European Review of Agricultural Economics Vol **42 (1)** (2015) pp. 99–127
doi:10.1093/erae/jbu007
Advance Access Publication 27 May 2014

Decentralised innovation systems and poverty reduction: experimental evidence from Central Africa

Haki Pamuk[†], Erwin Bulte[†‡]*, Adewale Adekunle[§] and Aliou Diagne**

[†]*Tilburg University, Netherlands;* [‡]*Wageningen University, Netherlands;*
[§]*Forum for Agricultural Research in Africa (FARA), Accra, Ghana;*
[**]*Africa Rice Center, Cotonou, Benin*

Received July 2013; final version accepted March 2014

Review coordinated by Iain Fraser

Abstract

We use experimental data to investigate whether a decentralised approach to promoting innovation in central African agriculture outperforms conventional extension approaches. Our main result is that this decentralised approach, based on so-called innovation platforms, is effective in reducing poverty – more effective than conventional extension approaches. However, we also document considerable heterogeneity in terms of platform performance.

Keywords: poverty, adoption of agricultural innovations, innovation systems, impact assessment, bottom-up versus top-down, participatory development

JEL classification: O3, Q1

1. Introduction

Agricultural development in Africa has resurfaced as a priority issue on the international development agenda. In addition to obvious concerns about food security and prices, three factors are responsible for the recent re-appraisal of African farming: targeting, comparative advantage and inter-sectoral linkages. Some 75 per cent of the poor in developing countries live in rural areas, and the majority of them depend on agriculture for their livelihoods. Given agriculture's dominant role in the lives of the rural poor, it makes sense to centre strategies for cutting poverty on growth in this sector (World Bank, 2007). Moreover, most African countries are agriculture-based, and tend to have a comparative advantage in the production of primary commodities.

*Corresponding author: Email: erwin.bulte@wur.nl

Finally, agricultural growth has large multiplier effects in early stages of development (Haggblade, Hazell and Dorosh, 2007). The growth in GDP originating in agriculture raises incomes of the poor much more than growth originating elsewhere in the economy (Ligon and Sadoulet, 2007), especially for the poorest and especially in early stages of development (Christiaensen, Demery and Kuhl, 2010).

African rural society is characterised by high transaction costs and risk, hampered information flows and a weak institutional environment. As a result, both market development and access to existing markets are inhibited. Creating an enabling institutional and policy environment is a necessary condition for African farming to take-off (IFPRI, 2010). Therefore, the new development agenda emphasises (i) linking farmers to input and output markets, (ii) identifying governance arrangements to strengthen property rights and asset control and (iii) promoting technical innovation and diffusion of knowledge to increase land and labour productivity (Djurfeld *et al.,* 2006; World Bank, 2007; Dorward *et al.,* 2009; IFPRI, 2010). Increasingly it is recognised that these elements hang together, and that innovation in the domains of governance and technology could go hand-in-hand.

Agricultural innovation among African smallholders has progressed slowly, and efforts to promote the adoption of new technologies, even if occasionally successful locally, have largely proved unsuccessful. A challenging perspective of conventional, top-down approaches to extension argues that agricultural research should be embedded in a larger 'innovation system', integrating knowledge from various actors and stakeholders. This amounts to a participatory approach to innovation and diffusion, which implies a shift from viewing innovation as a 'product to a process' (Knickel *et al.,* 2009). In such an innovation system, agents such as firms, research institutes, intermediaries, customers, authorities and financial organisations are interacting partners resulting in non-linear, iterative processes (Geels, 2004; Mierlo *et al.,* 2010).

The main objective of this study is to compare the performance of traditional 'top-down' approaches to innovation and extension to the performance of a decentralised innovation system approach, and to compare both approaches to the default case of doing nothing. Specifically, we focus on the impact of so-called innovation platforms (IPs) on the alleviation of rural poverty and on food consumption. We also probe potential channels explaining impact, focusing on the adoption of specific technological and institutional innovations.

The question whether decentralised, local approaches to extension outperform centralized, top-down ones links to a broader debate that goers back to at least Scott (1989). Scott argues that centrally managed and highly schematic development visions do violence to complex local interdependencies, and systematically fail to achieve their objectives. As an alternative to such 'high-modernist' ideologies, based on epistemic knowledge, he proposes greater emphasis on local, practical knowledge (which he labels 'metis'). From a theoretical perspective, it is not obvious which approach to innovation is more efficient and effective – the traditional, centralised model or the local and participatory model. Economies of scale in innovation and transfer may imply greater benefits for the centralised approach. In contrast, the decentralised approach is presumably better able to capitalise on local knowledge about constraints and possibilities, and local understanding of needs and priorities.

Local institutions, such as the ones that facilitate capitalising on local knowledge, tend to co-evolve with communities, and respond to local regulatory or cultural issues. In models explaining economic performance based on observational data, local institutions are likely to be endogenous. Careful econometric analysis, based on propensity score matching or instrumental variable strategies,[1] may enable the analyst to attenuate these endogeneity concerns (even if some concerns will remain due to unobserved heterogeneity). An alternative approach to probe the causal impact of institutional innovations is to introduce variation in these institutions – as part of an experiment. This is the approach taken in this paper. Our identification strategy is based on experimental data obtained in the Sub-Sahara African Challenge Program (SSA CP). In a sample of villages in selected countries, IPs were introduced – forums where local stakeholders come together and search for practical ways to advance their livelihoods. We analyse how poverty in these IP villages compares with outcomes in communities served by the traditional innovation approach, and to outcomes in a sample of control villages.

Two remarks are in order. First, our data do not derive from a full-fledged randomised control trial (RCT). The intervention villages were not randomly drawn from the same sample as the control villages (but an effort was made to ensure that the treated and control villages were 'similar'). This has implications for the data analysis. Second, IPs were introduced in 2008 and 2009, and follow-up data were collected in 2010. Hence, we can only pick up short-term effects. Future work, based on additional data to be collected in the future (in 2014), should explore whether the results we obtain are sustainable, or are overtaken by other events, and explore whether the channels via which IPs have impact on poverty evolve over time.

We obtain a nuanced set of results. On average, the decentralised innovation systems approach is better able to alleviate poverty than the traditional or conventional approach (and both approaches are better than doing nothing). However, we also document considerable heterogeneity across IPs. There are successful IPs as well as unsuccessful ones in terms of poverty alleviation, and it appears as if some of the platforms have failed to engage the relevant stakeholders, or have otherwise been unable to mobilise stocks of local knowledge. Unearthing the determinants of IP performance is left as an urgent priority for future research.

The paper is organised as follows. In Section 2, we briefly summarise key lessons from the literature on agricultural innovations in Africa. In Section 3, we describe the Sub-Sahara African Challenge Program, and the nature of its main intervention – the creation of IPs in selected villages. In Sections 4 and 5,

1 For example, Mapila *et al.* (2011) uses propensity score matching to investigate the impact of agricultural innovation systems on rural livelihoods in Malawi. They conclude innovation systems increased the rural income, upland crop production and fertiliser use.

we summarise our data and outline our identification strategy, respectively. Section 6 presents the results, focusing on average poverty impacts of the innovation system approach and on heterogeneous treatment effects (across IPs and across individuals treated by the same platform). In Section 7, we probe the channels linking IPs to reduced poverty and Section 8 concludes.

2. Agricultural innovations in Africa

Agricultural yields in many African countries have been declining in recent decades. One reason for this disappointing outcome is imperfect adoption of innovations. Agricultural innovations may be a significant growth factor for the economy as a whole, via effects on demands for inputs and prices of food (see the recent paper on mechanisation in US farming by Steckel and White, 2012). While various yield-increasing technologies are available for African farmers, their uptake among smallholders remains far $<$100 per cent. Key factors identified in the literature include factors directly linked to the technology (availability or untimely delivery of innovations, high costs, demands on complementary inputs, 'riskiness'), factors at the level of individual farmers (e.g. education, access to credit, but also risk preferences and loss aversion – see Liu, 2012) and contextual factors such as poor extension, transaction costs (e.g. bad infrastructure), access to value chains (Barrett *et al.,* 2012) and geophysical conditions (for discussions, refer to Feder, Just and Zilberman, 1985; Rogers, 1995; Sunding and Zilberman, 2001; Suri, 2011). Recent academic work emphasises the role of social learning and networks in innovation and diffusion processes (e.g. Bandiera and Rasul, 2006; Conley and Udry, 2010).

Some analysts argue that an important cause of the limited impact of traditional research and extension activities in Africa is the simplistic yet dominant view on innovation processes (Leeuwis and van de Ban, 2004). According to the traditional adoption and diffusion model (or pipe-line model, sometimes referred to as technology-transfer model, delivery model or technology-push model) innovation is conceptualised as a linear process. It starts with conception by scientists and extends to adoption by farmers, via extension workers (Knickel *et al.,* 2009). Research, transfer and adoption are independent activities, and there is little attention for the context within which these processes are embedded.

Consequently, traditional extension – for which various modalities exist, including the well-known training and visit (T&V) and village agent model – often amounted to 'blanket recommendations'. Such recommendations might not fit with local conditions. For example, heterogeneity in returns to new technology has recently been documented by Duflo, Kremer and Robinson (2008) for the case of fertiliser and by Suri (2011) for the case of hybrid maize. The lack of a fit between recommended technologies and local needs may be especially pronounced when research and extension are biased towards big farmers. Not surprisingly, then, demand for extension may be weak among food producing smallholders in peripheral locations (Holmen, 2005). There are additional reasons for pessimism about the effectiveness of traditional extension. 'Public services have dominated extension. . . . But public financing and provision face profound problems of incentives of civil servants for accountability to their clients, weak political commitments to extension, extension workers not being abreast of relevant emerging technological and other developments, a severe lack of fiscal sustainability in many countries, and weak evidence of impact' (World Bank, 2007, p.173).

In fairness, the traditional approach to extension is gradually changing, shifting from the prescription of technological practices to focusing on capacity building among rural people – empowering them (World Bank, 2007). Accordingly, extension efforts now sometimes include a broader range of approaches, including public–private partnerships (collaboration between state, firms and NGOs) and farmer-to-farmer training. However, conventional extension in our study region is still characterised by a single line of command, based on 'expert knowledge' flowing to farmers through a network of public extension agents. We seek to explore whether participatory approaches to innovation and diffusion are more or less successful in reducing rural poverty in our study region.

3. Programme description: introducing innovation systems in African farming

The SSA CP started in 2004. To remedy perceived problems with the traditional approach to extension, a new approach was proposed named Integrated Agricultural Research for Development (IAR4D). It aims to bring stakeholders together and integrate their knowledge so as to generate network effects and stimulate innovation relevant for the local context. The ultimate objective is to alleviate rural poverty.

The IAR4D approach aims to promote innovations via IPs. IPs are introduced in selected locations (serving various villages), and serve as vehicles to bring together representatives of farmers' associations, private firms and traders, researchers, extension workers, NGOs and government policy-makers. Ideally, an IP should decide on membership of stakeholder groups through a participatory and bottom-up process. Selected stakeholders should come together, diagnose common challenges and bottlenecks, and decide on strategies to overcome key problems. This includes raising awareness among local communities for adopting the innovations prioritised in the action plan – assigned IP members go to the field and facilitate adoption (FARA, 2008).[2] The innovation system operates at the local level, responding to local challenges, hence, across IPs the diagnosis and strategy setting stages may produce different outcomes. Importantly for the purposes of this evaluation the intervention did *not* include subsidised access to certain inputs (which would otherwise have confounded the poverty impact of the institutional innovation).

2 However, there is always a risk that IPs might not function ideally. For instance, some stakeholders might promote the adoption of specific innovations before other stakeholders have decided on the bottlenecks.

The Forum for Agricultural Research in Africa coordinated the implementation of the SSA CP, and aimed to investigate IAR4D's effectiveness relative to doing nothing and conventional research and extension approaches. For the latter purpose, the implementation plan was designed as an experiment. The objective was to obtain results informative about agricultural development across the African continent, hence the programme was rolled out in three major subregions (so-called project learning sites (PLS)): (i) 'Lake Kivu' in Eastern and Central Africa, (ii) 'Kano-Katsina-Maradi' in West Africa and (iii) Zimbabwe-Malawi-Mozambique in Southern Africa. In total, 36 IPs were created – 12 per PLS. An IP serves multiple intervention villages (typically between 5 and 10 villages, so the number of treated villages was expected to be between 60 and 120 villages per PLS). Per village, 10 households were randomly sampled and surveyed, so the total number of households surveyed per PLS is in the range of 600–1,200. To evaluate the performance of IAR4D villages, data were also collected in two types of comparison villages (conventional extension villages and control villages without any intervention – see below). The total number of respondents per PLS is therefore in the range of 1,800–3,600.

How were intervention and control villages selected? The details of the sampling procedure vary slightly across PLSs. For our analysis, we use data from the Lake Kivu region, capturing parts of Uganda, Rwanda and the Democratic Republic of Congo (DRC). In each country, a sample of *sites* or *wards* was selected (named *subcounties* in Uganda, *secteurs* in Rwanda and *groupements* in the DRC). These wards represent administrative groupings of multiple villages, and were selected to provide a representative sample in terms of market access and agro-ecological conditions. In total, 24 wards are included in the Lake Kivu PLS, evenly split across the three countries.

When designing the study, a trade-off had to be struck between the management of spill-over effects (e.g. counterfactual villages benefitting from activities or ideas generated at nearby platforms) and the balance of the sample. If treatment status would be randomly assigned at the village level, then treatment and counterfactual villages are expected to be similar at the baseline, both in terms of observables and unobservables. But random assignment at the village level also implies that treated villages may be located next to counterfactual villages. To attenuate potential spill-over bias, assignment into treatment was done at the level of the ward. This implies treated and counterfactual villages are clustered in space, minimising spill-over effects – a benefit that comes at the cost of reduced balance between treated and counterfactual villages (as will be evident below).

Twelve wards were assigned to receive the treatment, and consequently a random subsample of (clean) villages from these wards received an IP. We define 'clean villages' as villages that did not receive any (conventional) projects in the 5 years preceding the intervention (i.e. no extension or NGO activities during the period 2003–2008). The other 12 wards were assigned to control status, and a random sample of villages from these wards comprises our samples of counterfactual villages. Specifically, villages from these 'control wards' were assessed and classified into one of two types of villages: (i) clean villages that had neither received IAR4D nor conventional projects in the previous 2–5 years and (ii) conventional extension villages, that had received projects identifying, promoting and disseminating technologies in the same period. Hence, based on their individual history of exposure to extension, some villages drawn from the control wards were labelled as 'control (clean) villages', and others as 'conventional (extension) control villages'.

It is important to note that the historical allocation of extension workers across the African landscape is possibly non-random. Hence, we need to delve into selection issues and potential endogeneities when assessing the impact of IPs. Details of our identification strategy are discussed in Section 5.

4. Data

We use data from the Lake Kivu PLS containing villages in the DRC, Rwanda and Uganda. For this site, 76 villages were randomly selected to be 'treated' by IAR4D (i.e. received an IP). There was no non-compliance – all villages accepted the IP (but there is variation in the nature of the intervention across sites; see below). A village census was carried out in adjacent wards to construct a sample frame and stratify villages into the sets of '(clean) control' and '(conventional) extension' villages. Next, 85 villages were drawn from the set of control villages, and another 85 villages were drawn from the set of traditional extension villages. Note that control and conventional extension villages were drawn from the same 12 wards, and that these wards are not the same as the ones from which the IAR4D villages were selected.

Baseline data were collected in the DRC, Rwanda and Uganda in 2008–2009, and the next wave of data was collected in 2010. Since some of the baseline data are collected in late 2008 and others in early 2009, we control for the timing of data collection via a dummy variable. Over both surveys, we observe some 2,230 households, residing in 244 villages (indicating some attrition as the number of respondents in the baseline wave was 2,402). The average number of respondents per village was 9.5 (standard deviation 1.6). A summary of the sampling frame is provided in Table 1.[3]

Table 2 summarises our outcome variables. These include innovation proxies (as intermediate outputs) and two poverty indicators. As poverty indicators, we use the commonly used headcount ratio (measured at the village level) as our primary measure, and a less-standard household-level food consumption score (FCS). Our poverty rate estimate is not based on census income data,

3 One reason for attrition was oversampling at the baseline. At the baseline, we slightly oversampled villages and households in Rwanda. Subsequently, one village (Remera) was randomly dropped from the analysis. Moreover, 44 households were randomly dropped from other oversampled villages as well. One other village in the DRC could not be visited because of security concerns. A reason for remaining attrition is 'relocation' of the respondent. The analysis below is based on <2230 households because of missing values in either the base- or endline controls. However, we have also estimated the key models based on parsimonious specifications (fewer controls, more observations) and the results are very similar.

Table 1. Sample design

Survey	Control	Conventional	IAR4D (intervention)	Total
Households				
Baseline	806	816	780	2,402
Endline	769	776	685	2,230
Villages				
Baseline	85	85	76	246
Endline	84	85	75	244

but represents an estimate provided by the village leader and several other local 'leaders' (including school teachers etc.). During a focus group discussion, these leaders tried to reach a consensus regarding the number of households below the poverty line.[4] Poverty was defined as per capita income below USD 1.25. We discuss potential shortcomings of this variable in the final section.

The FCS index is based on daily food consumption of respondents during a short interval of time, corrected for the nutritional value of food items consumed.[5] It is well known that such measures may fluctuate over the seasons. However, since our data were collected in treatment and comparison villages simultaneously, we are able to control for such seasonal influences in our empirical analysis.[6]

We distinguish between four different categories of innovation variables: technology indicators, marketing strategies, access to village resources and land regulations. We construct innovation indices for each category separately, by summing the relevant binary innovation variables. Hence, following van der Ploeg *et al.* (2004) and Pamuk *et al.* (2014), we interpret 'innovation' quite broadly, encompassing technologies as well as governance arrangements, the adoption of new regulations, changes in market participation practices or access to new infrastructure. Unlike the adoption of techniques, we treat

4 While we appreciate the potential concern that focus-group estimates of local poverty may be less than perfect, we believe it is fair to say that household poverty data are typically also imperfect – obtaining reliable income data is notoriously difficult, which is why the challenge programme opted for the focus group methodology. Note that the focus group data are available in panel format (for both treated and control groups) so systematic errors in measurement should not concern us.

5 To construct this index, we used information about household consumption of certain groups of food during the last 24h. Food groups are cereals, vitamin-rich vegetables and tubers, white tubers and roots, dark green leafy vegetables, other vegetables, vitamin A-rich fruits and other fruits, meat, eggs, fish, legumes, nuts and seeds, milk and milk products, oils and fats, sweets, spices, caffeine or alcoholic beverages. We score each food group based on the World Food Program Technical Guidance Sheet for Food Consumption Score (UN, 2008). Scores increase with the nutrition level of the food group, and the index score for each household is calculated by summing group scores.

6 Specifically, our estimates of the impact of the intervention relative to the control and conventional extension villages will be unaffected if all types of villages respond the same way to seasonal fluctuations.

Table 2. Outcome (dependent) variable definitions

Variable	Definition
Poverty indicators	
Headcount ratio	Percentage of the people living under poverty line
FCS	FCS, calorie weighted average of daily consumption of a respondent
Technology indicators	
Mulching	Equals 1 if a household uses mulching, 0 otherwise
Trenches/terraces	Equals 1 if a household uses trenches/terraces, 0 otherwise
Water harvesting	Equals 1 if a household uses water harvesting, 0 otherwise
Irrigation	Equals 1 if a household uses irrigation techniques, 0 otherwise
Conservation farming	Equals 1 if a household uses conservation farming, 0 otherwise
Animal manure	Equals 1 if a household uses animal manure 0 otherwise
Cover crops	Equals 1 if a household uses cover crops, 0 otherwise
Crop rotation	Equals 1 if a household uses crop rotation, 0 otherwise
Inter cropping	Equals 1 if a household uses inter cropping, 0 otherwise
Rhizobiainoculation	Equals 1 if a household uses Rhizobiainoculation, 0 otherwise
Chemical fertiliser	Equals 1 if a household uses chemical fertiliser, 0 otherwise
Row planting	Equals 1 if a household uses row planting, 0 otherwise
Plant spacing	Equals 1 if a household uses plant spacing, 0 otherwise
Organic pesticide	Equals 1 if a household uses organic pesticide, 0 otherwise
Inorganic pesticide	Equals 1 if a household uses inorganic pesticide, 0 otherwise
Drying	Equals 1 if a household uses drying, 0 otherwise
Threshing/shelling	Equals 1 if a household uses threshing shelling equipment, 0 otherwise
Improved storage facilities	Equals 1 if a household uses improved storage facilities, 0 otherwise
Pest control	Equals 1 if a household uses pest control, 0 otherwise
Grading	Equals 1 if a household uses grading, 0 otherwise
Land regulation	
Nrmbylaws	Equals 1 if the local council in the village enacted any bylaws related with natural resource management, 0 otherwise
Landbylaws	Equals 1 if there any bylaws affecting land management in the village, 0 otherwise
Marketing strategies	
Notsold	Equals 1 if household did not sell at least one type of product it produced, 0 otherwise
Consumers	Equals 1 if household sold at least one type of product on farm to consumers, 0 otherwise
Middleman	Equals 1 if household sold at least one type of product on farm to middleman, 0 otherwise
On the roadside	Equals 1 if household sold at least one type of product on the road side, 0 otherwise
Local market	Equals 1 if household sold at least one type of product at the local/village market, 0 otherwise

(*continued*)

Table 2. (*continued*)

Variable	Definition
District town	Equals 1 if household sold at least one type of product at the district town market, 0 otherwise
Distant market	Equals 1 if household sold at least one type of product at a distant market, 0 otherwise
Sold	Equals 1 if household sold at least one type of product it produced, 0 otherwise
Village resources	
Wells	Equals 1 if the village have boreholes/wells, 0 otherwise
Veterinary	Equals 1 if the village have cattle dips/veterinary, 0 otherwise
Woodlots	Equals 1 if the village have village woodlots, 0 otherwise
Water body	Equals 1 if the village have water bodies, 0 otherwise
Watering points	Equals 1 if the village have livestock watering points

institutional or access innovations as community variables – common to all households in the village.

Finally, our control variables are summarised in Table 3. We distinguish between household and village characteristics. While we focus on village variables, the household variables allow us to analyse heterogeneous impact across various dimensions, and test for potential selection bias (e.g. education, gender, household structure and wealth, farming practice, access to credit, community development). As mentioned, we also created a survey time dummy, capturing whether the household was first surveyed in 2008 or 2009.

4.1 Testing for balance

Since the IAR4D and counterfactual villages were not randomly selected from the (same) population of villages, it is imperative to check how the three groups of villages compare at the baseline. Table 4 compares control, conventional and IAR4D villages in terms of dependent variables and (household and village) controls. The first three columns provide subgroup averages for the various variables, and the other three columns test whether observed differences are significant or not.

While there are no significant differences in poverty variables between conventional extension and control villages nor between the IAR4D and conventional extension villages, we do observe that on average the number of poor people in IAR4D villages is higher than in control villages. Failing to account for such pre-existing differences will bias impact assessments. In terms of food consumption, we do not measure significant differences across the three types of villages.

In terms of our household controls, there are hardly any differences between the three types of villages. It appears as if the number of respondents with secondary education is somewhat smaller in IAR4D villages than in control villages and households living in IAR4D villages have more access to formal credit. But the differences are very small and some random differences are

Table 3. Variable definitions for control variables

Variable	Definition
Household characteristics	
edu_primary	Equals 1 if household member having highest education level at most have completed primary school, 0 otherwise
edu_secondary	Equals 1 if household member having highest education level at least have some vocational training and at most have completed secondary education, 0 otherwise
edu_univer	Equals 1 if household member having highest education level at least have attended to a college and at most have completed a university, 0 otherwise
Gender	Equals 1 if household head is male
Hhsize	Number of persons living in the household
Duration	Number of years of experience in farming of household head
Age 15–24	Equals1 if age of the household head between 15 and 24, 0 otherwise
Age 25–34	Equals1 if age of the household head between 25 and 34, 0 otherwise
Age 35–44	Equals1 if age of the household head between 35 and 44, 0 otherwise
Age 45–54	Equals1 if age of the household head between 45 and 54, 0 otherwise
Age 55–64	Equals1 if age of the household head between 55 and 64, 0 otherwise
Age 65+	Equals1 if age of the household head is >65, 0 otherwise
Dependency	Ratio of the number of household members aged <16 and >64 to the number of members aged between 16 and 64
borrowed_formal	Equals 1 if household borrowed from bank or micro or government credit schemes credit institutions, 0 otherwise
borrowed_informal	Equals 1 if household borrowed from informal savings, money lender, NGO/Church, relatives, 0 otherwise
rooms1	Equals 1 if household lives in a house having no rooms or one room, 0 otherwise
rooms2	Equals 1 if household lives in a house having two rooms, 0 otherwise
rooms3	Equals 1 if household lives in a house having three rooms, 0 otherwise
rooms4	Equals 1 if household lives in a house having four rooms, 0 otherwise
rooms5	Equals 1 if household lives in a house having five or more rooms, 0 otherwise
survey time	Equals 1 if baseline of survey is applied in 2009, 0 if it is applied in 2008
Village characteristics	
School	Equals 1 if the village have schools, 0 otherwise
Hospital	Equals 1 if the village have hospitals/clinic/health, 0 otherwise
Telephone	Equals 1 if the village have telephones, 0 otherwise
Roads	Equals 1 if the village have all-weather roads passing, 0 otherwise
Country1	Equals 1 if the village is in DRC, 0 otherwise
Country2	Equals 1 if the village is in Rwanda, 0 otherwise
Country3	Equals 1 if the village is in Uganda, 0 otherwise

Table 4. Mean values for baseline variables

Indicators	Control	Conventional	IAR4D	Conv. – Control	IAR4D – Control	IAR4D – Conv.
Poverty indicators						
Headcount ratio	43.09	51.82	56.45	8.73	13.36**	4.63
FCS	13.69	13.41	13.15	−0.27	−0.54	−0.26
Household characteristics						
Gender	0.79	0.82	0.82	0.03	0.02	−0.01
Age 15–24	0.05	0.06	0.06	0.01	0.00	0.00
Age 25–34	0.20	0.20	0.22	0.00	0.02	0.02
Age 35–44	0.25	0.24	0.24	−0.01	−0.01	0.00
Age 45–54	0.22	0.24	0.23	0.02	0.01	−0.02
Age 55–64	0.15	0.13	0.15	−0.02	0.00	0.01
Hhsize	6.55	6.74	6.37	0.19	−0.17	−0.36*
edu_secondary	0.33	0.33	0.26	0.00	−0.07**	−0.07**
edu_univer	0.05	0.08	0.06	0.03	0.01	−0.02
Dependency	1.34	1.31	1.30	−0.03**	−0.04	−0.01
rooms1	0.06	0.04	0.04	−0.02	−0.02*	0.00
rooms2	0.16	0.13	0.13	−0.03	−0.03	0.00
rooms3	0.24	0.25	0.27	0.00	0.02	0.02
rooms4	0.33	0.33	0.35	0.00	0.01	0.02
rooms5	0.21	0.26	0.22	0.05*	0.01	−0.04
borrowed_formal	0.03	0.04	0.06	0.01	0.03*	0.02
borrowed_informal	0.64	0.68	0.68	0.03	0.03	0.00
Duration	22.43	22.06	21.40	−0.37	−1.03	−0.65
Village characteristics						
School	0.48	0.44	0.47	−0.04	0.00	0.03
Hospital	0.13	0.20	0.12	0.07	−0.02	−0.08
Telephone	0.52	0.49	0.53	−0.03	0.00	0.04
Roads	0.45	0.51	0.59	0.07	0.14*	0.08
Survtime	0.29	0.29	0.20	0.00	0.09	−0.09
Country1	0.35	0.35	0.26	0.00	−0.09	−0.09
Country2	0.29	0.29	0.34	0.00	0.05	0.05
Country3	0.35	0.35	0.40	0.00	0.04	0.04

Standard errors for the differences in household characteristics are calculated by using robust standard errors clustered at village level.
*$p < 0.05$.
**$p < 0.01$.
***$p < 0.001$.

not unexpected given the size of our sample. It is interesting to observe, however, that in terms of household variables there are hardly any differences between the conventional and control villages. We observe that, in conventional villages, average house size is slightly larger, and the number of respondents who completed secondary education is slightly higher than in control villages.

The situation is also similar for village characteristics. When comparing IAR4D villages to control ones, there does not seem to be a systematic bias. The only finding is that IAR4D villages are more likely to be connected via an all-weather road than control villages (so we control for this in the empirical analysis below). We again do not observe any difference between conventional and control villages in terms of observed characteristics. So, if extension workers purposefully selected some villages and not others, it appears as if they are not basing their selection on village characteristics.

5. Identification: average treatment effects and heterogeneous impact

We now outline our identification strategy. We are evaluating the impact of IPs on poverty rates and innovation proxies as intermediate outcome variables. Note, this is not necessarily the same as evaluating the impact of IAR4D on poverty rates. The reason is that there may be non-compliance in the sense that not all IPs function as intended by the IAR4D philosophy. While all treatment villages received their treatment (i.e. they received an IP), the level of stakeholder engagement and bottom-up priority setting may vary from one IP to the next. As an extension of the current analysis, one might develop an index measuring the 'degree of IAR4Dness' across the platforms. This would enable the analyst to estimate an IV model using assignment status as an instrumental variable for index scores, and regress poverty and adoption rates on predicted IAR4Dness. Such a strategy would yield a local average treatment effect of IAR4D on poverty rates. The current analysis based on a comparison of poverty rates and food security across IP villages and counterfactual villages yields an intent-to-treat (ITT) estimator of the average treatment effect (ATE) of receiving an IP. In what follows, and slightly abusing terminology, we also refer to this as the ITT of receiving IAR4D treatment.

We seek to gauge impact by comparing IAR4D villages (i.e. villages benefitting from an IP) and either conventional or control villages in terms of reduced poverty. If extension workers selected the set of conventional villages non-randomly, then failing to account for this may introduce selection bias. The literature suggests several ways to accommodate this concern (e.g. Angrist and Pischke, 2009; Imbens and Wooldridge, 2009). We use (i) a difference-in-difference (DD) methodology that combines aggregate baseline and endline data and (ii) a first-difference (panel) methodology, where we base impact assessment on intra-unit comparisons over time.

Lack of control over conventional extension activities introduces another problem. By definition, conventional extension activities started *before* the SSA CP started. Hence, conventional villages started receiving their intervention before the IAR4D concept was implemented, and cumulative effort in conventional villages could easily exceed effort in IAR4D villages. This cumulative effect could confound simple comparisons of endline data. However, *ex ante* there is no significant difference in the headcount ratio between conventional and control villages, according to the evidence in Table 4. This might simply reflect that conventional approaches to innovation and diffusion have been ineffective.

Another factor may be relevant. Insofar as it takes time to gain momentum and genuinely achieve impact, the deck is stacked against IAR4D – the conventional villages made a flying start at $t = 0$, and, hence, should be able to accomplish more during the interval from $t = 0$ until $t = 1$ (thus, perform superiorly according to the DD or panel model). In contrast, if there are diminishing returns to intervention effort, then perhaps the 'greenfield' start of IAR4D implies an advantage in a panel setting. The reverse is true in case of increasing returns to intervention effort. These are caveats that should be borne in mind when interpreting the empirical results, but which cannot be addressed rigorously with the data currently at our disposal.

5.1 Intention to treat effects

Define outcome variables, which are introduced in Table 2, for individual *i,* living at village *v* at time *t* by Y_{0ivt}, Y_{1ivt}, Y_{2ivt} for control (subscript 0), conventional (subscript 1) and intervention/IAR4D treatment groups (subscript 2), respectively. We will drop the *i* subscript for outcome variables at the village level. Treatment groups are denoted by $Control_v$, $Conv_v$ and $IAR4D_v$ for control, conventional and IAR4D villages, respectively. Treatment dummies are equal to 1 if the household (or village) belongs to that group and 0 otherwise. Since villages can only belong to one treatment group, we know

$$Control_v + Conv_v + IAR4D_v = 1. \tag{1}$$

The simplest analysis rests on a comparison of endline data. Estimates are unbiased if a classical conditional independence assumption holds

$$E[Y_{iv}|X_i, Z_v, IAR4D_v,\ Conv_v] = E[Y_{iv}|X_i, Z_v], \tag{2}$$

where X_i refers to a vector of observed household characteristics and Z_v denotes the vector of village level characteristics. Condition (2) states that, after controlling for household and village characteristics, the likelihood of being in a control, conventional extension or IAR4D village is same for all households. If we also assume there is a linear relationship between outcome and treatment plus other control variables, we can formulate the following regression model:

$$Y_{iv} = \alpha + \gamma_1 IAR4D_v + \gamma_2 Conv_v + \beta' X_i + \theta' Z_v + \varepsilon_{iv}, \tag{3}$$

where ε_{iv1} denotes an error term. In equation (3), γ_1 and γ_2 capture the ATE of IAR4D and conventional policies on control villages. Since we aim to assess whether IPs are more effective than conventional policies, we test whether $\gamma_1 - \gamma_2 \neq 0$. To test $\gamma_1 \neq 0$ and $\gamma_1 - \gamma_2 \neq 0$ within one model, we reformulate (3) by using equation (1) such that

$$Y_{iv} = \alpha + \delta_1 Control_v + \delta_2 Conv_v + \beta' X_i + \theta' Z_v + \varepsilon_{iv}. \tag{4}$$

This gives us $-\delta_1 \equiv \gamma_1$ and $-\delta_2 \equiv \gamma_2 - \gamma_1$. However, estimating equation (4) likely produces biased estimates of impact because it is unlikely that the assumption of conditional independence holds. Relaxing this assumption, we now introduce a DD model that combines endline and baseline data. With the usual constant trend assumption, we obtain the following model for outcome variable, Y_{ivt}:

$$Y_{\mathrm{ivt}} = \alpha + \mu\, endline_{\mathrm{t}} + \sigma_1 Control_{\mathrm{v}} + \sigma_2 Conv_{\mathrm{v}} + \delta_1(endline \times Control_{\mathrm{v}}) + \delta_1(endline_{\mathrm{t}} \times Conv_{\mathrm{v}}) + \beta' X_{\mathrm{it}} + \theta' Z_{\mathrm{vt}} + \varepsilon_{\mathrm{ivt}}, \quad (5)$$

where $endline_t = 1$ if $t = 1$ (i.e. for the endline survey) and $endline_t = 0$ otherwise. In equation (5), $-\delta_1$ and $-\delta_2$ provide the ATE of IPs on control villages, and the difference between IAR4D and conventional approaches, respectively.

Unobserved variables may drive the selection of conventional villages and also be correlated with the outcome variable. Assuming that these unobserved characteristics are constant and separable, the expected outcome variable can be formulated as follows:

$$Y_{\mathrm{ivt}} = \alpha_i + \mu\, endline_{\mathrm{t}} + \sigma_1 Control_{\mathrm{v}} + \sigma_2 Conv_{\mathrm{v}} + \delta_1(endline_{\mathrm{t}} \times Control_{\mathrm{v}}) + \delta_2(endline_{\mathrm{t}} \times Conv_{\mathrm{v}}) + \beta' X_{\mathrm{it}} + \theta' Z_{\mathrm{vt}} + \varepsilon_{\mathrm{ivt}}. \quad (6)$$

To eliminate unobserved fixed effects, we first-difference (6) so that

$$\Delta Y_{\mathrm{iv1}} = \mu + \delta_1 Control_{\mathrm{v}} + \delta_2 Conv_{\mathrm{v}} + \beta' \Delta X_{\mathrm{it}} + \theta' f \Delta Z_{\mathrm{vt}} + \Delta\varepsilon_{\mathrm{ivt}}. \quad (7)$$

In what follows we will refer to this model as the first difference, or FD, model. The DD and FD models are complementary approaches to dealing with potential selection effects caused by the non-random selection of conventional villages. Models (5) and (7) are estimated using OLS.[7] In all estimations, we include household and village characteristics summarised in Table 3. As the headcount ratio indicator, land regulations and village resources variables are available at the level of the village, we estimate models for those variables at the village level, and take unweighted averages of relevant household variables to arrive at village-level variables. Finally, since there may be correlation among households within villages, we cluster standard errors at the village level, and use robust standard errors (i.e. models explaining FCSs).[8]

7 This means we use linear probability models to deal with binary outcomes, allowing ready comparison across specifications. Our specifications should be robust with respect to these commonly used methodologies as most of the covariates are dummy variables. If we assume that treatment heterogeneity is limited, regression estimations are close to the average effects (indeed, fitted probabilities will be between 0 and 1 – see Section 5 for evidence on heterogeneity). However, we have also estimated non-linear models and our qualitative results do not change much then (even if for two of the innovation indicators different results emerge – estimates available on request).

8 In theory, our estimates could be biased if alternative organisations implemented other interventions systematically targeting IAR4D villages or comparison villages. We have kept track of other interventions in IAR4D villages, and found this hardly occurred. We have no data on other projects in comparison villages. If another organisation specifically targeted our comparison villages and implemented a project that alleviated (enhanced) local poverty, then our DD and FD models will underestimate (overestimate) the true impact of the IAR4D intervention.

5.2 Tackling heterogeneity

While the average impact of conventional and IAR4D treatments in terms of reduced poverty may be assessed using the above strategy, it ignores that the returns to the treatment may vary across IPs, depending on local circumstances. To probe into this issue, we analyse heterogeneity in impact. We take the entire sample of control villages as the counterfactual for each IP (but obtain similar results when using, instead, only control villages from the same country as the IP in question as the counterfactual), and explore how impact varies for the 12 IPs by using the following model:

$$\Delta Y_{\mathrm{ivt}} = \mu + \delta_1 Control_{\mathrm{v}} + \Sigma_{\mathrm{ip}} \theta_{2\mathrm{ip}} \mathrm{IP}_{\mathrm{ip}} + \beta' \Delta X_{\mathrm{it}} + \theta' \Delta Z_{\mathrm{vt}} + \Delta_{\varepsilon\mathrm{ivt}}, \quad (8)$$

$$\Delta Y_{\mathrm{ivt}} = \mu + \delta_2 Control_{\mathrm{v}} + \Sigma_{\mathrm{ip}} \theta_{1\mathrm{ip}} \mathrm{IP}_{\mathrm{ip}} + \beta' \Delta X_{\mathrm{it}} + \theta' \Delta Z_{\mathrm{vt}} + \Delta_{\varepsilon\mathrm{ivt}}, \quad (9)$$

where ip denotes each IP (ip $= 1, \ldots, 12$). $\mathrm{IP}_{\mathrm{ip}} = 1$ if a household lives in an IAR4D village. If IAR4D has an impact for a specific IP, then $\theta_{1\mathrm{ip}} \neq 0$. Moreover, if $\theta_{2\mathrm{ip}} \neq 0$, then this impact is different from the effect of the conventional approach. Heterogeneity in terms of impact implies $\theta_{1\mathrm{ip}} \neq \theta_{1\mathrm{ip}'}$ where ip $\neq$ ip$'$.

Heterogeneity might also materialise at the household, rather than the IP, level. Not all households may be able to benefit from the proposed innovation (e.g. because it does not meet their capabilities, skills, assets or desires). Indeed, if IPs are hi-jacked to serve the interests of local elites, they could aggravate local inequality. We therefore speculate that the impact of IAR4D might vary with certain household characteristics. To examine whether this is true, we estimate the following model, which is based on equation (7) but includes interaction terms:

$$\Delta Y_{\mathrm{ivt}} = \mu + \delta_{31}\, IAR4D_{\mathrm{v}} + \varphi'(IARD4D_{\mathrm{v}} \times F_{\mathrm{it}}{}^{\mathrm{k}}) + \delta_2 Conv_{\mathrm{v}} + \beta' \Delta X_{\mathrm{it}} + \theta' \Delta Z_{\mathrm{vt}} + \Delta \varepsilon_{\mathrm{ivt}}, \quad (10)$$

$$\Delta Y_{\mathrm{iv}} = \mu + \delta_{32} IAR4D_{\mathrm{v}} + \varphi'(IARD4D_{\mathrm{v}} \times F_{\mathrm{it}}{}^{\mathrm{k}}) + \delta_2 Control_{\mathrm{v}} + \beta' \Delta X_{\mathrm{it}} + \theta' \Delta Z_{\mathrm{vt}} + \Delta \varepsilon_{\mathrm{ivt}}, \quad (11)$$

where $IARD4D_{\mathrm{v}}$ is a dummy variable equal to 1 for intervention/IAR4D villages, and F_{it} is a vector of characteristics (a relevant subset of $\boldsymbol{X}_{\mathrm{i1}}$, see below). Parameters associated with the relevant interaction term, φ, reveal whether impact varies with different characteristics (note that φ from equations (10) and (11) are equivalent). Parameters δ_{31} and δ_{32} indicate ATEs relative to control and conventional villages, as before.

We interact four groups of variables with $IARD4D_{\mathrm{v}}$. Three groups are candidates for heterogeneous impact: (i) education (*edu_secondary* and *edu_univers*), (ii) agricultural experience (*duration*) and (iii) access to finance (*borrowed_formal* and *borrowed_informal*). The fourth variable for interacting captures the baseline survey time (*Surv_time*). This interaction term has a different interpretation, and allows us to tentatively explore whether the length of the intervention matters. By extension, this may be informative regarding the potential bias introduced by the fact that conventional villages have benefitted from intervention for a longer time than the IAR4D villages. If the estimate of the coefficient interaction term is jointly significant together with the estimated coefficient for *Surv_time* then the impact of intervention varies with the intervention length.

6. Estimation results for poverty indicators

We now turn to the regression results. In Table 5 we report ATEs of the IAR4D approach in terms of poverty. We report regression outcomes for the DD and FD model. For each model, the left column provides the estimated impact on control villages, and the right column reports differences between IAR4D and conventional extension. When estimating the models we included a full vector of control variables (see Table 3), but do not report these coefficients to economise on space.[9]

6.1 Intention to treat effects

We believe Table 5 contains the most important result of this paper. The IAR4D intervention successfully reduced poverty, and is more effective than conventional extension efforts in reducing poverty. Both the DD and FD models indicate that, compared with the control group of ‘control villages’, the number of people below the poverty line has fallen by some 17 per cent on average. Comparing IAR4D and conventional extension approaches produces a slightly smaller impact (~14 per cent fewer poor people), suggesting that the conventional extension strategy hardly outperforms doing nothing. These are striking results, in light of the fact that the IAR4D approach has been implemented for just 2 years, so that we are only picking up short-term effects.

The positive signs for the food consumption indicator in row 2 provide (very) weak support for the above conclusion. Note that the FCS coefficients are not statistically significant from 0. This could indicate various possibilities. Perhaps we measure food consumption with error (inflating standard errors), or perhaps the poor prefer to spend part of their extra income on other items than food. Or, alternatively, perhaps extra expenditures on food do not translate into extra calories (but in better-tasting food, say, as argued by Banerjee and Duflo, 2011). Subsequent results also suggest considerable heterogeneity in terms of food consumption at the IP level, masking aggregate impact (see below).

As mentioned above, these estimates may over- or underestimate the effectiveness of IPs. Note that, if there are diminishing (increasing) returns to

9 Due to missing observations for poverty indicators and control variables, the number of observations reported in Table 4 is lower than documented in Table 1. To test whether missing observations bias our results, we also estimated parsimonious models without control variables and with limited sets of control variables (varying sample size). We conclude our results are robust. To economise on space, we do not report those estimates, and they are available upon request.

Table 5. Estimated impacts of intervention on poverty and food consumption

	DD		FD	
	IAR4D – Control	*IAR4D – Conventional*	*IAR4D – Control*	*IAR4D – Conventional*
Headcount ratio	−18.26*** (6.468)	−12.96* (6.948)	−17.13** (7.582)	−14.25* (8.131)
	[$n = 402$]		[$n = 163$]	
FCS	0.776 (0.662)	0.328 (0.619)	0.636 (0.652)	0.166 (0.605)
	[$n = 3879$]		[$n = 1580$]	

Note: In all regression models, the controls listed in Table 2 are included (details available on request). Robust standard errors are in parentheses. The number of observations is reported in square brackets.
*$p < 0.1$.
**$p < 0.05$.
***$p < 0.01$.

intervention, then the estimated 14 per cent difference between IAR4D and conventional extension efforts according to the DD and FD model is an overestimate (underestimate) of the true gap in effectiveness over the 2-year study period. Regardless, since the headcount ratio in the IAR4D villages was greater than in the conventional villages at the time of the baseline survey (see Table 4), it appears as if the IAR4D villages have 'caught up'.

6.2 Heterogeneity across IPs

In Table 6, we examine whether there are differences, in terms of impact on the incidence of poverty, across IPs. We provide estimates for θ_{1ip} and θ_{2ip} from equations (8) and (9), respectively, for each IP separately. For any IP, the first row corresponds to the impact of IAR4D on the food consumption index and the number of poor people. The second row shows how this estimated impact compares to the impact of conventional extension efforts.

The results suggest considerable heterogeneity across IPs. Indeed, there are (i) successful IPs where poverty went down (Bufundi, Chahi, Mudende and Kituva), (ii) IPs where poverty appears unaffected, but also (iii) IPs where poverty has increased after the implementation of IPs (Rubare). Hence, ATEs mask large differences across platforms. Similar heterogeneity exists for our food consumption measure. It is interesting to note that the most successful IPs are the ones with high poverty rates at the baseline, suggesting a catching up process. It is also interesting to note that successful platforms are scattered across the study region, and not confined to one or two wards or countries with specific characteristics: Bufindi and Chahi are located in different districts in Uganda, Mudende is in Rwanda and Kituva is in the DRC. Hence, the results are not driven by cultural and institutional factors limited to a specific locality. However, and supporting the view that the impact of IPs varies with local conditions, not all poor IPs have above-average growth. For example, Rubare and Rumangabo are poor but not successful. Average poverty rates at the baseline were 57 and 66 per cent, respectively, and poverty rates went up after the intervention.

Note that the poverty and FCSs go hand-in-hand for several IPs (e.g. Bufundi, Chahi, Kituva and Rubare). But for one IP (Mudende), our data suggest a (dramatic) decrease in poverty rates that is not accompanied by an increase in food security scores. This is a puzzling result – perhaps reminiscent of results reported for India by Deaton and Dreze (2009). As mentioned above, it may reflect a near zero income elasticity for the food items included in the FCS measure, but in light of the low baseline score, this may not be plausible. Other candidate explanations exist. The estimated poverty impacts may be mismeasured. Recall our poverty data are based on focus group discussions, so they may be imprecise or open to manipulation. Alternatively, perhaps the poverty impact is actually less dramatic than it appears – if the platform translates into a small income gain for a large number of people just below the poverty line, then the headcount ratio falls a lot without affecting consumption patterns

Table 6. Poverty impacts at the level of individual IPs

IP name	Estimated coefficients	Dependent variables: FCS	Dependent variables: Headcount ratio
Kayonza	Θ_{11}	−0.0799	–[a]
	Θ_{21}	−0.497	
Bubare	Θ_{12}	−0.229	−6.087
	Θ_{22}	−0.646	−3.417
Bufundi	Θ_{13}	2.718***	−44.73***
	Θ_{23}	2.301**	−42.06**
Chahi	Θ_{14}	3.486***	−30.21**
	Θ_{24}	3.069***	−27.54*
Gataraga	Θ_{15}	−0.250	−20.51
	Θ_{25}	−0.667	−17.85
Remera	Θ_{16}	3.784***	9.299
	Θ_{26}	3.367**	11.97
Rwerere	Θ_{17}	1.533	−9.552
	Θ_{27}	1.116	−6.883
Mudende	Θ_{18}	−0.117	−38.35***
	Θ_{28}	−0.534	−35.68**
Kituva	Θ_{19}	3.747**	−29.13**
	Θ_{29}	3.330**	−26.46*
Bweremana	Θ_{110}	−0.207	8.425
	Θ_{210}	−0.624	11.09
Rubare	Θ_{111}	−5.841***	39.21***
	Θ_{211}	−6.258***	41.88***
Rumangabo	Θ_{112}	−4.402***	11.59
	Θ_{212}	−4.819***	14.25

[a] Not available, because baseline headcount ratio data from Kayonza IP are missing.

of affected households a lot. In other words, a dramatic reduction in the poverty headcount should not be confused with a dramatic increase in income.

Of course we are interested in exploring the determinants of IP performance. However, we lack the data to analyse this in any level of detail (we have only 12 observations at the IP level), and believe this question is best addressed at the programme level – pulling together data from the three sites (36 IPs in total). A look at our data, however, suggests IP performance may vary with certain key baseline community characteristics.[10] For example, IP performance varies with a few proxies of social capital. We find robust (partial) correlations between IP success and whether community members make voluntary financial contributions to support community activities or to remedy communal problems. Hence, pre-existing levels of social capital may be a factor explaining the success or failure of IPs.

10 To circumvent reverse causality concerns, we use pre-IP intervention baseline measures of community characteristics in this analysis.

Table 7. Heterogeneous treatment effects

	Dependent variables	
Estimated impact	FCS	Poverty
IAR4D – Control	1.428	−28.61
IAR4D – Conventional	0.956	−25.88
IAR4D × Duration	−0.0360	0.492
IAR4D – Control	0.757	−27.93**
IAR4D – Conventional	0.287	−24.45**
IAR4D × edu_secondary	−0.494	38.98
IAR4D × edu_univer	0.799	−14.69
IAR4D – Control	0.399	−28.37
IAR4D – Conventional	−0.0677	−25.86
IAR4D × borrow_formal	1.673	69.27
IAR4D × borrow_informal	0.230	2.272
IAR4D – Control	0.492	−20.33**
IAR4D – Conventional	0.0257	−18.02**
IAR4D × survtime	0.677	25.70*

While determining the exact mechanism linking innovations to poverty reduction is beyond the scope of the current paper, we emphasise this is an important area for follow-up work. Both the selection of innovations and the impact of adoption of specific innovations appear to be context-specific.

6.3 Heterogeneity across households

Next, we examine whether the impact of IAR4D is conditional on household characteristics – is a subset of villagers able to reap the benefits (if any), while others cannot? In Table 7, we provide estimation results for models (10-11). The two top rows for each group k give estimated values for δ_{31} and δ_{32}, respectively. The other rows present estimates of φ. It is clear that there is no evidence of heterogeneous impact. The impact of IAR4D does not vary with household agricultural experience, access to finance or education. That is, IAR4D benefits, if any, are shared within the community.

The only interaction term to enter significantly in Table 7 measures heterogeneity in time – this interaction term is the product of the IAR4D intervention and the survey time dummy (significant at the 10 per cent level). The interaction term suggests that IPs that have been in existence for 2 years outperform IPs that have been in existence for only 1 year. All successful IPs started in 2008 (but not all IPs starting in 2008 were successful). Specifically, the more established IPs have on average a reduction in poverty of 20 per cent and the immature IPs see the poverty rate go up by 5 per cent.

The latter result, however, should be taken with a pinch of salt, because the nature of our poverty data may not permit strong statements about tiny (short-term) effects. Nevertheless, we speculate that any negative start-up effects may capture the investment component of building an IP – there are significant short-term (opportunity) costs and medium-term benefits will only materialise after the IP is functioning. Such non-linearities in the response to intervention effort may imply that we underestimate the impact of IAR4D relative to the conventional policy (where intervention started earlier, so that initial investment costs have been borne before the experiment started).

7. Probing the mechanism: platforms and innovation

How does IAR4D lower poverty? As a first stab regress the adoption of our innovation indicators on the IP treatment by using linear probability models (i.e. we estimate equations (6) and (7) using innovation variables as dependent variables). We ask whether there are significant differences in terms of adoption between the three types of villages. Estimation results are given in Tables 8 and 9. We only report (differences in) coefficients of interest, but again these models were estimated with a full vector of controls.[11]

The innovation impact, as summarised in Tables 8 and 9, is less pronounced than the poverty impact summarised in Table 5. On average, IAR4D does not have a robust and significant positive impact on the adopting of these innovations. Instead, the DD and FD models suggest that IAR4D is associated with the dis-adoption of certain technologies, such as the probability of using animal manure or the use of certain post-harvest technologies (drying). A similar picture emerges with respect to other innovation proxies, related with regulation, marketing strategies and village resources. According to Table 9, IAR4D does not have a significant positive impact on the average probability of adoption.

However, these results should not be surprising, and do not discredit the innovation systems hypothesis. For instance, Pamuk *et al.* (2014) analyse the impact of IAR4D on technology adoption for all PLSs (i.e. not just the Lake Kivu PLS analysed in this paper), and show that priorities vary across IPs. Indeed, the lack of significant ATEs in terms of adoption of specific innovations is the natural outcome given that priority setting is decentralised. Since each IP decides on its own priorities, reflecting local preferences, opportunities and constraints, each IP should settle on its own ‘innovations’ and ATEs are difficult to detect.

For this reason, we also tested for heterogeneity in terms of the types of innovations that are adopted. This implies estimating equations (10) and (11) and using our innovation indicators as dependent variables. Detailed regression results are many, and are not shown here to economise on space (but they are available on request). Summarising the main insights, and consistent with results by Pamuk *et al.* (2014), adoption priorities vary from one IP to another. This is true both for the technical as well as the governance-related innovations. For example, in Bubare conservation farming has significantly

11 The adoption of many technologies is frequently undertaken simultaneously (e.g. Dorfman, 1996). This aspect of technology adoption is ignored in the modelling.

Table 8. Estimated impact of intervention on agricultural technologies

Dependent variables	DD		FD	
	Interv – Control $(-\delta_1)$	*Interv – Conv* $(-\delta_2)$	*Interv – Control* $(-\delta_1)$	*Interv – Conv* $(-\delta_2)$
Mulching	−0.0528 (0.0472)	0.00842 (0.0465)	−0.0310 (0.0476)	−0.00697 (0.0461)
Trenches/terraces	−0.0186 (0.0443)	0.0459 (0.0446)	0.00162 (0.0444)	0.0740* (0.0431)
Water harvesting	−0.0107 (0.0400)	−0.0280 (0.0375)	−0.0209 (0.0461)	−0.0416 (0.0421)
Irrigation	−0.0229 (0.0284)	−0.0163 (0.0286)	−0.0242 (0.0306)	−0.0162 (0.0300)
Conservation farming	−0.00353 (0.0537)	−0.0169 (0.0478)	−0.0195 (0.0622)	−0.00684 (0.0523)
Animal manure	−0.103** (0.0472)	−0.0778* (0.0426)	−0.117** (0.0523)	−0.0831* (0.0470)
Cover crops	0.0195 (0.0432)	0.00778 (0.0428)	−0.00680 (0.0474)	−0.00445 (0.0472)
Crop rotation	−0.0474 (0.0430)	0.0361 (0.0385)	−0.0462 (0.0444)	0.0388 (0.0407)
Inter cropping	−0.0245 (0.0532)	−0.0615 (0.0492)	−0.0161 (0.0567)	−0.0504 (0.0510)
Rhizobiainoculation	−0.0150 (0.0155)	−0.0113 (0.0150)	−0.0254 (0.0173)	0.000283 (0.0150)
Chemical fertiliser	0.00326 (0.0298)	0.00767 (0.0301)	−0.00561 (0.0305)	0.0189 (0.0318)
Row planting	0.0348 (0.0458)	0.0208 (0.0406)	0.0562 (0.0488)	0.0385 (0.0443)
Plant spacing	−0.0281 (0.0540)	−0.0160 (0.0529)	−0.0166 (0.0580)	0.00418 (0.0546)
Organic pesticide	−0.0355 (0.0322)	0.00329 (0.0334)	−0.0162 (0.0325)	0.0143 (0.0339)
Inorganic pesticide	0.0264 (0.0352)	0.0177 (0.0387)	0.0383 (0.0365)	0.0172 (0.0416)
Drying	−0.108** (0.0537)	−0.0790 (0.0483)	−0.0895* (0.0533)	−0.0718 (0.0478)
Threshing/shelling equipment	−0.0110 (0.0540)	0.0559 (0.0500)	0.00296 (0.0528)	0.0802 (0.0504)
Improved storage facilities	−0.00540 (0.0486)	−0.00798 (0.0423)	0.0167 (0.0497)	0.0131 (0.0431)
Pest control	0.0683 (0.0603)	0.0209 (0.0546)	0.0731 (0.0646)	0.0362 (0.0594)
Grading	−0.0225 (0.0611)	−0.00513 (0.0588)	−0.0142 (0.0641)	−0.00868 (0.0615)

Note: Estimates from the regressions indicated in the text are given. In all regression, controls listed in Table 2 are used during the estimation. Robust standard errors are in parentheses.
$^{*}p < 0.1$.
$^{**}p < 0.05$.
$^{***}p < 0.01$.

Table 9. Estimated impact of intervention on the probability of land regulations and marketing strategies and village resources

	DD		FD	
Dependent variables	*Interv – Control* ($-\delta_1$)	*Interv – Conv* ($-\delta_2$)	*Interv – Control* ($-\delta_1$)	*Interv – Conv* ($-\delta_2$)
Land regulations				
nrmbylaws	−0.131 (0.0889)	−0.107 (0.0874)	0.0108 (0.0708)	−0.000929 (0.0712)
landlaws	−0.0959 (0.0869)	−0.127 (0.0933)	0.0511 (0.0796)	−0.0213 (0.0798)
Marketing strategies				
notsold	−0.0674 (0.0602)	−0.0585 (0.0601)	−0.0575 (0.0612)	−0.0685 (0.0629)
consumers	0.0384 (0.0339)	0.0611 (0.0378)	−0.00995 (0.0374)	0.0547 (0.0380)
middleman	−0.0274 (0.0351)	−0.00650 (0.0350)	−0.0389 (0.0365)	−0.0160 (0.0362)
on road side	−0.0205 (0.0297)	−0.00927 (0.0288)	−0.0102 (0.0332)	−0.0279 (0.0301)
local market	−0.00508 (0.0548)	0.00334 (0.0517)	−0.0104 (0.0571)	0.0188 (0.0544)
district town	0.00468 (0.0203)	−0.0125 (0.0203)	0.00439 (0.0217)	−0.0121 (0.0224)
distant market	−0.0477* (0.0252)	−0.0366 (0.0267)	−0.0449 (0.0293)	−0.0274 (0.0313)
sold	−0.0376 (0.0374)	−0.0471 (0.0356)	−0.0640* (0.0377)	−0.0567 (0.0354)
Village resources				
wells	0.0422 (0.0881)	0.0244 (0.0843)	0.0393 (0.0962)	0.0160 (0.0884)
veterinary	−0.00523 (0.0749)	−0.0640 (0.0656)	−0.0285 (0.0764)	−0.0969 (0.0690)
woodlots	0.0724 (0.108)	−0.0374 (0.109)	0.108 (0.107)	−0.0371 (0.110)
waterbody	−0.0435 (0.115)	−0.00289 (0.115)	−0.0683 (0.125)	−0.0776 (0.131)
wateringpoint	−0.232** (0.0901)	−0.131 (0.0863)	−0.232** (0.0945)	−0.138 (0.0888)
agriresearch	0.0928 (0.0594)	0.0211 (0.0598)	0.0203 (0.0615)	−0.0471 (0.0595)

Note: Estimates from the regressions indicated in the text are given. In all regression, controls listed in Table 2 are used during the estimation. Robust standard errors are in parentheses.
*$p < 0.1$.
**$p < 0.05$.
***$p < 0.01$.

increased while plant spacing and organic pesticide usage decreased. In Bweremana mulching and row planting usage increased, and manure use decreased. Similarly, in some IPs market integration has gone up, whereas in others it went down. There does not appear to be a systematic pattern in terms of innovations adopted by IPs.

We can focus on the choices made by the four IPs most successful from a poverty alleviation perspective: Bufindi, Chahi, Mudende and Kituva. For these IPs, adopted innovations appear predominantly institutional in nature. Given the short-time frame after the intervention (2 years), this is perhaps not unexpected. Insofar as it is easier to change institutions and governance arrangements than to pioneer with technical innovations and upscale their use, we might expect institutional reform to have impact relatively quickly. Key innovations in successful IPs are enhanced market access (Chahi, Kituva), adoption of new land regulations (Bufindi) and improved access to village resources (Mudende). Occasionally these governance innovations were complemented with technical innovations (e.g. post-harvest pest control in Chahi, and mulching, irrigation, inter cropping, row planting, organic pest and post-harvest management in Kituva).

While determining the exact mechanism linking innovations to poverty reduction is beyond the scope of the current paper, we emphasise that this is an important area for follow-up work. Both the selection of innovations and the impact of adoption of specific innovations appear to be context-specific. For example, while successful IPs have focused on developing marketing strategies, changing access to land and the application of specific technologies, our dataset also provides counter examples to these success stories. Bweremana adopted the same technologies as Kituva, but in Bweremana this did not result in a lower poverty rate. The match between local conditions and innovations determines the success of an IP, but this will have to be explored more carefully (perhaps using qualitative methods).

8. Conclusions and discussion

Conventional extension efforts have by and large failed to generate the widespread adoption of innovations that are considered necessary to advance the agricultural development agenda. In response, the search is on for alternative mechanisms that foster innovation, adoption and diffusion and alleviate poverty. We report short-term evidence on the effectiveness of one such initiative – decentralised and participatory innovation systems. As part of a large experiment, so-called IPs have been introduced in a sample of selected villages. The performance of these villages, in terms of poverty reduction, is compared with the performance of two different counterfactual groups; control villages and villages benefiting from traditional extension approaches. Even though the period between baseline and follow-up survey was short, extending to not >2 years, surprisingly we are able to document some impact of IPs on poverty rates.

Our main conclusions are fourfold. First, we show positive ATEs of the innovation system intervention. On average, IPs reduce poverty. Second, the participatory approach appears more effective than traditional extension efforts in alleviating poverty. Third, the positive impact of the intervention is not limited to local elites. Instead, the impact does not vary (much) with household characteristics. Fourth, and reflecting the decentralised nature of the innovation systems approach, different platforms prioritise different types of innovations. We speculate that this diversity reflects variation in local opportunities and constraints. Next steps in our research agenda on innovation systems are (i) to analyse the mechanism linking IPs to poverty reduction (the adoption of specific innovations – see Pamuk *et al.* (2014), who document that the participatory model promotes the adoption of crop management innovations, but find no significant effects for other types of innovation) and (ii) to systematically compare the costs and benefits of IAR4D and alternative approaches to innovation and diffusion.

Two caveats should be mentioned. First, we did not implement an RCT where villages are randomly assigned to either the IAR4D treatment, or to one of the two counterfactual groups. We aim to control for potential selection bias by estimating DD models and panel models, but cannot completely rule out that some estimation bias eventuates due to unobservable and time-varying factors. Second, we obtain the most interesting results for our poverty data, which are not based on detailed household measurements but reflect the outcome of focus group discussions of local village leaders. The reduction in poverty for some villages is dramatic, and is not consistently matched by improvements in our measure of food consumption. This could point to an interesting empirical puzzle, inviting follow-up analysis or could point to mis-measurement of local poverty rates. Perhaps the focus group approach to data collection did not produce precise measures of local poverty, or perhaps it resulted in biased assessments. For example, the enhanced social interaction associated with the IP treatment could affect the outcomes of the poverty assessment (as both are inherently social processes). Alternatively, 'local leaders' may have strategic reasons to misrepresent local poverty rates. However, it is not evident (to us) whether they should over- or under-represent such rates. If they do not want to disappoint the researchers, village leaders in IAR4D villages may under-represent poverty rates during the endline. But if the aim is to attract additional funding and projects, then perhaps poverty rates are over-estimated (sending a signal of urgency). Hence, and also in light of the observation that poverty rates were not balanced during the baseline, we emphasise the importance of efforts to verify our findings in other contexts, perhaps using alternative proxies for poverty and (food) consumption.[12]

Notwithstanding these important caveats, the evidence suggests that decentralised innovation systems, based on participation of a wide range of stakeholders,

12 Note that our results may also be explained by non-random selection of IP sites for additional interventions. However, there is no evidence of 'other interventions' systematically benefitting the villages selected for IAR4D. We have not kept track of all 'other interventions' in comparison areas, so we cannot rule out that another intervention targeted non-intervention sites and had *negative* impacts on poverty alleviation there (explaining the positive estimated impacts in our study). However, we believe this to be unlikely.

may represent a promising vehicle to promote agricultural development. It provides tentative support for the recent transition to 'new demand-led approaches to extension' identified by the World Bank (World Bank, 2007). However, other considerations are relevant and should be mentioned here. First, while the IP approach on average generates positive impacts, there are also platforms that apparently have failed to generate any short-term benefits. It is clearly a first-order priority to analyse and explain the variation in performance. Does short-term success depend on the nature of the platform implementation process – anecdotal evidence suggests that there has been variation in the way that these platforms have been initiated and governed – or does it depend on characteristics of the affected communities (e.g. pre-existing levels of social capital)? Or is it simply true that in some platforms a consensus was reached to focus on innovations that pay off in the longer term, so that lack of a short-term effect is not indicative of platform failure at all? Follow-up research is needed to analyse this issue. Our preliminary analysis of the data suggests that the nature of the innovations selected and adopted varies across IPs – as is to be expected with a decentralised innovation approach.

Second, decentralising priority setting in the domain of innovation involves a trade-off. While decentralised approaches, such as IAR4D, allow tapping into pools of local knowledge and understanding, it might imply foregoing potential economies of scale in R&D. A particularly bad outcome – not one that is consistent with our data – would be where many platforms are inventing the same wheel. Moreover, a decentralised approach might induce a focus on bottlenecks that can be addressed locally. The macro-perspective, involving large-scale investments in physical infrastructure or national, sectoral or trade policies, might be overlooked. It appears important to give more space and attention to the use of the decentralised approach to innovation, while engaging policymakers of the 'right level' as well. The challenge will be to find the right balance between centralised and decentralised efforts to get African agriculture going.

Acknowledgements

We thank N.W.O. for financial support (N.W.O. grant # 453-10-001).

Conflict of Interest

A.A. works of FARA, the organisation responsible for coordinating the implementation of the SSA CP.

References

Angrist, J. D. and Pischke, J-S. (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton, NJ: Princeton University Press.

Bandiera, O. and Rasul, I. (2006). Social networks and technology adoption in northern Mozambique. *Economic Journal* 116(514): 869–902.

Banerjee, A. and Duflo, E. (2011). *Poor Economics: A Radical Rethinking of the Way to Fight Global Poverty*. New York: Public Affairs.

Barrett, C. B., Bachke, M. E., Bellemare, M. F., Michelson, H. C., Narayanan, S. and Walker, T. F. (2012). Smallholder participation in contract farming: comparative evidence from five countries. *World Development* 40(4): 715–730.

Christiaensen, L., Demery, L. and Kuhl, J. (2010). The (evolving) role of agriculture in poverty reduction: an empirical perspective. UNU-WIDER Working Paper # 2010/36.

Conley, T. and Udry, C. R. (2010). Learning about a new technology: pineapple in Ghana, American. *Economic Review* 100(1): 35–69.

Deaton, A. and Dreze, J. (2009). Food and nutrition in India: facts and interpretations. *Economic and Political Weekly* 19(7): 42–64.

Djurfeld, G., Holmen, H., Jirstrom, M. and Larsson, R. (2006). *The African Food Crisis: Lessons from the Asian Green Revolution*. Wallingford: CABI Publishing.

Dorfman, J. H. (1996). Modeling multiple adoption decisions in a joint framework. *American Journal of Agricultural Economics* 78(3): 547–557.

Dorward, A., Kirsten, J., Omamo, S., Poulton, C. and Vink, N. (2009). Institutions and the agricultural development challenge in Africa. In J. Kirsten, A. Doward, C. Poulton and N. Vink (eds), *Institutional Perspectives on African Agricultural Development*. Washington, DC: IFPRI.

Duflo, E., Kremer, M. and Robinson, J. (2008). How high are rates of return to fertilizer? Evidence from field experiments in Kenya. *American Economic Review* 98(2): 482–488.

FARA. (2008). *Sub-Saharan Africa Challenge Program (SSA CP) – Medium Term Plan 2009–2010*. Accra: Forum for Agricultural Research in Africa (FARA).

Feder, G., Just, R. and Zilberman, D. (1985). Adoption of agricultural innovations in developing countries: a survey. *Economic Development and Cultural Change* 33: 255–298.

Geels, F. W. (2004). From sectoral systems of innovation to socio-technical systems: insights about dynamics and change from sociology and institutional theory. *Research Policy* 33(6): 897–920.

Haggblade, S., Hazell, P. T. and Dorosh, P. (2007). Sectoral growth linkages between agriculture and the rural nonfarm economy. In S. Haggblade, P. Hazell and T. Reardon (eds), *Transforming the Rural Nonfarm Economy: Opportunities and Threats in the Developing World*. Baltimore: The Johns Hopkins University Press.

Holmen, H. (2005). The state and agricultural intensification in Sub-Saharan Africa. In G. Djurfeldt, H. Holmen, M. Jirstrom and R. Larsson (eds), *The African Food Crisis: Lessons from the Asian Green Revolution*. Oxfordshire, UK: CABI.

IFPRI. (2010). *Policies, Institutions and Markets to Strengthen Assets and Agricultural Incomes for the Poor*. Washington, DC: IFPRI.

Imbens, G. W. and Wooldridge, J. M. (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature* 47(1): 5–8.

Knickel, K., Brunori, G., Rand, S. and Proost, J. (2009). Towards a better conceptual framework for innovation processes in agriculture and rural development: from linear models to systemic approaches. *The Journal of Agricultural Education and Extension* 15(2): 131–146.

Leeuwis, C. and van den Ban, A. (2004). *Communication for Rural Innovation: Rethinking Agricultural Extension*. Oxford: Blackwell Science.

Ligon, E. and Sadoulet, E. (2007). Estimating the effects of aggregate agricultural growth on the distribution of expenditures. Background paper for the World Development Report 2008. Washington DC: World Bank.

Liu, E. (2012). Time to change what to sow: Risk preferences and technology adoption decisions of cotton farmers in China. *The Review of Economics and Statistics* 95(4): 1386–1403.

Mapila, M., Kirsten, J. and Meyer, F. (2011). *Agricultural rural innovation and improved livelihood outcomes in Africa*. http://www.csae.ox.ac.uk/conferences/2011-EDiA/papers/017-Mapila.pdf.

van Mierlo, B., Leeuwis, C., Smits, R. and Woolthuis, R. (2010). Learning towards system innovation: evaluating a systemic instrument. *Technological Forecasting and Social Change* 77(2): 318–334.

Pamuk, H., Bulte, E. and Adekunle, A. (2014). Do decentralized innovation systems promote agricultural technology adoption? Experimental evidence from Africa. *Food Policy* 44: 227–236.

van der Ploeg, J. D., Bouma, J., Rip, A., Rijkenberg, F., Ventura, F. and Wiskerke, J. (2004). On regimes, novelties and co-production. In J. Wiskerke and J. D. van der Ploeg (eds), *Seeds of Transition*. Assen: Van Gorcum, 1–30.

Rogers, E. (1995). *Diffusion of Innovations*. New York: Free Press.

Scott, J. (1989). *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed*. New Haven: Yale University Press.

Steckel, R. and White, W. J. (2012). Engines of Growth: Farm Tractors and Twentieth Century US Economic Welfare. Massachusetts: NBER Working Paper Series # 17879.

Sunding, D. and Zilberman, D. (2001). The agricultural innovation process: research and technology adoption in a changing agricultural sector. In B. Gardner and G. Rausser (eds), *Handbook of Agricultural Economics, Vol. 1*. Amsterdam: North Holland.

Suri, T. (2011). Selection and comparative advantage in technology adoption. *Econometrica* 79(1): 159–209.

United Nations. (2008). *Food Consumption Analysis, Calculation and Use of the Food Consumption Score in Food Security Analysis*. Rome: World Food Programme Vulnerability Analysis and Mapping Branch.

World Bank. (2007). *World Development Report 2008: Agriculture for Development*. Washington, DC: World Bank.