms are defined as before, and $T_{i,C}$ and $T_{i,AP}$ are binary variables indicating random assignment to the conventional voucher and agglomeration payment treatments, respectively, with associated parameters δ_1 and δ_2 capturing the effects of these alternative payment modalities on adoption of CA.

In addition, we consider the differential impacts of monitoring intensity on farmers' practicing CA. As discussed above, there is evidence that farmers may have inflated their level of CA adoption if they were not subject to visual inspection, but it is not immediately clear whether the extent of this misrepresentation may be lesser or greater in villages with the agglomeration payment compared with those that were offered the conventional voucher. The extent of the misrepresentation may be lesser in villages with the agglomeration payment, since this payment modality intrinsically incorporates peer monitoring. On the other hand, since the total compensation under this payment modality has the potential to be greater, and the peer bonus is conditioned on one's own level of CA adoption, there may be an increased temptation to overstate the extent of one's own adoption of CA in an effort to maximize potential compensation. Assessing the differential impacts of monitoring intensity further modifies equation (2) by introducing interaction terms between the two binary indicators for payment modality with two binary indicators for monitoring intensity (partial monitoring or full monitoring). Specifically, we estimate the following regression:

$$\begin{aligned} CA_{i,t} = \alpha + \sum_{j} \gamma_j x_{ij,0} + \sum_{p} \beta_p T_{i,p} + \sum_{p} \sum_{m} \theta_{p,m} T_{i,p} \times M_{i,m} + \phi Post_t + \sum_{p} \lambda_p T_{i,p} \times Post_t + \\ \sum_{p} \sum_{m} \delta_{p,m} T_{i,p} \times M_{i,m} \times Post_t + \varepsilon_{i,t} \end{aligned} \tag{3}$$

where $T_{i,p},\ p \in \{C, AP\}$ refers to random assignment to one of the two PES payment modes, either the conventional voucher ($p = C$) or the agglomeration payment ($p = AP$); and $M_{i,m},\ m \in \{P, F\}$ refers to the monitoring intensities, ranging from partial monitoring ($m = P$) to full monitoring ($m = F$). Consequently, λ_C and λ_{AP}, are the parameters associated with the effect of the different payment modalities on practicing CA in villages *without* any monitoring; and $\delta_{C,P}$, $\delta_{C,F}$, $\delta_{AP,P}$, and $\delta_{AP,F}$ reflect the additive effect of coupling monitoring efforts with these two payment modalities. Note that in Equation (3) we do not allow M_P and M_F to enter the regression apart from interactions with the payment modality indicators, since there is no monitoring efforts outside of the two payment treatment groups.

The results of estimating equations (2) and (3) by difference-in-differences are reported in Table 5 (for total area under CA) and Table B1 in Appendix B (for the binary adoption decision and the intensity of adoption). In Figure 2, we plot the primary treatment effects of interest from Table 5 for ease of exposition. The first two columns in Table 5 and the two coefficient plots in panel (A) of Figure 2 report regression results with treatment status disaggregated between the conventional voucher and the agglomeration payment treatments, based on estimating equation (2). Here, we focus on the *extent* of CA adoption, based on farmers' total area under the full set of three CA practices (cf. columns 3-4 in Table 3). From our preferred specification (with controls for baseline imbalances), we see that the largest increase in the area under CA can be attributed to the conventional voucher, with an average increase of roughly 0.15 ($p = 0.01$) acres compared to their counterparts in the comparison villages. Farmers in villages randomly assigned to receive the agglomeration payment also increased their area under CA, though by a smaller margin (an average increase of roughly 0.07 acres), with this effect also measured less precisely ($p = 0.17$). Despite these estimates seemingly being quite different, however, the associated p-value on this difference is 0.81.

The final two columns Table 5 and the six error bar plots in panel (B) of Figure 2 (to the right of the vertical dashed line) report regression results assessing the effects of varying monitoring intensities on adoption of CA in addition to providing direct compensation, based on estimating equation (3) using least squares. Focusing first on farmers in villages offered the conventional voucher, there is suggestive evidence that farmers may take advantage of the lack of monitoring and over-state the intensity of their adoption of CA. Specifically, for farmers in villages that were offered the conventional voucher and yet were not subject to monitoring, they reported an increase in the adoption of CA adoption of roughly 0.26 ($p = 0.001$) acres relative to farmers in comparison villages. When there is full monitoring, however, in which all registrants have their fields inspected, there is a marked *reduction* in the area under CA relative to their counterparts who only self-reported their adoption of CA. Farmers in these villages were observed to have adopted CA on 0.21 ($p = 0.03$) *fewer* acres than counterparts who only self-reported their adoption of CA. Consequently, the net effect of the PES program consisting of a conventional voucher with extensive monitoring is relative to the comparison group is dramatically reduced, to the point of being virtually eliminated.[7] This suggests that much of the increase in CA adoption that was discussed above (cf. Table 3) may have been largely driven by the self-reported adoption in villages that were not subject to any compliance monitoring. Since this self-

[7] Referring back to equation (3), the effect of the conventional voucher in villages without monitoring is given by λ_C, while the effect of the conventional voucher in villages subject to partial or full monitoring is given by $\lambda_C + \delta_{C,P}$ and $\lambda_C + \delta_{C,F}$, respectively. The linear combination $\lambda_C + \delta_{C,P} = 0.10$ ($p = 0.82$) and the linear combination $\lambda_C + \delta_{C,F} = 0.06$ ($p = 0.90$).

reported adoption was never actually confirmed through visual inspection, we cannot be certain that these farmers were actually practicing CA, but the stark difference in farmers' self-reported level of compliance with program requirements and the confirmed level of compliance in villages subjected to monitoring is certainly suspicious. Further, if they were not actually practicing CA, we cannot ascertain whether the self-reported level of adoption reflects deliberate fraud or a simple misunderstanding of the actual program requirements. In any case, it seems very likely that the level of self-reported adoption of CA is an overstatement of actual program compliance, and thus reflects some farmers receiving compensation despite their actual noncompliance with the program requirements. This result certainly suggests the importance of monitoring compliance with program requirements to ensure that resources are not being wasted. This is the standard logic underlying most PES programs: Since PES payments are made *conditional* on verified compliance with the program requirements, monitoring is almost always required to confirm that participants have actually undertaken the desired land use practice needed to generate the ecosystem services.

Across villages that were offered the agglomeration payment structure, there is considerably less evidence that "official" monitoring would actually be required to confirm that registrants are complying with their obligations. From Table 5, we observe that farmers from villages that were offered the agglomeration payment adopted CA on 0.07 ($p = 0.17$) more acres than counterparts in comparison villages. When we decompose this sample further based on the intensity of monitoring to which they were subject, we find that the extent of adoption in un-monitored villages is indistinguishable from the extent of adoption in fully monitored villages.[8] Although these different effects are not measured very precisely, these results nonetheless suggests that there is no evidence of any systematic overstatement of CA adoption in villages in which farmers merely self-report their practices. This, in many ways, exemplifies one of the principal benefits of the agglomeration payment structure: because total payments are conditional on others' behavior, there is an interconnectivity of rational self-interests that may almost organically engender a peer-policing mechanism among villagers. We do not have data with which to explicitly test this supposition (i.e., we do not have information on whether farmers in these villages actively monitored their neighbors' activities or whether they expended any efforts in peer-policing), but the results are certainly consistent with this hypothesis. This is an important result, since monitoring participants' compliance with program requirements can be very expensive, representing a significant share of the overall transaction costs for these types of programs. When overall transaction costs are large, they may even outweigh the private gains from participation (Alston et al., 2013). In the case of

[8] Similar to what was mentioned in Footnote 7, the effect of the agglomeration payment in villages without monitoring is given by λ_{AP}, while the effect of the conventional voucher in villages subject to partial or full monitoring is given by $\lambda_{AP}+\delta_{AP,P}$ and $\lambda_{AP}+\delta_{AP,F}$, respectively. In un-monitored treatment villages, farmers reported practicing CA on 0.07 ($p = 0.36$) acres of land (λ_{AP}). The linear combination $\lambda_{AP} + \delta_{AP,P} = 0.02$ ($p = 0.96$) and the linear combination $\lambda_{AP} + \delta_{AP,F} = 0.12$ ($p = 0.77$).

the agglomeration payment, the structure of the payment mechanism itself may produce sufficient incentives for peer-policing that external "official" monitoring may not be necessary.

Table 5: Treatment effects estimation: effect of alternative incentive mechanisms and monitoring intensities on adoption of CA

Dependent variable: Area under CA (ac)	(1)	(2)	(3)	(4)
Conventional voucher (β_C)	0.017	0.008	0.014	−0.006
	(0.039)	(0.017)	(0.057)	(0.018)
Agglomeration payment (β_{AP})	0.064	−0.012	0.047	−0.008
	(0.043)	(0.020)	(0.054)	(0.022)
Post (ϕ)	0.136	0.142	0.136	0.142
	(0.033)	(0.037)	(0.033)	(0.037)
Conventional voucher × Post (λ_C)	0.169	0.149	0.252	0.265
	(0.058)	(0.059)	(0.085)	(0.083)
Agglomeration payment × Post (λ_{AP})	0.074	0.071	0.073	0.068
	(0.048)	(0.052)	(0.070)	(0.075)
Conventional voucher × Partial monitoring ($\theta_{C,P}$)			0.018	0.057
			(0.057)	(0.017)
Conventional voucher × Full monitoring ($\theta_{C,F}$)			−0.003	0.001
			(0.077)	(0.023)
Agglomeration payment × Partial monitoring ($\theta_{AP,P}$)			0.083	0.002
			(0.094)	(0.046)
Agglomeration payment × Full monitoring ($\theta_{AP,F}$)			−0.020	−0.015
			(0.061)	(0.025)
Conventional voucher × Partial monitoring × Post ($\delta_{C,P}$)			−0.094	−0.161
			(0.123)	(0.110)
Conventional voucher × Full monitoring × Post ($\delta_{C,F}$)			−0.164	−0.208
			(0.104)	(0.095)
Agglomeration payment × Partial monitoring × Post ($\delta_{AP,P}$)			−0.051	−0.048
			(0.090)	(0.094)
Agglomeration payment × Full monitoring × Post ($\delta_{AP,F}$)			0.049	0.053
			(0.077)	(0.081)
Additional controls	No	Yes	No	Yes
Observations	2751	2507	2751	2507
R^2	0.044	0.401	0.048	0.406
Mean for comparison group at endline (ac)		0.234		

Source: The authors.
Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline levels of the dependent variable. Additional controls (where indicated) include area under the three constituent practices of CA, number of plots cultivated, maize yields, and soil type at baseline.

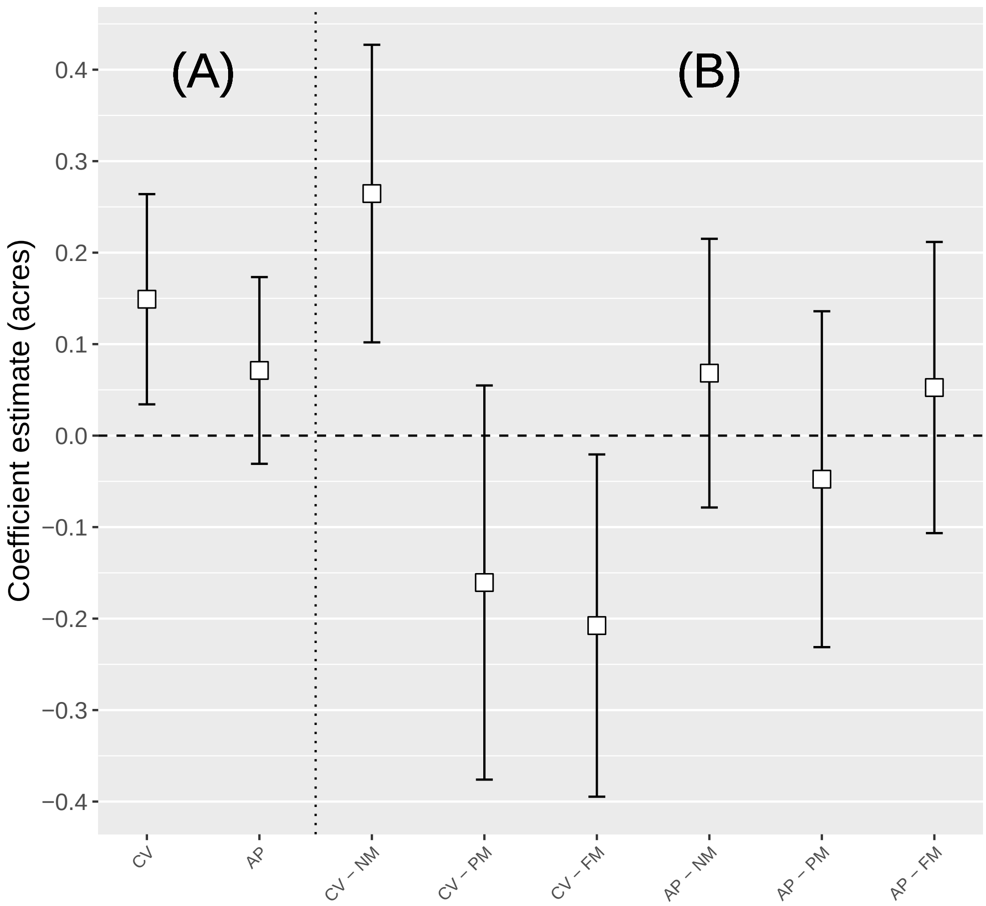

Figure 2: Regression coefficients for treatment effects of different PES payment modes and monitoring intensities on intensity of CA adoption (% of total area)

Notes: CV = Conventional voucher; AP = Agglomeration payment; NM = No monitoring; PM = Partial monitoring; FM = Full monitoring. Error bars represent 95 percent confidence intervals, based on standard errors adjusted for clustering at the village level. Additional controls (where indicated) include area under the three constituent practices of CA, number of plots cultivated, maize yields, and soil type at baseline.

5.5 Effectiveness of financial incentives in encouraging adoption of the three constituent practices of CA

Despite the evidence that complete adoption may produce the greatest benefits, partial adoption remains a widespread phenomenon, perhaps because complete adoption of CA requires multiple behavioral changes that may entail real or perceived trade-offs. Indeed, Ward et al. (2018) find that farmers in Malawi tend to view adoption of CA practices as a series of separate – though not wholly independent – decisions. In particular, they found a strong interrelatedness between zero tillage and mulching crop residues, but rather weak relationships between these two practices and intercropping or crop diversification, though practicing intercropping or rotation in conjunction with either of the other two practices did not have a negative effect on the adoption of the third practice. Chiputwa et al. (2011) found that relatively few of the farmers in their sample (about 20 percent) adopted the full set of CA practices, but the practice or set of practices that were adopted were conditioned by distinct farm household and agroecological characteristics, including factors such as household labor capacity, access to credit for procuring inputs, and the frequency of visits from extension agents.[9]

While farmers that only partially adopt CA may be forgoing some agricultural or environmental benefits vis-á-vis full adoption, there may nevertheless be some important soil conservation or productivity benefits from partial adoption. Table C1 in Appendix **??** reports the results of treatment effects regressions assessing the impact of random assignment to being offered PES incentives on each of the three constituent practices, with Panel (A) pertaining to adoption of zero tillage, Panel (B) pertaining to the mulching of crop residues, and Panel (C) pertaining to intercropping with legumes or integrating legumes into a crop rotation. These regressions are identical to equation (1), except the outcome variables (the Ys) are *each* of the different practices, rather than *all* of the practices. Similar to Table 3, the first two columns in each panel reports the results of the incentive treatment on binary adoption (without and with additional controls, respectively), while the remaining columns report the results of the incentive on the extent (area) and the intensity (proportion of total cultivated area) of adoption (columns 3-4 and columns 5-6, respectively).

From Panels (A) and (B) of Table C1, we see that financial incentives increased the proportion of farmers of adoption of zero tillage and the retention and mulching of crop residues by 5 ($p = 0.18$) and 8 ($p = 0.04$) percentage points, respectively. This compares to the levels of adoption among the comparison group (at endline) of approximately 30 percent and 40 percent, respectively. Consequently, incentives increased the rate of adoption of zero tillage by about 20 percent and the rate of adoption of retention and mulching of crop residues by about 20 percent over levels in comparison villages.

Further, although there is little evidence that the incentives had any effect on the extent of area under zero tillage or mulched residues, there is some evidence that the incentives increased the *intensity* with which farmers adopted these practices. The incentives increased the intensity of zero tillage by about 4 ($p = 0.29$) percent of the total area, relative to a base level of about 20 percent of cultivated area in the comparison villages. Similarly, the incentives increased the intensity of mulching crop residues by about 4 ($p = 0.27$) percent of the total area, relative to a level of roughly 35 percent of cultivated area in the comparison villages.

We find no evidence that the incentives had any appreciable effect on the adoption of intercropping

[9] In Chiputwa et al. (2011), the authors considered zero tillage, crop rotations, and contour ridging to comprise "CA" rather than the more conventional definition integrating mulching of crop residues.

or integrating legumes into a crop rotation. This is not particularly surprising, since the baseline level of intercropping in the full sample was more than 80 percent, which is clearly difficult to improve upon in any meaningful way even with financial incentives.

Tables C2, C3, and C4 in Appendix C report results of regressions parsing out the differential effects of the two payment modalities and the varying monitoring intensities on the adoption of the three constituent practices. In Tables C2 and C3, we see that the conventional voucher increased the adoption of both zero tillage and the mulching of crop residues. In particular, the conventional voucher led to an average of a 7.3 ($p = 0.11$) percent increase in the intensity of zero tillage adoption and a 7.1 ($p = 0.06$) percent increase in the intensity of mulching crop residues relative to the comparison group, though there is no evidence that monitoring program compliance had any effect on the overall intensity of the adoption of either of these two practices. Indeed, these average effects mask the considerably larger effect that is observed in villages in which program registrants were *not* subject to any monitoring. In these villages, we observe an increase of 15 ($p = 0.01$) percentage points under zero tillage and a 11 ($p = 0.03$) percentage points retaining and mulching crop residues. We find little evidence that the agglomeration payment had any appreciable impact on the intensity of either zero tillage adoption or mulching residues, regardless of whether there was any monitoring in force. Further, we find little evidence that either form of payment had any effect on intercropping or integrating legumes into a crop rotation, with or without any form of monitoring.

One of the rationales behind the agglomeration payment is that it essentially turns program registrants into unofficial extension agents, since they have a financial incentive to increase the adoption of CA among their neighbors. Table D1 in Appendix D reports the effects of the program on various dimensions of communication among villagers.[10] Though the effects are not measured precisely, the point estimates reported in Table D1 are at least consistent with the notion that the agglomeration payment may have induced greater communication among peer networks. On average, farmers from villages randomly selected to be offered the agglomeration payment incentive reported more neighbors than their counterparts in comparison villages , and more neighbors than their counterparts from villages randomly selected to be offered the conventional voucher . There is also evidence (albeit again statistically weak) that the agglomeration payment increased the extent of farmers' communication with their peers about agriculture. There is not, however, evidence that would be consistent with the agglomeration payment increasing the extent of farmers' conversations about CA, and indeed the point estimate suggests the opposite, though this effect is measured imprecisely.

Despite the lack of evidence that the agglomeration payment led to increased communication with neigh-

[10]The results in Table D1 are based on simple cross-sectional regressions, since the information on communication with neighbors was only gathered during the endline survey.

bors about CA, and even though we do not find that the agglomeration payment increased the adoption of zero tillage among the farmers in our sample, there is nevertheless rather convincing evidence that soil conservation practices became more widespread during the lifetime of the project. Overall, from 2014 to 2016, farmers in our sample reported a more than threefold increase in the number of their neighbors that were practicing reduced tillage (from an average of 0.3 neighbors practicing reduced tillage in 2014 to 1 neighbor practicing reduced tillage in 2016).[11] There is evidence that some of this expansion in reduced tillage is due to the introduction of the agglomeration payment. In the first column of Table D2, we see that farmers in villages randomly selected to receive the agglomeration payment reported roughly 0.35 ($p = 0.04$) more of their neighbors that were practicing reduced tillage at endline compared with their counterparts in comparison villages. Farmers in the latter group reported an average of only 0.8 neighbors practicing reduced tillage at the time of our endline survey, so the agglomeration payment effect represents a roughly 40 percent increase in neighbors' adoption of reduced tillage due to the agglomeration payment.

There is also some evidence that perhaps the agglomeration payment might be effective in increasing the retention and mulching of crop residues, though the statistical support for these assertions is mixed (column 2 of Table D2). Although there is not convincing statistical evidence that farmers from the villages randomly selected to receive the agglomeration payment reported a larger number of community members mulching crop residues relative to farmers from the comparison villages (average effect of 12.9; $p = 0.02$), there is quite convincing evidence that the agglomeration payment structure is more effective at increasing the retention and mulching of crop residues relative to a conventional voucher. On average, farmers from villages with agglomeration payments reported 6 ($p = 0.01$) more members of their community mulching crop residues than farmers from villages with only conventional vouchers.

There are other practices apart from those explicitly included in the relatively narrow definition of CA that could have similar effects on reducing soil erosion: specifically, planting in stone bunds, sowing seeds in planting pits, or using terraces. In columns 3 through 5 of Table D2, we report the estimated effects of the conventional voucher and the agglomeration payment on the expansion of these practices in our sample communities. Though measured somewhat imprecisely, the average effects of the agglomeration payment on each of these alternative practices are all positive, suggesting that the presence of the agglomeration payment may increase the spread of a number of other soil conserving agricultural practices, even though they may not have been directly promoted through the PES program.

Even though there may be rather weak evidence that the agglomeration payment increased the use of

[11] Here, reduced tillage is any land preparation method that reduces soil disturbance relative to conventional tillage methods, including but not limited to zero tillage.

these alternative soil conserving measures relative to the comparison villages, there is rather convincing evidence that the agglomeration payments are more effective at increasing the use of these measures relative to conventional vouchers. In the agglomeration payment villages, respondents reported an average of 6.3 ($p = 0.0003$) more members of the community sowing in planting pits, 5.7 ($p = 0.07$) more members of the community planting in stone bunds, and 1.9 ($p = 0.03$) more members of their community using terraces than their counterparts in villages offered the conventional voucher.

One possible explanation for why the agglomeration payment appears to be effective in expanding soil conservation practices is that the nature of the agglomeration payment encourages – and indeed leverages – social interactions, and these social interactions amplify the impacts of the financial encouragements and contribute to expanded soil conservation. As we have seen, there is some evidence – albeit not measured very precisely – that farmers in agglomeration payment villages reported a greater number of "neighbors" and reported talking with more "neighbors" about agriculture, though not necessarily about CA. This is perhaps especially true relative to those farmers from villages participating in the conventional voucher program, who may have feared that increased adoption of CA or other soil conservation measures among the other members of their community might result in reduced per capita payments in future years.

5.6 Economics of CA adoption: yield and labor demand effects

5.6.1 Effects of CA adoption on maize yields

How sustainable are the observed changes in land management practices? It is frequently suggested that farmers would require sustained financial support – at least over a 5-10 year period – to offset the yield losses that they suffer when initially transitioning from conventional tillage to CA. But it is believed that, after overcoming this initial transition period, the yield gains that result from the improvements in soil structure and soil fertility could create a reinforcing feedback mechanism that could encourage sustained adoption of CA, even in the absence of external support.

Figure 3 plots the empirical distributions of maize yields (log transformed) at endline between farmers that practiced CA and those that did not.[12] We focus on maize because it is by far the most widely cultivated crop among farmers in our sample area, and indeed throughout Malawi. Although there is considerable overlap between the two yield distributions, and nearly indistinguishable modes, the yield distribution for households that practiced CA does appear to suggest slightly higher average yields than households that did not practice CA. Based on a Kolmorogov-Smirnov test, we reject the null hypothesis that the distributions

[12] Yield data were Winsorized at the 2.5th and 97.5th percentiles of the raw data.

of maize yields for these two sub-samples are equivalent in favor of the one-directional alternative hypothesis that indeed the distribution of maize yields for households that practiced CA is indeed greater than the distribution of maize yields for households that did not adopt CA ($p = 0.01$). Although this may suggest of a positive impact of the PES program on maize yields, it fails to consider the potential role of unobservable characteristics that may have simultaneously contributed to both farmers' decision to adopt CA and the resultant higher yields.

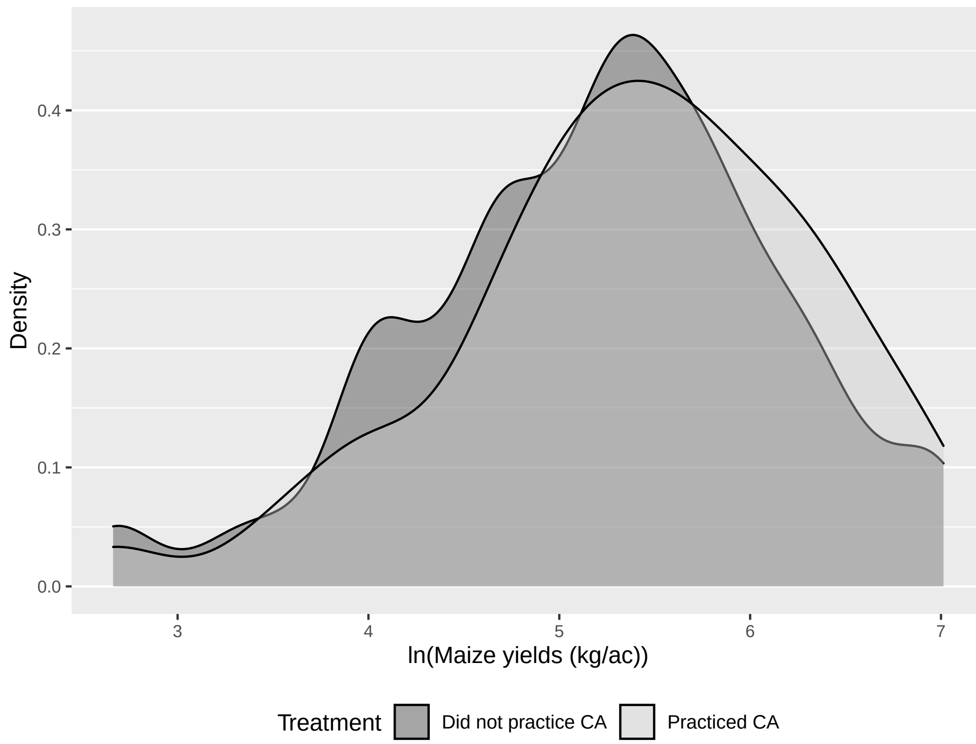

Figure 3: Empirical distribution of yields between those practicing CA and those practicing conventional tillage at project endline

To estimate the effect of practicing CA on maize yields, we estimate the following regression:

$$\ln(Y_{it}) = \alpha + \delta CA_{it} + \sum_{j=1}^{J} \beta_j x_{ijt} + \mu_i + \phi_t + \varepsilon_{it} \tag{4}$$

where $\ln(Y_{it})$ is the natural lograrithm of household i's maize yields in period t; CA_{it} is one of the three measures of CA adoption we have previously introduced (i.e., the binary indicator for practicing CA, the measure of the extent of CA adoption, or the measure of the intensity of CA adoption); $x_{it} = \langle x_{i1t}, x_{i2t}, ..., x_{iJt} \rangle$ is a vector of household and farm-level controls that condition yields (e.g., such as family and hired labor, expenditures on pesticides, number of plots and total area cultivated, and soil conditions); μ_i which reflects individual and farm fixed effects; and ϕ_t is a control for time fixed effects.[13] We first restrict $\mu_i = 0$, which permits straightforward estimation by least squares. Further, this permits a more inclusive vector x_{it} since time-invariant facftors can be included. The results of estimating this series of regressions via least squares are reported in columns (1)-(3) of Table 6. Based on these regressions, it would appear as though practicing CA has a beneficial effect on maize yields. Farmers that practice CA produce yields about 12 higher than farmers not practicing CA, even after accounting for agricultural suitatibility (e.g., land and soil characteristics).

The problem with restricting $\mu_i = 0$, of course, is that the decision to practice CA or the area or proportion of area on which to practice CA is an individual decision that may be correlated with other unobservable factors that condition yields, so estimates of δ would very likely be biased. To limit the influence of this selection bias, we allow $\mu_i \neq 0$ which allows us to control for the unobservable heterogeneity at the individual level. Now, however, the vector of individual and farm-level controls thought to influence maize yields is now restricted to those factors that are time-varying.

The results from estimating equation (4) via two-way fixed effects linear regression are reported in columns (4)-(6) of Table 6. Contrary to what we observed above when ignoring this potential selection bias, there is now no longer an apparent yield gain from practicing CA. This suggests that much of the yield gains observed in columns (1) and (3) are attributable not so much to the underlying principles of CA, but rather to unobservable farmer and farm characteristics. However, even after controlling for unobservable farmer and farm characteristicds, there also does not appear to be a yield penalty in the first years after adoption, contrary to what might be expected given the prevailing belief about the yield effects of CA.

The results in columns (1)-(6) of Table 6 mask a potentially important and relevant source of heterogeneity, namely the potential that CA may affect yields differently under varying weather conditions. Although we are not able to control for specific weather conditions experienced by the farmers in our sample, we are able to assess whether there are different yield effects at different points in time, namely at the time of project baseline and at the time of project endline. If there were different weather conditions in these two agricultural seasons, such differences might produce different yield effects from CA, as has been found in

[13]Data have been aggregated to the household level, so there is not a one-to-one matching of plots with agricultural practices and maize yields. However, given the predominance of maize cultivation and the relatively limited variation in the number of plots cultivated, we think it seems reasonable to assume that the agricultural practices (CA or otherwise) that serve as the primary explanatory variable in these regressions are reasonable determinants of the subsequently observed maize yields. In addition, since there is very little change in total cultivated area between baseline and endline, any increase in either the extent or intensity in the area under CA almost certainly represents a conversion of land that had been previously been used for maize cultivation under conventional tillage.

Table 6: Effect of practicing CA on ln(maize yields)

Dependent variable: ln(Maize yields (kg/ac))	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Practiced CA	0.12			−0.05			−0.18		
	(0.06)			(0.08)			(0.12)		
Practiced CA×Post							0.16		
							(0.12)		
Area under CA (ac)		0.02			−0.04			−0.08	
		(0.04)			(0.05)			(0.08)	
Area under CA (ac)×Post								0.05	
								(0.07)	
Area under CA (% total area)			0.13			−0.04			−0.25
			(0.08)			(0.10)			(0.18)
Area under CA (% of total area)×Post									0.24
									(0.17)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Farmer fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2456	2456	2456	2456	2456	2456	2456	2456	2456
R^2	0.26	0.25	0.26	0.35	0.35	0.35	0.35	0.35	0.35

Source: The authors.
Note: In columns (1)-(3), standard errors adjusted for clustering at the village level. Across all regressions, additional controls include the age of the household head, family agricultural labor (male and female), hired agricultural labor (male and female), pesticide expenditures, the number of plots cultivated (and its square), and total area cultivated (and its square). In columns (1)-(3), the regressions include controls for soil type, soil quality, the extent of soil erosion, and the slope of agricultural lands. These 'agricultural suitability' controls are omitted from fixed effects regressions in columns (4)-(9) because they are time invariant and are eliminated through the fixed effects transformation. Full regression results including all additional controls are reported in Table E1 in Appendix E.

previous studies. To examine whether there are potentially heterogeneous yield effects of CA, we modify equation (4) to incoprorate CA-by-time interactions:

$$\ln(Y_{it}) = \alpha + \delta_1 CA_{it} + \delta_2 CA_{it} \times \phi_t + \sum_{j=1}^{J} \gamma_j x_{ijt} + \mu_i + \phi_t + \varepsilon_{it} \tag{5}$$

The results from estimating equation (5 are reported in columns (7)-(9) in Table 6. Although not measured very precisely, these results suggest that there may be a yield penalty in some years and a yield advantage in other years.[14] In particular, practicing CA led to 18 percent lower yields during the 2013-2014

[14]One concern with estimating the yield effects of CA is that we do not observe the actual agricultural practices for those farmers in villages that were not subject to any monitoring, and, as discussed above, there may be a tendancy for farmers to over-report practicing CA if their actual practices are not monitored. In Table E2 in Appendix E report the regression results under two alternative specifications, one in which those households in villages that were not subject to monitoring are deemed to have not practied CA, and one in which we estimate the yield effects of CA only among households in villages subject to *some* level of monitoring (vis-á-vis the comparison group). Qualitatively, the results are similar to those reported in Table 6, though the evidence in Table E2 suggests that perhaps there is not a serious problem of over-reporting. In particular, we note that the yield effects in Column pairs (1) and (3), (4) and (6), and (7) and (9) in Table E2 – which pertain to the full sample and the restricted sample excluding treatment

season, while practicing CA led to 16 percent higher yields during the 2015-2016 season. There are some important nuances in the interpretation of these apparently differential yield effects. First, the 18 percent lower yields during the 2013-2014 season might not be as bad as the raw number may at first appear. Nationwide, maize production during 2013-2014 appears to have been extraordinarily good. According to national production statistics (FAO, 2020), maize yields in 2014 (which roughly corresponds to our baseline period of the 2013-2014 rainy season) were the second highest on record (dating back to 1960) and were 38 percent higher than the average yields over 2000-2013.[15] Consequently, although the observed yields among farmers practicing CA during 2013-2014 are lower than those observed among farmers practicing conventional agriculture, their yields would likely still be higher than the average yields over the 2000-2013 period.

Second, the 16 percent yield gains that are observed from practicing CA during the 2015-2016 season coincide with a period of overall low yields nationally. In our data, average maize yields fell by roughly 45 percent between 2013-2014 and 2015-2016. Nationwide, maize yields in 2016 (roughly corresponding to our endline period of the 2015-2016 rainy period) were 15 percent below those from 2015, and nearly 40 percent below maize yields from 2014 (roughly corresponding to our basline period of the 2013-2014 rainy period) (FAO, 2020). The decline in maize yields observed during the 2015-2016 rainy season may be largely due to abnormally low rainfall that was observed during some key periods of maize growth cycle during that season as a result of the El Niño Southern Oscillation. Although total rainfall during the rainy season was only about 5 percent below the average rainy season rainfall over the 1991-2016 period, rainfall during February and March, which roughly corresponds to the grain formation and ripening stages of the maize growth cycle, were about 15 percent and 25 percent below the 1991-2016 averages, respectively. Crops are especially water-sensitive during these latter stages, and in the absence of supplemental irrigation (only 11 percent of farmers in our sample had access to irrigation during 2015-2016, and of those that had access to irrigaiton, they only had access on about 35 percent of their cultivated area), rainfall deficiencies such as these can have significant effects on yields under conventional practices without means of moisture retention (like mulch). In addition, rainfall during the planting period (beginning in November 2015) was approximately 45 percent below the 1991-2016 average, and although this is not a period in which crop growth is particularly sensitive to water deficiencies, the deficient rainfall during this period may actually have been disproportionately beneficial for farmers practicing CA, and this in turn may contribute to explaining the yield differentials. One of the frequent criticisms of CA is that reducing or minimizing soil disturbance increases weed pressure,

villages that were not monitored – are very similar.

[15] These national agricultural statistics do not differentiate between rainy season and the dry season yields, so there is not a perfect correspondence in the periods over which yields are measured.

which leads to either increased labor or increased expenditures on herbicides. But weeds – like plants – require moisture to grow, so the below average rainfall early in the season may have limited weed formation relative to what might be expected under normal weather conditions, which may have benefitted – or at least not harmed – farmers practicing CA. Further, because mulching of crop residues in a complete CA system helps to retain soil moisture, farmers adopting CA might have had sufficient residual soil moisture that the deficient rainfall early in the season may not have impacted farmers practicing CA the way it did farmers practicing conventional tillage.

This is consistent with the meta analysis in Pittelkow et al. (2015) that finds positive effects of CA in dry environments. But the Shire basin is not an inherently dry environment on a year-in, year-out basis. When taking the wide-angle view that, on average across the duration of the pilot, yields under CA were statistically indistinguishable from those under conventional practices, but were less extreme on either the upside (under good conditions) or the downside (under adverse conditions), it would seem that CA yields may *second-order stochastically dominate* yields under conventional agriculture: expected yields may be the same (to a rough approximation) under CA as compared with under conventional agriculture, but yields under CA may exhibit a lower variance.[16] As such, any risk-averse farmer would prefer the yield provile conferred by a CA system to the yield profile under a conventional agriculture system. These results are thus further consistent with some recent evidence (e.g., Michler et al., 2019) that suggests CA may help to build farmer's resilience to climate variability. Adjusting current year practices to adverse growing conditions (even without considering the longer-term soil health benefits accruing) could be sufficient to reduce vulnerability, which could be an important livelihood objective in its own right.

5.7 Impacts of CA on agricultural labor demand

Another dimension that may determine the sustainability of CA adoption is its effect on demand for agricultural labor. As mentioned previously, one of the potential effects of CA is that, because of the reduced labor requirements during land preparation, total agricultural labor demand may simply shift from the land preparation phase to later in the season, such as for weeding or applying pesticides. To assess the effect of CA on agricultural labor demand, we recorded household labor use over various agricultural activities during the course of the long rainy season, disaggregated by sex (male and female) and whether the labor was family labor or hired labor. The various activities for which we recorded labor use included land preparation, planting, weeding, applying pesticides, applying fertilizers, harvesting, and postharvest activities. Labor use

[16]Some studies suggest that CA systems may increase *expected yields* with the passage of time, so CA yields may potentially first-order stochastically dominate yields under a conventional system over time.

was reported in “person-days”, where a person-day was defined as an 8-hour work day for each individual involved in a particular activity.

As was the case when discussing the effects of CA adoption on yields, including fixed effects allows us to control for unobservable confounderes and identify the causal effects of practicing CA on farmers' labor demand. Here, we estimate a regression of the form

$$L_{ikt} = \alpha + \delta CA_{it} + \sum_{j=1}^{J} \beta_j x_{ijt} + \mu_i + \phi_t + \varepsilon_{it} \tag{6}$$

where terms are similar to equation (4), except the dependent variable, L_{ikt}, is the amount of labor (person-days) that farm household i applied on activity k for agricultural production in period t. We estimate a series of regressions (one for each of the three definitions of CA adoption) for each of a series of agricultural activities, including land preparation, planting, weeding, applying pesticides, applying fertilizer, harvesting, and postharvest activities. The results of estimating these regressions using least squares are reported in Table 7.[17] As would be expected, given the adoption of zero tillage as a component of CA, there is evidence of a reduction in labor demand during the land preparation phase as a result of practicing CA. In particular, those farmers that reported practicing CA reported using about 8 ($p = 0.02$) fewer person-days on land preparation than those farmers practicing conventional tillage. Given that the mean labor demand for land preparation among farms relying on conventional tillage is only about 45 person-days, this would suggest that adopting CA reduces labor by about 20 percent.

But does CA simply shift labor around the crop growth cycle, for example by increasing the demand for labor weeding or in applying pesticides (including, but not exclusively herbicides) as is suggested by Giller et al. (2009)? The point estimates in Table 7 suggest that practicing CA might actually lead to a small reduction in the demand for labor for weeding (an estimated reduction of roughly 3 person-days compared to farmers practicing conventional tillage; $p = 0.23$).

There is evidence of an additional increase in labor for applying fertilizers, but this increase is quite small in absolute magnitude. There is also some evidence that practicing CA might increase labor during postharvest activities, though this effect appears to be highly variable and imprecisely measured. We do not have data with which to specifically identify the postharvest activities that might have absorbed an increase in labor, but this increase could be due to the nature of the constituent practices. For example, since CA requires either intercropping or crop rotation, the apparent increase in postharvest labor could potentially pertain to activities related to the second (non-maize) crop. If farmers practice intercropping – and especially

[17] Labor use data were Winsorized at the 2.5th and 97.5th percentiles of the raw data.

if they practice mixed intercropping – there may be additional labor required for processing the second crop. If they practice temporal intercropping or relay cropping, the additional postharvest labor that is observed may be attributable to activities for the second crop temporally separated from the main maize crop. If the mechanism underlying the increase in postharvest labor is indeed attributable to activities related to the second crop, there may be economic benefits to this increase in postharvest labor that would not be reflected in the effects on maize yields reported above in Table 6. It should be noted, however, that intercropping is a fairly commonplace traditional practice in Malawi, and indeed nearly 79 percent of farmers in the sample reported practicing intercropping or crop rotation at the time of project baseline. It thus seems perhaps unreasonable to suspect that the increase in postharvest labor could be attributable to intercropping or crop rotation. Alternatively, the mechanism underlying this apparent increase in postharvest labor could be the retention and active mulching of maize residues, rather than burning them or leaving them in the field as livestock fodder, which could be accomplished rather passively. Since mulching crop residues is a much more novel practice than intercropping (only about 18 percent of farmers in the sample reported retaining and mulching crop residues at the time of project baseline), this may seem a more plausible explanation. But again, without a more detailed accounting of the specific activites on which farmers spent their time, it is not possible to make any authoritative claims as to why there might be an increase in postharvest labor under CA, and again we caution that the effect is not measured very precisely.

Are some of these observed labor effects simply deficient rainfall during the 2015-2016 season due to El Niño, as we suggest may have been the case with yields. In other words, if there had not been a severe drought as a result of El Niño during 2015-2016, might weed pressure have been more intense, and might there have been an observable difference in labor for weeding and applying pesticides among farmers practicing CA. In an attempt to answer this question, and about whether there are heterogeneous labor effects over time more broadly, we allow for CA-by-time interactions:

$$L_{ikt} = \alpha + \delta_1 CA_{it} + \sum_{j=1}^{J} \beta_j x_{ijt} + \delta_2 CA_{it} \times \phi_t + \mu_i + \phi_t + \varepsilon_{it} \tag{7}$$

These results are reported in Table 8. The results clearly suggest that the El Ninño effect in 2015-2016 had an impact on labor use, as would be expected given the large impacts on overall agricultural production. But even after accounting for the El Ninño shock, there is no evidence that farmers practicing CA would have higher labor demands for weeding or in applying pesticides. Further, the results suggest that some of the observed effects on postharvest labor discussed above may be simply one-time transitional costs. Although there is (statistically weak) evidence that farmers who practiced CA required more postharvest labor than

Table 7: Fixed effects estimation of the effect of CA on total labor use by activity

Dependent variable	Land preparation labor (person days)	Planting labor (person days)	Weeding labor (person days)	Pesticide application labor (person days)	Fertilizer application labor (person days)	Harvest labor (person days)	Postharvest labor (person days)
Practiced CA (=1)	−8.24	0.48	−2.95	−0.27	0.71	−0.61	7.09
	(3.43)	(0.81)	(2.44)	(0.20)	(0.59)	(1.69)	(5.67)
Number of observations	2456	2456	2456	2456	2456	2456	2456
R^2	0.69	0.41	0.64	0.34	0.24	0.55	0.25
Area under CA (ac)	−4.03	0.36	−0.45	−0.31	0.78	0.17	6.06
	(2.34)	(0.55)	(1.66)	(0.13)	(0.40)	(1.15)	(3.86)
Number of observations	2456	2456	2456	2456	2456	2456	2456
R^2	0.69	0.41	0.64	0.34	0.24	0.55	0.25
Area under CA (percent of total area)	−8.08	1.35	0.42	−0.19	1.90	1.99	8.50
	(4.29)	(1.01)	(3.05)	(0.25)	(0.74)	(2.11)	(7.09)
Number of observations	2456	2456	2456	2456	2456	2456	2456
R^2	0.69	0.41	0.64	0.34	0.25	0.55	0.25
Individual/farm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean for comparison group at endline	42.884	11.534	37.192	0.145	7.069	19.932	34.352

Source: The authors.
Note: In all regressions, additional controls include the age of the household head, family agricultural labor (male and female), hired agricultural labor (male and female), pesticide expenditures, the number of plots cultivated (and its square), total area cultivated (and its square), and maize yields (and its square).

farmers who practiced conventional agriculture, and some further (statistically weak) evidence that farmers who practiced CA in 2015-2016 had higher postharvest labor requirements than those who only practied CA in 2013-2014, there is no evidence that increasing the scale of CA has any effect on postharvest labor after the initial conversion. There is some (statistically weak) evidence that this is not the case for the labor savings associated with land preparation, as these labor savings appear to continue accruing even as the scale of CA increases.

Although practicing CA does not seem to increase the amount of labor used in applying pesticides, a relevant question regarding the pursuant farm economics is whether there might instead be an increase in expenditures on pesticides, perhaps especially for herbicides to deal with increased weed pressure. Unfortunately, the Shire River Basin in Malawi may not be the best test case with which to evaluate the effects of CA on pesticide expenditures, since the levels of pesticide use are so low. For example, at the time of the project baseline, only about 25 percent of farmers used herbicides, and of those that did use herbicides, the average expenditures were around USD 5. At the time of the project endline, the proportion of farmers that had used herbicides fell to only about 5 percent, while the average expenditures for those purchasing herbicides remained roughly unchanged at USD 5. [18] Consequently, there seems to be little to no evidence

[18] When we regress herbicide expenditures on adoption of CA controlling for both time and individual and/or farm

Table 8: Effect of practicing CA on agricultural labor use: Two-way fixed effects estimation with CA × time interactions

	Dependent variable:						
	Land preparation	Planting	Weeding	Pesticide	Fertilizer	Harvest	Postharvest
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Practiced CA (=1)	−4.605	−1.482	−2.838	0.154	−1.941	−1.572	4.108
	(5.361)	(1.265)	(3.818)	(0.310)	(0.917)	(2.644)	(8.869)
Post	−13.698	2.182	−6.535	−0.648	1.739	1.298	18.334
	(2.192)	(0.517)	(1.561)	(0.127)	(0.375)	(1.081)	(3.627)
Practiced CA (=1) × Post	−4.566	2.472	−0.137	−0.539	3.337	1.206	3.753
	(5.186)	(1.224)	(3.694)	(0.300)	(0.888)	(2.558)	(8.580)
Observations	2,456	2,456	2,456	2,456	2,456	2,456	2,456
R^2	0.693	0.411	0.643	0.342	0.252	0.548	0.250
Area under CA (ac)	−2.254	−1.498	−3.389	−0.012	−0.992	−0.383	6.044
	(3.545)	(0.833)	(2.519)	(0.204)	(0.605)	(1.746)	(5.853)
Post	−14.493	2.118	−7.664	−0.631	1.762	1.193	18.702
	(2.131)	(0.501)	(1.514)	(0.123)	(0.363)	(1.049)	(3.518)
Area under CA (ac) × Post	−2.153	2.252	3.557	−0.366	2.145	0.669	0.018
	(3.225)	(0.758)	(2.292)	(0.186)	(0.550)	(1.589)	(5.326)
Observations	2,456	2,456	2,456	2,456	2,456	2,456	2,456
R^2	0.692	0.414	0.643	0.345	0.254	0.548	0.251
Area under CA (percent of total area)	−6.767	−3.599	0.346	0.026	−2.524	−1.257	6.921
	(7.806)	(1.832)	(5.555)	(0.452)	(1.329)	(3.842)	(12.897)
Post	−14.592	1.922	−7.168	−0.728	1.626	0.736	18.805
	(2.164)	(0.508)	(1.540)	(0.125)	(0.368)	(1.065)	(3.575)
Area under CA (percent of total area) × Post	−1.507	5.700	0.083	−0.247	5.086	3.732	1.823
	(7.503)	(1.761)	(5.340)	(0.434)	(1.278)	(3.693)	(12.397)
Observations	2,456	2,456	2,456	2,456	2,456	2,456	2,456
R^2	0.692	0.416	0.642	0.340	0.257	0.548	0.250
Individual/farm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: The authors.
Note: In all regressions, additional controls include the age of the household head, family agricultural labor (male and female), hired agricultural labor (male and female), pesticide expenditures, the number of plots cultivated (and its square), total area cultivated (and its square), and maize yields (and its square).

that adoption of CA increased expenditures on pesticides to control increased weed pressure, though we clearly acknowledge that this result may not hold in contexts in which the use of herbicides to control weeds is more prevalent.

In sum, however, the fact that farmers that practiced CA were able to achieve equivalent yields on average compared with farmers practicing conventional tillage – and perhaps less variable yields during abnormal years – and at the same time were able to significantly reduce their labor requirements for land preparation without increasing either their labor requirements for pest control or expenditures on agro-chemicals speaks rather convincingly about the farm economics of adopting CA. Although there may be an increase in labor for postharvest activities, as we have mentioned, some of these activities *may* be attributable to the second

fixed effects, family and hired labor, number of plots cultivated and total area cultivated, the estimated effect is actually negative (-134, p-value = 0.35), suggesting that practicing CA might *reduce* expenditures on herbicides, but this effect is measured very imprecisely.

crop in an intercrop system, which may have economic benefits not reflected in the effects on maize yield. Futhermore, this overall discussion of farm economics and the sustainability of adopting CA does not even consider the socio-environmental impacts of reduced soil erosion and siltation, enhanced soil structure and soil fertility, and at least a maintaining of status quo levels of agro-chemical additions. These other *in situ* benefits would be expected to accrue over time – well beyond the timeframe of our project – but there is rather convincing biophysical evidence that they should arise under continuous adoption of these soil conserving practices.

6 Concluding remarks

In the present study we have presented some of first evidence on the effectiveness of a small-scale PES program in which financial incentives are used to encourage soil conservation, specifically encouraging the adoption of conservation agriculture (CA). Through reducing or eliminating tillage, retaining and mulching crop residues after harvest, and integrating legumes into either a cereal-legume rotation or through intercropping, agronomists and other agricultural scientists are hopeful that CA could prove to be an important instrument in the battle to conserve earth's diminishing and degrading soil resources. Although soil may not immediately come to mind as a important producer of ecosystem services, there are certainly a number of both *in situ* and *ex situ* ecosystem services that soils produce, and the worldwide prevalence of conventional agricultural practices and resultant high rates of soil erosion pose a serious threat to the sustained production and enjoyment of these services.

Our randomized evaluation in the Shire River Basin in southern Malawi introduced financial incentives to farmers, much like other payments for ecosystem services (PES) programs have done for other conservation purposes around the world. In our experiment, we included two different payment modalities that allowed us to evaluate the relative effectiveness of these two payment regimes in encouraging adoption of CA. The first payment modality, a conventional subsidy, provided a fixed payment to farmers who complied with the program requirements and adopted all three of the constituent practices of CA. The second payment modality, an agglomeration payment, provided a slightly smaller fixed payment to farmers who complied with the program requirements, but also provided a bonus to the farmer for each of his or her contiguous neighbors who also complied with the program requirements and practiced CA. This study provides one of the first rigorous evaluations of the agglomeration payment structure in a real-world setting.

In all, the results suggest that providing financial incentives to farmers to encourage the adoption of CA is an effective solution, with adoption rates in villages that were randomly selected for participation in the program increasing by nearly 10 percentage points compared to comparison villages. Not only that, both the extent and intensity of CA adoption increased as well. In evaluating the "additionality" of CA adoption, we find that, by and large, the provision of financial incentives was successful in bringing into the fold farmers that might not have otherwise practiced CA. In particular, only about 13 percent of registered farmers had practiced CA prior to being invited to participate in our PES program, suggesting that as many as 87 percent of registrants were potentially additional adopters of CA. Although the overall level adoption (both in terms of total area and fraction of total cultivated area) remained low following the introduction of the financial incentives, the proportionate effect of the program was considerable, with the area under CA increasing by more than 40 percent compared to the comparison group. The conventional subsidy appears to be more effective at encouraging individual farmers to adopt CA, but the agglomeration payment also proved to be effective, and there is additional evidence that the agglomeration payment was successful in encouraging higher rates of CA adoption among farmers' peers and other members of the community. Further, the agglomeration payment seems to have stimulated increased communication between farmers and their neighbors about agriculture, and there seems to be increased concern for soil conservation, as the adoption of other soil conservation measures, such as planting pits and stone bunds, also increased. Further, the structure of the agglomeration payment seems to encourage increased peer policing, which may encourage increased compliance with PES program requirements even in the absence of formal monitoring.

We also demonstrate that – contrary to the conventional wisdom – there need not be adverse private economic outcomes from the transition from conventional tillage to CA. Most previous research has suggested that such yield gains may not be achievable for 5-10 years, but our results suggest that average CA yields may be on par with those obtainable under a conventional agricutlure system, but may exhibit lower year-over-year yield variability. Although farmers in our sample who practiced CA may have obtained lower yields than farmers practicing conventional agriculture during the 2013-2014 rainy season, farmers who practiced CA during the drought-ridden 2015-2016 rainy season were able to achieve higher yields than farmers who continued to practice conventional agriculture. Further, there are other positive economic outcomes that emerge as well, namely a substantial reduction in labor requirements for land preparation, without necessarily substantial increases in labor requirements for other agricultural activities such as weeding or increases in agro-chemical expenditures.

The policy implications of this research are multifaceted. First and foremost, our results provide convincing evidence that providing farmers with financial incentives to undertake soil conservation practices is an effective means of increasing the adoption of socially- and environmentally-desirable land management practices. To the extent that the social benefits that emerge from the sustained production of*in situ* and*ex* *situ* ecosystem services exceed the total costs of encouraging these practices on a landscape scale, this would suggest an economically efficient means of correcting an important market failure. As Bell et al. (2018) have demonstrated, even just considering the*ex situ* ecosystem services associated with hydropower generation, the costs associated with a PES program like the one evaluated here would be orders of magnitude less expensive than alternative means for dealing with soil erosion and siltation, suggesting a highly cost-effective solution. Second, our results suggest that the agglomeration payment mechanism may be an effective means for encouraging the consolidation of fragmented land under a unified conservation objective. Though the rates of CA adoption among surveyed members of our sample are indistinguishable (or at worst slightly less) under the agglomeration payment vis-á-vis the conventional voucher, there is evidence that the agglomeration payment may generate positive externalities in increasing the rates of adoption among other members of the community that do not appear in our sample. Third, the neutral to modestly positive economic outcomes of adopting CA among farmers in our sample suggest that perhaps subsidies may not be needed to incentivize CA into perpetuity. Indeed, given the less variable yields under CA and greater yield stability under adverse conditions, one could reasonably argue that payments may not be needed beyond the initial transition. We are cautious to make such bold assertions, given the substantial rainfall deficiencies that were observed at the time of project endline, and the potential for these deficiencies to have disproportionately benefitted farmers practicing CA. Yet this also highlights the potential for CA to provide greater resilience in the face of increasing climate variability. We also note that the economics of CA are bolstered by the substantial reduction in labor required for land preparation, without concomitant increases in labor requirements for weeding or applying pesticides. This too, contributes to the positive economic outlook for private farmers adopting CA, which again may limit the need for external assistance in encouraging the sustained adoption of CA.

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Appendix A Additional robustness checks

A.1 Treatment effects under variations of controls

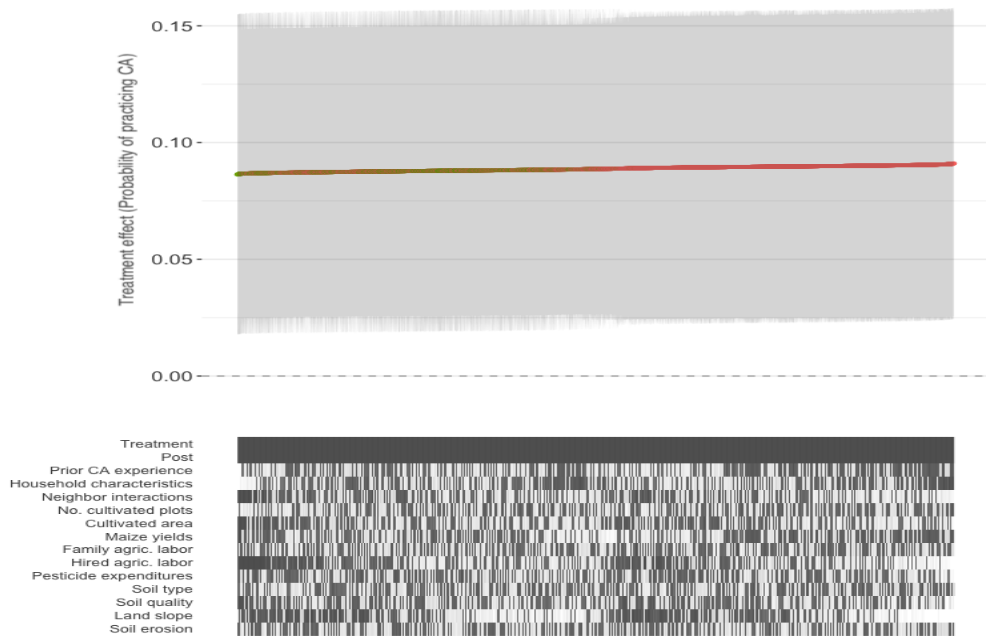

Figure A1: Regression coefficients for PES treatment effects on binary adoption of CA under different combinations of control variables

Notes: Dots represent coefficient point estimates. Green dots indicate probability of Type I error less than or equal to 10 percent; red dots indicate probability of Type I error less than or equal to 5 percent. Error bars represent 95 percent confidence intervals, based on standard errors adjusted for clustering at the village level. All regressions include binary indicators for whether the household was in a village randomly assigned to the treatment group and a binary indicator for whether the observation is post-intervention. Some of the elements reported in the bottom panel are individual control variables, while others are groups of control variables. “Prior CA expeiences” include controls for area under the three constituent practices of CA; “Household characteristics” includes the age and gender of the household head, as well as household size; “Neighbor interactions” includes binary indicators whether the respondent received assistance from their neighbor(s) in the last agricultural season and whether the respondent received advice from their neighbor(s) in the last agricultural season; “Family agric. labor” includes person-days for both male and female members of the household; “Hired agric. labor” includes person-days for both male and female hired laborers; “Soil type” includes binary indicators for whether the household’s soil was sandy, sandy-clay, clay, or other (excluded); “Soil quality” includes binary indicators for whether the soil quality was good, fair, or poor (excluded), “Land slope” includes binary indicators for whether the land slope was flat (no slope), slight, moderate, steep, or hilly (excluded); and “Soil erosion” includes binary indicators for whether there is no problem of soil erosion, or whether there is low, moderate, or high erosion excluded).

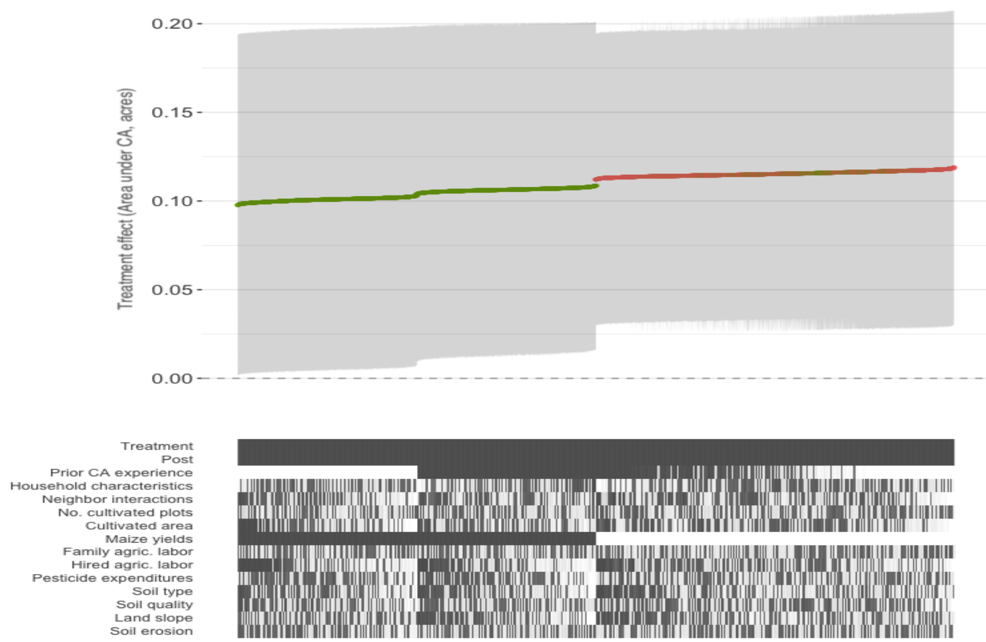

Figure A2: Regression coefficients for PES treatment effects on total cultivated area under CA under different combinations of control variables

Notes: Dots represent coefficient point estimates. Green dots indicate probability of Type I error less than or equal to 10 percent; red dots indicate probability of Type I error less than or equal to 5 percent. Error bars represent 95 percent confidence intervals, based on standard errors adjusted for clustering at the village level. All regressions include binary indicators for whether the household was in a village randomly assigned to the treatment group and a binary indicator for whether the observation is post-intervention. Some of the elements reported in the bottom panel are individual control variables, while others are groups of control variables. "Prior CA expeiences" include controls for area under the three constituent practices of CA; "Household characteristics" includes the age and gender of the household head, as well as household size; "Neighbor interactions" includes binary indicators whether the respondent received assistance from their neighbor(s) in the last agricultural season and whether the respondent received advice from their neighbor(s) in the last agricultural season; "Family agric. labor" includes person-days for both male and female members of the household; "Hired agric. labor" includes person-days for both male and female hired laborers; "Soil type" includes binary indicators for whether the household's soil was sandy, sandy-clay, clay, or other (excluded); "Soil quality" includes binary indicators for whether the soil quality was good, fair, or poor (excluded), "Land slope" includes binary indicators for whether the land slope was flat (no slope), slight, moderate, steep, or hilly (excluded); and "Soil erosion" includes binary indicators for whether there is no problem of soil erosion, or whether there is low, moderate, or high erosion excluded).

A.2 Placebo tests

Maintaining the same proportion of "treatment" and comparison observations, we randomly assign a placebo treatment to a subsample of observations and re-estimate equation (1) with the placebo treatment in place of the actual PES program treatment. We conduct these placebo analyses a large number of times (10,000). Figure A4 plots the placebo effect coefficients (with 95 percent confidence intervals) for these regressions. If the treatment effect reported in Table 3 was due to anything other than random allocation to participation

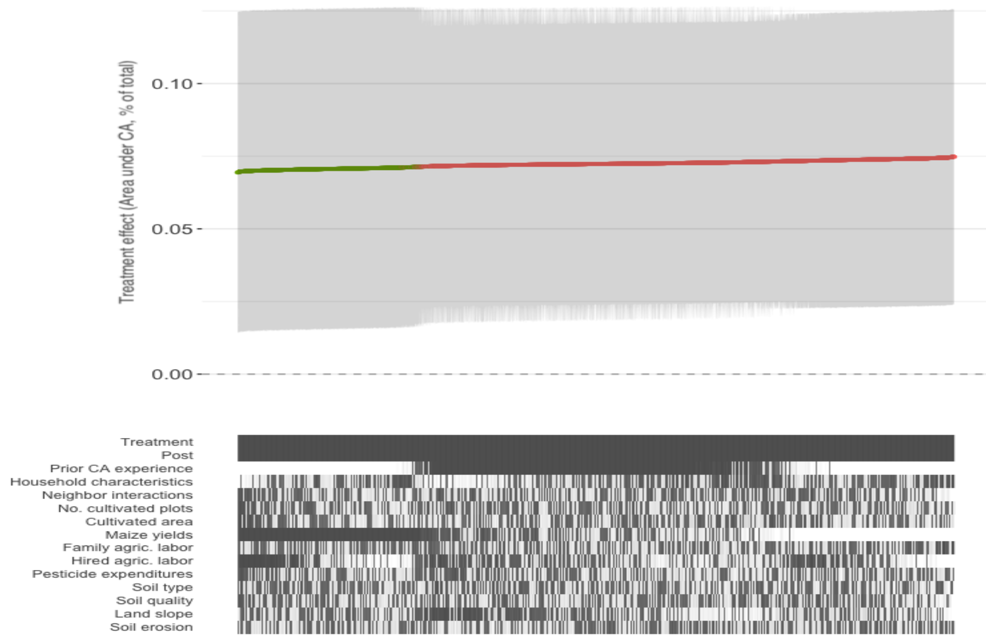

Figure A3: Regression coefficients for PES treatment effects on percentage of cultivated area under CA under different combinations of control variables

Notes: Dots represent coefficient point estimates. Green dots indicate probability of Type I error less than or equal to 10 percent; red dots indicate probability of Type I error less than or equal to 5 percent. Error bars represent 95 percent confidence intervals, based on standard errors adjusted for clustering at the village level. All regressions include binary indicators for whether the household was in a village randomly assigned to the treatment group and a binary indicator for whether the observation is post-intervention. Some of the elements reported in the bottom panel are individual control variables, while others are groups of control variables. "Prior CA expeiences" include controls for area under the three constituent practices of CA; "Household characteristics" includes the age and gender of the household head, as well as household size; "Neighbor interactions" includes binary indicators whether the respondent received assistance from their neighbor(s) in the last agricultural season and whether the respondent received advice from their neighbor(s) in the last agricultural season; "Family agric. labor" includes person-days for both male and female members of the household; "Hired agric. labor" includes person-days for both male and female hired laborers; "Soil type" includes binary indicators for whether the household's soil was sandy, sandy-clay, clay, or other (excluded); "Soil quality" includes binary indicators for whether the soil quality was good, fair, or poor (excluded), "Land slope" includes binary indicators for whether the land slope was flat (no slope), slight, moderate, steep, or hilly (excluded); and "Soil erosion" includes binary indicators for whether there is no problem of soil erosion, or whether there is low, moderate, or high erosion excluded).

in the PES program, we would expect to see a nontrivial share of these placebo effects to be nonzero. But that is clearly not evident in Figure A4. The "cloud" of placebo effects is not only much more disperse than the "cloud" of treatment effects from the random sub-samples in Figure A5, but this "cloud" is also clearly centered around zero, indicating a near-universal null placebo effect. Indeed, in only about 8 percent of the simulated cases is p-value on the the placebo effect less than or equal to 0.05.

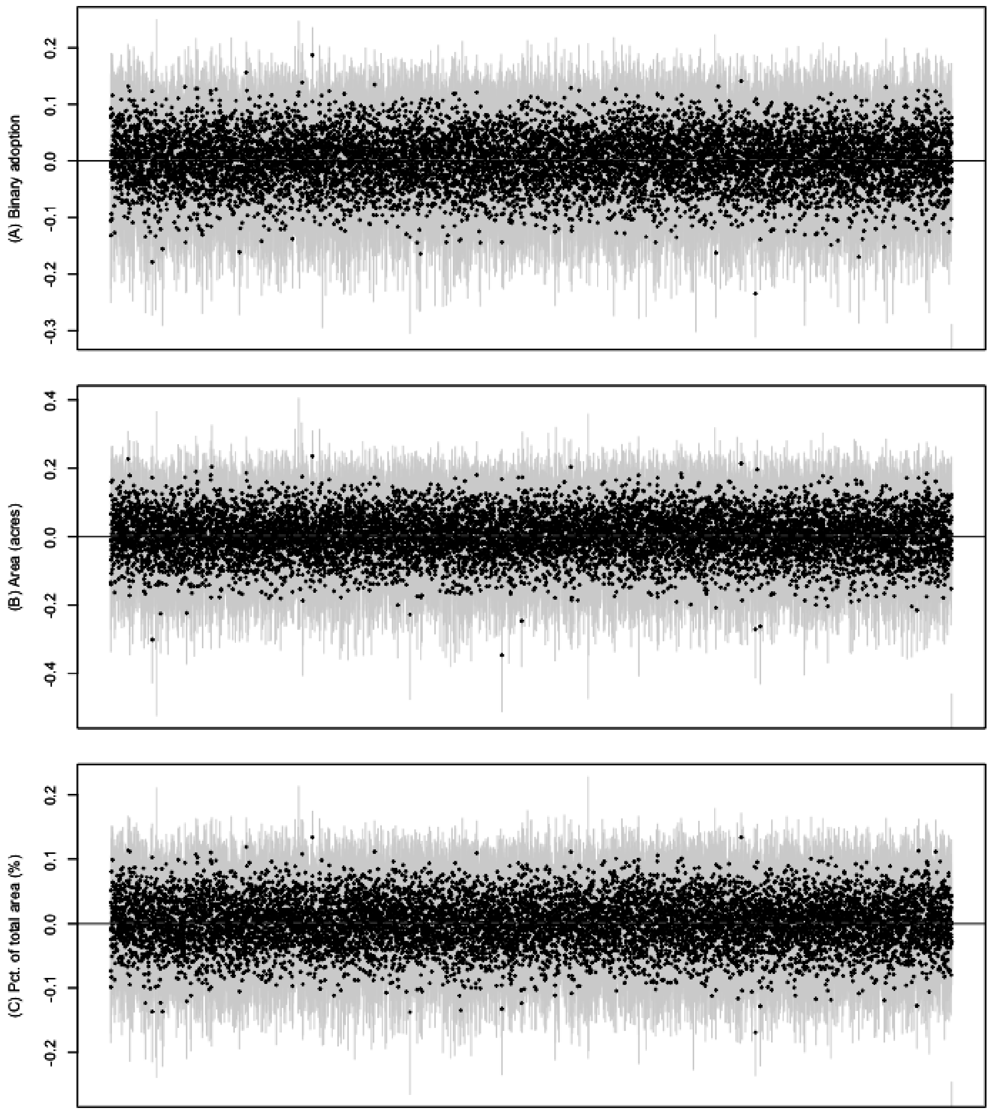

Figure A4: Placebo effects on adoption of CA across 10,000 random allocations

Notes: Black dots represent point estimates of placebo effect for each of 10,000 simulated samples. In each simulation, assignment of the placebo is random, with the probability of being assigned the placebo equivalent to the proportion of treated observations in the original sample. Light grey error bars represent 95 percent confidence intervals, based on standard errors adjusted for clustering at the village level. Dashed lines indicate average placebo effect estimates across all simulations. All regressions include additional controls for area under the three constituent practices of CA, number of plots cultivated, maize yields, and soil type at baseline.

A.3 Treatment effects under alternative subsamples

In conducting the sensitivity analysis, we randomly select 10 percent of observations, exclude them from the sample, and re-estimate the effect of the PES program. We repeat this process a large number of times (10,000) to estimate the average program effect over alternative "realities" and assess how sensitive the program effect is to the underlying sample being analyzed. Figure A5 plots the treatment effect coefficients (with 95 percent confidence intervals) for these regressions. As can be clearly seen in this figure, the estimated treatment effects are highly robust to alternative samples, and under virtually every simulated sample, we can be 95 percent confident that the actual treatment effect is greater than zero. The effect of the PES program on the area of CA adoption (panel B in Figure A5) is the noisiest of the three regressions, yet even then, in only 14 of 10,000 simulated cases (0.14 percent of cases) did the 95 percent confidence interval span zero. When we consider the effect of treatment on the proportion of farmers in treatment villages that adopted CA, the average (ITT) treatment effect across the 10,000 simulations is roughly 0.09, with point estimates ranging from 0.07 to 0.11. This is virtually identical to the estimated effect from our full sample, confirming that the effect of the program on adoption of CA is very insensitive to the specific sample under which the effects are being analyzed.

We also restrict the sample to those individuals who had *not* practiced the full set of CA practices at the time of the baseline survey. If there was a reason why CA adoption was systematic higher among the randomly selected treatment group prior to the initiation of the program, and if there was selection bias that led to these non-adopting farmers choosing not to adopt CA, we would expect that the effect of the PES program might be attenuated among these non-adopting farmers. That is not what we observe, however. In columns (1)-(3) of A1, we report the treatment effect coefficients among this restricted sample. Although the differences between the estimated treatment effects in this restricted sample and the larger unrestricted sample (in Table 3) are slight, we actually observe that the estimated treatment effects are larger (or at least no less) among this restricted sample. Consequently, rather than the evidence suggesting that the pre-intervention rates of CA adoption might contribute to upwardly biased estimates of program effects, the evidence actually suggests the opposite: that perhaps the farmers' adoption at baseline might already be locked in, and the program effects might be larger among those farmers that had not or perhaps would not have adopted CA in the absence of the intervention.

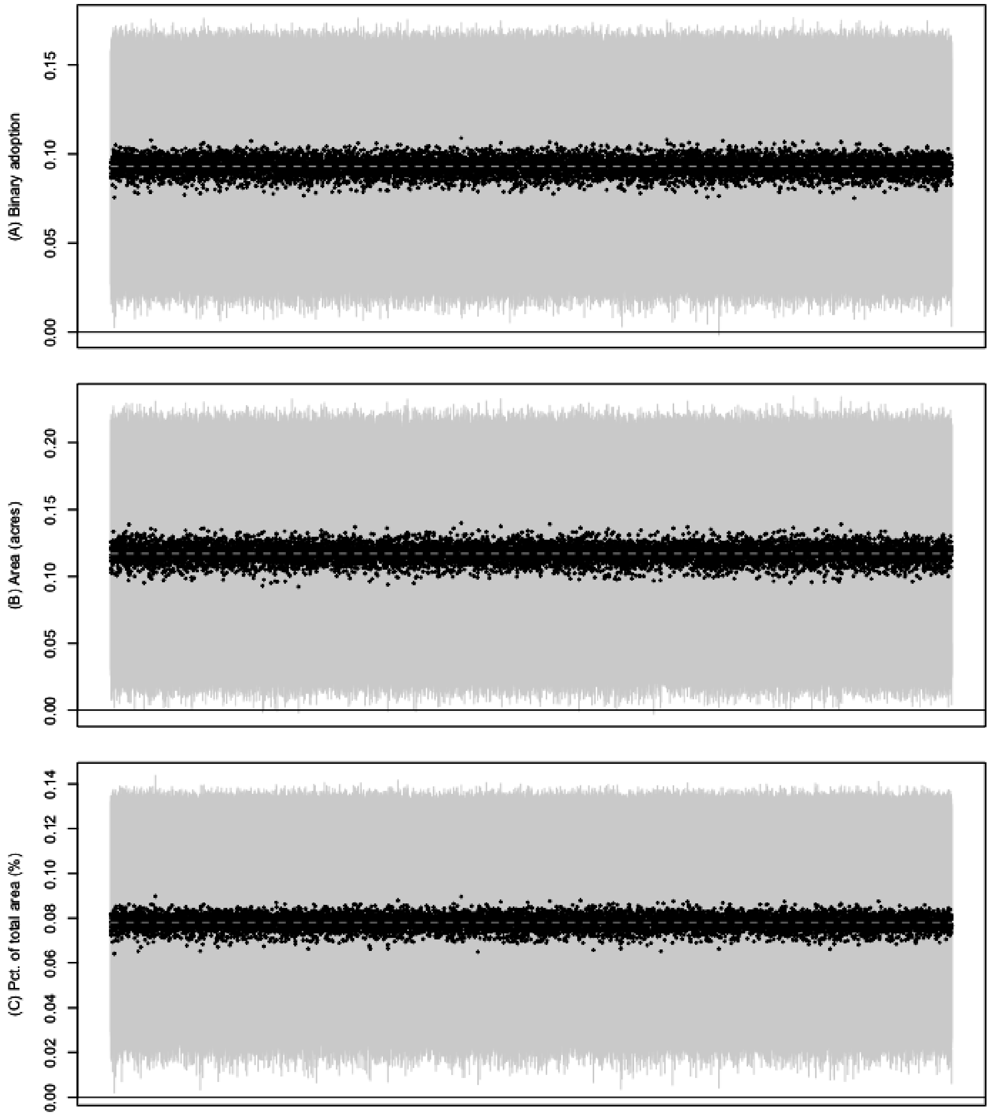

Figure A5: PES treatment effects on adoption of CA across 10,000 simulated sub-samples

Notes: Black dots represent point estimates of treatment effect for each of 10,000 simulated samples, each consisting of 90 percent of observations from original sample, with the omitted 10 percent randomly selected (without replacement) in each simulation. Light grey error bars represent 95 percent confidence intervals, based on standard errors adjusted for clustering at the village level. Dashed lines indicate average treatment effect estimates across all simulations. All regressions include additional controls for area under the three constituent practices of CA, number of plots cultivated, maize yields, and soil type at baseline.

Table A1: Treatment effects estimation: difference-in-difference regressions with restricted sample of non-adopters and fixed effects regressions controlling for both individual/farm and time fixed effects

Dependent variable	Practiced CA (0/1)	Area (acres)	Area (pct. of total)	Practiced CA (0/1)	Area (acres)	Area (pct. of total)
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	−0.004	−0.009	0.005			
	(0.003)	(0.006)	(0.002)			
Post	0.129	0.180	0.100	0.110	0.148	0.080
	(0.031)	(0.042)	(0.027)	(0.033)	(0.048)	(0.026)
Treatment × Post	0.110	0.113	0.076	0.081	0.106	0.067
	(0.039)	(0.052)	(0.032)	(0.035)	(0.052)	(0.028)
Baseline imbalance controls	Yes	Yes	Yes			
Additional controls	No	No	No	Yes	Yes	Yes
Individual/farm fixed effects	No	No	No	Yes	Yes	Yes
Time period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2263	2263	2263	2456	2456	2456
R^2	0.146	0.120	0.126	0.166	0.127	0.177
Mean for comparison group at endline	0.173	0.234	0.125	0.173	0.234	0.125

Source: The authors.
Note: In columns (1)-(3), standard errors adjusted for clustering at the village level in parentheses. Baseline imbalance controls (where indicated) include area under the three constituent practices of CA, number of plots cultivated, maize yields, and soil type at baseline. Additional controls (where indicated) include the age of the household head, number of plots cultivated and total area cultivated, and maize yields.

Appendix B Effects of alternative incentive mechanisms and monitoring intensities on constituent CA practices

Table B1: Treatment effects estimation: effect of alternative incentive mechanisms and monitoring intensities on adoption of CA

Dependent variable:	Practiced CA		Area under CA (% of total)	
	(1)	(2)	(3)	(4)
Conventional voucher	0.009	−0.006	0.006	−0.003
	(0.015)	(0.018)	(0.010)	(0.012)
Agglomeration payment	0.021	0.019	0.004	0.004
	(0.019)	(0.027)	(0.011)	(0.012)
Post	0.109	0.142	0.087	0.087
	(0.027)	(0.027)	(0.021)	(0.021)
Conventional voucher × Post	0.135	0.201	0.106	0.167
	(0.043)	(0.061)	(0.034)	(0.051)
Agglomeration payment × Post	0.052	0.034	0.043	0.043
	(0.035)	(0.045)	(0.029)	(0.037)
Conventional voucher × Partial monitoring		0.049		0.026
		(0.017)		(0.011)
Conventional voucher × Full monitoring		0.017		0.005
		(0.019)		(0.014)
Agglomeration payment × Partial monitoring		0.002		0.010
		(0.037)		(0.017)
Agglomeration payment × Full monitoring		0.004		−0.007
		(0.028)		(0.014)
Conventional voucher × Partial monitoring × Post		−0.082		−0.078
		(0.088)		(0.066)
Conventional voucher × Full monitoring × Post		−0.125		−0.114
		(0.069)		(0.059)
Agglomeration payment × Partial monitoring × Post		−0.007		−0.025
		(0.054)		(0.052)
Agglomeration payment × Full monitoring × Post		0.063		0.021
		(0.051)		(0.041)
Additional controls	Yes	Yes	Yes	Yes
Observations	2507	2507	2507	2507
R^2	0.279	0.284	0.275	0.280
Mean for comparison group at endline	0.173		0.125	

Source: The authors.
Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline levels of the dependent variable. Additional controls (where indicated) include area under the three constituent practices of CA, number of plots cultivated, maize yields, and soil type at baseline.

Appendix C Effects of PES program on practicing constituent CA practices

Table C1: Treatment effects estimation: effect of random allocation to treatment on adoption of constituent CA practices

	(A) Zero-tillage					
Dependent variable:	Binary adoption		Area (acres)		Area (pct. of total)	
Treatment effect	0.058	0.053	0.055	0.041	0.049	0.042
	(0.042)	(0.039)	(0.069)	(0.061)	(0.042)	(0.039)
Additional controls	No	Yes	No	Yes	No	Yes
Observations	2751	2507	2751	2507	2751	2507
R^2	0.072	0.404	0.047	0.588	0.085	0.411

	(B) Retention and mulching of crop residues					
	Binary adoption		Area (acres)		Area (pct. of total)	
Treatment effect	0.089	0.080	0.072	0.054	0.045	0.036
	(0.041)	(0.040)	(0.073)	(0.068)	(0.033)	(0.032)
Additional controls	No	Yes	No	Yes	No	Yes
Observations	2751	2507	2751	2507	2751	2507
R^2	0.128	0.401	0.080	0.552	0.127	0.408

	(C) Intercropping or crop rotations					
	Binary adoption		Area (acres)		Area (pct. of total)	
Treatment effect	−0.024	−0.024	−0.118	−0.119	−0.052	−0.058
	(0.039)	(0.039)	(0.084)	(0.081)	(0.038)	(0.040)
Additional controls	No	Yes	No	Yes	No	Yes
Observations	2751	2507	2751	2507	2751	2507
R^2	0.010	0.239	0.003	0.806	0.0620	0.358

Source: The authors.
Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline levels of the dependent variable. Additional controls (where indicated) include area under the three constituent practices of CA, number of plots cultivated, maize yields, and soil type at baseline.

Table C2: Treatment effects estimation: effect of alternative incentive mechanisms and monitoring intensities on adoption of zero-tillage

Dependent variable:	Area (pct. of total)			
Conventional voucher	0.029	0.011	0.024	0.008
	(0.025)	(0.012)	(0.031)	(0.014)
Agglomeration payment	0.067	0.020	0.054	0.023
	(0.029)	(0.012)	(0.031)	(0.012)
Post	0.155	0.157	0.155	0.157
	(0.037)	(0.035)	(0.037)	(0.035)
Conventional voucher × Post	0.081	0.073	0.157	0.152
	(0.049)	(0.046)	(0.068)	(0.062)
Agglomeration payment × Post	0.019	0.014	0.028	0.026
	(0.042)	(0.040)	(0.050)	(0.048)
Conventional voucher × Partial monitoring			0.001	0.003
			(0.034)	(0.017)
Conventional voucher × Full monitoring			0.013	0.006
			(0.038)	(0.017)
Agglomeration payment × Partial monitoring			0.061	0.002
			(0.063)	(0.014)
Agglomeration payment × Full monitoring			−0.013	−0.011
			(0.032)	(0.010)
Conventional voucher × Partial monitoring × Post			−0.092	−0.098
			(0.078)	(0.073)
Conventional voucher × Full monitoring × Post			−0.146	−0.148
			(0.070)	(0.061)
Agglomeration payment × Partial monitoring × Post			−0.052	−0.061
			(0.056)	(0.052)
Agglomeration payment × Full monitoring × Post			0.018	0.017
			(0.043)	(0.044)
Additional controls	No	Yes	No	Yes
Observations	2751	2507	2751	2507
R^2	0.087	0.413	0.093	0.419

Source: The authors.
Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline levels of the dependent variable. Additional controls (where indicated) include area under the three constituent practices of CA, number of plots cultivated, maize yields, and soil type at baseline.

Table C3: Treatment effects estimation: effect of alternative incentive mechanisms and monitoring intensities on adoption of residue mulching

Dependent variable:	Area (pct. of total)			
Conventional voucher	0.003	0.012	0.033	0.024
	(0.024)	(0.013)	(0.032)	(0.018)
Agglomeration payment	0.038	0.017	0.010	−0.004
	(0.025)	(0.014)	(0.027)	(0.013)
Post	0.234	0.240	0.234	0.240
	(0.027)	(0.026)	(0.027)	(0.026)
Conventional voucher × Post	0.084	0.071	0.111	0.106
	(0.040)	(0.038)	(0.051)	(0.050)
Agglomeration payment × Post	0.010	0.005	0.015	0.015
	(0.036)	(0.036)	(0.048)	(0.050)
Conventional voucher × Partial monitoring			−0.048	−0.020
			(0.033)	(0.019)
Conventional voucher × Full monitoring			−0.048	−0.020
			(0.034)	(0.018)
Agglomeration payment × Partial monitoring			0.056	0.039
			(0.041)	(0.016)
Agglomeration payment × Full monitoring			0.038	0.031
			(0.033)	(0.019)
Conventional voucher × Partial monitoring × Post			−0.025	−0.040
			(0.076)	(0.075)
Conventional voucher × Full monitoring × Post			−0.059	−0.069
			(0.064)	(0.060)
Agglomeration payment × Partial monitoring × Post			−0.016	−0.021
			(0.050)	(0.054)
Agglomeration payment × Full monitoring × Post			−0.004	−0.015
			(0.061)	(0.061)
Additional controls	No	Yes	No	Yes
Observations	2751	2507	2751	2507
R^2	0.129	0.410	0.134	0.412

Source: The authors.
Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline levels of the dependent variable. Additional controls (where indicated) include area under the three constituent practices of CA, number of plots cultivated, maize yields, and soil type at baseline.

Table C4: Treatment effects estimation: effect of alternative incentive mechanisms and monitoring intensities on adoption of intercropping or crop rotation

Dependent variable:	Area (pct. of total)			
Conventional voucher	0.054	0.058	0.035	0.059
	(0.044)	(0.028)	(0.053)	(0.031)
Agglomeration payment	0.066	0.048	0.042	0.044
	(0.046)	(0.030)	(0.054)	(0.034)
Post	0.234	0.233	0.234	0.233
	(0.035)	(0.037)	(0.035)	(0.037)
Conventional voucher × Post	−0.033	−0.045	−0.021	−0.039
	(0.041)	(0.041)	(0.043)	(0.045)
Agglomeration payment × Post	−0.069	−0.068	−0.068	−0.073
	(0.041)	(0.042)	(0.046)	(0.049)
Conventional voucher × Partial monitoring			0.044	0.019
			(0.062)	(0.034)
Conventional voucher × Full monitoring			0.020	−0.013
			(0.048)	(0.024)
Agglomeration payment × Partial monitoring			0.064	0.022
			(0.063)	(0.039)
Agglomeration payment × Full monitoring			0.019	−0.007
			(0.060)	(0.033)
Conventional voucher × Partial monitoring × Post			−0.037	−0.017
			(0.057)	(0.050)
Conventional voucher × Full monitoring × Post			−0.007	−0.004
			(0.039)	(0.035)
Agglomeration payment × Partial monitoring × Post			0.011	0.014
			(0.053)	(0.052)
Agglomeration payment × Full monitoring × Post			−0.017	0.001
			(0.048)	(0.040)
Additional controls	No	Yes	No	Yes
Observations	2751	2507	2751	2507
R^2	0.062	0.359	0.065	0.359

Source: The authors.

Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline levels of the dependent variable. Additional controls (where indicated) include area under the three constituent practices of CA, number of plots cultivated, maize yields, and soil type at baseline.

Appendix D Effects of PES program on interactions with neighbors and neighbors' adoption of CA and other soil conservation practices

Table D1: Cross-sectional treatment effect estimation results: effect of alternative incentive mechanisms on social network interactions

Dependent variable:	Number of neighbors	Number of neighbors discussing agriculture	Number of neighbors discussing CA	Number of neighbors talked to last week	Number of neighbors stored in cell phone
Conventional voucher	−0.047	−0.070	−0.015	−0.347	−0.050
	(0.208)	(0.186)	(0.183)	(0.165)	(0.077)
Agglomeration payment	0.104	0.042	−0.025	−0.190	−0.114
	(0.217)	(0.175)	(0.180)	(0.165)	(0.072)
Observations	1158	1158	1158	1158	1158
R^2	0.102	0.071	0.078	0.059	0.045

Source: The authors.
Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions include controls for area under the three constituent practices of CA, number of plots cultivated, maize yields, and soil type at baseline.

Table D2: Treatment effects estimation: effect of alternative incentive mechanisms on soil conserving agricultural practices

Dependent variable:	Number of neighbors practicing reduced tillage	Number of community members mulching crop residues	Number of community members planting in bunds	Number of community members using planting pits	Number of community members planting in terraces
Conventional voucher	−0.139	−3.593	−2.216	−0.978	0.115
	(0.090)	(3.388)	(1.458)	(0.442)	(0.723)
Agglomeration payment	−0.049	−9.138	−2.098	−0.496	−0.499
	(0.104)	(3.165)	(1.531)	(0.479)	(0.603)
Post	0.418	2.774	13.786	12.066	0.437
	(0.090)	(3.907)	(5.544)	(2.638)	(0.782)
Conventional voucher × Post	0.258	−0.319	−2.601	−5.077	−0.200
	(0.144)	(4.864)	(5.934)	(3.269)	(0.968)
Agglomeration payment × Post	0.333	12.934	3.129	1.245	1.740
	(0.165)	(5.386)	(6.969)	(3.636)	(1.179)
Observations	2507	2507	2507	2507	2507
R^2	0.116	0.027	0.049	0.084	0.010

Source: The authors.
Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline levels of the dependent variable, the area under the three constituent practices of CA, the enumber of plots cultivated, maize yields, and soil types.

Appendix E Effects of adopting CA on maize yields: Full regression results including controls

Table E1: Least squares (columns 1-3) and two-way fixed effects (columns 4-9) estimation of the effects of practicing CA on ln(maize yields): Full regression results

Dependent variable:	ln(Maize yields (kg/ac))								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Intercept	6.35	6.35	6.34						
	(0.52)	(0.52)	(0.51)						
Practiced CA (=1)	0.12			−0.05			−0.18		
	(0.06)			(0.08)			(0.12)		
Practiced CA (=1) × Post							0.16		
							(0.12)		
Area under CA (ac)		0.02			−0.04			−0.08	
		(0.04)			(0.05)			(0.08)	
Area under CA (ac) × Post								0.05	
								(0.07)	
Area under CA (percent of total area)			0.13			−0.04			−0.25
			(0.08)			(0.10)			(0.18)
Area under CA (percent of total area) × Post									0.24
									(0.17)
Household head age	−0.00	−0.00	−0.00	−0.00	−0.00	−0.00	−0.00	−0.01	−0.00
	(0.001)	(0.001)	(0.001)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Male family labor (,000 person-days)	1.36	1.36	1.35	0.80	0.80	0.81	0.80	0.80	0.81
	(0.234)	(0.234)	(0.235)	(0.39)	(0.39)	(0.39)	(0.39)	(0.39)	(0.39)
Female family labor (,000 person-days)	−0.31	−0.30	−0.31	0.20	0.21	0.20	0.19	0.21	0.19
	(0.176)	(0.177)	(0.176)	(0.32)	(0.32)	(0.32)	(0.32)	(0.32)	(0.32)
Male hired labor (,000 person-days)	0.71	0.70	0.71	0.30	0.30	0.30	0.31	0.30	0.30
	(0.536)	(0.534)	(0.535)	(0.26)	(0.26)	(0.26)	(0.26)	(0.26)	(0.26)
Female hired labor (,000 person-days)	0.07	0.07	0.07	0.02	0.03	0.02	0.03	0.03	0.03
	(0.007)	(0.007)	(0.007)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pesticide expenditures	0.02	0.02	0.02	−0.00	−0.00	−0.00	−0.00	−0.00	−0.00
	(0.007)	(0.007)	(0.007)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Cultivated area	−0.50	−0.50	−0.50	−0.62	−0.62	−0.62	−0.62	−0.62	−0.63
	(0.046)	(0.045)	(0.046)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Cultivated area^2	0.04	0.04	0.04	0.05	0.05	0.05	0.05	0.05	0.05
	(0.006)	(0.006)	(0.006)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Number of plots cultivated	0.50	0.50	0.51	0.36	0.36	0.36	0.36	0.36	0.36
	(0.058)	(0.058)	(0.058)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Number of plots cultivated^2	−0.04	−0.04	−0.04	−0.02	−0.02	−0.02	−0.02	−0.02	−0.02
	(0.011)	(0.011)	(0.011)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Sandy soils (percent)	−0.17	−0.18	−0.18						
	(0.140)	(0.143)	(0.141)						
Sandy-clay soils (percent)	−0.05	−0.05	−0.05						
	(0.134)	(0.137)	(0.134)						
Clay soils (percent)	−0.04	−0.04	−0.05						
	(0.146)	(0.148)	(0.146)						
Good quality soils (percent)	0.11	0.12	0.12						
	(0.059)	(0.059)	(0.059)						
Fair quality soils (percent)	0.05	0.05	0.05						
	(0.057)	(0.057)	(0.057)						
Flat slope (percent)	−0.65	−0.64	−0.65						
	(0.476)	(0.474)	(0.474)						
Slight slope (percent)	−0.67	−0.66	−0.67						
	(0.470)	(0.468)	(0.468)						
Moderate slope (percent)	−0.59	−0.58	−0.59						
	(0.475)	(0.473)	(0.473)						
Steep slope (percent)	−0.39	−0.39	−0.39						
	(0.501)	(0.501)	(0.499)						
No soil erosion (percent)	0.34	0.34	0.34						
	(0.097)	(0.098)	(0.097)						
Moderate soil erosion (percent)	0.21	0.21	0.21						
	(0.105)	(0.106)	(0.105)						
Low soil erosion (percent)	0.29	0.29	0.29						
	(0.096)	(0.097)	(0.096)						
Individual/farm fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2456	2456	2456	2456	2456	2456	2456	2456	2456
R^2	0.26	0.25	0.26	0.35	0.35	0.35	0.35	0.35	0.35

Table E2: Effect of practicing CA on ln(maize yields): Two-way fixed effects estimation

Dependent variable:	ln(Maize yields)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Practiced CA (=1)	−0.180	−0.311	−0.229						
	(0.121)	(0.167)	(0.153)						
Area under CA (ac)				−0.083	−0.162	−0.112			
				(0.080)	(0.112)	(0.096)			
Area under CA (percent of total area)							−0.246	−0.355	−0.276
							(0.176)	(0.237)	(0.212)
Practiced CA (=1) × Post	0.164	0.281	0.192						
	(0.117)	(0.146)	(0.142)						
Area under CA (ac) × Post				0.049	0.115	0.065			
				(0.073)	(0.094)	(0.087)			
Area under CA (percent of total area) × Post							0.241	0.359	0.269
							(0.169)	(0.211)	(0.201)
Individual/farm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,456	2,456	1,674	2,456	2,456	1,674	2,456	2,456	1,674
R^2	0.353	0.354	0.327	0.352	0.353	0.326	0.353	0.353	0.327

Source: The authors.
Note: In columns (1), (4), and (7), results are based on estimating equation (5), replicating the results from columns (7)-(9) in Table 6. In columns (2), (5), and (8), we assume that only those households in villages that were subject to some monitoring are deemed to have practiced CA, with treatment households in villages that self-reported their land management practices deemed to have *not* practiced CA, even if they reported otherwise. In columns (3), (6), and (9), we restrict the sample to exlude treatment villages that were not subject to any monitoring. Across all regressions, additional controls include the age of the household head, family agricultural labor (male and female), hired agricultural labor (male and female), pesticide expenditures, the number of plots cultivated (and its square), and total area cultivated (and its square).