Quantitative Evaluation of the Social Fund for Development Labor Intensive Works Program (LIWP)

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Summary

The Labor Intensive Public Works Program (LIWP) in Yemen transfers funds to poor rural households by employing community members in local public works projects.

This evaluation uses a panel of household surveys collected before and after the intervention in randomly selected communities to examine the program's effectiveness in improving economic outcomes. Due to the timing of the evaluation during the 2010-2011 political and economic crisis, economic indicators worsened for all communities in the sample, but treatment communities fared better than control communities in some measurable ways. We use a difference in differences approach to test for the causal effect of the LIWP program on various outcomes at the community level. The total benefit of the program to individual participating households is greater than suggested by this community level analysis, as households in treatment communities that had low or no participation in the program are included in calculating the program effect. Because participation level is endogenous, we are unable to control for variation in treatment by household.

In the survey sample, 74% of households in treatment communities had at least one member participating in LIWP. There was a wide range in the level of income that these households received from the program, with 29% of households receiving 50,000 riyals ($235) or less, and 5% of households receiving more than 300,000 riyals ($1415). This variation is a result of several factors. First, at the household level, LIWP was designed to be self targeting by setting a wage lower than the prevailing unskilled wage in the area, but due to the economic crisis, the average wage level fell, resulting in LIWP employment being more attractive than originally designed. Secondly, there was originally an intention to limit households to a maximum number of work days, but this was not enforced during implementation. Finally, program wages were set by piece rate, resulting in higher wages per day for workers involved in more skill intensive tasks, or who worked longer hours. Correspondingly, the number of men in the family was the strongest predictor of program participation and total benefits received. In spite of this variation, LIWP benefits were

progressive overall, with more benefits going to households whose scores on a proxy means test on baseline were associated with greater probability of poverty, and Gini coefficients for household income falling in communities with LIWP programs.

There is evidence that the LIWP program increased the total number of days worked per household by approximately 50 days per year. More clearly, the data show that the program increased average wages and shifted the structure of the workforce away from work in the lowest paid sectors. The program also caused a significant increase in the probability of female employment.

In response to direct questions, 95% of program participants indicated that most funds from the program were used to buy food and pay off debt. 59% of households reported using income from the project for food and paying off debt only, while an additional 36% spent on food and debt as well as other types of consumption. We find statistically significant program effects on food consumption, debt repayment, and durable goods ownership. On average, households in treatment communities had increased per capita calorie consumption of staple foods relative to control of between 320 and and 435 per day, and were less likely to self-report that adults and children were forced to skip meals due to food shortage. Among indebted households, households in treatment communities were able to pay off more debt, by $123 on average. Households in treatment communities also had less decrease in the value of durable goods owned. These findings suggest that the LIWP program played a role in cushioning targeted communities from the economic shock of 2010-2011, averting possible longer term consequences related to selling off assets and increased debt. Economic indicators in which we do not find a significant treatment effect include animal asset ownership, consumption of higher value food items, and consumption of non-durables such as clothing and household goods.

When asked directly about their perception of the program effect on community assets, 95% of respondents reported that the project was needed by the community and 79% of households reported that they benefited directly from the infrastructure built by the project. Only a few projects were officially completed at the time of the expost survey. In spite of this small sample size, we find significant improvements in access to water in communities where the LIWP funded project was related to water availability. [1]

Chapter 1

Introduction

1.1 Social Assistance and Productive Safety Nets

Social assistance programs are increasingly being recognized as a key part of development. By protecting the assets of the poor and reducing vulnerability to poverty, they can play an important role in driving growth. Social protection programs are especially effective and attractive when a "'twin-track"' strategy is used. This refers to providing short-term social protection while making positive long-term investments. Conditional cash transfers, for example, promote investment in human capital, while public works projects generate physical capital, each while providing direct benefits in the short term. [2]

1.2 LIWP Program Design

The Labor Intensive Public Works Program (LIWP) in Yemen was designed using the "'twin-track"' approach. The program transfers funds to poor rural households by creating short term employment in the construction of local infrastructure. The goal is for income from program wages to provide short term protection against negative consumption shocks, while the public works projects themselves provide medium to long term benefits for the community in adapting to water scarcity. The infrastructure created by the program was chosen by the communities. LIWP construction projects included reclamation of agricultural lands from harmful plants, protection of irrigation canals and water sources, improvement of rural roads, paving of rural markets, rainwater harvesting, construction of shallow wells, and terrace repair. [1]

The second phase of LIWP included 190 of the poorest communities that had been recently hit by shocks to food consumption. These targeted communities were selected based on village-level poverty indicators in consultation with SFD local branch offices, followed by field verification. At the household level, LIWP was designed to be self-targeting. Project wages were set 10% lower than the prevailing wage in the area for unskilled work. This strategy was designed to make project work attractive to the poorest community members, while discouraging better-off community members from participating.

The construction projects funded by LIWP were chosen by targeted communities in consultation with SFD technical staff. Skilled labor required for the projects could be drawn from outside of the community, but unskilled labor was required to be provided from within the community only and it was intended to be allocated fairly among households. The projects were scaled to provide enough work for households that indicated their interest in participating to work an average of 115 days. While an earlier phase of LIWP had attempted to allocate workdays in proportion

to the food needs of participating households, this approach was amended to equal allocation for all participating households. However, neither form of allocation appears to have been used consistently in the field during the second phase.

1.3 Research Questions

The purpose of this evaluation is both to examine the effectiveness of LIWP at targeting the poorest households and to measure changes in welfare that can be attributed to LIWP. The following questions provide an outline for the report:

Targeting and Participation Who in the villages benefited most from the program? How did program participants differ from non-participants?

LIWP Impact on Household Employment, Income, Assets, and Consumption Was the program effective in increasing total employment or did it replace other employment? Did the program protect households from selling off assets to survive during the crisis? How did LIWP impact household consumption and debt repayment?

LIWP Impact on Food Security Focusing specifically on food security, what was the impact of LIWP on household consumption?

Impact of LIWP Constructed Infrastructure How did households benefit from the infrastructure constructed by LIWP? Was access to water improved by water projects? Was access to local markets improved by road projects?

1.4 Evaluation Design

The evaluation was designed to measure the impact of LIWP in communities scheduled for treatment by the program. The target population consists of 120 communities that were to receive LIWP in the second phase during either 2010 or 2011. The basic idea was to randomize the communities that benefited during 2010. To increase the quality of the randomization, communities were regrouped in pairs or small clusters with similar geographic and economic characteristics. This grouping was done by branch staff. They were explicitly told that the objective was to group communities that would a priori be expected to benefit in a similar way from LIWP, so as to be guided on what characteristics matter most in the matching (employment opportunities, economic activities, etc.). Then within each of these clusters, half of the communities were randomly selected to receive LIWP projects in 2010 ("treatment communities"), and half of the communities to remain as control until after the expost survey. The outcome of this process was thus 60 treatment and 60 control communities.

In each of the treatment and control communities, a list of all households was established, and 12 households were randomly selected to participate in the evaluation. A baseline survey, including both community and household surveys was conducted in May 2010. Due to delays related to the political situation in Yemen, communities and households were re-surveyed in November 2011. This gives the basis for a double difference estimation strategy, on a clustered randomization of communities.

1.4.1 Impact Evaluation Strategy

The primary challenge in impact evaluation is to identify changes which are attributable to the program intervention (the causal effect of the program) rather than to other factors. In this impact evaluation, we are fortunate in having access to a randomized control trial (RCT) of the LIWP program. Because the assignment to treatment or control for the communities in this sample was carefully randomized, all other factors except for treatment assignment should be similar in

the two groups. This means that the control communities provide evidence for what would have happened to the treatment communities in the absence of the LIWP intervention, and we can attribute any differences between treatment and control to the causal impact of the program.

While we are confident that the original randomization was unbiased and do not see much evidence of differential attrition (see tests in Appendix I), we do have a relatively small sample size, which means that treatment and control samples may differ in some dimensions more than we would like. To control for this, we also use the differences in differences strategy. This estimation strategy allows us to control for cases in which other factors may not be perfectly balanced between the treatment and control. First, we measure the difference in outcome variables over time between baseline (prior to program intervention) and expost (after the program intervention) in treatment communities. This difference includes both changes attributable to the program, and changes attributable to other factors that would have occurred without the program. By measuring the same difference in control communities, we can estimate the magnitude of the change due to other factors over this period of time. Then, we take the "double difference": the difference over time in treated communities minus the difference over time in control communities. The difference in outcome variables that remains after subtracting change due to other factors is the change attributable to the program.

The use of both a randomized control trial and double differences provides an extra level of assurance that the differences we find between changes in treatment and control communities is attributable to the LIWP program, and not to any other factors.

1.4.2 Issues Encountered with Randomization During the Survey Period

Delays occurring in one of the governorates prevented the inclusion of one of the control communities in the baseline survey. Further, between baseline and expost, there were 12 instances of treatment assignment not being respected. Some of the causes of changes in treatment assignment were confusion about the names of communities, community disagreement delaying implementation, and decisions by local project officers to increase the number of programs via intervening in control communities. In addition, some communities were dropped from the sample due to breaking conditions of the program regarding qat plantations, ongoing conflicts that made resurveying impossible, or flooding. In most of these cases, neither the community nor its pair was resurveyed. In total, 84 communities participated in the expost survey, 44 treatment and 40 control.

In 8 communities, the change in treatment assignment was not discovered until the expost survey. We will deal with this issue by using treatment assignment as an instrument for actual treatment status.

Attrition at the household level was reasonably low. Only 100 of the baseline 1004 households could not be relocated. The following table summarizes the sample definition and number of observations.

	Baseline Sample Original Assignment	Resurveyed Communities Actual Assignment	Resurveyed Communities with no Change to Assignment
Communities	119 communities (60 control, 59 treatment),	84 communities (40 control, 44 treatment)	76 communities (38 control, 38 treatment)
All households (including replacement)	1428 households (720 treatment, 708 control)	1004 households (526 treatment, 478 control)	898 households (443 treatment, 455 control)
Panel households	n/a	954 households (461 treatment, 493 control)	854 households (426 treatment, 428 control)

1.4.3 Balancing Tests on the Randomization

Because we eliminated clusters in most cases whenever there were a problem with the treatment communities, and household level attrition was low, balance of variables should be achieved both at baseline among the 76 panel communities. Generally, we find that community characteristics were balanced in the sample used for analysis. Results of these tests are reported in Appendix I. The only characteristic which differed significantly between treatment and control was average skill level, and this characteristic differed significantly even in the original sample, so the difference is likely to be simply a random occurrence given the small sample size, rather than indicating a systematic difference between communities that remained in the sample and communities that had problems or treatment assignment was not respected.

1.4.4 Estimation of the Intention to Treat Effects

We use the difference-in-differences approach to test for the causal effect of the LIWP program on various outcomes at the community level. Since treatment was randomized at the community level, communities in the control group serve as an appropriate counter-factual.

For the majority of the outcome variables, data is reported at the household level. The estimating equations are of the form: $y_{cth} = \mu_c + Expost_t + \beta * LIWP_{ct} + \epsilon_{cth}$ where y_{cth} is the outcome for household h in community c at time t, μ is a community fixed effect, $Expost$ is a dummy variable indicating the time trend, and $LIWP$ is the variable of interest which is equal to one for treated communities in the expost survey. Where indicated, fixed effects are sometimes included at the household level. In all regressions, standard errors are clustered at the community level.

The total benefit of the program to participating households (effect of treatment on the treated) is greater than suggested by this community level analysis, as households in treatment communities that had low or no participation in the program are included in calculating the program effect. Because participation level is endogenous and poorly predicted by observable characteristics, we are unable to control for variation in treatment by household. The average effect of treatment on the treated can be estimated by dividing the results in this analysis by the average participation level of 0.74.

1.5 Political and Economic Context

The baseline survey for this evaluation was collected in May 2010, and the expost survey was collected in November-December 2011. Between these two dates, the Arab Spring of 2011 was associated in Yemen with widespread protests, incidents of armed conflict, and economic paralysis due to fuel shortages and general instability.

Consequently, we find a negative time trend in most outcome variables between the baseline and expost surveys. Since we can assume that treatment and control communities would have been similarly affected by the crisis, the differences in differences design controls for this negative trend. Measurably positive results of the LIWP program in the differences and differences framework should therefore be seen as mitigation of the negative effects of the crisis in treatment communities.

Chapter 2

Participation in LIWP

The initial issue addressed by this impact evaluation is to describe the level of program participation and analyze how well the program targeted poor households. In this chapter, we first present data showing how project income was distributed among households. Then we look at two measures of how well program benefits were directed towards poor households. Finally, we summarize responses in the survey regarding satisfaction with the way that program benefits were distributed.[1]

2.1 Summary Statistics on Project Benefits

Out of 426 households in treatment group, 315 households (74%) had at least one member participating in LIWP during the past year. 287 households (67%) participated in the unskilled labor portion of the project. We focus much of our analysis on participation in unskilled labor.

As noted above, the original program design evolved from allocating days of work in proportion to household need to the more easily administered equal distribution of workdays by household. However, the data show that this intention of allocating workdays equally was not enforced in practice. For example, it was intended to set an upper limit on the household benefit of $700, or approximately 115 days of work, but in our survey data, some households worked much more than this limit and most households worked less.

According to program staff, during the round of the program analyzed in this evaluation, there were known problems with tracking the total number of days worked per household. Also, due to the economic crisis, many more households than originally estimated wanted to work in the program once it began and field managers did not have a plan in place for responding to this influx of workers. Future rounds will use a new computerized system for tracking, which should result in greater equality of benefits among participants.

In both household surveys and in administrative data collected by the program, it is clear that there was a wide variety across households in the total number of days worked in LIWP, with most households working less than 50 days. Some of the variation is due to differences in household size and other factors that determine level of demand for program participation, such as outside employment opportunities. In this case, the variation would support the program's effort to target benefits to the households with the most need. The variation in benefits is concern, however, to the extent that it was due to factors influencing household access to project benefits.

Figure 2.1 shows the distribution of days worked per household among households participating in the unskilled portion of the program. Examining the distribution of days worked by community (not shown) also does not indicate any clustering around a single level per community, as would be expected if local staff had reduced the maximum level of participation to accommodate increased demand for participation. We focus only on days worked in the unskilled labor portion of the

[1] Further discussion about community perspectives on the program design is found in the qualitative evaluation.

project,[2] as this was the portion in which benefits were intended to be focused on the target community and distributed equitably among participating households.

Looking at money received rather than days worked, there is also a wide range of values, with 29% of households receiving 50,000 riyals ($235) or less, and 5% of households receiving more than 300,000 riyals ($1415). Figure 2.2 shows the distribution of total LIWP income among households participating in the unskilled portion of the project.[3]

In addition, there is a weak relationship between money received and days worked, due to the fact that compensation was based on piece rate rather than days worked. Participants worked in groups which were paid piece rates based on work completed by the group and the group leader calculated the units of work achieved by individual group members in cooperation with the technical consultant. More productive individuals, therefore, or those who worked longer hours, could receive higher implied wages per day, even for the same type of work. Also, even within the category of "unskilled" labor, some types of work were paid at higher rates because they were more difficult. These factors may explain the significantly higher implied daily wages for males than females.

While the survey did not include a question about wages or piece rates, we calculate implied daily wages as total LIWP income per participant divided by days worked. Figure 2.3 shows the distribution of implied wages. Most participants received an implied wage of between 1000 and 2000 riyals per day (approximately $5-$10), but the distribution has a long right-hand tail even though skilled workers are excluded. [4]. As shown in table 2.5, most of the variation in implied daily wage is not explained by task dummies or community fixed effects, as would be expected if the variation was driven only by different wage levels. Part of the remaining variation may be explained by hours worked per day, which is not captured by the survey data, or by family members assisting informally without enrolling in the program.

2.2 Evaluation of Targeting Effectiveness

We are interested in measuring the degree to which the program was effective in encouraging self-targeting in participation. As explained above, the program design called for the equal allocation of unskilled work by household among any household interested in participating, but in the survey data, we find a degree of variation of benefits between participating households that suggests the program design of equal allocation was not fully adhered to and that labor-constrained households benefited less from the program than households with more able-bodied men. We do find, however, that poor households were overall more likely to participate in the program and to work for more days and to receive greater income from the program than better-off households.

A proxy means test designed by the Social Fund was administered to a sample of 10% of the community prior to implementation to estimate the labor size of the project. Since the same variables are available in the household survey, we are able to calculate the the proxy means score for households in our sample as an indicator of their poverty level to see how well the program was targeted. The components of the score with the greatest weight are size of household, number of rooms in the house, enrollment of children ages 12-18, and ownership of durable goods.

[2]The survey asked whether individuals participated in unskilled or skilled labor or both, and for total workdays per individual. It was uncommon for households to have participants in both skilled and unskilled labor, so we exclude all households participating in skilled labor (leaving 235 households) when analyzing the distribution of workdays.

[3]When looking at total benefits received, we restrict the sample to only communities where the program is completed (159 households), since data on days of work was based on a question that asked about total days for the duration of the project including days scheduled by contract but not yet completed, while the question about project income referred only to income received so far in incomplete projects.

[4]The value for days of work in LIWP is taken from section 11 in the survey (LIWP participation), rather than from the employment module to avoid the problem of miscounting days of work due to greater than full-time employment

2.2.1 Correlates of Participation and Wage Levels

Participation rates in the unskilled labor portion of the project varied by branch, with the lowest rates of participation in Aden, Taiz, and Amran. Table 2.2 summarizes participation rates at the branch level.[5] The amount of variation in benefits also differed by project branch, as shown in table 2.1, with Taiz and Amran having particularly high coefficients of variation for total benefits as well as days worked.

The regressions in table 2.3 indicate determinants of participation at the household level. We find that the number of men (especially men who are unemployed and underemployed (less than half year)), proxy means score (particularly subscores based on floor type and household size), and average education level were all correlated with participation in LIWP. As suggested by the community level variation summarized above, the inclusion of community level fixed effects dwarfs all other determinants in terms of explanatory power. However, the negative coefficients on proxy means and education and positive coefficients on excess labor show that more vulnerable households were indeed more likely to participate in the program.

Table 2.4 examines the determinants of days worked per household among households that participated. Correlation of days of participation by community is expected, since some projects lasted longer than others and more than a few are still in progress, so community level fixed effects are included in all specifications. The only explanatory variables significantly related to the number of days worked in LIWP are the total number of men in the household and proxy means score. None of the individual components of the proxy means score are individually significant, but to the extent that the proxy score as a whole predicts poverty, this result suggests that the targeting was effective. The positive association with number of men likely reflects the higher wages that men received for participating. However, neither variable explains a very large share of the variation in days worked.

2.2.2 Overall Targeting of Benefits

Figure 2.4 plots total LIWP income against proxy means score for all households in the sample and shows a non-parametric regression line. We see that in spite of a great deal of variation, the LIWP benefits were progressive overall, with more benefits going to households with lower scores (where lower scores are associated with greater probability of poverty). Both skilled and unskilled work are included in this figure.

2.2.3 LIWP Impact on Inequality

To directly measure the degree to which targeting was effective, we look at the change in inequality of wage income in the past month. Table 2.7 shows regression results for LIWP impact on the Gini index of monthly income. As discussed below, wage income in the past month is the more accurate measure of income available in the household survey. The variable of interest- "Active in past month" indicates communities with LIWP treatment and active programs during the month prior to the survey. Out of 44 treated communities, only 21 were active during the month prior to the survey. (We assume that whether or not the program was active during a given month is determined by community characteristics that are uncorrelated with its effect on inequality.)

Active programs reduced the average Gini index of monthly income by about 0.08, compared to a baseline value of about 0.5. which implies that benefits were well-enough targeted within each community to decrease inequality. As seen in the summary statistics in table 2.6, there was a general trend of worsening inequality between baseline and expost, which the LIWP treatment ameliorated. This observed trend of increasing inequality is consistent with conversations with SFD consultants involved in the program implementation, who noted that while the 2011 crisis affected everyone, the poor were hit harder.

We also find indications of heterogeneity of impact by type of program. Splitting the sample to include only communities with at least one road component, water component, or land component

[5]Each branch includes 4-10 communities

(most communities had multiple components so the groups overlap), we find that the strongest effect on reducing inequality was in communities with land projects, and the lowest in communities with road projects. This heterogeneity is consistent with observations by program staff that road and water projects required more skilled labor and physically demanding tasks, while land projects such as clearing harmful plants could employ more women and unskilled labor generally.

2.3 Satisfaction with Program Implementation

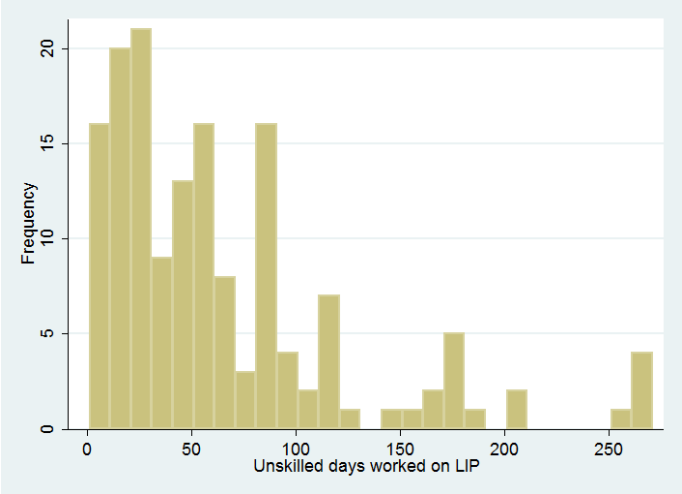
Satisfaction levels with the program implementation were generally high. Two of the areas where satisfaction was lower were regarding satisfaction with the timing and calculation of wage payments. Average responses are reported in tables 2.8 and 2.9.

Regarding direct questions about satisfaction with the program, the major reasons for non-participation were the non-availability of the head of household or other members. From the point of view of targeting, this is ideal, since it suggests that people with better alternative employment opted out of the program. A large number of households also mentioned lack of certainty about the project which is an implementation problem that should be addressed in the future. A small number of households mentioned favoritism (as a free response), suggesting that there were at least some instances of deliberate misallocation of program benefits, though the problem was not widespread. Responses to the survey question asked of non-participating households about their reason for non-participation are summarized in table 2.10. Respondents could choose one of a list of possible reasons, or give a free response.

Table 2.1: Participation Rate by Branch

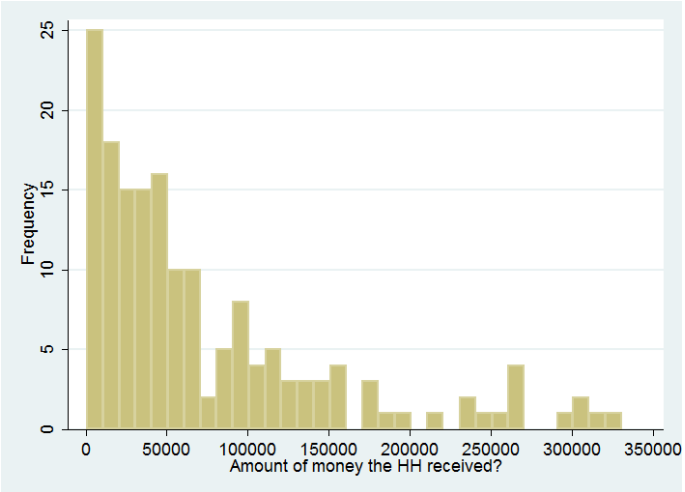
Branch	Participation Rate	Sample Size
Sana'a	87.5%	48
Aden	31.7%	60
Hodeidah	100%	24
Hajjah	69.0%	71
Mukallah	86.1%	72
Taiz	35.4%	48
Ibb	75.0%	24
Amran	59.3%	59
Dhamar	73.0%	48

Figure 2.1: Distribution of Unskilled Days Worked



Households in communities where the LIWP program was still ongoing at the date of survey are excluded.

Figure 2.2: Distribution of Money Received from LIWP



Households in communities where the LIWP program was still ongoing at the date of survey are excluded.

Figure 2.3: Distribution of implied wages for individuals with unskilled work in LIWP

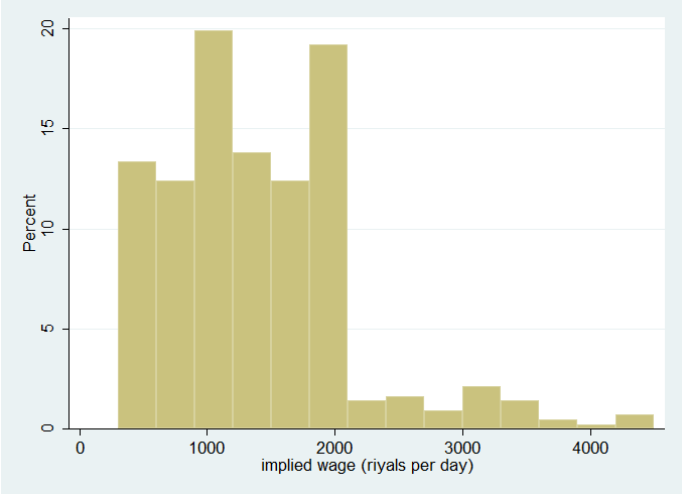


Figure 2.4: Distribution of Program Benefits by Poverty Score

Non-parametric regression of proxy-means test score on total program income

Table 2.2: Mean Project Benefits among Unskilled Workers

	days worked		total benefit		implied wage		observations
Sana'a	31.89	(1.294)	44253.9	(1.639)	3116.0	(1.018)	76
Aden	49.80	(1.147)	38732	(1.082)	1151.2	(0.458)	25
Al-Hodeidah	31.69	(0.817)	36303.4	(1.219)	1192.9	(0.588)	59
Hajjah	66.63	(0.861)	97467.3	(0.937)	1586.6	(0.450)	48
Mukallah	19.66	(0.752)	24916.8	(0.996)	1433.3	(0.568)	164
Taiz	38.72	(1.624)	109327.8	(3.182)	1810.7	(0.774)	18
Ibb	23.13	(0.943)	33746.8	(1.352)	1635.2	(0.641)	31
Amran	83.26	(1.176)	139450	(3.158)	1672.9	(1.169)	50
Dhamar	44.04	(1.139)	44631.3	(1.372)	1153.4	(0.411)	46

The figures in parentheses are coefficient of variation.

Table 2.3: Explaining Household Participation in LIWP

	(1)	(2)	(3)	(4)	(5)
Unemployed males (ages 15-60)	0.06***	0.06**	0.07***	0.08***	0.06***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Underemployed males (15-60)	-0.04	-0.06**	-0.06**	-0.06**	0.01
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Proxy means test score		-0.00***	-0.00***		
		(0.00)	(0.00)		
At least primary education			-0.11**	-0.10*	-0.02
			(0.05)	(0.05)	(0.04)
Subscore for floor type				-0.00**	-0.00
				(0.00)	(0.00)
Subscore for HH size				-0.00***	-0.00*
				(0.00)	(0.00)
Comm FE	No	No	No	No	Yes
Observations	526	493	493	493	493
R^2	0.020	0.047	0.056	0.063	0.306
Adjusted R^2	0.016	0.041	0.048	0.053	0.231

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The dependent variable for each column is a binary variable for household participation. The regressors variously included in the specifications (1)-(5) are number of unemployed males in the household, number of underemployed males (employment is either seasonal or temporary), proxy means test score (lower scores are associated with poverty), a dummy variable indicating whether anyone in the household has at least primary level education, and subscores from the proxy means test for floor type and household size. The proxy means test score is excluded when any of its component subscores are included to avoid collinearity.

Table 2.4: Determinants of Days of Work in LIWP

	(1)	(2)	(3)	(4)	(5)
Number of men in HH	10.35*				8.19
	(5.13)				(5.53)
Unemployed males (ages 15-60)		7.98			
		(4.75)			
Underemployed males (15-60)		16.85*			
		(8.84)			
Proxy means test score			-0.24**		-0.13
			(0.11)		(0.11)
At least primary education				15.51	
				(10.80)	
Comm FE	Yes	Yes	Yes	Yes	Yes
Observations	394	394	371	394	371
R^2	0.343	0.346	0.351	0.329	0.360
Adjusted R^2	0.260	0.262	0.264	0.245	0.272

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: Determinants of Implied LIWP wages

	(1) implied wage	(2) implied wage	(3) implied wage	(4) implied wage
dig (dummy)	425.777	289.141	261.243	
	(259.224)	(247.471)	(247.037)	
carry (dummy)	550.790**	450.557**	426.311**	
	(219.815)	(198.289)	(197.673)	
mix (dummy)	238.159	147.187	117.521	
	(261.863)	(249.453)	(259.490)	
pave (dummy)	1255.016*	1140.531	1114.596	
	(741.375)	(718.817)	(721.986)	
cut (dummy)	-264.502	-229.260	-240.006	
	(324.192)	(319.968)	(330.706)	
male		380.441**	359.580**	510.503**
		(151.995)	(163.130)	(216.914)
age		5.592	4.950	9.533
		(17.480)	(17.113)	(15.636)
age squared		0.010	0.015	-0.055
		(0.234)	(0.234)	(0.209)
experience			89.296	259.662
			(208.763)	(205.081)
community FE	Yes	Yes	Yes	Yes
Observations	455	455	455	455
R^2	0.340	0.352	0.352	0.322

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

LIWP implied wages are calculated as total wages divided by days worked per individual. Unskilled category of work only. The dummy variables refer to types of work performed in the project: digging sand or dirt; carrying stones, sand, or water; mixing sand and gravel; paving and construction; and cutting and shaping stone.

Table 2.6: Summary Statistics on in Wage Income Inequality Within Communities

	Gini Coefficient			
	All	Roads	Water	Land
Control and Inactive Baseline	0.479	0.505	0.479	0.487
	(0.128)	(0.136)	(0.131)	(0.126)
Control and Inactive Expost	0.562	0.597	0.561	0.575
	(0.153)	(0.129)	(0.159)	(0.141)
Active LIWP Baseline	0.496	0.503	0.493	0.506
	(0.111)	(0.109)	(0.114)	(0.116)
Active LIWP Expost	0.537	0.540	0.542	0.528
	(0.150)	(0.145)	(0.134)	(0.146)
Observations	160	110	134	118

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7: Change in Wage Income Inequality Within Communities

	Gini Coefficient			
	All	Roads	Water	Land
Active in past month	-0.079**	-0.047	-0.080*	-0.100**
	(0.037)	(0.047)	(0.045)	(0.045)
Expost	0.076***	0.064***	0.077***	0.067***
	(0.019)	(0.021)	(0.021)	(0.019)
Fixed effects	Comm	Comm	Comm	Comm
N	160	110	134	118

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8: Opinions on Project Implementation

Criteria	Average score out of 5
Wages received on time	**3.1**
Wages distributed in appropriate place	**4.1**
Calculation of wages was acceptable	**3.3**
Amount of money received was not less than agreed	**3.5**
Number of hours of work was appropriate	**3.9**
Work on the project did not interfere with other work	**4.1**
Timing of work hours during the day was good	**4.2**
Dealing with SFD staff was good	**3.9**
Dealing with community coordinators was good	**3.6**
Learned new skills from project	**3.4**
Project was implemented at a time when no other work	**4.2**
N	**749**

Responses by participants in LIWP to questions about their opinion of project implementation. 5 point scale, 0= disagree strongly 3=neutral 5= agree strongly

Table 2.9: Opinions on Payment Timing

How often delayed	Median days delayed	Percent of responses
Often	**15**	**23%**
Sometimes	**7**	**34%**
Few times	**5**	**16%**
No delay	**0**	**27%**
N		**749**

Responses by participants in LIWP to questions about the timing of payments

Table 2.10: Reasons that Households did not Participate in LIWP

	frequency	percent
Head absent during registration	3	2.4%
Head not available to work	43	29.3%
Members are not able to work	16	10.9%
Members do not have time (free response)	9	6.1%
Members not qualified to work (free response)	7	4.8%
Not sure about the project	44	29.9%
Respondent does not know about the project	3	2.4%
Household far from the project location	10	6.8%
Favoritism (free response)	12	8.2%
N	147	147

Chapter 3

LIWP Impact on Household Employment, Income, and Assets

3.1 LIWP Impact on Household Employment and Wage Income

The direct goal of the LIWP program was to provide increased employment. One potential concern is that employment in LIWP may substituted for other alternative employment. We find, however, that the LIWP program had a increased total days worked by approximately the amount of program employment. We also find that due to the timing of the crisis, LIWP wages ended up being more attractive than originally designed, so that the program also had the effect of increasing average wages and shifting the structure of the workforce away from work in the lowest paid sectors.

3.1.1 Days Worked

The summary statistics in table 3.1 show changes in household average days worked. A huge increase in days worked between baseline and control is mostly explained by increased reporting of self-employment. Some of the increase in self-employment may be due to the economic crisis as alternative work opportunities became scarcer, however, there was also a change in the survey design that increased reporting of self-employment.[1] We control for this effect when looking at days per month by excluding jobs in unpaid self-employment, and when looking at days worked per year, by excluding individuals whose primary job is unpaid "self-employment".[2]

Since many of the outcome variables- such as days worked and wage income- are based on the past month, it is relevant that in only 23 of 38 communities did any households participate in LIWP during the month before the expost survey. These will be referred to as "active projects." Among the active projects, participating households worked an average of 14 days during the past month in LIWP. The variable active in past month is equal to one in LIWP communities at expost where the project was active during the month preceding the expost survey. We expect to see program impacts on days worked in past month concentrated in communities that had LIWP programs active in the past month.[3] As seen in the regression results presented in table 3.2, there was no significant impact of LIWP treatment on days worked in the past month, possibly because the number of communities with active projects in the past month was too small. We also add

[1] At the beginning of the module on employment, in the question asking for types of work that had been performed in the past year, work for the family was added as an explicit choice, which seems to have increased reporting of non-paid work in agriculture as self-employment.

[2] We are unable to precisely control for the change in reporting self-employment in looking at days worked per year, because days of work are measured at the individual level rather than the level of job.

[3] The data used on days worked here is from the module on employment.

"Active in the past month" as an explanatory variable, to focus on the effect in communities where we expect LIWP treatment to be relevant. However, project timing is not necessarily exogenous-LIWP projects were scheduled to provide employment in the season with the least agricultural work so while the LIWP treatment effect on employment should be strongest in communities where there is active work, there will also be a negative selection bias since the communities where LIWP is active will tend to be those with the least employment during this month.

For days worked per year, however, we do see coefficients of approximately the expected magnitude. According to the household survey, the average number of days worked in LIWP was 51 days. This is consistent with administrative data showing a program wide average of 64 days per household, since some projects were still ongoing at the time of the expost survey. We find a LIWP impact on days worked per year of approximately 57, suggesting that LIWP employment did not displace other employment, although the impact is not significant. We can also see further evidence of high variance in program benefits. Dividing the sample by average days worked per year per adult at baseline, it is also notable that the increase in days worked per year was concentrated among households that had greater labor supply at baseline. In the subsample of households with low employment at baseline the estimated impact of LIWP on employment is very low and statistically insignificant (20 days per year), while among households with high employment at baseline, LIWP had a much higher, and statistically significant, positive impact of 75 days per year. Regression results are reported in table 3.3 This difference is not driven by gender composition of the household as repeating the analysis restricting to only days worked by men gives a similar pattern (not shown). This may indicate that these households were less able to benefit from the program due to age or disability.[4]

Visually, we can compare the distributions of days worked last year (excluding unpaid self-employment) between treatment and control at both baseline and expost using quantile by quantile plots. See figures 3.1. Each point represents ith largest observations in each distribution. In control communities, the quantile by quantile plot closely follows the 45 degree line, while in treatment communities, the plot also follows the 45 degree line, with a jump above around 500 days of work showing that the LIWP programs increased employment in the middle of the distribution.

3.1.2 Wage Income

By providing additional days of work, we expect that LIWP should also have had a positive impact in protecting wage income from the negative effect of the crisis. We find positive program impacts of approximately 5000 riyals per month or $23 in active projects, and estimate a program impact for the past year of approximately $500. While these are reasonable magnitudes, the impact is not statistically significant.

Since monthly income fell from $166 (35,313 riyal) at baseline to $112 (23,802 riyal) at expost in control communities as a result of the crisis, the LIWP impact of $23 per month is economically meaningful, although due to the small sample size, the coefficient is not statistically significant. Table 3.4 shows summary statistics on wage income in treatment and control communities in baseline and expost, and regression results are reported in table 3.6.

As noted above, program timing is important, since we do not expect to see an effect on wage income in the past month in communities where LIWP was not active. When adding the variable "active in the past month," we find that the LIWP impact increases from an average of approximately 5000 riyal for all projects to 6000 riyal for active projects (taking the sum of the coefficients on LIWP and active). Due to the small sample size, however, the standard errors are relatively large, so the LIWP impact is not statistically significant.

Since we have data on days of work during the past year, we also attempt to estimate the LIWP impact on wage income for the past year. Because wage data is only available for jobs in

[4]Data on days worked per year from the baseline is adjusted to enforce a maximum employment of 365 days per year per person. Employment per person of greater than 365 days per year is an artifact of the survey design in the baseline survey which counted days per year for each type of employment separately, double counting days in which the individual worked at multiple jobs. In the expost survey, enumerators were instructed to record no more than 365 days of employment per person.

the past month, we assume a similar distribution of days worked in different jobs for the past year. This extrapolation certainly introduces error since many jobs, including LIWP itself, are seasonal. However, as a rough approximation, we find a LIWP impact of approximately 100,000 riyals per year or approximately $500, which is very close to the program goal of providing an average of $550 in program benefits per household.

3.1.3 Demographic Composition of Workforce

One of the program goals was particularly to target employment by women. We find that LIWP increased the probability of being employed (outside of self-employment in agriculture) by approximately 5 percentage points for both men and 3 percentage points for women. The impact is only marginally statistically significant, but the magnitude, especially for women, is meaningful. Women's employment started from a baseline of about 3% in treatment communities compared to approximately 65% for men (see summary statistics table 3.7). [5] In contrast to men's employment, women's employment increased significantly between baseline and expost. Since culturally, it was not expected in this context for women to work outside the home, the increase in women's employment may be a case of economic necessity during the crisis over-riding cultural norms. The LIWP impact complemented the increase in women's employment during the crisis. Table 3.8 reports regression results. Since there may have been some internal migration of household members which would change the composition of the potential labor force, we also control for gender, age, and literacy. Including these controls slightly decreases the magnitude of the effect on employment.

We can also look visually at the change in employment by age group. The histograms in figure 3.2 represent the share of total days worked contributed by different ages ranges for males and females in by treatment and expost. (Light bars in front represent women while dark bars represent men.[6]) The impact of both the crisis and LIWP among women was strongest among younger women (under 20), whose employment rate was actually higher than that of young men. Among men, the age distribution of employment was roughly similar in treatment and control communities.

3.1.4 LIWP Impact on Enrollment

We would expect the positive income effect of LIWP employment to allow families to keep children in school. Due to the way that work was organized in groups paid by piece-rate it was also possible for children to contribute to work in LIWP, however, so there might have been incentive to reduce enrollment for children and young adults to allow them to work. It is reassuring therefore, that we indeed find a positive impact of LIWP on enrollment.

Summary statistics on enrollment are presented in table 3.9 and regression results in table 3.10.[7] We see a positive trend in enrollment for boys between baseline and expost, which likely points to the success of other programs during this time period in encouraging education. Looking at the LIWP impact on enrollment rates, we find a positive impact on enrollment for boys under age 15 of about 8 percentage points. For males older than age 15, there is no impact on enrollment, which makes sense as baseline rates of enrollment for this age group are low, and young men in this age range could employed directly in the LIWP program. Enrollment rates are much higher

[5] We also note that baseline women's employment was significantly lower in treatment communities than in control communities. In the original sample, both treatment and control communities had approximately 8% employment for women over 15, and the much lower level of employment in treatment communities is the result of attrition at the community level between baseline and expost. There would not seem to be any reason that a higher level of female employment would be related to the reasons for not resurveying communities as described in chapter, so we assume that this difference occurred randomly.

[6] To control for the change in survey format, individuals whose primary employment was unpaid are excluded.

[7] It is noticeable in the table of summary statistics that the distribution of observations is not evenly split between boys and girls. Ages are usually estimated in Yemen and we have seen in other data sets when comparing number of boys to number of girls in given age groups, girls appear to be either under-reported or reported with older ages relative to their actual age than boys.

for boys than for girls and there does not appear to be any impact on girls enrollment, while there is a slight negative (but not statistically significant) impact on enrollment for young women.

3.1.5 Sectoral Change in Work

The LIWP program was designed to attract otherwise unemployed labor by setting wages below market rates. However, after the economic crisis, the average level of wages fell. There was both a decline in wages within sectors and a shift in employment composition from higher wage jobs in construction to lower wage jobs in private agriculture. As a result of this change, LIWP employment was more attractive than originally planned, so participation was broader than anticipated. The relatively high wages also mean that the benefit of the program is not fully captured by the increase in the number of days worked, since LIWP work sometimes substituted for lower wage work.

Table 3.11 shows the number of jobs held before and after the LIWP intervention. Based on the trends in number of jobs, LIWP employment seemed to substitute for unskilled work in agriculture and self-employed skilled work. The number of jobs in both these sectors increased more in control than in treatment communities. We also see a general trend due to the crisis of a decline in construction and non-agricultural private sector jobs due to the impact of the 2011 crisis.

There is a notable increase in unskilled work in agriculture and self-employment in control communities, which balances the decline in construction and other private sector jobs lost as a result of the economic crisis. In treatment communities, on the other hand, the same loss in non-agricultural jobs is seen, but the increase in unskilled agricultural employment is smaller. Self-employed skilled work also showed an decrease in both treatment and control communities, with the increase being slightly greater in treatment communities.

On the other hand, in unskilled self-employed work, there was a substantial decline in control communities, but no change (at a lower level) in treatment communities. One possible explanation is that LIWP employment supported unskilled self-employment by protecting assets from decapitalization. For example, a common type of unskilled self-employed is transport services, and as will be shown below, LIWP had a positive effect on ownership of taxis and buses.

Looking at the average wages by type of work (Table 3.12), we can see that both self-employed skilled jobs and unskilled jobs in agriculture have lower average wages than LIWP, confirming that LIWP employment may have substituted for work in these sectors. Whereas unskilled wages in construction are comparable to SFD wages, unskilled wages in private agriculture are about 50% lower. We also note that SFD unskilled wages on average were much higher than unskilled wages in agriculture (1317 riyals/day compared to 963 riyals/day for agriculture).

We also test to see how wage levels by sector were affected by LIWP and the economic crisis. Table 3.14 reports regression results. We find a strong program impact on wages in construction and other skilled work. While the impact on skilled wages may be a general equilibrium effect of LIWP, in unskilled work it is more likely that LIWP reduced employment in jobs with wages lower than the LIWP wage, raising the observed average wage in the survey. Nominal wages remained constant or declined in most sectors as a result of the economic crisis. It should also be noted that wages declined in real terms due to approximately 20% inflation during the crisis period.

3.1.6 Program Effect on Average Wages and Skill Level

As indicated above, LIWP ended up being a relatively well paid job for participants due to the decline in alternative employment. Correspondingly, we find that LIWP significantly increased average daily wages in treatment communities.

We find a LIWP impact on average wages of about 300 riyals per day. (See tables 3.15 and 3.16) Average daily wages were calculated for all working individuals based days per month spent in each form of employment. [8] The impact is significant before separating between current programs

[8] Average daily wages were calculated for each individual based on proportion of days worked in each position in the last month. Only days worked in paid employment are counted.

and previous programs. When an explanatory variable "Active in past month" is added, about half of the increase in average daily wages is captured by this variable. The remainder may be attributed to persistent effects of the program, such as the protection of assets for self-employment as mentioned above.

Between baseline and expost, average wages fell by about 170 riyals per day. The increase in wages caused by LIWP, therefore, not only compensated for the drop in wage levels caused by the crisis, but also increased the average wage level compared to baseline.
[9]

We also see that a major effect of the crisis on employment was to decrease the average skill level. This is related both to the loss of skilled construction jobs, and to the increased employment by women. Since the LIWP program primarily created unskilled jobs, it is not surprising to see that the program had a significant negative impact on skill level. The increase in average wages due to the LIWP program occurred in spite of this decline in the average skill level. Interestingly, the LIWP impact on average skill appears to persist after the program ends in treatment villages, as shown by the fact that the change is not picked up by the variable "active in past month." [10]

3.2 LIWP Impact on Other Income Sources

While the above discussion has shown that LIWP increased household wage income, what really matters is overall income. It is possible that part of the LIWP benefits went towards reducing dependence on transfers, rather than directly increasing household income.

As a result of the economic crisis and declines in wage income, other income sources became more important for households in Yemen. The summary statistics in table 3.17 show that the share of households receiving any income from employment fell slightly. Meanwhile, there was a dramatic increase in the share of households receiving income from agricultural production, almost doubling from a base of about 40% to about 70% after the crisis. There were similar increases in the share of households receiving any income from charity or transfers from the Social Welfare Fund (almost doubling in control communities from 16% to 40%), receiving income from rental of private property (2% to 4%), and receiving remittances from both within Yemen (5% to 10%) and from abroad (9% to 12% in control communities). These various income sources represent coping strategies for dealing with the economic crisis and the decline in wage income, and reflect the increase in coverage by the Social Welfare Fund.

To the extent that LIWP cushioned wage income, we should not be surprised to see that there is less reliance on other sources of income in treated communities after the crisis. We find a LIWP impact of about 10 percentage points in the share of households receiving income from charity or Social Welfare Fund. This impact is marginally statistically significant. The share of households receiving remittances from abroad also decreased by about 3 percentage points from a baseline of 9%, although the change was not significant. We have no data on the magnitude of these transfers, but they likely partially compensated for the lack of LIWP employment in control communities. Regression results are reported in table 3.18.

3.3 Household Asset Ownership and Indebtedness

Another method of coping with the negative income shock caused by the crisisis decapitalization of assets. The question we ask here is whether the program could partially protect households

[9] This approach ignores seasonal variation in time allocation between different types of employment, but is the best estimate available given the survey data. There are also unobserved wages associated generally with self-employment (including both missing data and employment where the worker did not receive a regular wage).

[10] A potential concern with the observed shift into self-employment, whether agricultural or non-agricultural, is that daily wages are likely to be unknown or reported even if the activity generates income over the course of the year, so it will be misleading to look for a program effect in terms of total wages. Since we exclude unpaid self-employment in agriculture from our estimates, and assuming that self-employment in agriculture is less remunerative than wage labor would mean our estimate of the wage effect underestimates the true impact (details in Appendix III)

from selling off assets and/or increase investment in durable goods and animal assets. We find that LIWP had a significant impact on decreasing indebtedness, and also on ownership of motor vehicles.

3.3.1 Durable Goods

We find that the LIWP program reduced sales of durable goods and increased the probability of acquiring new durable goods, both of which are positive results for households in the long term.

The survey asked whether households owned any of a list of twelve common durable goods. By comparing whether each household owned the good at baseline and expost, we calculated the number of instances in which a household had owned the good at baseline but lost it by the time of the expost survey, and the number of instances in which a household had not owned the good at baseline, but had acquired the good by the time of the expost survey.

In spite of the crisis, total ownership increased for mobile telephones, gas and kerosene canisters, gas ovens, radios, kerosene lamps, and generators. (The increase in generator and lamp ownership may have been caused by the crisis for households on the electric grid, as electricity was cut frequently during this period). With the exception of motorcycles, more households in treatment communities acquired durable goods, and fewer households lost them. (See table 3.21) Most notably, in control communities, more households sold than bought taxis and/or minibuses, while in treatment communities, the opposite occurred.

Combining household assets into a single index based on estimated values shows a strong program effect of approximately 31 thousand riyals ($146) on durable good ownership. Regression results are reported in table 3.22. This index does not include changes in real-estate ownership as there was not way to estimate the value of this asset, and the effect is highly driven by ownership of taxis/ buses.

Ownership of taxis and buses is significantly affected by LIWP (see above). Since their values dwarfs that of the other goods listed in the survey, our estimated LIWP impact on asset ownership is highly dependent on the 65 observations in which taxi/bus ownership changes. Therefore, we analyze the sensitivity of the results to the value used as the price of taxi/ bus. The program effect is just significant at 10% level if taxi/buses are valued at 750,000 riyals ($3538) on average, which we believe is a reasonable estimate. Omitting taxi/ bus ownership from the calculation of the value of durable goods results in no estimated impact of LIWP, while varying the average value between 500 thousand ($2358) or 1 million riyal ($4716) changes the estimated magnitude of the LIWP impact from 21 thousand ($100) to 43 thousand riyal ($203) on average.

3.3.2 Animal assets

The survey also asked about household livestock assets. In general, we find that LIWP did not have a significant effect on causing households to increase their investment in livestock. Rather, the livestock assets in general increased and in some categories, assets increased more in control communities than in treatment communities. We interpret this positive effect of the crisis as part of a retreat to agricultural production caused by the loss of alternative employment, from which treatment communities were partially sheltered by LIWP.

Animal assets were categorized as either owned individually by the households, or animals from which the households benefits but does not own (for example, arrangements in which a member of the household feeds and cares for an animal in exchange for a share of the benefits.) Table 3.23 presents summary statistics on livestock asset ownership. The most important livestock in the sample are sheep and goats, which are mostly owned individually. Sheep and goat ownership increased from about 5 to 6 per household in both treatment and control communities. While this impact is not statistically significant, it is worth noting that the increase in sheep and goat ownership would certainly have been even larger if the expost survey had been conducted prior to, rather than just after, the Eid al-Adha holiday. Generally we do not see any impact of the LIWP program on average ownership of animal assets. (See table 6.22)

Instead of looking only at the overall increase in animal asset ownership between baseline and expost, we can break the change into an extensive component (change in the share of households owning any animals) and an intensive component (change in the number of animals owned among households owning any animals at baseline.) We find that most of the increase in animal ownership occurred on the extensive margin. The share of households benefiting from sheep and goats, for example, increased by 10 percentage points between baseline and expost, from a baseline value of about 0.5. This gain occurred solely in control communities, however. We actually find a negative impact of LIWP of 8 percentage points on the share of households owning sheep or goats. Summary statistics on the share of households with animals are presented in table 3.25. Within households that owned any animals, however, the trend between baseline and expost was consistently negative. Regressions results are reported in table 3.26 for the extensive margin, and table 3.27 for the intensive margin.

3.3.3 Household Debt and Sale of Assets

In addition to survey modules on ownership of durable goods, we also have direct questions about sales of assets and debt. Like decapitalization of assets, consumption smoothing via borrowing was a coping mechanism for responding to the economic crisis, and households in treatment communities ended with substantially less debt than households in control communities.

Summary statistics on the share of households that sold or pawned any assets are reported in tables 3.28 and 3.29. The share of households that sold off assets increased significantly between baseline and expost, from 25% to 40% in control communities, showing that this was a common strategy for coping with the economic crisis. We have a small and statistically insignificant LIWP impact on the share of household selling or pawing assets. (See table 3.31. Among households that sold or pawned any goods, the share that sold or pawned each type of good (land, gold, automobiles, or animals) is also reported. Respondents could indicate that they sold or pawned goods in more than one category. We notice that households in control communities were more likely to sell or pawn cars in expost, while households in treatment communities were more likely to sell or pawn animals. This is consistent with the changes in durable goods and livestock assets seen above.

The vast majority (approximately 80%) of sampled households were indebted, and the rate of indebtedness increased between baseline and expost by about 3 percentage points. Table 3.30 reports summary statistics on the share of households in debt. Among households in debt, the survey included a question asking about who the household was in debt to. Respondents could list multiple creditors. Of the listed creditor types, by far the most common was for households to be indebted to store owners. Theoretically, the effect of the program could have been either positive or negative on total debt, shopkeepers may have been more willing to extend credit to program participants, while conversely, households with extra income from the project may have had less need to buy goods on credit. We find a positive effect of 6 percentage points on the total amount of debt owed by the household in the last 12 months, however neither the program impact nor the change over time is statistically significant. Regression results are reported in table 3.31.

The greatest changed between baseline and expost is not in the probability of indebtedness, but in the amount of debt owned. Outstanding debt (defined as the difference between total amount borrowed and paid off during the past 12 months) increased by 29 thousand riyal or $136 as a result of the crisis. (There is probably some seasonality in the time trend as well).

We find large and statistically significant negative program effect on the amount of outstanding household debt of about 26 thousand riyal or $123, relative to expost outstanding debt in control communities of about 43 thousand riyal or $202. Credit in Yemen is generally interest-free, however, there is a cost to households of holding a high amount of debt since storekeepers will limit the total amount of credit extended, and, for some types of debt there is a possibility of imprisonment if the debt is not paid in time. Summary statistics are presented in table 3.19 while regression results are presented in table 6.20.

Figure 3.1: Quantile-Quantile Plots of Days Employed per Year

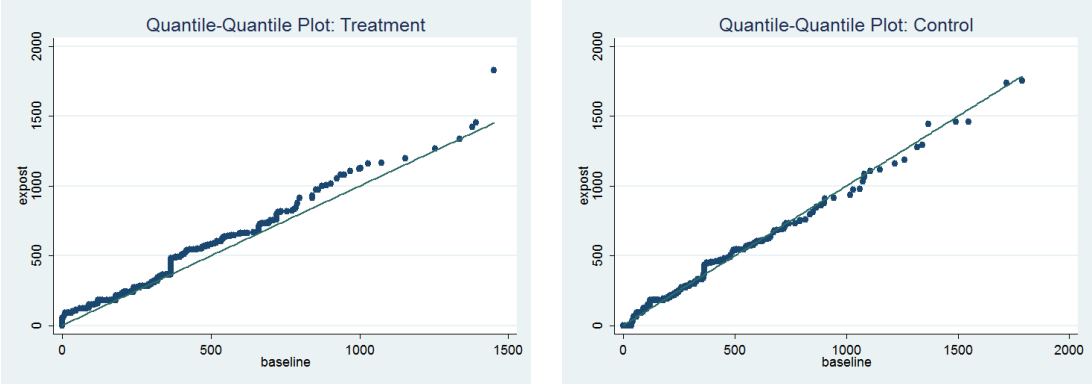
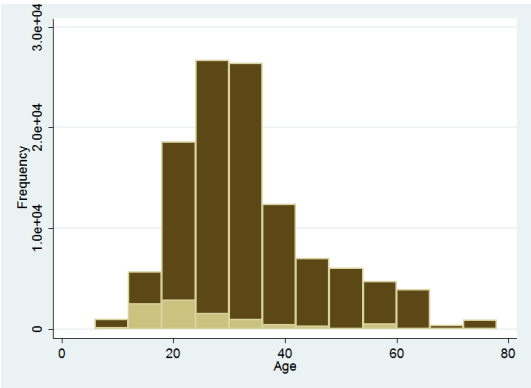
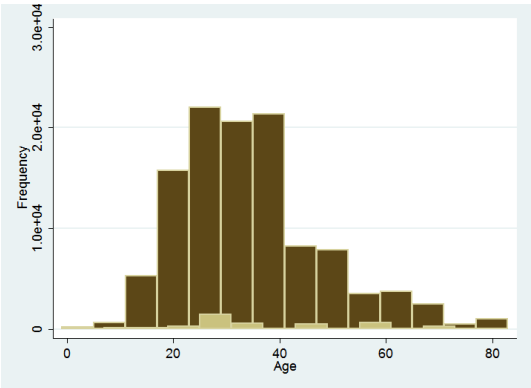


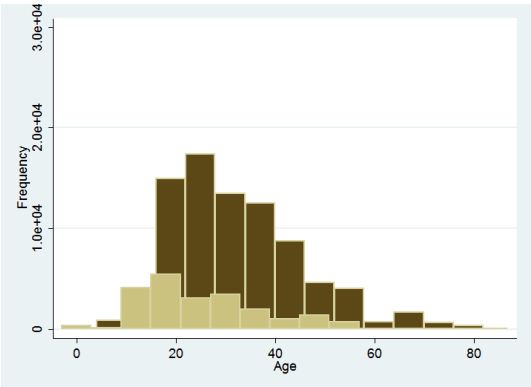
Figure 3.2: Age distribution of workforce weighted by days worked: males vs. females



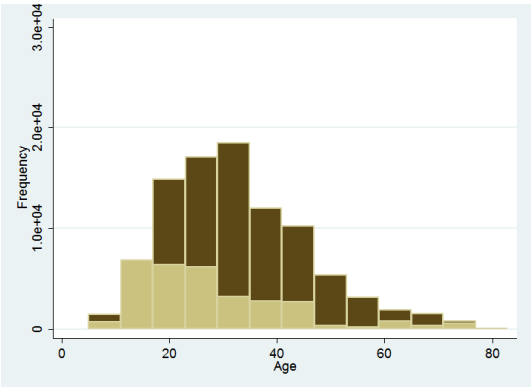
Control Baseline



Treatment Baseline



Control Expost



Treatment Expost

Table 3.1: Summary Statistics for Household Total Days Worked

	Days/month	Days/year	Days/month	Days/year	Corrected days/year
Control Baseline	38.84	435.0	32.12	363.5	339.0
	(39.00)	(403.0)	(33.06)	(334.4)	(304.6)
Control Expost	60.98	722.7	23.42	313.7	312.5
	(60.64)	(550.6)	(26.32)	(267.8)	(266.0)
Treatment Baseline	35.52	411.3	29.94	353.3	321.1
	(29.97)	(354.7)	(26.45)	(323.1)	(251.6)
Treatment Expost	58.03	737.5	22.77	350.5	346.8
	(49.52)	(531.4)	(19.57)	(274.1)	(266.2)
Observations	1708	1708	1708	1708	1708

mean coefficients; sd in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The last three columns exclude days in self-employed work

Table 3.2: LIWP Impact on Household Total Days Worked Last Month

	Days/month	Days/month	Days/month	Days/month
LIWP Program	1.368	1.368	1.097	1.097
	(3.733)	(3.733)	(5.053)	(5.053)
Expost	-8.199***	-8.199***	-8.185***	-8.185***
	(2.558)	(2.558)	(2.583)	(2.583)
Active in past month			0.469	0.469
			(4.243)	(4.243)
Fixed effects	Comm	HH	Comm	HH
N	1908	1908	1908	1908

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions exclude days in self-employed work. Original treatment assignment is used as an instrument for "LIWP Program."

Table 3.3: LIWP Impact on Household Total Days Worked Last Year

	Days/year	Days/year	Days/year	Days/year
LIWP Program	57.616	57.616	19.665	74.652*
	(40.540)	(40.540)	(55.091)	(41.706)
Expost	-23.049	-23.049	159.422***	-122.208***
	(24.858)	(24.858)	(34.344)	(25.064)
Fixed effects	HH	HH	HH	HH
N	1908	1908	726	1254

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions exclude days worked in self-employed agriculture. Third column includes only households with baseline days worked per adult greater than 60; fourth column includes only households with baseline days per adult less than or equal to 60. Original treatment assignment is used as an instrument for "LIWP Program."

Table 3.4: Summary Statistics on Household Wage Income

	wage income last month	est. wage income last year
Control Baseline	35313.8	405565.9
	(63345.4)	(671029.8)
Control Expost	23801.9	317772.9
	(35712.9)	(380657.8)
Treatment Baseline	35293.5	434888.5
	(44898.8)	(563599.4)
Treatment Expost	28794.4	452421.1
	(48072.1)	(545742.4)
Observations	1708	1708

mean coefficients; sd in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Wage income for past year is estimated based on days worked in past year and assuming the same wages per day as during past month.

Table 3.5: Household Wage Income

	Wage Income					
	month	month	month	month	year	year
LIWP Program	4921.7	4921.7	3684.0	3684.0	112575.5	74818.4
	(5546.1)	(5546.1)	(7566.2)	(7566.2)	(72401.9)	(65760.2)
Expost	-10969.2***	-10969.2***	-10901.7***	-10901.7***	-84157.3**	-99306.2***
	(3563.5)	(3563.5)	(3602.8)	(3602.8)	(41625.2)	(38362.3)
Active in past month			2148.4	2148.4		
			(6674.9)	(6674.9)		
Fixed effects	Comm	HH	Comm	HH	Comm	HH
N	1908	1908	1908	1908	1908	1908

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.6: Regression Test for Program Effect on Household Total Wage Income

Table 3.7: Summary Statistics on the Employment Rate

	All	Male	Female
Control Baseline	0.356	0.643	0.0925
Control Expost	0.360	0.648	0.116
Treatment Baseline	0.332	0.657	0.0342
Treatment Expost	0.390	0.719	0.114
Observations	6314	2956	3358

Unpaid employment is excluded from the employment rate to control for the change between baseline and expost.

Table 3.8: LIWP Impact on Employment Rate

	All	Male	Female	All	Male	Female
LIWP Program	0.044	0.055	0.034	0.038	0.054	0.034
	(0.029)	(0.044)	(0.033)	(0.029)	(0.042)	(0.034)
Expost	0.018	0.019	0.044*	0.033	0.023	0.043
	(0.020)	(0.027)	(0.026)	(0.021)	(0.025)	(0.027)
Age				0.027***	0.049***	0.006***
				(0.001)	(0.002)	(0.002)
Age sqrd				-0.000***	-0.001***	-0.000***
				(0.000)	(0.000)	(0.000)
Male				0.566***		
				(0.015)		
Literate				0.044***	0.052***	0.006
				(0.015)	(0.019)	(0.020)
Fixed effects	Comm	Comm	Comm	Comm	Comm	Comm
N	7004	3302	3702	6903	3261	3751

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent variable is employment rate for individuals ages 15-60. Only paid employment is included.

Table 3.9: Summary Statistics on Schooling by Age and Gender

	male age 5-14	male age 15-25	femal age 5-14	female age 15-25
Control Baseline	0.711	0.267	0.448	0.0350
Control Expost	0.753	0.273	0.444	0.0557
Treatment Baseline	0.631	0.294	0.463	0.0779
Treatment Expost	0.743	0.268	0.448	0.0703
Observations	2014	1077	1808	1282

Table 3.10: LIWP Impact on School Enrollment

	male age 5-14	male age 15-25	femal age 5-14	female age 15-25
LIWP Program	0.084*	-0.034	0.009	-0.021
	(0.047)	(0.065)	(0.044)	(0.023)
Expost	0.025	0.006	-0.003	0.019
	(0.025)	(0.045)	(0.030)	(0.014)
Observations	2265	1215	2065	1444

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Proportion of Children Enrolled in School by Age and Gender

Table 3.11: Summary Statistics on Employment by Category

	Control Baseline	Treat. Baseline	Control Expost	Treat. Expost
Governmental	128	96	81	91
Private agriculture, skilled	17	20	25	5
Private agriculture, unskilled	88	69	146	112
Private construction, skilled	42	58	17	34
Private construction, unskilled	57	83	32	32
Private other, skilled	96	99	44	42
Private other, unskilled	171	156	85	133
Self-employed, skilled	193	159	113	96
Self-employed, unskilled	342	188	395	376
SFD, skilled	0	5	1	86
SFD, unskilled	0	0	5	661

Average wage is calculated across all communities and includes in-kind payment.

Table 3.12: Summary Statistics on Wages by Category

	Control Baseline	Treat. Baseline	Control Expost	Treat. Expost
Governmental	1122.0	1115.6	1402.1	1428.7
Private agriculture, skilled	1085.3	857.8	691.7	941.5
Private agriculture, unskilled	889.9	833.9	950.0	985.8
Private construction, skilled	1726.2	1790.2	1872.3	2589.0
Private construction, unskilled	1370.2	1533.5	1340.6	1782.8
Private other, skilled	1344.7	1423.5	1156.1	1697
Private other, unskilled	1290.4	1085.2	1181.8	1313.3
Self-employed, skilled	1154.0	1074.2	1056.1	1405.7
Self-employed, unskilled	916.3	972.1	562.9	723.0
SFD, skilled		3333.3	2163	2501.2
SFD, unskilled			1259.2	1313.7

Average wages include in-kind payment.

Table 3.13: LIWP Impact on Wages Level by Sector

	treatexpost	expost
Governmental	187.8	134.0
	(249.4)	(212.6)
Private agriculture, unskilled	-35.20	210.7
	(287.5)	(143.6)
Private construction, skilled	562.3	473.7
	(639.9)	(480.3)
Private construction, unskilled	146.6	-104.5
	(1039.7)	(387.4)
Private other, skilled	190.1	-59.67
	(490.5)	(378.1)
Private other, unskilled	837.2	-598.6
	(780.1)	(681.3)
Self-employed, unskilled	-221.6	370.1
	(379.2)	(288.7)
Self-employed, skilled	-61.24	-304.4***
	(185.8)	(101.9)
Fixed effects	Comm	Comm
N		

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.14: Includes community fixed effects.

Table 3.15: Summary Statistics on Average Wages and Skill Level

	Avg. Wage	Avg. Skill (all)	Avg. Skill (paid only)
Control Baseline	1099.9	0.317	0.376
	(1475.8)	(0.460)	(0.477)
Control Expost	978.1	0.126	0.313
	(712.9)	(0.326)	(0.452)
Treatment Baseline	1212.2	0.402	0.450
	(920.4)	(0.485)	(0.490)
Treatment Expost	1292.8	0.0984	0.277
	(1463.4)	(0.292)	(0.437)
Observations	2238	4959	2238

mean coefficients; sd in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.16: LIWP Impact on Average Wages and Skill Level

	Avg. Wage	Avg. Wage	Avg. Wage	Avg. Wage	Avg. Wage	Avg. Skill
LIWP Program	290.796**	307.243**	189.538	175.448		-0.222**
	(130.010)	(155.722)	(164.464)	(177.213)		(0.096)
Expost	-183.180*	-164.732	-176.662*	-156.145		-0.164***
	(95.080)	(108.681)	(95.414)	(108.698)		(0.049)
Active in past month			172.486	225.930		0.095
			(133.838)	(157.169)		(0.086)
Avg. Skill					277.359***	
					(82.139)	
Fixed effects	Comm	HH	Comm	HH	Comm	Comm
N	2238	2038	2238	2038	2238	4959

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Average daily wages are calculated per worker based on days worked at different jobs in the past month. For the fourth column, unit of observation is the job.

Table 3.19: Summary Statistics on Household Debt and Value of Assets Sold

	Total Debt	Store Debt	Paid Off	Outstanding Debt	Assets Sold
Control Baseline	98.23	54.08	15.81	17.14	20.83
	(162.7)	(115.5)	(64.98)	(55.87)	(68.56)
Control Expost	155.2	86.71	21.80	43.24	33.00
	(965.5)	(245.7)	(52.86)	(187.3)	(154.1)
Treatment Baseline	85.72	52.30	10.66	25.33	16.11
	(215.8)	(178.6)	(28.67)	(91.62)	(98.54)
Treatment Expost	94.64	61.20	27.41	31.29	21.35
	(146.4)	(98.78)	(70.00)	(88.07)	(54.09)
Observations	1908	1908	1908	1908	1908

Average Level of Household Debt and Value of Assets Sold. Values are measured in thousands of riyal. (1000 riyal= $4.70)

Table 3.20: LIWP Impact on Household Debt and Value of Assets Sold

	Total Debt	Paid Off	Outstanding Debt	Assets Sold
LIWP Program	-70.88	10.20*	-26.57**	-1.95
	(57.27)	(6.11)	(12.74)	(11.08)
Expost	68.76	6.28*	29.42***	9.60
	(50.35)	(3.57)	(9.46)	(9.11)
Fixed effects	Comm	Comm	Comm	Comm
N	1908	1908	1908	1908

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regression Test for Program Effect on Household Debt and Value of Assets Sold. Values are measured in thousands of riyal. (1000 riyal= $4.70)

Table 3.17: Summary Statistics on Income Sources

	Employment	Ag. Production	Rent	Charity	Remit. Yemen	Remit. Abroad
Control Baseline	0.763	0.470	0.0209	0.161	0.0418	0.0851
Control Expost	0.609	0.709	0.0397	0.406	0.105	0.119
Treatment Baseline	0.776	0.399	0.0139	0.177	0.0399	0.0867
Treatment Expost	0.625	0.688	0.0361	0.344	0.0913	0.0856
Observations	2251	2251	2251	2251	2251	2251

mean coefficients; t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Share of households who reported getting any income from the source. Households could select multiple income sources.

Table 3.18: LIWP Impact on Income Sources

	Employment	Ag. Production	Rent	Charity	Remit. Yemen	Remit. Abroad
LIWP Program	-0.002	-0.005	0.005	-0.101	0.006	-0.033
	(0.061)	(0.073)	(0.020)	(0.070)	(0.040)	(0.043)
Expost	-0.173***	0.294***	0.026*	0.262***	0.048*	0.031
	(0.039)	(0.050)	(0.014)	(0.044)	(0.026)	(0.030)
Fixed effects	HH	HH	HH	HH	HH	HH
N	1908	1908	1908	1908	1908	1908

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regression test for program effect on share of households receiving income from different sources.

Table 3.21: Summary Statistics on Changes in Durable Good Ownership

	Lost: Control	Lost: Treatment	Gained: Control	Gained: Treatment
Real Estate	2	1	5	3
Taxi or Bus	24	9	15	18
Motorcycle	4	5	11	3
Sewing Machine	21	20	19	18
Fixed Telephone	7	13	7	13
Mobile Telephone	51	39	87	85
Gas Canister	29	25	64	73
Kerosene Cansister	37	33	69	80
Gas Oven	30	28	73	89
Radio	66	65	99	113
Gas Lamp	56	43	42	63
Kerosene Lamp	46	44	121	100
Generator	24	25	30	40

Number of households in treatment and control communities that lost or gained the item between treatment and control. Only panel households are used. For easier presentation of these small numbers of households, the total number of households rather than the shares of households. There are 461 households in control communities and 493 households in treatment communities, so the denominator would be slightly different when calculating the share of households owning each type of goods.

Table 3.22: LIWP Impact on Value of Household Durables Owned

	Taxi/ bus =750 b/se	Excluding taxi/bus b/se	Taxi/ bus =500 b/se	Taxi/bus =1000 b/se
LIWP Program	31.82*	-2.01	20.55	43.10*
	(19.31)	(5.10)	(13.82)	(24.92)
Expost	-4.13	12.57***	1.44	-9.70
	(14.15)	(3.14)	(9.99)	(18.37)
Fixed effects	Comm	Comm	Comm	Comm
N	1908	1908	1908	1908

Value measured in thousands of riyals

Table 3.23: Summary Statistics on Livestock Assets

	Cattle		Donkeys		Sheep or Goats		Chickens	Beehives
	own	share	own	share	own	share	own	own
Control Baseline	0.451	0.145	0.477	0.0152	5.215	0.649	1.157	1.002
	(0.732)	(0.478)	(0.638)	(0.122)	(10.50)	(3.425)	(2.178)	(5.730)
Control Expost	0.555	0.104	0.487	0.00868	6.039	0.716	1.748	1.245
	(0.882)	(0.364)	(0.668)	(0.0928)	(9.988)	(4.309)	(3.440)	(7.893)
Treatment Baseline	0.392	0.110	0.357	0.0122	4.984	0.499	0.984	0.690
	(0.727)	(0.377)	(0.576)	(0.110)	(9.789)	(2.230)	(2.008)	(7.150)
Treatment Expost	0.526	0.0771	0.385	0.0101	5.935	0.367	1.728	0.168
	(1.402)	(0.296)	(0.579)	(0.100)	(11.81)	(2.018)	(3.260)	(1.170)

Average number of animals per household. Animal assets are categorized as either owned by the household individually or shared (owned in partnership).

Table 3.24: LIWP Impact on Livestock Assets Owned

	Cattle		Donkeys		Sheep or Goats		Chickens	Beehives
	own	share	own	share	own	share	own	own
LIWP Program	0.06	0.01	0.03	0.00	0.30	-0.26	0.29	-0.99
	(0.11)	(0.04)	(0.05)	(0.01)	(1.07)	(0.27)	(0.42)	(0.72)
Expost	0.09*	-0.04*	0.01	-0.01	0.73	0.10	0.52**	0.36
	(0.05)	(0.02)	(0.04)	(0.01)	(0.58)	(0.20)	(0.22)	(0.47)
Fixed effects	HH	HH	HH	HH	HH	HH	HH	HH
N	1890	1908	1906	1908	1908	1908	1906	1908

LIWP impact on average number of animals per household.

Table 3.25: Summary Statistics for Number of Households Benefiting from Animals

	Cattle		Donkeys		Sheep or Goats		Chickens	Beehives
	own	share	own	share	own	share	own	own
Control Baseline	0.336	0.104	0.416	0.0152	0.523	0.0716	0.395	0.0824
Control Expost	0.380	0.0868	0.401	0.00868	0.614	0.0629	0.419	0.0933
Treatment Baseline	0.288	0.0892	0.314	0.0122	0.493	0.0913	0.325	0.0609
Treatment Expost	0.320	0.0690	0.337	0.0101	0.515	0.0609	0.391	0.0365

Number of households that own at least one animal (individually or in partnership).

Table 3.26: LIWP Impact on Share of Households Benefiting from Animals

	Cattle		Donkeys		Sheep or Goats		Chickens	Beehives
	own	share	own	share	own	share	own	own
LIWP Program	-0.01	-0.01	0.05	0.00	-0.08*	-0.03	-0.05	0.06
	(0.04)	(0.03)	(0.04)	(0.01)	(0.05)	(0.03)	(0.03)	(0.06)
Expost	0.04	-0.02	-0.02	-0.01	0.10***	-0.00	0.02	0.01
	(0.03)	(0.01)	(0.03)	(0.01)	(0.03)	(0.01)	(0.02)	(0.04)
Fixed effects	HH	HH	HH	HH	HH	HH	HH	HH
N	1908	1908	1908	1908	1908	1908	1908	1908

LIWP impact on the extensive margin. Includes both animals owned individually and animals owned in partnership.

Table 3.27: LIWP Impact on Number of Animals from Among Households with Animals at Baseline

	Cattle		Donkeys		Sheep or Goats		Chickens	Beehives
	own	share	own	share	own	share	own	own
LIWP Program	-0.03	-0.05	-0.03	-0.00***	0.43	-2.47	-0.04	-8.56
	(0.16)	(0.16)	(0.07)	(0.00)	(1.42)	(2.37)	(0.63)	(6.34)
Expost	-0.21**	-0.81***	-0.23***	-1.00***	-0.52	-1.30	-0.19	-1.87
	(0.09)	(0.12)	(0.05)	(0.00)	(0.91)	(2.21)	(0.34)	(4.16)
Fixed effects	HH	HH	HH	HH	HH	HH	HH	HH
N	578	184	692	26	960	156	682	136

LIWP impact on the intensive margin. Includes both animals owned individually and animals owned in partnership

Table 3.28: Numbers of households that pawned possessions

	Pawned:	land	gold	car	animals
Control Baseline	0.080	0.900	0.429	0.000	0.385
Control Expost	0.100	0.213	0.213	0.043	0.174
Treatment Baseline	0.091	0.600	0.708	0.000	0.278
Treatment Expost	0.105	0.200	0.297	0.000	0.441

Table 3.29: Numbers of households that sold possessions

	Sold:	land	gold	car	animals
Control Baseline	0.245	0.200	0.667	0.040	0.886
Control Expost	0.405	0.043	0.144	0.145	0.715
Treatment Baseline	0.211	0.224	0.547	0.048	0.786
Treatment Expost	0.389	0.018	0.165	0.071	0.835

Table 3.30: Number of households in debt by owner of debt

	In debt:	to store owner	to sheikh	to bank	to other	Paid off some	Paid off all
Control Baseline	0.779	0.657	0.008	0.006	0.730	0.275	0.041
Control Expost	0.824	0.786	0.024	0.011	0.657	0.479	0.030
Treatment Baseline	0.738	0.654	0.030	0.011	0.679	0.341	0.030
Treatment Expost	0.822	0.768	0.010	0.012	0.602	0.513	0.045
Observations	1907	1507	1507	1507	1507	1908	1908

Table 3.31: LIWP Impact on Probability of Being in Debt or Repaying Debt

	In debt	Paid off some	Paid off all	Pawned	Sold
LIWP Program	0.06	-0.03	0.03	-0.05	-0.03
	(0.05)	(0.08)	(0.03)	(0.04)	(0.06)
Expost	0.03	0.20***	-0.01	0.04*	0.18***
	(0.03)	(0.04)	(0.02)	(0.02)	(0.04)
Fixed effects					
N	1906	1908	1908	1908	1900

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.32: Summary Statistics for Household Debt and Value of Assets Sold

	Total Debt	Store Debt	Paid Off	Outstanding Debt	Assets Sold
Control Baseline	98.23	54.08	15.81	17.14	20.83
	(162.7)	(115.5)	(64.98)	(55.87)	(68.56)
Control Expost	155.2	86.71	21.80	43.24	33.00
	(965.5)	(245.7)	(52.86)	(187.3)	(154.1)
Treatment Baseline	85.72	52.30	10.66	25.33	16.11
	(215.8)	(178.6)	(28.67)	(91.62)	(98.54)
Treatment Expost	94.64	61.20	27.41	31.29	21.35
	(146.4)	(98.78)	(70.00)	(88.07)	(54.09)
Observations	1908	1908	1908	1908	1908

Values are measured in thousands of riyal. (1000 riyal= $4.70)

Chapter 4

LIWP Impact on Household Expenditures and Food Security

4.1 Self Reported Impact on Expenditures by Participating Households

One indication of the way in which program income increased household welfare can be taken from the answers to direct questions to participants how they spent their income money from the LIWP program. While program income is fungible, this may indicate where participants felt that their spending had changed most after participating in the program. In the survey, participants were given a list of categories including food, debt, medicine, household items, clothing, and and could indicate more than one category where they spent program income. Almost all (94%) participants indicated consumption spending on food, and 44% of participants indicated spending on debt repayment. This is consistent with the finding above of a large LIWP impact on outstanding debt, and suggests that we should expect to find a positive effect on food consumption in the analysis below. There was relatively little report of spending in any of the other categories, including free responses not included in the table. Shares of households reporting spending in each category are summarized in tables 4.1.

Considering spending on food, debt, and medicine to be less discretionary than spending in the other categories (such as clothing, household furniture, etc.) we recombined reported spending into three super-categories: food, debt, or medicine only; food, debt, or medicine and other, and other only. This arrangement shows that almost 60% of participants spent only food, medicine, or debt repayment. See table 4.2. In figure 4.1, we separate participants into two groups- those who spent only on food, medicine, or debt repayment and those who spent on other categories as well, and graph the distribution of benefits received in each group. Interestingly, it becomes more likely that household will report spending at least some of project income on goods other than food, debt, or medical care at the level of approximately 100,000 riyals, which is the per household benefit that the project was originally designed to deliver, based on the estimated amount needed to address the food crisis. We conclude was indeed the correct amount of benefit to deliver on average.

4.2 Non-durable Expenditures

The survey included a short section on consumption of non-durable goods other than food during the past month to see if the program had increased household welfare in this dimension, but we do not find any significant program impact. The categories of goods asked for in the survey were qat, tobacco, medicine, clothes, bedding, and housing. Table 4.3 presents summary statistics and table 4.4 presents regression results. None of the coefficients on program impact are statistically

significant. Both tobacco and qat spending appear to show a significant positive time trend between baseline and expost, however, this difference is likely due to increased spending and higher prices around the Eid holiday.

4.3 Self Reported Food Shortage

Both treatment and control communities showed an increase in self-reported food shortage between baseline and expose. Households that reported they had experienced a shortage of food in the past 12 months were asked about how they had coped with the shortage of food. Between baseline and expost, the number of households in which both children and adults skipped meals due to food shortage more than doubled, reaching almost 10% in control communities. We find a negative LIWP impact on the share of households with this type of severe food shortage of 3.6 percentage points, compared to an increase between baseline and expost of 7.4 percentage points, however, the coefficient on LIWP impact is not significant, due to the small number of observations. Summary statistics are presented in table 4.5 and regression results in table 4.6.

Regarding food insecurity where adults skip meals or some other form of coping is used, coefficients on LIWP impact are mostly smaller and non-significant, although in some cases they are positive. It is worth remembering that self-reported food insecurity is subjective and may be biased by the respondents expectations about receiving government benefits.

4.4 Calorie Consumption per Capita

The survey included a detailed module on household daily consumption of staple carbohydrates. We estimate average daily calorie consumption per household based on reported data on the dry volume consumption of staple carbohydrates in the past two days. [1] Consumption per household is divided by the number of equivalent adults in the house adjusted for the presence of guests during the past two days. We define equivalent adults here based on standard calorie requirements by age and gender relative to adult men. [2]

Looking at consumption per capita by grain (table 4.7), most consumption comes from wheat and white flour. There is an increase in consumption of rice, which may be a seasonal effect of Eid holiday dishes being more likely to include rice. Most noticeably, there is a sharp decrease in the amount of wheat consumed between baseline and expost, which is not balanced by increases in consumption of any other grain.

After converting the volume of grains and sugar consumed to their caloric equivalents and adjusting upwards to accommodate 20% of calories from other sources, we found average per capita calorie consumption of between 2600 and 3000 calories per day. Importantly, the process of estimating average calories introduces many opportunities for measurement error from rounding errors and variations in density in measuring the original volume, to data entry errors that are not immediately apparent, to error introduced in the process of estimating the number of equivalent adults, so we are left with a distribution that has a high variance with a long right-hand tail and includes daily calorie consumptions that seem unrealistic. The distributions of estimated calories per capita are drawn in figure 4.2. As a partial solution, we check for robustness of the estimates after trimming the top and bottom 0.5 and 1% of observations. Summary statistics are presented in 4.9. For the regression analysis, we also use household fixed effects, since we hope that many of the errors related to measurement and household size will be constant between baseline and expost. We also repeat the analysis using logs rather than levels as the dependent variable to estimate the average percentage change per household.

We find a program impact of between 320 and and 435 calories per day, equivalent to a 11-13% increase in calorie consumption in treated communities relative to untreated communities. Using

[1] See Egel and Al-Maweri 2011 [1] for full detail.

[2] Daily calorie requirements relative to adult men are taken from "Human Energy Requirements" FAO technical report using moderate activity level (http://www.fao.org/docrep/007/y5686e/y5686e06.htm)

our level regressions, this change is slightly greater than the decrease in calorie consumption caused by the crisis which we estimate at between 244 and 345 calories per day. Regression results are reported in table 4.10.

4.5 Consumption of High Values Foods

In addition to total caloric consumption, food variety and macronutrient content is an important dimension of food security.

In the household survey, we have data on how often during the past month households consumed several types of "high-value" foods. The survey asked about the most common protein sources (meat, chicken, fish, eggs, tuna, and milk), as well as honey as an example of a luxury good. As seen in the summary statistics (table 4.11), households only ate meat, chicken, fish, eggs or tuna about 1-2 times per month on average, and milk only every three days on average.

In looking at changes in consumption of these high value foods, a major issue is that much of the expost data was collected within the month following the Eid al-Adha holiday. [3] The celebration of Eid al-Adha involves the sacrifice of a sheep or goat and distribution of meat to the poor, so meat consumption is much higher than at other times of the year. We can see this in the data, as there is a significant increase of 2 times per month in the frequency of eating meat, and lesser in magnitude but statistically significant decreases in the consumption of chicken and eggs. Figure 4.3 shows graphically how the frequency of meat consumption was related to the timing of the expost survey date relative to Eid al-Adha.

We attempt to control for the effect of increased meat consumption during Eid by adding a control variable for days since the Eid included the 30 day survey time frame, as well as a squared term. This adjustment is imperfect, however, since survey time was non-random, with more remote communities surveyed later. Estimated program impacts remain insignificant. Regression results are reported in tables 4.12 and 4.13.

4.6 Anthropometric Measurement of Children

The household survey collected height and weight data for children five years of age and younger. Unfortunately, due to problems with the measuring equipment and training, the baseline data for anthropometric measures included a high degree of measurement error. There are also numerous missing values in the baseline survey, which may or may not be randomly distributed. As seen in the distributions below in figures 4.4 and 4.5, the recorded data on weight-for-height in the baseline survey have a large variance, and include many unrealistic values, including numerous z-scores over 5 and under -5, which WHO standards would consider a sufficient flag for omitting as physically impossible. Regression tests for program impacts including baseline values are extremely sensitive to the cutoff used for the exclusion of physically impossible unrealistic values that represent data entry error or enumerator error. A differences-in-differences regression including baseline data (not shown) would imply that the program had an insignificant impact on average z-scores, however, given the unreliability of baseline data, we focus rather on simple differences in the expost measurements.

Average z-scores are slightly worse in treatment villages than in control villages, and there is a slightly, but not significantly, higher rate of wasting (z-score<-2). This difference in levels is probably related to the smaller number of very young children in the control villages. Figure 4.6 shows the distribution of ages in months in treatment and control villages.[4] In the expost survey, 26% of children recorded with ages 12 months or less had weight for height z-scores below -2, while only 11 % of children overall had z-scores below -2, so it is not surprising that average

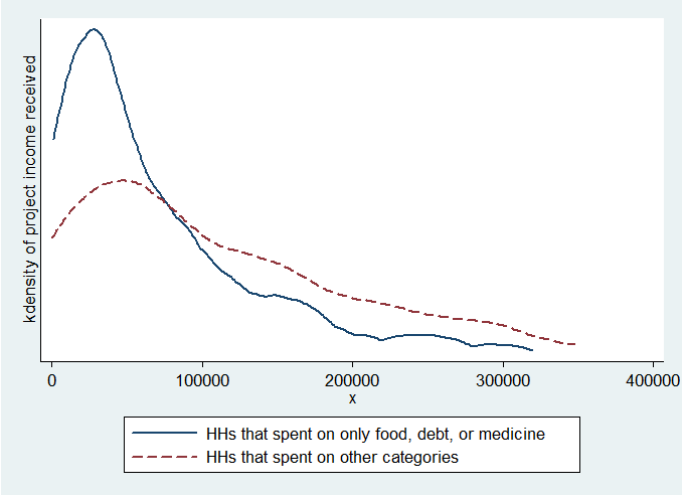
[3] For the staple grain consumption analysis on the previous section, the timing of Eid is not a problem, since grain consumption is measured for only 2 days previous so holiday consumption is not included.

[4] Exact birthdays are usually not known, so most ages are rounded to the nearest year. For this reason, we focus on weight-for-height and bmi rather than weight-for-age measures, although the later would be more informative about nutritional status.

z-scores are lower in treatment communities. Table 4.14 reports average rates of wasting in expost communities. Depending on how the sample is subdivided by age, the rates of wasting can be higher or lower in the treatment communities relative to control communities.

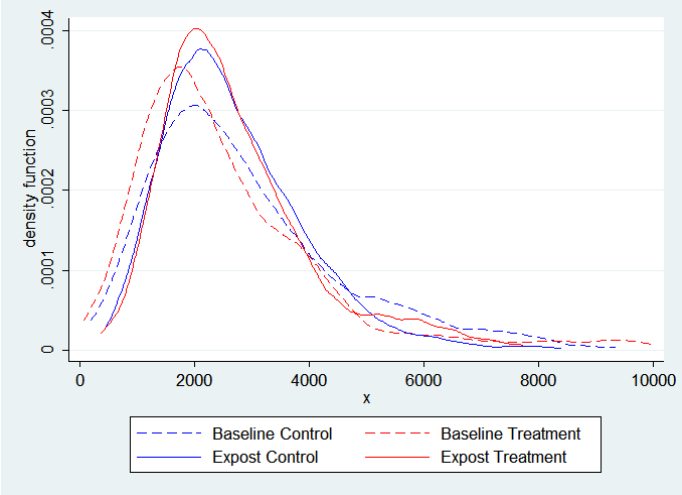
We conclude that it is not possible to see an LIWP impact in the anthropometric data. The simple differences between treatment and control communities in expost are influenced by a differing distribution of ages, and the baseline data include too much measurement error to look at relative changes over time.

Figure 4.1: Density of Benefit Level By Type of Spending



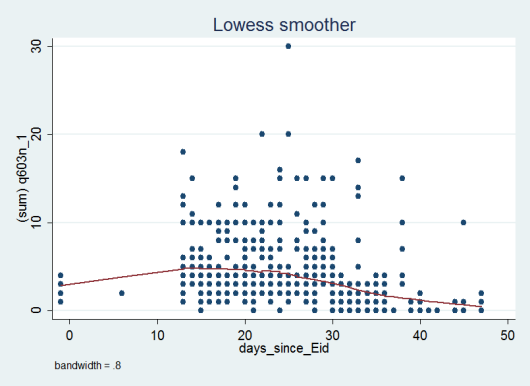
Kernel density of project benefits for households that reported spending *only* on food, debt, or medical care, vs. households that also reported spending on household goods, investments, clothes, gold, etc.

Figure 4.2: Density of Estimated Calorie Consumption per capita



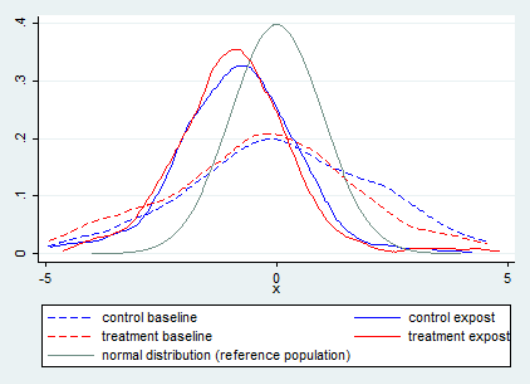
Distribution of estimated calorie consumption per capita. Variance is high, including many unrealistic values, and the distribution is skewed to the right.

Figure 4.3: Meat Consumption and Eid al-Adha



Number of times meat consumed per month as a function of days since Eid al-Adha included in 30 day survey recall time frame

Figure 4.4: Distributions of Weight for Height for Children Age 0-5



Weight for length is used for instead of weight for height for children under 2 years

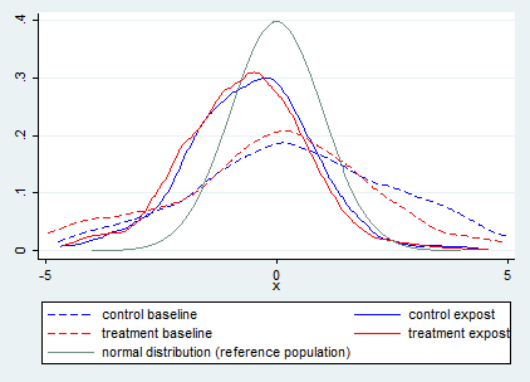


Figure 4.5: Distributions of Weight for Height for Children Age 0-5

Weight for length is used for instead of weight for height for children under 2 years

Figure 4.6: Distributions of Age in Months for Children Age 0-5

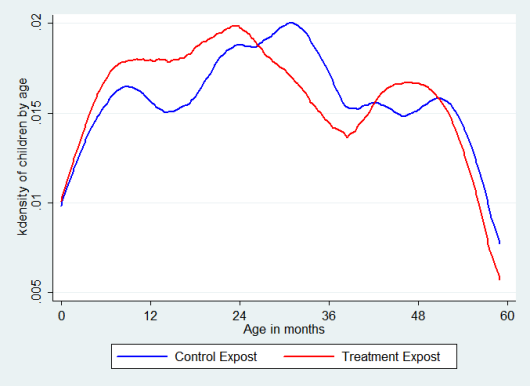


Table 4.1: Self-reported Use of LIWP Income by Participating Households

	Food	Equipment	Animals	Marriage	Debt	Home Improvement	Furniture	Other
	0.94	0.04	0.08	0.04	0.4	0.03	0.04	0.04
N	394	392	391	391	393	391	391	391

Share of participating households that reported using program income for different categories of spending.

Table 4.2: Self-reported Use of LIWP Income (Continued)

	Food, Debt, or Medical Only	Food, Debt, or Medical and Other	Other only
	0.59	0.36	0.04
N	391	391	391

Share of participating households that reported using program income for different categories of spending.

Table 4.3: Summary Statistics for Expenditure on Non-Food Consumption in Past Month

	Qat	Tobacco	Medicine	Clothes	Bedding	Housing and Kitchen
Control Baseline	2904.3	576.1	58098.4	21698.0	1987.0	1736.5
	(5678.0)	(1439.1)	(110478.3)	(35706.7)	(6360.4)	(5315.8)
Control Expost	7233.0	927.5	56457.6	26673.1	2491.8	1009.4
	(12189.1)	(2922.4)	(95566.6)	(53255.0)	(8856.3)	(3305.6)
Treatment Baseline	2996.6	785.8	51602.5	22489.0	3323.5	2058.6
	(5825.6)	(2074.1)	(97019.7)	(39186.6)	(12565.2)	(6482.9)
Treatment Expost	6525.8	701.0	58581.5	24528.5	2919.9	1829.6
	(10563.1)	(1566.8)	(106130.0)	(27891.6)	(9675.3)	(6351.2)
Observations	1910	1910	1910	1910	1910	1910

Table 4.4: LIWP Impact on Expenditure on Non-Food Consumption in Past Month (Continued)

	Qat	Tobacco	Medicine	Clothes	Bedding	Housing and Kitchen
LIWP Program	-575.2	-389.5	2303.0	1877.9	-2472.1	60.2
	(1587.9)	(264.2)	(11997.8)	(5979.5)	(1523.0)	(669.2)
Expost	4215.7***	326.5*	1626.7	2491.9	1312.2	-501.3
	(1107.7)	(182.2)	(7835.7)	(3233.4)	(1045.9)	(396.3)
Fixed effects	HH	HH	HH	HH	HH	HH
N	1910	1910	1910	1910	1910	1910

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Expenditure measured in riyal (212 riyal= $1)

Table 4.5: Summary Statistics for Self Reported Food Shortage

	Shortage	Adults skip meal	All skip meal	Loan	Less variety	Store credit
Control Baseline	290	76	13	138	75	198
Control Expost	314	80	45	151	145	191
Treatment Baseline	312	53	18	171	84	182
Treatment Expost	335	71	39	169	158	206

Number of households reporting that they experienced food shortage and how the household responded to the food shortage. For clarity of presentation number rather than share of households is given. The denominator would be slightly larger for treatment (461 control vs. 493 treatment).

	Shortage	Adults skip meal	All skip meal	Loan	Less variety	Store credit
LIWP Program	0.022	0.007	-0.036	-0.007	0.085	0.030
	(0.082)	(0.083)	(0.044)	(0.076)	(0.063)	(0.100)
Expost	0.038	0.019	0.074***	0.015	0.107***	0.002
	(0.057)	(0.059)	(0.027)	(0.055)	(0.041)	(0.065)
Fixed effects	Comm	Comm	Comm	Comm	Comm	Comm
N	1908	1908	1908	1908	1908	1908

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.6: Regression test for program effect on self reported food shortage and coping strategies

Table 4.7: Summary Statistics for Grain Consumption Per Capita

	wheat	white flour	sorghum	maize	millet	rice	sugar
Control Baseline	871.3	305.6	63.96	80.16	29.81	60.84	80.15
Control Expost	492.2	304.5	91.90	27.39	32.05	177.3	76.64
Treatment Baseline	650.2	224.2	34.11	12.19	12.83	65.74	86.30
Treatment Expost	543.1	282.0	88.86	16.13	33.66	200.7	71.67
Observations	1711	1711	1711	1711	1711	1711	1711

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Consumption is measured in volume (mL) per equivalent adult per day by type of grain. Scaling of equivalent adults is based on relative calorie needs.

Table 4.8: Summary Statistics for Calorie Consumption Per Capita

	Avg calories per capita		
	(1)	(2)	(3)
	all observations	trimmed 0.5%	trimmed 1%
Control Baseline	3021.9	3040.2	2949.5
	(2010.9)	(2005.0)	(1725.8)
Control Expost	2724.1	2691.2	2696.2
	(1405.1)	(1218.1)	(1214.7)
Treatment Baseline	2701.2	2676.3	2630.6
	(2097.1)	(1863.5)	(1708.5)
Treatment Expost	2777.0	2781.9	2745.8
	(1492.6)	(1490.1)	(1346.9)
Observations	1884	1870	1852

mean coefficients; sd in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.9: Estimated average calories consumed per equivalent adult. Scaling of equivalent adults is based on relative calorie needs.

Table 4.10: LIWP Impact on Calorie Consumption Per Capita

	calories			ln(calories)		
	(1)	(2)	(3)	(4)	(5)	(6)
	all obs	trimmed 0.5%	trimmed 1%	all obs	trimmed 0.5%	trimmed 1%
LIWP Program	324.25	435.71*	324.64	0.13	0.13	0.11
	(273.23)	(259.39)	(226.11)	(0.09)	(0.08)	(0.08)
Expost	-282.87*	-345.31**	-244.78	-0.01	-0.03	-0.02
	(166.12)	(171.76)	(151.35)	(0.06)	(0.06)	(0.05)
Fixed effects	HH	HH	HH	HH	HH	HH
N	1864	1836	1800	1864	1836	1800

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.11: Consumption of High Value Foods

	Meat	Chicken	Fish	Eggs	Tuna	Milk	Honey
Control Baseline	0.806	2.019	2.431	2.159	1.266	10.93	1.085
	(2.089)	(3.629)	(6.366)	(5.697)	(3.639)	(13.87)	(4.160)
Control Expost	3.346	1.355	2.235	1.458	1.056	13.36	1.563
	(3.602)	(3.086)	(5.952)	(4.477)	(3.722)	(13.71)	(4.864)
Treatment Baseline	0.787	1.869	2.656	2.239	1.928	11.27	0.521
	(1.636)	(2.514)	(6.703)	(6.057)	(5.299)	(15.32)	(2.716)
Treatment Expost	3.362	1.227	2.221	1.945	1.305	12.94	1.298
	(3.429)	(2.081)	(5.860)	(4.977)	(3.665)	(14.08)	(4.256)
Observations	2071	2071	2071	2071	2071	2071	2071

Consumption is measured as number of times food type consumed in past month.

Table 4.12: LIWP Impact on Consumption of High Value Foods

	Meat	Chicken	Fish	Eggs	Tuna	Milk	Honey
LIWP Program	0.111	-0.296	-0.356	0.425	0.164	-0.504	0.118
	(0.297)	(0.306)	(0.579)	(0.540)	(0.441)	(1.466)	(0.436)
Expost	2.606***	-0.412**	-0.090	-0.590*	-0.441	2.510***	0.559**
	(0.194)	(0.200)	(0.378)	(0.352)	(0.288)	(0.957)	(0.284)
Fixed effects	HH	HH	HH	HH	HH	HH	HH
N	2023	2023	2023	2023	2023	2023	2023

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.13: LIWP Impact on Consumption of High Value Food Controlling for Survey Date

	Meat	Chicken	Fish	Eggs	Tuna	Milk	Honey
LIWP Program	-0.027	-0.371	-0.196	0.325	0.056	-0.946	-0.025
	(0.290)	(0.315)	(0.591)	(0.555)	(0.453)	(1.509)	(0.448)
Expost	-6.401***	-1.552	8.339***	-1.672	-1.361	-7.883	-4.282**
	(1.275)	(1.384)	(2.595)	(2.440)	(1.990)	(6.630)	(1.968)
Days since Eid	0.053	0.100	-0.099	0.141	0.158*	0.518*	0.139
	(0.057)	(0.062)	(0.117)	(0.110)	(0.090)	(0.299)	(0.089)
Days since Eid sqrd	-0.004***	-0.002	0.004*	-0.002	-0.002	-0.010*	-0.003**
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.006)	(0.002)
Fixed effects	HH	HH	HH	HH	HH	HH	HH
N	2023	2023	2023	2023	2023	2023	2023

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.14: Rate of Wasting Among Children

	Weight for Height	BMI
Control Expost	17.4%	12.9%
Treatment Expost	18.0%	15.7%
P-value from test of proportions	0.74	0.22

Chapter 5

Impact of LIWP Constructed Infrastructure

Reported satisfaction with the LIWP projects was quite high and we show that water-related projects brought significant benefits to the local community.

Most community projects included multiple component projects. In the 44 treatment communities in our sample, 84 different projects were undertaken via the LIWP program. The most common type of project was terrace repair, followed by rural road improvement. The full list of project types can be found in table 5.1. The vast majority of respondents indicated that the project was needed by the community. 95% of respondents indicated that the project was beneficial to the community as a whole, and 80% indicated that their household benefited directly from the project. Of those that did not benefit, the main reasons were either because they were far from the project location, or did not own land. Survey responses to questions about the project usefulness are reported in table 5.2.

At the time of the expost survey, most projects were still incomplete. Out of 44 treatment communities, only 8 were officially completed at the time of the survey, and an additional 7 were completed less than two months after this expost survey. This is a small sample on which to test for project impacts. To avoid selection bias, we compare outcomes in the communities with completed projects to outcomes in their paired community from the stratification step of randomization. We exclude 2 of the 8 communities because changes in treatment status disrupted the pairing. Among communities with completed projects and no changes to the pairing, there were 4 completed projects related to water and 2 completed projects related to roads. If we include communities where the project was almost complete at the time of the survey, we have 9 projects related to water and 6 projects related to roads.

Due to the small number of villages, in the regressions in this section we use bootstrapped standard errors following the approach of Cameron, A. Colin; Miller, Douglas L.; Gelbach, Jonah B. (2006) : Bootstrap-based improvements for inference with clustered errors, as implemented for Stata by Judson Caskey. Instead of using household fixed effects, in this section we define the dependent variable as the difference between expost and baseline for each household.

5.1 Water Accessibility

Water-related projects included constructing water storage tanks and cisterns, rainwater harvesting tanks, and improvement of shallow wells. We test for improvements in water accessibility in communities with completed water-related projects.

One of the main costs for villagers with poor access to water is the time spent in fetching water. The length of the trip in the rainy season was shortened as a result of the LIWP intervention. Table 5.3 presents summary statistics on the length of a trip to fetch water. During the rainy season, the length of time fell within the subsample of certainly completed projects by an average

of 9 minutes, and within the full sample of probably complete projects by 18 minutes. In the later case, the impact is highly statistically significant. Regression results are presented in table 5.4.

In addition, the increased access to water resulted in 1-2 fewer months of water shortage per year. This represents a decrease of about 50% compared to an average for 3-4 months on average of water shortages. In both the subset of complete and full set of probably complete projects, this change was significant, with greater magnitude in the subset of certainly complete projects. Summary statistics are presented in table 5.5, while regression results are reported in 5.6.

5.2 Transportation Costs

Households were asked for the travel time, cost, and frequency of visiting the nearest market. Recall data for two years previous to baseline as well as baseline and expost is available. As seen in the summary statistics in table 5.7, trends in travel time and cost were similar in treatment and control communities prior to the intervention, with decreases in travel time and increases in cost (due to inflation) prior to baseline. We do not find any significant change related to the LIWP intervention. See table 5.8

Table 5.1: Types of Projects in Surveyed Communities

	Number of Projects
Rural Roads	16
Tanks/ Springs	8
Cistern/ Kareif	12
Dam / Barrier	5
Surface /Shallow wells	12
Water Channels	3
Terraces	19
Removing Harmful Plants	1
Total Projects	82
Total Communities	44

Table 5.2: Self-reported Benefits of LIWP Infrastructure Projects

Share of HHs who say project was needed by the community as a whole	**95.5%**
Share of HHs who benefit directly from the project or plan to benefit in the future	**79.5%**
Of households that do not benefit:	
Share that do not benefit because too far from project location	**40%**
Share that do not benefit because do not own land	**40%**
Share that do not benefit because project was not completed	**26.2%**

Table 5.3: Summary Statistics for Time to Fetch Water

	Certain Completion		Probable Completion	
	Rainy	Dry	Rainy	Dry
Water Control Baseline	31.76	150.3	36.36	110.6
	(18.10)	(121.4)	(30.11)	(109.5)
Water Control Expost	33.59	140.8	50.85	121.6
	(18.99)	(111.9)	(40.52)	(103.3)
Water Treatment Baseline	34.69	64.89	48.90	83.68
	(33.78)	(54.04)	(50.60)	(63.74)
Water Treatment Expost	28.18	73.89	42.62	91.76
	(40.02)	(76.98)	(41.90)	(70.48)

Time in minutes for a trip to fetch water during the rainy season and during the dry season (including travel time both directions and time spent waiting). First two columns include only projects completed at the time of the expost survey, while last two columns also include projects which were officially completed within two months of the expost survey.

Table 5.4: LIWP Impact on Trip Time to Fetch Water

	Certain Completion		Probable Completion	
	Rainy	Dry	Rainy	Dry
LIWP Water Project	-8.781	20.247	-18.254	0.577
	(7.712)	(30.462)	(12.943)	(8.199)
N	90	90	201	205

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

LIWP project impact on time in minutes for a trip to fetch water during the rainy season and during the dry season (including travel time both directions and time spent waiting). First two columns include only projects completed at the time of the expost survey, while last two columns also include projects which were officially completed within two months of the expost survey.

Table 5.5: Summary Statistics for Number of Months of Water Shortage during Past Year

	Months of Shortage	
	Certain Completion	Probable Completion
Water Control Baseline	3.644	3.753
	(3.891)	(3.253)
Water Control Expost	2.556	2.543
	(2.896)	(2.678)
Water Treatment Baseline	4.081	4.415
	(3.121)	(2.772)
Water Treatment Expost	1.778	1.382
	(2.194)	(1.856)

Table 5.6: LIWP Impact on Number of Months of Water Shortage during Past Year

	Months of Shortage	
	Certain Completion	Probable Completion
LIWP Water Project	-2.634***	-1.800**
	(0.846)	(0.832)
N	373	163

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5.7: Summary Statistics for Travel Time and Cost of Trip to Market

	Time	Cost
Road Project Control Recall	193.2	359.1
	(56.26)	(394.8)
Road Project Control Baseline	192.3	386.4
	(56.81)	(390.4)
Road Project Control Expost	163.6	773.8
	(58.35)	(422.4)
Road Project Treatment Recall	167.6	576.2
	(62.76)	(391.0)
Road Project Treatment Baseline	164.1	622.7
	(64.12)	(404.7)
Road Project Treatment Expost	138.3	939.1
	(37.01)	(563.1)

2008 values are recall data from the baseline survey.

Table 5.8: LIWP Impact on Travel Time and Cost of Trip to Market

	Time	Time	Cost	Cost
LIWP Road Project	0.909	15.444	-34.416	32.056
	(2.637)	(23.488)	(94.644)	(160.484)
N	44	131	43	129

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2008 values are recall data from the baseline survey.

Chapter 6

Conclusion

The LIWP program was effective in reaching large number of rural households and had a significant effect on increasing average days worked and average wages. Participants generally reported that projects were useful, well administered, and provided direct benefits. In addition, the program increased the probability of non-agricultural employment, and the probability of employment of women.

A major finding in this report is that there was a wide variation in the level of benefits received by community members. This variation is a result of several factors. First, at the household level, LIWP was designed to be self targeting by setting a wage lower than the prevailing unskilled wage in the area, but due to the economic crisis, the average wage level fell, resulting in LIWP employment being more attractive than originally designed. Secondly, there was originally an intention to limit households to a maximum number of work days, but this was not enforced during implementation. Finally, program wages were set by piece rate, resulting in higher wages per day for workers involved in more skill intensive tasks, or who worked longer hours. In spite of this variation, LIWP benefits were progressive overall, with more benefits going to households whose scores on a proxy means test on baseline were associated with greater probability of poverty.

Impact evaluation results are not as strong as expected because benefits were not spread evenly, but we do find positive program effects on debt repayment, food security, and durable good ownership. These findings suggest that the LIWP program played a role in cushioning targeted communities from the economic shock of 2010-2011, averting possible longer term consequences related to selling off assets and increased debt. We also find positive impacts of the LIWP created infrastructure on water availability, and note that longer term benefits in projects that were still incomplete were not captured by this survey.

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Appendix I: Attrition

The 120 communities sampling frame were randomly selected from among a list of suitable communities proposed by the local branches, then paired by economic type and geographic types and randomly assigned to treatment or control within these pairs. Due to some confusion about community names or other complications, the original sample at baseline was slightly different.

Most non-resurveyed communities were not resurveyed due to treatment assignment changing between baseline and expost, while some were inaccessible due to the crisis. A smaller set of communities were resurveyed, but excluded from analysis due to switch in treatment assignment.

Sample size at baseline:	originally assigned to control	originally assigned to treatment
original sample	732	696
community not resurveyed	218	206
household not resurveyed	23	27
resurveyed but assignment switched	65	24
total attrited	307	258
final sample	426	439

Communities with lower employment and lower skill levels at baseline were more likely to be switched or more generally dropped from the sample. However, both differential attrition more generally and switching more specifically, appear to be balanced between original treatment and original control. This implies that the findings of the analysis below are unlikely to be biased by the attrition, although it is not as representative of the effects of the program in all parts of the country. In particular, a disproportional share of the attrition occurred among communities administered through the Hajjah branch due to some administrative problems.

In each set of t-tests, the first column shows the difference between treatment and control communities as assigned at baseline to test for true randomness of assignment. As the procedure for assignment is known and used a random number generator to assign paired communities to either treatment or control, we do not expect to find significant differences in baseline characteristics.

The second and third columns compare switched communities to non-switched communities among those originally assigned respectively to treatment and control. Switched communities are a subset of all attrited communities which were dropped from the sample. Other communities were dropped due to being paired with switched communities, or due to conflicts in the area preventing data collection.

The fourth and fifth columns compare the baseline characteristics of the final sample (after dropping non-panel households and non-matched communities) to the original sample. The final column compares treated communities to control communities in the final sample used in the remainder of the analysis.

	original balance	switch C	switch T	attrition C	attrition T	final balance
avg. skill	0.0817***	-0.106***	-0.125***	-0.0436*	-0.135***	0.118***
	(4.39)	(-2.76)	(-3.80)	(-1.67)	(-4.98)	(4.93)
avg. wage	138.4**	-47.07	128.8	92.25	67.70	146.3*
	(2.16)	(-0.52)	(0.87)	(0.96)	(0.70)	(1.90)
Observations	2520	1090	1126	1273	1247	1574

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.1: Tests for differences in average skill level and wages at baseline

	assign to treatment	switch C	switch T	attrited T	attrited C	final treated
days last year	-27.46	-99.07***	-96.14***	-73.66**	-81.71***	-23.66
	(-1.44)	(-3.81)	(-2.88)	(-2.52)	(-3.43)	(-0.91)
days last month	-2.533	-7.267***	-10.53***	-7.628***	-5.552**	-3.319
	(-1.48)	(-3.06)	(-3.58)	(-3.06)	(-2.54)	(-1.39)
Observations	1428	648	599	708	720	854

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.2: Tests for differences in average days worked per household at baseline

	original balance	switch C	switch T	attrition C	attrition T	final balance
male employment	-0.00186	-0.00777	0.0116	-0.00953	-0.00581	-0.00329
	(-0.28)	(-0.45)	(1.26)	(-0.96)	(-0.57)	(-0.42)
female employment	-0.0502**	-0.0499	-0.00441	-0.0431	0.0382	-0.0777**
	(-2.12)	(-0.69)	(-0.08)	(-1.35)	(0.94)	(-2.52)
Observations	2241	978	997	1141	1100	1397

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.3: Tests for differences in employment rates for men and women at baseline

	original balance	switch C	switch T	attrition C	attrition T	final balance
gov	0.00596	-0.0943***	-0.0369	-0.0190	-0.0541**	0.0203
	(0.32)	(-3.17)	(-1.35)	(-0.72)	(-2.07)	(0.81)
own ag.	-0.00155	-0.0652	0.0677*	-0.0135	0.0340	-0.0207
	(-0.06)	(-1.15)	(1.70)	(-0.35)	(0.90)	(-0.61)
other ag.	0.00294	0.0382	0.0820***	0.0989***	0.123***	-0.00756
	(0.15)	(0.84)	(2.61)	(3.40)	(4.24)	(-0.35)
non-ag	0.0324	-0.0474	-0.110***	-0.0863**	-0.108***	0.0420
	(1.23)	(-0.82)	(-2.77)	(-2.26)	(-2.87)	(1.23)
rent	0.00669	-0.00412	0.0254*	0.00954	0.0318**	-0.00239
	(0.93)	(-0.32)	(1.81)	(0.95)	(2.53)	(-0.34)
charity	0.0182	0.0620	0.0234	0.0176	0.000512	0.0250
	(0.92)	(1.31)	(0.75)	(0.62)	(0.02)	(0.98)
remit. Yemen	-0.0189*	-0.00366	-0.0209*	-0.00789	-0.0192	-0.0143
	(-1.79)	(-0.15)	(-1.70)	(-0.47)	(-1.52)	(-0.99)
remit. abroad	-0.0167	0.00870	-0.0455**	-0.0218	-0.0270	-0.0145
	(-1.17)	(0.25)	(-2.52)	(-1.03)	(-1.43)	(-0.74)
Observations	1428	708	720	708	720	854

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.4: Tests for differences in sources of income at baseline

	original balance	switch C	switch T	attrition C	attrition T	final balance
Alt_avg_cal	-479.7	-871.3*	-315.9	1428.6**	173.5	-978.5
	(-1.11)	(-1.68)	(-1.46)	(2.04)	(0.85)	(-1.39)
N	1412	703	709	703	709	845

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.5: Tests for differences in calorie consumption at baseline

Appendix II: Average Wages by Governate

	control baseline	control expost	treat baseline	treat expost
Aden	979.5	513.0	918.8	812.9
Aden	1894.0	917.7	1281.2	1115.1
Al-Hodeidah	1213.8	1536.3	1310.3	1591.0
Hajjah	963.2	1155.8	973.2	1246.1
Taiz	1107.3	912.7	1315.3	1284.6
Taiz	1009.6	556.8	1461.2	1341.1
Dhamar	875.2	1124.8	1464.3	2177.5
Amran	851.8	860.3	991.4	1231.0
Dhamar	1080.8	1106.3	1335.5	1566.8

Table 6.6: Average wages by governate before and after LIWP intervention

Appendix III: Notes on Direction of Potential Bias from Unknown Wages

Both treatment and control communities showed large increases in self-employment, for most of which the wage was unknown. Overall, the treatment communities in expost had fewer positions with unknown wages, but more positions with unknown wages if restricted to non-self-employed jobs than control communities. Since we exclude self-employment in agriculture from our estimates, and assuming that self-employment in agriculture is less remunerative than wage labor would mean our estimate of the wage effect underestimates the true impact. Likewise, with non-self-employment, if all positions with unknown wages had lower than average wages, the program effect would be underestimated.

	Self-Employed	Non-Self-Employed
Control Baseline	175	48
Control Expost	1309	27
Treatment Baseline	196	41
Treatment Expost	1176	55

Table 6.7: Number of jobs with unknown wages

	All	Self-Employed	Non-Self-Employed
	count	count_se	count_nonse
LIWP Program	-0.442***	-0.491***	0.255
	(0.136)	(0.160)	(0.164)
treatment	0.115	0.143	-0.033
	(0.150)	(0.177)	(0.116)
expost	1.023***	1.165***	-0.590***
	(0.095)	(0.123)	(0.125)
Observations	6675	6675	6675

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.8: Probit test of program effect on share of jobs with unknown wages

Appendix IV: Recommendations for Program Design

- 1 There is a fundamental conflict between the program objectives of efficiently employing labor to construct local public works, and directing benefits to poor households, which are less likely to have members that are highly skilled in the types of labor needed for these projects. The lack of an enforced household level of benefits from the project favors the efficiency objective over the equity objective.
- 2 Because daily wages vary greatly as a result of the piece-rate payment system and flexible hours of work, maximum or minimum household benefits should be expressed in monetary terms, rather than in days of work.

Appendix V: Recommendations for Data Collection and Evaluation

Several lessons on evaluation design emerged from this round of analysis:

- 1 Some steps may be taken to reduce the community level attrition between baseline and expost. Better communication with branch offices regarding the importance of respecting treatment assignment in a randomized evaluation may be helpful. In cases where treatment assignment is not respected, however, resurveying these communities and maintaining a record of the change in status and reason for the change would be preferable to completely dropping them from the analysis.
- 2 Understanding that some amount of attrition will always occur due to real world conditions such as conflict and flooding, the initial sample size may need to be larger to accommodate the expected loss.
- 3 A system of consistent codes for households between administrative records and the evaluation sample frame would have allowed us to more easily verify benefits received from LIWP and days worked.
- 4 The survey question on days worked allowed reporting of employment in multiple jobs. Each job is associated with a sector, skill level, average daily wage, and days worked in the past month. Days worked in the past year, however, were only reported as an aggregate per person. Consequently, we could not associate changes in days worked with the sector in which they were worked or accurately calculate wage income for the past year.
- 5 There may be opportunities to improve data entry accuracy by allowing enumerators to use Arabic numerals or adjusting the data entry interface.
- 6 A survey designed to match member line numbers across surveys (by giving enumerators copies of the numbered list of household members from the baseline survey) would have allowed us to look at changes in employment for individuals, rather than household averages.
- 7 The timing of data collection was considerably complicated by the political situation in Yemen. We note that, ideally, the expost survey would have been administered at the same time of year as the baseline survey, avoiding seasonal changes in consumption and employment. When looking specifically at relatively short term effects, it would also be preferable to assure that projects are the same stage of completion in the various communities at the time of the expost survey, rather than some having been completed several months previously and others being still in progress. Alternatively, if project timing could be verified to be randomly assigned assigned and the sample size slightly increased, the analysis could control for this variation directly and measure project impacts at different points in time. If neither of these approaches are feasible, an alternative assessment strategy could be considered.

Appendix VI: Regression Tables from Report Using OLS instead of IV

Table 6.19: LIWP Impact on Probability of Being in Debt or Repaying Debt

	In debt	Paid off some	Paid off all	Pawned	Sold
LIWP Program	0.06	-0.03	0.03	-0.05	-0.03
	(0.05)	(0.08)	(0.03)	(0.04)	(0.06)
Expost	0.03	0.20***	-0.01	0.04*	0.18***
	(0.03)	(0.04)	(0.02)	(0.02)	(0.04)
Fixed effects					
N	1906	1908	1908	1908	1900

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.9: LIWP Impact on Household Total Days Worked Last Month

	Days/month	Days/month	Days/month	Days/month
LIWP Program	1.662	1.662	1.586	1.586
	(2.991)	(2.991)	(3.626)	(3.626)
Expost	-8.351***	-8.351***	-8.351***	-8.351***
	(2.386)	(2.386)	(2.386)	(2.386)
Active in past month			0.148	0.148
			(3.620)	(3.620)
Fixed effects	Comm	HH	Comm	HH
N	1908	1908	1908	1908

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions exclude days in self-employed work.

Table 6.10: LIWP Impact on Household Total Days Worked Last Year

	Days/year	Days/year	Days/year	Days/year
LIWP Program	47.808	47.808	35.665	51.905
	(31.898)	(31.898)	(38.625)	(34.630)
Expost	-17.980	-17.980	151.092***	-110.562***
	(22.093)	(22.093)	(26.687)	(23.532)
Fixed effects	HH	HH	HH	HH
N	1908	1908	726	1254

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions exclude days worked in self-employed agriculture. Third column includes only households with baseline days worked per adult greater than 60; fourth column includes only households with baseline days per adult less than or equal to 60.

Table 6.11: Household Wage Income

	Wage Income					
	month	month	month	month	year	year
LIWP Program	5080.8	5080.8	4122.8	4122.8	99520.0*	60624.3
	(4396.3)	(4396.3)	(5577.5)	(5577.5)	(56628.8)	(51533.4)
Expost	-11051.5***	-11051.5***	-11051.5***	-11051.5***	-77410.6**	-91971.0***
	(3225.2)	(3225.2)	(3225.2)	(3225.2)	(37154.4)	(34676.8)
Active in past month			1859.5	1859.5		
			(5995.9)	(5995.9)		
Fixed effects	Comm	HH	Comm	HH	Comm	HH
N	1908	1908	1908	1908	1908	1908

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.12: Regression Test for Program Effect on Household Total Wage Income

Table 6.13: Summary Statistics on the Employment Rate

	All	Male	Female
Control Baseline	0.356	0.643	0.0925
Control Expost	0.360	0.648	0.116
Treatment Baseline	0.332	0.657	0.0342
Treatment Expost	0.390	0.719	0.114
Observations	6314	2956	3358

The last three columns treat employment in self-employed agriculture as unemployment to control for change in reporting between baseline and expost.

Table 6.14: LIWP Impact on School Enrollment

	male age 5-14	male age 15-25	femal age 5-14	female age 15-25
LIWP Program	0.020	0.026	0.011	-0.016
	(0.036)	(0.050)	(0.037)	(0.020)
Expost	0.058***	-0.025	-0.009	0.016
	(0.022)	(0.038)	(0.027)	(0.014)
Observations	2265	1215	1985	1403

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Proportion of Children Enrolled in School by Age and Gender

Table 6.15: LIWP Impact on Wages Level by Sector

	LIWP Program	Expost
Governmental	156.1	150.3
	(236.6)	(206.9)
Private agriculture, unskilled	31.05	182.1**
	(162.0)	(77.76)
Private construction, skilled	550.5	481.9
	(526.7)	(420.7)
Private construction, unskilled	778.7**	-387.1*
	(312.0)	(231.5)
Private other, skilled	212.1	-72.07
	(397.1)	(319.6)
Private other, unskilled	646.5	-492.9
	(581.0)	(574.5)
Self-employed, unskilled	213.2	161.5
	(339.8)	(177.9)
Self-employed, skilled	-1.842	-331.7***
	(150.5)	(95.16)
Fixed effects	Comm	Comm
N		

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.16: Includes community fixed effects.

Table 6.17: LIWP Impact on Average Wages and Skill Level

	Avg. Wage	Avg. Wage	Avg. Wage	Avg. Wage	Avg. Wage	Avg. Skill
LIWP Program	252.679**	252.679**	151.262	151.262		-0.112
	(105.867)	(105.867)	(120.663)	(120.663)		(0.071)
Expost	-164.009*	-164.009*	-164.009*	-164.009*		-0.200***
	(86.929)	(86.929)	(86.948)	(86.948)		(0.042)
Active in past month			198.108*	198.108*		0.022
			(118.791)	(118.791)		(0.076)
Avg. Skill					277.359***	
					(82.650)	
Fixed effects	Comm	HH	Comm	HH	Comm	Comm
N	2238	2238	2238	2238	2238	4959

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Average daily wages are calculated per worker based on days worked at different jobs in the past month. For the fourth column, unit of observation is the job.

Table 6.18: LIWP Impact on Income Sources

	Employment	Ag. Production	Rent	Charity	Remit. Yemen	Remit. Abroad
LIWP Program	0.012	0.014	0.000	-0.094*	0.007	-0.028
	(0.048)	(0.059)	(0.016)	(0.055)	(0.032)	(0.035)
Expost	-0.180***	0.284***	0.028**	0.258***	0.048**	0.028
	(0.034)	(0.048)	(0.012)	(0.039)	(0.023)	(0.027)
Fixed effects	HH	HH	HH	HH	HH	HH
N	1908	1908	1908	1908	1908	1908

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regression test for program effect on share of households receiving income from different sources.

Table 6.20: LIWP Impact on Household Debt and Value of Assets Sold

	Total Debt	Paid Off	Outstanding Debt	Assets Sold
LIWP Program	-48.02	10.76**	-20.13*	-6.93
	(48.06)	(4.83)	(10.36)	(9.06)
Expost	56.94	5.99*	26.09***	12.18*
	(46.54)	(3.30)	(8.66)	(7.26)
Fixed effects	Comm	Comm	Comm	Comm
N	1908	1908	1908	1908

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regression Test for Program Effect on Household Debt and Value of Assets Sold. Values are measured in thousands of riyal. (1000 riyal= $4.70)

Table 6.21: LIWP Impact on Value of Household Durables Owned

	Taxi/ bus =750 b/se	Excluding taxi/bus b/se	Taxi/ bus =500 b/se	Taxi/bus =1000 b/se
LIWP Program	27.13*	-2.73	17.18	37.08*
	(15.57)	(4.02)	(11.12)	(20.11)
Expost	-1.70	12.94***	3.18	-6.58
	(12.77)	(2.82)	(9.02)	(16.58)
Fixed effects	Comm	Comm	Comm	Comm
N	1908	1908	1908	1908

Value measured in thousands of riyals

Table 6.22: LIWP Impact on Livestock Assets Owned

	Cattle		Donkeys		Sheep or Goats		Chickens	Beehives
	own b/se	share b/se	own b/se	share b/se	own b/se	share b/se	own b/se	own b/se
LIWP Program	0.03	0.01	0.02	0.00	0.13	-0.20	0.15	-0.76
	(0.09)	(0.03)	(0.04)	(0.01)	(0.83)	(0.22)	(0.33)	(0.57)
Expost	0.11**	-0.04**	0.01	-0.01	0.82	0.07	0.60***	0.24
	(0.05)	(0.02)	(0.03)	(0.01)	(0.51)	(0.18)	(0.19)	(0.42)
Fixed effects	HH	HH	HH	HH	HH	HH	HH	HH
N	1890	1908	1906	1908	1908	1908	1906	1908

LIWP impact on average number of animals per household.

Table 6.23: LIWP Impact on Share of Households Benefiting from Animals

	Cattle		Donkeys		Sheep or Goats		Chickens	Beehives
	own	share	own	share	own	share	own	own
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
LIWP Program	-0.01	-0.00	0.04	0.00	-0.07*	-0.02	-0.04	0.04
	(0.04)	(0.02)	(0.03)	(0.01)	(0.04)	(0.02)	(0.02)	(0.05)
Expost	0.04*	-0.02	-0.02	-0.01	0.09***	-0.01	0.01	0.02
	(0.03)	(0.01)	(0.02)	(0.01)	(0.03)	(0.01)	(0.02)	(0.03)
Fixed effects	HH	HH	HH	HH	HH	HH	HH	HH
N	1908	1908	1908	1908	1908	1908	1908	1908

LIWP impact on the extensive margin. Includes both animals owned individually and animals owned in partnership.

Table 6.24: LIWP Impact on Number of Animals from Among Households with Animals at Baseline

	Cattle		Donkeys		Sheep or Goats		Chickens	Beehives
	own	share	own	share	own	share	own	own
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
LIWP Program	-0.03	-0.05	-0.03	-0.00***	0.43	-2.47	-0.04	-8.56
	(0.16)	(0.16)	(0.07)	(0.00)	(1.42)	(2.37)	(0.63)	(6.34)
Expost	-0.21**	-0.81***	-0.23***	-1.00***	-0.52	-1.30	-0.19	-1.87
	(0.09)	(0.12)	(0.05)	(0.00)	(0.91)	(2.21)	(0.34)	(4.16)
Fixed effects	HH	HH	HH	HH	HH	HH	HH	HH
N	578	184	692	26	960	156	682	136

LIWP impact on the intensive margin. Includes both animals owned individually and animals owned in partnership

Table 6.25: LIWP Impact on Expenditure on Non-Food Consumption in Past Month (Continued)

	Qat	Tobacco	Medicine	Clothes	Bedding	Housing and Kitchen
LIWP Program	-805.3	-434.7**	8606.5	-2939.2	-908.8	498.7
	(1279.8)	(212.4)	(9571.7)	(4725.4)	(1202.4)	(526.6)
Expost	4334.5***	349.8**	-1627.5	4978.7	505.2	-727.7**
	(999.2)	(161.8)	(7133.2)	(3741.8)	(806.8)	(352.8)
Fixed effects	HH	HH	HH	HH	HH	HH
N	1910	1910	1910	1910	1910	1910

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Expenditure measured in riyal (212 riyal= $1)

	Shortage	Adults skip meal	All skip meal	Loan	Less variety	Store credit
LIWP Program	-0.005	0.028	-0.027	-0.032	-0.002	0.064
	(0.066)	(0.067)	(0.035)	(0.061)	(0.049)	(0.079)
Expost	0.052	0.009	0.069***	0.028	0.152***	-0.015
	(0.054)	(0.052)	(0.025)	(0.046)	(0.035)	(0.058)
Fixed effects	Comm	Comm	Comm	Comm	Comm	Comm
N	1908	1908	1908	1908	1908	1908

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.26: Regression test for program effect on self reported food shortage and coping strategies

Table 6.27: LIWP Impact on Calorie Consumption Per Capita

	(1) calories	(2) calories	(3) calories	(4) ln(calories)	(5) ln(calories)	(6) ln(calories)
LIWP Program	377.71*	466.69**	373.55**	0.16**	0.16**	0.14**
	(214.07)	(206.10)	(178.83)	(0.07)	(0.07)	(0.06)
Expost	-310.51**	-361.24**	-269.94**	-0.02	-0.04	-0.03
	(140.90)	(148.73)	(128.31)	(0.05)	(0.05)	(0.04)
Fixed effects	HH	HH	HH	HH	HH	HH
N	1864	1836	1800	1864	1836	1800

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.28: LIWP Impact on Consumption of High Value Foods

	Meat	Chicken	Fish	Eggs	Tuna	Milk	Honey
LIWP Program	0.101	-0.068	-0.114	0.388	-0.226	-0.654	0.318
	(0.237)	(0.244)	(0.461)	(0.430)	(0.351)	(1.169)	(0.347)
Expost	2.612***	-0.529***	-0.214	-0.571*	-0.240	2.588***	0.455*
	(0.170)	(0.175)	(0.332)	(0.309)	(0.252)	(0.840)	(0.250)
Fixed effects	HH	HH	HH	HH	HH	HH	HH
N	2071	2071	2071	2071	2071	2071	2071

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6.29: LIWP Impact on Consumption of High Value Food Controlling for Survey Date

	Meat	Chicken	Fish	Eggs	Tuna	Milk	Honey
LIWP Program	-0.076	-0.127	0.086	0.298	-0.337	-1.093	0.177
	(0.230)	(0.249)	(0.467)	(0.439)	(0.358)	(1.194)	(0.354)
Expost	-6.429***	-1.413	8.500***	-1.687	-1.585	-7.966	-4.167**
	(1.273)	(1.381)	(2.592)	(2.437)	(1.986)	(6.621)	(1.965)
Days since Eid	0.055	0.088	-0.113	0.142	0.177**	0.525*	0.129
	(0.057)	(0.062)	(0.116)	(0.109)	(0.089)	(0.296)	(0.088)
Days since Eid sqrd	-0.004***	-0.001	0.004**	-0.002	-0.003	-0.010*	-0.003*
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.006)	(0.002)
Fixed effects	HH	HH	HH	HH	HH	HH	HH
N	2071	2071	2071	2071	2071	2071	2071

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$