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

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Abstract: The Green Revolution bolstered agricultural yields and rural well-being in Asia and Latin America, but bypassed sub-Saharan Africa. We study the first randomized controlled trial of a government-implemented input subsidy program (ISP) in Africa. A *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.

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1. Introduction

The "Green Revolution" reshaped agriculture across Asia and Latin America during the last four decades of the 20th century, but largely bypassed sub-Saharan Africa. Adoption of high-yielding seeds and chemical fertilizers led to rapid growth of agricultural yields in Asia and Latin America. In contrast, in sub-Saharan Africa, the modest growth in agricultural production that did occur was driven by land expansion, with little technological change, nor yield growth (Evenson and Gollin, 2003). While Green Revolution technology adoption allowed the rest of the developing world to take off economically, sub-Saharan Africa has become the repository of an ever-larger share of the world's severely poor people. Twenty-five years ago, 17% of the world's absolutely poor lived in sub-Saharan Africa. Since that time, that figure has risen to 51% (World Bank, 2018).

In response to the Green Revolution that wasn't, African nations signed the Maputo Declaration in 2003, pledging to invest 10% of their national budgets in agriculture to achieve a 6% 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 Revolution technologies that drove the economic take-off 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.[4] These "second-generation" input subsidy programs are intended to be "smart" in the sense that they (i) complement development of private input markets; (ii) target beneficiaries with high potential gains from input adoption; and (iii) are temporary rather than

[4] Input 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.

permanent. Input subsidy programs are now consuming substantial resources across the continent (Morris *et al*., 2007). While not all African nations have reached the Maputo Declaration budget targets, ten countries currently implement "second-generation" input subsidy programs, devoting 20-25% of public spending on agriculture (nearly US$1 billion annually) to such programs. Contrary to the third guiding principle behind "smart" subsidies, these programs appear to be relatively permanent fixtures of implementing governments' agricultural policies, with no apparent phase-out plans (Jayne *et al*., 2018).

In the context of this recent wave of African ISPs, fundamental questions remain unresolved. What are the impacts of ISPs on adoption of subsidized technologies, agricultural output, and household well-being? Conclusions of existing observational studies are mixed, and have difficulty establishing causal impacts: no prior research on ISPs has used a randomized control methodology. Other questions have been barely addressed, if at all, in prior studies. If subsidies are only temporary, would impacts persist in later, unsubsidized periods? Do ISP impacts extend to social networks of subsidy recipients? What market failures, if any, are ISPs helping to address?

Answers to these questions are key to credibly estimating 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 gains of temporary programs and to understand whether governments can phase out ISPs over time. Relatedly, estimates of spillover impacts to social networks allow a full accounting of societal gains.

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 non-rival, 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 *et al.*, 2011).

To contribute to these open questions, we conducted the first and (so far) only randomized controlled trial of a "second-generation" ISP. 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. The program is representative of ISPs that have recently spread in Sub-Saharan Africa. Our results therefore have direct bearing on understanding the impact of ISPs in the region more generally.

We find that a 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 are persistent in later unsubsidized years. Magnitudes of impacts are substantial, but comfortably within the range of potential yield impacts of Green Revolution technologies (as detailed below in Section 5.2). We also observe spillovers from subsidized farmers to their social networks: agricultural contacts of subsidized farmers also see increases in technology adoption and yields. Spillovers account for the vast majority of subsidy-induced gains. 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.

Our main contribution is providing the first causal estimates based on a randomized controlled trial of the new generation of input subsidy programs in Africa. In addition, we contribute a new combination of findings to the literature on technology adoption in developing countries. In this context, ours is the first paper to show that a temporary subsidy for agricultural production technology has lasting impacts on adoption after the subsidy ends. In non-agricultural technology contexts, persistent impacts of a temporary technology subsidy on adoption have been found for health goods (Dupas, 2014) and for labor migration (Bryan *et al.*, 2014). One prior non-experimental study failed to find that ISPs lead 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 *et al*. 2008, Beaman *et al.* 2013, Abate *et al*. , 2018). Social learning about technologies has been documented previously in prior studies, using both observational approaches (e.g., Bandeira 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 forthcoming), 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 sustantially when spillovers are taken into account, highlighting the importance of incorporating long-term and spillover effects in program evaluation.

This paper is organized as follows. In Section 2, we provide further detail about the subsidy program. In Section 3, we describe the randomized research design and the study sample. Section 4 details the empirical regression specification. We present the empirical results in Section 5. In Section 6, we provide a cost-effectiveness calculation. We provide concluding thoughts in Section 7. An Online Appendix provides additional analyses and robustness tests, to which we refer throughout the main text.

2. Mozambique's Input Subsidy Program

Mozambique's Farmer Support Program provided once-off 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, targeted farmers thought to have high potential gains from the inputs, and provided only temporary subsidies. The technology package we study was designed for maize production.[5] The subsidy provided a 73% discount on a package of chemical fertilizer (50 kg of urea and 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 2010-11 agricultural season. Subsidy users had to make the 27% co-pay (863 MZN or about $US32) when redeeming the voucher at an agricultural dealer.

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

While 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. We therefore estimate impacts of an actual government-implemented program, rather than a potentially unrepresentative researcher-implemented intervention (Muralidhran and Niehaus, 2017).

The randomized controlled trial of the input subsidy program was designed by the research team in cooperation with the Ministry of Agriculture and IFDC. Lists of eligible farmers were created by government agricultural extension officers, with input from local leaders and agro-input retailers. Individuals were eligible for a voucher coupon if they met the following program criteria: 1) farming between 0.5 hectare and 5 hectares of maize; 2) being a "progressive farmer," defined as a producer interested in modernization of their production methods and commercial farming; 3) having access to agricultural extension and to input and output markets; and 4) being able and willing to pay for the remaining 27% 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% 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 PIs, and the list of voucher winners was provided to agricultural extension officers. Extension officers were responsible for voucher distribution to beneficiaries. Voucher distribution occurred at a meeting to which only farmers who won the lottery were invited. Random assignment and distribution of vouchers occurred in September to December 2010. Vouchers were intended to be used for inputs for the 2010-11 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 names were verified by the input retailer when redeemed.

3. Research Design and Sample

To estimate causal effects, the government collaborated with us to randomly assign subsidies among eligible farmers in 32 villages in Manica province. Within each village, the agricultural extension agency identified farmers eligible for subsidy vouchers (using the same criteria used in the over-arching program). The research team randomly selected half of farmers in each study village to receive subsidy vouchers. These farmers comprise the treatment group, and the remainder comprise the control group.[6]

The sample consists of 514 farmers (247 and 267 in the treatment and control groups, respectively). Agricultural extension officials informed study participants that vouchers would be assigned via random lottery in study villages, announced lottery winners, and distributed vouchers accordingly. We implemented surveys of treatment and control participant households, tracking outcomes in the subsidized 2010-11 season and two annual agricultural seasons afterwards, 2011-12 and 2012-13 (Online Appendix B gives further detail on the sampling and tracking of respondents). We collected data on social network connections, asking each participant to identify others in the village with whom they discuss agriculture (Conley and Udry, 2010). The subsidy was assigned randomly, so controlling for the size of one's social network, the extent to which members of one's social network received the subsidy was also random. We are therefore able to establish the causal impact of the subsidies on a beneficiary's

[6] Online Appendix A gives further detail on the study context and locality definitions.

own technology adoption over time, and on adoption by social network contacts of beneficiaries. We also examine direct and spillover impacts on agricultural production, consumption and learning about returns to the subsidized inputs.

Table 1 presents key baseline summary statistics. Less than a quarter of households used any chemical fertilizers in the season that preceded the subsidy, and slightly more than a half planted improved maize seeds. Farmers' experience with chemical fertilizer prior to the study appears to be quite limited. We also asked farmers how many years they used fertilizer out of the last 10 years (prior to the beginning of the study). 67% of farmers reported zero years, and 87% reported two years or less (panel C). 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.

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 a day poverty line. Finally, we can see in panel C of the table that our network survey instrument registers substantial variation in the extent to which study respondents were connected to other voucher winners, with 44% registering no connections, and another 23% registering 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% of farmers in the treatment group used their vouchers. Most such non-compliance was due to inability to make the input package co-payment (even though claimed ability to pay was a participant selection criterion). Moreover, 12.4% of control group farmers reported using subsidy vouchers for the input package, due to imperfect compliance by extension agents distributing vouchers (see Online Appendix D). 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, Appendix Table A3). This treatment-control difference in subsidy voucher use drives all treatment effect estimates.

4. Empirical Specification

Our study focuses on three key outcome variables: use of Green Revolution inputs, learning about returns to those inputs, and living standards. We estimate "intent to treat" (ITT) effects, for the season in which the subsidy was offered, as well as for subsequent unsubsidized seasons (to study post-subsidy persistence of impacts). We also seek to 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 household *i* in locality c in time period *t*:

$$(1) \qquad y_{ict} = \alpha_{Dur} Treat_{ic} Dur_t + \alpha_{Aft} Treat_{ic} Aft_t + \sigma_{Dur} Soc_{ic} Dur_t + \sigma_{Aft} Soc_{ic} Aft_t + \boldsymbol{X_{ict}} \gamma + \theta_c + \varepsilon_{ict}$$

Outcome variables include measures of input use, yield, consumption and beliefs about the returns of the input package (see Online Appendix E for detailed variable definitions). Time periods are the 2010-11 subsidized season, and two subsequent seasons (2011-12 and 2012-13) when no subsidy was offered. Right-hand-side variables are all indicators (equal to 1 if so; 0 otherwise).

TABLE 1—BASELINE DESCRIPTIVE STATISTICS

A. Percentages for key binary outcomes

	%
Used fertilizer on maize	23%
Used improved maize seeds	54%

B. Descriptive statistics of continuous variable

	Mean	Standard Deviation	Percentiles 5th	25th	median	75th	95th
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

C. Frequency of discrete variables

	Mean	0	1	2	3	4	5 or more
Number of years using fertilizer out of 10 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 households that were surveyed in all three of the annual surveys. Fertilizer, seed, maize yield, and expected yield are in kilograms. Daily consumption per capita is in Mozambican meticais (27 MZN ≈ 1 USD). Continuous variables are truncated at their 99th percentile prior to calculation of means and standard deviations.

$Treat_{ic}$ indicates treatment group households. Dur_t indicates the observation is in the subsidized ("during") time period, and Aft_t the subsequent ("after") time periods. Soc_{ic} indicates the household has above-median (two or more) social network contacts who were randomized into the treatment group (we discuss below the rationale for this specification). Social network contacts are defined as those with whom the participant discussed agriculture in the season prior to the subsidized 2010-11 season.[7] Households with $Soc_{ic} = 1$ have 3.63 treatment group contacts on average; for households with $Soc_{ic} = 0$, the average is 0.29.

The vector of controls $\boldsymbol{X_{ict}}$ includes indicators 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 Soc_{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 Soc_{ic} can be interpreted as the causal effect of having two or more social network contacts who were offered the input subsidy. To capture common time effects, $\boldsymbol{X_{ict}}$ includes an indicator for each time period, and interactions between the time period indicators and the social network size indicators (allowing social network size effects to vary over time). θ_c are locality fixed effects (treatment is randomized within locality). ε_{ict} is a mean-zero error term. We employ robust (heteroscedasticity-consistent) standard errors.

[7] Following 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 asked one by one whether they talked about agriculture with this person in the prior season.

Random assignment led to balance on key time-invariant household characteristics with respect to $Treat_{ic}$ and Soc_{ic}.[8] Survey attrition was low (8.6% on average across rounds) and uncorrelated with $Treat_{ic}$ and Soc_{ic}.[9]

5. Empirical Results

This section presents intention to treat estimates for equation (1) above. Given the statistical and especially significance of the estimated social network effects, we also examine alternative network specifications and test for alternative mechanisms that might underlie our findings.

5.1 Regression results

Regression results are presented graphically in Figure 1, and regression coefficients are reported in Appendix Table A4. Outcome variables in the regressions are expressed in logarithms (findings are robust to alternate dependent variable specifications, such as indicators, kilograms, or Mozambican meticais as shown in Online Appendix F and Appendix Table A5). In the left-hand column of Figure 1, coefficients represent ITT effects of subsidy assignment on households in the treatment group, during and after the subsidized season (α_{Dur} and α_{Aft}, respectively). The right-hand column of Fig. 1 presents σ_{Dur} and σ_{Aft}, spillover impacts on study participants "more connected" to the treatment group (with above-median treatment group

[8] See Table A1 in Online Appendix C.

[9] Table A2 in Online Appendix C, which reports these results.

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 treatment group members 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_{Dur}) are large and positive for adoption of fertilizer and improved seeds, as well as for maize yields. In the after-period (α_{Aft}), 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% level. Direct impacts on yields remain almost as high in the period following the subsidy, which can be due to the farmers re-using 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 (Figure 1, fourth row). 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 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.

Finally, to examine learning, we estimate impacts on beliefs about expected yields with the technology package. To do this, for each study participant, after identifying their main maize plot, we asked what production the farmer would expect in this plot if they used the technology package on this parcel in 1) a normal year, 2) a very good year, and 3) 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 simple questions allows us to calculate the expected yield when using the technology package. The last row of Figure 1 shows that 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_{Dur} coefficients). However, in subsequent seasons (as represented by σ_{Aft} 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 all these outcomes in the after period are statistically significantly different from zero at conventional levels.

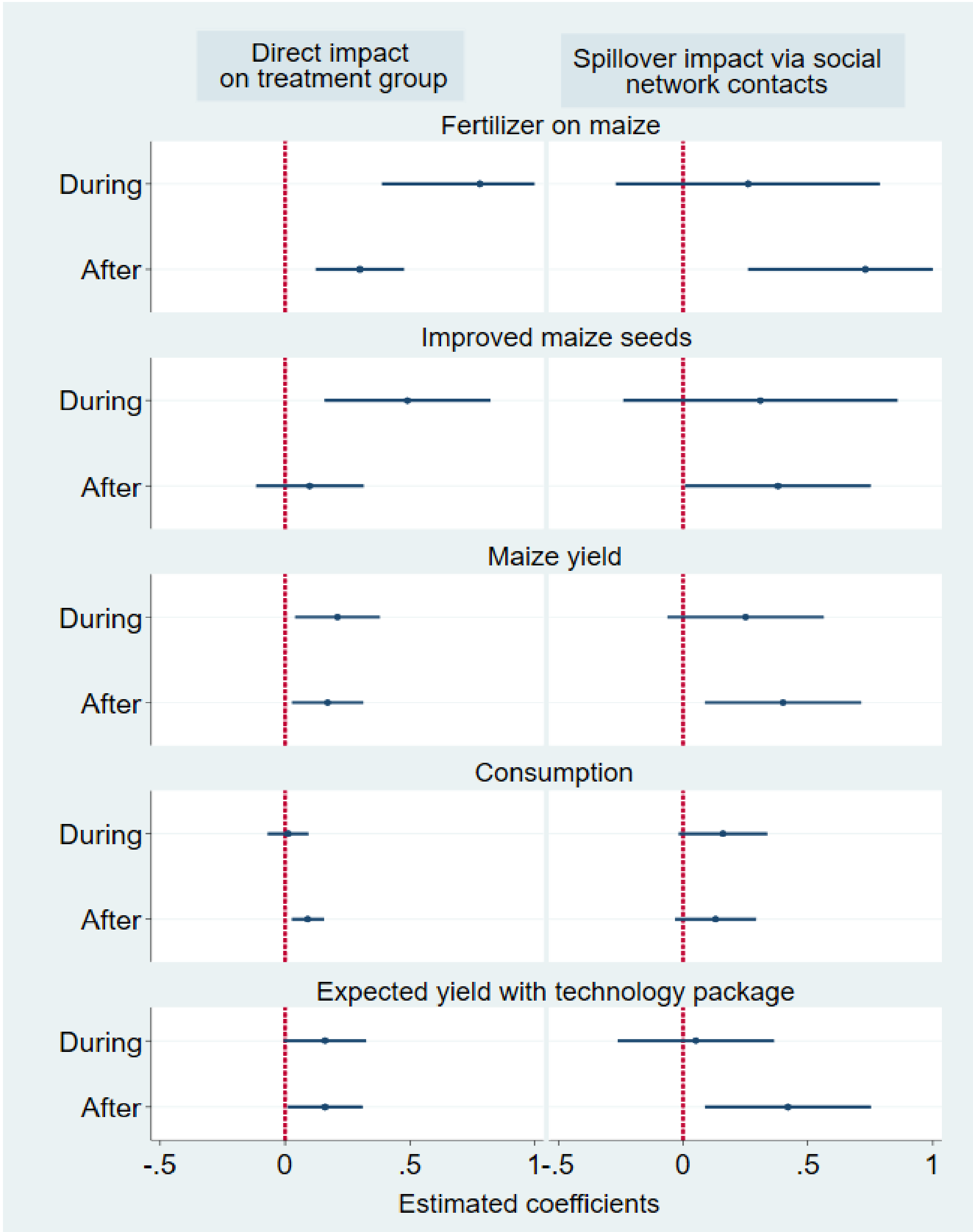

FIGURE 1—DIRECT AND SPILLOVER IMPACTS OF SUBSIDIES FOR GREEN REVOLUTION TECHNOLOGY

Notes: Results from 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 meticais. 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_{Dur}, α_{Aft}; those in right-hand column are σ_{Dur}, and σ_{Aft}. Lines represent 95% confidence intervals. Regression coefficients are presented in Appendix Table A4.

5.2 Magnitudes of Effects

Our estimated intent-to-treat (ITT) impacts are large and economically consequential. Given 28.8% compliance with treatment assignment, our ITT estimate of a 0.21 increase in log yields (a 19% yield increase) for subsidy-recipient households implies an undiluted treatment-on-the-treated (TOT) impact of a 66% yield gain. 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).[10] Hence farmers in our study area, who produced a bit less than 1 ton per hectare before the program, could have tripled their yields if they closed the gap. From this perspective, our results are well within the bounds of what is believed to be technologically feasible.

We find that in the post-subsidy period, network effects often exceed the direct impact of the subsidy. Since farmers with above median number of neighbors treated have on average 3.34 more farmers in their network who are treated, it is possible that the spillover impact exceeds the direct impact if farmers learn sufficiently from discussing agriculture with their neighbors. Even when summing the estimates of the direct and spillover impacts, we find a yield increment of 143%, which is high, but remains within the possible effects of closing the gap, estimated by Sadras *et al*. (2015).[11] Section 6.1 shows that the yield response to input use is very much within expectations from an agronomic perspective.

Finally, given that about 60% of household income comes from maize, the estimated consumption impacts are also in line with what is possible and what would be expected from Asian experience with Green Revolution technologies (Otsuka and Larson, 2016).

[10] The yield gap is the difference between the yields farmers obtain and what is technologically possible using improved seeds and fertilizers given farmers' soils and the weather conditions they face.
[11] An increase of 0.51 in the log is equivalent to a 41% increase as the ITT, and a 143% increase in the average treatment effect.

5.3 Alternate specification of network effects

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

5.3.1 Effects of number of subsidy beneficiaries in social network

Spillover effects are specified in our main regressions (Figure 1 and Table A4) 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 in such a 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. Figure 2 displays the spillover effect coefficients for the after period (σ_{Aft}), using the same flexible specification, for fertilizer use and maize yields.

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
Direct impacts on treatment group members	During	0.78*** [0.21]	0.51*** [0.15]	0.20** [0.088]	0.019 [0.043]	0.15* [0.086]
	After	0.31*** [0.092]	0.11 [0.11]	0.17** [0.072]	0.096*** [0.033]	0.16** [0.079]
Spillover impacts DURING subsidy period of having x contacts in treatment group	1 contact	-0.53* [0.31]	-0.19 [0.29]	0.14 [0.13]	0.052 [0.080]	0.059 [0.17]
	2 contacts	-0.025 [0.39]	0.12 [0.35]	0.33 [0.22]	0.21** [0.096]	0.12 [0.20]
	3 contacts	-0.57 [0.61]	0.20 [0.45]	0.44 [0.27]	0.084 [0.13]	0.056 [0.22]
	4 contacts	-0.079 [0.71]	0.68 [0.43]	0.28 [0.23]	0.11 [0.17]	-0.25 [0.28]
	5 contacts	-0.057 [0.54]	1.01 [0.52]	0.030 [0.28]	0.29 [0.15]	-0.18 [0.29]
Spillover impacts AFTER subsidy period of having x contacts in treatment group	1 contact	0.33* [0.18]	-0.29 [0.26]	0.18 [0.15]	0.069 [0.058]	-0.055 [0.19]
	2 contacts	0.95*** [0.25]	0.14 [0.27]	0.53*** [0.20]	0.18* [0.092]	0.44** [0.22]
	3 contacts	0.98*** [0.32]	0.21 [0.34]	0.48** [0.20]	0.16* [0.085]	0.15 [0.22]
	4 contacts	0.94** [0.46]	0.53 [0.37]	0.60*** [0.23]	0.27** [0.13]	0.24 [0.24]
	5 contacts	1.17*** [0.35]	0.66* [0.35]	0.39 [0.24]	0.31*** [0.12]	0.11 [0.28]
Observations		1,428	1,404	1,346	1,393	1,273

Notes: Data are from 2011, 2012, and 2013 follow-up surveys. Dependent variables are as in Figure 1. Regressions are based on modified version of equation 1 in main text, 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. Robust standard errors in brackets.

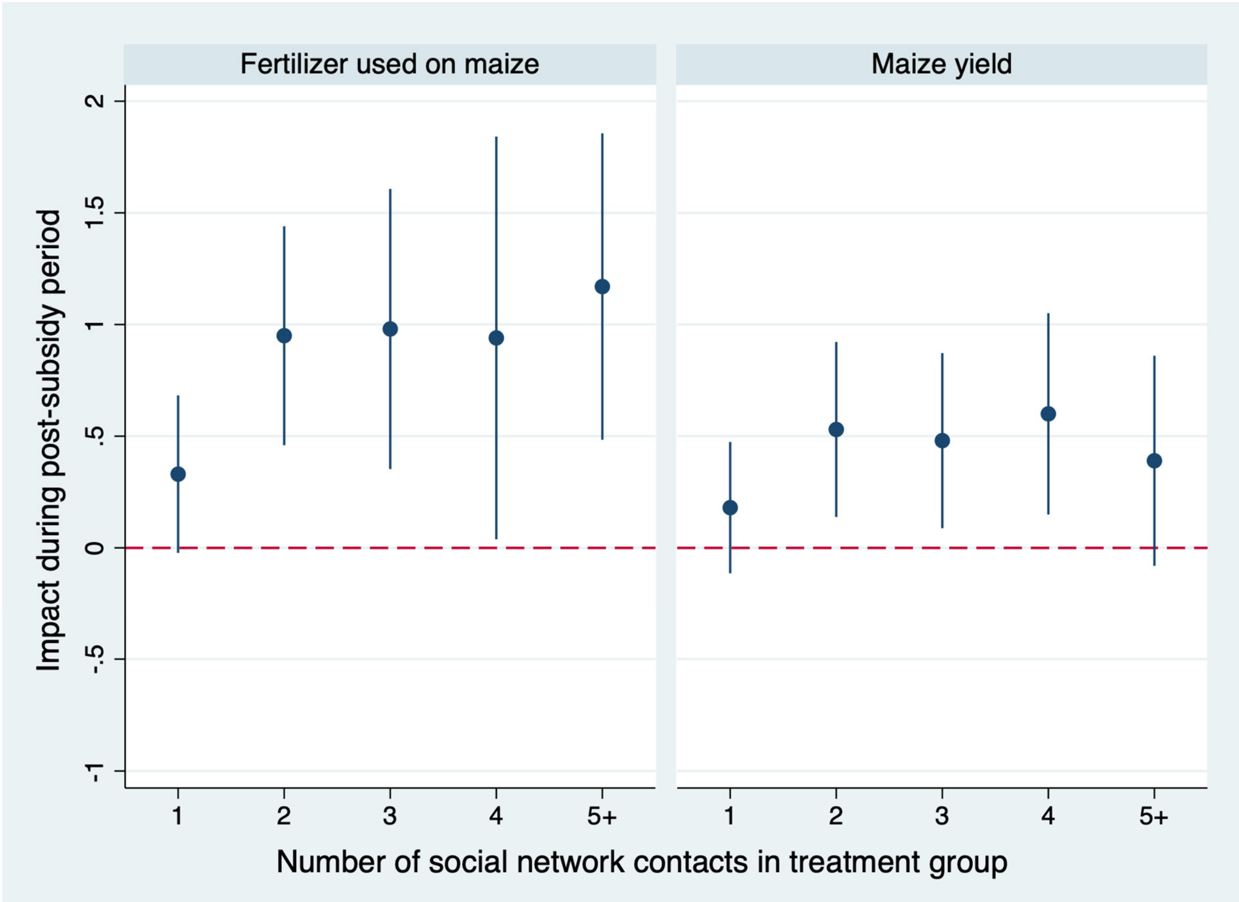

FIGURE 2—SPILLOVER EFFECTS BY NUMBER OF SOCIAL NETWORK CONTACTS IN TREATMENT GROUP

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

5.3.2 Separate social network effects for subsidy recipients and non-recipients

Spillovers are often thought of as impacts on those who did not receive the treatment themselves (the control group, subsidy non-recipients). 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_{Dur} and σ_{Aft}) 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 $Soc_{ic}Dur_t$ and $Soc_{ic}Aft_t$ are each interacted with the indicator for the treatment group ($Treat_{ic}$), and separately with an indicator for the control group ($Cont_{ic}$).

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
Direct Impacts	During (σ_{Dur})	0.83*** [0.24]	0.37* [0.21]	0.11 [0.11]	0.0028 [0.057]	0.11 [0.10]
	After (σ_{Aft})	0.31* [0.16]	0.055 [0.15]	0.12 [0.082]	0.073* [0.039]	0.18** [0.086]
Spillover Impacts on Control Group	During (σ_{Dur}^{Cont})	0.33 [0.31]	0.16 [0.30]	0.12 [0.17]	0.14 [0.10]	-0.012 [0.18]
	After (σ_{Aft}^{Cont})	0.75*** [0.24]	0.33 [0.25]	0.33* [0.17]	0.11 [0.076]	0.44** [0.17]
Spillover Impacts on Treatment Group	During (σ_{Dur}^{Treat})	0.19 [0.35]	0.47 [0.35]	0.37** [0.17]	0.17 [0.11]	0.11 [0.17]
	After (σ_{Aft}^{Treat})	0.72** [0.31]	0.44* [0.24]	0.47*** [0.18]	0.16 [0.099]	0.40** [0.19]
Observations		1,428	1,404	1,346	1,393	1,273

Notes: Variable definitions are as in Figure 1. Regressions are a modified version of in equation 1 in main text, with $Soc_{ic}Dur_t$ and $Soc_{ic}Aft_t$ each interacted with the indicator for the treatment group ($Treat_{ic}$), and separately with an indicator for the control group ($Cont_{ic}$). Robust standard errors in brackets.

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 novel technologies more productively in the initial subsidized year.

5.4 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. First, we directly observe that study participants report higher expected returns to the technology package when they are treated or have more than two treated social network contacts. Second, the increase in technology adoption, yield and consumption persists 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) is reasonable, as farmers may wait to fully observe outcomes of neighbors' experimentation before experimenting themselves (Foster and Rosenzweig, 1995). Altogether, these findings strongly suggest that the subsidy alleviates information imperfections related to 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, shared it with neighbors, or sold it to neighbors. We ask about fertilizer saving or sharing in our surveys, and find that it is quite rare: immediately following the subsidized 2010-11 season, the vast majority of respondents (88.8%) reported they had already used all the inputs for agriculture, 2.8% reported that they had not used it, and only 1.4% reported that they sold the inputs.[12] Even though it was an option, exactly zero farmers reported that they had given away

[12] Also, 1.4% declared that they used the inputs in some other way and 5.6% did not respond to this question

any of the inputs. Based on these data, there appears to be little scope for farmers to have shared their inputs with others. This is consistent with our qualitative observations during our presence in the field. 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 not significant (Appendix Table A6).

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 to others (Appendix Table A7 and Online Appendix G). Resource transfers are therefore unlikely to explain the large spillovers that we observe.

6. 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 and improved seeds, while costs include the cost of the subsidies to the government, as well as all logistical costs (calculated from detailed implementation budgets).[13]

The top panel of Table 5 displays the decomposition of benefits. Most studies, without a post-subsidy-period follow-up and a specific design to capture spillovers, would focus on estimating benefits accruing to direct subsidy beneficiaries during the subsidized period; we find that this only accounts for 10% of total benefits. But even when only accounting for this small minority of total benefits, the benefit-cost ratio would be 2.0. The remaining 90% of benefits accrues via spillovers from subsidized farmers to their social network contacts, as well as in post-subsidy periods. 69% of benefits occur through spillovers. 74% 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 2.0 to 20.5.

7. Conclusion

In sum, 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 as well as their social network contacts. 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

[13] Online Appendix H details calculation of the benefits and costs.

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, moving society closer to socially optimal levels of experimentation. When information constraints are important, well-designed public policy that successfully encourages experimentation can generate the sort of spillover-driven 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 once-off inducement to experiment and learn.

TABLE 5—INPUT SUBSIDY PROGRAM BENEFIT-COST ESTIMATES

A. Shares of benefits

	Subsidized year	Two years following the subsidy	All years
Direct effect	10%	21%	31%
Spillover effect (through social network contacts)	17%	53%	69%
Direct and indirect effects	26%	74%	100%

B. Benefit-Cost Ratios

	Subsidized year	Two years following the subsidy	All years
Direct effect	2.0	4.3	6.3
Indirect effect (through social network contacts)	3.4	10.8	14.2
Direct and indirect effects	5.4	15.1	20.5

Notes: Benefits are increases in value of additional maize yields, minus costs of additional fertilizer and improved seeds used for maize. Direct effects accrue from being randomly assigned to treatment group (being eligible for subsidy voucher oneself). Indirect (spillover) benefits 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.

As with all empirical work, subsequent studies should determine the generalizability of these results. It is important to note that prior investments in crop research led to the existence of improved seeds and fertilizers suitable for the conditions in our study area. In addition, a prior donor-funded effort led to availability of the technologies through a network of private agro-input dealers (Nagarajan 2015). Future research may find that effects of subsidies are attenuated in areas where available technologies are less suitable, or that are less accessible to input markets. Other dimensions along which subsidy effects may be heterogeneous include prior experience with modern inputs, geographical and climate conditions, crop types, and formal financial development. Policymakers should be cautious about expanding ISPs before future studies can measure direct impacts, post-subsidy persistence, and social network spillovers under different conditions, as guidance for locally-specific benefit-cost analyses.

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. 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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