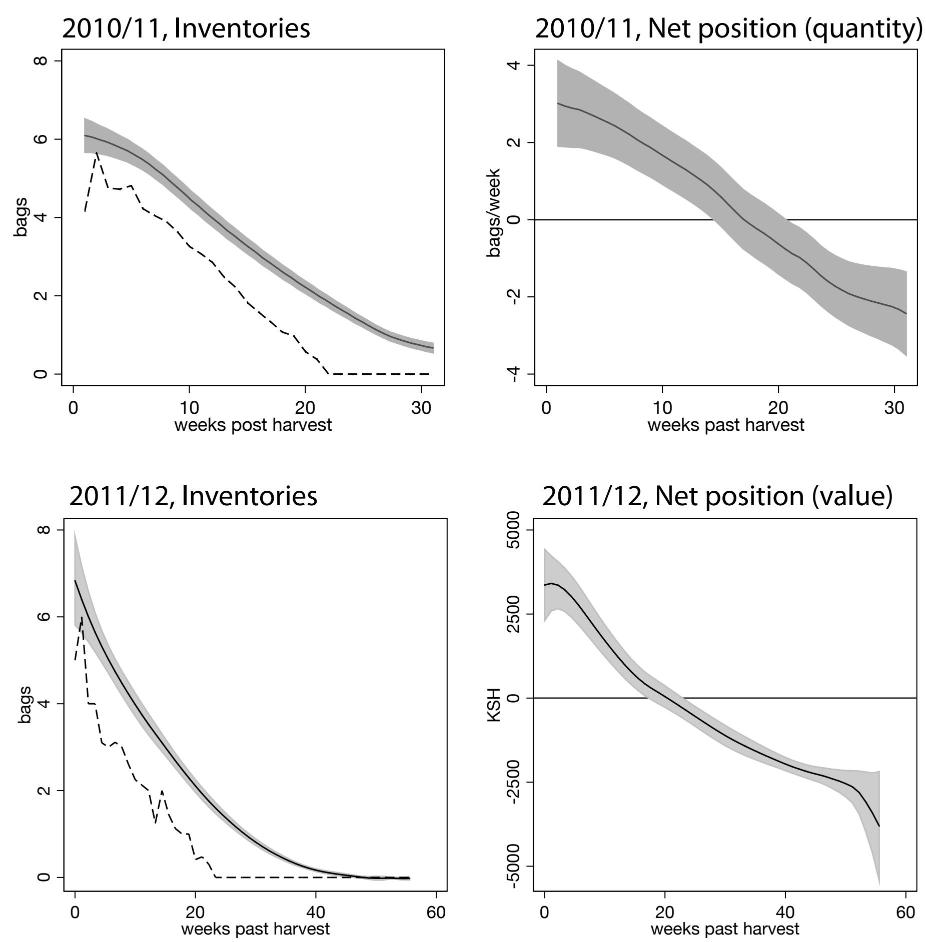
Supplementary Appendix

A Pilot Results

Figure A.1: **Pilot data on maize inventories and marketing decisions over time**, using data from two earlier pilot studies conducted with One Acre Fund in 2010/11 with 225 farmers (top row) and 2011/12 with 700 different farmers (bottom row). *Left panels*: inventories (measured in 90kg bags) as a function of weeks past harvest. The dotted line is the sample median, the solid line the mean (with 95% CI in grey). *Right panels*: average net sales position across farmers over the same period, with quantities shown for 2010/11 (quantity sold minus purchased) and values shown for 2011/12 (value of all sales minus purchases).



B Storage Costs, Knowledge of Price Increase, and Other Factors that may Limit Storage

There could be other reasons beyond credit constraints why farmer are not taking advantage of apparent arbitrage opportunities. The simplest explanations are that farmers do not know about the price increases, or that it is actually not profitable to store – i.e. arbitrage opportunities are actually much smaller than they appear because storage is costly. These costs could come in the form of losses to pests or moisture-related rotting, or they could come in the form of "network losses" to friends and family, since maize is stored in the home and is visible to friends and family, and there is often community pressure to share a surplus. Third, farmers could be highly impatient and thus unwilling to move consumption to future periods in any scenario. Finally, farmers might view storage as too risky an investment.

Evidence from pilot and baseline data, and from elsewhere in the literature, argues against several of these possibilities. We can immediately rule out an information story: farmers are well-aware that prices rise substantially throughout the year. When asked in our baseline survey about expectations for the subsequent season's price trajectory, the average farmer expected prices to increase by 107% in the nine months following the September 2012 harvest (which was actually an over-estimate of the realized price fluctuation that year).[54]

Second, pest-related losses appear surprisingly low in our setting, with farmers reporting losses from pests and moisture-related rotting of 2.5% for maize stored for six to nine months.[55] Similarly, the marginal costs associated with storing for these farmers are small (estimates suggest that the cost per bag is about 3.5% of the harvest-time price) and the fixed costs have typically already been paid (all farmers store at least some grain; note the positive initial inventories in Figure A.1), as grain in simply stored in the household or in small sheds previously built for the purpose.[56]

Third, while we cannot rule out impatience as a driver of low storage rates, extremely high discount rates would be needed to rationalize this behavior in light of the substantial prices increase seen over a short nine-month period.[57] Furthermore, farm households are observed to make many other investments with payouts far in the future (e.g. school fees), meaning that rates of time preference would also have to differ substantially across investments and goods. Finally, while discount factors are crucial for determining the optimal pattern of consumption over time, in the presence of functioning financial markets, one should be able to compare the relative return of an investment opportunity such as storage against the interest rate on credit and, if the interest rate on credit is lower, fund today's consumption through borrowing while still taking advantage of the higher-return investment opportunity.

[54]The 5th, 10th, and 25th percentiles of the distribution are a 33%, 56%, and 85% increase, respectively, suggesting that nearly all farmers in our sample expect substantial price increases.

[55]While low, these estimates of post-harvest losses are not out of line with those typically seen in the region. Kaminski and Christiaensen (2014), drawing on nationally representative LSMS-ISA household surveys from Uganda, Malawi, and Tanzania, find post-harvest losses ranging from 1.4-5.9% for the region. Ambler et al. (2018) estimate post-harvest losses in Malawi range between 5-12% among those who experience any losses. In a nearby study site in western Kenya, Aggarwal et al. (2017) find average post-harvest losses of 9%.

[56]Though note that Aggarwal et al. (2017) find that offering group-based grain storage can encourage greater storage.

[57]Given a minimum price increase of 40%, post-harvest losses of 2.5%, and storage costs of 3.5% of price, an individual would have to discount the 9-month future by over 33% to make the decision to sell at harvest rational under no other constraints. Given the distribution of estimated discount rates from a time preference question asked at baseline, this would apply to only 12% of our sample.

Fourth, existing literature shows that for households that are both consumers and producers of grain, aversion to price risk should motivate *more* storage rather than less: the worst state of the world for these households is a huge price spike during the lean season, which should motivate "precautionary" storage (Saha and Stroud, 1994; Park, 2006).

Costs associated with network-related losses appear a more likely explanation for an unwillingness to store substantial amounts of grain. Existing literature suggests that community pressure is one explanation for limited informal savings (Dupas and Robinson, 2013; Brune et al., 2011), and in focus groups farmers often told us something similar about stored grain (itself a form of savings). Our main credit intervention might also provide farmers a way to shield stored maize from their network. To further test this hypothesis, in the first year of the experiment we add an additional treatment arm to determine whether this shielding effect is substantial on its own.

C Treatment Heterogeneity

Table C.1: **Heterogeneity in Treatment Effects.** Heterogeneity in treatment effects, as pre-specified in pre-analysis plan. All variables are from the baseline run in 2012 prior to Year 1. Because those who are new to the sample in Year 2 are missing baseline variables, the specification presented below only presents Year 1 results, for which we have full baseline data. In the "Takeup" column, an indictor for loan take-up is regressed on the standardized baseline heterogeneity variable (sample restricted in this column to Round 1 observations for the treatment group). For "Inv" (inventories), "Rev" (net revenues), and "Cons" (log household consumption), the outcome variable is regressed on a treatment indicator, the standardized baseline heterogeneity variable, and an interaction term. "Impatience" is the percent allocated to the early period (versus later period) in standard time preference questions, such that a greater value represents less patience. "Children" is number of school-aged children in the household.

	Impatience				Children			
	(1) Takeup	(2) Inv	(3) Rev	(4) Cons	(5) Takeup	(6) Inv	(7) Rev	(8) Cons
Treat		0.523***	271.385	0.005		0.591***	325.002	0.015
		(0.164)	(290.349)	(0.033)		(0.153)	(285.796)	(0.032)
Main	0.032**	-0.035	-227.072	0.032	0.032*	0.270**	-161.354	0.132***
	(0.014)	(0.172)	(182.452)	(0.024)	(0.017)	(0.109)	(221.310)	(0.025)
Interact		0.070	-40.027	-0.015		-0.197	-100.754	-0.011
		(0.194)	(225.823)	(0.028)		(0.146)	(268.407)	(0.030)
Observations	882	3819	3779	3775	879	3806	3765	3764
R squared	0.00	0.35	0.01	0.00	0.00	0.35	0.01	0.04

Table C.2: **Heterogeneity in Treatment Effects.** Heterogeneity in treatment effects, as pre-specified in pre-analysis plan. All variables are from the baseline run in 2012 prior to Year 1. Because those who are new to the sample in Year 2 are missing baseline variables, the specification presented below only presents Year 1 results, for which we have full baseline data. In the "Takeup" column, an indictor for loan take-up is regressed on the standardized baseline heterogeneity variable (sample restricted in this column to Round 1 observations for the treatment group). For "Inv" (inventories), "Rev" (net revenues), and "Cons" (log household consumption), the outcome variable is regressed on a treatment indicator, the standardized baseline heterogeneity variable, and an interaction term. "Wealth" is the combined value of total assets, livestock, and cash savings. "Early Sales" is the percentage of 2011-2012 total season sales that occurred prior to January 1, 2012 (a variable only defined for those who sold anything in the 2011-2012 season)

	Wealth				Early Sales			
	(1) Takeup	(2) Inv	(3) Rev	(4) Cons	(5) Takeup	(6) Inv	(7) Rev	(8) Cons
Treat		0.546***	299.323	0.010		0.649***	422.990	0.009
		(0.150)	(278.972)	(0.031)		(0.223)	(398.791)	(0.042)
Main	0.019	0.742***	439.614**	0.175***	-0.009	-0.824***	-1069.976***	-0.069*
	(0.019)	(0.119)	(205.278)	(0.026)	(0.023)	(0.165)	(303.858)	(0.035)
Interact		0.024	476.536*	0.012		0.400*	674.054*	0.015
		(0.150)	(267.076)	(0.033)		(0.204)	(366.003)	(0.040)
Observations	862	3726	3689	3685	437	1884	1871	1874
R squared	0.00	0.39	0.02	0.09	0.00	0.38	0.02	0.02

Table C.3: **Heterogeneity in Treatment Effects.** Heterogeneity in treatment effects, as pre-specified in pre-analysis plan. All variables are from the baseline run in 2012 prior to Year 1. Because those who are new to the sample in Year 2 are missing baseline variables, the specification presented below only presents Year 1 results, for which we have full baseline data. In the "Takeup" column, an indictor for loan take-up is regressed on the standardized baseline heterogeneity variable (sample restricted in this column to Round 1 observations for the treatment group). For "Inv" (inventories), "Rev" (net revenues), and "Cons" (log household consumption), the outcome variable is regressed on a treatment indicator, the standardized baseline heterogeneity variable, and an interaction term. "Price Expect" is the percentage expected change in price expected between September 2012 and June 2013.

	Price Expect			
	(1) Takeup	(2) Inv	(3) Rev	(4) Cons
Treat		0.499***	232.478	0.008
		(0.163)	(297.093)	(0.034)
Main	-0.010	-0.001	-90.122	-0.018
	(0.016)	(0.126)	(191.814)	(0.023)
Interact		-0.034	6.407	0.014
		(0.146)	(227.614)	(0.028)
Observations	864	3746	3707	3706
R squared	0.00	0.35	0.01	0.00

D Effects of Loan Timing

In Year 1, the loan was (randomly) offered at two different times: one in October, immediately following harvest (T1) and the other in January, immediately before school fees are due (T2). Splitting apart the two loan treatment arms in Year 1, results provide some evidence that the timing of the loan affects the returns to capital in this setting. As shown in Figure D.1 and Table D.1, point estimates suggest that those offered the October loan held more in inventories, reaped more in net revenues, and had higher overall consumption. Overall effects on net revenues are about twice as high as pooled estimates, and are now significant at the 5% level (Column 5 of Table D.1), and we can reject that treatment effects are equal for T1 and T2 ($p = 0.04$). Figure D.2 shows non-parametric estimates of differences in net revenues over time among the different treatment groups. Seasonal differences are again strong, and particularly strong for T1 versus control.

Why might the October loan have been more effective than the January loan? Note that while we are estimating the intent-to-treat (ITT) and thus that differences in point estimates could in principle be driven by differences in take-up, these latter differences are probably not large enough to explain the differential effects. For instance, "naive" average treatment effect estimates that rescale the ITT coefficients by the take-up rates (70% versus 60%) still suggest substantial differences in effects between T1 and T2. A more likely explanation is that the January loan came too late to be as useful: farmers in the T2 group were forced to liquidate some of their inventories before the arrival of the loan, and thus had less to sell in the months when prices rose. This would explain why inventories began lower, and why T2 farmers appear to be selling more during the immediate post-harvest months than T1 farmers. Nevertheless, they sell less than control farmers during this period and store more, likely because qualifying for the January loan meant carrying sufficient inventory until that point.

Figure D.1: **Year 1 Treatment effects by loan timing**. Plots shows how average inventories, net revenues, and log total househoeld consumption evolve over the study period for farmers assigned to T1 (blue line), T2 (red line), and C (black dashed line), as estimated with fan regressions.

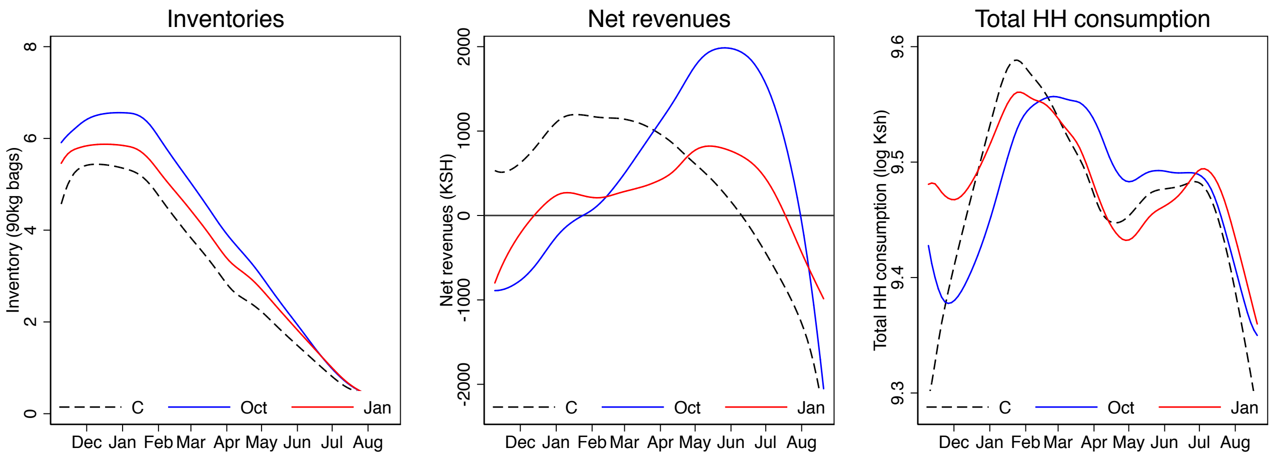


Figure D.2: **Year 1 Revenue treatment effects by loan timing**. Plots show the difference in net revenues over time for T1 versus C (left), T2 versus C (center), and T1 versus T2 (right), with the bootstrapped 95% confidence interval shown in light grey and the 90% confidence interval shown in dark grey.

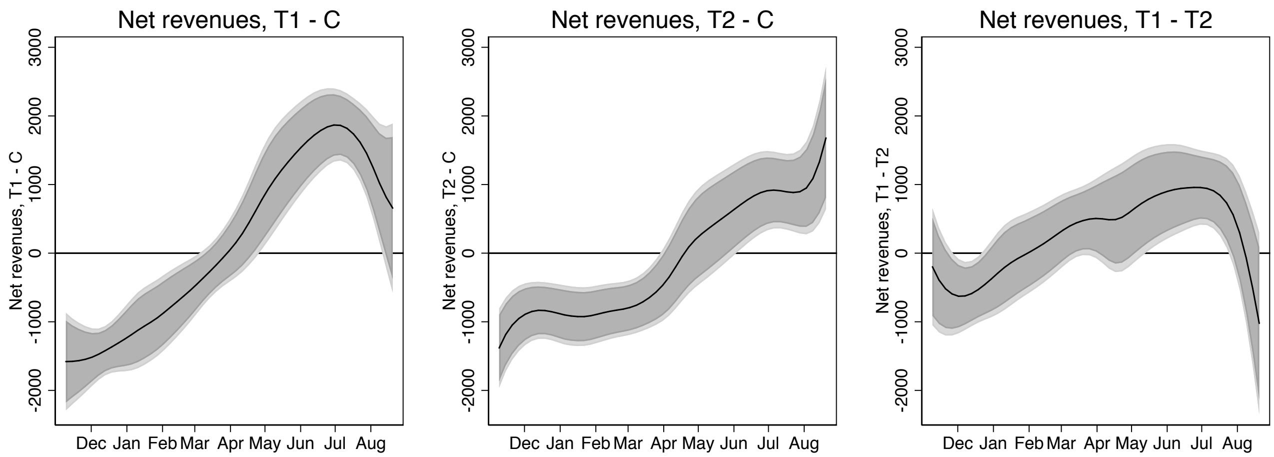


Table D.1: **Year 1 Results by Loan Timing.** Regressions include round-year fixed effects and strata fixed effects, with errors clustered at the group level.

	Inventories		Prices		Revenues		Consumption	
	(1) Pooled	(2) By round	(3) Purchase price	(4) Sales prices	(5) Pooled	(6) By round	(7) Pooled	(8) By round
T1	0.77***		-47.81**	10.51	541.95**		0.04	
	(0.13)		(23.20)	(129.67)	(248.78)		(0.03)	
T2	0.46***		2.47	-34.93	36.03		0.01	
	(0.13)		(22.47)	(114.55)	(248.15)		(0.03)	
T1 - Round 1		1.25***				-1218.96***		-0.00
		(0.27)				(353.43)		(0.05)
T1 - Round 2		0.91***				924.50*		0.08*
		(0.19)				(512.50)		(0.05)
T1 - Round 3		0.18				1840.70***		0.04
		(0.13)				(483.92)		(0.04)
T2 - Round 1		0.54**				-951.27***		-0.01
		(0.27)				(347.35)		(0.05)
T2 - Round 2		0.65***				156.58		0.01
		(0.16)				(503.66)		(0.05)
T2 - Round 3		0.18				851.70**		0.02
		(0.12)				(410.53)		(0.04)
Observations	3816	3816	1914	1429	3776	3776	3596	3596
Mean of Dep Variable	3.03	3.03	2936.14	2991.23	501.64	501.64	8.02	8.02
SD of Dep Variable	3.73	3.73	425.20	2007.53	6217.09	6217.09	0.66	0.66
R squared	0.49	0.50	0.30	0.07	0.13	0.14	0.21	0.21
T1 = T2 (pval)	0.02		0.04		0.04		0.19	

E Secondary Outcomes

Table E.1: **Non-Farm Profit** Non-farm Profit is the household's profit from non-farm activities in the last month (Ksh).

	Y1		Y2		Pool	
	(1) Overall	(2) By Intensity	(3) Overall	(4) By Intensity	(5) Overall	(6) By Intensity
Treat	197.30	-150.81	-127.45	-309.72	-35.28	-264.58
	(170.57)	(272.18)	(164.75)	(304.34)	(127.06)	(236.14)
Hi		-145.48		-28.99		-55.22
		(323.59)		(314.79)		(232.59)
Treat * Hi		489.84		256.78		323.31
		(335.08)		(354.45)		(287.61)
Observations	1305	1305	2938	2938	4243	4243
Mean DV	984.02	1056.54	1359.52	1337.37	1270.51	1269.33
R squared	0.00	0.00	0.00	0.00	0.00	0.00

Table E.2: **Non-Farm Hours** Hours Non-Farm is the number of hours worked by the household in a non-farm businesses run by the household in the last 7 days.

	Y1		Y2		Pool	
	(1) Overall	(2) By Intensity	(3) Overall	(4) By Intensity	(5) Overall	(6) By Intensity
Treat	1.40	0.73	0.77	-0.67	0.96	-0.25
	(1.59)	(1.71)	(1.23)	(2.07)	(0.99)	(1.54)
Hi		2.40		1.14		1.41
		(3.05)		(1.62)		(1.20)
Treat * Hi		0.84		2.04		1.69
		(2.66)		(2.37)		(1.81)
Observations	1305	1305	2942	2942	4247	4247
Mean DV	11.90	10.27	13.60	12.49	13.20	11.95
R squared	0.00	0.00	0.00	0.01	0.00	0.01

Table E.3: **Salaried Employment.** Hours Salary is the total number of hours worked by household members in a salaried position.

	Y1		Y2		Pool	
	(1) Overall	(2) By Intensity	(3) Overall	(4) By Intensity	(5) Overall	(6) By Intensity
Treat	0.47	0.86	0.18	-2.07	0.30	-0.96
	(1.42)	(3.46)	(1.16)	(2.86)	(0.90)	(2.41)
Hi		0.17		-1.71		-1.16
		(2.86)		(2.05)		(2.09)
Treat * Hi		-0.56		3.29		1.82
		(3.71)		(3.11)		(2.54)
Observations	1295	1295	2012	2012	3307	3307
Mean DV	11.16	10.70	6.74	7.33	8.12	8.35
R squared	0.00	0.00	0.01	0.01	0.01	0.01

Table E.4: **Average Wage** Avg Wage is the average monthly wage for those household members who are salaried.

	Y1		Y2		Pool	
	(1) Overall	(2) By Intensity	(3) Overall	(4) By Intensity	(5) Overall	(6) By Intensity
Treat	2293.22	-908.20	-333.47	1822.23	1296.43	-743.50
	(1720.62)	(3021.61)	(1620.91)	(4004.50)	(1243.91)	(1988.44)
Hi		-1843.78		-1092.62		-1476.21
		(3017.19)		(2511.99)		(1962.38)
Treat * Hi		4556.76		-2495.62		2933.25
		(3284.10)		(4534.13)		(2212.83)
Observations	284	284	135	135	419	419
Mean DV	11486.64	12087.50	5232.03	5682.00	8984.80	9278.07
R squared	0.02	0.02	0.02	0.04	0.10	0.10

Table E.5: **Food Expenditure** Food Expenditure is the household's expenditure on food purchases in the last month (Ksh).

	Y1		Y2		Pool	
	(1) Overall	(2) By Intensity	(3) Overall	(4) By Intensity	(5) Overall	(6) By Intensity
Treat	-94.37	-205.03	40.18	-359.47*	-33.21	-285.49
	(152.11)	(204.06)	(167.47)	(191.29)	(112.34)	(174.62)
Hi		182.75		-197.90		-15.19
		(199.39)		(243.14)		(168.73)
Treat * Hi		147.21		566.21*		356.35
		(258.24)		(300.90)		(229.03)
Observations	3817	3817	2919	2919	6736	6736
Mean DV	6665.50	6611.09	7430.94	7617.81	7057.83	7120.57
R squared	0.01	0.01	0.00	0.01	0.03	0.03

Table E.6: **Maize Eaten** Maize Eaten is the household's consumption of maize (in goros, 2.2kg tins) over the past 7 days.

	Y1		Y2		Pool	
	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	By Intensity	Overall	By Intensity	Overall	By Intensity
Treat	-0.07	-0.32	-0.02	-0.41	-0.05	-0.37
	(0.14)	(0.24)	(0.17)	(0.34)	(0.11)	(0.25)
Hi		-0.07		-0.10		-0.09
		(0.21)		(0.27)		(0.18)
Treat * Hi		0.36		0.55		0.45
		(0.29)		(0.41)		(0.30)
Observations	3844	3844	2947	2947	6791	6791
Mean DV	5.48	5.55	5.55	5.75	5.52	5.65
R squared	0.01	0.01	0.00	0.01	0.00	0.01

Table E.7: **School Fees Paid.** School Fees Paid are the expenditure on school fees over the past month (Ksh).

	Y1		Y2		Pool	
	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	By Intensity	Overall	By Intensity	Overall	By Intensity
Treat	150.82	31.71	213.27	-329.82	191.55	-94.21
	(118.32)	(214.96)	(377.33)	(609.10)	(186.63)	(237.24)
Hi		-272.68		-662.03		-485.39
		(203.93)		(562.92)		(320.02)
Treat * Hi		178.21		773.26		414.02
		(241.47)		(679.59)		(282.05)
Observations	3867	3867	2905	2905	6772	6772
Mean DV	1217.27	1369.71	3851.29	4077.54	2560.84	2740.01
R squared	0.05	0.05	0.03	0.03	0.09	0.09

Table E.8: **Happiness Index.** Happy is an index for the following question: "Taking everything together, would you say you are very happy (3), somewhat happy (2), or not happy (1)?"

	Y1		Y2		Pool	
	(1) Overall	(2) By Intensity	(3) Overall	(4) By Intensity	(5) Overall	(6) By Intensity
Treat	0.07**	0.04	0.01	0.03	0.04*	0.03
	(0.03)	(0.05)	(0.03)	(0.04)	(0.02)	(0.03)
Hi		-0.03		-0.02		-0.02
		(0.05)		(0.04)		(0.03)
Treat * Hi		0.04		-0.03		0.01
		(0.06)		(0.05)		(0.03)
Observations	3870	3870	2969	2969	6839	6839
Mean DV	2.57	2.58	2.68	2.68	2.63	2.63
R squared	0.01	0.01	0.00	0.00	0.01	0.01

E.1 Consumption: All, Non-Maize, Non-Food

Table E.9: **Consumption (All)**

	Y1		Y2		Pool	
	(1) Overall	(2) By Intensity	(3) Overall	(4) By Intensity	(5) Overall	(6) By Intensity
Treat	0.00	0.01	0.07*	-0.05	0.04	-0.01
	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.02)
Hi		-0.00		-0.08		-0.05
		(0.05)		(0.05)		(0.04)
Treat * Hi		-0.01		0.17***		0.07*
		(0.05)		(0.06)		(0.04)
Observations	3792	3792	2944	2944	6736	6736
Mean DV	9.48	9.47	9.61	9.65	9.55	9.56
R squared	0.00	0.00	0.01	0.02	0.02	0.03

Table E.10: **Consumption (Non-Maize)**

	Y1		Y2		Pool	
	(1) Overall	(2) By Intensity	(3) Overall	(4) By Intensity	(5) Overall	(6) By Intensity
Treat	-0.01	-0.01	0.07	-0.04	0.03	-0.02
	(0.03)	(0.05)	(0.04)	(0.04)	(0.03)	(0.03)
Hi		0.00		-0.07		-0.04
		(0.06)		(0.05)		(0.05)
Treat * Hi		0.00		0.15**		0.06
		(0.06)		(0.05)		(0.04)
Observations	3808	3808	2947	2947	6755	6755
Mean DV	9.50	9.49	9.62	9.65	9.56	9.57
R squared	0.00	0.00	0.01	0.02	0.02	0.02

Table E.11: **Consumption (Non-Food)**

	Y1		Y2		Pool	
	(1) Overall	(2) By Intensity	(3) Overall	(4) By Intensity	(5) Overall	(6) By Intensity
Treat	-0.02	-0.01	0.12*	-0.02	0.05	-0.01
	(0.06)	(0.08)	(0.07)	(0.08)	(0.04)	(0.04)
Hi		0.01		-0.07		-0.04
		(0.11)		(0.08)		(0.08)
Treat * Hi		-0.01		0.20*		0.08
		(0.09)		(0.09)		(0.05)
Observations	3808	3808	2945	2945	6753	6753
Mean DV	8.68	8.64	8.81	8.81	8.74	8.73
R squared	0.00	0.00	0.02	0.02	0.02	0.02

F Long-Run Follow-up (LRFU) Survey Results

The Long-Run Follow-Up (LRFU) survey was run Nov-Dec 2015. Results presented in this appendix show the limited effects of the loan on long-run outcomes.

F.1 Long-Run Main Effects

Table F.1: **LRFU 2014-2015 Outcomes:** Effect of Year 1 (2012-2013) and Year 2 (2013-2014) treatment on Year 3 (2014-2015) outcomes. The "Year 1" column contains observations that were in the sample in the Year 1 study, the "Year 2" column contains observations that were in the sample in the Year 2 study, and the "Both" column contains the (select) subset of respondents who were in both samples.* "Y1" refers to treatment in Year 1, while "Y2" refers to treatment in Year 2. "2014 Harvest" is the size of harvest in 90kg bags. "Net Sales" is the total number of 90kg bags sold - the total number of 90kg bags purchased between the 2014 long-rains harvest and 2015 long-rains harvest. "Percent Sold Lean" is the percentage of total sales completed from January onward. "Percent Purchased Harvest" is the percentage of total purchases completed prior to January. "Revenues" are the net revenues from all maize sales and purchases from the 2014 long-rains harvest to the 2015 long-rains harvest.

	Net Sales			Percent Sold Lean			Percent Purchased Harvest			Revenues		
	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both
Treat Y1	0.31		-0.01	0.04		-0.02	-0.02		0.09	350.50		-763.60
	(0.35)		(0.59)	(0.05)		(0.09)	(0.03)		(0.07)	(950.10)		(1854.40)
Treat Y2		0.29	0.29		-0.03	-0.05		-0.03	0.01		1286.62	1330.40
		(0.35)	(0.61)		(0.04)	(0.10)		(0.04)	(0.07)		(1094.42)	(1777.33)
Treat Y1*Y2			0.21			0.10			-0.10			1126.71
			(0.80)			(0.12)			(0.09)			(2510.70)
Observations	979	937	557	532	534	327	724	665	399	979	938	558
R squared	0.00	0.00	0.00	0.00	0.01	0.00	0.02	0.00	0.05	0.00	0.00	0.01
MDV	-0.10	0.35	0.46	0.60	0.64	0.64	0.26	0.24	0.20	397.23	1052.01	1422.30

*Note that differential attrition from Year 1 to Year 2 means that those in the "Both" column are a select subsample. "Y1" should be interpreted with particular caution in this column, given the possibility that treatment in year 1 affected selection into this sample and may therefore no longer represent a causal effect. While "Y2" was re-randomized among the remaining sample and therefore represents a causal effect, it should be remembered that this the causal effect among a specific subset of respondents.

Table F.2: **LRFU 2014-2015 Total Sales and Purchases:** Effect of Year 1 (2012-2013) and Year 2 (2013-2014) treatment on total Year 3 (2014-2015) sales. The "Year 1" column contains observations that were in the sample in the Year 1 study, the "Year 2" column contains observations that were in the sample in the Year 2 study, and the "Both" column contains the (select) subset of respondents who were in both samples.* "Y1" refers to treatment in Year 1, while "Y2" refers to treatment in Year 2. Amounts are in 90 kg bag units and values are in Ksh.

	Amount Sold			Value Sold			Amount Purchased			Value Purchased		
	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both
Treat Y1	0.10		0.01	557.96		252.96	0.08		0.20	298.39		407.96
	(0.23)		(0.47)	(645.31)		(1363.64)	(0.15)		(0.25)	(452.26)		(726.89)
Treat Y2		0.17	-0.12		338.96	-236.18		-0.23	-0.33		-811.94	-1274.11
		(0.22)	(0.55)		(670.48)	(1534.45)		(0.17)	(0.28)		(531.18)	(792.22)
Treat Y1*Y2			0.29			773.24			0.13			829.11
			(0.67)			(1893.17)			(0.35)			(1010.21)
Observations	979	935	555	979	936	556	978	938	557	978	938	557
R squared	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.01	0.01	0.01	0.01	0.01
MDV	2.01	2.13	2.26	5646.07	6342.74	6387.60	1.90	1.86	1.72	5560.79	5590.23	5220.76

*Note that differential attrition from Year 1 to Year 2 means that those in the "Both" column are a select subsample. "Y1" should be interpreted with particular caution in this column, given the possibility that treatment in year 1 affected selection into this sample and may therefore no longer represent a causal effect. While "Y2" was re-randomized among the remaining sample and therefore represents a causal effect, it should be remembered that this the causal effect among a specific subset of respondents.

Table F.3: **LRFU 2014-2015 Sales by Season:** Effect of Year 1 (2012-2013) and Year 2 (2013-2014) treatment on Year 3 (2014-2015) sales. The "Year 1" column contains observations that were in the sample in the Year 1 study, the "Year 2" column contains observations that were in the sample in the Year 2 study, and the "Both" column contains the (select) subset of respondents who were in both samples.* "Y1" refers to treatment in Year 1, while "Y2" refers to treatment in Year 2. Amounts are in 90 kg bag units and values are in Ksh.

	Harvest Amount			Harvest Value			Lean Amount			Lean Value		
	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both
Treat Y1	0.03		0.25	77.46		530.06	0.22		0.16	679.47		392.38
	(0.09)		(0.16)	(243.35)		(481.72)	(0.20)		(0.41)	(574.93)		(1155.90)
Treat Y2		0.18**	0.22		334.68	600.49		0.04	0.06		303.79	115.41
		(0.08)	(0.21)		(221.93)	(603.64)		(0.20)	(0.49)		(568.03)	(1307.93)
Treat Y1*Y2			-0.22			-572.62			0.05			513.65
			(0.24)			(707.79)			(0.60)			(1676.81)
Observations	980	937	555	980	935	556	981	937	557	981	935	557
R squared	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
MDV	0.52	0.46	0.36	1346.28	1267.63	1079.90	1.34	1.53	1.49	3974.15	4383.35	4354.60

*Note that differential attrition from Year 1 to Year 2 means that those in the "Both" column are a select subsample. "Y1" should be interpreted with particular caution in this column, given the possibility that treatment in year 1 affected selection into this sample and may therefore no longer represent a causal effect. While "Y2" was re-randomized among the remaining sample and therefore represents a causal effect, it should be remembered that this the causal effect among a specific subset of respondents.

Table F.4: **LRFU Purchases by Season:** Effect of Year 1 (2012-2013) and Year 2 (2013-2014) treatment on Year 3 (2014-2015) purchases. The "Year 1" column contains observations that were in the sample in the Year 1 study, the "Year 2" column contains observations that were in the sample in the Year 2 study, and the "Both" column contains the (select) subset of respondents who were in both samples.* "Y1" refers to treatment in Year 1, while "Y2" refers to treatment in Year 2. Amounts are in 90 kg bag units and values are in Ksh.

	Harvest Amount			Harvest Value			Lean Amount			Lean Value		
	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both
Treat Y1	-0.04		0.17	-149.68		347.10	0.10		-0.03	370.11		-294.98
	(0.09)		(0.15)	(233.61)		(375.77)	(0.12)		(0.20)	(356.84)		(628.83)
Treat Y2		-0.08	-0.01		-298.29	-146.51		-0.09	-0.31		-279.60	-1092.92
		(0.08)	(0.17)		(215.31)	(406.71)		(0.13)	(0.21)		(416.36)	(668.77)
Treat Y1*Y2			-0.19			-370.52			0.34			1432.54
			(0.20)			(494.05)			(0.27)			(869.14)
Observations	977	941	557	977	940	557	982	939	559	979	938	558
R squared	0.01	0.00	0.02	0.01	0.00	0.02	0.00	0.01	0.01	0.00	0.01	0.01
MDV	0.58	0.52	0.44	1484.23	1317.58	1144.34	1.29	1.25	1.27	3922.78	3926.80	4040.25

*Note that differential attrition from Year 1 to Year 2 means that those in the "Both" column are a select subsample. "Y1" should be interpreted with particular caution in this column, given the possibility that treatment in year 1 affected selection into this sample and may therefore no longer represent a causal effect. While "Y2" was re-randomized among the remaining sample and therefore represents a causal effect, it should be remembered that this the causal effect among a specific subset of respondents.

Table F.5: **LRFU 2015 Harvest and Input Use:** Effect of Year 1 (2012-2013) and Year 2 (2013-2014) treatment on 2015 LR harvest and input usage. The "Year 1" column contains observations that were in the sample in the Year 1 study, the "Year 2" column contains observations that were in the sample in the Year 2 study, and the "Both" column contains the (select) subset of respondents who were in both samples.* "Y1" refers to treatment in Year 1, while "Y2" refers to treatment in Year 2. Harvests are in 90kg bag units. Non-labor input expenditure are the amount spent in Ksh on all fertilizers, hybrid seeds, DAP, CAN, and other physical inputs excluding labor. Labor person-days record the number of person-days of labor applied. All results are for maize plots only.

	Labor Person-Days			Non-Labor Input Expenditure			2015 Harvest		
	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both
Treat Y1	-4.76		-13.76	18.46		315.04	-0.22		-1.53*
	(5.98)		(9.85)	(213.39)		(393.59)	(0.56)		(0.92)
Treat Y2		-9.66	-16.38		122.23	-153.46		0.92	-0.42
		(7.04)	(13.00)		(194.98)	(404.36)		(0.59)	(0.94)
Treat Y1*Y2			14.63			402.65			2.39*
			(15.84)			(526.04)			(1.27)
Observations	979	940	560	978	940	559	987	946	561
R squared	0.01	0.00	0.06	0.01	0.00	0.01	0.00	0.00	0.02
MDV	126.15	131.48	142.58	2620.61	2271.07	2001.67	9.78	9.97	10.95

*Note that differential attrition from Year 1 to Year 2 means that those in the "Both" column are a select subsample. "Y1" should be interpreted with particular caution in this column, given the possibility that treatment in year 1 affected selection into this sample and may therefore no longer represent a causal effect. While "Y2" was re-randomized among the remaining sample and therefore represents a causal effect, it should be remembered that this the causal effect among a specific subset of respondents.

Table F.6: **LRFU 2015 Food Consumption, Food Expenditure, Total Consumption, and Happiness:** Effect of Year 1 (2012-2013) and Year 2 (2013-2014) treatment on food consumption, expenditure, total consumption, and happiness. The "Year 1" column contains observations that were in the sample in the Year 1 study, the "Year 2" column contains observations that were in the sample in the Year 2 study, and the "Both" column contains the (select) subset of respondents who were in both samples.* "Y1" refers to treatment in Year 1, while "Y2" refers to treatment in Year 2. Maize Eaten in the past week in 2kg "goros." Food expenditure is the value of maize purchases, own production consumed, and gifts given to others over the past 30 days. HH consumption is the total household consumption (logged) over the past 30 days. Happy is an index for the following question: "Taking everything together, would you say you are very happy (3), somewhat happy (2), or not happy (1)?"

	Maize Eaten			Food Expenditure			HH Consumption			Happy		
	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both
Treat Y1	-0.11		0.43	40.82		-124.26	-0.03		-0.00	0.10**		0.05
	(0.19)		(0.38)	(247.76)		(492.87)	(0.05)		(0.10)	(0.05)		(0.08)
Treat Y2		-0.26	-0.13		99.58	-97.26		0.04	0.08		0.01	0.00
		(0.22)	(0.41)		(251.35)	(556.87)		(0.05)	(0.11)		(0.04)	(0.10)
Treat Y1*Y2			-0.47			254.32			-0.09			-0.03
			(0.54)			(658.28)			(0.13)			(0.12)
Observations	976	937	554	977	939	557	976	939	556	985	945	560
R squared	0.00	0.00	0.01	0.02	0.00	0.02	0.01	0.00	0.01	0.01	0.00	0.01
MDV	5.68	5.74	5.51	6840.11	6786.12	6928.43	9.50	9.47	9.49	2.40	2.47	2.48

*Note that differential attrition from Year 1 to Year 2 means that those in the "Both" column are a select subsample. "Y1" should be interpreted with particular caution in this column, given the possibility that treatment in year 1 affected selection into this sample and may therefore no longer represent a causal effect. While "Y2" was re-randomized among the remaining sample and therefore represents a causal effect, it should be remembered that this the causal effect among a specific subset of respondents.

Table F.7: **LRFU 2015 Education:** Effect of Year 1 (2012-2013) and Year 2 (2013-2014) treatment education and non-farm profit. The "Year 1" column contains observations that were in the sample in the Year 1 study, the "Year 2" column contains observations that were in the sample in the Year 2 study, and the "Both" column contains the (select) subset of respondents who were in both samples.* "Y1" refers to treatment in Year 1, while "Y2" refers to treatment in Year 2. Attendance is the proportion of days the children in the household attended school in the last 5 days. Educational expenditure is the total household expenditure on children's education (in Ksh) over the past 12 months.

	Educational Expenditure			Attendance		
	Y1	Y2	Both	Y1	Y2	Both
Treat Y1	-3654.14		-6576.46	0.00		0.02
	(3854.68)		(6998.49)	(0.01)		(0.02)
Treat Y2		-1168.61	-4367.33		-0.01	0.02
		(2917.71)	(8041.06)		(0.01)	(0.02)
Treat Y1*Y2			2391.45			-0.04
			(9231.27)			(0.03)
Observations	979	936	556	927	876	528
R squared	0.00	0.00	0.01	0.00	0.00	0.01
MDV	38371.63	37452.55	43373.16	0.94	0.95	0.93

*Note that differential attrition from Year 1 to Year 2 means that those in the "Both" column are a select subsample. "Y1" should be interpreted with particular caution in this column, given the possibility that treatment in year 1 affected selection into this sample and may therefore no longer represent a causal effect. While "Y2" was re-randomized among the remaining sample and therefore represents a causal effect, it should be remembered that this the causal effect among a specific subset of respondents.

Table F.8: **LRFU 2015 Non-Farm Business and Salaried Employment:** Effect of Year 1 (2012-2013) and Year 2 (2013-2014) treatment on non-farm business and salaried employment. The "Year 1" column contains observations that were in the sample in the Year 1 study, the "Year 2" column contains observations that were in the sample in the Year 2 study, and the "Both" column contains the (select) subset of respondents who were in both samples.* "Y1" refers to treatment in Year 1, while "Y2" refers to treatment in Year 2. Hours Non-Farm is the number of hours worked by the household in a non-farm businesses run by the household in the last 7 days. Non-farm profit is the household's profit from non-farm activities in the last month (Ksh). Hours Salary is the total number of hours worked by household members in a salaried position. Average Wage is the average monthly wage for those household members who are salaried.

	Hours Non-Farm			Non-Farm Profit			Hours Salary			Average Wage		
	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both	Y1	Y2	Both
Treat Y1	0.94		1.41	-186.29		48.03	-2.28		1.47	1892.96		884.26
	(1.75)		(2.71)	(285.72)		(528.13)	(1.77)		(3.57)	(1697.63)		(3231.62)
Treat Y2		0.22	0.63		-244.86	-47.72		-0.98	-1.74		3651.39**	528.77
		(1.87)	(3.43)		(315.71)	(607.26)		(1.98)	(4.49)		(1700.71)	(3525.65)
Treat Y1*Y2			4.05			-47.91			-4.57			3027.24
			(4.25)			(744.40)			(5.19)			(4752.24)
Observations	979	937	556	975	933	552	982	939	559	292	274	155
R squared	0.01	0.00	0.02	0.00	0.00	0.01	0.00	0.00	0.01	0.00	0.02	0.02
MDV	15.97	14.87	13.32	2138.25	2019.84	1966.83	15.03	14.30	15.50	13014.88	12646.63	12714.71

*Note that differential attrition from Year 1 to Year 2 means that those in the "Both" column are a select subsample. "Y1" should be interpreted with particular caution in this column, given the possibility that treatment in year 1 affected selection into this sample and may therefore no longer represent a causal effect. While "Y2" was re-randomized among the remaining sample and therefore represents a causal effect, it should be remembered that this the causal effect among a specific subset of respondents.

F.2 Long-Run Price Effects

Table F.9: **LRFU Market prices for maize as a function of local treatment intensity.** "Hi" intensity is a dummy for a sublocation randomly assigned a high number of treatment groups. "Month" is a linear month time trend (beginning in Nov at 0 in each year). Standard errors are clustered at the sublocation level. Prices measured during the long-run follow-up year (Nov-Aug in the year following Y2 (2014-2015)). Price normalized to 100 in Nov 2014 in "low" sublocations.

	3km	1km	5km
Hi	1.87	0.90	0.93
	(2.73)	(2.80)	(2.50)
Month	3.34***	3.22***	3.06***
	(0.29)	(0.32)	(0.29)
Hi Intensity * Month	-0.67	-0.45	-0.04
	(0.75)	(0.76)	(0.71)
Observations	253	253	253
R squared	0.25	0.25	0.25

F.3 Long-Run Effects Interacted with Treatment Intensity

Table F.10: **LRFU 2014-2015 Outcomes:** Effect of Year 1 (2012-2013) and Year 2 (2013-2014) treatment on Year 3 (2014-2015) outcomes. The "Year 1" column contains observations that were in the sample in the Year 1 study, while the "Year 2" column contains observations that were in the sample in the Year 2 study. "Y1" refers to treatment in Year 1, while "Y2" refers to treatment in Year 2. "Percent Lean Sales" is the percentage of total sales completed from January onward. "Percent Harvest Purchases" is the percentage of total purchases completed prior to January. "Revenues" are the net revenues from all maize sales and purchases from the 2014 long-rains harvest to the 2015 long-rains harvest.

	Percent Lean Sales		Percent Harvest Purchases		Revenues	
	Y1	Y2	Y1	Y2	Y1	Y2
Treat Y1	0.04		0.02		1089.62	
	(0.11)		(0.04)		(2135.42)	
Treat Y1*Hi	-0.00		-0.06		-1052.68	
	(0.12)		(0.05)		(2263.49)	
Treat Y2		0.08		0.10**		2156.20**
		(0.06)		(0.04)		(969.54)
Treat Y2*Hi		-0.16*		-0.19***		-1204.33
		(0.07)		(0.06)		(1296.75)
Hi	-0.10	-0.01	0.08	0.18**	1007.50	648.42
	(0.12)	(0.08)	(0.06)	(0.06)	(1989.55)	(1080.75)
Observations	532	534	724	664	979	937
R squared	0.01	0.02	0.02	0.03	0.00	0.00

G Effects of Tags

Table G.1: **Effects of tags.** Regressions include round fixed effects, with errors clustered at the group level.

	(1) Inventories	(2) Inventories	(3) Revenues	(4) Revenues	(5) Consumption	(6) Consumption
Year 1 - Treat	0.52***		276.29		0.00	
	(0.16)		(291.42)		(0.03)	
T1 (Oct Loan)		0.69***		520.96		-0.00
		(0.19)		(321.38)		(0.04)
T2 (Jan Loan)		0.36*		31.78		0.00
		(0.19)		(346.02)		(0.04)
Tags	0.06	0.06	71.00	71.30	-0.01	-0.01
	(0.23)	(0.23)	(411.42)	(411.44)	(0.05)	(0.05)
Observations	4273	4273	4229	4229	4223	4223
R squared	0.34	0.34	0.01	0.01	0.00	0.00
Year 1 Treat-tags p-val	0.06		0.63		0.89	
T1-tags p-val		0.02		0.31		0.93
T2-tags p-val		0.26		0.93		0.86

H Savings Constraints and Effect of Lockboxes

How long might it take for a farmer to "save his way out" of this credit constraint? While the amount he would need to be fully released from this credit constraint is an ill-defined concept, one useful threshold is the point at which the farmer would be able to self-finance the loan.

We consider a few scenarios as benchmarks. If he receives the loan continuously each year and saves all of the additional revenue generated by the loan (1,548Ksh each year, according to our pooled estimate) under his mattress, he should be able to save the full average amount of the loan in 3.5 years. If instead the farmer reinvested this additional revenue, such that it compounds, he could save the full amount of the loan in a little less than 3 years. If the loan is only offered once, it would take more than 6 years of reinvesting his returns to save the full amount of the loan.

These may seem like fairly short time periods required for the farmer to save his way out of his credit constraint. However, the above estimates assume the the farmer saves 100% of the return from the loan. This may not be empirically accurate, nor optional, given that the farmer has urgent competing needs for current consumption. As an example, take the case in which the farmer instead saves only 10% of his return under her mattress. It would then take him 34 years to save the the full amount of the loan, even if it were continually offered during that period. Therefore, low savings rates are important to understanding why credit constraints persist in the presence of high return, divisible investment opportunities.

H.1 Effects of the Lockbox

In order to test the importance of savings constraints, we examine the impact of the lockbox, as well as its interaction with the loan. First, in Table H.1, we explore the immediate effects of the lockbox for outcomes in Year 1 (recall the lockbox was only offered in Year 1, and was crosscut with the loan treatment). We observe no primary significant effects of the lockbox on inventories, revenues, or consumption (Columns 1, 3, and 5). Interestingly, when interacted with the loan, we see that receiving the lockbox alone is associated with significantly *lower* inventories; perhaps the lockbox serves as a substitute savings mechanism, rather than grain (see Column 2). However, receiving both the lockbox and the loan is associated with a reversal of this pattern. We see no such heterogeneity on revenues (Column 4). Interestingly, the point estimates on consumption are negative (though not significant) for the lockbox and loan when received separately; however, the interaction of the two is large and positive (and significant, at 95%), canceling out this effect.

Table H.1: **Effects of lockboxes.** Regressions include round fixed effects, with errors clustered at the group level.

	(1) Inventories	(2) Inventories	(3) Revenues	(4) Revenues	(5) Consumption	(6) Consumption
Lockbox	-0.08	-0.45*	96.63	-1.35	0.02	-0.07
	(0.15)	(0.25)	(234.65)	(419.79)	(0.03)	(0.05)
Treat		0.35*		232.93		-0.04
		(0.21)		(356.31)		(0.04)
Lockbox*Treat		0.54*		141.94		0.13**
		(0.31)		(509.35)		(0.06)
Observations	3836	3836	3795	3795	3792	3792
Mean DV	3.06	2.82	432.03	317.41	9.47	9.50
R squared	0.34	0.35	0.01	0.01	0.00	0.00

I Price Effects Robustness

I.1 Binary and Ratio Treatment Estimates

In this subsection, we test the robustness of price effects to functional form assumptions. Table I.1 presents a binary version of Equation 4, replacing $month_t$ with an indicator $lean_t$ for being in the lean season (defined as April-August) and the interaction term with $lean_t * H_s$. Results suggest similar significant increases in price post-harvest in high-intensity markets.

Table I.1: **Market prices for maize as a function of local treatment intensity (binary).** "Lean" is a binary variable for being in the lean season (Apr-Aug). "Month" is a linear month time trend (beginning in Nov at 0 in each year). Standard errors are clustered at the sublocation level. Prices measured monthly following loan disbursal (Nov-Aug in Y1; Dec-Aug in Y2). Price normalized to 100 in Nov control ("low") sublocations.

	Main Specification (3km)			Robustness (Pooled)	
	Y1	Y2	Pooled	1km	5km
Hi	3.69**	1.24	2.75**	1.61	2.12
	(1.46)	(1.17)	(1.19)	(1.13)	(1.23)
Lean	5.89***	11.01***	8.70***	8.44***	9.65***
	(1.84)	(1.29)	(1.58)	(1.54)	(1.26)
Hi Intensity * Lean	-3.74*	-1.25	-2.80	-2.39	-4.37**
	(2.00)	(1.60)	(1.66)	(1.61)	(1.51)
Observations	491	381	872	872	872
R squared	0.06	0.12	0.09	0.08	0.09

We also check the robustness of these results to a more continuous measure of treatment at the market-level, following the technique described in Miguel and Kremer (2004). We construct an estimate of the ratio of total treated farmers to the total farmers in our sample within a 3km radius around each market.[58]. We re-estimate an equation identical to Equation 4 with H_s replaced with $ratio_m$, the aforementioned ratio. Results are presented in Table I.2.

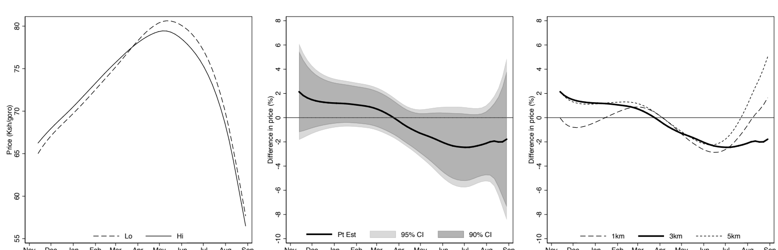
We also present non-parametric estimates of this specification in Figure I.1, displaying average prices in markets with above- vs. below-median ratios. While results are slightly noisier in this specification, the broad patterns remain consistent: prices are higher in the post-harvest period and lower in the lean period in markets with a greater proportion of treated individuals in the area.

[58] Because we draw twice the sample from high-intensity areas compared to low (in accordance with our randomized intensity), for the total farmer count, we weight the low-intensity observations by two to generate a count reflective of the true underlying OAF population.

Table I.2: **Market prices for maize as a function of local treatment intensity (ratio).** "Ratio" is the number of treated farmers within a given radius around the market/the total number of farmers (weighted) in our sample within the same radius. "Month" is a linear month time trend (beginning in Nov at 0 in each year). "Lean" is a binary variable for being in the lean season (Apr-Aug). Standard errors are clustered at the sublocation level. Prices measured monthly following loan disbursal (Nov-Aug in Y1; Dec-Aug in Y2). Price normalized to 100 in Nov control ("low") sublocations.

	Main Specification (3km)			Robustness (Pooled)	
	Y1	Y2	Pooled	1km	5km
Ratio	9.52*	7.19	4.33	2.23	4.78
	(5.27)	(4.11)	(4.12)	(2.45)	(4.88)
Month	1.27**	1.01**	1.33***	1.29***	1.34**
	(0.55)	(0.40)	(0.41)	(0.33)	(0.49)
Ratio * Month	-0.83	0.03	-0.59	-0.57	-0.59
	(0.95)	(0.91)	(0.69)	(0.60)	(0.87)
Observations	491	381	872	872	872
R squared	0.07	0.04	0.05	0.05	0.05

Figure I.1: **Pooled market prices for maize as a function of local treatment intensity (ratio)**. Market prices for maize as a function of the Miguel-Kremer treatment intensity ratio. The ratio is the total number of treated farmers/total OAF population within 3km radius. The left panel shows the average sales price in markets whose treatment ratio is above the median (solid line) versus below the median (dashed line) over the study period. The middle panel shows the average difference in log price between above- and below-median-ratio markets over time, with the bootstrapped 95% confidence interval shown in light grey and the 90% confidence interval shown in dark grey. The right panel shows prices over time in markets binned by the quarter of this ratio.



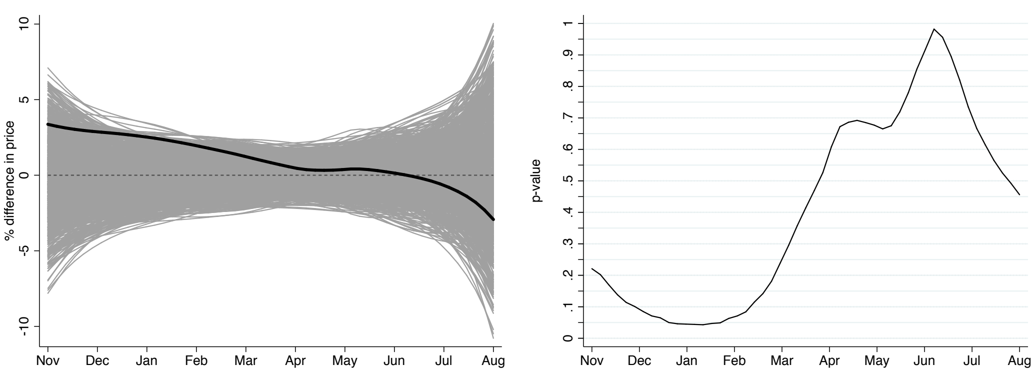
I.2 Randomization Inference, Wild Bootstrap, and Outlier Robustness

These market-level price results rely on the treatment saturation randomization being conducted at the sublocation level, a higher level than the group-level randomization employed in the individual-level results. While we cluster standard errors at the sublocation level, one might be concerned due to the small number of sublocations – of which we have 17 – that asymptotic properties may not apply to our market-level analyses and that our standard errors may therefore be understated. We run several robustness checks to address these small sample concerns.

First, building on other experimental work with small numbers of randomization units (Bloom et al., 2013; Cohen and Dupas, 2010), we use nonparametric randomization inference to confirm our results. We generate 1000 placebo treatment assignments and compare the estimated price effects under the "true" (original) treatment assignment to estimated effects under each of the placebo assignments.[59] Results are shown in Figure I.2. The left-hand panel of each figure shows price differences under the actual treatment assignment in black, and the placebo treatment assignments in grey. "Exact" p-values on the test that the price difference is zero are then calculated by summing up, at each point in the support, the number of placebo treatment estimates that exceed the actual treatment estimate (in absolute value) and dividing by the total number of placebo treatments (1000 in this case); these are shown in the right-hand panel of each figure.

Figure I.2 suggests that prices differences observed in the pooled data are significant at conventional levels from December to mid-February. This is roughly consistent with the results shown in Figure 6.

Figure I.2: **Nonparametric Randomization Inference** *Left panel*: price effects under the "true" treatment assignment (black line) and 1000 placebo treatment assignments (grey lines). *Right panel*: randomization-inference based p-values, as derived from the left panel.



As an alternative method of accounting for the small number of clusters, we implement the wild bootstrap procedure proposed by Cameron et al. (2008). As a point of reference, Columns 1, 3, and 5 of Table I.3 present the results from the primary specification (that presented in Table 6) with p-values presented in the notes. Columns 2, 4, and 6 present the results from the wild bootstrapping exercise, with the empirical p-values in the notes (empirical p-values represent twice

[59] With 17 sublocations, 9 of which are "treated" with a high number of treatment farmers, we have 17 choose 9 possible treatment assignments (24,310). We compute treatment effects for a random 1,000 of these possible placebo assignments.

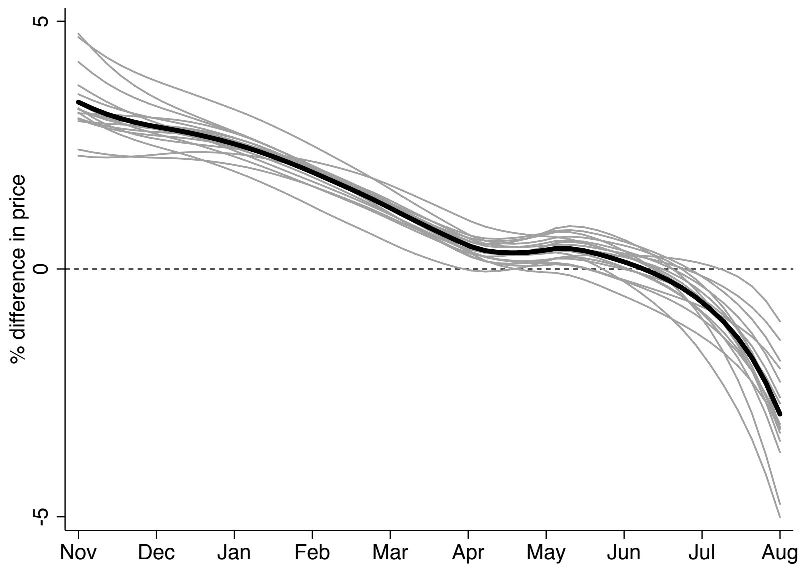
the fraction of t-statistics from the bootstrap samples that are above (below) the initial t-statistic for positive (negative) t-statistics). Comparing columns of Table I.3, we see only a small decrease in statistical precision.

Table I.3: **Wild bootstrap** Specifications as presented in Table 6, but with empirical p-values assessed using the wild bootstrap procedure proposed by Cameron et al. (2008), clustering at the sublocation level.

	Y1		Y2		Pooled	
	(1)	(2)	(3)	(4)	(5)	(6)
Hi	4.41	4.41	2.85	2.85	3.97	3.97
Month	1.19	1.19	1.22	1.22	1.36	1.36
Hi Intensity * Month	-0.57	-0.57	-0.48	-0.48	-0.57	-0.57
Observations	491	491	381	381	872	872
Mean of Dep Var	62.15	62.15	62.15	62.15	62.15	62.15
R squared	0.08	0.08	0.03	0.03	0.06	0.06
Wild Bootstrap	No	Yes	No	Yes	No	Yes
P-val Hi	0.05	0.10	0.17	0.20	0.04	0.08
P-val Month	0.01	0.04	0.01	0.00	0.00	0.03
P-val Hi*Month	0.19	0.18	0.32	0.32	0.16	0.17

Finally, to ensure that the trends observed are not driven by a single sublocation, we drop sublocations one-by-one and re-estimating prices differences. The results of this exercise are presented in Figure I.3. Differential trends over time in the two areas do not appear to be driven by particular sublocations.

Figure I.3: **Robustness to dropping each sublocation** Difference in prices between high and low-density markets over time for the full sample (black line) and for the sample with each sublocation dropped in turn (grey lines).



I.3 Pre-Specified Measures of Price Effects

As noted in Section 3, the pre-analysis plan (PAP) specifies the outcome of interest to be the percent price spread from November to June. We selected these dates to roughly match (i) the trough and peak price periods, respectively; and (ii) the period during which the loan was disbursed. However, there is variation in timing of both periods. For example, in Year 1 prices peaked in April (the exact trough is unknown, as price data collection only began in November of that year) and in Year 2 prices reached their trough in September and peaked in June. As for the loan disbursal period, loans were offered in October and January in Year 1 and in November in Year 2. Therefore, the impact of the loan may not map exclusively to the November-to-June price change. To allow for greater flexibility in the timing of these effects, the primary specification employed in the main text presents the non-parametric effect of treatment on the evolution of monthly prices, as well as a level and time trend effect. This also allows greater use of the full data. While we have 872 monthly observations of price across these markets over the pooled study period, because the pre-specified metric only allows for a single outcome per market per year, our observations fall to 95 in this specification.

However, for completeness, here we present the pre-specified effect of treatment saturation on the percentage change in prices from November to June. We hypothesized that the treatment would cause a reduction in this gap in treated areas, representing smoother prices across the season. We observe no effect of the treatment on the percent price increase from November to June. Looking at Figure 6, we observe a sizable increase in prices in the immediate post-harvest period in November, a gap which slow tappers off until June, when prices equalize in high and low treatment density markets. The simple comparison of November to June, which bookends this period, ignores data from the interim period, during which we also observe differences in prices between high and low treatment intensity markets. It also ignores the subsequent fall in prices in high markets relative to low in the following period. This analysis is therefore vastly underpowered relative to the analysis conducted in the main text.

Table I.4: **Pooled Price Gap Nov - June** Percent increase in price from November to June regressed on indicator for being in a high saturation sublocation.

	(1) Y1	(2) Y1	(3) Pooled
Hi	-0.02	0.02	0.00
	(0.04)	(0.02)	(0.03)
Observations	52	43	95
Mean DV	0.14	0.25	0.19
R squared	0.01	0.01	0.00

I.4 Effect on Related Outcomes

We explore whether treatment intensity had effects on related outcomes. First we check whether treatment effects can be seen in farmgate prices (see Table I.5). Using individual-level sales prices as reported in the household survey, we estimate a specification identical to Equation 8. We normalize prices in the low-intensity households in round 1 to 100, such that estimates can be interpreted as percentage changes relative to this baseline. We see similar patterns to those presented in Table 6. Point estimates suggest that prices are 3.32% higher in round 1 (significant at 5%), 2.92% higher in rough 2 (significant at 10%), and 0.72% lower in round 3 (not significant).

Note that these results should be interpreted with caution, as farmgate sales price is only observed for farmers who sell maize during the round in question. Any extensive margin response to treatment may bias these estimates. However, it is reassuring that they roughly aline with the main estimates using the market data (which does not suffer from such selection biases).

We also explore whether trader movement responds to treatment intensity. In Table I.6, we see some evidence that fewer traders enter high-intensity treated markets in the immediate post-harvest period in Year 2, which may be a sensible demand respond to the increase in price observed during a time when traders are typically purchasing. This may also contribute to the weaker price effects observed in Year 2.

Table I.6 presents effects of treatment intensity on the number of traders present in the market. We see these local markets are quite small; there are only 0.55 traders in a given market on average.

Table I.5: **Farmgate prices for maize as a function of local treatment intensity.** "Hi" intensity is a dummy for a sublocation randomly assigned a high number of treatment groups. "Round" represents the round of the survey (1, 2, or 3). Standard errors are clustered at the sublocation level. Regression includes round-year fixed effects and a control for the interview date. Price normalized to 100 in round 1"low" sublocations.

	(1)	(2)	(3)
	Y1	Y2	Pooled
Hi - R1	4.66**	1.52	3.32**
	(2.03)	(1.27)	(1.40)
Hi - R2	3.16*	2.21	2.95*
	(1.59)	(2.86)	(1.47)
Hi - R3	-0.35	-3.51	-0.72
	(1.27)	(5.31)	(1.56)
Observations	1582	636	2218
R squared	0.45	0.20	0.42

Table I.6: **Number Traders**

	Y1		Y2		Pooled	
Hi	-0.13	-0.07	-0.34	-0.37**	-0.22	-0.17*
	(0.11)	(0.09)	(0.24)	(0.16)	(0.15)	(0.09)
Month		0.02		0.03		0.04*
		(0.02)		(0.03)		(0.02)
Hi Intensity * Month		-0.02		0.01		-0.01
		(0.02)		(0.04)		(0.02)
Observations	451	451	419	419	870	870
Mean of Dep Var	0.32	0.32	0.82	0.82	0.55	0.55
R squared	0.01	0.01	0.02	0.03	0.01	0.02

J Treatment Heterogeneity Robustness

For robustness, we also estimate wild bootstrapped standard errors for the individual treatment effects with general equilibrium heterogeneity. Although the main treatment is randomized at the group level, the heterogeneity is induced at the sublocation level. In the main specification, we therefore cluster by sublocation. However, due to the small numbers of clusters of sublocations, we also test the robustness of these results by estimating wild bootstrapped standard errors. In Table J.1, we present the original p-values, calculated using robust standard errors clustered at the sublocation level, as presented in Table 7, in Columns 1, 3, and 5. Columns 2, 4, and 6 present the same specification, but with empirical p-values assessed using the wild bootstrap procedure proposed by Cameron et al. (2008), clustering at the sublocation level.

We see that p-values on inventories are essentially unchanged. Therefore, we find the strength of these results not to be strongly changed by the adjustment for a small number of clusters.

Table J.1: **Individual Effects, Accounting for Treatment Intensity (Wild bootstrap)** Columns 1, 3, and 5 present the original p-values, from robust standard errors clustered at the sublocation level, as presented in Table 7. Columns 2, 4, and 6 present the same specification, but with empirical p-values assessed using the wild bootstrap procedure proposed by Cameron et al. (2008), clustering at the sublocation level.

	Inventories		Revenues		Consumption	
	(1)	(2)	(3)	(4)	(5)	(6)
	Cluster	Wild Boot	Cluster	Wild Boot	Cluster	Wild Boot
Treat	0.74	0.74	1101.39	1101.39	-0.01	-0.01
Hi	0.02	0.02	164.94	164.94	-0.05	-0.05
Treat*Hi	-0.29	-0.29	-816.77	-816.77	0.07	0.07
Observations	6780	6780	6730	6730	6736	6736
Mean DV	2.04	2.04	-1980.02	-1980.02	9.56	9.56
R squared	0.29	0.29	0.09	0.09	0.03	0.03
SE	Cluster	Wild Boot	Cluster	Wild Boot	Cluster	Wild Boot
P-val Treat	0.00	0.00	0.02	0.06	0.63	0.62
P-val Hi	0.94	0.96	0.73	0.71	0.29	0.29
P-val Treat*Hi	.149	.15	.136	.142	.091	.084

K Balance, Take-up, and Other Outcomes by Treatment Intensity

While our experiment affected local maize markets differentially in high- and low-treatment density areas, changes in treatment density could drive other spillovers beyond just those on local markets. In this appendix, we explore whether there is evidence for these other effects, as well as any other differences in balance or take-up that could potentially drive differential treatment effects.

First, we note that covariates were balanced at baseline across high- and low-intensity areas (Table K.1), as expected given the random assignment.

We also explore whether there are differences in loan take-up by treatment intensity. Among the pooled data, we see no differences in the (unconditional) loan size across the low and high intensity groups. We do, however, find some imbalances in loan take-up by intensity (see Table K.2). In high intensity areas, loan take-up is 5 percentage points lower than in low areas (significant at 5%) overall (Row 3). Interestingly, though, this pattern reverses from Year 1 (when loan take-up is 13 percentage points lower in high intensity areas) to Year 2 (when loan take-up is 8 percentage points higher in high intensity areas).[60] This differential take-up could affect our intent-to-treat (ITT) estimates; given a constant treatment-effect-on-the-treated, ITT estimates should be mechanically closer to zero in cases where take-up is lower. One might worry that, in particular in Year 1 when take-up is lower in the high intensity areas, this explains why revenue effects are also lower in high intensity areas. Two factors argue against this concern. First, the difference appears too small to explain our results fully. If there were no other spillovers, and treatment-on-treated effects were the same in high and low intensity areas, then ITT estimates in the high intensity ares should be 83% as large (0.61/0.74). However, point estimates on revenue treatment effects in Year 1 are roughly *zero* in the high-intensity areas (compared to 1,060 in low-intensity areas), a much bigger gap that could be explained by differential take-up. Second, and moreover, in Year 2, the differential take-up pattern switches; in this year, take-up is *higher* in high-intensity areas. If take-up were driving these results, we should see that a switch in the take-up patterns by intensity results in a switch in the revenue effects by intensity. However, we consistently across Years 1 and 2 see that revenue effects are greater among low-intensity areas. Take-up is therefore unlikely to be driving results.

We do additionally see some differences in loan size by intensity in Year 2. In this year of the experiment, loans were larger in high intensity areas.[61] However, this should have driven *greater* revenue effects in high intensity areas, rather than the lower effects that we find. We therefore believe it is unlikely that differential take-up or loan size are driving these results.

Finally, given the importance of social safety nets in rural communities, it is possible that informal lending between households could also be differentially affected by having a locally higher density of loan recipients; as an untreated household, one's chance of knowing someone who received the loan is higher if one lives in a high-treatment-density areas. Perhaps high-intensity households have lower revenue effects because they share more with neighbors and others in their social network. Table K.3 explores this possibility, testing the impact of treatment on maize given away (as a gift or loan) and cash given away (as a loan). We find that the amount of transfers other households does not appear to respond to either treatment or to treatment intensity.

Overall, then, the individual-level spillover results are perhaps most consistent with spillovers through local market effects.

[60]The Year 1 results may be the result of repayment incentives faced by OAF field staff: our loan intervention represented a substantial increase in the total OAF credit outlay in high-intensity areas, and given contract incentives for OAF field staff that reward a high repayment rate for clients in their purview, these field officers might have more carefully screened potential adopters. We are still exploring why the Year 2 results would have switched; given that the returns are more concentrated among low-intensity individuals, we would expect if anything higher take-up in Year 2 among the low-intensity individuals.

[61]Again, we are exploring why this might be the case, given that we would have expected, if anything, the lower returns in Year 1 in the high-intensity areas to lead to *smaller* rather than larger loans. It may be that given the price effects, a larger loan is necessary to arbitrage (e.g. if prices are higher at harvest, farmers would require a greater infusion of cash to supplement their outside option of sale at harvest and/or or fund purchases of maize at harvest).

Table K.1: **Balance among baseline covariates, high versus low treatment intensity areas.** The first two columns give the means in the high and low treatment intensity areas, the 3rd column the total number of observations across the two groups, and the last two columns the differences in means normalized by the standard deviation in the low intensity areas, with the corresponding p-value on the test of equality.

	Hi	Lo	Obs	Hi-Lo	
				std diff	*p-val*
Male	0.31	0.32	1,589	-0.02	0.72
Number of adults	3.07	3.11	1,510	-0.02	0.74
Kids in school	2.98	3.15	1,589	-0.09	0.11
Finished primary	0.75	0.71	1,490	0.08	0.13
Finished secondary	0.25	0.27	1,490	-0.04	0.51
Total cropland (acres)	2.35	2.60	1,512	-0.08	0.15
Number of rooms in hhold	3.08	3.31	1,511	-0.08	0.10
Total school fees (1000 Ksh)	27.88	29.23	1,589	-0.04	0.51
Average monthly cons (Ksh)	14,943.57	15,586.03	1,437	-0.05	0.38
Avg monthly cons./cap (log Ksh)	7.97	7.98	1,434	-0.02	0.77
Total cash savings (KSH)	6,516.09	5,776.38	1,572	0.04	0.56
Total cash savings (trim)	4,947.51	5,112.65	1,572	-0.01	0.82
Has bank savings acct	0.42	0.42	1,589	0.01	0.91
Taken bank loan	0.09	0.07	1,589	0.06	0.30
Taken informal loan	0.24	0.25	1,589	-0.02	0.72
Liquid wealth	98,542.58	87,076.12	1,491	0.12	0.06
Off-farm wages (Ksh)	3,829.80	3,965.65	1,589	-0.01	0.84
businessprofitmonth	2,201.34	1,859.63	1,589	0.04	0.53
Avg %Δ price Sep-Jun	138.18	121.58	1,504	0.21	0.00
Expect 2011 LR harvest (bags)	8.70	10.52	1,511	-0.08	0.03
Net revenue 2011	-4,200.36	-2,175.44	1,428	-0.03	0.45
Net seller 2011	0.30	0.34	1,428	-0.08	0.16
Autarkic 2011	0.07	0.06	1,589	0.04	0.53
% maize lost 2011	0.01	0.01	1,428	-0.00	0.95
2012 LR harvest (bags)	10.94	11.57	1,484	-0.07	0.19
Calculated interest correctly	0.74	0.68	1,580	0.12	0.03
Digit span recall	4.60	4.49	1,504	0.10	0.08
Maize giver	0.27	0.25	1,589	0.05	0.37
delta	0.13	0.14	1,512	-0.07	0.28

See Table N.1 and the text for additional details on the variables.

Table K.2: **Loan Take-up and Size by Treatment Intensity.**

	Loan Take-up					**Loan Size (Cond)**					**Loan Size (Uncond)**				
	Low Mean	High Mean	N Obs	Diff SD	Diff p-val	Low Mean	High Mean	N Obs	Diff SD	Diff p-val	Low Mean	High Mean	N Obs	Diff SD	Diff p-val
Year 1	0.74	0.61	954	0.30	0.00	7,457.50	7,573.14	617	-0.05	0.60	5,524.07	4,616.96	954	0.23	0.00
Year 2	0.56	0.64	525	-0.17	0.07	9,434.52	11,281.25	324	-0.53	0.00	5,248.34	7,239.30	525	-0.37	0.00
Pooled	0.67	0.62	1,479	0.11	0.05	8,042.25	8,927.70	941	-0.30	0.00	5,425.18	5,543.95	1,479	-0.03	0.68

Table K.3: **Effect of Treatment on Transfers.** "Maize Given" represents the amount of maize (in terms of 90kg bags) given away to others outside the household, either as a gift or loan, in the past round (∼3 months). "Cash Given" represents the amount of cash (in Ksh) given to others outside the household as a loan in the past round.

	Maize Given		Cash Given	
	(1)	(2)	(3)	(4)
Treat	0.44	1.43	-31.12	-1.41
	(0.78)	(1.94)	(93.64)	(183.97)
Hi		-0.77		52.16
		(0.95)		(178.97)
Treat*Hi		-1.37		-42.92
		(2.07)		(224.83)
Observations	6850	6850	5987	5987
Mean DV	3.96	4.44	541.97	460.80
R squared	0.03	0.03	0.03	0.03

L Attrition and Sample Composition

L.1 Attrition in Main Study

Attrition was relatively low in both years. In Year 1, overall attrition was 8%, and not significantly different across treatment groups (8% in the treatment group and 7% in the control). In Year 2, overall attrition was 2% (in both treatment and control, with no significant difference).

L.2 Sample Composition

Table L.1: **Sample Composition.** Summary statistics for the Year 1 study sample (from the baseline survey) and for all farmers in Bungoma, Kenya, where the study takes place (from the Kenyan Integrated Household Budget Survey of 2006).

	Sample Mean	Bungoma Mean
Landholding (acres)	2.35	2.50
Any livestock	0.92	0.86
Grow maize	0.92	0.97
Any fertilizer	0.91	0.81
Finished primary	0.74	0.86
Finished secondary	0.25	0.25
HH members	7.12	6.40
Num rooms	3.00	2.70
Earth floor	0.81	0.81
Iron roof	0.83	0.82
Mud and sticks wall	0.81	0.80
Money given (if any)	1,363	1,405
Food given (if any)	1,732	1,649

Table L.1 compares the composition of the Year 1 sample (using summary statistics from the baseline survey) to that of all farmers in the county in which the study takes place (using summary statistics for the study county, as collected in the Kenyan Integrated Household Budget Survey of 2006). We observe that the Year 1 sample appears to be roughly representative of the typical farmer in Bungoma, Kenya.

The Year 2 sample attempted to follow the same OAF groups as Year 1. However, a prerequisite for inclusion in the study sample is membership in OAF. Each year, farmers must opt into renewed engagement with OAF's services. There is some natural churn in this membership from year-to-year, with some existing members dropping out while new members join. Treatment in Year 1 had the effect of increasing farmers' interest in renewed engagement with OAF (a sensible result, given that the maize storage loan offer appears to be beneficial for farmers and therefore likely increased the perceived value of OAF's services).

As a result, the Year 2 sample, which was designed to include all farmers from Year 1 of the study, in practice includes a disproportionate number of farmers from the Year 1 treatment group.[62] Treated individuals were 10 percentage points more likely to return to the Year 2 sample than control individuals (significant at 1%).

Because Year 2 treatment status is stratified by Year 1 treatment status, the sample composition does not alter the internal validity of the Year 2 results. However, because this effect slightly alters the composition of the Year 2 sample, we may be interested in exploring how this affects the external validity, or generalizability, of our results.[63] This is particularly relevant in the presence of heterogeneous treatment effects. For example, it may be that those for whom treatment was more beneficial were more likely to return to OAF, such that the Year 2 results are estimated on a sample for whom treatment effects are particularly strong.

Table L.2 presents several key Year 1 outcome variables regressed on a dummy for Year 1 treatment status, a dummy for whether the individual returned to the sample in Year 2, and an interaction term. In Column 1, for example, we see that those who returned to the sample were famers with larger inventories. However, the insignificant interaction term suggests no evidence of a differential treatment effect on inventories (at least in Year 1) for those who returned. In Column 2, we observe that returners, on average, are those farmers who face higher purchase prices (perhaps for these farmers, the loan is more useful because they are facing high consumer prices). The interaction term is significant and negative, suggesting that treatment results in a particularly low purchase prices for returners. This is consistent with the idea that those who returned were those for whom the loan was most beneficial. We see similar patterns for sales prices (but with opposite signs, as expected), though these results are not significant. We see no significant differences for returners, nor any significant interaction effects for revenues or consumption.

[62]Note that a second, broader result of this churn was a mix in the composition of the Year 2 sample between those drawn from the Year 1 sample (those who stayed from Year 1, comprising 602 individuals) and those who were new to the sample (417 individuals) Recall that the Year 1 sample consists of 240 existing One Acre Fund (OAF) farmer groups drawn from 17 different sublocations in Bungoma county, and our total sample size at baseline was 1589 farmers.

[63]Though the likely more important feature for external validity is how OAF farmers compare to typical farmers in the area, as explored above.

Table L.2: **Year 1 treatment heterogeneity for Year 2 returners.** Year 1 outcome variables regressed on dummy for whether treated in Year 1, dummy for whether returned to the sample in Year 2, and interaction term. Sample is all Year 1 subjects. Treatment effects at the individual level, all rounds. Regressions include round-year fixed effects, with errors clustered at the group level.

	(1) Invent	(2) Purchase price	(3) Sales prices	(4) Rev	(5) Log HH Cons
Treat Y1	0.53***	9.88	-19.52	315.01	-0.01
	(0.17)	(23.90)	(27.55)	(302.74)	(0.04)
Returned Y2	0.68***	78.91**	-44.41	380.62	0.01
	(0.24)	(31.38)	(39.10)	(338.42)	(0.05)
Treat Y1 * Returned Y2	-0.06	-100.28**	43.30	-158.30	0.05
	(0.29)	(38.84)	(45.41)	(408.97)	(0.06)
Observations	3836	1914	1425	3776	3792
Mean of Dep Variable	2.67	2982.02	2827.58	334.41	8.00
R squared	0.37	0.30	0.47	0.13	0.03
Controls	Yes	Yes	Yes	Yes	Yes

Table L.3 presents additional results on how returners may differ from non-returners. Returners have significantly more children in school and pay more in school fees. This is consistent with focus groups that stated that farmers are often forced to sell maize early to pay for school fees; this group may get the most benefit from the loans and therefore be more eager to return to OAF with the hopes of taking up the loan. Returners also had significantly larger harvests in 2011 and 2012, and were more likely to be net sellers in 2011. This is consistent with the idea that those with the most to sell have the most to gain from properly timing their sales. It could also reflect some underlying correlation between wealth and returning behavior. Consistent with this later interpretation, returners are more likely to have a bank savings account. They also have greater liquid wealth, higher average monthly consumption, and more rooms in their household. Interestingly, despite being more likely to have completed primary school, returners have significantly lower digit span recall. Sensible, returners have higher values of δ, representing greater patience.

Table L.3: **Summary statistics for returners vs. non-returners.** "Non-returner" is an indicator for having exited the sample between Year 1 (2012-13) and Year 2 (2013-14). "Returner" is an indicator for being in the Year 1 and Year 2 samples

Baseline characteristic	Non-Returner	Returner	Obs	Non-Return - Return	
				sd	*p-val*
Treatment 2012	0.56	0.66	1,589	-0.20	0.00
Male	0.28	0.25	1,816	0.07	0.13
Number of adults	3.01	3.12	1,737	-0.05	0.30
Kids in school	2.89	3.23	1,816	-0.17	0.00
Finished primary	0.73	0.77	1,716	-0.08	0.10
Finished secondary	0.25	0.25	1,716	-0.01	0.81
Total cropland (acres)	2.26	2.50	1,737	-0.08	0.12
Number of rooms in hhold	2.94	3.34	1,738	-0.16	0.00
Total school fees (1000 Ksh)	25.93	30.08	1,816	-0.11	0.02
Average monthly cons (Ksh)	14,344.56	15,410.58	1,652	-0.09	0.10
Avg monthly cons./cap (log Ksh)	7.94	7.96	1,649	-0.04	0.49
Total cash savings (KSH)	5,355.05	6,966.35	1,797	-0.09	0.13
Total cash savings (trim)	4,675.61	4,918.86	1,797	-0.02	0.70
Has bank savings acct	0.38	0.46	1,816	-0.15	0.00
Taken bank loan	0.07	0.08	1,816	-0.04	0.46
Taken informal loan	0.23	0.24	1,816	-0.01	0.86
Liquid wealth	89,564.21	100,021.77	1,716	-0.10	0.05
Off-farm wages (Ksh)	3,508.17	4,103.66	1,816	-0.05	0.31
Business profit (Ksh)	2,069.13	2,159.55	1,816	-0.01	0.86
Avg %Δ price Sep-Jun	130.30	141.63	1,728	-0.15	0.00
Expect 2011 LR harvest (bags)	8.13	9.55	1,732	-0.09	0.05
Net revenue 2011	-4,983.94	-4,156.75	1,633	-0.02	0.72
Net seller 2011	0.26	0.35	1,633	-0.19	0.00
Autarkic 2011	0.06	0.07	1,816	-0.03	0.53
% maize lost 2011	0.01	0.01	1,609	0.00	0.98
2012 LR harvest (bags)	9.26	11.94	1,708	-0.31	0.00
Calculated interest correctly	0.72	0.72	1,806	-0.01	0.91
Digit span recall	4.61	4.50	1,731	0.09	0.06
Maize giver	0.26	0.26	1,816	0.00	0.98
Delta	0.86	0.87	1,738	-0.08	0.09

M Gains Estimation Assumptions

Table 8 employs following summary statistics and assumptions:

1. Total population in the study area is 7,105 households (HH) (this figure is an approximation, as the sublocations used in this study are One Acre Fund (OAF) administrative districts and therefore do not directly correspond to the Kenyan census administrative districts. OAF estimates that it works with 30% of all farmers in the area. While this figure affects the total gains estimates, it does not affect any estimates of per-HH gains, ratios, or fractions in the table, nor does it affect any comparisons between low and high saturation areas) (A_1)

2. 50% of the study population resides in low saturation sublocations (this is roughly accurate; moreover, it allows a comparison of the size of the benefits across low and high saturation rates that is unconfounded by differences in underlying population sizes) (A_2)

3. 30% of HH in the region are One Acre Fund (OAF) members, a figure provided by OAF administrative records (A_3)

4. 40% of all OAF members were enrolled in the study in low saturation sublocations (A_{4a}) and 80% were enrolled in high saturation sublocation (A_{4b})

5. In each sublocation, 58% of individuals in the sample were randomly assigned to receive treatment (average across the pooled data from Year 1 and Year 2) (A_5)

Gains are estimated using the following calculations, using the above figures and the per-round point estimate on revenues β_1, β_2, and β_2 (estimated in Ksh) from Column 6 of Table 7 (multiplied by three to get the annual revenue gains):

1. Low saturation direct gains: $3 * \beta_1$

2. High saturation direct gains: $3 * (\beta_1 + \beta_3)$

3. High saturation indirect gains: $3 * \beta_2$

4. Ratio of indirect to direct gains: *Row 2/Row 3*

5. Low saturation direct beneficiary population (HH): $A_1 * A_2 * A_3 * A_{4a} * A_5 = 7,105 * 0.5 * 0.3 * 0.4 * 0.58$

6. High saturation direct beneficiary population (HH): $A_1 * (1 - A_2) * A_3 * A_{4b} * A_5 = 7,105 * 0.5 * 0.3 * 0.8 * 0.58$

7. Low saturation total local population: (HH): $A_1 * A_2 = 7,105 * 0.5$

8. High saturation total local population: (HH): $A_1 * (1 - A_2) = 7,105 * 0.5$

9. Total direct gains: *Row 1*Row 4*

10. Total indirect gains: *Row 2*Row 5*

11. Total gains (direct + indirect): *Row 6+Row 7*

12. Fraction of gains indirect: *Row 7/Row 8*

13. Low saturation private gains/HH: $3 * \beta_1$

14. High saturation private gains/HH: $3 * (\beta_1 + \beta_2 + \beta_3)$

15. Total private gains: *Row 10∗Row 4*

16. Fraction of gains private: *Row 11/Row 8*

N Pre-Analysis Plan

This document describes the plan for analyzing the impact of the Maize Storage project, and was written before the analysis of any follow-up data.

N.1 Introduction

Rural grain markets throughout much of the developing world are characterized by large, regular seasonal price fluctuations. Farmer behavior in light of these fluctuations is often puzzling: the vast majority appear to sell their produce when prices are low, buy when prices are high, or often both. This behavior appears to persist despite farmers' general recognition of these price patterns, and the availability of a simple technology - storage - which can be used to move grain inter-temporally.

Why don't farmers use storage to take better advantage of these seasonal price fluctuations? Working with 1589 smallholder maize farmers and an NGO implementing partner in Webuye District in Western Kenya, we designed and implemented an experiment to test two hypotheses: (1) farmers are liquidity constrained and thus sell their maize at low post-harvest prices because they need the cash, and (2) farmers' friends and family make frequent claims on stored maize, reducing the incentive to store.

In this experiment, our implementing partner, the NGO One Acre Fund, offered storage loans to a randomly selected subset of our farmer sample. These loans were announced during harvest, with cash delivered either just after harvest, or three months later just before school fees are typically paid – with school fees being the modal explanation given by farmers for why they liquidate their maize at low post-harvest prices. These loans were collateralized with bags of maize that farmers store in their home, and the collateralized bags were given large tags indicating that they were for loan repayment.

In focus groups before the intervention, many farmers said that sharing norms around surplus stored maize made storage more difficult, and indicated that the tags themselves would be a useful and credible way to shield maize from claims by their family and friends. To test the role of tags alone, we provided tags to a subset of the farmers who did not receive the loan. Finally, because the timing of the loans we provided was unlikely to perfectly match the timing of farmers' cash needs, and because a growing literature suggests that cash on hand is often difficult to shield from one's own immediate impulses or the claims of family and friends, we cross-randomized the loan treatments with a savings lockbox (a small metal box with a solid lock and key). The idea was that this lockbox could help farmers channel the loan to their planned investment, as well as make better use of any profits emanating from the loan. Finally, to understand whether our loan interventions might affect local maize prices by shifting storage behavior, we randomized the treatment intensity of the loan across sites, and followed maize prices at 53 local markets in the area.

Below we describe the experimental design, the data collection process, and the specific questions that we wish to address.

N.2 Study design

Our study sample is drawn from 240 existing groups of One Acre Fund (OAF) farmers in Webuye district, Western Province, Kenya. OAF is a microfinance NGO that makes in-kind, joint-liability loans of fertilizer and seed to groups of farmers, as well as providing training on improved farming techniques. OAF group sizes typically range from 8-12 farmers, and farmer groups are organized into "sublocations" – effectively clusters of villages that can be served by one OAF field officer. Our 240 groups were drawn from 17 different sublocations in Webuye district. Our total sample size at baseline was 1589 farmers.

Figure N.1 shows the basic setup of our experiment. The two loan treatments are the October loan (T1) and the January loan (T2), with the loan offers randomized at the group level (as shown in the white boxes). Grey boxes represent the individual-level lockbox and tags treatments, with the sub-codes indicating the different treatments – e.g. T1n are the individuals who received the T1 offer but not the lockbox. Treatments were stratified as follows. First, to help understand whether our loan interventions would have general equilibrium effects on local maize prices, we randomized the intensity of the loan treatments across sublocations (a sublocation is an administrative designation for OAF, but can be thought of as a cluster of villages). Additional detail on this sublocation-level randomization is provided below.

The loan treatments were then stratified at the sublocation level, and further stratified based on whether group-average OAF loan size in the previous year was above or below the sample median (data from the previous year were available from administrative data). Although all farmers in each loan treatment group were offered the loan, we follow only a randomly selected 6 farmers in each loan group, and a randomly selected 8 farmers in each of the control groups. The location of study households and the maize markets we follow are shown as small blue (treatment) and orange (control) dots in the left panel of Figure N.3.

Finally, using the sample of individuals randomly selected to be followed in each group, we stratified individual level treatments by group treatment assignment and by gender. So, for instance, of all of the women who were offered the October Loan and who were randomly selected to be surveyed, one third of them were randomly offered the lockbox (and similarly for the men and for the January loan). In the control groups, in which we were following 8 farmers, 25% of the men and 25% of the women were randomly offered the lockbox (Cl in Figure N.1), with another 25% each being randomly offered the tags (Ct). The study design allows identification of the individual and combined effects of the different treatments, and our approach for estimating these effects is described below.

The timing of the study activities is shown in Figure N.2. We collect 3 types of data. Our main source of data is farmer household surveys. All study participants were baselined in August/September 2012, and we undertook 3 follow-up rounds over the ensuing 12 months, with the last follow-up round concluding August 2013. The multiple follow-up rounds were motivated by three factors. First, a simple inter-temporal model of storage and consumption decisions suggests that while the loan should increase total consumption across all periods, the per-period effects could be ambiguous – meaning that consumption throughout the follow-up period needs to be measured to get at overall effects. Second, because nearly all farmers deplete their inventories before the next harvest, inventories measured at a single follow-up one year after treatment would likely provide very little information on how the loan affected storage and marketing behavior. Finally, as shown in McKenzie (2012), multiple follow-up measurements on noisy outcomes variables (e.g consumption) has the added advantage of increasing power.

The follow-up survey rounds span the spring 2013 “long rains” planting (the primary growing season), and concluded just prior to the 2013 long rains harvest. The baseline survey collected data on farming practices, on storage costs, on maize storage and marketing over the previous crop year, on price expectations for the coming year, on food and non-food consumption expenditure, on household borrowing, lending, and saving behavior, on household transfers with other family members and neighbors, on sources of non-farm income, on time and risk preferences, on ambiguity aversion, and on digit span recall. The follow-up surveys collected similar data, tracking storage inventory, maize marketing behavior, consumption, and other credit and savings behavior. Follow-up surveys also collected information on time preferences and on self-reported happiness. Our two other sources of data are monthly price surveys at 53 market points in the study area (which we began in November 2012 and will continue through August 2013), and loan repayment data from OAF administrative records that was generously shared by OAF.

N.2.1 Randomization of treatment intensity

Here we briefly provide additional details on the randomization of the treatment intensity across locations. Our goal in randomizing treatment intensity was to enable us to identify any general equilibrium effects of our intervention. In particular, if the intervention was effective in allowing farmers to shift grain purchase and sales intertemporally, *and* if maize markets are not perfectly integrated within the region (e.g. due to high transportation costs), then in areas with a high density of treatment farmers, we would expect post-harvest prices to be higher and late-season prices to be lower relative to areas with a lower density of treated farmers.

To identify these potential price effects, we need exogenous variation in the density of treatment farmers around each market point. The practical difficulty was that we were unable to gather location information on the relevant market points before the treatments needed to be rolled out, and so could not use these (unknown) market points as a unit of randomization.

The only available strategy was to randomize treatment intensity at the sublocation level, where “sublocations” in this context can be thought of as clusters of villages. To do this, we randomly divided the 17 sublocations in our sample into 9 “high” treatment intensity sites and 8 “low” treatment density sites, fixed the “high” treatment density at 80% (meaning 80% of groups in the sublocation would be offered a loan), and then determined the number of groups that would be needed in the “low” treatment sites in order to get our total number of groups to 240 (what the power calculations suggested we needed to be able to discern meaningful impacts at the individual level). This resulted in a treatment intensity of 40% in the “low” treatment-intensity sites, yielding 171 total treated groups in the high intensity areas and 69 treated groups in the low intensity areas.

Based on information from local OAF staff on the market points in which their farmers typically buy and sell maize, we chose to follow maize prices at 53 of these local market points. These are shown as red dots in the left panel of Figure N.3, and the histograms in the right panel show the distribution across the 53 markets of the number of treated farmers within a given distance from each of these market (1, 3, 5, or 10km). Our stratification procedure appears to have generated substantial variation in the the number of treated farmers surrounding different markets.

As described in the hypotheses on general equilibrium effects below, we pursue two strategies for using this random sublocation-level variation in treatment intensity in the analysis of price effects at these 53 market points.

N.3 Empirical approach and outcomes of interest

We have one baseline and three follow-up survey rounds, allowing a few different alternatives for estimating treatment effects. Pooling treatments for now, denote T_j as an indicator for whether group j was assigned to treatment, and y_{ijr} as the outcome of interest for individual i in group j in round $r \in (0, 1, 2, 3)$, with $r = 0$ indicating the baseline. Following McKenzie (2012), our main specification pools data across follow-up rounds 1-3:

$$Y_{ijr} = \alpha + \beta T_j + \phi Y_{ij0} + \eta_r + \varepsilon_{ijr} \tag{8}$$

where Y_{ij0} is the baseline measure of the outcome variable. The coefficient β estimates the Intent-to-Treat and, with round fixed effects η_r, is identified from within-round variation between treatment and control groups. β can be interpreted as the average effect of being offered the loan product across follow-up rounds. Standard errors will be clustered at the group level.

In terms of additional controls, we follow advice in Bruhn and McKenzie (2009) and include stratification dummies as controls in our main specification. Similarly, controlling linearly for the baseline value of the covariate generally provides maximal power (McKenzie, 2012), but because many of our outcomes are highly time-variant (e.g. inventories) the "baseline" value of these outcomes is somewhat nebulous. As discussed below, for our main outcomes of interest that we know to be highly time varying (inventories and net revenues), we control for the number of bags harvested during the 2012 LR; this harvest occurred pre-treatment, and it will be a primary determinant of initial inventories, sales, and purchases. For other variables like total household consumption expenditure, we control for baseline measure of the variable. Finally, to absorb additional variation in the outcomes of interest, we also control for survey date in the regressions; each follow-up round spanned 3+ months, meaning that there could be (for instance) substantial within-round drawdown of inventories. Inclusion of all of these exogenous controls should help to make our estimates more precise without changing point estimates, but as robustness we will re-estimate our main treatment effects with all controls dropped.

The assumption in (8) is that treatment effects are constant across rounds. In our setting, there are reasons why this might not be the case. In particular, the first follow-up survey began in November 2012 and ended in February 2013, meaning that it spanned the rollout of the January 2013 loan treatment (T2). This means that the loan treatment would not have had a chance to affect outcomes for some of the individuals in the T2 group by the time the first follow-up was conducted. Similarly, if the benefits of having more inventory on hand become much larger in the period when prices typically peak (May-July), then treatment effects could be larger in later rounds. To explore whether treatment effects are constant across rounds, we estimate:

$$Y_{ijr} = \sum_{r=1}^{3} \beta_r T_j + \phi Y_{ij0} + \eta_r + \varepsilon_{ijr} \tag{9}$$

and test whether the β_r are the same across rounds (as estimated by interacting the treatment indictor with the round dummies). Unless otherwise indicated, we estimate both (8) and (9) for each of the hypotheses below.

N.3.1 Main outcomes of interest

We have four main outcomes of interest at the individual level: maize inventories, maize prices paid and received, net maize revenues, and total consumption expenditure. Inventories are visually verified by our enumerator team (nearly all maize stored by smallholders is stored in their home). We define "maize net revenues" as the value of an individual's maize sales over the course of the year minus the value of their maize purchases and the interest paid on the maize loan (if they received it). Consumption expenditure is constructed from recall data on key consumption items across our 3 follow-up rounds, and we compute from these data monthly per capita consumption for each household. We are also interested in general equilibrium effects on maize prices in local markets, which we measure at 53 markets near our sample of farmers.

Baseline data suggest that three of our farm-level outcomes are likely to have a long right tail: there are a few farmers with maize acreage of about 10 times the median, meaning they likely both store and sell more maize. Because of this, our preferred measures of these variables will trim the top 1% of observations by round, although we will report un-trimmed results in robustness checks. For the net revenues, we will trim the bottom 0.5% and top 0.5%, since this measure is not bounded below by zero. Finally, our preferred specifications will estimate effects on inventories and revenues in levels, and on consumption in logs. We focus on levels for revenues because this variable will take on negative values whenever farmers purchase more than they sell. For robustness, we will also estimate effects on consumption in levels.

The study has a few other auxiliary outcomes of interest: the amount of farm inputs used during the 2013 LR, the amount of maize transfers to others, the amount of non-farm income, and measures of subjective well-being. They are described more in the hypotheses below.

N.3.2 Threats to internal validity

The study has two main threats to internal validity: imperfect balance in characteristics of interest between treatment and control groups at baseline, and differential attrition between treatment and control groups groups during the follow-up survey rounds. Baseline balance for a host of baseline characteristics is shown in Table N.1. These appear well balanced across the treatment groups – in only 3 out of 52 cases can we reject balance at 95% confidence, exactly what would be expected by chance – suggesting randomization "worked". Similarly, attrition through the third follow-up was relatively small (8%). Average rates of attrition were actually slightly higher in the treatment groups (8.2% in T1 and 9.6% in T2), relative to the control group (6.9%), but we can only marginally reject (p=0.103) that attrition was higher in T2 than in C, and cannot reject that T1 attrition was higher than in C. Nevertheless, for our family of "main hypotheses" discussed below, we will compute bounds on treatment effects following Lee (2009) in addition to reporting the typical un-adjusted treatment effects.

N.3.3 Approach to hypothesis testing

Our experiment has multiple treatments, multiple follow-up rounds, and collects data on many different outcomes of interest. With the diversity of possible specifications and outcomes available, we want to control for the increased possibility of falsely rejecting a true null hypothesis. To do so, we divide our hypotheses into five "families", and control the family-wise error rate (FWER - the probability of rejecting at least one true null hypothesis) within each family using the free step-down resampling method described in Anderson (2008). This method delivers p-values on each hypothesis that correct for the increased likelihood of incorrectly rejecting the null given multiple hypothesis tests. We will also report "naive" p-values, which are the standard p-values uncorrected for multiple hypothesis tests. Our families of hypotheses, described in detail below, are briefly as follows:

1. *Main hypotheses*: these are the hypotheses about the overall effects of loan access on inventories, revenue, and consumption.
2. *Hypotheses about heterogeneity*: these are hypotheses about how core treatment effects might vary across sub-populations in the sample.
3. *Hypotheses about sub-treatments*: these are hypotheses about treatment effects for the sub-treatments in our experiment (the multiple loan treatments, the lockbox, the tags).
4. *Hypotheses about general equilibrium effects*: these are hypotheses that focus on the market-level price effects of our interventions.
5. *Exploratory hypotheses*: these are additional hypotheses for which our priors are more diffuse, or that examine outcomes that were not the main focus of the study.

N.4 Hypotheses to test

N.4.1 Main hypotheses

For these main hypotheses, we are interested in the overall effect of the package of interventions (loan + tags for all treated farmers, plus lockbox for a subset of both treatment and control), and so pool the two loan treatments and utilize the full sample when evaluating each. Later on we test whether these main treatment effects are driven primarily by the loan itself or by the individually-randomized sub-treatments, and test whether the timing of the loan matters.

H1: Access to the loan package after harvest allows farmers to store maize for longer
The outcome of interest is the amount of maize that farmers have in their store at follow-up visits. Utilizing the full sample and pooling the two loan treatments, we will estimate equations (8) and (9) with maize inventories (measured in 90kg bags) as the outcome. As noted above, we control for the baseline (2012 long rains) harvest, which will be a primary pre-treatment determinant of initial inventories.

H2: Access to the loan package allowed farmers to receive higher prices for the maize they sell, and lower prices for the maize they purchase.
We believe the loan package should allow farmers to more optimally time when they sell and purchase maize. Using data on each farmer's sales and production in each follow-up round, we will average the sales and purchase prices that farmers reported paying or receiving within each round and estimate (8) for both sales prices and purchase prices. We focus on the pooled estimate rather than the round-by-round, because the reduction (gain) in purchase (sales) prices is likely to come through moving purchases or sales around in time, rather than receiving a different price in a given period conditional on buying or selling. We control for purchase and sales prices farmers report receiving in the months following the 2011 Long Rains harvest.

H3: Access to the loan package allowed farmers to increase their maize net revenues.

Net revenues are defined as the value of maize sold, net the value of maize purchased and any interest payments on the loan. We again pool the loan treatments, estimating both (8) and (9). We control for the baseline (2012) long rains harvest.

H4: Access to the loan package increased total consumption expenditure over the course of the year.
Follow-up surveys elicit total consumption expenditure for the household over the previous month, which we use to calculate per capita total monthly expenditure for the household. We again pool the loan treatments and estimate both (8) and (9), focusing on the log of per capita consumption, and controlling for baseline per capita consumption.

N.4.2 Hypotheses about heterogeneity in main treatment effects

We explore treatment effect heterogeneity by interacting the treatments with various baseline covariates of interest. Denoting a given baseline covariate as Z_{i0}, for the pooled model we estimate:

$$Y_{ijr} = \alpha + \beta_1 T_{ij} + \beta_2 Z_{i0} + \beta_3 (T_{ij} * Z_{i0}) + \phi Y_{ij0} + \eta_r + \varepsilon_{ijr} \tag{10}$$

In each case normalize Z_{i0} to be mean-zero, such that β_1 can be interpreted as the effect of the treatment holding the covariate at its sample mean. In these regressions, β_3 is the main coefficient of interest. For each of the below hypotheses, we analyze heterogeneity in treatment effects for inventories, revenues, and consumption, unless otherwise indicated. We again focus on the full sample, later analyzing results for sub-treatments.

H5: Loan treatment effects are larger for those who at baseline were more patient.
If a farmer prefers consumption in the present to consumption in the future, an intervention that allows him to move consumption to the future might have limited effects. Following procedures described in Andreoni and Sprenger (2012), we elicited measures of time preferences for each farmer at baseline (δ_{i0}) using hypothetical questions about when a farmer would choose to sell a given bag of grain under various changes in future maize prices relative to today's prices. We hypothesize that the effect of the loan treatment is larger for those who at baseline were more patient (higher δ). To test this, we pool treatments and estimate (10), with the prediction that $\beta_3 > 0$.

H6: Loan treatment effects are larger for those who have more school aged kids.
In our simple intertemporal model of the storage decision, the resources that are available to the farmer in the early period, and the size of the cash outlay that must be made in that period, determine the extent to which the farmer is forced to liquidate her maize early in the season. We hypothesize that the loan will be more effective for farmers with more school-aged kids in their household – i.e. those who presumably are faced with a bigger cash outlay following harvest. So we define Z_{i0} as the number of kids in the household who are 17 and younger (including kids who do not reside in the household but for whom the household pays school fees), and we pool treatments and estimate (10), with the prediction that $\beta_3 > 0$.

H7: Loan treatment effects are smaller for those with larger liquid non-farm wealth.
As in the previous hypothesis, the resources that are available to the farmer around the harvest period helps determine the extent to which the farmer is forced to liquidate her maize early in the season. With no other sources of income or access to capital, the farmers is forced to liquidate maize

to meet the cash constraint. We hypothesize that loan treatment effects will be smaller for farmers with higher liquid wealth, which we define as the baseline value of their non-farm assets + reported cash savings. For some of these assets (in particular, the non-livestock assets) we unfortunately did not collect baseline estimates of their value, so we will impute values using data from the Kenya Life Panel Survey.[64] We pool treatments and estimate (10), with the prediction that $\beta_3 < 0$: the treatment is less effective for those with higher baseline wealth.

H8: Loan treatment effects are larger for those who had previously liquidated more of their maize immediately post-harvest

A direct measure of farmers' ability to store is baseline data on the percentage of their harvest that they sold immediately post harvest in the previous season. Our hypothesis is that our treatment should be more effective for those farmers who in the previous year immediately sold a higher percentage of their maize harvest. So we define Z_{i0} as the percentage of their 2011 long rains harvest that they sold January 2012, and pool treatments and estimate (10), with the prediction that $\beta_3 > 0$ – i.e. the treatment is more effective for those who had liquidated early the previous year.

H9: Loan treatment effects are larger for those who at baseline expected larger price increases over the next nine months.

If a farmer does not expect prices to rise, then this removes the arbitrage motivation for storing maize. At baseline we elicited price expectations over the coming months. Defining Z_{i0} as an individual's expected percent change in price over the nine-month period following the August baseline (Sept - June), we pool treatments and estimate (10), with the prediction that $\beta_3 > 0$.

N.5 Hypotheses about sub-treatments

H10: On average, the October loan increases inventories, revenues, and consumption more than the January loan.

Our loan intervention was motivated by the hypothesis that farmers' optimal use of storage is constrained by some seasonal cash need. However, it's likely that the timing of when a particular farmer needs this cash will vary. Some individuals might need the cash immediately post-harvest (e.g. in October), and other perhaps some months later (e.g. January of February). If cash received in one month is perfectly transferrable to the next – i.e. if individuals face no pressure to divert this cash to "temptation" consumption, and/or no pressure to give it away to family or friends – then the October loan should on average be more useful than the January loan: it will arrive in time to be used for the October investments, but can also be saved and used for investments later in the season. The January loan will come too late for individuals whose cash needs are earlier, and they will have to liquidate their maize.

So we hypothesize that the October loan increases inventories, revenues, and consumption more than the January loan. To test this, we modify (8) and (9) to include separate dummies for each treatment, i.e.

$$Y_{ijr} = \alpha + \beta_1 T1_j + \beta_2 T2_j + \phi Y_{ij0} + \eta_r + \varepsilon_{ijr} \tag{11}$$

Our hypothesis is that $\beta_1 > \beta_2$ for inventories, revenues, and consumption.

[64] See the following website for more information on KLPS:
http://cega.berkeley.edu/research/kenya-life-panel-survey-long-run-outcomes-of-childhood-interventions-in-kenya/

H11: For those individuals with later-season consumption needs and for whom cash on hand is likely to be leaky, treatment effects are larger for the the January loan than the October loan.
There are specific instances when the January loan might prove more effective than the October loan. In particular, for individuals for whom it is both problematic to have cash lying around *and* for whom the major cash need is after January, the January loan could be more useful. That is, for a given loan amount, more of the January loan will directed toward the productive investment for these individuals.

At baseline, we asked individuals to anticipate their monthly expenditures over the next six months (Sept 2012 through Feb 2013). Let L_i represent the percent of 6-month expenditures that individual i expected to spend after January. Baseline data also give us two measures of "leakiness": the extent of an individual's present bias, and the extent to which they were "taxed" by their network at baseline. We calculate the former through standard hypothetical questions about inter-temporal choice, and we construct the latter by calculating whether, over the three months prior to the baseline survey, they gave away to friends and family more maize than they received. Denote either of these measures as γ_i, with larger values indicating either higher present bias or higher net transfers.

The hypothesis requires testing a triple interaction between the treatment indicator, the L_i measure, and the γ_i measure. Restricting our sample to the individuals in the two loan treatment groups, and ignoring rounds in the notation, we estimate:

$$Y_{ij} = \alpha + \beta_1 T2_j + \beta_2 L_{ij} + \beta_3 \gamma_{ij} + \beta_4 (T2_j * L_{ij}) + \beta_5 (T2_j * \gamma_{ij}) + \beta_6 (T2_j * L_{ij} * \gamma_{ij}) + \phi X_{ij0} + \varepsilon_{ijr} \quad (12)$$

Our hypotheses is then that $\beta_6 > 0$. The outcomes of interest are again inventories, revenues, and consumption.

H12: The effect of the loan treatment was not due to the tags alone.
All farmers who took up the loan also received tags that designated certain bags as collateral. As suggested by extensive focus group discussions with farmers, these tags could have their own impacts on storage and consumption, allowing farmers a way to shield stored maize from claims by friends and family. The overall treatment effects estimated in the "main hypotheses" are thus a combination of the effect of the loan, the effect of the tags, and their interaction:

$$\beta = \text{effect of loan} + \text{effect of tag} + \text{effect of (loan*tag)}$$

We do not have the full 2 x 2 design to isolate all three effects. Nevertheless, we can estimate:

$$Y_{ijr} = \alpha + \lambda Ct_{ij} + \beta T_j + \phi Y_{ij0} + \eta_r + \varepsilon_{ijr} \quad (13)$$

where Ct_{ij} is an individual who was in the loan control group but received tags, and T_j again denotes those in the (pooled) loan treatment groups. Here λ delivers the effect of the tag, and so in the case where there is no interaction effect between the loan and the tags, $\beta - \lambda$ measures the effect of the loan without tags. Our hypothesis is thus that $\beta > \lambda$. Nevertheless, we will not be able to rule out that this difference is due to an interaction effect between the loan and tags. However, the simple tag "treatment" is likely to be something included in any such loan offer in the future (if not a tag, then some comparable indication of a formal loan that the farmer could use for the same purpose), and so the interaction with the tag will likely be part of any scaled up effect.

H13: Tags alone increase inventories, revenues, and consumption.
Focusing on the individuals in the main control group who were not offered the loan or lockbox, we first run:

$$Y_{ir} = \alpha + \lambda Ct_i + \varepsilon_{ir} + \phi Y_{i0} + \eta_r + \varepsilon_{ir} \tag{14}$$

hypothesize that $\lambda > 0$ for our three main outcomes.

H14: The effect of tags is larger for people who were more "taxed" by their network at baseline.
The using the network taxation measure described above, we estimate the interacted model using the same individuals:

$$Y_{ir} = \alpha + \lambda_1 Ct_i + \lambda_2 \gamma_i + \lambda_3 (Ct_i + \gamma_i) + \phi Y_{i0} + \eta_r + \varepsilon_{ir} \tag{15}$$

and our hypothesis is that $\lambda_3 > 0$ for inventories, revenues, and consumption.

H15: Loan treatment effects are larger for those who received the savings lockbox.
We hypothesize that our simple savings technology could help cash "stick around" and get spent on the intended (presumably high return) maize storage investment, and/or it could help channel the earnings from this investment into other productive uses (including loan repayment). We will estimate:

$$Y_{ijr} = \alpha + \beta_1 Tn_{ij} + \beta_2 Tb_{ij} + \phi X_{ij0} + \eta_r + \varepsilon_{ijr} \tag{16}$$

where Tn is an indicator for being in a loan treatment group and not getting the lockbox, and Tb is an indicator for getting both the loan offer and the lockbox. Our basic prediction is that $\beta_2 > \beta_1$, i.e. the savings technology increases the effectiveness of the loan. As before, we look at inventories, revenues, and consumption, and the difference in coefficients will capture both the effect of an improved ability to invest in storage due to the lockbox as well as the gains from doing so.

N.5.1 Hypotheses about general equilibrium effects

H16: Markets with more treatment farmers nearby had smaller inter-seasonal price spreads.
Our hypothesis is that our intervention raised post-harvest prices at markets surrounded by more treatment farmers, and lowered prices during the peak season at these same markets, thus reducing the overall spread in prices between the two seasons. As explained above, we randomized the treatment intensity across the 17 sublocations in our sample, and we tracked monthly prices at 53 market points spread out across these sublocations. The difficulty is that the markets do not map cleanly into the sublocations, and it is almost certainly the case that some market points are used by farmers in multiple sublocations.

We pursue two strategies to estimate the effect of our package of interventions on market prices. In the first strategy, we use our farmer and market location information to calculate, for each market point, the modal sublocation of the farmers within a given radius – i.e. the sublocation to which the majority of farmers within a given radius of a particular market belong – thus matching each market point to its sublocation treatment. As a second strategy, we follow the approach in Miguel and Kremer (2004) and simply count up the number of treatment farmers within a given radius of each market point (the distributions of these counts for 1, 3, 5, and 10km are shown in Figure N.3). Because treatment was assigned randomly across groups, the number of treatment farmers in each location should also be random.

Our price surveys began in November 2012, and for each market point we define the price spread as the percentage change in price between November 2012 and June 2013. We regress this price change on either the matched sublocation binary treatment intensity indicator, or on the count of treated farmers within a 3km radius. We choose 3km as our base specification (somewhat arbitrarily), and will explore robustness to counts of farmers within 1km and 5km radii. Because prices are likely correlated across our market points, standard errors should account for this spatial correlation, and we report spatial standard errors following Conley (1999) as well as the unadjusted standard errors.

While of substantial empirical interest, we anticipate that these regressions will be substantially underpowered, both because (in the first case) treatment is measured with error, but more importantly because our treated farmers likely make up a small proportion of the total number of farmers participating in these markets – and thus our intervention will likely only have a small effect on local demand and supply. We will report results nevertheless.

N.5.2 Exploratory hypotheses

The following are hypotheses about outcomes that were not the main focus of the study, or are questions that we believe to be interesting but for which we have fairly diffuse priors on the direction of effect.

H17: Access to the loan package increased investment in farm inputs for the 2013 Long Rains

Basic models of profit maximization indicate that farmers' choices about the amount of a given input to use depend directly on the value of its marginal product. We hypothesize that the loan should raise this marginal product by raising effective output prices (H2) and thus, to the extent that farmers expected the loan program to continue – and there was no indication in the marketing that it wouldn't continue – it should thus raise the amount of inputs that treated farmers use in anticipation of marketing future harvests. It is also possible that farmers are liquidity constrained in input purchases. While this is less likely for our study sample – they are all OAF clients, and so receive some amount of inputs on credit already – many are capped at the amount of land they can enroll in the OAF program, and end up purchasing inputs for any remaining area they sow to maize or other crops. So access to the loan could also directly affect their ability to purchase inputs on this land.

At the third follow-up, we collected detailed data on the quantity and value of inputs used on each farmer's maize and two other main crops during the 2013 Long Rains. Our main outcome of interest will the be the value of all purchased fertilizer, hybrid seed, and other chemical inputs across the farmers' maize acreage (not counting any inputs that farmers received from OAF), and we will estimate treatment effects using equation (8) and data from the third follow-up.

H18: Access to the savings lockbox alone increased investment in farm inputs, and increased consumption expenditure.

Existing work suggests that access to a simple savings technology can increase business investment and boost consumption outcomes. Using the control farmers who did not get the loan, we compare outcomes for the farmers who received the lockbox to those who received nothing, i.e.:

$$Y_{ir} = \alpha + \lambda Ct_i + \phi Y_{i0} + \eta_r + \varepsilon_{ir} \tag{17}$$

Our outcomes of interest are the investment in farm inputs for the 2013 long rains (in Feb/March 2013, as measured in the 3rd follow-up), and total consumption expenditure. Our hypothesis in both cases is $\lambda > 0$.

H19: Access to the savings lockbox lead to faster loan repayment.
Using administrative data from OAF, we compare whether individuals who received the lockbox had more quickly repaid their OAF loan relative to individuals who did not receive the lockbox. Our outcome measure is the % of an individual's total loan that had been repaid by June 1.

H20: The loan treatment reduced maize transfers to others.
If farmers are choosing to make minimal use of storage because any stored maize is subject to external claims by friends and family, our treatments (if effective) could reduce transfers made to these outside members. We hypothesize that this is the case: that those in the loan treatment groups reduced their transfers of maize to family and friends not in their household. We collected data on maize transfers to outside members at each survey, and so will estimate (8) with maize transfers as the outcome. Our hypothesis is that $\beta < 0$.

We also want to know whether having the loan alone allowed them to reduce transfers (e.g. by credibly claiming that they needed either the cash or maize for loan repayment), or whether the tags were the key element (visual proof of the loan obligation). To analyze this, we estimate (13) again with maize transfers as the outcome. We do not have a strong prior on the relative magnitude of λ versus β.

H21: The loan treatment increased off farm income.
We conceived of the loan treatment as a way for farmers to meet a cash constraint (e.g. pay school fees). However, there was no restriction on how the money was spent, and it's possible that farmers invested the money in non-farm businesses. Alternatively, farmers could have used the loan to pay school fees, sold their maize at a higher price as intended, and then invested this income in non-farm businesses (as many indicated they would like to do at baseline). We collect data on non-farm income in both baseline and the third follow-up, and so will pool the treatments and estimate (8) using data from the third round, with off-farm income as the outcome. Our hypothesis is that $\beta > 0$.

H22: The loan treatment increased subjective well-being and optimism about the future.

In each follow-up survey, we asked two standard questions about subjective well-being: "*Taking everything together, would you say you are somewhat happy, very happy or not happy?*", and "*I believe that if I try hard, I can improve my situation in life*" (with 1=agree strongly to 4=disagree strongly). In the 3rd follow-up, we also included the following questions: "*Finally, please imagine a 10-step ladder, where on the bottom, on the first step, are the poorest 10% of people in your village, and at the top step are the richest 10% of people in your village. On which step out of 10 is your household today?*", and "*Where on that same ladder do you think your household will be a year from now?*". We will standardize each of these measures to be mean 0, standard deviation 1, and our main measure of subjective well-being will be an average across these standardized measures. We will estimate (8) with this average as the outcome, and will also examine each component of the average as robustness. Although the additional debt taken on by treatment households could lower well-being, our hypothesis is that the loan treatment had a positive effect on farmers' views of their current and future well-being.

H23: Loan treatment effects are larger for men than for women.

Past studies on cash grants have shown strong heterogeneity by gender, with returns much higher for men than for women in some settings (e.g. De Mel et al. (2009)). We test whether this is the case in our setting, defining Z_{i0} as a dummy for "male" and estimating (10) with inventories, revenues, and consumption as outcomes.

H24: The loan treatment altered time preferences.

The stability of time preferences is an unresolved topic of substantial theoretical and empirical interest (Meier and Sprenger, 2010), and given our repeated collection of time preference data over the follow-up rounds, it is something that can be examined in our data. It's possible that respondents in our sample could display seasonality in their time preferences – e.g. appearing more impatient in the lean season – and thus possible that our intervention could affect these preferences if it raises consumption during this period. Similarly, it's possible that a successful experience with longer-term storage could change individuals' preferences about present versus future consumption. We collected time preference data at each survey round, and will estimate both (8) and (9), with our estimate of δ as the outcome (described above). Our hypothesis is that $\beta > 0$.

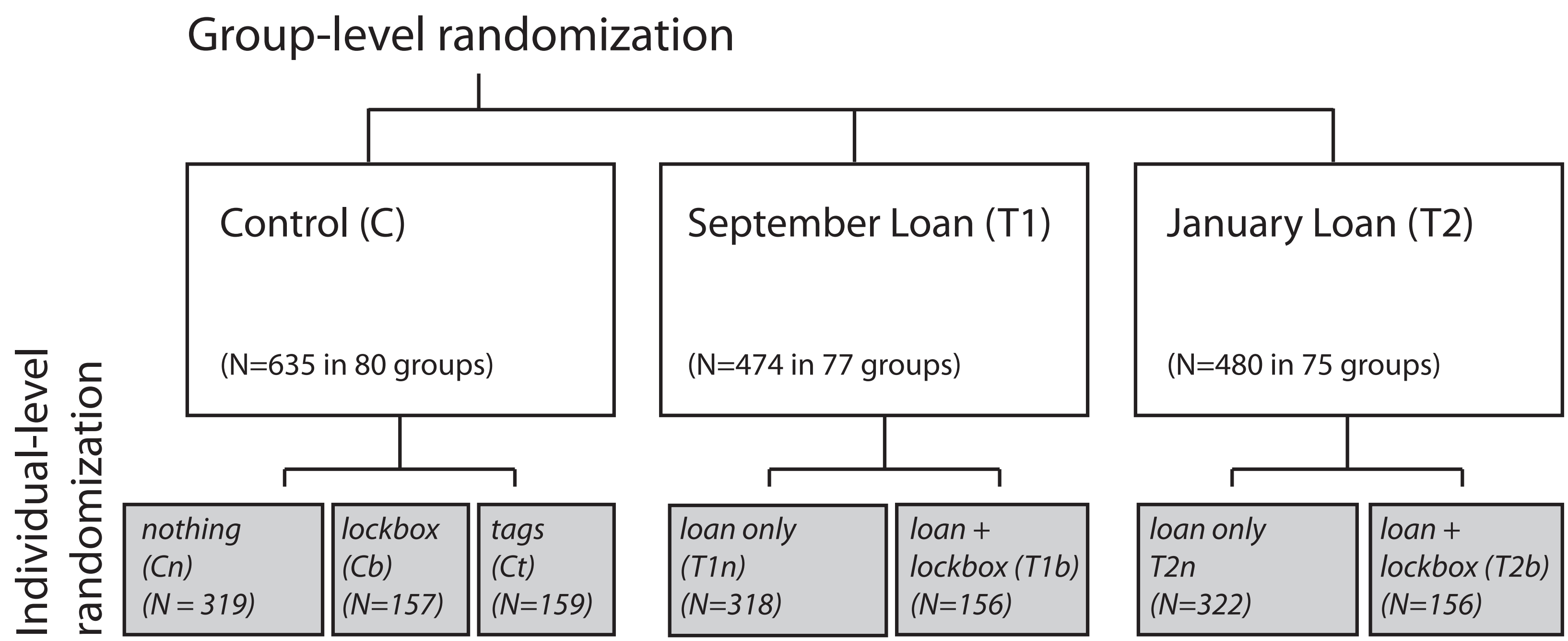


Figure N.1: **Study design**. White boxes represent group-level loan treatments, and grey boxes represent individual-level treatments. Sample sizes in each treatment are provided in parentheses.

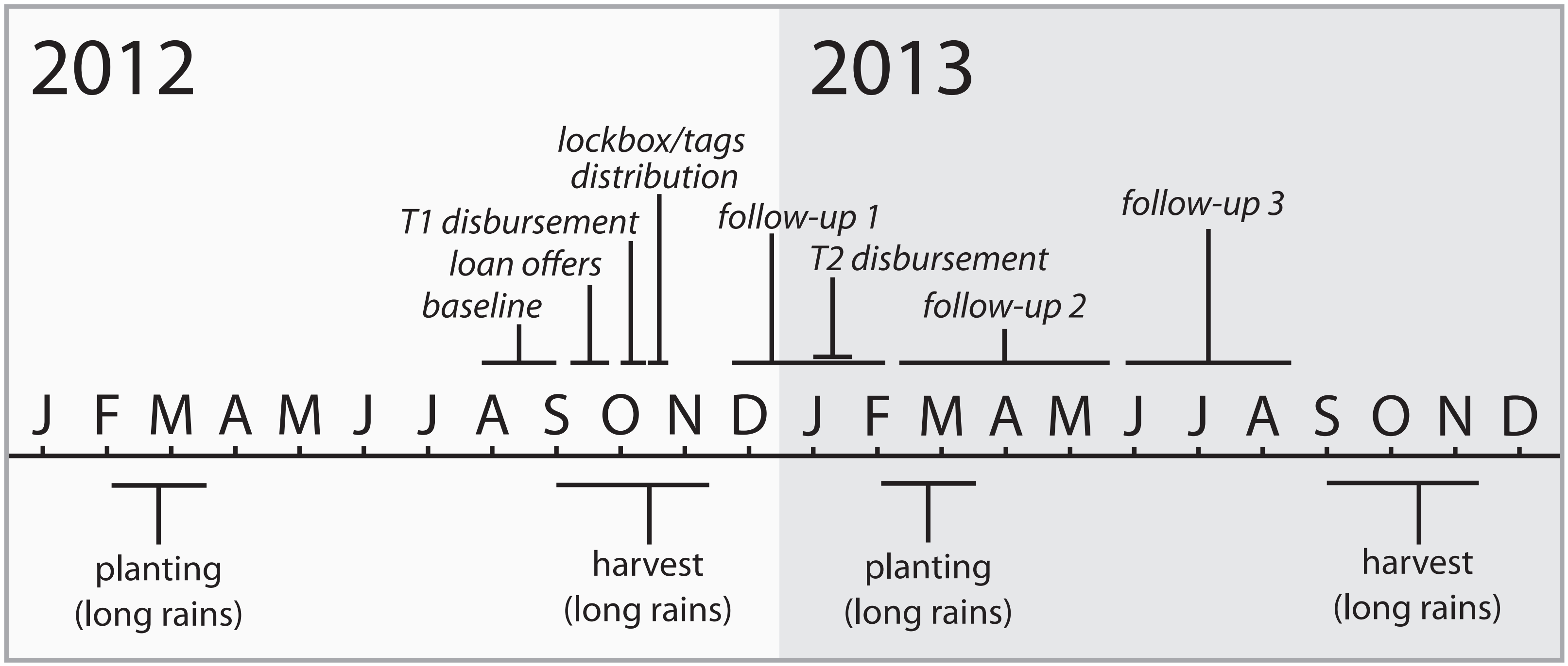


Figure N.2: **Timeline of interventions and data collection**. The timing of the main agricultural season is shown at the bottom.

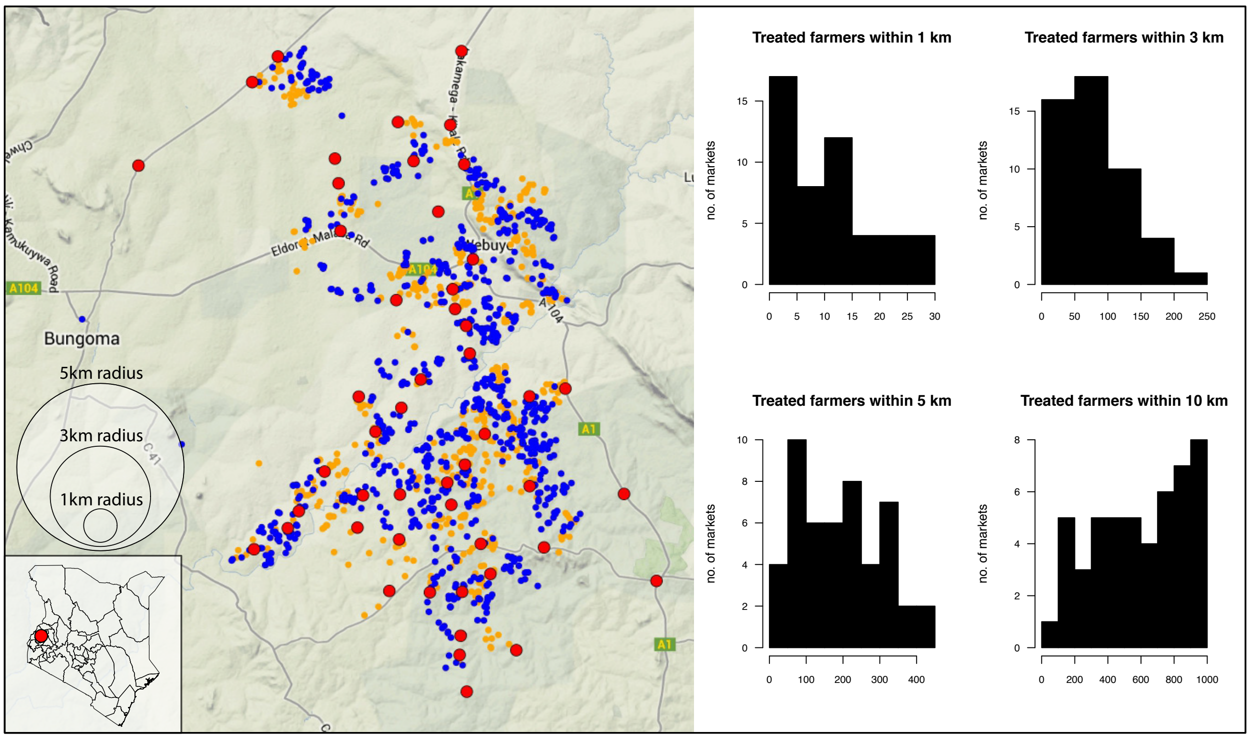


Figure N.3: **Location of households and markets**. Large red circles show the 53 markets where we measure maize prices, blue circles show loan treatment households, and orange circles show households in the control group. Histograms at right show the distribution across markets of the number of treatment farmers within the indicated number of kilometers of each market.

Table N.1: **Summary statistics and balance among baseline covariates.** The first three columns give the means in each treatment arm. The 4th column gives the total number of observations across the three groups. The last four columns give differences in means normalized by the Control sd, with the corresponding p-value on the test of equality.

Baseline characteristic	Treat	Control	Obs	T - C	
				std diff	*p-val*
Male	0.30	0.33	1,589	-0.08	0.11
Number of adults	3.00	3.20	1,510	-0.09	0.06
Kids in school	3.00	3.07	1,589	-0.04	0.46
Finished primary	0.72	0.77	1,490	-0.13	0.02
Finished secondary	0.25	0.27	1,490	-0.04	0.46
Total cropland (acres)	2.44	2.40	1,512	0.01	0.79
Number of rooms in hhold	3.07	3.25	1,511	-0.05	0.17
Total school fees (1000 Ksh)	27.24	29.81	1,589	-0.06	0.18
Average monthly cons (Ksh)	14,970.86	15,371.38	1,437	-0.03	0.55
Avg monthly cons./cap (log Ksh)	7.97	7.96	1,434	0.02	0.72
Total cash savings (KSH)	5,157.40	8,021.50	1,572	-0.09	0.01
Total cash savings (trim)	4,731.62	5,389.84	1,572	-0.05	0.33
Has bank savings acct	0.42	0.43	1,589	-0.01	0.82
Taken bank loan	0.08	0.08	1,589	-0.02	0.73
Taken informal loan	0.24	0.25	1,589	-0.01	0.84
Liquid wealth	93,878.93	97,280.92	1,491	-0.03	0.55
Off-farm wages (Ksh)	3,916.82	3,797.48	1,589	0.01	0.85
Business profit (Ksh)	2,302.59	1,801.69	1,589	0.08	0.32
Avg %Δ price Sep-Jun	133.49	133.18	1,504	0.00	0.94
Expect 2011 LR harvest (bags)	9.36	9.03	1,511	0.02	0.67
Net revenue 2011	-3,303.69	-4,088.62	1,428	0.03	0.75
Net seller 2011	0.32	0.30	1,428	0.05	0.39
Autarkic 2011	0.07	0.06	1,589	0.03	0.51
% maize lost 2011	0.02	0.01	1,428	0.03	0.57
2012 LR harvest (bags)	11.18	11.03	1,484	0.02	0.74
Calculated interest correctly	0.71	0.73	1,580	-0.03	0.50
Digit span recall	4.57	4.58	1,504	-0.01	0.89
Maize giver	0.26	0.26	1,589	-0.00	0.99