

Subsidies and the African Green Revolution: Direct Effects and Social Network Spillovers of Randomized Input Subsidies in Mozambique[†]

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The Green Revolution, which bolstered agricultural yields and economic well-being in Asia and Latin America beginning in the 1960s, largely bypassed sub-Saharan Africa. We study the first randomized controlled trial of a government-implemented input subsidy program (ISP) in Africa intended to foment a Green Revolution. We find that this temporary subsidy for Mozambican maize farmers stimulates Green Revolution technology adoption and leads to increased maize yields. Effects of the subsidy persist in later unsubsidized years. In addition, social networks of subsidized farmers benefit from spillovers, experiencing increases in technology adoption, yields, and beliefs about the returns to the technologies. Spillovers account for the vast majority of subsidy-induced gains. ISPs alleviate informational market failures, stimulating learning about new technologies by subsidy recipients and their social networks. (JEL O13, Q12, Q16, Q18)

The “Green Revolution” reshaped agriculture across Asia and South America during the last four decades of the twentieth century. Adoption of high-yielding seeds and chemical fertilizers led to a four-fold increase yields in Asia and South America. In contrast, sub-Saharan Africa saw no such yield increase. The solid (black) line in Figure 1 displays the dramatic yield divergence in the form of the gap between cereal yields in sub-Saharan Africa versus Asia and South America. In 1960—the first year for which such data are available—the gap between sub-Saharan

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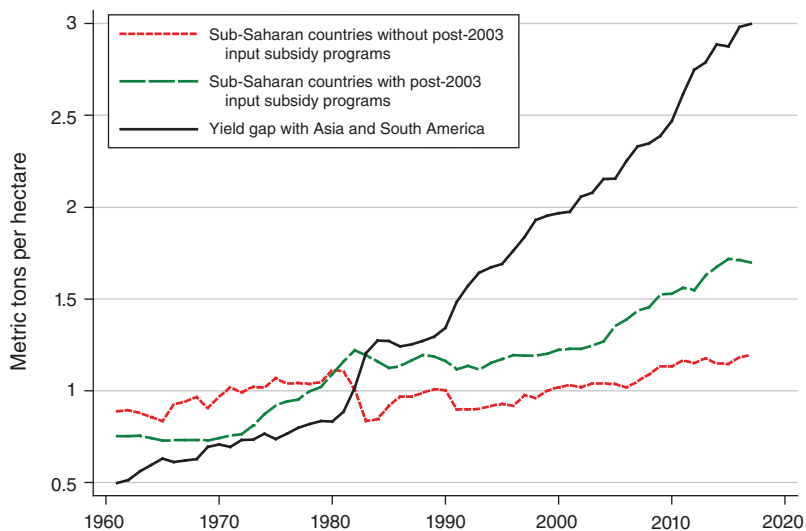


FIGURE 1. CEREAL YIELDS IN SUB-SAHARAN AFRICA

Notes: Data are three-year moving average cereal yields, weighted by agricultural land share, from the FAO Statistical Database (FAOSTAT, <http://www.fao.org/faostat/en/#data/QC>). The ten countries that implemented input subsidy programs after the 2003 Maputo Declaration are Mali, Burkina Faso, Ghana, Senegal, Nigeria, Kenya, Malawi, Tanzania, Zambia, and Ethiopia. Yield gap is the three-year moving average of the difference in land-share-weighted average cereal yields between Asia and South America on the one hand, and sub-Saharan Africa on the other. Data span 1961 to 2017 inclusive.

Africa and these other developing regions was less than a half ton per hectare. By the early 1990s, this gap had tripled to 1.5 tons/hectare, and had doubled again to almost 3 tons/hectare by 2017. Over most of this time period in which the yield gap exploded, cereal yields in sub-Saharan Africa stagnated at around one ton per hectare.¹ The modest growth in total agricultural production that did occur in sub-Saharan Africa over that time was driven by land expansion, with little technological change or yield growth (Evenson and Gollin 2003).

While the Green Revolution allowed the rest of the developing world to take off economically (Gollin, Hansen, and Wingender 2018), sub-Saharan Africa has since become the repository of an ever-larger share of the world's severely poor people. Twenty-five years ago, 17 percent of the world's absolutely poor lived in sub-Saharan Africa. Since that time, that figure has risen to 51 percent (World Bank 2018).

In response to this "Green Revolution that wasn't," African nations signed the Maputo Declaration in 2003, pledging to invest 10 percent of their national budgets in agriculture to achieve a 6 percent rate of annual agricultural growth. The aptly named Alliance for a Green Revolution in Africa (AGRA) was launched in 2006, led by former UN Secretary-General Kofi Annan. To promote the adoption of the Green

¹ While these data are the best available for most African countries, and are regularly deployed in discussions about agriculture in that region, they are probably best treated as indicative numbers rather than estimates based on a uniform and replicable statistical methodology.

Revolution technologies that drove the economic takeoff of other regions, a number of countries recently introduced (or revived) input subsidy programs (ISPs), which provide Green Revolution technologies (mainly fertilizer and improved seeds) at below-market prices.² These newer or second-generation input subsidy programs were intended to be “smart” in the sense that they were to (i) complement development of private input markets; (ii) target beneficiaries with high potential gains from input adoption; and (iii) be temporary rather than permanent (Morris et al. 2007).

Despite these three principles, these newer input subsidy programs have in almost all cases become permanent fixtures, and they annually consume substantial public resources across the continent. In the years since the Maputo Declaration, ten countries have implemented input subsidy programs. These ten countries have been devoting 20–25 percent of public spending on agriculture to such programs, amounting in total to nearly US\$1 billion annually (Jayne et al. 2018).

Input subsidy programs do appear to coincide with higher growth in aggregate yields in sub-Saharan Africa. In Figure 1, we distinguish between yields in the ten countries that have implemented input subsidy programs since the 2003 Maputo Declaration (the dashed green line), and yields in all other sub-Saharan African countries (the dotted red line).³ While prior to 2000, the former group of countries already had modestly higher cereal yields (amounting to about 0.5 tons per hectare from 1980 to 2000), ISP countries have thereafter experienced higher yield growth over the last 20 years: by 2017 the yield gap between ISP and non-ISP countries had widened to 1.1 tons per hectare.

This simple comparison of yield growth in ISP and non-ISP countries does not prove that ISPs caused yields to increase in ISP countries. Governments could have implemented ISPs alongside other policies that also increased yields, or that were in fact the true underlying cause of yield gains. Even if ISPs have positive causal impacts, these aggregate trends provide little insight into how the gains occurred. Are impacts concentrated among subsidized farmers themselves, or are there also spillovers to unsubsidized farmers? In addition, in all ten countries with large-scale ISPs, subsidies have become permanent fixtures of government policy, making it impossible to determine if subsidy impacts would persist if the subsidies were removed. What’s more, to determine whether ISPs yield societal gains, the value of any increases in cereal yields need to be weighed against the costs of ISP programs.

Against this backdrop, this paper asks if input subsidy programs have positive effects, if their effects persist once subsidies are removed, and if they are economically good policy. Existing household-level observational studies present mixed results concerning the impact of ISPs on the adoption of improved technologies, agricultural output, and household well-being. These studies have difficulties establishing causal impacts, as none of them employ a randomized controlled trial (RCT) methodology. In this paper, we study a randomized controlled trial of a temporary, one-off input subsidy program in Mozambique. We not only estimate impacts

²Input subsidy programs were widespread in sub-Saharan Africa in prior decades, mainly involving input distribution by state-owned enterprises. These earlier ISPs were discontinued in the context of 1980s and 1990s structural adjustment programs.

³Over the time period covered in this figure, the share of cultivated cereal area in sub-Saharan Africa has been roughly equal across the two groups of countries.

of subsidies on randomly selected farmers, but also ask whether impacts persist post-subsidy, and measure impacts on social networks of subsidy beneficiaries. In addition, the overall patterns of our results shed light on the particular market failures that ISPs help address.

The questions we explore are key to understanding the full societal benefits of ISPs, and for optimal policy design. It is important to understand persistence of impacts beyond the subsidized period, to fully assess the benefits of temporary programs. Relatedly, estimates of spillover impacts to social networks allow a full accounting of societal gains. Note that spillover impacts on those who learn by waiting and watching others will by definition occur with a time lag and require a longer-term study to detect.

In addition, an understanding of market failures helps identify optimal policy responses. For some market failures, subsidies are not the obvious remedy. For example, if farmers cannot finance the technology or bear the additional risk technology adoption implies, then policy should facilitate markets for financial services (e.g., credit, insurance, or savings) rather than providing subsidies (Karlan et al. 2014). By contrast, informational market failures (say, imperfect information on the returns to the technology) may call for subsidies that overcome individuals' reluctance to learn-by-doing. Because information is nonrival, it may readily spill over to social network connections of subsidy recipients, raising others' adoption and magnifying societal gains from the subsidy (Foster and Rosenzweig 1995). Information market failures may only justify temporary subsidies that can be removed once induced adopters learn about the technology. On the other hand, if persistent behavioral biases such as present bias inhibit adoption, permanent interventions may be needed (Duflo, Kremer, and Robinson 2011).

We study the Farmer Support Program (*Programa de Suporte aos Produtores*), a European Union-funded program for Mozambican farmers managed by the UN's Food and Agriculture Organization (FAO) and implemented by Mozambique's Ministry of Agriculture with technical advice from the International Fertilizer Development Center (IFDC). The program is representative of ISPs that have recently spread in sub-Saharan Africa (except that it ended after one season). Our results therefore have direct bearing on understanding the impact of ISPs in the region more generally.

We find that this temporary subsidy for Mozambican maize farmers promotes Green Revolution technology adoption and increases maize yields. While the subsidy was provided for just a single input package in one agricultural season, effects of the subsidy persist in later unsubsidized years. Magnitudes of impacts are substantial but comfortably within the range of the potential yield impacts of Green Revolution technologies. We also observe spillovers from subsidized farmers to their social networks: agricultural contacts of subsidized farmers see increases in technology adoption and yields. Both subsidized farmers and their social networks report higher beliefs about expected returns to the technologies. We interpret these results as revealing that ISPs help to reduce market failures related to information, stimulating learning about new technologies by subsidy recipients and their social networks.

To illustrate the importance of considering spillovers to other farmers, as well as persistence of impacts beyond the subsidized period, we analyze how benefit-cost

ratios of the subsidy program change when one does or does not consider spillovers and post-subsidy persistence. We make conservative assumptions regarding the implementation cost of the subsidy program. We also conservatively consider only persistence of impacts in the two observed post-subsidy periods (rather than assuming the impacts will continue into future periods), and we also ignore possible spillovers to other farmers not in our study sample. Our observed impacts are on maize yields rather than profits, so we make further assumptions to calculate farmer net benefits (profits).⁴ We calculate a modest benefit-cost ratio (1.8) when considering only the gains for subsidized farmers in the year of the subsidy. But when fully accounting for the spillover and post-subsidy effects, the benefit-cost ratio is magnified ten times, to 19.8. Seventy percent of benefits occur through spillovers, and 74 percent of benefits occur in the years after the subsidy ended.

Our main contribution is to provide the first causal estimates based on a randomized controlled trial of an input subsidy program in Africa. In addition, we contribute a new combination of findings to the literature on technology adoption in developing countries. Ours is the first paper to show that a temporary subsidy for agricultural production technologies has lasting impacts on adoption after the subsidy ends. In nonagricultural technology contexts, persistent impacts of a temporary technology subsidy on adoption have been found for health goods (Dupas 2014) and for labor migration (Bryan, Chowdhury, and Mobarak 2014). One prior nonexperimental study did not find that ISPs led to persistent adoption post-subsidy (Ricker-Gilbert and Jayne 2017). Recent randomized studies of Green Revolution agricultural inputs in Africa do not investigate post-subsidy persistence (Duflo, Kremer, and Robinson 2008; Beaman et al. 2013; Abate et al. 2018). Social learning about technologies has been documented in prior studies, using both observational approaches (e.g., Bandiera and Rasul 2006, Munshi 2004) and randomized study designs (e.g., Magnan et al. 2015, Laajaj and Macours 2016, Beaman and Dillon 2018, Beaman et al. 2018, and BenYishay and Mobarak 2019), but none of these have been in the context of input subsidy programs.

In addition, prior randomized studies showing social learning have been researcher-implemented interventions, not government-implemented programs. Muralidhran and Niehaus (2017) argue that more randomized evaluations of government-implemented programs are needed to enhance external validity of findings. Similar to the findings of Baird et al. (2016) in the context of a deworming program, we find that the benefit-cost ratio of the program increases substantially when spillovers are taken into account, highlighting the importance of incorporating long-term effects and spillover effects in program evaluation.

This paper is organized as follows. In Section I, we provide further detail about the subsidy program. In Section II, we describe the randomized research design and the study sample. Section III details the empirical regression specification. We present the empirical results in Section IV. In Section V, we provide a cost-effectiveness calculation. We provide concluding thoughts in Section VI. An online Appendix provides additional analyses and robustness tests, to which we refer throughout the main text.

⁴We take the value of increased maize output and subtract costs of fertilizer and improved seed inputs, as well as the estimated cost of increased labor time when using the new technologies.

I. Mozambique's Input Subsidy Program

Mozambique's Farmer Support Program provided one-time input subsidies to 25,000 smallholder farmers in five provinces. The program embodied key "smart" features considered ISP best practices: it supported development of input markets by providing the subsidies via vouchers to be redeemed at private agricultural dealers, it targeted farmers thought to have high potential gains from the inputs, and it provided only temporary subsidies. The technology package we study was designed for maize production.⁵ The subsidy provided a 73 percent discount on a package of chemical fertilizer (50 kg of urea and 50 kg of NPK 12-24-12) and improved maize seeds (12.5 kg of either a hybrid or an open-pollinated improved variety), valid for one use in the 2010–2011 agricultural season. The retail cost of the package was 3,163 MZN (US\$117). Subsidy users had to make the 27 percent copay of 863 MZN (US\$32) when redeeming the voucher at an agricultural dealer.

While the program design benefited from international technical advice, the Mozambican government had sole responsibility for implementation. The agricultural extension agency identified the beneficiary farmers and distributed the subsidy vouchers throughout the country, including for the farmers included in our study. The research team randomly assigned farmers to receipt of subsidy vouchers (as we describe below) but had no role in distributing or administering the vouchers. We therefore study impacts of an actual government-implemented program, rather than a potentially unrepresentative researcher-implemented intervention.

II. Research Design and Sample

In order to study the impacts of Mozambique's input subsidy program, we collaborated with the government to implement a randomized controlled trial among maize farmers in Manica Province in central Mozambique. This section details the randomization procedures, the sampling strategy and other aspects of the research design. Online Appendix A provides additional detail on the study context for the interested reader.

A. Randomization

We designed the randomized controlled trial of the input subsidy program in cooperation with the Ministry of Agriculture and IFDC. At the time of the study, one US dollar (USD) was worth roughly 27 Mozambican meticals (MZN). Government agricultural extension officers created lists of eligible farmers, with input from local leaders and agro-input retailers. Eligibility criteria were identical to those used for the maize-farming portion of the national subsidy program. Individuals were eligible for a voucher coupon if they met the following criteria:

- Farming between 0.5 hectare and 5 hectares of maize;

⁵Nationally, the program provided 15,000 subsidy vouchers for maize and 10,000 for rice production. Our study occurred in a maize-growing area.

- Being a “progressive farmer,” defined as being interested in modernization of production methods and in commercial farming;
- Having access to agricultural extension and to input and output markets; and
- Being able and willing to pay for the remaining 27 percent of the package cost.

Only one person per household was allowed to register. Extension officers informed participants that a lottery would be held and only half of those on the list would win a voucher. Vouchers were then randomly assigned to 50 percent of the households on the list in each locality. In other words, localities served as treatment stratification cells.

Randomization was conducted by the research team on the computer of one of the authors, and we then provided the list of voucher winners to agricultural extension officers. The treatment and control groups contain 247 and 267 farmers (and their households), respectively. Extension officers were responsible for voucher distribution to beneficiary farmers. Voucher distribution occurred at a meeting to which only farmers who won the lottery were invited. Random assignment and distribution of vouchers occurred from September to December 2010. Vouchers were intended to be used for inputs for the 2010–2011 season. The annual agricultural season in Mozambique runs from November (when planting starts) through end of the harvest period the following June. Vouchers expired on January 31, 2011, and this expiration date was strictly enforced. The vouchers were assigned to specific individuals, and the input retailer verified names when coupons were redeemed.

B. Sampling and Survey Data

Our sample consists of households of individuals who were included in the September–December 2010 subsidy voucher randomization (both voucher winners and losers), and who we were able to locate and survey in April 2011 (data available at Laajaj, Yang, and Carter 2019). Key research design decisions could only be made once the government had reached certain points in its implementation of the 2010 subsidy program. In particular, the government did not create the list of potential study participants in the study localities (among whom the voucher randomization took place) until very close to the actual date by which vouchers had to be distributed (at the start of the 2010–2011 planting season). It was therefore not feasible to conduct a baseline survey prior to the voucher randomization and distribution. Instead, we sought to locate individuals on the voucher randomization list (both winners and losers) in April 2011, and at that point we requested their consent to participate in the study. While this delay was not ideal, in Section IID we present evidence that it did not unbalance the treatment and control groups for the study.

In total, 704 individuals were included in the list for randomization of subsidy vouchers in 2010. Agricultural extension officials informed study participants that vouchers would be assigned via random lottery in study villages, announced lottery winners, and distributed vouchers accordingly. Of these 704 individuals, 514 (73.0 percent) were located and administered informed consent for study participation. Consenting individuals were then surveyed in what we refer to as the April 2011 “interim survey.” All surveys include treatment and control participant households.

Three additional follow-up surveys track outcomes in the subsidized 2010–2011 season and two annual agricultural seasons afterward, 2011–2012 and 2012–2013.

Surveys of study participants were conducted in person at their homes. Follow-up surveys were timed to occur after the May–June annual harvest period, to capture input use, production, and other outcomes in the agricultural season leading up to that harvest. These surveys provide the data on outcomes examined in this paper. In the April 2011 interim survey, we collected data on social network connections, asking each participant to identify others in the village with whom they discuss agriculture.⁶ Because the interim survey occurred after the subsidy treatment, to help ameliorate questions about selection bias or possible endogeneity of social networks, we asked study participants about their connections in the *prior* agricultural season (2009–2010), not the 2010–2011 subsidy season. As we discuss in more detail later, the idea behind this strategy was to ensure that the extent to which members of one’s social network received the subsidy was also random. The success of this strategy is further examined in Section IID below.

C. Summary Statistics

Table 1 presents key summary statistics. Farmers’ experience with chemical fertilizer prior to the study is quite limited. We asked farmers how many years they used fertilizer out of the last ten years prior to the beginning of the study (panel B). Sixty-seven percent of farmers reported zero years, and 87 percent reported two years or less. Based on these reports about prior use of the technologies, there appears to be room for learning about chemical fertilizer, and perhaps less about improved seeds.

Farmers use 23.9 kg of fertilizer and 19.0 kg of improved maize seeds on average. Twenty-three percent of farmers use some positive amount of fertilizer, and 54 percent use positive amounts of improved seeds (not shown in table). Average (median) maize yields are 975 (600) kilograms/hectare, indicating a large yield gap relative to yield expectations from agronomic trials of three to four times that level. Median per-capita consumption (measured using a standard LSMS instrument) is just above the World Bank’s standard \$1.95/day poverty line. We can see in panel B of Table 1 that our network survey instrument registers substantial variation in the extent to which study respondents were socially connected to subsidy voucher winners, with 44 percent reporting no connections, and another 23 percent reporting three or more network members who received the voucher.

As is common in studies of real-world programs, we have imperfect compliance with treatment assignment. Only 40.8 percent of farmers in the treatment group used their vouchers. Most such noncompliance was due to inability to make the input package copayment (even though claimed ability to pay was a participant selection criterion). Moreover, 12.4 percent of control group farmers reported using subsidy vouchers for the input package, due to imperfect compliance by extension agents distributing vouchers (see online Appendix C).

⁶Following Conley and Udry’s (2010) elicitation of “information links,” study participants were presented with the full list of other study participants in the same village and were asked one by one whether they talked about agriculture with this person in the prior season.

TABLE 1—DESCRIPTIVE STATISTICS

	Mean	Standard deviation	Percentiles				
			5th	25th	Median	75th	95th
<i>Panel A. Continuous variables</i>							
Fertilizer used on maize (kg)	23.9	61	0	0	0	0	150
Improved maize seeds used (kg)	19	30	0	0	10	25	75
Maize yield (kg/ha)	975	1,168	104	300	600	1,169	3,305
Expected yield with technology package (kg/ha)	1,944	2,540	221	603	1,163	2,176	6,216
Daily consumption per capita (MZN/day/hh member)	77	51	29	45	63	92	172
	Mean	0	1	2	3	4	5 or more
<i>Panel B. Frequencies of count variables</i>							
Number of years using fertilizer out of ten years before the study	1.03	67%	13%	6.6%	3.6%	1.8%	7.5%
Number of social network contacts who are study participants	3.17	31%	16%	12%	7.2%	8.6%	25%
Number of social network contacts in treatment group	1.54	44%	18%	15%	8.2%	5.7%	8.8%

Notes: Data are from the 514 study participant households. In panel A, data are from three survey rounds: the 2010–2011 “during” season and the 2011–2012 and 2012–2013 “after” seasons. In panel B, data are from the April 2011 interim survey, where respondents reported retrospectively on fertilizer use in prior years, and social network contacts in the prior (pre-treatment, 2009–2010) season. Fertilizer, seed, maize yield, and expected yield are in kilograms. Daily consumption per capita is in Mozambican meticalis (27 MZN \approx 1 USD). Continuous variables are truncated at their ninety-ninth percentile prior to calculation of means and standard deviations.

Imperfect compliance with treatment assignment reduces statistical power, but does not threaten internal validity of estimates. The intervention is therefore an “encouragement design” that affects the probability of using a subsidy voucher. The difference in voucher use rates in the treatment and control groups (28.8 percentage points) is statistically significantly different from zero (p -value $<$ 0.001, online Appendix Table A4). This treatment-control difference in subsidy voucher use drives all treatment effect estimates.

D. Balance with Respect to Treatment

Given that study participant enrollment and collection of social network connections data occurred after treatment, it is important to ask whether there are any signs of selection bias. In particular, it is important to investigate whether treatment status affected inclusion in the sample, time-invariant characteristics of households, and reports of past social network connections. In addition, we investigate whether treatment affects attrition from the follow-up survey rounds, which could be another source of selection bias. In none of these analyses do we find that treatment had large or statistically significant effects. We now discuss these analyses in turn.

First, out of 704 individuals included in the list for randomization of subsidy vouchers in 2010, 514 (73.0 percent) were located, consented, and surveyed in April 2011. These households constitute our study sample. We find no statistically significant difference in success rates in the April 2011 interim survey by subsidy treatment status: interim survey success rates for treatment and control group members

were 71.0 percent and 75.0 percent, respectively (p -value of the difference is 0.246). Moreover, rates of survey refusal were low. Only 2.0 percent of the initial sample of 704 actually refused to be surveyed. In the vast majority of cases, the reason an individual could not be surveyed in April 2011 was poor quality of the farmer lists prepared by extension workers at the time the vouchers were randomized. Extension agents had a relatively short period of time to make the lists of farmers to include in the voucher randomization, and hence sometimes used preexisting lists of farmers without carefully checking whether farmers were still present in the local area. The vast majority of individuals in the initial list of 704 who were not successfully surveyed in April 2011 had moved away or died prior to voucher randomization, or were otherwise not known to local guides who assisted in finding farmers for the April 2011 interim survey.

Digging deeper, we can also look to see if the observable characteristics of the households successfully located and interviewed are balanced with respect to treatment status. We focus on four key household characteristics that can plausibly be considered nonmanipulable: education, gender, age, and literacy of household head. Education and age are measured in years. Gender is an indicator for the head being male, and literacy is an indicator for being literate. We discuss this analysis in online Appendix B, and we present results in online Appendix Table A1. Random assignment led to balance on these key time-invariant household characteristics with respect to both treatment status, as well as treatment status of one's social network.

A second area of potential concern is that the delay in our initial data collection may have skewed the information we were able to collect on social networks. As mentioned above, to ameliorate this concern, we asked respondents to report their social network connections in the prior season (2009–2010), before treatment. Despite this approach, we might still worry that respondents' reports of their social network connections may be affected by treatment. However, as detailed in online Appendix B, online Appendix Table A2, there is no evidence that reports of prior social network connections are related to treatment status.

Finally, given that our study was comprised of multiple survey rounds, we might worry about differential attrition with respect to treatment status. However, overall attrition was a modest 8.6 percent on average across rounds. More importantly, the extent of attrition was uncorrelated with treatment status, as well as with the treatment status of one's social network. Online Appendix C and online Appendix Table A3 give further detail on this attrition analysis.

III. Empirical Specification

Our study focuses on four key outcome variables: (i) use of Green Revolution technologies; (ii) agricultural yields; (iii) beliefs about returns to the new technologies; and (iv) household living standards (see online Appendix D for detailed variable definitions). We estimate "intent-to-treat" (ITT) effects, for the season in which the subsidy was offered, as well as for subsequent unsubsidized seasons. We also measure spillover effects to social network contacts of treated farmers, over

the same time periods. We estimate the following regression equation for outcome variable y_{ict} of farm household i in locality c in time period t :

$$(1) \quad y_{ict} = \alpha_{During} Treat_{ic} During_t + \alpha_{After} Treat_{ic} After_t \\ + \sigma_{During} SocialTreat_{ic} During_t + \sigma_{After} SocialTreat_{ic} After_t \\ + \mathbf{X}_{ict} \gamma + \theta_c + \varepsilon_{ict},$$

where $Treat_{ic}$ is a treatment group dummy variable that takes on the value of 1 if the farm household won the ISP voucher lottery; $During_t$ and $After_t$ are time dummy variables that respectively take on the value 1 if t is during the subsidy period (2010–2011) or after the subsidy period (2011–2012 and 2012–2013); and, $SocialTreat_{ic}$ is a dummy that takes the value 1 when the farm household has above the median level (two or more) social network members who won the ISP voucher lottery. We discuss below the rationale for this specification. Households with $SocialTreat_{ic} = 1$ have 3.63 treatment group contacts on average; for households with $SocialTreat_{ic} = 0$, the average is 0.29. All study participants (whether in control or treatment groups) are selected for the study in the same way, so that direct and spillover effects apply to the population of progressive farmers as defined by ISP eligibility rules.

The vector of controls \mathbf{X}_{ict} includes time dummies to capture levels for the untreated control group. It also includes indicator variables for having one, two, three, four, or five or more social network contacts who are study participants (omitted category zero), to control for effects of social network size. Social network size is not exogenously determined and is mechanically positively correlated with $SocialTreat_{ic}$ (households with more contacts will also have more treatment group contacts). When controlling for social network size, the regression coefficients on the terms including $SocialTreat_{ic}$ can be interpreted as the causal effect of having two or more social network contacts who were offered the input subsidy. Finally, the θ_c are locality fixed effects (treatment is randomized within locality) and ε_{ict} is a mean-zero error term. Standard errors are clustered by household.

The regression coefficients of interest are as follows. Direct effects of assignment to the treatment group (being randomly assigned the input subsidy voucher), during and after the subsidized season, are α_{During} and α_{After} , respectively. Spillover effects of being “more connected” to a treatment group member (having above-median treatment group contacts) during and after the subsidy are σ_{During} and σ_{After} , respectively. Note that this specification restricts the spillover effects to be the same for those who were voucher winners (treated) and those who were not.⁷ Also note that equation (1) estimates a common effect of treatment in the post-subsidy “after” period to maximize power (McKenzie 2012).⁸

⁷Section IIID estimates spillover effects separately for voucher and nonvoucher winners.

⁸Online Appendix E shows separate treatment effects for the 2011–2012 and 2012–2013 agricultural years.

IV. Empirical Results

This section presents intention to treat estimates for equation (1) above. We also examine alternative network specifications and test for alternative mechanisms that might underlie our findings.

A. Regression Results

Regression results are presented graphically in Figure 2, and regression coefficients are reported in online Appendix Table A5. Outcome variables in the regressions are expressed in logarithms.⁹ In the left-hand column of Figure 2, coefficients represent ITT effects of subsidy assignment on households in the treatment group, during and after the subsidized season (α_{During} and α_{After} , respectively). The right-hand column of Figure 1 presents σ_{During} and σ_{After} , spillover impacts on study participants “more connected” to the treatment group (with above-median treatment group contacts) during and after the subsidy, respectively. We will first discuss direct impacts before turning our attention to the spillover impacts.

The direct effect of the subsidies on the treatment group is an increase in technology adoption and maize yields (coefficients in the left-hand side of the figure). Direct effects during the subsidized period (α_{During}) are large and positive for adoption of fertilizer and improved seeds, as well as for maize yields. In the “after” period (α_{After}), treated households show some persistence in use of fertilizer, but not seeds, which can be due to the fact that improved seeds were more widely used and known than fertilizer before the program. Direct impacts on fertilizer use after the subsidy become smaller in magnitude, which is to be expected after the end of the subsidy, but they remain substantial and statistically significant at the 1 percent level. Direct impacts on yields remain almost as high in the period following the subsidy, which can be due to the farmers reusing the inputs when not subsidized, and also to persistence in the benefits of fertilizer used in the subsidized season though nutrients remaining in soils. Returns to fertilizer can also increase because of learning about how to use fertilizer, or a selection effect, where only farmers who observed high yields purchase the inputs after the subsidy period.

We also show impacts on living standards, measured by per capita daily consumption in the household. Consumption is useful to examine as a summary measure of household well-being. It is additionally useful because we do not have a measure of agricultural profits, which would require data on all agricultural inputs used (in particular, difficult-to-measure labor inputs). Examining impacts on consumption can therefore indirectly reveal whether agricultural profits rose. Direct impacts on the treatment group are close to zero in the “during” period but are large and positive in the “after” period. Spillover impacts are large and positive, with magnitudes that are relatively stable across periods. These results provide an indirect indication that unobserved agricultural profits did rise and benefit households.

⁹ Findings are robust to alternate dependent variable specifications, such as indicators, kilograms, or Mozambican meticals as shown in online Appendix E and online Appendix Table A6.

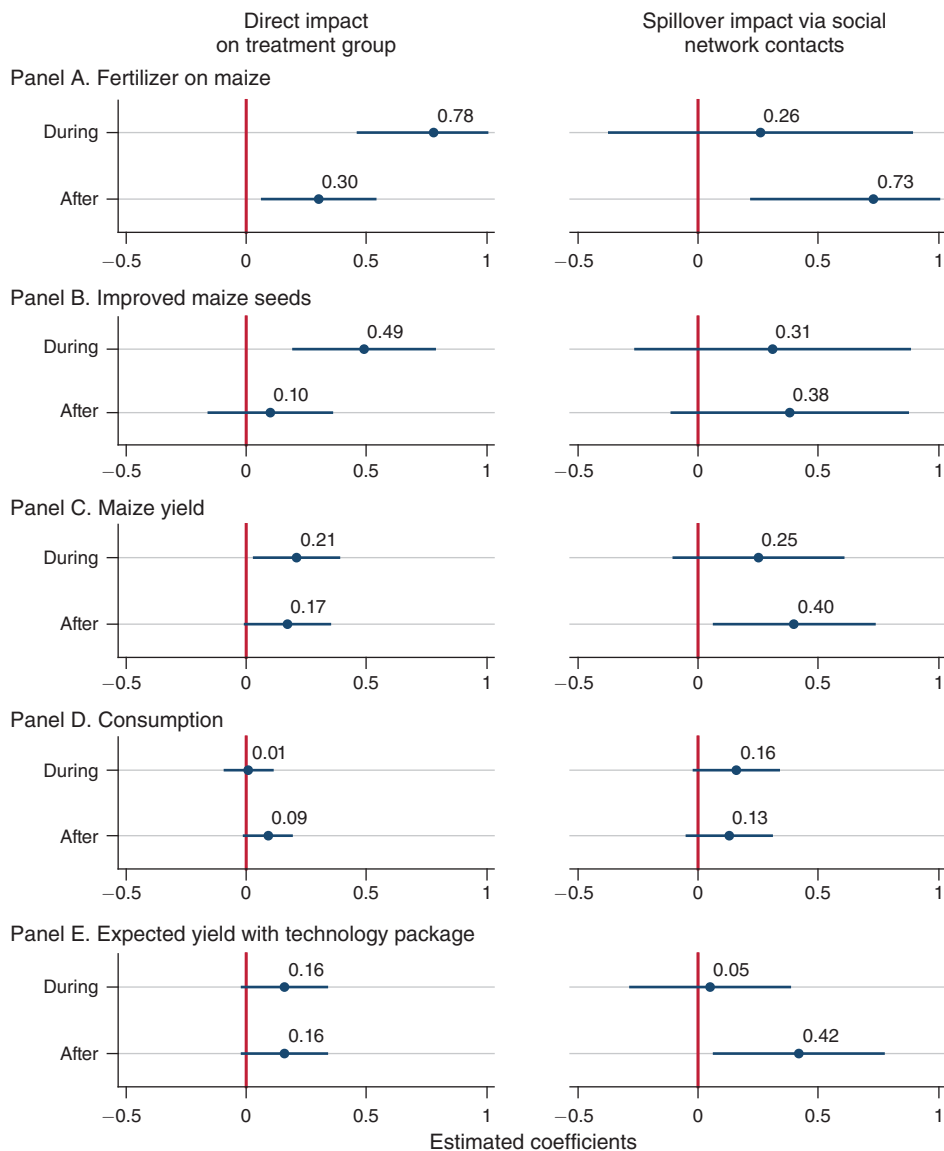


FIGURE 2. DIRECT AND SPILLOVER IMPACTS OF SUBSIDIES FOR GREEN REVOLUTION TECHNOLOGY

Notes: Results are from the estimation of equation (1). Dependent variable x expressed as $\log(1 + x)$ for fertilizer and improved seed outcomes (originally in kilograms, which includes zeros), and $\log(x)$ for other outcomes (data include no zeros). Maize yield originally expressed in kilograms per hectare. Daily consumption per capita originally expressed in Mozambican meticaís. Expected yield with the technology is respondent’s estimate of maize output (in kilograms per hectare) on household’s main farming plot if using the subsidized Green Revolution technology package. Regression coefficients presented in left-hand column are α_{During} and α_{After} ; those in right-hand column are σ_{During} and σ_{After} . Lines represent 95 percent confidence intervals. Standard errors are clustered by household. Regression coefficients are presented in online Appendix Table A5.

Finally, to examine learning, we estimate impacts on beliefs about expected yields with the technology package. To do this, after identifying the main maize plot for each study participant, we asked what production the farmer would expect

in this plot if they used the technology package on this parcel in (i) a normal year, (ii) a very good year, and (iii) a very bad year. We then asked the farmer to say, on average, out of 10 years, how many are very good years, very bad years, and normal years. This set of questions allows us to calculate the expected yield when using the technology package. Being a direct recipient of the subsidy significantly increased the yield expected by the farmer when using the technology package. The positive effects on expected returns are stable in the “during” and “after” periods (in terms of magnitudes and statistical significance).

In addition to positive direct effects on treated households, there are substantial spillover effects from treated households to their social network contacts. We find no statistically significant spillover impacts during the subsidy season (σ_{During} coefficients). However, in subsequent seasons (as represented by σ_{After} coefficients) households who have above-median connections to treated households saw increases in fertilizer use, improved seed use, maize yields, and beliefs about the returns to the technology package. Impacts on these outcomes in the “after” period are statistically significantly different from zero at conventional levels (or nearly so, for improved seed use). Indeed, in most instances, the spillover effects are as large or larger than the direct effects on a voucher recipient whose social network did not include at least two voucher recipients.¹⁰

B. Magnitudes of Effects

Our estimated intent-to-treat (ITT) impacts are large and economically consequential. Given a 28.8 percent direct effect of random assignment to treatment on use of the technology package, our ITT estimate of a 0.21 increase in log yields (an approximately 23 percent yield increase) for subsidy-recipient households implies an undiluted treatment-on-the-treated (TOT) yield gain of 80 percent. Recent efforts to estimate the yield gap for eastern and southern African maize farmers find the gap to be between 2.5 and 3.5 tons/hectare (Sadras et al. 2015).¹¹ Hence farmers in our study area who produced a bit less than one ton per hectare on average could have tripled their yields if they had fully closed the gap. From this perspective, our results are well within the bounds of what is believed to be technologically feasible.

As noted above, the network spillover effects are substantial. The after-subsidy ITT impact estimates in Figure 2 imply that a voucher recipient who had two or more social network contacts who also received vouchers would experience an increase in log yields of 0.57 or approximately 77 percent. Adjusting this figure for the 28.8 percent net compliance rate gives a TOT estimate in excess of 250 percent. While large, even an impact of this magnitude would not fully close the yield gap identified by 2.5–3.5 tons/hectare yield gap identified by Sadras et al. (2015).

Finally, given that about 60 percent of household income comes from maize, the estimated consumption impacts are also in line with what is possible and what would

¹⁰Note that a voucher recipient with two network members who received vouchers still experiences larger impacts than a nonvoucher recipient with that same network structure.

¹¹The yield gap is the difference between the yields that farmers obtain and the yields that are technologically possible using improved seeds and fertilizers given the farmer’s soils and the weather conditions.

be expected from Asian experience with Green Revolution technologies (Otsuka and Larson 2016).¹²

C. Alternate Specification of Spillover Effects: Number of Treatment Group Members in Social Network

While the presence of spillover effects is consistent with what we might expect if subsidies help resolve underlying information failures, this section is the first of two that provide additional analyses of the role of the social network. This section explores alternative specifications and tests for mechanisms beyond information spillovers that might drive the findings.

Spillover effects are specified in our main regressions (Figure 2 and online Appendix Table A5) as the effects of having above-median (two or more) social network members in the treatment group. This provides a reasonable approximation of the spillover effects observed when estimating a more flexible specification. Table 2 estimates spillover effects using such a flexible specification, with five separate indicators for the number of one's social network contacts in the treatment group (indicators for one, two, three, four, and five or more).

The general pattern in Table 2 is that the estimated coefficients on the social network variables tend to be positive and significant in the "after" period, but mostly not in the "during" period. In the after period, coefficient magnitudes rise as one moves from one social network contact to two social network contacts in the treatment group, with the effect remaining roughly stable thereafter. These patterns roughly approximate a step function at two or more social network contacts in the treatment group.¹³ Figure 3 displays the spillover effect coefficients for the after period (σ_{After}), using the same flexible specification, for fertilizer use and maize yields.

D. Alternate Specification of Spillover Effects: Social Network Effects for Treatment Group and Control Group Members

Spillovers are often thought of as impacts on those who did not receive the treatment themselves (the control group, subsidy nonrecipients). But spillovers can affect treatment group members (subsidy recipients) as well (Baird et al. 2014), and so the spillover effect coefficients in our analysis (σ_{During} and σ_{After}) incorporate spillovers to both treatment and control group members. In Table 3, we estimate these spillover effects to farmers in the control group and in the treatment group separately. We allow these spillover effects to differ by treatment group by modifying equation (1) so that $SocialTreat_{ic} During_t$ and $SocialTreat_{ic} After_t$ are each interacted

¹² Coefficients capturing the social network spillovers in the consumption regressions are surprisingly large, and their time pattern (roughly stable across during and after periods) is not consistent with the spillover effect patterns found in regressions for other outcome variables (larger effects in the "after" than the "during" period). We can offer no theoretical explanation for this, and simply note that these spillover effects on consumption are imprecisely estimated, so that 95 percent confidence intervals comfortably include smaller effects, as well as effects that rise in magnitude from the "during" to "after" periods.

¹³ We present the results in Table 2 to give a general sense of the nonlinear patterns in the coefficient estimates, not to highlight the statistical significance of any particular coefficient. We therefore refrain from discussing the statistical significance of individual coefficients in this table.

TABLE 2—REGRESSIONS WITH MORE FLEXIBLE SPECIFICATIONS OF SPILLOVER EFFECT

	Fertilizer on maize	Improved maize seeds	Maize yield	Daily consumption per capita	Expected yield with technology package
<i>Panel A. Direct impacts on treatment group members</i>					
During	0.78 [0.16]	0.51 [0.15]	0.20 [0.091]	0.019 [0.049]	0.15 [0.089]
After	0.31 [0.12]	0.11 [0.13]	0.17 [0.087]	0.096 [0.046]	0.16 [0.091]
<i>Panel B. Spillover impacts DURING subsidy period of having x contacts in treatment group</i>					
1 contact	-0.53 [0.34]	-0.19 [0.28]	0.14 [0.16]	0.052 [0.091]	0.059 [0.18]
2 contacts	-0.025 [0.44]	0.12 [0.35]	0.33 [0.23]	0.21 [0.11]	0.12 [0.23]
3 contacts	-0.57 [0.52]	0.20 [0.46]	0.44 [0.30]	0.084 [0.14]	0.056 [0.28]
4 contacts	-0.079 [0.59]	0.68 [0.49]	0.28 [0.30]	0.11 [0.16]	-0.25 [0.33]
5+ contacts	-0.057 [0.56]	1.01 [0.46]	0.030 [0.30]	0.29 [0.15]	-0.18 [0.32]
<i>Panel C. Spillover impacts AFTER subsidy period of having x contacts in treatment group</i>					
1 contact	0.33 [0.23]	-0.29 [0.23]	0.18 [0.15]	0.069 [0.076]	-0.055 [0.16]
2 contacts	0.95 [0.31]	0.14 [0.32]	0.53 [0.20]	0.18 [0.11]	0.44 [0.22]
3 contacts	0.98 [0.37]	0.21 [0.40]	0.48 [0.27]	0.16 [0.13]	0.15 [0.30]
4 contacts	0.94 [0.47]	0.53 [0.47]	0.60 [0.27]	0.27 [0.15]	0.24 [0.31]
5+ contacts	1.17 [0.39]	0.66 [0.41]	0.39 [0.27]	0.31 [0.14]	0.11 [0.28]
Observations	1,428	1,404	1,346	1,393	1,273
Control group mean in levels	26.9	20.6	869	79.7	1,776

Notes: Data are from 2011, 2012, and 2013 follow-up surveys. Dependent variables are as in Figure 2. Regressions are based on modified version of equation (1) but with five separate indicators for the number of one's social network contacts in the treatment group (indicators for one, two, three, four, and five or more) instead of a single indicator for above median (two or more) social network contacts in the treatment group. Standard errors are clustered by household in brackets. The last row of the table provides the control group mean of the variable (in levels) in the first ("during," 2010–2011) follow-up survey (consumption in metacais, all others in kilograms).

with the indicator for the treatment group ($Treat_{ic}$), and separately with an indicator for the control group ($Cont_{ic}$).

Spillover effects for treatment and control group members are quite similar, as it turns out, with some nuanced differences. The main difference is that for the treatment group only, spillovers lead to higher maize yield in the subsidized ("during") period, not only in the post-subsidy "after" period, suggesting that treatment group members may have helped each other learn to use the new technologies more productively in the initial subsidized year.

E. Learning versus Alternate Mechanisms

Our results are consistent with the benefits of the input subsidy programs being driven, at least in part, by learning about the Green Revolution technologies.

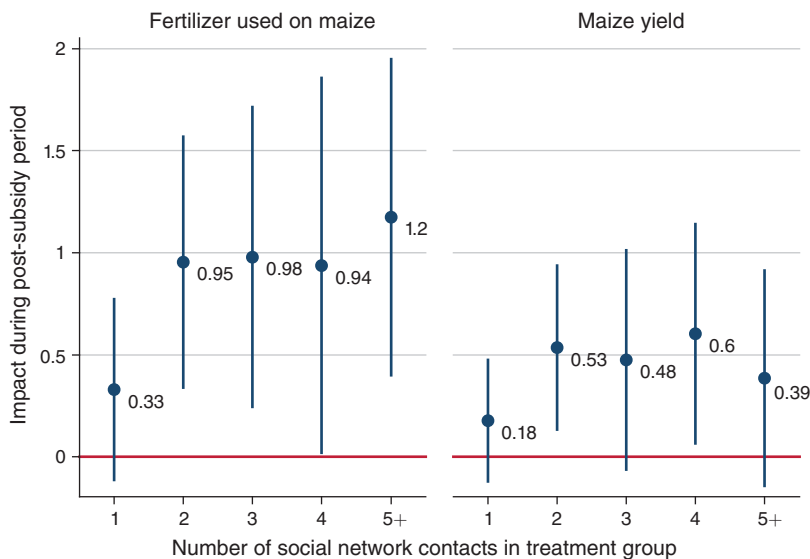


FIGURE 3. SPILLOVER EFFECTS BY NUMBER OF SOCIAL NETWORK CONTACTS IN TREATMENT GROUP

Notes: The specification is the same as Table 2. See Table 2 for regression coefficients and other details.

TABLE 3—DIRECT AND SPILLOVER EFFECTS OF INPUT SUBSIDIES, WITH SPILLOVER EFFECTS ESTIMATED SEPARATELY FOR TREATMENT AND CONTROL GROUP MEMBERS

	Fertilizer on maize	Improved maize seeds	Maize yield	Daily consumption per capita	Expected yield with technology package
<i>Panel A. Direct impacts</i>					
During	0.83 [0.20]	0.37 [0.19]	0.11 [0.11]	0.0028 [0.065]	0.11 [0.12]
After	0.31 [0.14]	0.055 [0.16]	0.12 [0.11]	0.073 [0.055]	0.18 [0.12]
<i>Panel B. Spillover impacts on control group</i>					
During	0.33 [0.36]	0.16 [0.32]	0.12 [0.19]	0.14 [0.11]	-0.012 [0.19]
After	0.75 [0.29]	0.33 [0.30]	0.33 [0.19]	0.11 [0.098]	0.44 [0.20]
<i>Panel C. Spillover impacts on treatment group</i>					
During	0.19 [0.38]	0.47 [0.33]	0.37 [0.21]	0.17 [0.11]	0.11 [0.20]
After	0.72 [0.30]	0.44 [0.28]	0.47 [0.20]	0.16 [0.10]	0.40 [0.21]
Observations	1,428	1,404	1,346	1,393	1,273

Notes: Dependent variables are as in Figure 2. Regressions are a modified version of in equation (1) in the main text. Standard errors are clustered by household in brackets.

First, we directly observe that study participants report higher expected returns to the technology package when they are treated or have two or more treated social network contacts. Second, the increases in technology adoption, yield, and

consumption persist in periods after the end of the subsidy. Third, the greater effect on fertilizer than on improved seeds is consistent with the fact that fertilizer was less used and known than improved seeds prior to the program. Finally, the fact that the spillover effects mostly occur with a lag (only appearing in the “after” period) also suggests a learning channel, as farmers may wait to fully observe outcomes of neighbors’ experimentation before experimenting themselves (Foster and Rosenzweig 1995).

In addition, it is of interest to examine whether the subsidy affects adoption on the extensive margin (the results presented so far combine the extensive and intensive margins). If the subsidy’s effects operate only on the intensive margin (affecting amount used, but not the decision to use at all), this would not be obviously reflective of learning. Extensive margin effects—decisions to adopt or disadopt entirely, rather than simply adjust use on the intensive margin—are more revealing of a learning mechanism. As it turns out, both the direct and spillover effects of the subsidy do appear on the extensive margin of technology use. In online Appendix E and online Appendix Table A6, we present treatment effects from estimation of equation (1) for alternate specifications of dependent variables, including indicators for use of any fertilizer (column 1) and any improved seeds (column 3). Extensive margin effects are very similar to those presented in Figure 2 in terms of directions and statistical significance of effects. Altogether, our findings strongly suggest that the subsidy alleviates information imperfections related to the subsidized technologies.

That said, social network spillovers are also consistent with mechanisms other than learning. A first possibility is that farmers simply kept some fertilizer for the following season, or shared or sold fertilizer to others. We ask about fertilizer saving and sharing in our surveys and find that it is quite rare: immediately following the subsidized 2010–2011 season, the vast majority of respondents (88.8 percent) reported they had already used all the inputs for agriculture, 2.8 percent reported that they had not used it, and only 1.4 percent reported that they sold the inputs.¹⁴ Even though it was an option, exactly zero farmers reported that they had given away any of the inputs. There appears to be little scope for farmers to have shared their inputs with others. Sharing may have also been limited by the fact that the vouchers could only be redeemed by the intended beneficiary named on the voucher certificate. In addition, we also estimate whether the likelihood of using the voucher for one’s own agriculture was affected by the indicator for having two or more social network members in the treatment group. If sharing was happening, one would expect that having more neighbors treated should reduce one’s own use of the voucher, but the effect is small in magnitude and statistically insignificant (online Appendix Table A8).

Another possible channel that can generate the spillovers is resource transfers from treated farmers to their social network contacts (Maertens and Barrett 2013). However, we find that the treatment and social network connections to the treatment group are not significantly related with the likelihood of providing assistance

¹⁴An additional 1.4 percent declared that they used the inputs in some other way and 5.6 percent did not respond to the question.

to others (online Appendix Table A9). Resource transfers are therefore unlikely to explain the large spillovers that we observe.

V. Cost Effectiveness

How cost effective was the subsidy program, and what fraction of the benefits occurs in subsequent (post-subsidy) periods and via spillovers? We calculate benefit-cost ratios of the subsidy program in total and then separately for direct subsidy beneficiaries and their social network contacts. We also distinguish between the subsidized and post-subsidy periods. In this calculation, benefits are taken as the increase in maize output net of increases in the costs of fertilizer, improved seeds, and estimated additional labor. Costs include the cost of the subsidies to the government, as well as logistical costs calculated from detailed implementation budgets.¹⁵ We make conservative assumptions throughout. Most importantly, we do not consider future years beyond the two-year post-subsidy period of our study, and we do not assume any gains to social network contacts who are not among our study participants. Any gains that accrue in future years, and to non-study-participants connected to subsidy beneficiaries, would lead to even larger benefit-cost ratios.

Panel A of Table 4 displays the decomposition of benefits. Most studies, without a post-subsidy-period follow-up and a specific design to capture spillovers, would focus on gains accruing to direct subsidy beneficiaries during the subsidized period. We find that such benefits only account for 9 percent of all benefits. But even when only accounting for this small minority of total benefits, the benefit-cost ratio would be 1.8 (panel B). The remaining 91 percent of benefits accrues via spillovers from subsidized farmers to their social network contacts and in post-subsidy periods. Seventy percent of benefits occur through spillovers. Seventy-four percent of benefits occur in the years after the subsidy ended. Accounting for both spillovers and post-subsidy effects leads to a roughly ten-fold increase in the benefit-cost ratio, from 1.8 to 19.8.

VI. External Validity

As with all empirical work, subsequent studies should test the generalizability of these results. Policymakers should be cautious about expanding ISPs before future studies can estimate direct impacts, post-subsidy persistence, and social network spillovers in different populations and under different conditions, as guidance for locally specific benefit-cost analyses.

In terms of the potential gains from Green Revolution technologies in sub-Saharan Africa, how representative is our study area? Agroecological factors (e.g., heterogeneous and poor-quality soils) may limit the profitability of Green Revolution seeds and fertilizers in many parts of Africa (Marenya and Barrett 2009, Suri 2011). In a recent effort to investigate this claim, Hurley, Koo, and Tesfaye (2018) use crop growth models, in combination with high-resolution soil maps and weather

¹⁵ Online Appendix G details calculation of the benefits and costs.

TABLE 4—INPUT SUBSIDY PROGRAM BENEFIT-COST ESTIMATES

	Subsidized year	Two years following the subsidy	All years
<i>Panel A. Shares of benefits</i>			
Direct effect	9%	21%	30%
Spillover effect	17%	53%	70%
Direct and spillover effects	26%	74%	100%
<i>Panel B. Benefit-cost ratios</i>			
Direct effect	1.8	4.2	5.9
Spillover effect	3.3	10.6	13.9
Direct and spillover effects	5.1	14.7	19.8

Notes: Benefits are increases in value of additional maize yields, minus costs of additional improved seeds, fertilizer, and associated labor costs. Direct effects accrue from being randomly assigned to treatment group (being eligible for subsidy voucher oneself). Spillover effects accrue from having above-median (two or more) social network contacts randomly assigned to treatment group. Costs include the value of input subsidies and subsidy program management and distribution costs.

patterns, to simulate farmers' willingness to pay for Green Revolution technologies at a highly disaggregated level across all of sub-Saharan Africa.¹⁶ While they find that there is some truth to the claim that the Green Revolution technologies are not desirable in parts of Africa, they estimate that the technologies would be desirable even for risk averse farmers across 75 percent of the cropped area in sub-Saharan Africa.¹⁷ The Hurley, Koo, and Tesfaye (2018) analysis therefore suggests that our findings may be applicable to agroecological conditions in a sub-Saharan Africa much more broadly than just our study area.

There are some other external validity considerations of note. A prior donor-funded effort led to availability of fertilizer and improved seeds through a network of private agro-input dealers in the area (Nagarajan 2015). It would be important for future research to investigate whether effects of subsidies are attenuated in areas less accessible to input markets.

It is also important to consider generalizability of our results from the standpoint of the types of farmers included in our study sample. As described above, our study participants are a subset of farmers with particular characteristics. Consistent with the emphasis in "second-generation" ISPs that beneficiary farmers should be those with high potential gains from the inputs (Morris et al. 2007), our study participants were judged by government agricultural extension workers (who distributed subsidy vouchers) to be "progressive farmers" interested in modern production methods and commercial farming. The direct effects of subsidies that we estimate

¹⁶They estimate willingness to pay at the level of 50 km by 50 km geographic grid cells.

¹⁷Hurley, Koo, and Tesfaye (2018) use average prices for maize and fertilizer in sub-Saharan Africa to calculate that it would require 122 kg/ha of additional maize to pay for the 40 kg/ha of urea needed for the Green Revolution technology package that they analyze. They do not calculate the additional cost of the improved seeds, which constitute the other half the Green Revolution package. Based on the price of hybrid maize seed in eastern and southern Africa, we calculate that it would take about three times the 122 kg/ha amount to pay for both seeds and fertilizer. Figure 6 in Hurley, Koo, and Tesfaye (2018) shows that when incremental Green Revolution input costs are three times the 122 kg/ha amount, then 75 percent of farmers would find the package worth adopting, even with high levels of risk aversion.

TABLE 5—COMPARISON OF STUDY PARTICIPANTS WITH AGRICULTURAL HOUSEHOLDS IN MOZAMBIQUE AND MANICA PROVINCE

	Mozambique	Manica	Our sample
Household head is female	27.48%	24.56%	15.08%
Household head can read or write	59.73%		77.80%
Household head completed primary school	22.68%		40.64%
Household head completed secondary school	5.18%		3.37%
Grows maize	68.9%		99.61%
Uses irrigation	5.27%	10.11%	4.89%
Uses fertilizer (on any crop)	3.75%	2.11%	48.23%
Uses pesticide	2.51%	1.73%	4.91%
Uses tractor	1.6%	1.8%	1.17%
Observations	3,827,754	265,489	511

Note: Data show shares of households meeting given condition.

Sources: Descriptive stats in the “Mozambique” and “Manica” columns are from the Censo Agro-Pecuario 2009–2010 Resultados Preliminares (INE 2011). Data in the last column are from authors’ calculations, using the first (2011) survey.

are for progressive farmers, and the spillover effects are from progressive farmers to other progressive farmers.

How are our study participants different from other farmers? Table 5 compares our study households to agricultural households in Manica province and in Mozambique as a whole. Our study participants have substantially higher literacy and education levels than Mozambican farm households overall. Their rate of female household headship is lower than for Manica and Mozambique as a whole. When it comes to agricultural outcomes, our study households also differ on some important dimensions. Because this was a condition for inclusion in the study, essentially all our households farm maize, compared to roughly two-thirds in Mozambique overall. Our households also are substantially more likely to use fertilizer on any crop (this figure of 48 percent is higher than the fertilizer use rate in Table 2 because the latter is fertilizer use on maize only). By contrast, our study participants’ rates of pesticide use, tractor use, and irrigation are not substantially different from those of other farm households in the province or country. Overall, because our study participants clearly are a selected subset of farmers, future studies should investigate whether our findings apply to populations more typical of their general populations. In particular, even if future ISPs continue to follow recommendations to target subsidies to farmers thought to have high potential gains, it would be of great interest to explore whether spillovers extend to non-“progressive” farmers in the social network, since the latter were not included in our study sample.

VII. Conclusion

We find that temporary input subsidies can cost-effectively promote learning about Green Revolution technologies, adoption of those technologies, and improvements in agricultural output and living standards among both subsidy beneficiaries and members of their social networks.

Viewed through the lens of economic theory, input subsidies address two kinds of market failures. First, they alleviate *imperfect information*, stimulating learning

about the true productive returns to the technology among farmers who were previously underestimating those returns. Second, they mitigate the underprovision of goods that generate *positive externalities*. Subsidies induce experimentation with the technologies, and information spills over from subsidy beneficiaries to their social network contacts, who benefit from the information as well. When goods generate positive information or knowledge externalities, individuals have incentives to *free-ride*, avoiding costly experimentation so as to learn from others' experimentation instead. Subsidies induce some who would have engaged in free-riding to experiment themselves, benefiting others who observe and also learn from the induced experimentation. Society thus moves closer to socially optimal levels of experimentation. When information constraints are important, well-designed public policy that successfully encourages experimentation and social learning can generate the highly favorable benefit cost ratio that we estimate for this program. In short, there is a strong economic case for temporary input subsidies, understood as a one-off inducement to experiment and learn.

While no single study can establish the general desirability of a program or intervention, the work of Hurley, Koo, and Tesfaye (2018) discussed above suggests that our findings have general relevance, as they estimate that Green Revolution technologies would make farmers better off on 75 percent of African cropland. Pending further studies to establish external validity, our findings have direct policy implications. In contexts with strong post-subsidy adoption persistence and social network learning spillovers, subsidy programs can achieve substantial gains even if scaled back, compared to current subsidy policies implemented by governments in Africa.¹⁸ Input subsidy programs need not be permanent nor universal to benefit farmers and their social networks in substantial ways. Temporary, targeted subsidies can make major progress in bringing the gains of new technologies to populations previously bypassed by the Green Revolution.

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¹⁸ While Mozambique itself announced a National Fertilizer Program as part of its strategic development plan, fiscal constraints have prevented the adoption of policies beyond the pilot intervention that is the basis for this study.

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