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Economic Impacts of the Labor Intensive Works Program in Yemen

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by

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Abstract

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This dissertation examines the impacts and functioning of a community led public works intervention in Yemen, the Labor Intensive Public Works Program (LIWP). LIWP was designed using the "twin-track" approach of combining short-term relief with long-term investments. The program transfers funds to poor rural households by creating temporary employment opportunities in projects that benefit the local community. Income from program wages provides short term protection against negative consumption shocks, while the public works projects themselves provide medium to long term benefits for the community. The first chapter describes an unexpected effect of the intervention on decreasing local prices and explains how this effect can occur as a result of the existing informal institution of store credit in rural communities. The second chapter examines heterogeneity in program impacts related to the type of project chosen by the community and the correlates of this choice. The third chapter provides a detailed evaluation of the impact of the LIWP intervention on households in communities where projects took place.

In Chapter 1, I show the surprising empirical finding that in villages with random assignment to participate in LIWP, the price of staple goods in local stores rose less over time, and connect this to the institution of store credit in isolated villages. I find that there is an increase in prices in communities with 5 or more stores, and a significant relative decline of about 10% in villages with 4 or fewer stores. I develop a simple model of credit as insurance based on qualitative interviews in the field which shows that villagers are willing to pay a premium to local store owners over a more distant and anonymous market because their local store owner can be relied on to let them buy on credit in the future. In this way, the cost of zero-interest credit is internalized in prices charged, however this informal contract is only sustainable if there are sufficient dynamic incentives, which will be more likely in less competitive markets, consistent with the empirical findings.

In Chapter 2, which is based on joint work with Alain de Janvry and Elisabeth Sadoulet, we examine the choice of project by communities in the LIWP intervention. While LIWP was designed with the short-term purpose of providing cash for work to underemployed villagers, the community as a whole decided on the type public infrastructure to be constructed using

the labor paid for by LIWP. We characterize the various projects in terms of skill intensity based on administrative data about relative pay receipts to unskilled and skilled workers and show that the skill-intensity of project choice is correlated with demographic features of the village which imply greater political power for older men. This result indicates that the choice of projects by the community is influenced by the relative distribution of benefits. To identify the channel at play, we show that in the immediate aftermath of the 2011 economic and political crisis in Yemen, there was a higher preference for skill-intensive projects. This is consistent with a model in which older, skilled men who prior to the crisis would not have self-selected as participants in the LIWP program due to the relatively low wages paid compared to alternative employment options, became potential participants due to the crisis. As a consequence, they pushed for skilled labor-intensive projects in which they could capture a larger share of the benefits.

In Chapter 3, which is based on joint work with Alain de Janvry, Elisabeth Sadoulet, and Daniel Egel, we use a randomized control trial to measure improvements in household welfare brought about by the program intervention. We find positive program impacts on household income and durable good ownership, reflecting the short term benefits of wages from work in the LIWP project. We also find surprisingly large program effects on decreasing the average outstanding debt. By analyzing the calorie content of staple grains consumed per person, we find a program impact equivalent to 11-13% higher calorie consumption in treated compared to untreated communities. We also summarize community perceptions of the project benefits and find that completed water related projects reduced water scarcity and fetching time.

To the staff of the Yemen Social Fund for Development who are struggling to continue their work amidst a deadly bombing campaign

Contents

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Chapter 1

Store Credit as Informal Insurance in Rural Yemen

Summary

While a common concern about cash transfers is that the increase in average income will cause inflation in shallow markets, a randomized antipoverty intervention in Yemen shows the opposite pattern. Demand for staple goods increased in treated villages, but the treatment effect on local prices was negative. This counter-intuitive result can be explained as the result of interlinked transactions of insurance and food purchases in non-competitive markets, where insurance takes the form of store credit with flexible repayment.

1.1 Introduction

In various developing country contexts, local shops allow customers to buy consumption goods on credit. If storekeepers take into consideration the economic situation of their customers when deciding how generous to be with the provision of credit, this implicit contract with the shopkeeper takes on an insurance function for customers.

Customers exchange willingness to pay a markup in prices at their local shop for the confidence that they will be able to meet their consumption needs by running up debt during periods when they have low income. I model an implicit contract between the store and the customer in which future access to credit is conditional on income as well as past repayments of debt. When the demand for insurance falls due to higher expected income in the future, the store must drop its prices to keep the implicit contract attractive.

I use this model to explain my primary empirical finding, which is the surprising result that in randomly assigned treatment villages, incomes and demand for staple goods increased but there was a large and significant negative effect on local prices. In particular, the negative price effect occurred in treatment villages with few stores. This is predicted by our model of credit as insurance, since stores that are faced with less competition can offer a more state-dependent insurance contract without worrying that customers with high incomes will default on the contract, because the customers fear losing access to any credit in the future. When faced with a positive income shock, therefore, the terms of the contract must change via dropping the price in order to keep customers from switching to purchasing at the lower price at the market. I contrast the model of credit as providing insurance with a model of credit as a source of precautionary savings only, in which a temporary change in income does not increase the risk of default, so the effect of a positive income shock would be limited to the expected inflation from an increase in demand being higher in non-competitive markets.

A supplementary data set from the same context supports the main findings. Using the supplementary data set, I show that shopkeepers self-reported behavior is consistent with a model where credit has an insurance function, and confirm that there is temporal pattern of prices falling during the program and then rising after the program. Finally, using both sets of data, I estimate the magnitude of the effect of the income shock on debt repayment and on prices, as a back-of-the-envelope calculation of the degree to the system of credit as insurance redistributes income received from the social safety net program within the village.

Previous Literature

This insurance-based prediction is in contrast to the ordinary intuition about the effect of cash transfers on local prices. Cunho, De Giorgi, and Jayachandran in an upcoming paper discuss the equilibrium effects of cash versus in-kind transfers on local prices and show that cash transfers caused higher prices in villages in Mexico. As expected from simple comparative statics, the in-kind transfers shift the supply curve, causing the price to drop, while cash transfers shift the demand curve outwards for normal goods, resulting in higher local prices.

Theoretically, this paper fits into the literature on informal arrangements that provide insurance functions for risk-averse households in contexts missing a formal market for insurance and interlinked transactions more generally. Notably, the insurance function of informal store credit for buying staple goods in Yemen is similar to the insurance function found in Udry's analysis of informal inter-household credit in Northern Nigeria. In contrast to models of mutual insurance with limited commitment as in Ligon, Thomas, and Worrall (2002) however, there is an important asymmetry where the store is risk neutral leading to a contract that needs to be sustained only from the point of view of keeping the customer from defaulting.

Theoretically, the existence of any kind of mutual insurance system should attenuate the effect of a positive income shock, with the gains spread through the system instead of focused on specific individuals. This is a policy relevant question in measuring the targeting effectiveness of social programs. This type of spillover via transfers and credit markets was examined in Progresa by Angelucci and De Giorgi (2009). They find substantial spillovers (11% of program transfers) to non-eligible households via inter-households transfers and borrowing. In the model presented below where mutual insurance is provided through store credit, the observed decline in local prices is a spillover effect that provides modest immediate measurable benefits to non-participants in the LIWP program

Data

The data used come from two surveys related to a workfare program in Yemen. LIWP (Labor Intensive Works Program) is a cash-for-work employment program run by the Yemen Social Fund for Development. LIWP provides short term work opportunities in poor communities in the construction of labor intensive community infrastructure projects such as repairing roads and constructing rainwater harvesting systems. In treated villages, approximately 74% of households have a member participate in the program, and participants receive an average payment of approximately $450 for two months of work. In the absence of the intervention, average monthly income per household in the target villages was about $160.

The first data source is a household survey conducted as part of an impact evaluation of the LIWP intervention. In 2010, half of a set of eligible communities were assigned to receive treatment in the second wave of the program, with the remainder deferred until the third wave. This randomized assignment provides exogenous variation in household income. Baseline household and community surveys were collected prior to the start of the interventions in May 2010, and the expost data were collected during or after the program had ended in October/ November 2011. The intervening months fell in a period of major economic and political crisis in Yemen, with rising prices for staple goods and petrol and and reduced opportunities for casual labor that was the main form of employment in the targeted villages.

The second data source is a survey of shopkeepers in villages with LIWP interventions collected in August 2013. Shopkeepers were asked specifically about debt levels and publicly known shocks experienced by 5 randomly selected LIWP participants and 5 non-participants from among their customers. While the political situation had stabilized, there has been only limited economic recovery since 2011.

1.2 Negative Price Effect

To motivate interest in the use of store credit as a form of insurance, I show show the surprising finding that the program, which increased average incomes, had a significant negative effect on prices for staple goods. While prices rose throughout Yemen, the increase was significantly lower in communities treated by the LIWP program. Data on prices are taken from from the household survey which had an average of 12 respondents per community. Households were asked for the price of a 50kg sack of flour in the village. Wheat flour, eaten in bread and dumplings, is the staple calorie source in Yemen, so the price of flour is an important indicator for food security. It is also primarily imported, so price variations are determined by international markets plus retail markups. I argue that this negative effect follows from decreases in store markup due to the interlinked transactions purchases and consumption insurance.

Table 1.1 shows the effect of the LIWP program on the price of wheat. Identification comes from the randomized assignment to LIWP treatment and double differences to control for any differences in baseline levels. The estimating equation is $d_{it} = Expost_t + LIWP_t\beta + FE_i + \epsilon_{it}$. We split the sample by number of stores per community, to focus specifically on the price effect in isolated, rural markets. The coefficient on LIWP treatment is the program treatment effect, while the coefficient on Expost is the time trend. As can be seen, there was a dramatic increase in prices of about 1800 riyal between baseline and expost, and in LIWP villages with fewer than 5 stores, a relative decline of 400-500 riyal. The average price of a sack of wheat at baseline was 4241 riyal (approximately $20), so this represents was a 42% increase in prices in all villages, which was reduced to about 32% in villages with LIWP programs.

1.3 Characteristics of Store Credit in Yemen

Selling on credit is an important feature of the market for staple goods in rural Yemen. Shopkeepers in small villages know their customers individually and are willing to sell on credit to customers they trust.

Due to an Islamic prohibition, there is never openly interest charged on the outstanding debt. When a question about interest on shop credit was asked in the most recent household budget survey, over 99% of households responded that there was no interest charged, and local consultants suggested that it was not worth including this question in the shopkeeper survey, as there is such as strong stigma against charging interest on debt. [1]

In spite of the prohibition on interest, most households in our sample population report taking credit from shopkeepers. Debt owed to storekeepers represents a major share of total credit owed by households in Yemen. According to data from the 2005-2006 Household Budget Survey, the majority of debts reported by households were to friends and relatives (65% of all debts by value). Almost all other debts reported, however, were owed to storekeepers and traders. Debts to storekeepers and traders represented 28% of the number of debts and 21% of value of all debts. In the LIWP household survey, which represents specifically households in poor villages, 73% of households with any debt reported that debt was owed to a shopkeeper. To give a sense of the magnitudes of the credit in question, the average household in our sample had a baseline debt from all sources of 93,451 riyal ($441), compared to an average monthly income of 35,313 riyal ($167). While the LIWP data set does not break down debt by source, in the shopkeeper survey, shopkeepers reported average current debt

[1] Microfinance organizations and formal bank loans used for purchasing capital assets do charge interest for credit, however these loan contracts are often structured to accord with religious requirements by making interest indirectly present in the structure of a rent-to-buy contract. For consumption loans, this type of loan contract is not possible, and it can be assumed that consumption smoothing using interest based loans is excluded from the household choice set.

among randomly selected active customers was 15,677 riyal ($74). This is slightly less than the cost of a monthly bundle of staple commodities sufficient for an average sized households (16,170 riyal ($76)). On average, 10,909 riyal ($51) of the current debt represents “fixed debt” - debt that was accumulated in the past and has not been repaid for multiple months. In addition, on average shopkeepers reported that 18% of all of their customers were no longer allowed to actively receive credit, due to having exceeded the maximum debt limit. Among these customers with “bad debt,” the shopkeepers reported an average amount of bad debt of 25,147 riyal ($118). While the average level of bad debt is greater than the average level of current debt, many individual consumers had current level higher than the average level of bad debt, showing that the determination of when the level of debt becomes “bad” is relative to individual characteristics of the customer.

In local interviews, storekeepers and customers described an understanding of mutually understood maximum debt balance which varies by customer depending on ability to repay and confidence in repayment. If the running debt balance reaches the maximum level without repayment, the debt is denoted as “bad debt” and no further credit is allowed until debt is repaid.

From the point of view of the storekeepers, the system of selling on credit is costly. Over the past 15 years, Yemen has experienced average inflation of 10%, increasing to 20% in the last two years, so even in the short-term non-interest bearing loans represent a substantial loss for the creditor. The cost of providing short term credit is increased by the risk of default, as even customers viewed as trustworthy may experience long-term negative income shocks and be unable to repay their debts to the storekeeper, resulting in “bad debt” that may never be repaid. Correspondingly, prices are generally higher in local shops that allow purchases on credit compared to the more anonymous and distant market where customers would need to buy on cash.

1.4 Model

To clarify the view of store credit as insurance and the mechanism by which the use of store credit can create an inverse relationship between prices and income, I propose a two-period principal-agent model in which the store maximizes profit by offering an insurance type contract and must adjust the parameters of this contract in response to an increase in average incomes.

Customers draw income in each period from a common distribution with the support y_L to y_H. Customer income draws are independent. The preponderance of idiosyncratic rather than aggregate shocks is realistic in this context as most income is from informal day labor or remittances, with migrants often crossing the border to work in Saudi Arabia for short periods until they are expelled. I will model expected aggregate income shocks as rightward shift by ω in the distribution of possible incomes.

There is a single consumption good and customers can choose between purchasing this good at an anonymous market, or at a store where their first period behavior effects their purchasing ability in the second period. The customer makes the choice about where to purchase after seeing his income realization in the first period, but while his second period income realization is unknown. The customer also chooses in each period how much of his income to spend and how much to save. All customers have identical CARA utility functions $u = -e^{-\gamma c}$

There are two basic types of implicit contract that the customer could be offered at a store. In the savings only credit contract, the amount of credit offered in the second period by the store depends only on the actions by the customer in the first period, while in the insurance type contract, the amount of credit offered in the second period is also conditional on the second period income draw. Below, subscripts nc, sc, and ic are used below respectively to refer to customer choices under no contract, a savings only contract, and an insurance contract.

Customer's Saving Problem

The customer always has the alternative of buying at the market, so the store needs to offer a contract with expected utility at least as high as the customer would receive without the contract.

Without purchasing at a store with an implied contract, the customer's consumption in each period will be:

$$\begin{aligned} c_1 &= y_1 - s \\ c_2 &= y_2 + s \end{aligned} \tag{1.1}$$

His optimal choice of savings in the first period is:

$$s^*_{nc} = \frac{1}{(1+r)} y_1 + \frac{1}{(1+r)\gamma} ln(r \frac{u(y_H + \omega) - u(y_L + \omega)}{(y_H - y_L)}) \tag{1.2}$$

And correspondingly his expected utility is:

$$EU(s^*_{nc}) = u(\frac{1}{(1+r)} y_1)(r \frac{u(y_H) - u(y_L)}{(y_H - y_L)})^{\frac{r}{1+r}} + \frac{1}{\gamma}(\frac{u(y_H) - u(y_L)}{(y_H - y_L)}) u(\frac{1}{2} y_1)(r \frac{u(y_H) - u(y_L)}{(y_H - y_L)})^{\frac{1}{1+r}} \tag{1.3}$$

Savings Only Credit Contract

In a savings only credit contract, the store sells with markup multiplier m, and the customer can invest in reputation with the store by buying at the markup and/or by repaying outstanding debt by the amount p. (Since our model only has two periods, I assume that all customers start with sufficient outstanding debt that they are not constrained by an upper bound on the amount of debt that they can repay.) In the second period, the store rewards customers with good reputation by allowing them to buy more on credit. The parameter α represents the degree to which the store responds to investment in reputation with expanded credit.

The customer's consumption is:

$$
\begin{aligned}
c_1 &= \frac{1}{m}(y_1 - s - p) \\
c_2 &= y_2 + \alpha(1 - \frac{1}{m})(y_1 - s - p) + \alpha p + rs
\end{aligned}
\tag{1.4}
$$

The second period consumption consists of second period income, plus a credit reward offered by the store which is proportional to the amount by which the store benefited from the customer's choices in the first period- the amount of markup paid and the amount of debt repaid.

It is intuitive and straightforward to demonstrate that if $\alpha > r$ then $\frac{dU}{dp} > \frac{dU}{ds}$ for all parameter values, while conversely, if $\alpha < r$ then $\frac{dU}{dp} < \frac{dU}{ds}$, so the store contract here is simply acting as an indirect form of savings which perfectly substitutes for the strategy of saving between periods if the implicit interest is equal. Because villagers have extremely limited access to formal financial services, no ability to lend money at positive interest rates, and cash savings are vulnerable to claims by family members as well as losing value due to inflation, it is reasonable to imagine repaying store credit in return for future access to credit having a higher implied "‘interest"’ rate than alternative forms of savings.

$$
p^*_{sc} = (\frac{\alpha - 1 - \alpha m}{1 + \alpha})y_1 + \frac{m}{\gamma(1 + \alpha)} ln(\alpha \frac{(u(y_H) - u(y_L))}{(y_H - y_L)})
\tag{1.5}
$$

The store maximizes profit by minimizing α, but due to competitive pressure from the market, customers will only accept to come to the store if $\alpha \leq r$, so in equilibrium $\alpha = r$. For simplicity, I assume below that $r = \alpha = 1$.

The customer is indifferent between buying at the store or buying at the market, since the amount transferred from period 1 to period 2 is the same whether it is transferred via savings or via investment in reputation: it is equal to p^*_{sc} plus the foregone consumption from paying the markup in period 1: $(m - 1)\frac{1}{m}(y_1 - p^*_{sc})$. (The store contract may be preferred to a savings strategy for other reasons such as providing a commitment device which are sufficient to allow explain the existence of this form of contract in equilibrium.) Using the model to make this more concrete, we can show that the amount transferred with the savings contract (s^*_{sc}) equals the amount of savings with no contract (s^*_{nc}):

$$
\begin{aligned}
s^*_{sc} &= (m-1)\frac{1}{m}(y_1 - p^*_{sc}) + p^*_{sc} \\
s^*_{sc} &= (1-\frac{1}{m})y_1 + (\frac{1}{m})\left((\frac{2-m}{2})y_1 + \frac{m}{2\gamma}ln(\frac{(u(y_H)-u(y_L))}{(y_H-y_L)})\right) \\
s^*_{sc} &= \frac{1}{(1+r)}y_1 + \frac{1}{(1+r)\gamma}ln(r\frac{u(y_H)-u(y_L)}{(y_H-y_L)}) \\
s^*_{sc} &= s^*_{nc}
\end{aligned}
\tag{1.6}
$$

1.5 Testable Predictions of Savings Only Contract

The model of a savings only credit contract predicts increases in current income (y_1) lead to an increase in debt repayment in the first period as long as $m < \frac{1+r}{r}$. For $r = 1$, this implies that the markup over market prices is less than 100%, which is a reasonable assumption and more than covers observed values in the data.

$$
\frac{dp^*}{dy_1} = 1 - \frac{rm}{1+r} \tag{1.7}
$$

Conversely, repayment decreases with increase in expected second period income (ω):

$$
\begin{aligned}
p^* =& (\frac{\alpha+1-\alpha m}{1+\alpha})y_1 + \frac{m}{\gamma(1+\alpha)}ln(\alpha\frac{(u(y_H+\omega)-u(y_L+\omega))}{(y_H-y_L)}) \\
\frac{d(u(y_H+\omega)-u(y_L+\omega))}{d\omega} &< 0 \Rightarrow \frac{dp*}{d\omega} < 0
\end{aligned}
\tag{1.8}
$$

However, because the contract is designed to return transfers to every customer in the second period, there is no change in store profits (always = 0) or incentive to change the level of markup as a result of aggregate income shocks.

Credit as Insurance Contract

The insurance contract is similar to the savings only contract, with the addition of a final term that gives more access to credit for customers with below average incomes, and less access to credit for customers with above average incomes. For simplicity, we ignore the possibility of savings, since we have already seen that it is completely equivalent to investing in reputation at the store.

Consumption in each period is:

$$
\begin{aligned}
c_1 &= \frac{1}{m}(y_1 - p^*) + \frac{\beta}{m}(\overline{y} - y_1) \\
c_2 &= y_2 + (\alpha - \frac{\alpha}{m})y_1 + \frac{\alpha}{m}p^* + \frac{\beta}{m}(\overline{y} - y_2)
\end{aligned}
\tag{1.9}
$$

The customer chooses at optimal level of debt repayment:

$$p^* = (1 - \frac{m+\beta}{2})y_1 + \frac{m}{2\gamma} ln(\frac{u(y_H(1-\frac{\beta}{m})) - u(y_L(1-\frac{\beta}{m}))}{(y_H - y_L)}) \tag{1.10}$$

We can show that expected utility with the insurance contract is greater than the expected utility for the savings only contract (see proof in appendix). So in the long term customers prefer an implict contract though maintaining their reputation with a shopkeeper.

$$EU_{ic} = 2u(\frac{m-\beta}{2m}y_1 + \frac{\beta}{m}\overline{y})(\frac{u(y_H(1-\frac{\beta}{m})) - u(y_L(1-\frac{\beta}{m}))}{(y_H - y_L)})^{\frac{1}{2}} \tag{1.11}$$

$$EU(p^*_{ic}) > EU(p^*_{nc}) \tag{1.12}$$

However, utility with the insurance contract is not greater than utility from savings only in every case. For example, if $y_1 = y_2 = y_H$, then the customer would have been better off with a savings strategy.

Testable Predictions of Credit as Insurance Contract

As y_1 increases, it is less likely that the customers prefer the insurance contract to buying at the market:

$$\begin{aligned} &\frac{d}{dy_1}(EU_{ic} - EU_{nc}) = \\ &= \frac{d}{dy_1}\left(2u(\frac{m-\beta}{2m}y_1 + \frac{\beta}{m}\overline{y})(\frac{u(y_H(1-\frac{\beta}{m})) - u(y_L(1-\frac{\beta}{m}))}{(y_H - y_L)})^{\frac{1}{2}} - 2u(\frac{1}{2}y_1)(\frac{u(y_H) - u(y_L)}{(y_H - y_L)})^{\frac{1}{2}}\right) \\ &= (-1 + \frac{\beta}{m})\gamma u(\frac{1}{2}y_1 - \frac{\beta}{2m}y_1 + \frac{\beta}{m}\overline{y})(\frac{u(y_H(1-\frac{\beta}{m})) - u(y_L(1-\frac{\beta}{m}))}{(y_H - y_L)})^{\frac{1}{2}} + \gamma u(\frac{1}{2}y_1)(\frac{u(y_H) - u(y_L)}{(y_H - y_L)})^{\frac{1}{2}} \\ &< 0 \end{aligned} \tag{1.13}$$

This holds for any values of $m > 1$, $\beta \leq 1$.

So, reasonably, higher current income always makes customers more likely to switch to purchasing at the market in the short term. The share of customers who switch to purchasing at the market depends on risk aversion (γ). But since the store has negative profit if any high current income customers constantly switch to going to the market, this contract can only be feasible if there are sufficient dynamic incentives that customers with high incomes in the current period do not switch to purchasing at the market.

The theoretical suggestion of dynamic incentives is supported by anecdotal evidence. My colleague reports that in his home village, it was not an infrequent occurrence for a customer who has established credit at a local shop to go buy in cash at another shop or market when he receives some income, rather than repaying his credit at the first shop. If the first shopkeeper becomes aware of this behavior, he would block the customer from extending his credit line any further in the future. For risk-averse customers who lack alternative forms of credit to access basic necessities in case of emergency of emergency, this is a substantial penalty.

Store's problem: Setting m

The store's profit (markup minus wholesale cost of 1 times demand in each period) for customer with income y_1 in period 1 is expressed as:

$$\pi = (m-1)((1-\beta)y_1 - p^* + \beta\overline{y}) + (m-1)(\alpha p^* + \beta\overline{y} - \beta y_1) \tag{1.14}$$

If all customers participate in the contract and there are a sufficient number of customers for the central limit theorem to apply, then $y_1 = \overline{y}$, and $\alpha = 1$:

$$\pi = (m-1)(y - p^*) + (m-1)(p^*) = (m-1)\overline{y} \tag{1.15}$$

Decreasing β decrease the attractiveness of the contract. Since with perfect information, store profit does not depend on β, the store sets β to the maximum value of 1. (In reality, store information about incomes is likely to be imperfect, and β lower than 1).

Store goal is to maximize m such that all customers on average are willing to participate in the contract. Store sets m as high as possible such that the participation constraint is met:

$$2u(\frac{m+1}{2m}\overline{y})(\frac{u(y_H(1-\frac{1}{m})) - u(y_L(1-\frac{1}{m}))}{(y_H - y_L)})^{\frac{1}{2}} > 2u(\frac{1}{2}y_1)(\frac{u(y_H) - u(y_L)}{(y_H - y_L)})^{\frac{1}{2}} \tag{1.16}$$

Increases in markup decrease the attractiveness of the contract:

$$\frac{d}{dm}(EU_{ic} - EU_{nc} = \frac{d}{dm}\left(2u(\frac{1}{2} + \frac{1}{2m}\overline{y})(\frac{u(y_H(1-\frac{1}{m})) - u(y_L(1-\frac{1}{m}))}{(y_H - y_L)})^{\frac{1}{2}} - 2u(\frac{1}{2}y_1)(\frac{u(y_H) - u(y_L)}{(y_H - y_L)})^{\frac{1}{2}}\right) < 0 \tag{1.17}$$

So profit maximizing level of m sets:

$$u(\frac{1}{m}\overline{y})(u(y_H(1-\frac{1}{m})) - u(y_L(1-\frac{1}{m}))) = (u(y_H) - u(y_L)) \tag{1.18}$$

The above equation gives m implicitly as a function of the distribution of second period income.

Expressing second period income as a function of ω and taking the derivative with respect to ω we can show (see appendix) that:

$$\frac{d\omega}{dm} < 0 \tag{1.19}$$

This is our predicted negative effect on prices in response to projected increase in average incomes if the store credit has an insurance role.

Intuitively, as demand for insurance decreases, the store has to change the terms of the contract to keep it attractive compared to buying at the market. The easiest way to make the contract "cheaper" is to reduce the markup, because the implict terms of the contract itself (α and β) are not easily renegotiated in the short term.

Note that unlike a few customers randomly receiving high income, the store cannot credibly threaten to drop all customers if they simultaneously default to the market in the short term during the time when they have less need of future income. So the dynamic incentives in place to maintain the contract in equilibrium are ineffective when all expected incomes rise simultaneously.

Summary of Testable Implications

In the analysis that follows, I consider three possible models of store credit in these villages.

The most restrictive model is to see credit as purely serving a convenience function to reduce the transactions costs of carrying cash at every visit to the store. Instead of an endogenous credit limit serving as an incentive for repayment, there is some external mechanism for enforcing repayment at random times. This implies that the expected value of observed level of credit at any time would be directly related to the level of consumption. Therefore an increase in income increases consumption and therefore average debt levels and negative shocks to available income decrease average debt levels. Aggregate increases in income have the expected positive effect on prices due to increased demand.

The second model is the credit as savings only contract. This model, by contrast, predicts that when current income is high, customers save more via investing in their reputation by repaying debt. There is no predicted effect of a temporary aggregate income shock on prices beyond the normal effect of increased demand and average prices are lower in areas with greater competition.

The third model is the credit as savings and insurance contract, in which credit available is conditional on customer income. In common with the second model, we expect customers to repay more as current income increases, and less as projected future income increases. In addition, due to the store's need to set m to prevent customers from buying at the market in the short term, we expect prices to fall with an increase in predicted future income.

To summarize:

Model	Income Effect on Debt	Income Effect on Price
Convenience	Positive	Positive
Savings only	Negative	Positive
Savings with insurance	Negative	Negative

The results below are most consistent with the third model of credit in areas with few stores (likely dynamic incentives) and with second model in areas with many stores (easier default by individuals). Customers respond to positive income shocks by repaying debt, and prices fall in the short term in response to positive income shocks in areas with few stores.

1.6 Results

Debt Repayment Increases with Transitory Income Gains

There is strong evidence that debt repayment increases with positive shocks to income. This relationship is identified using both the randomly assigned income shock caused by LIWP treatment and using publicly known shocks reported by the storekeeper.

Identification Using LIWP Income Shock

The LIWP intervention was a positive shock to incomes. The program effect on debt repayment is estimated using the equation:

$$Debt_{it} = Expost_t + LIWP_t\beta + FE_i + \epsilon_{it} \tag{1.20}$$

The survey did not specify in this question to whom the debt was repaid, however as noted above, the majority (71%) of households who were in debt at baseline reported that they were indebted to local store owners. Focusing on only households in village-type markets (fewer than 5 stores nearby), the presence of the LIWP program significantly increased the amount of debt paid off in the past year by 20,843 riyal ($98), or about 20% of the average amount borrowed. (See tables 3.41 and 3.84) Similar results but slightly smaller in magnitude are found if the sample to households is limited to that were indebted to the shopkeeper at baseline, showing that the program impact encompassed the repayment of old debts, rather than just the prevention of new debts. (See tables 1.4 and 1.5).

These empirical findings in the LIWP data are supported by self-reported behavior. When asked directly how they spent income from the program, 44% of respondents in the household survey indicated that they used project income for debt repayment. Also, in focus group discussions, both participants and shopkeepers mentioned debt repayment as a notable result of the LIWP intervention.

As far as estimating the magnitude of the effect of income shocks on debt repayment, the LIWP data is less useful. Assignment to LIWP at the village level was randomized, so the average treatment effect of LIWP on debt repayment is identified, however, not all households participated in LIWP and among those who did, there was heterogeneity in program income based on the total number of days worked, skill level of participants, the number of household members who participated. With the caveat that the regression result represents a correlation rather than a causal relationship, I find that approximately 14% of LIWP income went towards repaying debts (table 1.6).

The data in the shopkeeper survey provides a supplementary opportunity to test for the impact of fluctuations of income on debt repayment. By matching the storekeeper records on credit and debt repayment with LIWP payment records, month to month variation in program income can be used to identify the average share of LIWP income devoted to debt repayment. These month to month fluctuations in income from the project are more plausibly exogenous than total project income because 1) the most significant determinant of project income was the number of adult men in the household and their skill level and 2) cash paid per month did not necessarily correspond with earnings for the previous month because payrolls were delivered unpredictably and it often happened that households that missed the delivery in one month picked up their cash the following month.

This analysis suggests that approximately 10% of LIWP income is used for debt reduction. Table 1.7 shows the preferred specification (columns 2-4). As only have four months of credit and repayment data was collected, there is relatively little variation in LIWP income between periods for each customer, but there is still an indication of a negative relationship between income and debt level (column 5). Using purely cross-sectional variation is problematic, however, because clearly customers who participate in LIWP differ systematically from non-participants, and households with high LIWP income due to more days worked or more highly skilled work may be larger or better-off than households with low LIWP income. To address these issues, only LIWP participants are included in (columns 1-4). In addition, in (columns 2-4), the estimation controls for the fixed-debt of the household as an indicator of normal levels of credit and purchasing.

Publicly Known Idiosyncratic Income Shocks

Finally, the shopkeeper survey data allows us to test whether credit levels reflect idiosyncatic publicly known past shocks experienced by the customer. In the shopkeeper survey, shopkeepers were asked to recall negative shocks experienced by customers during the past year (including illness, deaths in family, loss of job, agricultural misfortune, etc.). The shopkeeper survey data thus by necessity focus on those shocks which are public knowledge. Shopkeepers recalled 25% of customers as having experienced at least one shock in the past year, while 7% experienced multiple shocks.

The regression results in table 1.13 show that having a negative shock in the past year increased the average debt level at the end of Ramadan (just prior to data collection) by approximately 8106 riyal ($38) for non-LIWP participants. Having more than one negative shock in the past year did not increase the average debt level beyond having a single negative shock, indicating combination of (perhaps) lower expectations about future earnings plus the existence of a debt limit. In specification (2) the coefficient on multiple shocks is negative, and while the standard error is large, at the maximum it suggests that true coefficient could be slightly positive, and far less than the impact on debt level of the first shock. Participants are excluded because, as will be shown below, the LIWP program income significantly affected their use of credit.

Although the panel structure of our data is limited, there are two months (Rajab and Shaaban) for which we have data from shopkeepers about both debt levels and negative shocks experienced by households within that month, allowing us to control for household fixed effects. This allows the estimation to control for cross-sectional variation in household average income and consumption levels. Again, for non-participants, the experience of a negative shock led to higher debt levels at the end of the month.

That negative shocks increase debt up to a certain point (after which the indirect "savings" of reputation are exhausted) is supported by direct questions to the shopkeepers in our survey about how whether they were asked whether the customer had asked for help in dealing with the shock and if so, how the shopkeeper had responded. (These questions were asked at the conclusion of the survey and separate from recall questions about shocks). In the shopkeeper survey dataset 67% of customers who experienced job loss or illness in family asked for help from shopkeeper (69% of customers who experienced single shock, 54% of customers who experienced multiple shocks). See table (1.17).

Storekeepers reported that 22% of their customers had bad debt, of which 36% cleared their debt during the year, showing that there is a strong incentive for customers to clear bad debt rather than writing it off.

Storeowners also reported that they are highly informed about customers' incomes and ability to repay. 94% of shopkeepers in villages with LIWP interventions in the shopkeeper survey reported that they were aware of which of their customers were participating in LIWP, and on checking with LIWP registration information, they were correct 91% of the time. 65% of shopkeepers claimed to know about labor incomes of their customers and 84% knew about transfers from the Social Welfare Fund.

More Competitive Markets are Correlated with Less Reported Use of Credit as Insurance

The important prediction of the insurance contract model which distinguishes it from the savings contract is that an aggregate income shock leads stores to drop prices to prevent customers from defaulting.

While the model above assumed a single local shop, the number of shops per village in our sample ranges from zero to 14, with a mean of 2.7. In communities with a single shop, dynamic reputational incentives are strongest because customers must return to the shop to request credit in the future with high probability. As the number of shops increases, however, it becomes possible for a customer to default on one shop, but still hope to get credit at another shop. In larger urban and peri-urban areas, it also becomes more difficult for shopkeepers to track customer behavior. For both these reasons, as the number of stores increases, the ability of stores to offer contracts with $\alpha>0$ is limited.

Self-reporting the LIWP household survey indicates that customers rely on store credit during hard times. During the baseline data collection, 66% of households in the LIWP sample reported that they had trouble providing food during the past year. Of these, 61% (39% of all households), listed buying on credit from local shops as a coping strategy for responding to food insecurity. Table 1.8 shows that customers in villages with more stores are less likely to report using shop credit as a coping strategy for food insecurity and more likely to have borrowed from sources other than a storekeeper. This is consistent with a story in which because of the lack of expected repeated interactions, insurance cannot be easily bundled with consumption, so it the insurance role of store credit is substituted by mutual insurance.

From informal interviews with LIWP participants in Al Hodeidah, the use of shop credit differed between village and city. While both sets of participants used shop credit, a conception of shop credit as “insurance” was only reported in isolated villages. Villages mentioned that they bought at the local store in spite of high prices because shopkeepers were “patient” about repayment if they knew that the customer was experiencing a negative shock. One villager mentioned that their shopkeeper would be patient with unpaid credit for multiple years if he trusted the customer, while in the city, respondents said that shopkeepers demanded debt repayment within at most two months and threatened to call the police if debts were unpaid.

Table 1.9 shows how the presence or absence of store credit as insurance affects the debt repayment reaction to shocks. Since customers benefit from an insurance role of credit to the extent that they are allowed to delay repayment, the degree to which the economic crisis allowed more delays or greater accumulation of debt is a proxy for the insurance function of the store credit. We see in the first column (All control) that repayment rates were significantly higher in communities with more stores. The second and third columns show that, in communities with more than 2 stores, the share of debt repaid increased as a result of the crisis, while in communities with fewer than 2 stores, there was no trend towards greater debt repayment in expost except for communities with the LIWP intervention.

In the shopkeeper dataset, the sample is limited to areas with 4 or fewer stores, but we do find a signficant correlation between the probability of customers having bad debt (as reported by shopkeepers) and the the number of stores in the community. (Table 1.10)

Aggregate Income Shock Leads to Lower Prices is Non-Competitive Markets

The insurance contract model predicts that an aggregate income shock leads to lower prices. As shown in the preceding section, markets with fewer stores are predicted to have more of an insurance role for credit, so the negative price effect of aggregate positive income shocks should specifically be evident in smaller markets, which is indeed the primary finding from the beginning of this paper in table 1.1.

The shopkeeper data also show that this effect on prices is temporary, with a drop during the program followed by an increase after the program, as expected future income reverts to normal levels after the LIWP wage payments end. While program timing was not random within the set of treated villages, heterogeneity in the program dates emerged based on factors such as how long it took for the community to agree the construction project, and the technical details of the project itself, which are plausibly exogenous to the price of staple goods in the village. Table 1.17 shows that prices fell during the LIWP program followed by increase after the program.

1.7 Excluding Alternative Explanations

Finally, it is worth considering if alternatives explanations could explain the relative drop in prices in treated villages.

Table 1.19 shows the same regressions as table 1.1, but excluding villages with LIWP programs where there was improvements to roads, to verify that the negative price effect was not driven by a decrease in transport costs.

Theoretically, wheat might be an inferior good, however, shopkeepers reported that demand increased and there is corroborating evidence in the nutrition module of the survey to the effect that consumption of wheat increased by approximately 10%.

Another possible explanation is quantity discounts, however in the shopkeeper survey only 7 out of 69 shopkeepers surveyed received any quantity discounts from their wholesalers and for these shopkeepers the discount received did not change compared to prior to the LIWP intervention.

Again, it is possible that storekeepers might engage in price discrimination, charging higher prices to customers that pay late in order to indirectly charge interest. According to informal conversations with shopkeepers, price discrimination is not an open practice as it would probably be understood to violate Islamic prohibition on selling basic commodities for different prices where there is no difference in quality. Discounts may be given quietly to customers who do not take credit, but in contexts where almost all customers take credit there is not price discrimination based on the amount of debt or lateness in repaying. The hypothesis of price discrimination can be tested in the household survey data. The variation in prices reported within each community is generally within 1000 riyals. (Within community variation in reported prices reported may result from prices being reported from different shops or with reference to purchases at different times.) As seen in table 1.18, there is no evidence that household that customers that take store credit report systematically higher prices than other households in the same community at baseline.

Finally, we check whether the differences seen in pricing between communities with more stores and communities with few stores is a function of market of structure of simply isolation and higher transport costs to the village by looking for the same negative effect on prices but using distance or cost of travel to the nearest market as explanatory variables. The regressions in table 1.20 show that the interaction of LIWP treatment and number of stores is significant in explaining the negative price effect, while cost and distance are not. In columns

(1) and (2) the coefficients on distance to market and distance interacted with LIWP are not significant. In columns (3) and (4) the coefficent on cost to market is significant and positive (i.e. where the transport cost to market is higher, prices are higher), but the coefficient on the interaction with LIWP is non-significant and positive, rather than negative as would be expected if the negative price effect in communities with few stores was being driven by higher transport costs. By contrast in columns (5) and (6) we see that communities with more stores have lower prices generally, but the interaction with LIWP is significant and positive, indicating lower prices in areas with fewer stores as seen earlier.

1.8 Conclusion

Using our estimated ratio of income to debt repayment of 10%-14% (estimates from shopkeeper survey and LIWP data respectively) of the on average transfer of $500 and an estimated decline in prices of 10%, we can make a rough calculation of the spillover benefits of the LIWP income transfers. Assuming that all villagers buy one basket of goods each month, the average transfer of $500 which went to 70% of the village would lead to additional total repayment per customer of $35-$49 for the storekeeper, and $8 per month each month or $48per household.

Our finding that the effect on debt repayment can mitigate or reverse the expected increase in prices due to an infusion on cash in shallow markets is important for understanding the general equilibrium effects of social programs. On the one hand, customers benefit from the insurance function as seen in their ability to accumulate large amounts of debt during the crisis in villages with few stores. On the other hand, a large share of their income went to repaying this debt, and protecting shopkeepers from defaults, which is only partially redistributed among customers via a decrease in price levels.

Tables

Table 1.1: Negative Price Effect

	All	$n < 2$	$2 <= n < 5$	$n >= 5$
LIWP treatment	-51.7	-414.9**	-506.3**	472.0
	(147.8)	(159.8)	(212.7)	(290.7)
Expost	1839.1***	1947.4***	1862.6***	1534.5***
	(94.0)	(122.5)	(124.2)	(238.6)
Observations	1475	609	490	252

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

Program treatment effect on the price of wheat. In the first column households in all communities in the sample are included, while subsequent columns restrict the sample to households in communities to those with the indicated number of grocery stores per village. From LIWP RCT dataset.

Table 1.2: Debt Repayment Summary Statistics for All Households

	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

Level of total debt, amount of debt repaid, and value of assets sold in past 12 months in control and treatment villages before and after the intervention. Only communities with fewer than 5 stores are included to focus on village-type markets. Missing observations for amount paid off are for respondents that knew some debt was paid off, but did not know how much. From LIWP RCT dataset.

Table 1.3: LIWP Impact on Debt Levels

	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 results for LIWP impact on total debt amount of debt repaid, and value of assets sold in past 12 months in control and treatment villages before and after the intervention. See summary statistics above. From LIWP RCT dataset.

Table 1.4: Debt Repayment Summary Statistics for Household in Debt at Baseline

	In Debt to Store	In Debt	Debt Amount	Amount Paid	Share Paid
Control	0.866	1	129485.5	21237.3	0.182
Baseline	(0.341)	(0)	(186959.0)	(81362.4)	(0.409)
Control	0.705	0.854	130172.3	22562.0	0.368
Expost	(0.457)	(0.353)	(312547.9)	(53174.9)	(0.502)
Treatment	0.871	1	119937.9	16968.4	0.208
Baseline	(0.336)	(0)	(220823.7)	(35754.5)	(0.282)
Treatment	0.713	0.875	120129.2	34821.3	0.522
Expost	(0.454)	(0.331)	(178976.1)	(77497.6)	(1.431)
Observations	1003	1003	1002	982	982

Summary statistics of debt repayment among households that were indebted to storekeeper at baseline. Only communities with fewer than 5 stores are included to focus on village-type markets. Missing observations for amount paid off are only for respondents that knew some debt was paid off, but did not know how much. Respondents that had zero debt are treated as having paid zero (212 observations).

Table 1.5: LIWP Impact on Debt Levels for Household in Debt at Baseline

	In Debt to Store	In Debt	Debt Amount	Amount Paid	Share Paid
LIP Program	0.00259	0.0206	-345.7	15170.4**	0.116
	(0.0743)	(0.0416)	(29072.2)	(7311.0)	(0.118)
Expost	-0.161***	-0.146***	536.9	2870.5	0.193***
	(0.0547)	(0.0253)	(22003.0)	(4731.0)	(0.0525)
Observations	1003	1003	1002	982	982

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

Summary statistics of debt repayment among households that were indebted to storekeeper at baseline, sample here restricted to households in communities with fewer than 5 stores.

Table 1.6: LIWP Income vs. Amount of Debt Repaid

	(1)	(2)
LIWP Income	0.147	0.136*
	(0.109)	(0.0806)
LIWP Income Squared	-0.000000139	-0.000000310**
	(0.000000192)	(0.000000142)
Debt Amount		0.385***
		(0.0319)
Observations	175	175

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

Regression results for amount of debt repaid as a function of LIWP income and debt level among LIWP participants in the treated villages. From LIWP RCT dataset.

Table 1.7: Change in Debt Level and LIWP Income

	Change	Change	Increase	Repay	Change
LIWP Income Prev. Month	-0.0440	-0.101**	-0.0558	0.0403	-0.0226
	(-0.81)	(-2.06)	(-1.06)	(1.11)	(-0.41)
Customer Fixed Debt		1.800***	2.083***	0.297***	
		(13.95)	(17.13)	(5.28)	
Month FE	Yes	Yes	Yes	Yes	Yes
Project FE	Yes	Yes	Yes	Yes	No
Customer FE	No	No	No	No	Yes
Observations	860	860	860	860	860

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

Table 1.8: Self-Reported Use of Credit as Insurance

	Use Credit as Coping Strategy		Borrowed from Other Source	
	(1)	(2)	(3)	(4)
At Least 5 Stores	-0.12		0.08	
	(0.08)		(0.06)	
2 to 4 Stores	-0.04			
	(0.08)			
Number of Stores		-0.02*		0.02
		(0.01)		(0.01)
Observations	390	390	774	774

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

Share of customers that reported using shop credit as a coping strategy for food insecurity and share of customers with non-store debt by type of market

Table 1.9: Effect of Shock on Debt Repayment

	Percent Repaid		
	All control	$n \geq 2$	$n < 2$
At Least 5 Stores	0.08*		
	(0.04)		
2 to 4 Stores	0.08**		
	(0.04)		
Expost	0.06	0.07	-0.01
	(0.04)	(0.04)	(0.05)
LIWP treatment		-0.02	0.13**
		(0.05)	(0.06)
Mean Dep. Var.	0.21	0.24	0.20

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

The economic crisis effect on debt repayment in competitive vs. monopolistic markets. (n refers to average number of stores in project villages)

Table 1.10: Correlation of Market Structure and Bad Debt

	Share Transactions Credit	Share Bad	Share Bad Clear
Number stores	-0.06**	0.06*	0.02
	(0.03)	(0.03)	(0.13)
N	69	69	56
Mean Dep. Var.	.503	.182	0.363

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

Table 1.11: Correlation Between Share of Customers Taking Credit and Price of Consumption Basket

	Price of Basket
Share of Customers that Buy on Credit	2103.55*
	(1203.17)
Price in City	0.03
	(0.02)
N	647
Mean Dep. Var.	14277

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

Correlation Between Share of Customers Taking Credit and Price of Consumption Basket.

Table 1.12: Correlation Between Food Insecurity and Debt Repayment

	Percent of Debt Repaid in Expost Survey			
	Control	Treatment	Control	Treatment
	none	none	none	none
Food Insecure	-0.084***	-0.045*	-0.102***	-0.059**
	(0.024)	(0.026)	(0.025)	(0.027)
Food Shortage			0.056***	0.046**
			(0.021)	(0.021)
Observations	393	429	393	429

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

Food shortage here means that household responded that had trouble providing food during past 12 months, food insecure means that adults or children skipped some meals due to food shortage. Data from LIWP RCT survey.

Table 1.13: Cumulative Effect of Shocks on Debt Repayment

	Non-Participants		Participants	
	(1)	(2)	(3)	(4)
Negative Shock in Past Year	8106.79*	10636.41*	1933.47	3742.11
	(4430.47)	(6089.77)	(3448.58)	(4331.82)
Multiple Negative Shocks in Past Year		-7299.73		-4756.53
		(6275.39)		(3475.69)
Observations	266	266	393	393

Random effects, standard errors clustered at the store level

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

Effect of shocks during past year on debt level of customer at the beginning of Ramadan (25% of customers had single shock, 7% had multiple shocks). Data from shopkeeper survey.

Table 1.14: Effect of Single Shock on Debt Repayment

	Debt Level			
	Non-Participants	Participants	Non-Participants	Participants
Shock in Past Month	7064.80**	-2983.95	7147.07**	-2191.16
	(2807.25)	(2797.79)	(2810.97)	(2838.00)
Month FE	No	No	Yes	Yes
Observations	542	774	542	774

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

Effect of shock during the month on debt level at the end of Rajab and Shaaban. Data from shopkeeper survey.

Table 1.15: Shopkeeper Reported Responsiveness to Customer Income

Shopkeeper's response:	Single Shock	Multiple Shocks	Total
Increased credit	22	1	23
Delayed repayment	44	23	67
Gave charity	9	7	16
Other	3	2	5

Table 1.16: Negative Price Effect in Shopkeeper Data

	Flour	Flour	Basket	Dry Basket
During LIWP	-1.62*	-1.61*	-388.96***	-276.71***
	(0.89)	(0.88)	(110.31)	(94.88)
After LIWP	1.91	1.88	473.88***	376.30***
	(1.24)	(1.23)	(134.31)	(138.29)
Project FE	Yes	Yes	Yes	Yes
Price in City	No	Yes	Yes	Yes
N	720	720	647	672
Mean Dep. Var.	126	126	14277	13059

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

Table 1.17: Monthly prices in village shops relative to start date of LIWP intervention. Dry basket excludes oil due to concerns about measurement error related to variation in container size.

	Reported Price of Flour in Village	
	HHs with Store Debt	All HHs
q502thous	-0.15	
	(0.12)	
Any Store Debt		-19.31
		(42.27)
Community FE	Yes	Yes
Observations	322	626

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

Table 1.18: Lack of price discrimination seen via regression of relative price level to household debt status (baseline only). Debt is measures in thousands of riyals and price in riyals.

Table 1.19: Negative Price Effect - Excluding Road Projects

	All	$n < 2$	$2 <= n < 5$	$n > 5$
LIWP treatment	-74.3	-604.9***	-685.0***	503.0
	(176.2)	(175.7)	(220.8)	(288.7)
Expost	1843.0***	1942.4***	1862.6***	1534.5***
	(91.2)	(115.4)	(124.8)	(241.3)
Observations	1205	594	412	209

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

Program treatment effect on the price of wheat, excluding villages in which the LIWP project involved road repair or construction.

Table 1.20: Market Structure Drives Negative Price Effect

	(1)	(2)	(3)	(4)	(5)	(6)
LIWP Treatment	-256.6**	-362.9**	-244.0*	-275.4**	-255.4**	-514.6***
	(126.0)	(154.6)	(128.1)	(134.5)	(126.1)	(193.0)
Expost	1788.3***	1788.8***	1786.6***	1786.8***	1789.7***	1789.7***
	(90.1)	(90.2)	(94.8)	(94.9)	(89.7)	(89.7)
Distance to Market	-0.2	-0.6				
	(0.4)	(0.5)				
Distance * LIWP		1.1				
		(0.8)				
Cost to Market			0.1***	0.1		
			(0.0)	(0.1)		
Cost * LIWP				0.1		
				(0.1)		
Num. of Stores					-113.0***	-173.5***
					(26.1)	(39.8)
Num. Stores * LIWP						128.2*
						(69.5)
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1150	1150	1064	1064	1159	1159

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

Comparison of distance to market, cost of travel to market, and number of stores as explanatory variables for the negative price effect of LIWP intervention in communities with few stores

Appendix I: Proof of Equation 1.12

Expected utility with insurance contract :

$$EU(p^*_{ic}) = u(\frac{1}{m}(y_1 - p^*) + \frac{\beta}{m}(\overline{y} - y_1)) + \frac{1}{y_H - y_L}\int_{y_L}^{y_H}(u(y_2 + (1 - \frac{1}{m})y_1 + \frac{1}{m}p^* + \frac{\beta}{m}(\overline{y} - y_2)))dy_2$$

$$= 2u(\frac{m-\beta}{2m}y_1 + \frac{\beta}{m}\overline{y})(\frac{u(y_H(1-\frac{\beta}{m})) - u(y_L(1-\frac{\beta}{m}))}{(y_H - y_L)})^{\frac{1}{2}}$$

$EU(y_1) = \overline{y}$ so :

$$EU(p^*_{ic}) = 2u((\frac{1}{2} + \frac{\beta}{2m})\overline{y})(\frac{u(y_H(1-\frac{\beta}{m})) - u(y_L(1-\frac{\beta}{m}))}{(y_H - y_L)})^{\frac{1}{2}}$$

Recall that :

$$EU(p^*_{nc}) = 2u(\frac{1}{2}\overline{y})(\frac{u(y_H) - u(y_L)}{(y_H - y_L)})^{\frac{1}{2}}$$

We want to show that:

$$2u((\frac{1}{2} + \frac{\beta}{2m})\overline{y})(\frac{u(y_H(1-\frac{\beta}{m})) - u(y_L(1-\frac{\beta}{m}))}{(y_H - y_L)})^{\frac{1}{2}} > 2u(\frac{1}{2}\overline{y})(\frac{u(y_H) - u(y_L)}{(y_H - y_L)})^{\frac{1}{2}}$$

Dividing through by $2u(\frac{1}{2}\overline{y})$ (which is negative):

$$u(\frac{\beta}{m}\overline{y})(\frac{u(y_H(1-\frac{\beta}{m})) - u(y_L(1-\frac{\beta}{m}))}{1})^{\frac{1}{2}} < (\frac{u(y_H) - u(y_L)}{1})^{\frac{1}{2}}$$

$$u(\frac{2\beta}{m}\overline{y})(u(y_H(1-\frac{\beta}{m})) - u(y_L(1-\frac{\beta}{m}))) < u(y_H) - u(y_L)$$

$$u(y_H(1-\frac{\beta}{m})) - u(y_L(1-\frac{\beta}{m})) < (y_H - \frac{2\beta}{m}\overline{y}) - u(y_L - \frac{2\beta}{m}\overline{y})$$

$$u(y_H - \frac{\beta}{m}y_H) - u(y_L - \frac{\beta}{m}y_L) < u(y_H - \frac{\beta}{m}y_H - \frac{\beta}{m}y_L) - u(y_L - \frac{\beta}{m}y_L - \frac{\beta}{m}y_H)$$

$$u(y_H - \frac{\beta}{m}y_H) - u(y_H - \frac{\beta}{m}y_H - \frac{\beta}{m}y_L) < (y_L - \frac{\beta}{m}y_L) - u(y_L - \frac{\beta}{m}y_L - \frac{\beta}{m}y_H)$$

Which holds for any utility function with decreasing marginal utility.

Appendix II: Proof of Inequality 1.19

Increase in second period incomes by ω

$$u(\frac{1}{m}(\overline{y}+\omega))(u((y_H+\omega)(1-\frac{1}{m}))-u((y_L+\omega)(1-\frac{1}{m})))=u(y_H+\omega)-u(y_L+\omega)$$

Let $W=(u((y_H+\omega)(1-\frac{1}{m}))-u((y_L+\omega)(1-\frac{1}{m})))$

$$\frac{\gamma}{m^2}u(\frac{1}{m}(\overline{y}+\omega))\left(\overline{y}W-y_H(u((y_H+\omega)(1-\frac{1}{m}))+y_Lu((y_L+\omega)(1-\frac{1}{m}))\right)dm+$$
$$+\left(\frac{-\gamma}{m}u(\frac{1}{m}(\overline{y}+\omega))W+(-\gamma+\frac{\gamma}{m})u(\frac{1}{m}(\overline{y}+\omega))W\right)d\omega=(-\gamma u(y_H+\omega)+\gamma u(y_L+\omega))\,d\omega$$

$$(-)dm=(\frac{\gamma}{m}u(\frac{1}{m}(\overline{y}+\omega))W+(\gamma-\frac{\gamma}{m})u(\frac{1}{m}(\overline{y}+\omega))W-\gamma u(y_H+\omega)+\gamma u(y_L+\omega))d\omega$$

$$(-)dm=\gamma u(\frac{\omega}{m})\left((u(y_H-\frac{y_H}{m}+\frac{\overline{y}}{m})-u(y_H))-(u(y_L-\frac{y_L}{m}+\frac{\overline{y}}{m})-u(y_L))\right)d\omega$$

$$(-)dm=\gamma u(\frac{\omega}{m})\left((u(y_H-(\frac{y_H-y_L}{2m})))-u(y_H))-(u(y_L+(\frac{y_H-y_L}{2m}))-u(y_L))\right)d\omega$$

$$(-)dm=(+)\,d\omega$$

$$\frac{d\omega}{dm}<0$$

Chapter 2

Community Based Project Choice

Summary

We find that the choice of project type in the LIWP intervention encompasses not only a choice between different types of public goods, but also a choice about the distribution of wages during the construction phase. Community project choice appears to be determined in part by the age composition of the community's male population, with a higher share of older men correlated with more skill intensive project choice. We also find that the economic crisis led to an increase in the skill intensity of selected projects, as it both increased the share of older men in all communities via out-migration of younger men and increased the probability that older men would be interested in participating in the program.

2.1 Introduction

Community driven development has been strongly promoted over the past decade by funding agencies with a goal of giving community members a greater voice in decisions about local public good provision (Mansuri Rao 2013). This participation has the advantage of mobilizing local information not otherwise accessible to outsiders in adapting public projects to community preferences.

The Labor Intensive Works Program (LIWP) is a cash-for-work rural employment program run by the Yemen Social Fund for Development (SFD). LIWP provides short term work opportunities in poor communities in the construction of labor intensive community infrastructure projects such as repairing roads, clearing land, or constructing rainwater harvesting systems. A hallmark of SFD's management of LIWP is the participatory engagement of communities in decision-making regarding the choice of local projects to be funded. This paper explores the determinants of community workfare project choices in a context of community heterogeneity and fluctuating economic conditions.

In particular, some projects proposed by communities for the LIWP program require more skilled labor, leading to a bias in the distribution of benefits towards older men, while other projects require little skilled labor and therefore result in a larger share of the program wages going to households with unskilled young men or females. In general, we contrast three major types of projects: road, water storage, and land projects. Road projects include repair and construction of rural roads. Water projects include construction of wells, cisterns, household storage tanks, and rain water harvesting projects. Land rehabilitation projects include clearing land of invasive bushes (especially prickly pear cactus) and terrace repairs. In general, road and water projects require more skilled labor inputs than land rehabilitation projects.

We show that a larger share of experienced, older men in the community population was associated with choice of more skill-intensive projects, and the selection of skill-intensive projects increased after an economic crisis which increased the share of older men and decreased their opportunity cost of participating.

Setting

Communities selected to participate in LIWP are actively involved in the design of the intervention. The participatory process starts when SFD consultants organize a community meeting to introduce the program. Original preferences from community members for a project are usually not realistic. During the following week, there is an awareness campaign about SFD objectives and standards and project consultants count the size of the potential workforce. SFD standards for projects require that at least 70% of the benefits of project infrastructure must go to the community as a whole. The project must also be technically feasible to complete using mostly unskilled labor and not harm the natural environment. There is not a strict menu of possible projects, but the projects proposed generally fall into categories with which SFD has past experience. By the end of the week, the SFD consultants have usually identified a short list of feasible projects. Another general meeting is held at which the community decides on the prioritization of these proposed projects. There is often vigorous discussion about the relative benefits of different projects. However, there is also a lot of emphasis on the necessity for the community to come to agreement, preferably unanimously, because they know that SFD projects can be canceled due to community disagreement. It is rare for the final decision to be made by a formal vote. After the community decides on prioritization of the potential projects, SFD sends officers to do an economic and technical study to see how much the projects will cost. The final implementation will include the top choice of the community, with lower ranked projects added as extra components of the intervention if there is sufficient funding.

Wages in the LIWP program are based on piece rates for different tasks and generally higher for more skilled or physically difficult work. The median wages reported for skilled work in our household sample was 2124 riyals($10) per day and for unskilled work was 1200 riyals ($5.66) per day.

Due to conservative gender norms in rural Yemen, men and women generally deliberate and recommend separately. The preferences of the two groups are reconciled in an unspecified fashion. In consequence of this gender segregated process, the relative size of the men's group and women's group are less likely to matter than the internal composition of the two groups. In this paper, we focus on the age composition of the male population.

Data

Our analysis uses administrative data on 447 projects with start dates in 2008-2013. For each project, we can see the budget, description of project components, start and end dates, projected number and gender of jobs created, and actual number and gender of participants. For a subset of these projects: 168 projects with start dates in 2010-2012, we additionally have administrative date on total wages received from the project for each participating household as well as the gender of participants from that household and skill/ unskilled status of the tasks completed.

The period of data is significant because project choices in the early years of the LIWP program were made by this group of communities in a context of lingering effects of the 2008 food crisis, but prior to the Arab Spring and accompanying political upheaval in Yemen starting in July 2011. For projects that began in 2012 or later, on the other hand, the sharp loss in employment and further rise in food prices changed the incentives for community members deliberating about project choice.

Finally, for a small number of projects, we have detailed household survey information for 12 randomly selected households from the baseline and expost surveys for an RCT impact evaluation. For the impact evaluation, 44 treatment and 40 control communities were surveyed at baseline (May 2010) prior to intervention in the treatment areas, and expost (November 2011), after which some of the control communities received the LIWP program.[1]

2.2 Vested Interest of Older Men in Skill Intensive Project Types

We find evidence that demographic composition of communities is predictive of the type of project selected, with communities with a larger share of older men opting for more skill intensive projects.

Skill-Intensity Varies by Project Type

An analysis of data on household earnings from the LIWP program shows that the relative cash benefit for households with only unskilled participants compared to households with at

[1]From the household survey data, 36 treatment communities and 20 control communities can be matched with administrative data mentioned above. (The imperfect matching here is due to unrecorded changes in project id numbers, plus implementation problems in which led to programs in control communities not actually being implemented.)

least one skilled participant was highest in projects that had primarily land interventions, followed by water interventions, and then by road interventions. The relative returns to unskilled labor for different project types parallel the returns to households with only female participants and the share of households with only unskilled workers or female workers in the project.

Most projects have multiple components. We broadly categorize each project component as: land interventions, water interventions, and road interventions.[2] Land interventions include terrace rehabilitation, flood protection, clearing invasive plants, irrigation, and reforestation. Water interventions include digging wells, rainwater harvesting schemes, and construction of large cisterns for water storage. Finally, road projects involve construction or repair of rural roads connecting the village to larger roads and local markets.

LIWP program staff characterized land interventions as requiring the greatest amount of unskilled labor. Clearing land of invasive cacti, for example, is a task that is often performed by women with minimal training and simple tools. Water and road projects by contrast require both more skilled labor and more physically intensive work such as cutting and shaping stones meaning that the relative gains for unskilled labor generally and for women specifically are lower.

For 190 LIWP projects between 2010 and 2012, administrative data provides the total LIWP program income received per household and number of males, females, and skilled workers in these households. We compare these projects in terms of the ratio of total project income for households with only unskilled workers compared to households with at least one skilled worker and categorize the projects by the type of the primary intervention. We concentrate on the ratio of returns because the level of piece rates varied geographically according to the prevailing wage in the local labor market. In Table 2.1 we see a gradient in relative returns of unskilled to skilled labor that corresponds to the qualitative descriptions given by program staff cited above.

The first variable considered ("Income") is the ratio of total project income for households with only unskilled labor relative to households with at least one skilled member. Road projects have the lowest relative income for unskilled labor (0.27), while land projects have the highest (0.45), with the differences between these project types being large and statistically significant. We find that land projects have somewhat more days worked by unskilled households, but the more notable difference is found in the relatively higher average daily wages received by unskilled households. In road projects the average wage of households with only unskilled members was less than half the average wage of households with skilled members, while in land projects, the average wages of the two types of household were almost equal.

For the remainder of the paper, we closely associate projects the share of female jobs with the share of unskilled jobs. Because we do not have data below the household level and very few households had only females participating in the project, we cannot show the differential returns to household as strongly by gender. In the fourth column of Table 2.1 we

[2]An additional category- sanitation projects- are not considered since the sample size is too small

compare the relative total income of households with only female members to the income of households with at least one male participant by project type. Because there are relatively few female only households, some communities drop out of the sample and the difference is not statistically significant, however we do see that land projects have the greatest relative benefit for only female households.

We also compare the projects in terms of the gender and skill level of participants, defining "Female Share" as the proportion of participating households where at least one of the workers was female and "Skilled Share" as the share of households with a skilled participant. While LIWP regulations required that unskilled labor could only be drawn from the local community since the program was intended to provide jobs for underemployed labor, the skilled labor was considered a complementary input that could be drawn from outside the community if necessary, which makes it more likely that the share of skilled households reflects the technical demands of the project, rather than a pre-existing characteristic of the community. We see that the share of households with a skilled participant ("Skilled Share") was significantly higher in road projects than water projects and slightly higher in water projects than land projects. Households with members that could potentially work as skilled workers in a LIWP project, therefore, were most likely to benefit and received the most project income from road projects, and to a lesser extent, water projects. The jobs appropriate for women in these projects are always unskilled and due to being less physically demanding, had lower piece rates. We see the same pattern in female share with lowest share in road projects and highest in land projects.

Planned Female Share of Jobs as an Indicator of Project Skill-Intensity

The expected number of female jobs was reported ex ante during the planning phase of the project, and it turns out be a useful indicator of the expected relative skill-intensity of the project. For the full sample of 477 LIWP interventions since 2008, we match system data on the intervention type with the 2001 Population Census and 2004 Agricultural Census.

Since the ratio of realized relative benefits to unskilled vs. skilled labor is affected by the choice of households to participate in the project as well as by the project choice itself, we use planned female job share as a proxy for the expected relative benefits to unskilled labor since female jobs are by definition unskilled as well as being less physically strenuous. Planned female job share varies as expected with the type of project chosen (Table 2.2). We can also show that planned female job share is strongly correlated with the actual female job share and with the relative returns to unskilled labor (Table 2.3). These correlations are robust to inclusion of branch fixed effects, which suggests that they are not due to other geographic factors. Below, we use planned female job share as a convenient indicator of skill-intensity, due to its availability as an ex-ante parameter of project type in the database and the usefulness of a continual variable rather than looking at discrete categories of project type.

Older Men Most Likely to Participate in Skilled Jobs in LIWP

There is no way to perfectly identify from household data which men are "skilled" in the sense that they would be eligible for higher paying types of jobs in the LIWP intervention. However, we find that age is a strong predictor for probability of being employed as a skilled worker.

Table 2.4 shows baseline characteristics which predicted which households would have a member engaged in skilled work in the LIWP intervention. In the context of the LIWP intervention, skilled work still refers to relatively low-skill tasks such as stone cutting for which the necessary qualification is work experience rather than education. Also, the employment offered in the LIWP intervention was short term only. So the potential beneficiaries of skilled work opportunities in the program were not the most skilled workers in the community, but workers with higher than average work experience but also low opportunity cost. The fact that some men may have been eligible for skilled work in the LIWP program but choose to continue in other employment instead is irrelevant to this analysis, since men who did not expect to work at all in the LIWP program even if skilled jobs are offered would not distinguish between project types based on their expected income from work in the program.

In Table 2.4, we see that age between 40 and 65 and work experience in the construction sector and are statistically significant predictors of skilled LIWP employment. Table 2.5 also shows the degree to which skilled workers are drawn disproportionately from men in older age groups, with only 10% of LIWP participants in the 20 to 30 age group employed as skilled workers, compared to 30% of LIWP participants in the 40 to 60 age group.

2.3 Age Composition of Male Population as a Determinant of Project Choice

The difference in the age composition of the male population is visible indirectly in the census. Because most out migration is by younger men, in communities with higher out migration (more women than men in the census), more of the remaining men are older and skilled. Since men and women deliberate separately, the relative gender composition does not matter as much as the age composition within the men's group. We find that communities with more out migration tend to choose more skill intensive projects.

Female Population Negatively Correlated with Female Job Share

As shown in Table 2.6, female job share is strongly negatively correlated with female population in the 2001 population census. The correlation is robust to the inclusion of geographic and time dummies. The correlation of female job share with female population and average land is robust to the inclusion of branch level fixed effects or average land area, implying that this correlation is not simply an artifact of other geographic characteristics. In particular, this addresses the concern that more agricultural communities are simply more likely to have potential land clearing or terrace building projects. (See Table 2.3). [3]

Regressions using average skill difference for project choice showed similar patterns, but were not statistically significant due to the small sample size compounded by using averages at the project type level.

High Female Population as a Proxy for Few Young Men

The relationship between high female population and low planned female job share must be interpreted in light of the fact that in most Yemeni villages, the gender ratio is driven by emigration of relatively young, unskilled men for employment in cities or abroad. Villages with higher shares of females are also those with the highest share of migrants. We argue below that having a higher share of migrants means that more of the remaining men are those most likely to benefit from skill-intensive projects.

There are also several reasons why the greater share of women in the village is unlikely to directly affect project choice decisions in the direction of increasing job opportunities for women. If men and women are segregated for the community discussions, the relative weight of men's and women's conclusions is not likely to be changed by having a larger number of women in the women's meeting. Secondly, women whose husbands are absent are less likely to attend community meetings due to cultural norms about unaccompanied women. Finally, in spite of SFD's emphasis on involving women in community decision making, traditional community norms are strongly patriarchal.

To justify the assumption that communities with a higher proportion of women have fewer young men, we can turn to the household survey data on age and gender composition of the sampled households. We see in Figures 2.1 and 2.2 that communities with a high share of women in the adult population are mostly missing men under age 35. We can test this directly to show that villages with higher shares of women had significantly more men in the 40-65 age group (Table 2.7). The regressions show that a 1 percentage point increase in the share of females in the population sampled for the survey corresponded to a 0.45 percentage point increase in the probability that a man in the population sample was over the age of 37. To get a better sense of the magnitudes, we can compare the ratio of men over 37 in the population in the 12 villages with highest female share (over 60%) and lowest female share (under 45%). In the high female share villages, the 31% of men were over 37, while in the low female share villages, only 24% of men were over 37. The inclusion of branch fixed effects to control for regional characteristics has little effect on the coefficient.

Finally, in Figure 2.3, we use data from a village survey taken at the same time as the household survey to show a similar relationship between the share of female-headed households in a community and the number of female jobs. Since this data is more likely to

[3]The land ownership Gini coefficient is calculated based on the shares of the population that own no land, less than 5 hectares, 5-20 hectares, and more than 20 hectares

correctly capture current community demographics than the 2001 population census, it is a useful robustness check.

In summary, therefore, the correlation we find between more females and more skill-intensive projects is consistent with communities with a higher share of females having more older, experienced men, who would potentially benefit from skilled employment in LIWP influencing the decision towards projects that are more likely to benefit them.

2.4 Project Selection After the 2011 Crisis

One of the impacts of the economic crisis was a loss of skilled work in construction or other non-agricultural sectors. This increased the share of older, skilled men with a vested interest in skill-intensive projects, since they became more likely to participate in the LIWP short term employment. Simultaneously, the crisis increased out-migration by younger men, reducing their influence on project choices.

Impact of the Crisis on Project Skill Intensity

We identify the effect of the crisis on project choice using two approaches: (1) using the randomized assignment among the 80 RCT projects and (2) an event analysis approach in which we look at all of the projects in the data set and test for a sharp change in projects with start dates following the crisis. While the randomized assignment is more clearly identified, our sample size is restricted and there is a possibility of bias since not all control communities proceeded with the intervention after the RCT.

Looking at the entire set of project choices, Figure 2.4 summarizes the changes in project choice over time based on the project planning year. Over time, more recent projects have increasingly had a greater share of female jobs. The LIWP program itself set increased female employment as a goal and the increase began even before the 2011 crisis. It encompassed both pressure from SFD consultants during the decision making process, and increased creativity in implementation design to allow females to participate in ways that do not violate cultural norms. This gradual increase in female jobs, therefore, should be seen as separate from changes in community preferences. What is notable after the time of the crisis, however, is a distinct drop in the number of planned female jobs for new projects.

The regression results in Table 2.8 show both the positive time trend noted above and a negative post-crisis. The indicator variable for post-crisis is a project start date after March 2011. Arab spring protests started in January 2011, but there was several months lag time between decisions on project choice and start time. For the two specifications, we estimate a negative crisis effects of approximately 8.3 percentage point on the planned share of female jobs. Controlling for the number of females in the population does not change the estimated coefficient. While the order of intervention is not randomized, we have no reason to believe that the date of intervention relative to the crisis is correlated with determinants of project choice, beyond the aforementioned increasing pressure for unskilled type projects by donors which we control for with a time trend.

As a check that there was not concurrent change in the static characteristics of communities selected to participate in LIWP, we can also show separately in the last columns of table 2.8 that there was no change in the gender ratio or average land area for communities with interventions after the crisis compared to before the crisis.

The communities in the randomized control trial should ideally allow us to identify the effect of the crisis independent of other factors since treatment communities were randomly assigned to receive the intervention prior to 2011. However, many control communities were dropped from the program or had their intervention delayed due to political instability. Our sample size of communities that were randomly assigned to intervention planning pre- or post-crisis is limited to 36 pre-crisis and 14 post-crisis communities. Possibly due to this limited sample size, our estimated crisis effects using this sample are somewhat smaller and not statistically significant but still negative: -3.4 or -3.0 percentage points, depending on whether we control for community characteristics.

Decrease in Opportunity Cost for Skilled Men

We can see the effect of the crisis on skilled households households in the loss of skilled employment reported in the construction and other non-agricultural sectors. Survey data from the control communities in the RCT shows that the number of individuals reporting skilled jobs or government jobs declined sharply from 2010 to 2012, balanced by increases in employment in unskilled jobs or self-employment. (See table 3.21).

We can model the community as made up of two groups: potential skilled workers concentrated in better-off households and potential unskilled workers concentrated in poorer households. During the decision making meeting, community members choose which type of project to advocate for based on their own self-interest, but are also aware of social benefits from supporting the choice of their neighbors. For better-off households who do not plan to participate in the project, the social benefits of supporting a low-skill project that their neighbors favor may be the deciding factor, so prior to the crisis when few better-off households plan to participate and a larger number of men demanding unskilled work, the community choice was tilted towards a low-skill project. The economic crisis changed the calculation for better-off households who lost other opportunities and were reported by program staff to participate in larger numbers than previous to the crisis, so we expect to see a switch towards more high-skill projects.

Decrease in Population Share of Older Men

We also find evidence for a second channel by which older, skilled men had a greater role in project choice: increased outmigration of young men after the crisis. Table 2.10 shows the difference in demographic composition of the communities in the RCT sample before (baseline) and after (expost) the 2011 crisis. Across all communities, there was an increase in the share of women in the sample, and in control communities specifically, there was a decrease in the share of younger men, suggesting that the LIWP treatment kept young men who would otherwise have migrated away in the village. In villages where the project choice occurred after the crisis, however, these young men would already have left.

Distributional Outcomes by Project Type

Households with older men that participated in skilled work in LIWP tended to have higher baseline monthly income but lower wealth. So the distributional tradeoff between higher income for skilled vs. unskilled workers is between heads of household living in village in poorer households, or younger men and women in households that are better off in terms of wealth. Skilled, older men in wealthy households presumably had sufficiently good external employment that they did not participate in LIWP.

In table 2.11 we show that households with skilled work in LIWP have higher baseline monthly income, but lower wealth as measure by a proxy wealth score developed by SFD. The inconsistency between income and wealth is due to wealthy households in this context being more likely to be receiving remittances from absent members.

Within the RCT subgroup of projects for which we have household surveys, we can directly measure the distributional impact of the LIWP intervention by measuring the change in inequality of wage income in the past month.

As seen in the summary statistics in table 3.6, there was a general trend of worsening inequality between baseline and expost in control communities due to the economic crisis. Splitting the sample by communities with at least one road component, water component, or land component, we find the greatest program effect on protecting communities from the increase in inequality in communities with land projects, while communities with road projects had the smallest program effect. This heterogeneity is consistent with the differences in distributional consequences by project type seen in the program-wide household income data described above.

Table 3.6 shows regression results for LIWP impact on the Gini index of monthly income. The variable of interest- “Active in past month” indicates communities with LIWP treatment and active programs during the month prior to the survey. The variable “Expost” captures the time trend between the baseline (2010) and expost (2012) rounds of household surveys.

Distribution of Benefits from Finished Projects

While the majority of this paper has focused on the distribution of cash benefits from projects, the long-term value provided by the completed project is also clearly an important factor in project selection. Our information on this aspect of the project is incomplete, but is there some evidence that the distributional characteristics of the completed projects are similar to the distributional characteristics of the work itself.

Qualitatively, it is expected that road projects reduce travel time and cost. Water projects are also good at generating longer-term gains for development through reduced fetching time and better sanitation. In contrast, land projects are less likely to enhance productivity, since terrace repair or invasive plant removal will require high effort in ongoing maintenance and the economic productivity of arable land is very low to start with.

The distribution of these benefits also differs by project type. For example, almost all respondents in road projects noted that they would benefit from reduced travel time, with only the most marginal of households are uninvolved in economic activities from which they would benefit by better roads, while presumably better off households that own vehicles benefit the most. With water storage projects, benefits are concentrated among households that live nearby, and a monitoring report on SFD water projects found that in spite of SFD rules, some elite capture occurs via charging for access to water. Land projects, on the other hand, appear in the data to have more direct beneficiaries. In the household survey, community members were asked if they directly benefited from the completed project or planned to benefit in the future. The overall share of households that stated they directly benefited from a land project was highest for road projects (92%), followed by land projects (82%), with the lowest share for water projects (68%).

Looking at the correlation between benefits and poverty, we find some suggestive evidence in Table 2.14 that the probability of benefiting directly from project infrastructure is positively correlated with wealth for skill-intensive projects. We look at the correlation between proxy wealth scores and whether a households reported directly benefiting from the completed project. [4] Overall, the coefficient on proxy wealth score is positive but not significant, indicating that better off households may be more likely to benefit from the project. When we split the sample between projects with low female share and projects with high female share, we see that the correlation between proxy wealth score and likelihood of benefiting from the project is stronger and statistically significant at the 10% level in the subset of projects with low female participation. Similarly, if we split the sample by project type, the correlation between wealth and probability of benefiting is strongest in road and water projects. In particular, poor households were less likely to benefit from water projects.

2.5 Conclusion

Our findings on the determinants of project choice in LIWP show that participatory engagement of communities in project choice makes the demographic composition of the community a factor in project choice when different subgroups have contrasting interests. LIWP projects are responsive not only to community preferences about long term benefits of project, but also to levels and short term changes in supply of skilled labor.

The equity consequences of pressure for skill-intensive projects are not necessarily regressive, as households with skilled members were less wealthy than the average household, even

[4] The proxy variables used include as house size, roof and floor type, sanitation type, family size, enrollment of children in school

though they also tended to have high wages before the intervention and captured the lion's share of project benefits in terms of wages. In the long term, the projects that were highly skilled also happened to be those that one would guess to be potentially most important for economic growth, so we are agnostic as to the eventual social optimality of the choices made by the communities. To the degree that there is a danger of skilled participants having too much of a role in determining project choice, this has been reduced over time as the data reflects increasing pressure from SFD consultants towards skill-intensive projects during their guidance of community decision making in order to direct more benefits towards youth and women.

The responsiveness of the program to changes in the supply of skilled labor can also be seen as an illustration of the way in which delegating decision making to the community level allows for greater community buy-in, a key feature of the Social Fund's success in administering programs in a highly unstable environment.

Female employment was collected as an indicator of gender equality, however, it appears to also be an indicator of the degree to which community projects benefited unskilled participants generally. We also note that the paradoxically negative relationship we find between female population and female jobs in this context shows that female influence on decision making should not be interpreted naively as a function of the number of women involved, especially in a context with gender segregated deliberation.

Tables

Table 2.1: Relative Benefits from Program Wages by Project Type

	Skill Ratio			Female Ratio	Skilled	Female
	Income	Days	Wage	Income	Share	Share
Road	0.271	0.491	0.546	0.248	0.652	0.142
	(0.183)	(0.349)	(0.183)	(0.324)	(0.235)	(0.224)
Water	0.363	0.484	0.731	0.261	0.383	0.215
	(0.194)	(0.170)	(0.304)	(0.262)	(0.236)	(0.250)
Land	0.455	0.607	0.970	0.329	0.302	0.277
	(0.270)	(0.801)	(0.617)	(0.309)	(0.310)	(0.301)
Road vs. Water	0.0915*	-0.00763	0.185***	0.0131	-0.268***	0.0734
t-test	(1.92)	(-0.13)	(2.67)	(0.15)	(-4.79)	(1.27)
Water vs. Land	0.0926**	0.123	0.239***	0.0674	-0.0810*	0.0623
t-test	(2.13)	(1.16)	(2.67)	(1.03)	(-1.67)	(1.29)
N	139	140	140	93	159	159

Comparison by project type of the inequality in total income received from program wages. The set of 159 projects used for this analysis are the projects in the MIS for which full household level income data is available (excluding 8 sanitation projects that do not fit easily into the three categories used here for project type). The first three columns compare households with only unskilled workers to households with at least one skilled worker. The fourth column compares households with only women participating in the program to households with at least one male participant (a number of projects drop out of the sample here due to not having an households with only female participants). The last two columns provide summary statistics about the total share of participating households with at least one skilled worker, or at least one female worker.

Table 2.2: Female Job Share by Project Types

	Planned Female Jobs	Actual Female Jobs
Road	0.115	0.0444
	(0.101)	(0.0632)
Water	0.182	0.119
	(0.139)	(0.142)
Land	0.199	0.111
	(0.104)	(0.132)
Road vs. Water	-0.0674***	-0.0750***
t-test	(-3.69)	(-4.11)
Water vs. Land	-0.0169	0.00802
t-test	(-1.30)	(0.56)
Observations	432	430

Planned and actual female job share for the projects in MIS that are categorized as land, water, or road. 15 projects from the full set are excluded due to missing description of project type or unusual types.

Table 2.3: Planned Female Job Share as an Indicator of Skill-Intensity

	Share of Planned Female Jobs			
	(1)	(2)	(3)	(4)
Ratio of Unskilled to Skilled Income	0.211***	0.112*		
	(3.01)	(1.72)		
Actual Female Job Share			0.204***	0.174***
			(4.98)	(4.50)
Branch FE	No	Yes	No	Yes
Observations	430	421	441	431

t statistics in parentheses

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

Table 2.4: Characteristics of Households with Skilled Work in LIWP

	(1)	(2)	(3)	(4)
Work in Construction	0.139***		0.130***	0.131***
	(3.29)		(3.09)	(3.12)
Man Age 40-65		0.0966***	0.0883***	0.0716*
		(2.81)	(2.59)	(1.89)
Literate Man Age 40-65				0.0483
				(1.03)
Observations	419	419	419	419

t statistics in parentheses

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

Dependent variable is indicator variable for at least one household member working in skilled work in LIWP as reported in expost household survey. Independent variables are taken from baseline household survey. The level of observation is the household in the (approximately 10 participating households per survey in 44 treatment communities).

Table 2.5: Skilled vs. Non-Skilled Work by Age

	20-30	30-40	40-60
Skilled	0.10	0.18	0.30
Observations	128	95	93

mean coefficients; t statistics in parentheses

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

Share of LIWP participants in each age group that participated in skilled work

Table 2.6: Correlates of Planned Female Job Share

	Share of Planned Female Jobs			
	(1)	(2)	(3)	(4)
Females as % of Pop.	-0.200**		-0.172*	
	(-2.27)		(-1.94)	
Average Land Area Owned		0.00266***	0.00241**	
		(2.62)	(2.36)	
Land ownership Gini Coefficient				-0.0191
				(-0.61)
Branch FE	Yes	Yes	Yes	Yes
Observations	447	449	447	449

t statistics in parentheses

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

Data from all projects in the MIS matched with census records.

Table 2.7: Share of Older Men Explained by Gender Ratio

	(1)	(2)
Females as % of Pop.	0.450***	0.463***
	(3.05)	(3.12)
Post-Crisis	-0.00103	-0.00218
	(-0.08)	(-0.17)
branch FE	No	Yes
Observations	2825	2825

t statistics in parentheses

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

Dependent variable is indicator variable for man being between ages 37-67. Analysis is at the individual level (men only). Data taken from the household survey.

Table 2.8: Effect of the Crisis on Planned Female Employment

	Share Female Jobs		Females as % of Pop.	Avg Land
	(1)	(2)	(3)	(4)
Post 2011	-0.0843***	-0.0825***	-0.00372	-0.405
	(-4.00)	(-3.95)	(-0.33)	(-0.39)
Project start day	0.000146***	0.000145***	-0.000000499	0.00189**
	(7.58)	(7.58)	(-0.05)	(2.04)
Females as % of Pop.		-0.272***		
		(-3.15)		
Branch FE	Yes	Yes	Yes	Yes
Observations	449	446	463	466

t statistics in parentheses

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

Post-2011 indicates start date of project after mid March 2011 (protests started in January, but there was several months lag time between decisions on project choice and start time). Data is taken from the full set of projects in the MIS.

Table 2.9: Employment By Sector in Household Survey Communities Before and After 2011 Crisis

	Control Pre-Crisis	Treatment Pre-Crisis	Control Post-Crisis	Treatment Post-Crisis
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

Table 2.10: Demographic Change and Crisis

	Female		17-36 among Men		37-63 among Men	
	All	Control	All	Control	All	Control
Post-Crisis Treated	0.00536		0.00150		-0.00474	
	(0.40)		(0.07)		(-0.20)	
Post-Crisis	0.0209**	0.0229**	-0.0181	-0.0304*	0.0156	0.0279
	(2.22)	(2.38)	(-1.10)	(-1.79)	(0.89)	(1.54)
Observations	6531	3366	3080	1597	3080	1597

t statistics in parentheses

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

Analysis is at the individual level with data from the household survey.

Table 2.11: Wealth and Income of Households with Skilled Participants

	Monthly Wage Income		Proxy Wealth Score	
	All	LIWP only	All	LIWP only
LIWP participant	1498.6		-4.838***	
	(0.30)		(-2.88)	
LIWP skilled part.		14054.1**		-2.136
		(2.07)		(-1.02)
[1em] Branch FE	Yes	Yes	Yes	Yes
Observations	493	364	486	359

t statistics in parentheses

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

Comparison of baseline monthly wage income and proxy wealth scores for households with and without skilled participants in LIWP. In first and third column, compare households with any LIWP participants to households without LIWP participants. In second and fourth column, compare households with at least one skilled LIWP participant to households with only unskilled LIWP participants.

Table 2.12: Summary Statistics on 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)
N	80	55	77	59

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

Date on inequality from the 80 communities in the RCT with at least 10 household surveys with complete wage income data. The Gini coefficient for inequality calculated based on 12 randomly selected households. Because wage income is collected for the previous month, 20 treatment communities with no program wages during the past month are combined with control communities.

Table 2.13: Impact on Wage Income Inequality by Project Type

	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

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

Estimated impact of LIWP intervention within the past month on wage inequality, controlling for time trend of worsening inequality due to the crisis.

Table 2.14: Correlation Between Direct Benefit from Project Infrastructure and Proxy Wealth Scores

	Directly Benefit from Project					
	All	Low Female	High Female	Road	Water	Land
Proxy Wealth Score	0.0853*	0.184*	0.0183	0.138	0.150**	0.0120
	(2.00)	(2.15)	(0.48)	(1.68)	(2.35)	(0.23)
Project FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	330	120	210	113	195	131

t statistics in parentheses

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

The first column includes all 35 pre-crisis projects in the household sample. In the next two columns, the sample is split between low female share (less than 12% female jobs) and high female jobs (greater than or equal to 12%). Proxy wealth score is normalized to range from 0 to 1.

Table 2.15: All LIWP Projects by Intervention Type Category

	Number of Projects
Water Only	134
Water and Land	42
Water and Road	24
Land only	117
Land and Road	7
Land and Water	50
Road only	40
Road and Water	23
Road and Land	10
Observations	447

Most projects had multiple components, with the first component being the larger part of the project. Where projects are divided into three groups, the type of the first component is used.

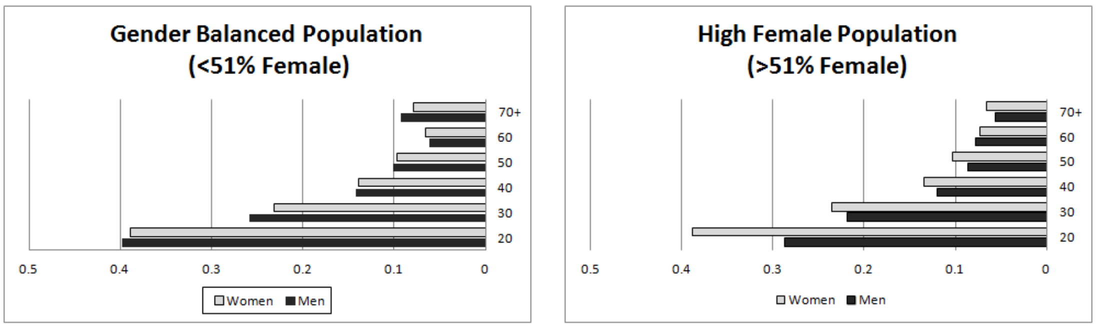
Table 2.16: Effect of the Crisis on Planned Female Employment

	Share of Planned Female Jobs	
	(1)	(2)
Post-Crisis	-0.0343	-0.0296
	(-1.04)	(-0.91)
Avg. Land		-0.00639*
		(-2.30)
Females as % of Pop.		-0.133
		(-0.46)
Branch FE	Yes	Yes
Observations	50	50

Only projects in the household survey sample are included, as assignment to treatment (pre-crisis) or control (post-crisis) is used to identify the impact of the impact.

Figures

Figure 2.1: Age Distribution by Gender in Communities with Low vs. High Female Shares



Age and gender distribution from household survey, split into two groups by share of female population.

Figure 2.2: Kernel Density of Ages of Men in Communities with Low vs. High Female Shares

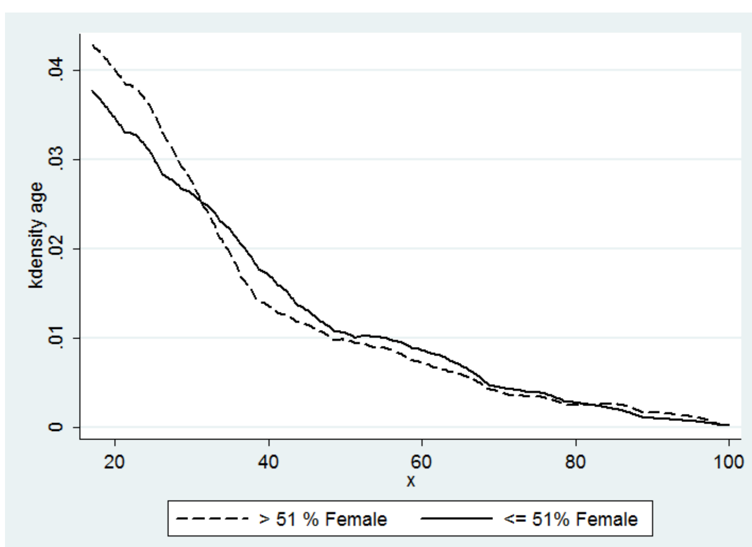
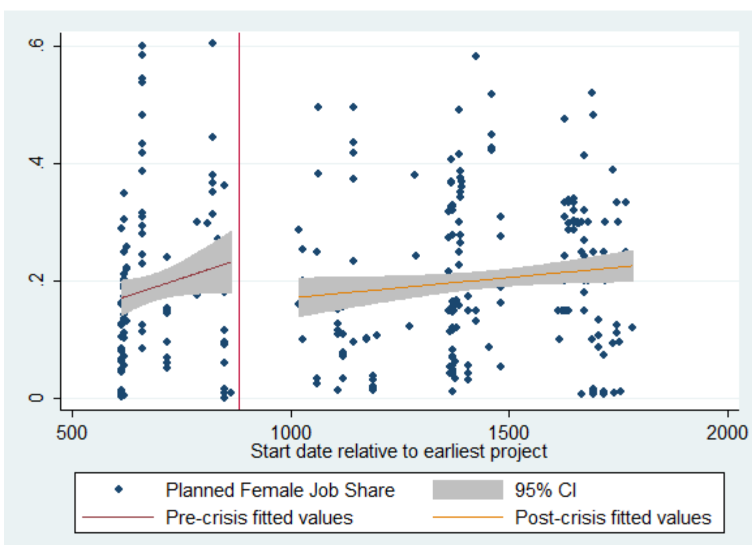


Figure 2.3: Inverse Relationship Between Share of Female Headed Households and Share of Female Jobs in Project

Observations are at the village level, weighted by the size of the village. Communities for the purpose of designing projects often were defined to include 2-3 neighboring villages. Data taken from MIS and village survey which accompanied the household survey for the RCT.

Figure 2.4: Planned Female Share by Project Date



Vertical line indicates January 2011, start of political upheaval in Yemen.

Chapter 3

Impact Evaluation of LIWP

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.

3.1 Introduction

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.

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.

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.

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 Employment and Income 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?

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.

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.

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	Resurveyed	Resurveyed
	Assigned	Actual treatment	Assignment respected
Communities	119 (60 C, 59 T)	84 (40 C, 44 T)	76 (38 C, 38 T)
All households (including replacement)	1428 (720 T, 708 C)	1004 (526 T, 478 C)	898 (443 T, 455 C)
Panel households	n/a	954 (461 T, 493 C)	854 (426 T, 428 C)

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.

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.

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.

3.2 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]

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.

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

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 3.1 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 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 3.2 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

[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.

implied daily wages as total LIWP income per participant divided by days worked. Figure 3.3 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 3.5, 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.

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.

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 3.2 summarizes participation rates at the branch level.[5] The amount of variation in benefits also differed by project branch, as shown in table 3.1, with Taiz and Amran having particularly high coefficients of variation for total benefits as well as days worked.

The regressions in table 3.3 indicate determinants of participation at the household level. We find that the number of men (especially men who are unemployed and underemployed

[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

[5]Each branch includes 4-10 communities

(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 3.4 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.

Overall Targeting of Benefits

Figure 3.4 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.

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 3.7 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 3.6, 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 (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.

Satisfaction with Program Implementation

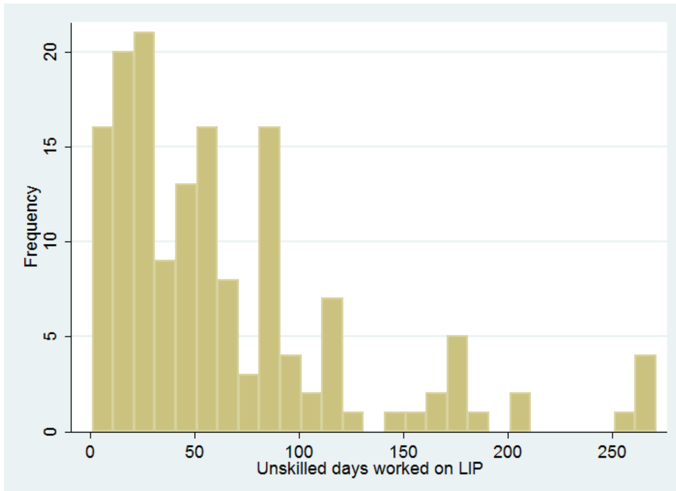
Satisfaction levels with the program implementation were generally high. Two of the areas where satisfaction was lower were regarding satisfaction with the timing and calculation of wage payments. Average responses are reported in tables 3.8 and 3.9.

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 3.10. Respondents could choose one of a list of possible reasons, or give a free response.

Table 3.1: Participation Rate by Branch

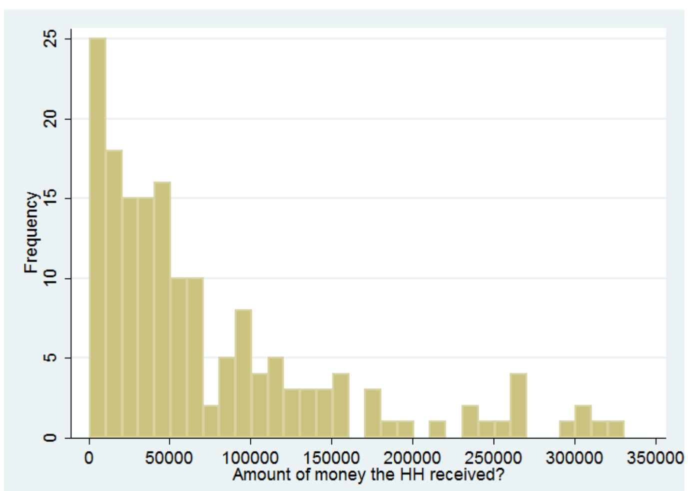
Branch	Participation Rate	Sample Size
Sana'a	87.5%	48
Aden	31.7%	60
Hodeidah	100%	24
Hajjah	69.0%	71
Mukallah	86.1%	72
Taiz	35.4%	48
Ibb	75.0%	24
Amran	59.3%	59
Dhamar	73.0%	48

Figure 3.1: Distribution of Unskilled Days Worked



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

Figure 3.2: Distribution of Money Received from LIWP



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

Figure 3.3: Distribution of implied wages for individuals with unskilled work in LIWP

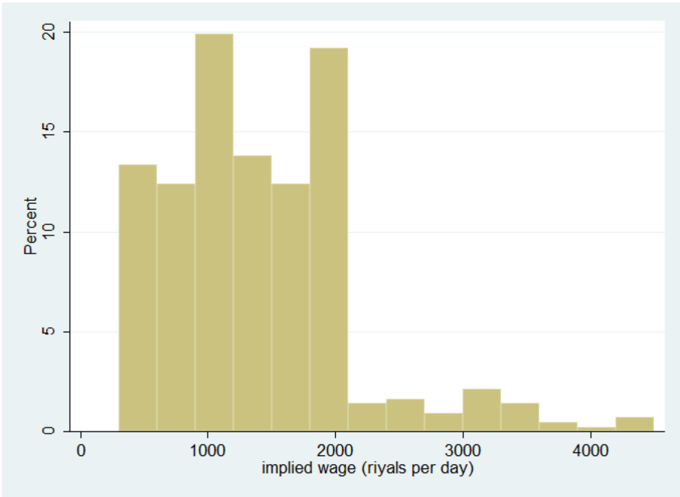


Figure 3.4: Distribution of Program Benefits by Poverty Score

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

Table 3.2: 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 3.3: 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 3.4: 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 3.5: 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 3.6: 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)
N	80	55	77	118

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

Table 3.7: 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 3.8: 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 3.9: 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 3.10: 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

3.3 LIWP Impact on Household Employment, Income, and Assets

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.

Days Worked

The summary statistics in table 3.11 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.[6] 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".[7]

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.[8] As seen in the regression results presented in table 3.12, 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 "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

[6] 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.

[7] 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.

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

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 3.13 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.[9]

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 3.5. 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.

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.

[9]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.

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 3.14 shows summary statistics on wage income in treatment and control communities in baseline and expost, and regression results are reported in table 3.16.

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 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.

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 3.17). [10] 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 3.18 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.

[10] 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.

We can also look visually at the change in employment by age group. The histograms in figure 3.6 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.[11]) 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.

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 3.19 and regression results in table 3.20.[12] 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 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.

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.

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

[12] 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.

Table 3.21 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 3.22), 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 3.23 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.

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 3.24 and 3.25) Average daily wages were calculated for all working individuals based days per month spent in each form of employment. [13] The impact is significant before separating between current programs 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. [14]

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." [15]

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 3.26 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

[13] 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.

[14] 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).

[15] 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)

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 3.27.

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 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.

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 3.30) 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 3.31. 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.

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 3.32 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 3.86)

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 3.34. Within households that owned any animals, however, the trend between baseline and expost was consistently negative. Regressions results are reported in table 3.35 for the extensive margin, and table 3.36 for the intensive margin.

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 3.37 and 3.38. 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 3.40. 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 3.39 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 3.40.

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 3.28 while regression results are presented in table 3.84.

Figure 3.5: Quantile-Quantile Plots of Days Employed per Year

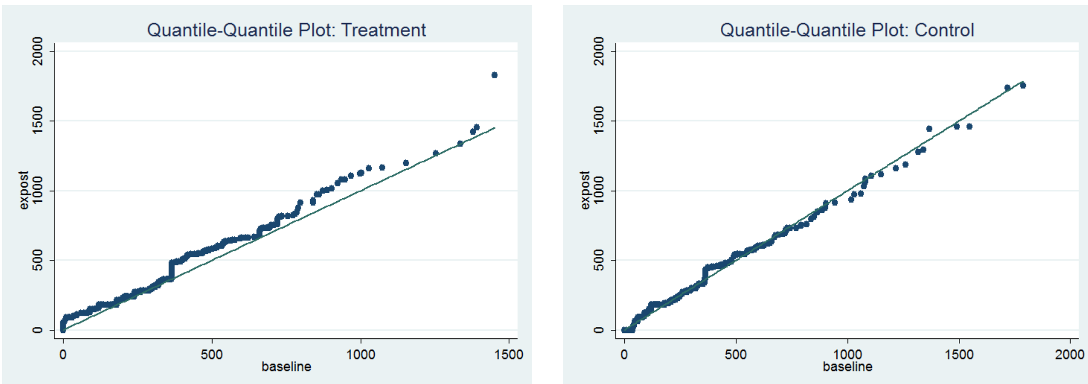
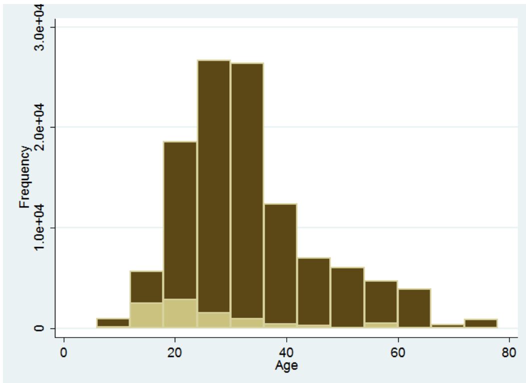
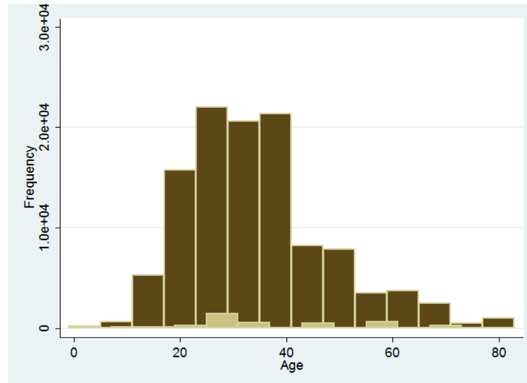


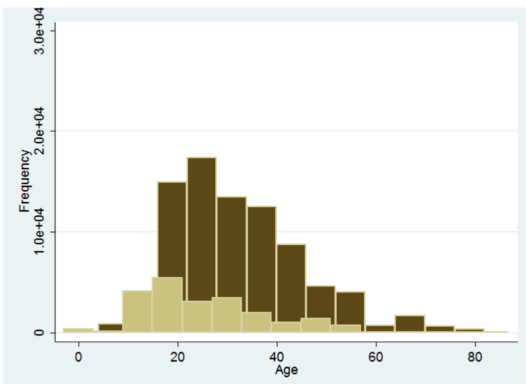
Figure 3.6: Age distribution of workforce weighted by days worked: males vs. females



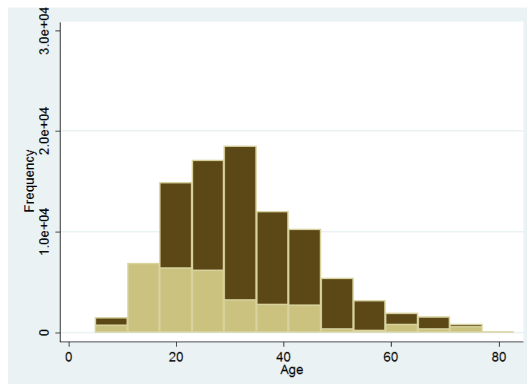
Control Baseline



Treatment Baseline



Control Expost



Treatment Expost

Table 3.11: Summary Statistics for Household Total Days Worked

	Days per month	Days per year	Days per month	Days per year	Corr. days per 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 3.12: 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 3.13: 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 3.14: 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 3.15: Household Wage Income

	Wage Income					
	Month	Month	Month	Month	Year	Year
LIWP Program	4921	4921	3684	3684	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 3.16: Regression Test for Program Effect on Household Total Wage Income

Table 3.17: 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 3.18: 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 3.19: Summary Statistics on Schooling by Age and Gender

	Male 5-14	Male 15-25	Female 5-14	Female 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 3.20: LIWP Impact on School Enrollment

	Male 5-14	Male 15-25	Female 5-14	Female 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 3.21: Summary Statistics on Employment by Category

	Control Pre-Crisis	Treatment Pre-Crisis	Control Post-Crisis	Treatment Post-Crisis
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 3.22: Summary Statistics on Wages by Category

	Control Baseline	Treatment Baseline	Control Expost	Treatment 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 3.23: LIWP Impact on Wages Level by Sector

	Coefficient on Independent Variable	
Dependent variable	LIWP	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

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

Coefficients from regression of wages on LIWP treatment and time trend after splitting the sample by sector of work. Includes community fixed effects.

Table 3.24: 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 3.25: LIWP Impact on Average Wages and Skill Level

	Wage	Wage	Wage	Wage	Wage	Skill
LIWP Program	291**	307**	190	175		-0.222**
	(130)	(156)	(164)	(177)		(0.096)
Expost	-183*	-165	-177*	-156		-0.164***
	(95)	(109)	(95)	(109)		(0.049)
Active in past month			172	226		0.095
			(134)	(157)		(0.086)
Avg. Skill					277***	
					(82)	
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 3.28: 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 3.26: Summary Statistics on Income Sources

	Wage Income	Ag. Prod.	Rental Income	Charity	Remit. Yemen	Remit. Abroad
Control Baseline	0.763	0.470	0.021	0.161	0.042	0.0851
Control Expost	0.609	0.709	0.040	0.406	0.11	0.119
Treatment Baseline	0.776	0.399	0.014	0.177	0.040	0.0867
Treatment Expost	0.625	0.688	0.036	0.344	0.091	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 3.27: LIWP Impact on Income Sources

	Wage Income	Ag. Prod.	Rental Income	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 3.29: 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 3.30: 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 3.31: LIWP Impact on Value of Household Durables Owned

	Total Value of Assets			
Value for Taxi/ Bus:	=750	=0	=500	=1000
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 3.32: Summary Statistics on Livestock Assets

	Cattle		Donkeys		Sheep or Goats		Chickens	Beehives
	own	share	own	share	own	share	own	own
Control	0.45	0.15	0.48	0.02	5.22	0.65	1.16	1.00
Baseline	(0.73)	(0.48)	(0.64)	(0.12)	(10.50)	(3.43)	(2.18)	(5.73)
Control	0.56	0.10	0.49	0.01	6.04	0.72	1.75	1.25
Expost	(0.89)	(0.36)	(0.67)	(0.09)	(10.00)	(4.31)	(3.44)	(7.89)
Treatment	0.39	0.11	0.36	0.01	4.98	0.50	0.98	0.69
Baseline	(0.73)	(0.38)	(0.58)	(0.11)	(9.79)	(2.23)	(2.01)	(7.15)
Treatment	0.53	0.077	0.39	0.01	5.94	0.37	1.73	0.17
Expost	(1.40)	(0.30)	(0.58)	(0.10)	(11.81)	(2.02)	(3.26)	(1.17)

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

Table 3.33: 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 3.34: 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 3.35: 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 3.36: 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 3.37: 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 3.38: 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 3.39: Number of households in debt by owner of debt

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

Table 3.40: 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 3.41: 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)

Table 3.42: Program 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

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

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

3.4 LIWP Impact on Household Expenditures and Food Security

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 3.43.

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 3.44. In figure 3.7, 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.

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 3.45 presents summary statistics and table 3.46 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.

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 3.47 and regression results in table 3.48.

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.

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. [16] 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. [17]

Looking at consumption per capita by grain (table 3.49), 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 3.8. 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 3.51. 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.

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

[17] 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)

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 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 3.52.

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 3.53), 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. [18] 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 3.9 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 3.54 and 3.55.

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 3.10 and 3.11, 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

[18] 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.

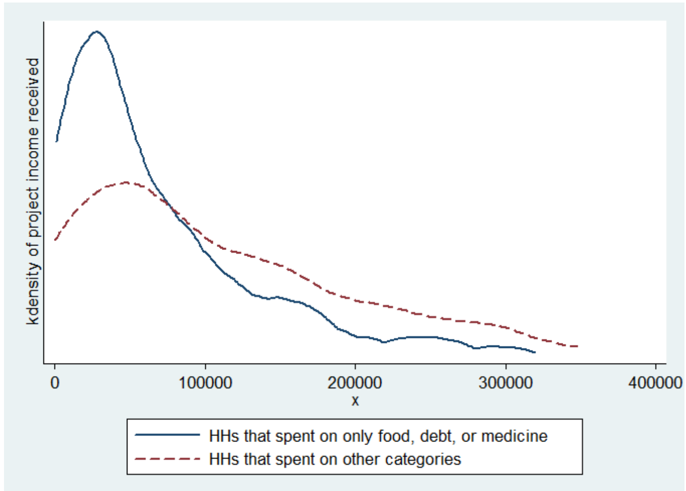
would consider a sufficient flag for omitting as physically impossible. Regression tests for program impacts including baseline values are extremely sensitive to the cutoff used for the exclusion of physically impossible unrealistic values that represent data entry error or enumerator error. A differences-in-differences 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 3.12 shows the distribution of ages in months in treatment and control villages.[19] 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 z-scores are lower in treatment communities. Table 3.56 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.

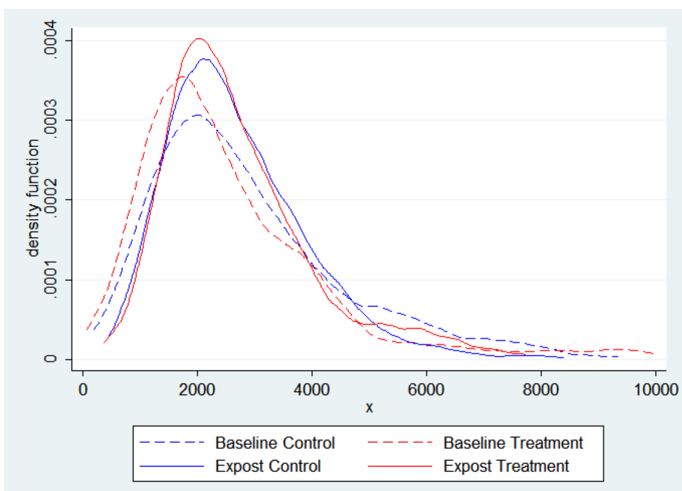
[19] Exact birthdays are usually not known, so most ages are rounded to the nearest year. For this reason, we focus on weight-for-height and bmi rather than weight-for-age measures, although the later would be more informative about nutritional status.

Figure 3.7: Density of Benefit Level By Type of Spending



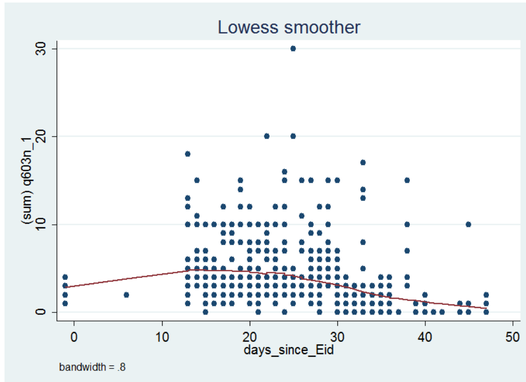
Kernel density of project benefits for households that reported spending *only* on food, debt, or medical care, vs. households that also reported spending on household goods, investments, clothes, gold, etc.

Figure 3.8: Density of Estimated Calorie Consumption per capita



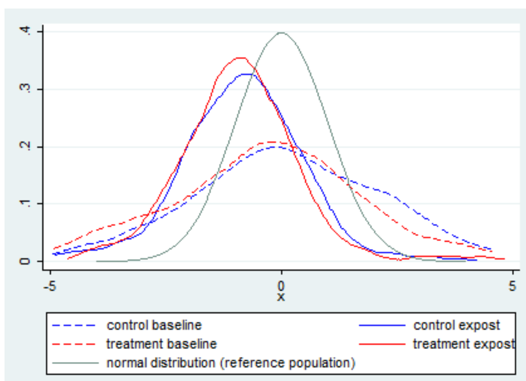
Distribution of estimated calorie consumption per capita. Variance is high, including many unrealistic values, and the distribution is skewed to the right.

Figure 3.9: 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 3.10: 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

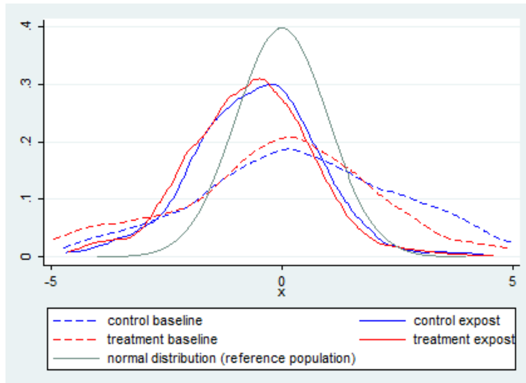


Figure 3.11: Distributions of Weight for Height for Children Age 0-5

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

Figure 3.12: Distributions of Age in Months for Children Age 0-5

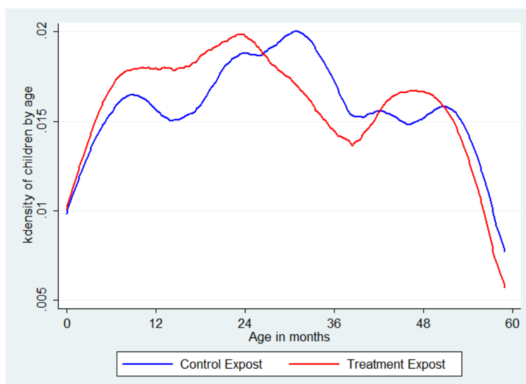


Table 3.43: Self-reported Use of LIWP Income by Participating Households

Food	94%
Equipment	4%
Animals	8%
Marriage	4%
Debt	40%
Home Improvement	3%
Furniture	4%
Other	4%

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

Table 3.44: Self-reported Use of LIWP Income (Continued)

Food, Debt, or Medical	59%
Food, Debt or Medical *and* Other	36%
Other *only*	4%

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

Table 3.45: Summary Statistics for Expenditure on Non-Food Consumption in Past Month

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

Table 3.46: LIWP Impact on Expenditure on Non-Food Consumption in Past Month (Continued)

	Qat	Tobacco	Medicine	Clothes	Bedding	Housing & 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 3.47: 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 3.48: Regression test for program effect on self reported food shortage and coping strategies

Table 3.49: 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 3.50: 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 3.51: Estimated average calories consumed per equivalent adult. Scaling of equivalent adults is based on relative calorie needs.

Table 3.52: 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	324.2	435.7*	324.6	0.13	0.13	0.11
	(273.2)	(259.4)	(226.1)	(0.09)	(0.08)	(0.08)
Expost	-282.9*	-345.3**	-244.8	-0.01	-0.03	-0.02
	(166.1)	(171.8)	(151.4)	(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 3.53: 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 3.54: 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 3.55: 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 3.56: Rate of Wasting Among Children

	Weight for Height	BMI
Control Expost	17.4%	12.9%
Treatment Expost	18.0%	15.7%
Test of proportions	0.74	0.22

3.5 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 3.57. 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 3.58.

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.

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 3.59 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 3.60.

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 3.61, while regression results are reported in 3.62.

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 3.63, 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 3.64

Table 3.57: 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 3.58: 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 3.59: 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 3.60: 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 3.61: 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 3.62: 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 3.63: 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 3.64: 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.

3.6 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.

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

	Assign to T	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 3.66: Tests for differences in average days worked per household at baseline

	Original balance	Switch C	Switch T	Attrition C	Attrition T	Final balance
Male	-0.00186	-0.00777	0.0116	-0.00953	-0.00581	-0.00329
employment	(-0.28)	(-0.45)	(1.26)	(-0.96)	(-0.57)	(-0.42)
Female	-0.0502**	-0.0499	-0.00441	-0.0431	0.0382	-0.0777**
employment	(-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 3.67: 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 3.68: 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 3.69: 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 3.70: 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 3.71: Number of jobs with unknown wages

	All	Self-Employed	Non-Self-Employed
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 3.72: Probit test of program effect on share of jobs with unknown wages

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

Table 3.83: 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 3.73: 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 3.74: 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 3.75: 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 3.76: Regression Test for Program Effect on Household Total Wage Income

Table 3.77: 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 3.78: LIWP Impact on School Enrollment

	Male age 5-14	Male age 15-25	Female 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 3.84: 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 3.79: 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 3.80: Includes community fixed effects.

Table 3.85: LIWP Impact on Value of Household Durables Owned

	Value of Durable Assets			
Value of Taxi/Bus	=750	=0	=500	=1000
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 3.81: LIWP Impact on Average Wages and Skill Level

	Avg. Wage	Avg. Wage	Avg. Wage	Avg. Wage	Avg. Skill
LIWP Program	252.7**		151.2		-0.112
	(105.9)		(120.7)		(0.071)
Expost	-164.0*		-164.0*		-0.200***
	(87.0)		(87.0)		(0.042)
Active in past month			198.1*		0.022
			(118.791)		(0.076)
Avg. Skill				277.4***	
				(82.7)	
Fixed effects	HH		HH	Comm	Comm
N	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 3.82: 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 3.86: LIWP Impact on Livestock Assets Owned

	Cattle		Donkeys		Sheep or Goats		Chickens	Beehives
	own	share	own	share	own	share	own	own
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 3.87: 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	-0.00	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 3.88: 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 3.89: LIWP Impact on Expenditure on Non-Food Consumption in Past Month (Continued)

	Qat	Tobacco	Medicine	Clothes	Bedding	Housing & 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 3.90: Regression test for program effect on self reported food shortage and coping strategies

Table 3.91: 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 3.92: 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 3.93: 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

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

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