ABSTRACT

Title of dissertation: CASH, TRAINING OR COMMITMENT? EVIDENCE ON BEHAVIORAL AND FINANCIAL INTERVENTIONS FROM DEVELOPING COUNTRIES

Mohammed Mehrab Bin Bakhtiar
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Dissertation directed by: Professor Kenneth Leonard
Department of Agricultural and Resource Economics

This dissertation analyzes how poor households in Africa and South Asia respond to large-scale policy experiments involving conditional and unconditional cash transfers as well as interventions that have training and behavioral components.

The first chapter, titled "Do Cash Transfers Improve Women's Agency? Evidence from Lab and Field Experiments", details results from lab-in-the-field experiments that I have designed and implemented for my PhD job market paper. The final version of this paper may include additional lab experiments and will be co-authored with Marcel Fafchamps, Markus Goldstein, Kenneth Leonard and Sreelakshmi Papineni. This chapter investigates whether receiving a cash transfer - in a very low-empowerment context - improves women's agency in household decision-making, particularly, in the longer term. I try to answer this question by combining

data from a female-targeted unconditional cash transfer (UCT) program with lab-in-the-field experiments. The UCT was designed as a Randomized Controlled Trial and was disbursed to adult women from ultra-poor households in rural Nigeria over a period of 15 months. The lab-in-the-field experiments were carried out on 503 married couples one year after the UCT program ended and can measure women's (as well as men's) agency in household decision-making. The main experimental measure of agency is the propensity to not defer decision-making to one's spouse. By randomly varying whether subjects' decisions can be observed by their spouses, a latent desire for agency (in the absence of spousal observation) versus an actual manifestation of one's agency (when decisions are observed by one's spouse) can be separately measured. I find that UCT-receiving women, one year after the completion of the program, defer 13 percentage point fewer decisions to their spouses; however, they do so only when their decisions cannot be observed by their spouses.

The second chapter, titled "Social and Financial Incentives for Overcoming a Collective Action Problem: Sanitation Underinvestment in Rural Bangladesh" is a co-authored paper with Ahmed Mushfiq Mobarak, Raymond Guiteras and Jim Levinsohn. Here, we study the effects of social and financial incentives on communities' ability to overcome collective action problems. Our specific context is a sample of 107 villages (approximately 19,000 households) in rural Bangladesh, and the collective action problem we study is investment in hygienic latrines and their subsequent maintenance and use. We randomized (1) whether and what type of incentive was provided - a financial reward (which was, essentially, a conditional cash transfer) or a non-financial "social recognition" reward, and (2) whether and what type of verbal commitment the households were encouraged to make - a private pledge vs. a public pledge. We measure short-term (3 months) and medium-term (15 months) effects and investigate the mechanisms behind the effects. Preliminary reduced form estimates suggest that a small financial reward has the strongest impact in the short term, inducing an approximately 12 percentage point increase in hygienic latrine ownership relative to 'control' villages, which received basic health messages. The public commitment treatment, in which members would publicly commit to have a hygienic latrine at the end of a group meeting, induced an approximately 5 percentage point increase in hygienic latrine ownership in the short term. In the medium term the effect of the financial reward dissipates; however, the smaller effect of the public commitment treatment in the short term continues to persist after 15 months. The results suggest that low-cost interventions which take advantage of social network dynamics have the potential to improve the effectiveness of group-meeting based rural health interventions common in developing countries.

The third chapter, titled "Training Mentors? Experimental Evidence from Female-Owned Microenterprises in Ethiopia" is a co-authored work with Gautam Bastian and Markus Goldstein. This paper reports findings from a randomized evaluation of a business training and mentoring intervention targeted at female-owned microenterprises in Ethiopia shows that formal business training produces an immediate impact on the adoption of beneficial business practices (that were highlighted in the training); however, no impact on business profit is observed in the short term. Three and four years after the training, we observe delayed impacts on business profits. Shortly after the training, the trained cohort is randomly assigned to provide mentoring to less-experienced women who own smaller businesses. These mentees were nominated by mentors at baseline from their own social networks. The overall impact of mentoring on mentees is more muted. There are early impacts on the adoption of beneficial business practices, and some measures of profits. However, the impacts on profits do not persist in the longer term.

CASH, TRAINING OR COMMITMENT? EVIDENCE ON BEHAVIORAL AND FINANCIAL INTERVENTIONS FROM DEVELOPING COUNTRIES

by

Mohammed Mehrab Bin Bakhtiar

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Advisory Committee:
Professor Kenneth Leonard, Chair/Advisor
Professor Anna Alberini
Professor Neslihan Uler
Professor Snaebjorn Gunnsteinsson
Professor Maureen Cropper

Dedication

Alhamdulillah.

For my mother, Anjuman Ara Begum, a wonderful amalgamation of fortitude, love and sincerity. Thank you for working so hard all your life to give me the best education possible.

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Table of Contents

Chapter 1: Do Cash Transfers Improve Women's Agency? Evidence from Lab and Field Experiments

1.1 Introduction

Growing empirical evidence from developing countries suggests that when financial decisions in poor households are made by women, savings and investment are often greater and repayment of debt is more likely.[1] Moreover, it has been documented that income given to women is more likely to be used towards improving educational and health outcomes of children.[2] Thus, in addition to having an intrinsic value on it's own (Fernandez et al. 2015), women's agency in household decision-making has an instrumental value for other dimensions. In reality, however, involvement of women in household decision-making processes is severely limited in many low-income contexts.

There is, therefore, a significant interest among policymakers and researchers to identify policy interventions that have the potential to improve women's agency

[1]De Aghion and Murdoch (2004) review findings that microfinance loans made to women are significantly more likely to be repaid. Duflo (2012) and [?] provide a detailed review on how household savings and investments are positively impacted if women have more say in household decision making.

[2]In particular, income given to women is more likely to be used for investments in education, children's nutrition, and housing than income in the hands of men (Thomas, 1994; Hoddinott and Haddad, 1995; Duflo, 2003).

in household decision-making in low-income contexts. Cash transfer (CT) programs and in particular, unconditional cash transfer (UCT) programs have received renewed attention as a tool for poverty alleviation in developing countries. Recent evidence on UCTs shows large short and medium-term effects on consumption and asset accumulation among other household outcomes (Haushofer and Shapiro, 2016; Blattman et al., 2013). Evaluations of CT programs in a variety of low-income contexts also show that transfers targeted to women decreases the incidence of intimate partner violence (IPV) in a significant way.[3] In several of these studies, an improvement of women's agency within the household has been considered as a likely channel for the observed reductions in IPV. However, there is limited evidence (and even less experimental evidence) on whether receiving cash transfers directly improves women's agency in household decision-making, particularly in the longer term.

In this paper, I investigate whether being the recipient of a cash transfer in a low-income and low-female-empowerment setting improves women's agency in household decision-making. I consider the potential effects of a UCT program in the longer term, when the immediate (and direct) income effect of the cash transfer is likely to have diminished. A 15-month-long UCT program in rural Nigeria provides an excellent setting to implement lab-in-the-field experiments to measure the effect of receiving the UCT on experimental measures of women's agency, one year after the program ended. The main measure of agency is the propensity to *not* defer

[3] Angelucci, 2008 and Bobonis et al., 2013 in Mexico; Hidrobo et al., 2016 in Ecuador; Perova & Vakis, 2013 in Peru; Haushofer & Shapiro, 2016 in Kenya.

decision-making to one's spouse, in the absence of any communication. The lab experiments allow for an additional measurement of agency when information on spousal preferences is available, i.e., to what extent a subject revises her initial decisions upon receiving spousal communication. Importantly, by varying whether a subject's decisions can be observed by his/her spouse, a *latent* (desire for) agency in the absence of spousal observation versus *manifested* agency, when decisions are observed by one's spouse, can be separately measured.

In a very low-empowerment setting, receiving a cash transfer may help women gain experience with exercising agency in household decisions (e.g., by deciding how to spend the cash). Moreover, UCT recipients may become more likely to engage in income-generating activities which could increase their share of household income and subsequently, their involvement in household decisions. This intuition has been formalized in the collective model of household decision-making which assumes that the wife's decision power increases when she obtains larger resources in terms of income and assets, by increasing her threat point utility in case of divorce or non-cooperative behavior within the household (Lundberg and Pollak, 1993; Browning and Chiappori, 1998). It is clear that there can be a number of mechanisms through which receiving a UCT could improve women's agency in household decision-making.

The context of this study is in the rural state of Kebbi, Nigeria. Historically, Kebbi is a region where women have experienced very low decision-making power and severely restricted mobility. Survey results from a recent Demographic and Health Survey can help illustrate this point. When asked about their involvement in decisions involving their own health care, major household purchases, and visits to family or relatives, less than 2% of married women in Kebbi report to have participated in all three decisions. Additionally, more than 90% of married women in Kebbi report to have participated in none of these decisions.[4]

A 15-month long unconditional cash transfer (UCT) program - which was targeted at women and was implemented as a Randomized Controlled Trial in Kebbi - provides us with a setting to test whether receiving cash for a period of time in a low female-empowerment context improves women's agency in household decision-making in the longer term. I investigate any potential impact of the UCT thirteen months after the cash transfer program ended. The direct income effect of the cash transfer is likely to have diminished by this point of time.

I make a methodological contribution towards the measurement of individual members' agency (relative to their spouses) in household decision-making through the use of lab-in-the-field experiments. Married couples from the Impact Evaluation (IE) sample of households of the aforementioned UCT program were invited to participate in incentivized decision-making processes that would yield an actual pay-off for the household based on their decisions within the experimental session. Experimental subjects, thus, had an incentive to reveal their true preferences. This is important because survey-based modules on household decision-making are often plagued with measurement issues. Besides the obvious sub-optimality of measures on stated preferences using surveys compared to those on revealed preferences in incentivized experiments, traditionally-used survey measures on household decision-

[4] Nigeria Demography and Health Survey (DHS), 2013: https://dhsprogram.com/pubs/pdf/fr293/fr293.pdf.

making often lack variation in responses.[5] This lack of variation has been considered as a possible explanation for the failure to observe direct impacts of cash transfer programs such as PROGRESA on measures of female involvement in household decision-making (Adato et al., 2000; Almas et al., 2018).

Several recent studies have proposed and tested alternative quantitative measures of women's empowerment within a household using experimental techniques. Lab-in-the-field experiments have been carried out in low-income settings to measure demand for agency and willingness to pay for agency in different types of joint decision-making within a household (e.g., Iversen et al., 2011; Jakiela and Ozier, 2015; Mani, 2011; Almas et al., 2018; Afzal et. al., 2018; Schaner, 2016; Abbink et al., 2018). There is, however, only a few studies that look into the effect of a cash transfer on experimental measures of female empowerment.

One such study has been done by Almas et al. (2018) who report results from a lab-in-the-field experiment in North Macedonia where a conditional cash transfer (for children staying in school) was randomly allocated to either the head of household (usually a man) or a woman. They find that the female recipients of this long-term conditional cash transfer (CCT) - compared to those whose husbands were chosen as the recipients - exhibit a lower willingness to pay (WTP) to receive a windfall (which is a smaller amount than the CCT) themselves instead of their spouses. This, they argue, is picking up an improved bargaining power of the CCT-

[5]Survey modules on household decision-making typically ask respondents to identify who within a household is in charge of certain decisions. Because the possible answer options are limited in scope - typically, some variation of "wife is in charge", "husband is in charge" or "both spouses make decisions jointly" - households often choose the "both" category as their response. This often results in a limited variation in data.

receiving women within their household – women who are willing to pay less than others have more say in the household. However, some other channels in addition to an improved bargaining power could explain why CCT-receiving women exhibit a lower WTP to receive a cash windfall (instead of their spouse).[6]

In everyday life, household members make decisions jointly across different decision domains. These include choice of privately consumed goods (e.g., goods specific to male and female household members), household public goods (e.g., goods commonly used by everyone in the household, such as bedspread and blankets) and decisions to allocate money among members within a household. In the controlled setting of a lab experiment in Nigeria, I document which household member's preferences dominate in these decision domains. Specifically, the experimental setting allows for the measurement of a subject's propensity to exert agency in household decision-making. By varying whether subjects' decisions can be directly observed by their spouses, the experiments can further measure whether there is a latent desire for agency in the absence of spousal observation (or 'latent agency') versus 'manifested agency' in household decision-making when decisions are observed by one's spouse.

The sample for the lab-in-the-field experiments consists of 503 married couples, i.e., 1,006 individuals in total, from 27 villages in the Northwestern state of Kebbi

[6]For example, fairness norms could explain why women who were beneficiaries of a CCT program are less likely to object to a new transfer to be given to their spouse instead of receiving it themselves again. Moreover, former recipients of a CCT might want to avoid 'taxation' from family and relatives (see Jakiela and Ozier, 2016) after experiencing it first-hand and may rather have their husbands deal with such attempts. This issue is salient in Almas et. al. (2018) because the information on being a recipient of the CCT program was common knowledge as the program was implemented in a visible/public way.

in Nigeria. 38 experimental sessions were carried out in 22 different locations. After arriving at the site, married couples were separated and placed in different rooms for the duration of the experimental games. In the beginning of the the lab-games a preference elicitation exercise is carried out, in which participants are asked to divide an endowment (usually, 2,500 Nigerian Naira/NGN) across several binary options or decision domains. Essentially, this forces subjects to make decisions in isolation, on behalf of their households, without allowing them to directly consult with their spouses. While quite artificial in nature, this nicely sets up later choice rounds in the experiment which can help measure subjects' agency in household decision-making.

The subsequent experimental rounds puts structure to the idea of agency in household decision-making in the following way. At first, 'executive agency' of household members is considered, which is defined in the literature as the power to make decisions autonomously within a household (Afzal et. al, 2018). Each subject is asked whether she[7] would like to use her own choice (that was made earlier) or would like to change her choice to the choice of her spouse, who has made similar decisions in another room. In other words, participants choose whether to defer decision-making to their spouse or exert agency by selecting their own choice. A lack of deferring of decision-making to one's spouse can be interpreted as higher executive agency of a household member.

The above option to defer decision-making to spouse (or not) is given to a

[7]Unless I am doing a comparative analysis by gender, I will use the female pronouns - "she/her" - for the experimental subject and the male pronouns - "he/him" - for the spouse, for the rest of the document.

subject without providing any information regarding their spouses' preferences. In a subsequent phase of the experiment, this information constraint is relieved by introducing spousal communication. Now, a participant is shown her spouse's communication and is also reminded of her earlier stated choice. She is then asked to make the allocation choice again. A larger difference in this new revision decision from her original preference (and a smaller difference with her spouse's communication) would presumably be indicative of a lower executive agency of a household member.

While not the main focus of the study, a second type of agency can be investigated through the experiments. Consultative agency in a household is defined in the literature as the extent of being consulted in household decisions. This form of agency allows individuals to inform the decision-maker about their preferences (Afzal et. al 2018). The design of the experiments allows for the measurement of consultative agency in household decision-making in two ways: how close a subject's communication choice (to spouse) is to her original preference and secondly, whether a subject's spouse chooses to consult with the communication sent to him. While the first measure would indicate how comfortable an individual feels in conveying her preferences to her spouse, the second measure reflects how much a subject's preferences are respected by her spouse.

In contexts with very low female agency and mobility, women may have a desire for agency but refrain from manifesting it for fear of retaliation from their spouse. In such a case, women maybe willing to exert agency but only if they can do so when their decisions are not being observed by their spouses. On the other hand, women may choose to not exert agency even when they are not being observed by their husbands because of an internalization of social norms, which may dictate women to have a dependent status and punish them for infringing the norms, i.e., through guilt or shame (Afzal et. al 2018). Moreover, repressed individuals may not even conceive that they could have their own free will (Alan and Artac, 2016).

In a traditionally low-empowerment setting for women like Kebbi, where women may not exert agency in a public way, it is important to distinguish between these two types of 'disempowerment'. Women may exhibit only a *latent* desire for agency - where they are willing to exert agency in secret, i.e., when they are not being observed by their husbands. On the other hand, women may have internalized a dependent status and as such, may not exert agency at all, whether or not they are being observed by their husbands. To distinguish between these two channels, I introduce a treatment in the lab-experimental design through which whether participants' decisions can be observed by their spouses (or not) is randomly varied. Through this "secret"/"no-secret" experimental treatment, a desire for agency in the absence of spousal observation versus an actual manifestation of agency under spousal observation can be separately measured.

These intuitions can then help formulate a model of women's agency with three distinct phases (also shown in Figure 1.1). The lowest phase of agency (or the 'internalized dependency status' phase) is one in which women have internalized a dependent status as dictated by social norms. Any possibility of infringing on these norms, say, by a woman's display of exerting agency, is punished by explicit retaliation by one's spouse through physical violence or a threat of violence (Roy, Hidrobo, Hoddinott and Ahmed, 2017). However, since these social norms have been internalized by women, they are mostly self-enforced, for example, through guilt and shame. Women, thus, do not display any desire for agency even when the possibility of sanctions are removed, e.g., by making their decisions unobservable to their spouses.

The second phase of agency (or the 'latent desire for agency' phase) is one in which social norms are still very much against any outward display of women's agency. However, unlike in the first category, such norms have not been internalized by women, i.e., there is a latent demand for agency. Women in this phase do not exert agency in a public way because of a fear of retaliation from their spouse. However, when the possibility of said retribution is removed - by making their decisions unobservable to their spouses - women choose to exert agency. The third and last category of agency (or 'full agency' phase) is when women can exert agency if they so choose and do not face any social sanction for doing so. There is no difference in their behavior with or without spousal observation - they exert high levels of agency in either situation.

I show that women in rural Kebbi exert very low levels of agency and that their behavior is consistent with women in the first phase/ 'internalized dependency status' phase in the model of women's agency outlined above. While they exhibit very low levels of agency when they are being observed by their spouses, women also refuse to exert agency when their spouses cannot observe their decisions. This indicates that women in this area have internalized the social norms of having a dependency status that discourages female agency within or outside households.

I hypothesize that an unconditional cash transfer (UCT) directed to a female recipient improves her agency in household decision-making in the longer run, when the immediate (and direct) income effect of the cash transfer is likely to have diminished. By varying whether subjects' decisions can be directly observed by their spouses, I also measure whether any potential improvement of women's agency due to the UCT is because of an increase in the latent desire for agency in the absence of spousal observation or because of an improvement in their ability to actually exercise agency in household decision-making when their decisions can be observed by their spouse.

If receiving the UCT improves women's desire for agency but only when their actions cannot be observed by their spouses, then it would indicate that they have moved from the first phase of 'internalized dependency status' to the second phase in the women's agency model - they now have a 'latent desire for agency'. Consistent with this observation, I find that UCT-receiving women (compared to non-recipients) exhibit higher levels of agency by deferring 13 percentage point fewer decisions to their spouse, 13 months after the UCT program ended; however, they do so only in the "secret" condition in which their spouses cannot observe their decisions. This suggests that receiving the UCT induced an increase in the latent desire for agency, i.e., UCT-receiving women value agency but do not exhibit it while being observed by their spouses, possibly due to a fear of retaliation.

It is important to acknowledge that the decision-making process in these lab-in-the-field experiments is clearly 'artificial'. The experimental sessions, essentially, force subjects to make decisions in isolation, on behalf of their households, without allowing them to directly consult with their spouses. This, of course, does not happen in reality. The research question that I am hoping to answer, however, requires the implementation of lab-in-the-field experiments for the following reasons. Firstly, an incentivized experiment is as close as one can get to elicit the 'true' preferences that each subject has about how to allocate resources across different decision domains in a household. Secondly, through observing the extent of deferring, revising and consultations, the experimental setting allows us to document which household member's preferences dominate the decision-making process within a household. Lastly, this setting lets us implement 'experimental treatments' that vary spousal observation of subjects' decisions, allowing us to test aforementioned competing theories of female disempowerment within households. None of these can be done effectively using survey-based methods.

1.2 Cash Transfer Program and Lab-experimental Sample

The World Bank's Africa Gender Innovation Lab (GIL) designed an unconditional cash transfer (UCT) that was carried out as a Randomized Controlled Trial by Catholic Relief Services (CRS) Nigeria in the north-western region of Nigeria. The UCT was implemented in two Local Government Authorities (LGAs) in the state of Kebbi. The original design of the UCT foresaw the payment of 75,000 Nigerian Naira (roughly USD 693 PPP) over a period of fifteen months ending in March 2017, delivered to the female primary decision maker in the household.

In April-June 2015, the GIL collected baseline data on 2,539 households in

Kebbi state who were identified as extremely vulnerable based on a Progress Out of Poverty Index (PPI) assessment. Using public lottery ceremonies held in June 2015, 1,269 of these households were randomly assigned to a treatment group who would receive the cash for a duration of 15 months and the remaining 1,270 households were assigned to a control group who would not receive the UCT . The level of randomization was at the household level - so, both the UCT/treatment households and non-UCT/control households were located within the same villages.

Villages were randomly assigned to become either a "UCT-Only" village or a "Program+UCT" village. In both types of villages, households were randomized into receiving or not receiving the UCT. However, in "Program+UCT" villages, all eligible households had access to most of the program components, which included nutrition messages, savings groups, farmer's groups, etc.[8] This means that the 'control' households in the "Program+UCT" villages were exposed to these programmatic components while the 'control' households in the (pure) UCT villages were not exposed to either the UCT or any programmatic component.

The impact evaluation sample is made up of rural, farming households with 80% of the working population engaged in crop and animal production as their primary income activity. The majority of households in the study sample fall below the USD 1.90/day poverty line athttps://www.overleaf.com/project/5d337dfd4118812221db81a4 baseline with 92% of households defined as "very poor" based on a measure of daily expenditures per capita. The two LGAs in the study are Birnin Kebbi and Danko

[8] Details of the different programmatic components can be found here: https://datacatalog.worldbank.org/dataset/nigeria-feed-future-nigeria-livelihoods-project-2015

Wasagu and are located in the state of Kebbi. Although these localities neighbor each other, Birnin Kebbi has a drier climate and villages are better connected to infrastructure than the villages in the Danko Wasagu LGA. At baseline, Birnin Kebbi households were found to be more land-wealthy and have higher daily per capita expenditures than households in Danko Wasagu. Although the majority of males in the two localities were involved in farming at baseline, females were more likely to be involved in agricultural work in Danko Wasagu than in Birnin Kebbi. On the other hand, more women were involved in small businesses and petty trade in Birnin Kebbi than in Danko Wasagu at baseline.

During the two-month period of April and May, 2018, I conducted lab-experimental sessions in Kebbi in which married couples from the UCT Impact Evaluation sample were invited to attend. The final experimental sample consisted of 503 households among which 252 households had received a UCT and 251 households which had not received a UCT. We targeted villages that had a school nearby where we could conduct the sessions. Up to 14 married couples could participate in each session. Depending on the sample size and availability of respondents in the nearby study village(s), we conducted one or two sessions in every experimental site. In total, 38 experimental sessions were carried out in 22 different sites. Participants from 27 villages attended the experimental sessions. Households were invited around four to five days before the day of the experimental session. Only married couples were invited to participate, i.e., households were told that potential participants should come only if both the husband and wife could arrive at the experimental session together.

The locations of the experimental sites were, usually, within walking distance from the household locations of the subjects. The sites were classrooms in schools located near the study village and sessions were conducted either on off-days or after regular school hours. Subjects spent around 2.5-3 hours at the experiment site. Each subject received a compensation of 500 NGN (or around $1.39 USD) (Nigerian Naira will be abbreviated as NGN in the rest of the document) for their time, which is higher than the average daily per capita income in this area. The participants were told that they could earn significantly more (money or in-kind) based on their decisions in the experimental games.

Table 1.2 provides disaggregated information of the experimental sample by geographical location. It shows that while the number of villages is similar across the two LGAs, the number of participating households is higher in Birnin Kebbi (n = 271) than in Danko Wasagu (n = 232).

1.3 Summary of Experimental Protocol

In this section, I summarize the protocol of the lab-in-the-field experiments. This can help understand how agency in household decision-making can be measured in this particular context through the proposed experiments. Detailed experimental protocol is available in Appendix B. The flowchart of the different decision rounds as well as the variables generated from the experiments are also reported in Appendix B.

Introductions and Payments: Married couples, who are from the Impact Evaluation (IE) sample of an Unconditional Cash Transfer program, arrive at the site. Potential subjects' identities are checked with a pre-defined list of household information. Upon arrival, male and female subjects are placed in separate rooms. Although they receive separate show-up fees, subjects are told that only one decision of all the decisions that a subject and his/her spouse will make could be chosen through a lottery as final pay-off for their household. A visual demonstration (involving colored and numbered balls that are put in a box) is shown which demonstrates how one decision - from all the decisions that will be made by the husband and the wife in the experimental session - could be chosen at the end by a lottery as final pay-off for the household. The decision rounds for female experimental subjects are represented with white numbered balls while the decision rounds for male subjects are represented by orange numbered balls. The numbers on the balls denote different rounds. Before each round a numbered ball is placed in a box. Subjects are told that at the end of the session if that particular ball is selected from the box, then the decision that they are about to make is likely to be implemented as a pay-off for their household. This is further explained through several 'example' rounds before the actual decision rounds begin.

Plausible deniability ("secret-keeping" round): The pay-off structure is designed to ensure that subjects have plausible deniability for any choice they make within an experimental session. This means that choices made during the experiment are not revealed to subject's spouse (or anyone else), unless warranted by the experimental treatment, as explained in the next discussion point. Plausible deniability is introduced in the following way.

Recall that at the end of a session, based on which numbered ball is randomly drawn out of the basket, a particular decision round would be chosen as pay-off for each household. This decision round could have been played by either the husband or the wife. The session supervisor would then find out the recorded decision (that corresponds to the selected numbered ball) and write the response on a piece of paper and insert it in an envelope. The envelope would then be put in a tin box.

One envelope randomly chosen from a bag that contained all the possible allocations written inside different envelopes, would also be put in the tin box. The tin box would then be shaken several times and one of the envelopes would be taken out, based on which the household would receive a pay-off.

Thus, either one of the decisions made by a participant or her spouse or a randomly chosen allocation (from all possible allocations) can be chosen as final pay-off for a household. This process offers plausible deniability to a subject for any choice she makes within the experimental session (unless otherwise stated). The possibility of a random allocation to be selected as the final pay-off is explained in the beginning of the session as the "secret-keeping" choice, which, unless otherwise explicitly stated, would keep the subjects' experimental decisions secret.[9]

Individual Preference Elicitation: A simple individual preference elicitation round is carried out in the beginning of the experimental session, i.e., as Round 1. This means that Round 1, irrespective of the randomly chosen order for the rest of the rounds in any given experimental session, is never a part of the randomization

[9] There is a 50% chance for one of the married couple's own preference decision to be implemented and a 50% chance for a completely random draw to be chosen as final pay-off.

process (that determines the order of the experimental rounds). Round 1 will also be referred to as the *Allocation* round. The decisions in this round involve dividing an experimental endowment (usually, 2,500 NGN) across different binary choices: (adult) male items vs. (adult) female items, (adult) male items vs. items commonly used by households (household public goods), (adult) female items vs. household public goods and finally, money allocation between self vs. spouse. We refer to these different binary choices as *decision domains*.

Order: The order of the rest of the rounds (after Round 1/Round *Allocation)* is chosen randomly for an experimental session.

Lab-Experimental Treatments: Recall that conditional on the availability of subjects from the UCT impact evaluations sample, each experimental site was targeted to have two consecutive experimental sessions. One session would be randomly selected to the "Secret" treatment while the other to the "No Secret" treatment. A married couple would always be in the same "Secret"/"No-Secret" treatment. In the "Secret" treatment, none of the decisions made by a subject could be observed by his/her spouse. This means that subjects in the "Secret" treatment have plausible deniability for all decisions made within the session. However, in the "No Secret" treatment, any decision made *after* the first round (i.e., the *Allocation* round*)* can be potentially observed by the participant's spouse. In other words, if any decision made by a subject after the *Allocation* round is chosen for pay-off for the household, his/her spouse would be informed what the decision was.

Thus, a subject's original allocation decisions in the first round can never be observed by one's spouse (or anyone else) - irrespective of the experimental treatment a subject is selected into. This is due to the possibility of the "secret-keeping" choice to be chosen as the pay-off, i.e., a random choice of allocation from all possible choices to be drawn as the final pay-off (explained above under "Plausible deniability") instead of one's own decision.

Defer: At the end of the individual preference elicitation round (or the *Allocation* round) and depending on the randomly chosen order of the later rounds, there is a *Defer* round, in which subjects decide whether to use their own earlier choice (made in the *Allocation* round) or the decision made by their spouses in another room, as the final decision for that round.

Communication: Subjects also play a *communication* round, in which they are asked to communicate their (allocation) preferences to their spouses, before the latter make an allocation decision again.

Consultation and Revision: Moreover, subjects play a *consultation* round in which they are asked whether they would like to see their spouses' *communication* choice before making the allocation decision again. They are then given the option to make a *revision* - they can opt to keep their original decision or they can make a new decision - in light of observing their spouses' (*communication*) choice.

Allocative and Consumption efficiency: Subjects play games that measure consumption and allocative efficiency within the household. The idea is to see whether subjects make efficient decisions for the household, i.e., do not 'leave anything on the table' while making decisions on consumption and allocation for the household. Specifically, subjects decide between an earlier allocation choice made by self vs. a similar choice made by spouse but with either a larger or a smaller endowment. In this way, the cost of deferring decision-making to subjects' spouse (or conversely, the cost of asserting agency) is varied.

Choice on immediate and private consumption within the experimental session [round: lab-consumption]: After being shown two types of cookies and two types of drinks, subjects are asked to decide which combination of drink and cookie they would like to immediately consume and which combination they would like their spouses to consume. Subjects are then told that their spouses have also been asked to select a food and drink for them (the subjects) to consume. Each subject can now choose to either: (a) use the choice she made earlier or (b) change her choice to her spouse's choice. For some sessions, the cost of deferring this decision-making to spouse (or conversely, the cost of asserting agency) is changed by offering half the amount for choice (a).

Norm elicitation and 'guessing' round: At the end of the above rounds, participants play an incentivized norm elicitation and 'guessing' round, in which they can get a small pay-off (200 Nigerian Naira) for correctly guessing the number of times a randomly chosen participant from the same session has deferred to his/her spouse; as well as guessing the response to different questions on 'socially appropriate' behavior of men and women of a randomly matched person from the same room. One of all the 'guesses' from this round is randomly chosen for pay-off for the subject. This is in addition to the payment that a household can potentially receive from one of the decisions made within the session by a subject or his/her spouse from that household.

Pay-off: At the end of the session, a lottery is carried out to determine the pay-off for each household. If the pay-off is monetary (e.g., husband would receive x NGN and the wife y NGN), each subject is given the pay-off separately (this is explained to them in the beginning of the experiment); however, for items/goods based pay-off for the household, married couples are brought together and are given tokens for items which they can get from the 'experimental' shop.

The sequence of the rounds during the experiment is explicated through a flowchart added in Appendix B. A summary table of the constructed variables on agency is also added in the same appendix.

1.4 Agency in the Context of Northern Nigeria

Kebbi and northern Nigeria, broadly, is a region where women, historically, have experienced low decision-making power, severely restricted mobility and very high rates of maternal morbidity and mortality. Previous empirical work in this region have enumerated factors which are usually associated with low empowerment of women: a perceived social need for women's reproductive capacities to be under strict male control; the practice of purdah (wife seclusion), which restricts women's access to medical care; almost universal female illiteracy; marriage at an early age and pregnancy often occurring before maternal pelvic growth is complete; a high rate of obstructed labor; directly harmful traditional medical beliefs and practices (Ogu and Okonofua, 2016; Braimah, 2014; Wall, 1998).

To understand the context and the study population better, this section documents how individual agency is distributed between married men and women in the cash transfer Impact Evaluation (IE) sample. For this exercise, only households which were (potentially) unperturbed by the UCT or any other NGO services associated with the program are considered, i.e., the 'control' households of the RCT which were randomly selected to *not* receive a cash transfer. With no violations of the SUTVA,[10] this would allow us a peek into the natural state of the world, in the absence of the cash transfer treatment.

Table 1.3 reports the means of the different measures of individual agency elicited through the experiments, across male and female subjects from the 'control' households of the cash transfer RCT. Women in this sample, compared to men, appear to exhibit very low levels of agency across different domains of individual and household decision-making and across different experimental measures. I discuss the descriptive statistics on these measures of agency below.

Executive Agency

Recall that executive agency in a household is defined in the literature as the power to make decisions autonomously (Afzal et. al 2018). The experimental setting offers subjects the option to exercise agency by not deferring decision-making to their spouse. Specifically, I investigate to what extent subjects defer decision-making to spouse (and thus, choose not to exercise agency) when they do not receive any information regarding spousal preferences. Rows (1) and (2) in Table 1.3 report the means of deferring of decision-making to spouse, i.e., subjects' propensity to choose their spouse's decision instead of their own.

[10] The Stable Unit Treatment Value Assumption requires that the response of a particular unit depends only on the treatment to which the unit was assigned to, not the treatments of others around it.

Row (1) is the rate of deferring of decision regarding the type of food/drink for own immediate consumption within the lab session. Note that approximately 75% of females defer this decision to their spouse, while only 18% of males do the same. Row (2) is the mean of deferring different household allocation and consumption decisions to one's spouse. Note that the numbers are similar to those in Row (1) - female subjects defer around 70% of decisions to their husbands, while male subjects defer 22% of their decisions to their wives. Together, these statistics show that adult married females in the study area exhibit very low levels of agency.

Recall that the decisions on deferring as described above are made without any information regarding spousal preferences. A separate experimental round provides further insight into the executive agency of subjects by relieving this information constraint regarding spousal preferences. The variable in Row (3) is constructed by considering whether a 'revision' decision (after receiving spousal communication) is different than a subject's original decision for each decision domain and then computing the mean across the different domains. Recall that a lack of revising of original preferences would be indicative of higher executive agency of a participant, which is not the case for the female sample. After receiving a communication from their spouse, female subjects revise their earlier allocation decision approximately 70% of the time. This is starkly different than the behavior of male subjects who do the same only around 14% of the time.

The variable in Row (4) in Table 1.3 is constructed by considering whether a subject's 'revision' decision (after receiving spousal communication) is exactly the same as the amount communicated by one's spouse. This can be considered to be similar to deferring, but in the presence of spousal communication. A lack of executive agency of the female subjects is again apparent with this statistic. Female subjects revise their earlier allocation decision to their spouses' communicated amount around 58% of the time. Male subjects do the same only about 16% of the time.

Consultative Agency

As discussed earlier, consultative agency in a household is defined in the literature as the extent of being consulted in household decisions. Consultative agency allows individuals to inform the decision maker about their preferences (Afzal et. al 2018). The experimental design allows for consultative agency to be measured in household decision-making in two ways: (a) how close a subject's communication (to spouse) is compared to her original preference/decision and (b) whether a subject's spouse chooses to consult with the subject's communication before making a decision.

For (a), a small (or no) difference between the original preference and the communication sent to one's spouse would, arguably, be indicative of higher consultative agency of an individual. The variable in Row (5) of Table 1.3 is constructed by considering whether a communication decision to the spouse is different than the originally stated preference for each decision domain and then computing the mean across the different domains. Note that around 25% of females choose to communicate an allocation to their spouse which is different than their originally stated preferences. For the male sample, this number is much lower at 9%.

For (b), if a subject chooses to consult with the communication sent by her spouse, then the spouse, arguably, has higher consultative agency. The variable in Row (6) is constructed by considering whether an individual wants to consult with his/her spouse's communication decision before making a decision again. Similar to the other measures, I calculate the mean across different domains. Notice again, that male subjects only want to see their spouses' communication 20% of the time, suggesting a relatively low consultative agency of the female subjects. On the other hand, female subjects want to 'consult' with their spouses around 53% of the time, suggesting a higher consultative agency of the male subjects.

1.5 Internalized Dependency Status vs. Latent Desire for Agency: An Investigation of Untreated/Non-recipient Households

The previous sub-section shows that women in this study area, compared to men, exhibit very low levels of agency across different domains of individual and household decision-making and across different experimental measures. In such a low-empowerment setting, women may have a desire for agency but refrain from manifesting it for fear of retaliation from their spouse. In such a case, women maybe willing to exert agency but only if they can do so when their decisions are not being observed by their spouses. On the other hand, women may choose to not exert agency even when they are not being observed by their husbands because of an internalization of social norms, which may dictate women to have a dependent status and punish them for infringing the norms, i.e., through guilt or shame.

The experimental design allows us to distinguish between these two types of female 'disempowerment' by varying whether subjects' decisions can be observed by their spouses or not. On the one hand, women may exhibit higher levels of *latent* agency when they are not being observed by their husbands. On the other hand, women may have internalized a dependent status and as such, may not exert agency at all, whether or not they are being observed by their husbands.

In Figure 1.1, I outline a simple model of women's agency which incorporates these ideas. The lowest phase of agency (or the 'internalized dependency status' phase) is one in which women have internalized her status as a dependent to her spouse to make decisions on her behalf, as dictated by social norms. They do not exhibit any agency, whether or not they are being observed by their spouse. Women in the second phase of agency (or the 'latent desire for agency' phase) do not exert agency in a public way because of a fear of retaliation from their spouse. However, when the possibility of said retribution is removed - by making their decisions unobservable to their spouses - women choose to exert agency. In other words, women in this phase have a latent desire to exercise agency. In both these phases, social norms are very much against any display of women's agency.

The third and last category of agency (or 'full agency' phase) is when women can exert agency if they so choose and do not face any social sanction for doing so. There is no difference in their behavior with or without spousal observation - they exert high levels of agency in either situation. This phase is not relevant to our study context.

The previous sub-section details how women in rural Kebbi exhibit very low levels of agency. This is consistent with the behavior of women in both the first phase and second phases of agency in the outlined model. In other words, it is possible that women in this area may have internalized a dependency status as dictated by social norms and do not exhibit agency, even when given a chance to do so in secret. On the other hand, it is possible that they exert agency only when they are not observed by their spouses, indicating their latent desire for agency. This can be investigated econometrically with the following equation:

$$y_{isv} = \beta_0 + \beta_1 \text{NoSecret}_{isv} + \varphi_v + \varepsilon_{1isv} \tag{1.1}$$

where y_{iv} is a participant's decision to defer decision-making to his/her spouse in household i, session s and village v. φ_v is village fixed effects, and ε_{1isv} is the error term, which is clustered at the session level. Because we seek to understand the behavior of this sample in the absence of the cash transfer, only the non-recipient/control households are considered for this analysis.

Table 1.4 shows the effect of being in the "no-secret" condition on deferring of decision-making to spouse. The outcome variables in Columns (1) and (2) are the means of 'deferring' variables across different decision domains. In Column (3), the outcome variable is the decision to defer the choice of juice flavor and cookie type (for own immediate consumption within the lab session).[11] The coefficient estimates in Table 1.4 suggest that women in "no-secret" condition do not defer decisions at a different rate than those in the "secret" condition across different

[11] Note that the number of observations for Column (3) is fewer because we only report results for sessions in which the cost of deferring this decision-making was zero, i.e., sessions in which participants chose between (full amount of) own choice and (full amount of) spouse's choice of cookies and drinks, for own immediate consumption.

decision domains. In other words, even when they are given a chance to do exert agency in secret, women choose not to do so. This indicates that women in this area have internalized a dependency status as dictated by social norms and they do not exhibit agency, whether or not they are being observed by their spouse.

1.6 Investigation and Hypotheses

Investigation A: Does being a recipient of a UCT change preference?

I do not have a strong hypothesis about whether being a UCT recipient changes preferences in the longer-term.[12] On the one hand, if receiving the UCT shifts permanent wealth/income positively (because of investments in productive assets - such as livestocks - that earns a female recipient and her household higher future income) then it is possible for preferences to shift as a result. On the other hand, it is entirely plausible that preferences do not change drastically because of the cash transfer in the longer term.

The decisions made in the first round of the lab-experiments, i.e., in the *Allocation* round, is vital for the experimental setup. Firstly, this is as close as one can get to elicit the 'true' preferences that each subject has about how a windfall amount should be spent across competing decision domains that may affect household members differently (for example, male vs. female items).[13] Secondly, and more importantly, this round sets up the subsequent rounds nicely which can

[12] Recall that the lab-in-the-field experiments were carried out around a year after the UCT ended.

[13] Subjects may take the preferences (and utility) of their spouse and other household members into account while making the allocation decisions.

help measure subjects' agency in household decision-making. For example, in the *Defer* round, subjects are asked to decide between either their own choice or their spouses' choice (both made in the *Allocation* round). In the *Revision* round, spousal communication is introduced (along with a reminder of their earlier decision in the *Allocation* round) before subjects are asked to make a revised decision. Thus, these later decision rounds would not be possible without the first round of preference elicitation.

The decisions in the *Allocation* round are made under complete privacy and subjects have plausible deniability regarding any decision made within this round. These allocation decisions, therefore, cannot be used to measure subjects' bargaining power or bargaining weights as traditionally observed in the literature. This is because theoretical and empirical work on intra-household decision-making usually infer such weights from household consumption decisions, which are *not* made in artificial lab conditions such as privacy and plausible deniability.

Hypothesis 1: In the longer term, UCT-receiving women, compared to non-recipients, are less likely to defer decision-making to their spouse.

When asked to choose between own decision and spouse's decision in the absence of any spousal communication, UCT-receiving women are expected to be more likely to choose their own decision in the longer term, i.e., they are less likely to defer decision-making to their spouses. This is because receiving a UCT may lead women to gain experience with exercising agency (e.g., by deciding how to spend the cash transfer money) which, in the longer-term, can increase their involvement in household decision-making.

Moreover, it is possible that UCT recipients become more involved with income generating activity compared to non-UCT women. This in turn could make them more comfortable with making decisions on money allocations within the household - which could be reflected in the experimental games. However, it is clear that there can be a number of mechanisms at work through which receiving a UCT could improve women's agency in household decision-making. I do not have any strong prior regarding the dominance of a particular channel over others.

To avoid any potential issue with multiple hypothesis testing, I create an index variable that combines the decision to defer to spouse across all the decision domains. Each decision domain is weighted equally in creating this index.

Investigation B: Is there a different rate of deferring of decision-making by women, when observed by their spouse and does it vary by UCT status?

In other words, do UCT women defer fewer decisions than non-UCT women in both the secret and non-secret conditions?

Recall that there are two experimental treatments – one of which is randomly chosen for any given experimental session. In the "Secret" condition, participants are assured of complete privacy/secrecy for all of their decisions within the experiment. In the "No-Secret" condition, participants are told after the *Allocation* round (play in the very beginning of each session) if one of their subsequent decisions (such as deferring or revising) is chosen as pay-off, it will be revealed to their spouse at the end of the session what their decision is. In other words, subjects' spouses can observe their decisions.

As observed earlier, women may have a *latent* desire for agency, but refrain from manifesting it for fear of retaliation from their spouse. Receiving the UCT might increase this latent desire for agency but not the actual manifestation of it. In such a case, UCT women would defer fewer decisions than non-UCT women but only in the secret condition.

I further investigate any differential impact of the UCT across the the "Secret"/ "No-Secret" condition on the rate of deferring to spouse by varying the cost of deferring. The experimental design allows within-subject variation in the cost of deferring with three different costs of deferring.

(i) *no cost of deferring*: this is the basic measure for deferring; subjects choose either their own choice or their spouse's choice, where both choices were made with the same endowment amount

(ii) *cost of deferring is decreased/cost of choosing own decision is increased:* subjects choose either their spouse's choice made with a larger endowment of 2,500 NGN or their own choice made with a smaller endowment of 2,100 NGN; in other words, a cost is imposed on subjects if they choose their own decision

(iii) *cost of deferring is increased/cost of choosing own decision is decreased:* subjects choose either their own choice with a larger endowment of 2,500 NGN or their spouse's choice made with a smaller endowment of 2,100 NGN; in other words, subjects are rewarded to choose their own decision

Hypothesis 2: After receiving communication about spousal preferences, UCT-receiving women, compared to non-recipients, are more likely to choose an allocation that is closer to their original preference.

When the information constraint regarding spousal preferences is relieved with an introduction of spousal communication, and subjects are asked to make an allocation decision again, we expect UCT-receiving women to choose an allocation amount closer to their earlier stated preference.

Again, it would be instructive to see whether any effect of the UCT on the likelihood to revise is driven by the “secret”/“no-secret” condition. If receiving the UCT only increased women’s *latent* desire for agency then it is likely that UCT-receiving women will choose an allocation closer to their earlier allocations but only in the “secret” condition when their decisions cannot be observed by their spouses.

1.7 Results

Investigation A: Does being a recipient of a UCT change preference?

To investigate this econometrically, I estimate the reduced form effects of the Unconditional Cash Transfer with the following equation

$$y_{iv} = \beta_0 + \beta_1 \text{Cash}_{iv} + \varphi_v + \varepsilon_{1iv} \tag{1.2}$$

where y_{iv} is the allocation for {female items (vs. male items), female items (vs. household public goods), household public goods (vs. male items), self (vs. spouse)} made by a subject in household i and village v. φ_v is village fixed effects, and ε_{1iv} is the error term, which is not clustered, since the UCT was randomized at the household level.

Results for female subjects are presented in Table 1.5. The outcome variable in each of Columns (1) - (4) is a decision that that involved dividing an endowment of 2,500 Nigerian Naira (NGN) towards one of two options; for example, the outcome variable in Column 1 denotes how much out of 2,500 NGN is allocated for female items instead of male items. Similarly, Column (4) denotes how much money out of 2,500 NGN is allocated for self instead of one's spouse. The results show that some preferences change in response to the UCT. Female recipients of the UCT seem to allocate around 80 NGN more towards female items and towards themselves, instead of male items and their spouses, respectively. These results are statistically significant at 5% level. Allocations involving male-specific or female-specific items versus household public goods do not show large or statistically significant movements.

Hypothesis 1: In the longer term, UCT-receiving women, compared to non-recipients, are less likely to defer decision-making to their spouse.

This hypothesis can be empirically tested with the following equation, which looks exactly the same as Eq. (1.2):

$$y_{iv} = \beta_0 + \beta_1 \text{Cash}_{iv} + \varphi_v + \varepsilon_{1iv} \tag{1.3}$$

where y_{iv} is a participant's decision to defer decision-making to his/her spouse in household i and village v. φ_v is village fixed effects, and ε_{1iv} is the error term, which again, is not clustered, since the UCT was randomized at the household level.

Here, I consider the effect of the UCT on deferring of decision-making to spouse when deferring is *costless*. Deferring can be considered costless when participants decide between their own choice or their spouse's choice and both these choices were made with the same endowment amount (which was usually 2,500 NGN in the experiment). Put differently, the cost of choosing own decision is also zero. Results for female subjects are presented in Tables 1.6 and 1.7. Table 1.6 reports the effect of the UCT on the mean of 'deferring' variables across different decision domains, while Table 1.7 reports results for the individual decisions separately.

In Column (1) of Table 1.6 the outcome variable is the mean of the binary variables of deferring across four decisions: (a) female vs. male items, (b) female vs. everyone's items, (c) male vs. everyone's items and (d) money allocation between self and spouse. The coefficient estimate in Column (1) suggests that UCT-receiving women, on average, defer 9.5 percentage point fewer decisions to their spouse than non-UCT receiving women. This coefficient estimate is statistically significant at <1% level. A similar effect is observed for the regression coefficient in Column (2) where the outcome variable is the mean of the binary variables of deferring across the three household consumption decisions listed above, i.e., (a) female vs. male items,(b) female vs. everyone's items and (c) male vs. everyone's items.

The coefficient estimate in Column (3) is similar in magnitude to the other coefficients in the table, although it becomes less precise. Table 1.11 reports the effect of the UCT decision to defer the choice of juice flavor and cookie type when the cost of choosing own decision is increased (i.e., cost of deferring is decreased).

Investigation B: Do women defer decision-making to their spouses at a different rate when their decisions are observed by their spouse and does this vary by UCT status?

Econometrically, we can look into the differential effect of the UCT in the "secret"/"no-secret" condition with the following equation:

$$y_{isv} = \beta_0+\beta_1(\text{NoCash} + \text{NoSecret})_{isv}+\beta_2(\text{Cash} + \text{Secret})_{isv}+\beta_3(\text{Cash} + \text{NoSecret})_{isv}+\varphi_v+\varepsilon_{1is} \tag{1.4}$$

where y_{isv} is the outcome variable of interest (e.g., deferring of decision-making to spouse) for household i, in experimental session s, in village v. The omitted category is (NoCash+Secret). φ_v is village fixed effects. ε_{1is} is the error term, which is clustered at the session level since the experimental treatment of "Secret"/"No Secret" condition was randomized at the session level. The main results in Tables 1.9- 1.10 are for the female sample.

(i) *deferring is costless*

Tables 1.9 and 1.10 summarize the effect of the UCT, in relation to the "Secret"/No-Secret" experimental treatment, when the cost of deferring (and conversely, the cost of choosing own decision) is zero. The results in these tables indicate that the previously reported effects of the UCT on women's rate of deferring of decision-making to their spouses (as seen in Tables 1.6 and 1.7) is mainly driven by the "Secret" condition. Recall that in the "Secret" condition, experimental subjects' decisions within the experimental session cannot be observed by their spouses; i.e., subjects have plausible deniability in their decisions within the experiment.

As shown in Columns (1) and (2) of Table 1.9, when the cost of deferring decision-making to one's spouse is zero, UCT-receiving women defer around 12-13 percentage point fewer decisions to their spouses, but only in the "Secret" condition. In the “No-Secret” condition, UCT-receiving women defer more decisions to their spouse; however, the regression coefficients are not statistically significant. Table 1.10 unpacks these results by reporting the regression coefficients separately for each of the decision domains aggregated in Columns (1) and (2) of Table 1.9. UCT-receiving women defer around 14-16 percentage point fewer decisions to their spouses in three of the four decisions, but again, only in the “Secret” condition. The magnitude of the effect is muted and statistically less precise for the decision on female versus everyone’s items.

Column (3) of Table 1.9 shows the coefficient estimate for the choice to defer decision-making regarding food and drink for own immediate consumption when the cost of deferring to spouse is zero. For this particular decision, the result suggests that there is no statistically significant difference in the rate of women’s deferring behavior across the different variations of UCT/non-UCT recipients and “Secret/No Secret” conditions.

(ii) *cost of deferring is decreased/cost of choosing own decision is increased*

In a separate experimental round, the cost of choosing one’s own decision is increased (i.e., the cost of deferring is decreased) by offering subjects to choose between an earlier allocation choice made by themselves versus a choice made by their spouse but with a larger endowment. The difference in the endowment size for own versus spouse’s choice is, essentially, the cost of exercising one’s agency.

Tables 1.11 - 1.13 summarize the effect of the UCT, in relation to the “Secret”/No-Secret” experimental treatment, when the cost of deferring is decreased (and conversely, the cost of choosing own decision is increased). Column (1) in all these tables reports results when the cost of deferring is zero to benchmark the results in the other columns, where it is not.

Table 1.11 shows the coefficient estimate for the choice to defer decision-making regarding own immediate consumption when the cost of deferring is decreased (i.e., cost of choosing own decision is increased). As mentioned earlier, the result in Column (1) suggests that there is no statistically significant difference in the rate of women's deferring behavior across the different variations of UCT/non-UCT recipients and "Secret/No Secret" conditions. However, Column (2) suggests that when the cost of deferring is decreased by allowing participants to consume only half the amount if they choose their own choice of juice flavor and cookie type, UCT-receiving women deferred around 14.9 percentage point fewer decisions to their spouses but do so only in the "Secret" condition.

I then consider decision-making regarding household consumption of adult male and female items when the cost of deferring is decreased. Again, for benchmarking purposes, Column (1) of Table 1.12 reports results when deferring decision-making to one's spouse is costless. It can be seen that UCT-receiving women deferred around 14 percentage point fewer decisions to their spouses, but only in the "Secret" condition. On the other hand, as reported in Column (2), when the cost of deferring is decreased (and conversely, the cost of exercising agency is increased) UCT-receiving women defer around 13 percentage point fewer decisions to their spouses but do so only in the "Secret" condition. Subsequent results in Column (2) of Table 1.13 show that the magnitude of the effects and statistical precision reported in Table 1.12 appear to be similar when the decision domain is on money allocation between self and spouse.

(iii) *cost of deferring is increased/cost of choosing own decision is decreased*

Column (3) of Table 1.12 reports the differential effect of the UCT across "secret"/"no-secret" condition when the cost of deferring is increased (and conversely, the cost of exercising agency is decreased). UCT-receiving women defer around 13 percentage point fewer decisions to their spouses but do so only in the "Secret" condition. The effect size appears to be remarkably similar across the different costs of deferring. But taken together with the omitted category mean, it can be seen that the rate of deferring is moving in the expect direction with cost. For example, when deferring is costless (Column 1 of Table 1.12), approximately, 53% of women in "Cash+Secret" category defer to their spouse. When deferring is cheaper (or exerting one's agency is more expensive), the rate of derring goes up, as one would expect - approximately, 61% of women in the "Cash+Secret" category defer to their spouse. On the other hand, when deferring is more expensive (Column 3), the rate of deferring goes down - only 44% of women in this category defer.

Hypothesis 2: After receiving communication about spousal preferences, UCT-receiving women, compared to non-recipients, are more likely to choose an allocation that is closer to their original preference.

If receiving the UCT only increased women's *latent* desire for agency then it is likely that UCT-receiving women will choose an allocation closer to their earlier allocations but only in the "secret" condition when their decisions cannot be observed by their spouses.

The results Table 1.14 indicate that the previously reported negative effects of the UCT on rates of deferring in the "Secret" condition is driven by women's desire to choose an allocation closer to their earlier allocations, instead of choosing exactly what their husbands ask them to do. UCT-recipient women, thus, choose to exert agency when they are not observed by their spouses. Specifically, UCT-receiving women are 12 percentage point *less* likely to change from their earlier stated preferences; however, they do so only in the "secret" condition.

1.8 Discussion: Program Spillovers

In the UCT program, beneficiary households were selected through a four-stage process involving the identification of vulnerable communities, followed by a community-based identification of vulnerable households and finally the use of a version of the "Progress out of Poverty Index" (PPI) to rank relative vulnerability. All of the households with PPI data were assigned to one of three vulnerability categories. The Extremely Vulnerable (EV) category was defined as the most vulnerable 16 percentiles of households in each ward (i.e. those with the highest PPI scores) which added up to 2,500 households across Kebbi state as required based on budget constraints and power calculations for the impact evaluation of the cash transfers experiment. The Very Vulnerable (VV) households were defined as the 17th to 85th most vulnerable percentiles and Market Limited (ML) households were the 15 least vulnerable percentiles of the PPI score distribution.

A public lottery was utilized to randomly assign eligible households in the extremely vulnerable (EV) category to either "cash" or "no cash" categories. Each EV household had a 50% chance of receiving a cash transfer. Because of household-level randomization of the cash transfer program, I focus on the within-village treatment effect for all of the econometric analyses.

The cash transfers in this project did not come with hard conditions but since households were told by traditional leaders that the money was for the female, the content during the sensitization campaign could have influenced the female's ability to keep more of the cash. Since the payments were made in a common area in each village, the amount and timing of the cash transfer were likely to be known by the woman's household and to others in the village. There is, thus, a possibility of sharing the received cash among treated households' informal risk-sharing arrangements (IRSAs), which is not an uncommon strategy in village economies to mitigate idiosyncratic and negative household-level income shocks within one's social network (Townsend, 1994). However, data from the midline survey of the impact evaluation of the UCT program shows that on average, female recipients kept approximately 54% of the cash for herself and gave most of the remaining cash to other people within her household (Bastian, Goldstein and Papineni, 2019). On average, cash-transfer receiving women shared 26% of the total transfer with her husband and 14% with her children. Direct spillovers of the cash to other households seem limited with only 4% being shared with friends or family in the same village and 2% outside the village.

Even with limited direct spillovers through sharing of cash between treatment and control households, spillover effects can occur through a change in perception regarding women's role in household decision-making and their involvement in economic activities. Earlier, I have discussed the specific local context of the program in which women, historically, have experienced low decision-making power and severely restricted mobility. It is possible that observing women receiving cash and potentially getting engaged in income-generating activities can improve perceptions regarding women's involvement household decision-making. This, in turn, can improve bargaining outcomes of women even among control households which did not receive any cash transfer. Unfortunately, the experimental (RCT) design of this particular cash transfer program only allows for within-village treatment effect to be measured. This means that the estimated effects are likely a lower bound of the true treatment effects.

The ideal design of the impact evaluation to measure spillover effects precisely would involve a two-stage randomization, one at the village level, resulting in treatment and control villages, and the other at the household level, resulting in "treatment" and "spillover" households in treatment villages, and "pure control" households in control villages. Neither the "spillover", nor the "pure control" households receive any cash. If spillovers are local, such a randomization at the level of villages can allow for the estimation of the program spillover effect within villages, i.e., by comparing the outcomes of the "spillover" and the "pure control" households.

One aspect of the current design of the program that can be explored further to understand spillovers is the extent of local treatment density. The main impact evaluation of the UCT has detailed data on GPS location of all the sample households, as well as information on the size of neighborhoods and villages. By aggregating all this information, one can create a variable on the proportion of households, within a pre-specified distance, δ, that received a cash transfer for each control household, and thereby create a measure of treatment density. With the reasonable assumption that such a measure is uncorrelated with any household-level unobservable, we can investigate whether outcome variables (from lab-in-the-field experiments) correlate with treatment density (for non-recipient/control households). A positive and statistically significant relationship between treatment density and an outcome variable would make a case for non-trivial spillover effects to neighboring non-recipient households.

1.9 Conclusion

Cash transfer programs and in particular, unconditional cash transfers (UCTs), have received renewed attention as a tool for poverty alleviation in developing countries. Who within a household receives a UCT is an important question that can influence the decision-making power within a household in the longer term.

To answer this question, I have designed and implemented a series of lab-in-the-field experiments on the impact evaluation sample of a female-targeted UCT. The UCT was carried out as a Randomized Controlled Trial in the state of Kebbi, a region in Nigeria, where women, historically, have experienced low decision-making power and severely restricted mobility. These lab-in-the-field experiments can measure agency of household members in household decision-making. The main measure of agency is the propensity to not defer decision-making to one's spouse, in the absence of any communication. I further measure the extent to which subjects revise their initial decisions upon receiving spousal communication. By randomly varying whether subjects' decisions can be observed by their spouses, a latent desire for agency (in the absence of spousal observation) versus an ability to actually exercise one's agency (when decisions are observed by one's spouse) can be separately measured.

The results from this study show that one year after the UCT program ended, UCT-receiving women appear to exercise higher levels of agency in household and (own) immediate consumption decisions. However, this appears to take place only in the "secret" condition where they have plausible deniability regarding their decisions. Specifically, I find that UCT-receiving women defer 13 percentage point fewer decisions to their spouse but they do so only when their decisions cannot be observed by their spouses. The effect persists across different decision domains and across different costs of exercising agency.

These results indicate that receiving the UCT induced an increase in the latent desire for agency, i.e., women value agency but do not exhibit it while being observed by their spouses. Thus, the observed effects of the UCT on women's longer-term agency do not appear to be transformative. However, given the very low levels of female empowerment in the context of Kebbi, Nigeria, any improvement in agency by women - even in secret - is still an important metric of success for potential policy interventions in low-income and low-empowerment settings for women.

1.10 Tables and Figures

Figure 1.1: A Model of Women's Agency

Phase 1: Internalized dependency status	*Phase 2: Latent desire for agency*	*Phase 3: Full agency*
○ social norms dictate women to have a dependent status ○ infringing these norms are punished by retaliation/threat of violence ○ however, these norms have been internalized and are mostly self-enforced, i.e., through guilt and shame ○ women have no desire for agency even when the possibility of sanction is removed (e.g., when their husbands can't observe their behavior)	○ social norms still against any outward display of women's agency ○ infringing these norms are punished by retaliation/threat of violence ○ however, such norms have not been internalized by women ○ women have a desire for agency but refrain from manifesting it for fear of sanction, i.e., exert agency when not being observed by spouse ○ when the possibility of sanction is removed, women exert agency	○ social norm not necessarily against any display of women's agency; or such female-disempowering social norms are not enforced; ○ women can exert agency and do not face any social sanction for doing so ○ women exert high levels of agency whether being observed by their spouses or not ○ efficiency consideration possibly drives women's decisions, such as deferring to husbands

Exp. Treatment	Num. of Sessions	Num. of Households
Secret	19	255 (50.7%)
No Secret	19	248 (49.3%)
Total	38	503 (100.0%)

Table 1.1: Lab-experiments Sample by Local Government Area (LGA).

LGA	Num. of Sessions	Num. of Villages	Num. of Households
Birnin Kebbi	20	14	271 (53.9%)
Danku Wasagu	18	13	232 (46.1%)
Total	38	27	503 (100.0%)

Notes: Nigeria has 774 local government areas (LGAs) in 36 states. Both Birnin Kebbi and Danku Wasagu are LGAs in the northwestern state of Kebbi.

Table 1.2: Lab-experiments Sample by Local Government Area (LGA)

Experimental Measures of Agency	Female Means	Male Means
(1) Defer to Sp.: Own Immed. Consump. Decision	0.750	0.181
(2) Defer to Sp.: HH. Consump. & Alloc. Decision	0.706	0.221
(3) Revision (after Sp.'s Communication) $\neq$ Orig. Preference	0.703	0.137
(4) Revision (after Sp.'s Communication) = Sp.'s Communication	0.576	0.155
(5) Communication to Sp. $\neq$ Orig. Preference	0.245	0.091
(6) Would Like to See Spouse's Communication	0.537	0.200

Notes: The computed means of different experimental measures of agency are reported separately for males and females. Only 'pure control' households which were randomly selected to *not* receive the UCT or any other service in the study area have been used for the calculations. Variables in Rows (1)-(4) denote measures for executive agency, while those in Rows (5)-(6) are measures for consultative agency.

Table 1.3: Experimental Measures of (Lack of) Agency for Pure Control Households - Mean Values

	(1) Mean of Deferring (HH Cons.+ Alloc.) Decis.	(2) Mean of Deferring HH Cons. Decis.	(3) Defer: Juice/Cookie Type for Immed. Consump.
No-Secret	-0.043	-0.054	0.095
	(0.060)	(0.065)	(0.077)
Secret Mean	0.725	0.725	0.707
Number of households	190	190	116
Number of sessions	29	29	18
Number of villages	20	20	12

* for p<0.10, ** for p<.05 and *** for p<.01

Notes: All regressions include village fixed effects. SEs are clustered at the level of experimental session.
The outcome variable in each column is deferring of decision-making to spouse.
The specific question on deferring to spouse was: "Recall the decision that you made earlier which involved [...]. Your spouse has also been asked to make the same decision in the other room. You can choose to either: use the choice you made earlier OR change your choice to your spouse's choice."
For Column (1) the outcome variable is the mean of the binary variables of deferring across four decisions: (a) female vs. male items (b) female vs. everyone's items (c) male vs. everyone's items (d) money allocation between self and spouse. For Column (2), the outcome variable is the mean of the binary variables of deferring across the three household consumption decisions listed above, i.e., (a) female vs. male items, (b) and (c).
For Column (3), the outcome variable is the decision to defer the choice of juice flavor and cookie type (for own immediate consumption within the lab session) to one's spouse. Note that the number of observations for Column (3) is less because we only report results for sessions in which the cost of deferring this decision-making was zero, i.e., subjects choose between (full amount of) own earlier choice or (full amount of) spouse's choice of cookies and drinks for own immediate consumption. In the remaining sessions, a cost is imposed for choosing own earlier choice, i.e., subjects choose between (*half* the amount) of own earlier choice or (full amount of) spouse's choice of cookies and drinks for own immediate consumption.

Table 1.4: Reduced Form Effects of *No-Secret* Experimental Treatment on Deferring to Spouse (Non-UCT Female Sample)

	(1) Female (vs. Male Items)	(2) Female (vs. Everyone Items)	(3) Everyone's (vs. Male Items)	(4) Allocation - Self (vs. Spouse)
Cash	80.544**	23.910	40.429	76.824**
	(33.248)	(35.864)	(36.030)	(35.538)
Omitted category mean	1409.761	1332.400	1396.215	1264.542
Number of households	503	502	503	503
Number of sessions	38	38	38	38
Number of villages	27	27	27	27

* for p<0.10, ** for p<.05 and *** for p<.01

Notes: The outcome variable in each of Columns (1) - (4) is a decision that involved dividing an endowment of 2,500 Nigerian Naira (NGN) towards one of two options; for example, the outcome var. in Column (1) denotes how much out of 2,500 NGN is allocated for female items instead of male items. These decisions are carried out in the very beginning of the experimental session in the preference elicitation round under complete privacy and plausible deniability.
All regressions include village fixed effects. SEs are not clustered since the UCT was randomized at the household level.

Table 1.5: Reduced Form Effects of Receiving Cash Transfer On Preferences (Female Sample)

	(1) Mean of Deferring (HH Cons.+ Alloc.) Decis.	(2) Mean of Deferring HH Cons. Decis.	(3) Defer: Juice/Cookie Type for Immed. Consump.
Cash	-0.097***	-0.095***	-0.085*
	(0.034)	(0.035)	(0.050)
Omitted category mean	0.672	0.671	0.685
Number of households	503	503	337
Number of sessions	38	38	25
Number of villages	27	27	17

* for $p<0.10$, ** for $p<.05$ and *** for $p<.01$

Notes: All regressions include village fixed effects. SEs are not clustered (since the UCT was randomized at the HH level).
The outcome variable in each column is deferring of decision-making to spouse when it is *costless*, i.e., participants decide between own earlier choice or spouse's choice and both these choices were made with the same endowment amount.
The specific question on deferring to spouse was: "Recall the decision that you made earlier which involved [...]. Your spouse has also been asked to make the same decision in the other room. You can choose to either: use the choice you made earlier OR change your choice to your spouse's choice."
For Col.(1) the outcome variable is the mean of the binary variables of deferring across four decisions: (a) female vs. male items (b) female vs. everyone's items (c) male vs. everyone's items (d) money allocation between self and spouse. For Col.(2), the outcome variable is the mean of the binary variables of deferring across the three household consumption decisions listed above, i.e., (a) female vs. male items, (b) and (c). For Col.(3), the outcome variable is the decision to defer the choice of juice flavor and cookie type (for own immediate consumption within the lab session) to one's spouse. Note that the number of observations for Col.(3) is less because we only report results for sessions in which the cost of deferring this decision-making was zero, i.e., participants choose between (full amount of) own earlier choice or (full amount of) spouse's choice of cookies and drinks for own immediate consumption. In the remaining sessions, a cost is imposed for choosing own earlier choice, i.e., participants choose between (*half* the amount) of own earlier choice or (full amount of) spouse's choice of cookies and drinks for own immediate consumption.

Table 1.6: Reduced Form Effects of Receiving Cash Transfer on Deferring to Spouse (Female Sample)

	(1) Defer: Female or Male Items	(2) Defer: Female or Everyone Items	(3) Defer: Male or Everyone Items	(4) Defer: Money Alloc. Self or Spouse
Cash	-0.106**	-0.095**	-0.098**	-0.098**
	(0.045)	(0.042)	(0.042)	(0.043)
Omitted category mean	0.673	0.665	0.681	0.673
Number of households	450	503	503	503
Number of sessions	34	38	38	38
Number of villages	24	27	27	27

* for p<0.10, ** for p<.05 and *** for p<.01

Notes: All regressions include village fixed effects. SEs are not clustered (since the UCT was randomized at the HH level).
The specific question on deferring decision-making to spouse was: "Recall the decision that you made earlier which involved [...]. Your spouse has also been asked to make the same decision in the other room. You can choose to either: use the choice you made earlier OR change your choice to your spouse's choice."

Table 1.7: Reduced Form Effects of Receiving Cash Transfer on Deferring to Spouse (Female Sample)

	(1) Female (vs. Male Items)	(2) Female (vs. Everyone Items)	(3) Everyone's (vs. Male Items)	(4) Allocation - Self (vs. Spouse)
NoCash + NoSecret	-44.445	2.813	-51.246	16.016
	(44.159)	(57.160)	(55.937)	(61.373)
Cash + Secret	15.548	22.745	30.504	51.522
	(54.152)	(41.461)	(41.211)	(61.904)
Cash + NoSecret	74.341	13.696	17.959	102.812*
	(56.994)	(71.358)	(55.826)	(57.855)
(NoCash + Secret) Mean	1434.583	1330.252	1422.917	1255.833
Number of households	503	502	503	503
Number of sessions	38	38	38	38
Number of villages	27	27	27	27

* for p<0.10, ** for p<.05 and *** for p<.01

Notes: The outcome variable in each of Column 1-4 is a decision that involves dividing an endowment of 2,500 Nigerian Naira (NGN) across one of two options; for example, the outcome var. in Column 1 denotes how much is allocated for female items out of 2,500 NGN, instead of male items, which gets the rest. These decisions are carried out in the very beginning of the experimental session in the preference elicitation round (or round: *Allocation*) under complete privacy and plausible deniability.
In *Secret* condition, for any round of decision-making, spouses cannot observe participant's decisions within the experiment, i.e., subjects have plausible deniability in their decision-making by the potential selection of either one of their own decisions or their spouse's decisions or a randomly chosen allocation (from all possible outcomes) as final pay-off for their household. However, in the *No-Secret* condition, spouses can observe subjects' decisions for most of the rounds, i.e., if any round after the first round, i.e., round: *Allocation*, is selected as final pay-off for the household, the actual decision by a participant will be disclosed to his/her spouse.
All regressions include village fixed effects. SEs are clustered at the level of experimental session.

Table 1.8: Reduced Form Effects of *No-Secret* Experimental Treatment and Receiving Cash Transfer on Preferences (Female Sample)

	(1) Mean of Deferring (HH Cons.+ Alloc.) Decis.	(2) Mean of Deferring HH Cons. Decis.	(3) Defer: Juice/Cookie Type for Immed. Consump.
NoCash + NoSecret	0.003	-0.001	0.060
	(0.063)	(0.067)	(0.088)
Cash + Secret	-0.134***	-0.125**	-0.086
	(0.045)	(0.051)	(0.080)
Cash + NoSecret	-0.023	-0.031	-0.032
	(0.070)	(0.074)	(0.101)
(NoCash + Secret) Mean	0.670	0.671	0.658
Number of households	503	503	337
Number of sessions	38	38	25
Number of villages	27	27	17

* for $p<0.10$, ** for $p<.05$ and *** for $p<.01$

Notes: All regressions include village fixed effects. SEs are clustered at the level of experimental session.
The outcome variable in each column is deferring of decision-making to spouse when it is *costless*, i.e., subjects decide between own earlier choice or spouse's choice and both these choices were made with the same endowment amount.
The specific question on deferring to spouse was: "Recall the decision that you made earlier which involved [...]. Your spouse has also been asked to make the same decision in the other room. You can choose to either: use the choice you made earlier OR change your choice to your spouse's choice."
For Column (1) the outcome variable is the mean of the binary variables of deferring across four decisions: (a) female vs. male items (b) female vs. everyone's items (c) male vs. everyone's items (d) money allocation between self and spouse. For Column (2), the outcome variable is the mean of the binary variables of deferring across the three household consumption decisions listed above, i.e., (a) female vs. male items, (b) and (c).
For Column (3), the outcome variable is the decision to defer the choice of juice flavor and cookie type (for own immediate consumption within the lab session) to one's spouse. Note that the number of observations for Column (3) is less because we only report results for sessions in which the cost of deferring this decision-making was zero, i.e., subjects choose between (full amount of) own earlier choice or (full amount of) spouse's choice of cookies and drinks for own immediate consumption. In the remaining sessions, a cost is imposed for choosing own earlier choice, i.e., subjects choose between (*half* the amount) of own earlier choice or (full amount of) spouse's choice of cookies and drinks for own immediate consumption.

Table 1.9: Reduced Form Effects of *No-Secret* Experimental Treatment and Receiving Cash Transfer on Deferring to Spouse (Female Sample)

	(1) Defer: Female or Male Items	(2) Defer: Female or Everyone Items	(3) Defer: Male or Everyone Items	(4) Defer: Money Alloc. Self or Spouse
NoCash + NoSecret	-0.002	0.028	-0.034	0.015
	(0.075)	(0.082)	(0.073)	(0.066)
Cash + Secret	-0.143**	-0.093	-0.161***	-0.157***
	(0.063)	(0.065)	(0.056)	(0.047)
Cash + NoSecret	-0.053	-0.038	-0.022	0.003
	(0.084)	(0.086)	(0.081)	(0.075)
(NoCash + Secret) Mean	0.673	0.650	0.700	0.667
Number of households	450	503	503	503
Number of sessions	34	38	38	38
Number of villages	24	27	27	27

* for p<0.10, ** for p<.05 and *** for p<.01

Notes: All regressions include village fixed effects. SEs are clustered at the level of experimental session.
The specific question on deferring decision-making to spouse was: "Recall the decision that you made earlier which involved [...]. Your spouse has also been asked to make the same decision in the other room. You can choose to either: use the choice you made earlier OR change your choice to your spouse's choice."

Table 1.10: Reduced Form Effects of *No-Secret* Experimental Treatment and Receiving Cash Transfer on Deferring to Spouse (Female Sample)

	(1) Def.: Own Choice (Full) Sp.'s Choice (Full)	(2) Def.: Own Choice (Half) Sp.'s Choice (Full)
NoCash + NoSecret	0.060	-0.039
	(0.088)	(0.053)
Cash + Secret	-0.086	-0.149**
	(0.080)	(0.061)
Cash + NoSecret	-0.032	-0.054
	(0.101)	(0.067)
(NoCash + Secret) Mean	0.658	0.951
Number of households	337	166
Number of sessions	25	13
Number of villages	17	10

* for p<0.10, ** for p<.05 and *** for p<.01

Notes: Subjects were asked to choose from 2 different types of cookies and 2 different kinds of drinks to consume within the lab session. The outcome variable in Columns (1) and (2) is the decision to defer the choice of juice flavor and cookie type (for own immediate consumption) to one's spouse, i.e., spouse's choice is selected instead of own choice.
The outcome variable in Column (1) is the basic variable for deferring decision-making to spouse, i.e., when cost of deferring is zero. Here, subjects choose between the full amount, i.e., 2 cookies and 1 glass of juice, of own earlier choice or full amount of spouse's choice.
In Column (2), a cost is imposed for choosing one's own earlier choice, i.e., the cost of deferring is *decreased*. Here, Subjects choose between *half* the amount, i.e., 1 cookie and $\frac{1}{2}$ glass of juice, of own earlier choice or the full amount of spouse's choice, i.e., 2 cookies and 1 glass of juice.
All regressions include village fixed effects. SEs are clustered at the level of experimental session.

Table 1.11: Effects on Deferring of Decision-making on *Food and Drink Type* when Cost of Deferring is Decreased (Female Sample)

	(1) Defer: Own Choice (2,500N) or Sp.'s Choice (2,500N)	(2) Defer: Own Choice (2,100N) or Sp.'s Choice (2,500N)	(3) Defer: Own Choice (2,500N) or Sp.'s Choice (2,100N)
NoCash + NoSecret	-0.002	-0.007	-0.099
	(0.075)	(0.071)	(0.081)
Cash + Secret	-0.143**	-0.132***	-0.135**
	(0.063)	(0.042)	(0.062)
Cash + NoSecret	-0.053	-0.051	0.007
	(0.084)	(0.065)	(0.086)
(NoCash + Secret) Mean	0.673	0.740	0.575
Number of households	450	312	312
Number of sessions	34	24	24
Number of villages	24	19	19

* for p<0.10, ** for p<.05 and *** for p<.01

Notes: All regressions include village fixed effects. SEs are clustered at the level of experimental session.
The outcome variable in Columns (1)-(3) is the decision to defer the choice of allocating an endowment across male and female specific items, commonly used by adult household members in the study area.
The outcome variable in Column (1) is the basic variable for deferring decision-making to spouse, i.e., when cost of deferring is zero. Here, subjects decide between own earlier choice or spouse's choice (i.e., they defer decision-making) and both these choices were made with the same endowment amount of 2,500 NGN. Deferring or not deferring, in this case, does not leave anything on the table.
In Column (2) a cost is imposed for choosing one's own choice, instead of spouse's choice, i.e., the cost of deferring is *decreased*. Subjects choose between an own earlier choice made with a smaller endowment (2,100 NGN) amount and spouse's choice made with a larger endowment (2,500 NGN).
In Column (3) a cost is imposed for choosing spouse's choice, instead of own choice, i.e ., the cost of deferring is *increased*. Subjects choose between an own earlier choice made with a larger endowment (2,500 NGN) amount and spouse's choice made with a smaller endowment (2,100 NGN).

Table 1.12: Effects on Deferring of Decision-making on *HH Consumption Mix of Male v. Female items* when Cost of Deferring is Changed (Female Sample)

	(1) Defer: Own Choice (2,500N) or Sp.'s Choice (2,500N)	(2) Defer: Own Choice (2,100N) or Sp.'s Choice (2,500N)
NoCash + NoSecret	0.015	-0.058
	(0.066)	(0.064)
Cash + Secret	-0.157***	-0.163***
	(0.047)	(0.029)
Cash + NoSecret	0.003	-0.060
	(0.075)	(0.091)
(NoCash + Secret) Mean	0.667	0.760
Number of households	503	204
Number of sessions	38	16
Number of villages	27	12

* for p<0.10, ** for p<.05 and *** for p<.01

Notes: All regressions include village fixed effects. SEs are clustered at the level of experimental session.
The outcome variable in Columns (1) and (4) is the decision to defer the choice of allocating an endowment across self and one's spouse.
The outcome variable in Column (1) is the basic variable for deferring decision-making to spouse, i.e., when cost of deferring is zero. Here, subjects decide between own earlier choice or spouse's choice (i.e., they defer decision-making) and both these choices were made with the same endowment amount of 2,500 NGN. Deferring or not deferring, in this case, does not leave anything on the table.
In Column (2) a cost is imposed for choosing one's own choice instead of spouse's choice (i.e., the cost of deferring is *decreased*). Subjects choose between an own earlier choice made with a smaller endowment (2,100 NGN) amount and spouse's choice made with a larger endowment (2,500 NGN).

Table 1.13: Effects on Deferring of Decision-making on *Money Allocation Decision* when Cost of Deferring is Decreased (Female Sample)

	(1) 1(Revised Amount $\neq$ Orig. Alloc.)	(2) 1(Revised Amount = Sp's Communication)
NoCash + NoSecret	-0.000	0.020
	(0.048)	(0.056)
Cash + Secret	-0.116**	0.002
	(0.046)	(0.048)
Cash + NoSecret	-0.027	0.063
	(0.045)	(0.047)
(NoCash + Secret) Mean	0.708	0.550
Number of households	394	394
Number of sessions	30	30
Number of villages	23	23

* for p<0.10, ** for p<.05 and *** for p<.01

Notes: All regressions include village fixed effects. SEs are clustered at the level of experimental session.
The specific question on revising was: "Recall the decision that you made earlier about [...]. You made the following allocation: [...]. Your spouse has made a choice in another room which is: [...].You can decide to choose your original decision or your spouse's decision as the final decision for this round or you can make a different decision all together. How would you like to allocate the money between the two options? "
The outcome variables in Column (1) is coded as 1 if the revised allocation amount is different than the allocation amount in the preference elicitation round carried out in the beginning, i.e., in (round: *Allocation*); otherwise, it is 0.
The outcome variables in Column (2) is coded as 1 if the revised allocation amount is equal to the amount communicated by one's spouse; otherwise, it is 0.
In *Secret* condition, for any round of decision-making, spouses cannot observe participant's decisions within the experiment, i.e., subjects have plausible deniability in their decision-making by the potential selection of either one of their own decisions or their spouse's decisions or a randomly chosen allocation (from all possible outcomes) as final pay-off for their household. However, in the *No-Secret* condition, spouses can observe subjects' decisions for most of the rounds, i.e., if any round after the first round, i.e., round: *Allocation*, is selected as final pay-off for the household, the actual decision by a participant will be disclosed to his/her spouse.

Table 1.14: Reduced Form Effects of *No-Secret* Experimental Treatment and UCT Status on Decision to Change/Revise (Dummy Var.) earlier Allocation after Observing Communication from Spouse (Female Sample)

1.11 Appendix A: Flowchart of Decision Rounds

Nigeria Intra-HH Decision-Making Games Flowchart

- Married couples, who are from a Impact Evaluation (IE) sample of an Unconditional Cash Transfer program, arrive at the site
- Potential subjects' identities checked with a pre-defined list of household information - half these households were UCT recipients and the other half, non-recipients.
- Participants enter the session room (while their spouses go to another session room)
- Receive 250 NGN individual show-up/participation fee
- "You will make a number of decisions today, through which you can earn additional money for your household.
- Only **one** of the decisions that **you and your spouse both** make today is likely to be chosen/implemented for final pay-off for your household. Since any of your decision can be chosen, you should take each of your decisions seriously and wisely."
- In addition to the participation fee, based on your decision or your husband's decision, the pay-off for your household can be between 1,800 NGN and 2,700 NGN in cash or in kind. This is in addition to the participation fee."

Husbands and wives are taken to different rooms

Individual Preference Elicitation (round: ***allocation***):

For each decision in the (binary) mix for {private good consumption composition, private good-HH public good composition as well as distribution of an unearned income between husband and wife} individual preferences are elicited privately. The specific decisions to be made are:
- Female vs. Male items (1)
- Male items vs. HH public goods (2)
- Female items vs. HH public goods (3)
- Money Allocation - husband vs. wife (4)

Secret Treatment:

All decisions, including the following rounds, will be kept private from spouse.
Participants will have plausible deniability in their decision-making by the selection of either one of their own decisions or their spouse's decisions or a randomly chosen allocation (from all possible allocations) to be chosen as final pay-off for their household. This random allocation will be termed as the "secret-keeping" choice.

No-Secret Treatment:

Any decision made after (round: ***allocation***) can be potentially observed by spouse.
Any final pay-off can still be a random allocation/"secret-keeping" choice.
However, if any of the following rounds are chosen, the actual decision by a participant will be disclosed to the spouse

Deferring of Decision-making to spouse (round: ***defer***):

For each decision domain in {1, 2, 3, 4} participant is asked to make one of two choices after being told that "Your spouse has made a similar allocation decision in the next room":
• "Would you like to use the choice you made earlier as the final decision for this round? OR
•Would you like to change your choice to your spouse's choice as the final decision for this round?"

Deferring of Decision-making to spouse with different costs of deferring (round: ***allocative-eff*** and round: ***consumption-eff***):

For each decision domain in {1, 4} participant is asked to make one of two choices after being told that "Your spouse has made a similar decision in the next room":
• "Would you like to use the choice you made earlier with 2,100 [2,500] NGN OR
•Would you like to change your choice to your spouse's choice which was made with 2,500 [2,100] NGN"

Suggestion/Communication Decision (round: ***communication***):

For each decision domain in {2, 3, 4} participant is asked:
"Recall the decision that you made earlier with [ ]. Your spouse is going to making a similar decision again in another room. We are going to pass on information about what you choose to your spouse before he/she makes her decision. What would you like this to be? You made the following allocation earlier: _____. "

Consultation Decision (Round: ***consult***):

"Remember the decision that you made about {2, 3, 4} earlier. Your spouse has also been asked to make the same decision in the other room. You can choose to either:
(a) still use the choice you made earlier or
(b) see your spouse's choice before making the choice again

Revision Decision (implemented regardless of the decision above) (Round: ***revision***):

Remember the decision that you made about {2, 3, 4}. You will now be shown the decision you made earlier and a decision that your spouse has made. You can choose your earlier decision, your spouse's decision or something else.
Please make the decision again" _____.

Decision on Immediate Consumption (round: ***lab-consumption***):

"You will now be offered to consume within the session, a food and a drink item from several options. At any point of this round, you can refuse to taste or consume the items you are being offered.
•We have two types of drinks available for you, i.e., Coke and Fanta, with the same market price. We also have two types of cookies available for you, all valued locally at the same price.
• You must drink and eat these items here. You cannot take them outside.
• Which food and drink do you want to consume?
• Which food and drink do you want your spouse to consume?
We have also asked your spouse to select a food and drink for you to consume. You can choose to either: (a) use the choice you made earlier or (b) change your choice to your spouse's choice.

For some sessions, we used half the amount for choice (a)

(round: ***incentivized-guessing***)
Incentivized norm elicitation of deferral decisions of session participants and role of women in HHs

1.12 Appendix B: Variables from Experimental Decisions

The table below lists all the variables generated from the subjects' experimental decisions.

Experimental Decisions/Variables

Experimental Variables*	**Description of Variables**	**Decision Domain****	**Exp. Treatment: Secret /No Secret*****	**Male/ Female**
A - Allocation	*Continuous var.:* Individual preference on how to allocate experimental endowment across different binary choices (decision domains); $A_{ig} \in [0, 2500 \text{ NGN}]$, where i is the decision domain and g is the gender of participant; Always carried out in the first round, while the order of the rest of the decision rounds is randomized	All*	Secret	Both
D - Defer	*Dummy*: Choose either own earlier choice (i.e., not deferred) OR Spouse's earlier choice (i.e., deferred); Both choices were made with the same endowment amount – 2,500 NGN, i.e., cost of deferring is 0; $D_{ig} \in \{0, 1\}$	All	Both "Secret" and "No Secret"	Both
D – Defer Choice of Juice/Cookie	*Dummy*: Choose either own choice (i.e., not deferred) OR Spouse's earlier choice (i.e., deferred); For immediate consumption in the session. Cost of deferring is varied.	All	Both	Both
D^+ - Defer (when more expensive to defer); round: ***allocative-eff*** and round: ***consumption-eff***	*Dummy*: Choose either own earlier choice made with a larger endowment (not deferred) OR spouse's earlier choice made with a smaller endowment (deferred); $D^+_{ig} \in \{0, 1\}$	Men's items vs. women's items	Both	Both
D^- - Defer (when less expensive to defer); round: ***allocative-eff'*** and round: ***consumption-eff***	*Dummy*: Choose own earlier choice with a smaller endowment (not deferred) OR Spouse's earlier choice made with a larger endowment (deferred); $D^-_{ig} \in \{0, 1\}$	Men's items vs. women's items; Self vs. spouse	Both	Both
S – Communication	*Continuous:* Subjects are asked to communicate their preferences for allocation to their spouses, who will make an allocation decision again after being shown this information (as well as their earlier allocation/ preference, A_{ig}); $S_{ig} \in [0, 2500 \text{ NGN}]$	Men's items vs. everyone's items; Women's items vs. everyone's items; Self vs. spouse	No Secret	Both
C - Consult	*Dummy*: Subjects are asked whether they would like to see their spouses' communication choice before making an allocation decision again; $C_{ig} \in \{0, 1\}$	Men's items vs. everyone's items; Women's items vs. everyone's items; Self vs. spouse	Both	Both
R - Revision	*Continuous:* Subjects are asked to make an allocation decision again after being reminded of their own earlier allocation decision as well as their spouses' communication choice; $R_{ig} \in [0, 2500 \text{ NGN}]$	Men's items vs. everyone's items; Women's items vs. everyone's items; Self vs. spouse	Both	Both

* Note that continuous variables are denoted as nouns (e.g., Allocation) and dummy variables, as verbs (e.g., Defer)
* The different decision domains involve dividing an experimental endowment across binary choices: male items vs. female items, male items vs. items commonly used by households, women items vs. items commonly used by households, self vs. spouse.
** We have two experimental conditions – one of which we randomly choose for any given experimental session. One is the "Secret Condition" - in which participants are assured of complete privacy/secrecy for all of their decisions within the

experiment. The other is the "No Secret Condition" - in which participants are told (after the first round of Allocation) that if one of their own subsequent decisions is chosen through the lottery for pay-off, it will be revealed to their spouse at the end of the session. However, if the first round is chosen, participants will still have plausible deniability.

1.13 Appendix C: Experimental Protocol

1.13.1 Introduction

- Upon arrival, subject pairs (i.e., married couples) were assigned to their respective gender-specific room. This means that all the husbands were put in one room, while all the wives were put be in a separate room. Thus, husbands and wives could not directly interact with each other during the sessions.

- Our plan was to conduct two experimental sessions for each experimental site on the same day. However, this depended on the size of the impact evaluation sample in the nearby villages. If we had enough participants to have two sessions, we conducted both sessions consecutively, allowing no possibility for interaction between experimental subjects in the different sessions. We ensured this by having the subjects from the first round leave the premises of the experiment immediately after their session ended, without providing any opportunity to interact with subjects from the following round, who would wait in a separate room. Research Assistants (RAs) at the site implemented this "no interaction" protocol between subjects in consecutive sessions. At the end of the first round, one RA was asked to stand outside the waiting room for the subjects of the second round, while another RA guided subjects from the first round out of the experimental site. Both RAs were instructed to ensure that experimental subjects from the two sessions did not interact with each other.

- Experimental subjects were seated in compartments separated by curtains made of opaque clothing material. Thus, subjects were not able to see each other or observe each others' experimental decisions. This ensured the privacy of decisions during the experiment. Any contact between subjects during the session was strongly discouraged. Moreover, subjects were not allowed to verbally communicate any of the experimental decisions to their assigned enumerators. Rather, they communicated all their decisions using experimental prompts, e.g, envelopes and pictures that denoted binary choices and (laminated) cash prompts to allocate between binary choices. This further protected privacy and subdued any mimicking effort.

- Subjects were thanked for coming to the session and were handed the participation fee: "Thanks for coming today. Before we start, we would like to give you 250 NGN as a compensation for your time. This amount is not part of today's activities and is yours to keep. You and your spouse, who is in a different room now, will make a number of decisions today, through which your household can earn additional money. At the end of this session, only one of the decisions that you and your spouse make will be chosen via a lottery, which is likely to be implemented as final pay-off for your household. Since any of your decisions can be chosen, you should take each of your decisions seriously and carefully. In addition to the participation fee, the pay-off for your household can be between 1,800 NGN and 2,700 NGN, in cash or in kind, depending on which round is chosen from the lottery for pay-off. Again, only one decision of all the decisions that both you and your spouse make today will be chosen through a lottery as final pay-off for your household."

- A visual demonstration was then shown which demonstrated how one decision - from all the decisions made by the husband and the wife in the experimental session - was going to be chosen by a lottery as final pay-off for the household. The decision rounds for female experimental subjects were represented with white numbered balls while the decision rounds for male subjects were represented by orange numbered balls. The numbers on the balls denoted different rounds. In the demonstration, subjects were told that at the end of all the decisions by the husband and the wife, all the white and orange numbered balls would be put in a basket. One ball would be taken out and the corresponding decision was likely to be implemented as the final pay-off for the household.

- Plausible deniability ("secret-keeping" round): The pay-off process, at the end of the experimental session was designed to ensure that the individual, private choices made during the experiment were not revealed to the subject's spouse or anyone else (unless warranted by the experimental treatment as explained in the following discussion point). Recall that at the end of a session, based on which numbered ball was randomly drawn out of the basket, a particular decision round would be chosen as pay-off for each household. This decision round could have been played by either the husband or the wife. The session supervisor would then find out the recorded decisions and write the response on a piece of paper and insert it in an envelope. The envelope would then be put in a tin box.

One envelope randomly chosen from a bag that contained all the possible allocations written inside different envelopes, would also be put in the tin box. The tin box would then be shaken several times and one of the envelopes would be taken out, based on which the household would receive a pay-off.

For example, at the end of all the decisions, if a white ball with number "1" is picked from the lottery basket, then the female participant's decision to allocate 2,500 NGN between men's and women's items (the specific rounds will be discussed in Sub-section 1.13.2) would be recorded on a paper which would then be put inside an envelope. The envelope would later be placed inside a tin box. Another envelope would be drawn from a bag of envelopes which contained all the possible allocations for 2,500 NGN (e.g., 0 NGN for men's items and 2,500 NGN for women's items, 50 NGN for men's items and 2,450 NGN for women's items, ..., 2,450 NGN for men's items and 50 NGN for women's items, 2,500 NGN for men's items and 0 NGN for women's items).

Thus, either one of the decisions made by a participant or her spouse or a randomly chosen allocation (from all possible allocations) could be chosen as final pay-off for a household. This process offered plausible deniability to a subject for any choice she makes within the experimental session. The possibility of a random allocation to be selected as the final pay-off was explained in the beginning of the session as the "secret-keeping" choice, which, unless otherwise explicitly stated, would keep the subjects' experimental decisions secret.[14]

- Experimental Treatments: Recall that conditional on the availability of subjects from the UCT impact evaluations sample, each experimental site was targeted to have two consecutive experimental sessions. One session would be randomly selected to the "Secret" condition while the other to the "No Secret" condition. A married couple would always be in the same "Secret"/"No-Secret" condition. In the "Secret" condition, none of the decisions made by a subject could be observed by his/her spouse. However, in the "No Secret" condition, any decision made *after* after the first round (i.e., 'round: ***allocation')*** could potentially be observed by the participant's spouse, if that round was selected for pay-off in the lottery. Thus, the actual allocation decisions could never be observed by one's spouse (or anyone else). This is due to the possibility of the "secret-keeping" choice to be chosen as the pay-off, i.e., a random choice of allocation from all possible choices to be drawn as the final pay-off (explained above under "Plausible deniability") instead of one's own decision.

  For example, suppose in the "No Secret" treatment a white colored ball was chosen that represented a decision from 'round: ***defer'*** of the female participant, and suppose, the female participant had not deferred her decision. Then the couple while receiving the pay-off would be informed that she had not deferred the final decision-making (for the chosen decision to her husband. For

[14]There is a 50% chance for the one of the married couple's own preference decision to be implemented and a 50% chance for a completely random draw to be chosen as final pay-off.

pay-off, either the female subject's own decision or the "secret-keeping" choice will be selected as pay-off for the household, allowing for plausible deniability for the actual allocation decision made by the wife.

- Experimental subjects, in all the decision rounds will be requested not to communicate their experimental decisions verbally. Rather, every decision will be made using cash prompts, envelopes and other experimental prompts.

1.13.2 Experimental Round: Individual Preference Elicitation (round 1: *allocation*)

- Individual preference elicitation is carried out on experimental subjects in different decision spheres in the very beginning, i.e., in round 1. This means that this round, irrespective of the randomly chosen order of rounds to be carried out on any given experimental session, will not be part of the randomization of the order. The decisions in this round will involve dividing an experimental endowment between two options. For each of the following decisions, experimental subjects will use plastic laminated colored-photocopies of four 500 NGN bills, four 100 NGN bills and two 50 NGN bills (adding up to 2,500 NGN) as cash prompts. Subjects will divide the cash prompts between two envelopes, one representing allocation for the husband and the other for the wife. The different decisions are denoted by numbers 1 through 4. The order of these decisions will be randomized across sessions. The four different (preference elicitation) decisions are:

1. Men's vs. Women's items
2. Men's vs. Everyone's items
3. Women's vs. Everyone's items
4. Money Allocation - husband vs. wife

- In preparatory field-work before carrying out the experiments, we have carried out market and consumer surveys to make sure that the "men's items" in the experiment are indeed identified as distinctly male items in the study area, while "women's items" are considered to be distinctly female items. Similarly, "everyone's items" will consist of materials that are not typically assigned to any gender and are usually used by everyone in a household.

- In the first three numbered decisions above, subjects will be asked to divide an experimental endowment between binary choices of commonly used household items (e.g., between men's items vs. women's items). A 'lab shop' will be set up for the experiment and if any of the decisions numbered 1, 2 or 3 is chosen through the end-of-the-session lottery as the final pay-off for the household, participants can choose items from the 'lab shop' according to either their endowment choices they would have made earlier or a randomly chosen allocation from all possible allocations (which gives participants plausible deniability). Either way, depending on the round chosen for pay-off, participants will be given tokens with a type of good (e.g., women's items) and a Nigerian Naira amount written on it which they can show at the lab shop. Suppose, a female participant chooses to allocate 2,100 NGN for women's items and 1,000 NGN for men's items using a 2,500 NGN endowment (in Decision #1) and this decision is chosen for final pay-off through the lottery at the end of the session (i.e., a white ball numbered '1' is randomly drawn in the lottery). Assume, further, that her own decision is chosen instead of a random allocation.[15] Then, the female participant will be given a token for "Men's items" with 1,000 NGN written on it and another token for "Women's items" with 2,100 NGN written on it. Since, the female round is chosen, the wife is given the tokens. The subject can herself visit the 'lab shop' with the tokens, or she can give them to her husband or they can both go together. An example script for this round is:

– "For this decision round, you are given 2,500 NGN [Enumerator: Please, give participant the cash prompts]. You will need to decide how much you want to spend for items in Picture A vs. items in Picture B. The items in Picture A and Picture B are ... [Enumerator: Please, describe the items]. As you can see, these items are also displayed in the middle of the room.

  These are just sample items. Varieties/options in fashion and color are available in the shop we have set up outside.

  You will need to make a decision NOW on how much to spend. You can decide LATER which items you want to pick.

[15] There is a 50% chance that one's own allocation decision will be chosen for final pay-off

[Enumerators: Please use two envelopes: one envelope for "Items in A" which you put near picture A and the other envelope for "Items in B" which you put near picture B]

Remember that this decision is private. No one else, including your spouse, will know what your exact decision is. This is because of the "secret-keeping" choice explained earlier.

How much of this 2,500 NGN will you spend for items in A vs. items in B?"

- In the last decision of this round (Decision # 4), participants will allocate 2,500 NGN between himself/herself and his/her spouse.
  - "For this decision round, you are given 2,500 NGN. [Enumerator: Please, give participant the cash prompts]. You will need to decide how much you want to keep for yourself and how much you would like to give to your spouse?

    [Enumerators: Please put two envelopes in front of the participant] Please use these two envelopes. One envelope, the one near you, is for "Yourself" while the other envelope is for "Your Spouse".

Remember that this decision is private. No one else, including your spouse, will know what your exact decision is. This is because of the "secret-keeping" choice explained earlier."

- For the purposes of playing "efficiency games" (in a different round), we will also have the subjects replay one of the decisions from Decision # {1, 2, 3} as well as Decision # 4 (money allocation between self vs. spouse), but both with a smaller endowment of 2,100 NGN. This is explained in detail in Sub-section 1.13.7.

1.13.3 Experimental Round: Deferral Decision (round*: defer*)

- For each of the different decisions from the *allocation* round, participants will be reminded of their previous choice and that their spouse has made the same decision in another room. Participants will then be told that they can choose whether they would like to use their previous choice, or change their choice to their husband's choice. The deferral decision for the different spheres will be elicited in the same order as the original preference decisions. The script for the "Secret" Treatment will be:

  "Recall the decision that you made earlier on ________. Your spouse has also been asked to make the same decision in the other room. You can choose to either: (a) use the choice you made earlier or (b) change your choice to your spouse's choice

Put the matchbox on the envelop near you to suggest that you will use the choice you made earlier. Put the matchbox on the envelop away from you to suggest that you will change your decision to your spouse's choice.

[Enumerator, please show the experimental subject the two envelopes representing each of the two choices. Placing the matchbox on one of the envelopes, represents making a particular choice. At no point should the subject relay his/her response verbally]

This round is represented by this white ball with a number [Enumerator, please mention number] on it that I am putting in this basket. At the end of the session today, if this ball is picked from the bag, then your decision now may determine the final pay-off. For example, if you decide to use your spouse's choice then your spouse's choice will be chosen as your household's final pay-off. Otherwise, your previous choice will be chosen as your household's final pay-off.

However, you can also get the "Secret-keeping" choice. Thus, from this decision, your spouse will not know whether you used your own choice or his choice."

- The script for the "No-Secret" Treatment will have the following line, instead of the last line in the "Secret" Treatment script:

"If this round is selected, your spouse will be told whether you decided to use your earlier choice or your spouse's choice. However, your earlier allocation choice on how to divide the money will still remain private/secret, due to "secret-keeping" option."

1.13.4 Experimental Round: Communication Decision (round*: communication*):

- For each of the different decisions from the ***allocation*** round, participants will be asked to communicate their preferences to their spouse for the different allocation decisions. The script for this round will be:

"Recall the decision that you made earlier on _______.

Your spouse is making a similar decision in another room. We are going to pass on information about what you choose to your spouse before he or she makes a decision for a second time. What would you like this to be? You made the following allocation earlier, which you can pass on to your spouse: _______. Or, you can decide to send him/her a different choice. _______"

[Enumerator, please use cash prompts and envelopes to collect information on their communication for their spouse. No verbal communication, please.]

1.13.5 Experimental Round: Consultation and Revision (round*: consult-and-revision*)

- *Consult*: For each of the different decisions from *allocation* round, male participants will be asked whether they would like to use their previous decision as the final decision, or or they would like to see their spouse's choice before making the final decision. The script for this round will be:

  "Recall the decision that you made about Decision # {1, 2, 3, 4}. You made the following allocation: ________. Your spouse has also been asked to make the same decision in the other room. You can choose to either: (a) still use the choice you made earlier or (b) see your spouse's choice before making the choice again."

- For "Secret" Treatment, the following line is added: "No one will know exactly what your decision is going to be. This means that your spouse will not know if you decide to see your spouse's choice before making the choice again."

- The "No-Secret" Treatment will have the following line added:

  "If this round is selected, your spouse will be told whether you decided to see her choice or not before making the final decision. However, your earlier allocation choice still remains private and secret. "

- *Revision*: For each of the different decisions *allocation* round, subjects will be told their spouse's choices (irrespective of whether they wanted to see their wives' decisions or not) and will be given the option to redo each of the decisions in {2, 3, 4} or keep their original decision. Participants will also be reminded what their original decision was. The script for this round will be: "Recall the decision that you made about Decision # {2, 3, 4}. You will now be shown the decision you made earlier and a decision that your spouse has made. You can choose your earlier decision, your spouse's decision or something else. Please make the decision again ________."

- For "Secret" Treatment, the following line is added: "This round is represented by this white ball with a number [Enumerator, please mention number] on it that I am putting in this basket. At the end of the session today, if this ball is picked from the bag, then your decision now may determine the final pay-off. For example, if you decide to use your spouse's choice then your spouse's choice will be chosen as your household's final pay-off. Otherwise, your previous or a new choice will be chosen as your household's final pay-off. However, you can also get the "Secret-keeping" choice. Thus, from this decision, your spouse will not know whether you used your own choice or his choice."

- The "No-Secret" Treatment will have the following line added:

  "If this round is selected, your spouse will be told what your choice was. However, your earlier allocation choice still remains private and secret. "

- Round*:* *consult-and-revision* is a joint round and will be jointly implemented if chosen in the final lottery.

1.13.6 Experimental Round: Decision on Immediate Consumption in the Lab (round: *lab-consumption*)

- During the piloting of the experiments, we were able to identify popular food and drink items in the study area which have similar market prices. For drinks, a plastic cup of Coke or Fanta, valued locally at around 150 NGN, will be made available for subjects. For food items, one of two different types of cookies will also be offered, which are of similar market prices. Two cookie pieces of the type chosen by the participant will be offered.

- Subjects will be asked to choose which food and drink pair he/she wants to consume in the experimental session. It will be explained that he/she can only have it before the session ends. Moreover, it will be explained that participants will not be allowed to take the food and drink outside (this is because we want to observe choices when consumption is completely private from spouse).

- Each subject is asked to guess the choice of their spouse.

- Each participant selects which food and drink pair they want their spouse to consume.

- Enumerators from both rooms exchange the above information, i.e., which food and drink pair participants want their spouses to consume.

- Each subject could now choose to either: (a) use the choice she had made earlier or (b) change her choice to her spouse's choice. For some sessions, we lowered the cost of deferring this decision-making to spouse by offering half the amount for choice (a)

- Half the participants (in the "Secret" Treatment) will be told that their decision to consume their own choice or their spouse's choice of food and drink will remain completely private from their spouse or anyone else.

- The other half of the participants (in the "No-Secret" Treatment) will be told that their decision to consume their own choice or their spouse's choice of food and drink maybe disclosed to their spouse at the end of all the decisions.

- Based on their decisions, subjects will be offered a food and drink pair.

- If anyone refuses to drink (either their own choice or their spouse's choice) we will still record that data.

- Each subject could now choose to either: (a) use the choice she had made earlier or (b) change her choice to her spouse's choice. For some sessions, we lowered the cost of deferring this decision-making to spouse by offering half the amount for choice (a) .

1.13.7 Experimental Round: Efficiency Rounds

- Allocative Efficiency (round: ***allocative-eff***): We plan to measure inefficiency in the decision to divide money between household members (i.e., between the husband and the wife). To do this we will, at first, play an additional decision game in the individual preference elicitation round (i.e., ***allocation*** rounds). This decision is similar to Decision # 4 (i.e., "How would you like to divide some money between yourself and your spouse?") but instead of a (larger) 2,500 NGN endowment, this will involve a smaller endowment of 2,100 NGN. Secondly, to be able to measure allocative efficiency in this later round (round: ***allocative-eff***), we will remind the subject of these two decisions that they made earlier, one with a smaller endowment and the other with a larger endowment and that their spouse had done the same. We will now ask subjects whether they would like to choose the allocation decision they made earlier with an endowment of 2,100 NGN or the decision their spouses made but with an endowment of 2,500 NGN. Conversely, we will ask subjects whether they would like to choose the decision they made earlier with an endowment of 2,500 NGN or the decision their spouses made but with with an endowment of 2,100 NGN. The following script will be used:

"Remember the previous decision in which you were given 2,500 NGN [just show cash prompts; no need to give]. You decided how much to keep for yourself and your spouse. You also made a similar decision, in which you were given 2,100 NGN [just show cash prompts; no need to give]. You decided how much to keep for yourself and your spouse.

Your spouse has also made the same decisions in the other room. You can choose to either:

(a) use the choice you made earlier you made with 2,500 NGN OR

(b) change your choice to your spouse's choice which was made with 2,100 NGN

Put the matchbox on the envelop near you to suggest that you will use the choice you made earlier with 2,500 NGN. Put the matchbox on the envelop away from you to suggest that you will change your decision to your spouse's choice made with 2,100 NGN.

What do you choose?

(a) use the choice you made earlier you made with 2,500 NGN

OR

(b) change your choice to your spouse's choice which was made with 2,100 NGN"

- Consumption Efficiency (round: ***consumption-eff***): We plan to measure inefficiency in household consumption decisions. For this, we will, at first, play an additional money allocation game for consumption items in the individual preference round (i.e., ***allocation*** rounds). This decision will be similar to Decision # 1, 2 or 3 (for example, men's vs. women's items) but with a smaller endowment, e.g., with 2,100 NGN instead of 2,500 NGN. Secondly, to be able to measure consumption efficiency in this later round (round: ***consumption-eff***), we will ask participants whether they would like to choose the decision they made earlier with an endowment of 2,100 NGN or the decision that their spouses have made but with an endowment of 2,500 NGN. Conversely, we will ask subjects whether they would like to choose the decision they made earlier with an endowment of 2,500 NGN or the decision their spouses have made but with an endowment of 2,100 NGN. The following script will be used:

"Remember the previous decision in which you were given 2,500 NGN [just show cash prompts; no need to give]. You decided how much you want to spend for items in Picture A vs. items in Picture B. [Show the pictures and describe some of the items to remind her]. You were also given 2,100 NGN to make the same choice, i.e., how much to spend for items in Picture A vs. items in Picture B.

Your spouse has also made the same decisions in the other room. You can choose to either:

(a) use the choice you made earlier you made with 2,500 NGN OR

(b) change your choice to your spouse's choice which was made with 2,100 NGN

Put the matchbox on the envelop near you to suggest that you will use the choice you made earlier with 2,500 NGN. Put the matchbox on the envelop away from you to suggest that you will change your decision to your spouse's choice made with 2,100 NGN.

What do you choose?

(a) use the choice you made earlier you made with 2,500 NGN

OR

(b) change your choice to your spouse's choice which was made with 2,100 NGN"

- "Secret" and "No-Secret" Treatment will be implemented as in earlier rounds.

1.13.8 Experimental Round: Incentivized Norm elicitation (round: *incentivized-guessing*):

- We will elicit social norms on the lab-based measures of deferral and consultation decisions - at the session level. Moreover, we will elicit social norms on gender-specific agency regarding trivial and important household decisions, using Likert scale based statements on attitude towards issues regarding female mobility and agency.

- Participants will play an incentivized norm elicitation and 'guessing' round, in which they would get an additional pay-off (200 Nigerian Naira) for correctly guessing the number of times a randomly chosen participant from the same session and room had deferred or consulted with his/her spouse; as well as guessing the response to different questions on 'socially appropriate' behavior of men and women of a randomly matched person from the same room. One of all the 'guesses' from this round would be randomly chosen for pay-off for the subject.

1.13.9 Pay-off to Household

- At the end of all the rounds, for each household, all the numbered white balls, representing the wife's decisions and all the numbered orange balls, representing the husband's decisions will be put in one bag. One ball will be taken out to determine the pay-off for the household.

- The enumerator will then find out the recorded decision (that corresponds to the selected numbered ball) and write the response on a piece of paper and insert it in an envelope. The envelope will then be put in a tin box.

- There will be a bag which will contain all the possible allocations written inside different envelopes, for the chosen decision round. One of the envelopes from this bag will also be put in the tin box.

- The tin box will be shaken several times and one of the envelopes will be taken out, based on which the household will receive the pay-off.

- A 'lab shop' will be set up for the experiment and if any of the decisions numbered 1, 2 or 3 is chosen through the end-of-the-session lottery as the final pay-off for the household, participants can choose items from the 'lab shop'. Depending on the round chosen for pay-off, participants will be given tokens with a type of good (e.g., women's items) and a Nigerian Naira amount written on it which they can show at the lab shop.

- If Decision # 4, or any decision that involves cash as pay-off, is chosen, the husband and the wife will be given the cash separately.
- Suppose, a female participant chooses to allocate 1,500 NGN for women's items and 1,000 NGN for men's items using a 2,500 NGN endowment (in Decision # 1) and this decision is chosen for final pay-off through the lottery at the end of the session (i.e., a white ball numbered '1' is randomly drawn in the lottery). Assume, further, that her own decision is chosen instead of a random allocation through the "secret choice". Then, the married couple will be given a token for "Men's items" with 1,000 NGN written on it and another token for "Women's items" with 2,100 NGN written on it. The couple can visit the 'lab shop' with the tokens, or either the husband or the wife can go.
- An enumerator will be at the 'lab shop' and will hand out the items.

Chapter 2: Social and Financial Incentives for Overcoming a Collective Action Problem

2.1 Introduction

Many inexpensive, efficacious technologies that have the potential to address important development challenges are used at surprisingly low rates in developing countries: drinking water disinfectants (Ashraf, Berry and Shapiro, 2010), fertilizers (Duflo, Kremer and Robinson, 2011), nutritional supplements (Maluccio, et al., 2009), high-yield crop varieties (Udry, 2010), rainfall insurance (Cole, Stein and Tobacman, 2014) and hygienic toilet access (Guiteras, Levinsohn and Mobarak, 2015). The potential productivity and welfare consequences of many of these technologies are very large, which makes the low adoption rates for these seemingly cost-beneficial products a central puzzle in development.

Social scientists and development practitioners have proposed a variety of explanations for the low take-up: poor households may be liquidity or credit constrained (Dupas and Robinson, 2013), they may simply not understand a technology's benefits (Foster and Rosenzweig, 2010), they may be highly risk averse (Bryan, et al., 2014), they may suffer from self-control problems (Banerjee and Mullainathan, 2010; Duflo, Kremer and Robinson, 2010), or there may be learning externalities, and inefficiently little experimentation (Munshi, 2003; BenYishay and Mobarak, 2013).

These models generally identify household-specific internal frictions that prevent investment in profitable new technologies. In this paper we explore a different type of market failure that may arise when investment decisions are inter-linked across households. If one household's returns to adoption of a technology depends on how many other households around it are also adopting, then a coordination failure across households can keep investment rates inefficiently low. We experimentally evaluate interventions designed to address coordination failures undermining adoption of a technology for which investment decisions are likely to be inter-linked: hygienic latrines.

One billion people, or about 15% of the world's population, currently practice open defecation (WHO/UNICEF, 2014), in spite of the existence of simple pour flush latrines that effectively confine fecal matter. Open defecation spreads bacterial, viral, and parasitic infections, including diarrhoea, polio, cholera and hookworm and has been identified as a leading cause of child stunting (Spears, 2013; Chambers and Von Medeazza, 2013; Coffey et al., 2017) and infant death (Hathi, et. al., 2013). Diarrheal diseases kills nearly one million people per year (Pruss-Ustun et al., 2014), and is the cause of nearly 20% of deaths of children under five in low income countries (Mara et al., 2010).

In a rural, developing-country setting where a centrally connected sewage system does not exist, the private benefit of using toilets (which are usually, stand-alone, simple flush latrines and can come with either a single pit or a double-pit) can be substantial. The direct health benefit of having one's immediate living space clean and hygienic – which a hygienic toilet can contribute towards - is obvious. Moreover, there are clear safety features that a toilet located inside one's own residential compound can grant.[1] However, toilets have very high levels of positive externalities. Empirical literature suggests that a toilet may only generate a return in the household's health production function once the surrounding environment becomes sufficiently clean (Gertler et al., 2015). It is, therefore, important to understand the lack of individual adoption of hygienic toilets from the point of view of "strategic complementarity in demand", which is when the utility of a purchase of a product often depends on the number or share of others making the same purchase.

We posit that strategic complementarities are likely important in sanitation adoption decision for at least three reasons. First is an epidemiological or technical complementarity that has been mentioned above. The health improvements a household experiences from adoption can be substantially larger if neighboring households also adopt, while the health benefits of adoption are muted or nullified if neighbors continue to practice open defecation (OD). Investing in a toilet will then produce a larger marginal benefit to the household's own health if the surrounding environment is sufficiently clean, creating a complementarity in the health production function.[2] Second, social norms are important. In a community in which OD is

[1]Several studies have suggested that women in rural India without toilets at home risk sexual violence when travelling to and from public facilities or open fields (BBC, 2018).

[2]For example, in a multi-country study with health outcomes data, (Gertler et al. 2015) argues that communities need to reach a threshold in sanitation coverage before we see impacts on health outcomes such as child height.

the norm, the "social cost" or shame associated with practicing OD may be absent, whereas it may be very high in a community in the social norm is to use a toilet (Coffey et al., 2017; Pattanayak et al. 2009). Third, investing in latrines may allow neighbors to learn about the technology and change their perceptions about the net benefits of adoption. A large literature on social learning suggests that when some households experiment with a beneficial new technology, their social connections learn and subsequently become more likely to adopt (Conley and Udry, 2010; Bandiera and Rasul, 2006).

Given our earlier work that identifies possible complementarities in latrine investments (Guiteras et. al, 2015; Guiteras et. al, 2019), we implement new interventions in that same context which are designed to help households overcome any collective action failures that may be keeping adoption rates low. We form groups of 15-20 neighboring households, and have them participate in monthly meetings with a health worker to discuss sanitation, OD and disease risk. We introduce experimental variation in which the members of some groups are asked to make a joint public (but non-binding) commitment in front of each other, stating that they will try to address the OD issue in their neighborhood by investing in hygienic latrines. As a contrast, members of other randomly chosen groups are asked to make a private non-binding commitment to invest. This private pledge is made to the health worker individually, away from the group setting. In a second contrast which acts as a control, groups participate in meetings where they have the same discussions about sanitation behavior, OD and disease risk as in the treatment groups, but no public or private commitment is required of them.

As a cross-cutting treatment, we introduce a form of joint liability, where members of some (randomly chosen) groups are offered monetary rewards conditional on individual adoption of hygienic latrines as well as the group attaining a threshold latrine adoption rate. As contrasts, we offer either a non-pecuniary reward (a certificate from the local politician) to other groups for achieving that same threshold, or no reward at all. These interventions are implemented at a large scale, covering a sample of 19,345 households in 107 villages in Bangladesh.

We hypothesized in our pre-analysis plan that the joint public commitment and joint liability through group-rewards would increase the latrine adoption rate, if latrine investments are indeed strategic complements. If a failure of collective action is keeping adoption rates inefficiently low, then a joint commitment is a cost-effective way to address the negative public health externalities that emanate from open defecation. Group level rewards may also be more cost-effective than direct household-level treatments such as subsidies for adoption, or individual-level marketing effort. Our earlier work (Guiteras et. al, 2015) had implemented those household-level interventions on this same sample of households, which allows us to directly compare the cost-effectiveness of our group-level strategies to other household-level supply-side and demand-side strategies that are common in development practice, and often evaluated by development economists.

We find that group-level monetary reward has the strongest impact in the short term, inducing an approximately 12 percentage point increase in hygienic latrine ownership. The public commitment treatment, on the other hand, affected an approximately 5 percentage point increase in hygienic latrine ownership in the short term. In the medium term, the effect of the financial reward dissipates; however, the effect of the public commitment treatment continues to persist after 15 months.

The paper proceeds as follows. In Section 2, we discuss the context and the sample , while in Section 3, we discuss design of our RCT. Section 3 introduces our econometric framework. Sections 4, 5 and 6 discuss the hypotheses, estimating equations and the results, respectively.

2.2 Program Description

The study sample consists of 19,271 rural households in 107 villages in 4 unions (sub-districts) of Tanore district, Bangladesh. The 107 study villages were further divided into 1,236 groups of approximately 14-17 neighboring households, roughly 4-16 groups per village, and the intervention was conducted at this group level. While the unit of intervention was the group, treatment was randomized at the village level. There were 23 'pure' control villages (4,069 households, 256 groups) in which no intervention was conducted.

All 980 treatment groups (15,202 households, 84 villages) received a common intervention consisting of a group-level meeting every month for 3 consecutive months with a health motivator to encourage investment in, maintenance of, and use of hygienic latrines. We randomized two different aspects of the encouragement: 1. whether and what type of incentive was provided, and 2. whether and what type of verbal commitment the households were encouraged to make. In the incentive dimension, households were provided either (a) no incentive, (b) a financial reward based on both the household's sanitation status and the group's average status at the end of the program, or (c) "social recognition", in which the household received a certificate of hygiene attainment from the local government, again based both on the household's sanitation status and the group's average status at the end of the program. We define the standards precisely in Section 2.3.1 below. In the commitment dimension, households (a) were not asked to make a commitment, (b) were asked to make a public pledge, in front of the rest of the group, that they would invest in sanitation, or (c) were asked to make a private pledge, not in front of other group members. The private pledge was added to separate the effect of a public commitment, which could carry social sanction, from the direct psychological effect of making a commitment.

The intervention was conducted from November 2013 through February 2014. Households and groups were evaluated by May 2014, and rewards distributed by June 2014. The primary outcomes of interest are household-level ownership of hygienic latrines as well as group-level share of households owning an hygienic latrine, as defined in Section 2.3.1 below. Data were collected at the end of the intervention and again 12-15 months later in an endline survey.

2.2.1 Context

This study was conducted with 19,271 households in 107 villages in 4 unions (sub-districts) of Tanore district, Bangladesh. These villages were the site of a randomized evaluation of a set of demand and supply interventions designed to study interdependencies in household investment in improved and hygienic latrines (Guiteras et. al, 2015). Going forward, we will refer to this first set of interventions as the "first set of interventions" or the "demand study interventions," and the second set, the focus of the current study, as the "second set of interventions" or the "incentives for use interventions."

This first set of interventions was conducted from February 2012 – August 2012, and data on latrine investment and other relevant variables was collected at baseline (December 2011 – February 2012) and four subsequent monitoring rounds (Round 1 April 2012 – May 2012; Round 2 June 2012 - July 2012; Round 3 December 2012 - January 2013; Round 4 May 2013 - July 2013). The study area was chosen in part because of its low level of latrine coverage: at baseline, 30.8% of households reported some level of open defecation among adults, and only 50.4% reported that they had access to a hygienic latrine. (Guiteras et. al, 2015) show that subsidies increased ownership of hygienic latrines, both directly – among households receiving a subsidy voucher – and indirectly – among households that did not receive vouchers but were neighbors of households that did. This latter result suggests the presence of a social multiplier, i.e., an inter-linked decision-making processes.

If latrine adoption decisions are interdependent across households, then increasing sanitation coverage and improving sanitation behavior may require addressing collective action problems. To further isolate the specific mechanics of the collective action problem, we designed a second round of interventions to examine the effects of public commitments and rewards based on group performance.

2.2.2 Sample

As noted in Section 2.2.1, the sample consisted of households living in villages that were part of the previous demand study. In that study, villages were subdivided into neighborhoods, and that first set of interventions was conducted at the neighborhood level.[3] The median number of households per neighborhood was 40 (IQR 26-56), and the median number of neighborhoods per village was 4 (IQR 2-6).

For this study, a somewhat smaller intervention unit was appropriate because our fieldwork and qualitative background-information gathering suggested that having households making public commitments to smaller groups of immediate neighbors was more sensible, and that there would be less free-riding and meeting nonattendance in smaller groups. Therefore, we further divided neighborhoods into "groups": sets of 15-20 roughly contiguous households within the neighborhood.[4] These groups were formed before the randomization of the "incentives for use" treatments. Thus, we have a comparable unit of analysis for the treatment, control (which only received the basic/common intervention involving group-level meetings with a health motivator) and pure control (where nothing happened) villages.

The median number of households per group was 16 (IQR 14-17) and the median number of groups per village was 8 (IQR 4-16). In total, 1,236 groups were created, including 256 groups in 23 villages allocated to pure control. 121 groups

[3]The neighborhood, or *para* in Bangla, is not a formal or official designation, but unofficial neighborhood boundaries were usually common knowledge in the community, and in these cases we followed local convention. If there were not well-defined neighborhoods in a village, or if a neighborhood needed to be divided because of its size, we used natural divisions such as rivers or roads where such existed. If such natural pre-existing divisions did not exist or were not practical, we grouped households into simple, contiguous sets.

[4]See Appendix 2.10 for additional details on the group formation process.

in 8 villages were allocated to control, i.e., basic/common intervention involving group-level meetings with a health motivator.

In each treated group, a contact person was selected as the spokesperson for the group and to arrange subsequent study events such as the monthly meetings described below. This contact person was chosen by the households attending the meeting, the only requirement was ownership of a mobile phone.

2.3 Interventions

2.3.1 Common intervention

All 980 treatment groups (in 84 treatment villages) received a basic intervention consisting of monthly meetings for three consecutive months with a Health Motivator to encourage investment in and maintenance and use of hygienic latrines.[5] Health Motivators, trained by and contracted from our implementation partner VERC, discussed the health risks of open defecation and unhygienic sanitation practices, the collective nature of the problem (i.e., the externality in non-technical terms), the types and costs of hygienic latrines, and the current level and monthly change in the share of households with or advancing towards a hygienic latrine. A detailed protocol is provided in Appendix 2.11.

In the common as well as in the cross-cutting interventions, the Health Mo-

[5]Different sources define “hygienic” in different ways, and there are also other labels such as “improved” or “sanitary.” Conceptually, a hygienic latrine safely confines feces. For pour-flush latrines (the relevant type in our context), this typically requires a water seal to block flies and other insects, and a sealed pit to store fecal matter for safe disposal (Hanchett, et. all, 2011). In our survey data, we define an *unimproved latrine* as a bucket, a simple pit with no slab or cover, or a “hanging latrine” (a platform over open land or water), and a *hygienic latrine* as having a functional, non-broken slab and water seal leading to a sealed pit.

tivator provided both a general, conceptual definition of a hygienic latrine and a specific, technical definition. The conceptual definition emphasized that a hygienic latrine was one that:

1. Limits the spread of diseases caused by feces in the water and keeps the environment pollution free;

2. Confines feces in an enclosed space such that they cannot be seen or smelled;

3. Prevents flies or other insects from entering the pit.

The specific, technical definition listed the characteristics based on which a latrine was judged to be hygienic, in particular:

1. There must be a slab and it cannot be broken.

2. There must be a water-seal (gooseneck or siphon) and it cannot be broken.

3. There must be an adequate number of rings depending upon the depth of the pit and those must not be broken.

4. Flies must not be able to enter / exit the pit:

   (a) for single-pit latrines, there should be no gap between the cover of the slab and ring, and earth or cement must be molded around the slab

   (b) for offset latrines, there should be no gap between between the ring and pit cover

5. There should be no feces, flies or bad smell in or around the latrine.

6. For offset latrines: Delivery pipe, Y-junction, pit cover cannot be broken.

7. The latrine cannot pollute the environment. In particular, the latrine cannot be be connected to a lake, stream or any body of water

These characteristics of a hygienic latrine were relayed to participants at each of the three group meetings. Participants were made aware of the fact that for latrines to be considered hygienic all the above mentioned requirements had to be met by the specified deadline, approximately 4 months after the intervention began.

In addition, Health Motivators described how to keep a latrine clean, and discussed basic maintenance. See Appendix 2.11 for details.

Because the reward and commitment groups also received the basic/common intervention involving group-level meetings with a health motivator, groups that received *only* the basic intervention are a more appropriate 'control' to identify the treatment effects. Comparison with the 'pure control' groups will identify the effect of the health meetings plus the different additional reward and commitment components.

2.3.2 Reward treatments

There were two reward treatments, monetary and non-monetary, both of which were conditioned on both the household's own ownership status and the average ownership status of hygienic latrines among households in the group.

The monetary reward consisted of a cash payment to the household if, at the end of the intervention period, (a) the household owned a hygienic latrine and (b) the share of households in that group with a hygienic latrine was above a designated threshold. Hygienic was defined as described in Section 2.3.1 above. The reward was BDT 250 (USD 3.33) in groups that surpassed the lower of two thresholds and BDT 500 (USD 6.67) in groups that surpassed the higher of the two thresholds.[6] For comparison, the cost of building a single-pit hygienic latrine was approximately BDT 2350 (USD 31.33), while common improvements to existing latrines that would be necessary to reach hygienic status cost substantially less, e.g., new water seal BDT 65 (USD 0.87), delivery pipe BDT 360 (USD 4.8).

The non-monetary reward consisted of a certificate of hygiene attainment from the local government, presented to qualifying households in a public ceremony.[7] The non-monetary reward used the same standard for "hygienic" as the monetary reward.

Thresholds were determined based on the group-level distribution of hygienic latrine ownership in the four unions at the time of the third round of follow-up surveys. A lower threshold was set such that even low performing groups would feel they could attain something, and the best performing groups would have something to reach for. We also chose thresholds that were simple and easy to explain at a group meeting: a phrase like "two out of every three households" is easier to understand than a phrase like "sixty-two percent of all households." These thresholds are listed in Table **??**.

The assessment was conducted approximately 4 months after the intervention began, after three group meetings with the Health Motivator. Health Motivators

[6] US dollar equivalents at 75 BDT/USD, the approximate market exchange rate at the time.

[7] This certificate was printed on thick glossy paper, so it could be displayed on an interior wall. However, no weatherproof frame was provided. As a result, it would be difficult for a household to display the certificate outdoors where it would be publicly visible.

did not conduct assessments in villages where they had worked. See Section 2.4.3 for a discussion of the assessment process, with further details provided in Appendix 2.12. Households knew the deadline for achieving hygienic status, and that the assessment would occur within 1-2 weeks after the deadline, but did not know the specific day of the assessment.

2.3.3 Commitment treatments

There were two commitment treatments, public and private.

In the public commitment, during each group meeting, members from all the households of a group were encouraged to make a public pledge that those who did not yet have hygienic latrines would meet hygienic latrine standards as set by the project. Those with hygienic latrines pledged to help others reach the goal within the time limit set by the project. The script of the pledge, in English translation, was: "I hereby promise before everyone present that I will do my best to set up hygienic latrines or improve existing ones into hygienic latrines for myself and for my neighbors by February 5, 2014." In the public commitment arm, this pledge was repeated at the end of each monthly group meeting.

In the private commitment arm, health motivators visited each household in the group after each group meeting. The member of the household attending at the meeting would be encouraged by the Health Motivator to make a commitment before the health motivator that he/she would transform their unhygienic latrines to hygienic ones within the time limit set by the project. The script of the pledge was identical to that in the public commitment arm.

2.3.4 Experimental Design

The reward and commitment treatments lead to a 3x3 design, plus a pure control group. The design is summarized in Table 2.1. Although the treatments were implemented at the group level, randomization was conducted at the village level because of the potential for spillovers within village. We allocated approximately 25% of villages to pure control, and then the remaining villages were allocated equally across the commitment and reward treatments. With 107 villages (84 treatment villages), we did not expect to have adequate power to detect interaction effects. We therefore intend to study the effects of reward type (None, Monetary or Certificate) independently of the commitment type (None, Private or Public). We will control for commitment type while studying the effect of rewards, and vice versa. These treatments were randomized independently. The resulting assignment is presented in Tables 2.1, 2.2 and 2.3.

As we mentioned earlier, because the reward and commitment groups also received the basic/common intervention involving group-level meetings with a health motivator, groups that received *only* the basic intervention are a more appropriate 'control' to identify the treatment effects.

2.4 Data

2.4.1 Previous Surveys

As noted above, several rounds of surveys had been conducted for the previous study. Most relevant to this study are standard demographic and socio-economic status questions asked at baseline (December 2011 – February 2012) and latrine ownership data from Round 3 of the monitoring survey (December 2012 – January 2013).

2.4.2 Process Monitoring

During the intervention, Health Motivators recorded which households attended meetings and, in the commitment arms, which households agreed to make the pledge. Health Motivators also visited each household after each monthly meeting to check on the household's latrine. The primary purpose of this visit was to help the household assess its progress towards the goal, but it was also an opportunity to collect interim data.

2.4.3 Short-term outcomes

At the end of the intervention, we collected data on latrine investment, use and maintenance. In reward and recognition groups, these data were collected as part of the reward determination process. For budgetary reasons and because Health Motivators already had the training to assess latrine conditions, we used Health

Motivators to collect these data, but no Health Motivator collected data in a villages where he or she had led an intervention. The Health Motivators that collected data were not informed of the village's treatment status, nor which Health Motivators had led the intervention in that village. Similarly, Health Motivators were not told which of their peers had collected the evaluation data in villages where they had led the intervention In addition, to understand the mechanisms for the success or failure of the intervention, households were asked whether they received any assistance (financial, labor, advice) from community members, and whether they were pressured or encouraged by others in their group.

The criteria by which a household's latrine was judged "hygienic" for the purpose of the reward are given in Section 2.3.2. Precise definitions for the key outcome variables of interest are provided in Appendix 2.9.

Data were collected following the same protocol in non-reward and control villages. This allows us to see whether the reward treatment leads to a general improvement in the condition of latrines, or if the the reward leads maintenance leads households to focus on the specific criteria.

2.4.4 Follow-up

Longer-term outcome data were collected from a random 50% sample of households at endline in June 2015 – August 2015. This survey includes comparable measures of latrine status, use and maintenance.

2.5 Hypotheses and Empirical Tests

Hypothesis 1: Making a public pledge of joint commitment to build latrines will increase latrine ownership/access rates in the group. We expect the public commitment groups to have stronger investment effects than group meetings without any commitment or group meetings with private commitment (made by households privately with the Health Motivator). The private commitment has been added in the design to separate the effect of a public commitment, which could carry social sanction, from the direct psychological effect of making a commitment.

Hypothesis 2: Incentives and Rewards for group-level performance in achieving a given level of latrine ownership will increase latrine ownership rates. We expect incentives and rewards to have stronger investment effects than group meetings without incentives. We do not have a strong prior on whether the financial or the non-monetary reward will have a stronger effect.

2.5.1 Outcomes of interest

Overall, increasing access to and regular use of hygienic latrines while decreasing rates of open defecation is of primary importance. However, these are difficult to measure objectively. Households under observation have an incentive to overstate their use of and access to hygienic latrines, and understate their rate of open defecation, especially when a monetary reward is on the line. In contrast, whether a household owns a functional latrine and whether that latrine is being kept clean can be assessed in a fairly objective manner. Therefore, ownership of a latrine and the latrine's basic condition are our primary outcomes of interest, with self-reported use and open defecation as secondary measures. In addition, to understand mechanisms, we will assess households' spending on latrines, latrine parts and maintenance, as well as whether households report received any assistance (financial, labor, advice), encouragement, or pressure from other community members.

Our primary outcome of interest is household's access to as well as ownership of a hygienic latrine. The secondary outcomes that we are interested in include investment into specific latrine components as well as leakages from any latrine component. This also includes signs of hygienic behavior and maintenance

In Appendix 2.9, we describe how some of these outcome variables are constructed from our survey instrument.

2.5.2 Estimating Equation: Basic Treatment Effects

To measure basic program effects, we estimate

$\mathrm{y}_{igv} = \beta_0 + \beta_1 \mathrm{Incent}_v + \beta_2 \mathrm{Cert}_v + \beta_3 \mathrm{Priv}_v + \beta_4 \mathrm{Publ}_v$

$+ \delta y_{0igv} + \alpha' X_{igv} + \varphi_u + \varepsilon_{igv}$ where y_{igv} is the outcome variable of interest (e.g., ownership of a hygienic latrine) for household i, in grodup g, in village v, Incent_v and Cert_v are indicators for village v's reward treatment assignment (financial incentive and social incentive, respectively), Priv_v and Publ_v are indicators for village v's commitment treatment assignment (private commitment and public commitment, respectively), y_{0igv} is the pre-intervention level of the outcome variable, X_{igv} is a vector of pre-intervention level control variables, φ_u is a set of union fixed effects, and ε_{1gv} is an error term, which may be correlated at the village level.

The coefficients β_1 and β_2 represent the effects of the reward treatments, controlling for potential imbalances in the commitment treatment, while coefficients β_3 and β_4 represent the effects of the commitment treatment, controlling for potential imbalances in the reward treatment. These effects are conditional on receiving the common messaging treatment. To assess effects relative to no treatment at all we will include the pure control villages (Cell J in Table 2.1) in the sample and add an indicator for pure control. However, the default excluded category in Equation 2.5.2 consists of groups that received the basic health message treatment. This way we can compare between groups that received at least the common health message.

As noted above, we do not expect to have power to estimate interaction effects.

2.6 Results

Hygienic Latrine Access

The short-term and medium-term effects of the reward and commitment treatments on households' access to hygienic latrines are presented in Tables 2.4 and 2.5, respectively. Columns (1) and (2) report results for only the reward treatments, while columns (3) and (4) report the results for only the commitment treatments. Columns (5) and (6) report results for both the reward and commitment treatments in a combined specification. The regressions in the even numbered columns control for the baseline value of the outcome variable.[8]

[8] Mckenzie (2012) shows that such ANCOVA analyses can improve statistical power

Table 2.4 reports the short-term effects of the different treatment arms. Column (6) shows that in the short term, households in monetary reward and public commitment treatments both do significantly better than the control groups which received the basic health message. The treatment effect appears to be much larger for households in the monetary reward treatment - on average, an 8.1 percentage point higher access to hygienic latrine is observed compared to control households. The effect size is lower at 5.5 percentage point for households in the public commitment groups. The coefficient estimates for these two treatments are statistically significant at <1% and <5% levels, respectively. In absolute percentage terms, the effect size is much larger - at around 15% and 10%, for monetary reward and public commitment, respectively.

Column (6) from Table 2.4 suggests that we cannot reject the hypothesis that the impact of public commitment is equal to that of private commitment in the short term (p=0.318). On the other hand, we can reject the hypothesis that the impact of monetary reward is equal to that of certificate treatment in the short term (p=0.003)

Table 2.5 reports the medium-term effects of the different treatment arms. Point estimates suggest that after 15 months, the effect of the financial reward almost completely dissipates; however, the effect of the public commitment treatment on hygienic latrine access continues to persist. In fact, the effect size improves slightly to 7 percentage point over the control mean of 64%. The coefficient estimate is statistically significant at <1% level.

Column (6) from Table 2.5 suggests that we cannot reject the hypothesis that the impact of public commitment is equal to that of private commitment in the medium term (p=0.191). Similarly, we cannot also reject the hypothesis that the impact of monetary reward is equal to that of certificate treatment in the medium term (p=0.555)

Hygienic Latrine Ownership

While evaluating the effect of the different treatments, it is important to look into the ownership of hygienic latrine, not just access. This is because some of the treatment arms may induce households to share hygienic latrines within their group/neighborhood instead of investing into new latrines, which may drive the results for access but may not show any increase in own investments into hygienic latrines. However, Tables 2.6 and 2.7 (as well as in Figures 2.1 and 2.2) show that the treatment effects observed for hygienic latrine access can also be observed for ownership of hygienic latrines.

Consistent with the effects on hygienic latrine access, Column (6) in Table 2.6 shows that in the short term, households in monetary reward and public commitment treatments do significantly better than the control groups which received the basic health message. The treatment effect, again, appears to be much larger for the monetary reward group - households in this treatment arm report a 7.8 percentage point higher ownership of hygienic latrines compared to the control group. The effect size is lower at 4.7 percentage point for households in the public commitment groups. The coefficient estimates are statistically significant at <1% level. In absolute percentage terms, the effect size is higher at around 18% and 11% for monetary reward and public commitment, respectively.

Table 2.7 shows that after 15 months, the effect of the financial reward almost completely dissipates; however, the effect of the public commitment treatment on hygienic latrine ownership continues to persist. In fact, the effect size improves slightly to 5 percentage point over the control mean of 55.3%. The coefficient estimate is statistically significant at <5% level.

Because of a lack of statistical power, we do not report the point estimates for the interaction effects of the cross-cutting treatment arms. However, Figures 2.3 and 2.4 suggest that the interaction effects of commitment and reward arms are roughly additive both in the short and medium terms. In other words, there is no crowding out effect of the cross-cutting treatment arms.

Latrine Components, Leakage and Maintenance

Tables 2.8-2.13 show the short-term and medium-term effects of the reward and commitment treatments on overall maintenance and upkeep of latrines to keep them hygienic.

One possible reason why we see monetary incentives results in large short-term effects that dissipate after a year is that the monetary incentives induce simple, cheap and non-durable investments (like fixing a water seal or a broken slab or pit cover/ring). On the other hand, public commitment treatment induces more durable changes such as an actual toilet investment. There is some evidence to support this. Table 2.8 shows that the monetary incentive has a large and statistically significant effect in the short term on these relatively cheaper investments to make latrines hygienic, which fade in the medium term (see Table 2.13).

2.7 Conclusion

Research into the lack of adoption of relatively cheap and efficacious technologies usually looks into household-specific internal frictions and constraints. In this paper we explore a different type of market failure that may arise when investment decisions are inter-linked across households - adoption of hygienic latrines. We experimentally evaluate interventions designed to address coordination failures undermining adoption of a technology for which investment decisions are likely to be inter-linked.

We form groups of 15-20 neighboring households, and have them participate in monthly meetings to discuss sanitation, OD and disease risk. We introduce experimental variation in which the members of some groups are asked to make a joint public but non-binding commitment in front of each other, stating that they will try to address the OD issue in their neighborhood by investing in hygienic latrines. As a cross-cutting treatment, we introduce a form of joint liability, where all members of some (randomly chosen) groups are offered monetary rewards if the group attains a threshold latrine adoption rate. If a failure of collective action is keeping adoption rates inefficiently low, then a joint commitment is a cost-effective way to address the negative public health externalities that emanate from open defecation. Group level rewards may also be more cost-effective than direct household-level treatments such as subsidies for adoption, or individual-level marketing effort.

Preliminary reduced form estimates suggest that a group-level (small) financial reward has the strongest impact in the short term, inducing an approximately

12 percentage point increase in hygienic latrine ownership. The public commitment treatment, in which members would publicly commit to have a hygienic latrine at the end of a group meeting, induced an approximately 5 percentage point increase in hygienic latrine ownership in the short term. In the medium term the effect of the financial reward dissipates; however, the smaller effect of the public commitment treatment in the short term continues to persist after 15 months. The results suggest that low-cost interventions which take advantage of social network dynamics have the potential to improve the effectiveness of group-meeting based rural health interventions common in developing countries.

2.8 Tables

	Commitment		
Reward	None	Private	Public
None	A: 8 villages; 121 groups 1,898 households (9.8%)	B: 11 villages; 177 groups 2,626 households (13.6%)	C: 11 villages; 69 groups 1,088 households (5.6%)
Monetary	D: 10 villages; 79 groups 1,159 households (6.0%)	E: 5 villages; 58 groups 885 households (4.6%)	F: 9 villages; 97 groups 1,568 households (8.1%)
Certificate	G: 12 villages; 145 groups 2,314 households (12.0%)	H: 9 villages; 110 groups 1,694 households (8.8%)	I: 9 villages; 124 groups 1,970 households (10.2%)

Pure Control	J: 23 villages; 256 groups 4,069 households (21.1%)

Table 2.1: Randomization

Reward Type	Villages	Groups	Households
None	30	367	5,612 (29.1%)
Monetary	24	234	3,612 (18.7%)
Certificate	30	379	5,978 (31.0%)
Pure Control	23	256	4,069 (21.1%)
Total	107	1,236	19,271 (100.0%)

Table 2.2: Randomization: Reward Treatment

Commitment Type	Villages	Groups	Households
None	30	367	5,612 (29.1%)
Private	25	345	5,205 (27.0%)
Public	29	290	4,626 (24.0%)
Pure Control	23	256	4,069 (21.1%)
Total	107	1,236	19,271 (100.0%)

Table 2.3: Randomization: Commitment Treatment

	(1)	(2)	(3)	(4)	(5)	(6)
Monetary reward	0.129***	0.083***			0.125***	0.081***
	(0.038)	(0.020)			(0.037)	(0.020)
Reward certificate	0.054	0.013			0.050	0.011
	(0.044)	(0.021)			(0.044)	(0.020)
Hyg. latrine access (baseline)		0.607***		0.609***		0.607***
		(0.017)		(0.016)		(0.016)
Private commitment			0.014	0.027	0.025	0.032*
			(0.047)	(0.020)	(0.045)	(0.019)
Public commitment			0.074*	0.058**	0.068	0.055**
			(0.043)	(0.024)	(0.043)	(0.023)
Monetary – Certificate	0.075	0.070			0.075	0.070
	(0.048)	(0.025)			(0.045)	(0.023)
p-value	0.120	0.006			0.103	0.003
Public – Private			0.060	0.031	0.043	0.023
			(0.047)	(0.026)	(0.042)	(0.023)
p-value			0.209	0.240	0.307	0.318
Number of villages	84	84	84	84	84	84
Number of groups	980	980	980	980	980	980
Number of households	14,443	14,085	14,443	14,085	14,443	14,085
Omitted category mean	0.467	0.468	0.495	0.495	0.526	0.528

Notes: The ommited category in columns (1) and (2) consists of groups that did not receive a reward treatment. The ommited category in column (3) and (4) consists of groups that did not receive a commitment treatment. The ommited category in columns (5) and (6) consists of groups that received basic health message on hygienic latrines and sanitation practices. All regressions include union fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4: Short-term Effects – Household-level Data (Comparison: Health Info. Groups)
Household has access to hygienic latrine

	(1)	(2)	(3)	(4)	(5)	(6)
Monetary reward	0.040	0.013			0.035	0.011
	(0.037)	(0.024)			(0.035)	(0.023)
Reward certificate	0.053	0.029			0.047	0.026
	(0.042)	(0.032)			(0.042)	(0.030)
Hyg. latrine access (baseline)		0.360***		0.359***		0.358***
		(0.021)		(0.021)		(0.021)
Private commitment			0.013	0.027	0.020	0.030
			(0.047)	(0.032)	(0.046)	(0.032)
Public commitment			0.085**	0.072***	0.082**	0.070***
			(0.034)	(0.024)	(0.034)	(0.024)
Monetary – Certificate	-0.013	-0.016			-0.012	-0.015
	(0.042)	(0.029)			(0.038)	(0.026)
p-value	0.752	0.578			0.751	0.555
Public – Private			0.072	0.045	0.063	0.040
			(0.043)	(0.031)	(0.042)	(0.030)
p-value			0.096	0.156	0.137	0.191
Number of villages	84	84	84	84	84	84
Number of groups	979	979	979	979	979	979
Number of households	6,284	6,172	6,284	6,172	6,284	6,172
Omitted category mean	0.615	0.616	0.616	0.615	0.638	0.640

Notes: The ommited category in columns (1) and (2) consists of groups that did not receive a reward treatment. The ommited category in column (3) and (4) consists of groups that did not receive a commitment treatment. The ommited category in columns (5) and (6) consists of groups that received basic health message on hygienic latrines and sanitation practices. All regressions include union fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.5: Medium-term Effects – Household-level Data (Comparison: Health Info. Groups)
Household has access to hygienic latrine

	(1)	(2)	(3)	(4)	(5)	(6)
Monetary reward	0.129***	0.081***			0.124***	0.078***
	(0.034)	(0.016)			(0.033)	(0.016)
Reward certificate	0.048	0.016			0.043	0.012
	(0.035)	(0.015)			(0.035)	(0.014)
Owns hyg. Latrine (baseline)		0.642***		0.644***		0.641***
		(0.012)		(0.012)		(0.012)
Private commitment			0.005	0.008	0.014	0.013
			(0.037)	(0.015)	(0.036)	(0.014)
Public commitment			0.067*	0.049***	0.062*	0.047***
			(0.037)	(0.017)	(0.036)	(0.016)
Monetary – Certificate	0.080	0.065			0.081	0.065
	(0.041)	(0.019)			(0.038)	(0.017)
p-value	0.051	0.001			0.039	0.000
Public – Private			0.063	0.041	0.047	0.034
			(0.042)	(0.019)	(0.037)	(0.016)
p-value			0.136	0.035	0.199	0.041
Number of villages	84	84	84	84	84	84
Number of groups	980	980	980	980	980	980
Number of households	14,432	14,072	14,432	14,072	14,432	14,072
Omitted category mean	0.371	0.374	0.401	0.403	0.428	0.432

Notes: The ommited category in columns (1) and (2) consists of groups that did not receive a reward treatment. The ommited category in column (3) and (4) consists of groups that did not receive a commitment treatment. The ommited category in columns (5) and (6) consists of groups that received basic health message on hygienic latrines and sanitation practices. All regressions include union fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.6: Short-term Effects – Household-level Data (Comparison: Health Info. Groups)
Household owns hygienic latrine

	(1)	(2)	(3)	(4)	(5)	(6)
Monetary reward	0.059*	0.033			0.054*	0.031
	(0.034)	(0.021)			(0.031)	(0.021)
Reward certificate	0.050	0.031			0.044	0.028
	(0.036)	(0.027)			(0.036)	(0.025)
Owns hyg. Latrine (baseline)		0.427***		0.426***		0.425***
		(0.016)		(0.016)		(0.016)
Private commitment			0.006	0.017	0.013	0.021
			(0.040)	(0.028)	(0.039)	(0.027)
Public commitment			0.075**	0.053***	0.072**	0.050**
			(0.031)	(0.020)	(0.030)	(0.020)
Monetary – Certificate	0.010	0.002			0.011	0.003
	(0.036)	(0.023)			(0.033)	(0.021)
p-value	0.789	0.929			0.747	0.903
Public – Private			0.068	0.035	0.058	0.029
			(0.039)	(0.027)	(0.037)	(0.026)
p-value			0.086	0.194	0.122	0.262
Number of villages	84	84	84	84	84	84
Number of groups	979	979	979	979	979	979
Number of households	6,282	6,167	6,282	6,167	6,282	6,167
Omitted category mean	0.524	0.527	0.533	0.532	0.551	0.553

Notes: The ommited category in columns (1) and (2) consists of groups that did not receive a reward treatment. The ommited category in column (3) and (4) consists of groups that did not receive a commitment treatment. The ommited category in columns (5) and (6) consists of groups that received basic health message on hygienic latrines and sanitation practices. All regressions include union fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.7: Medium-term Effects – Household-level Data (Comparison: Health Info. Groups)
Household owns hygienic latrine

	(1) Broken or No Slab	(2) Broken or No Water Seal	(3) Broken or No Pan	(4) Broken Pit Cover	(5) Broken Pit Ring
Monetary reward	-0.009	-0.099***	-0.004	-0.060***	-0.063***
	(0.013)	(0.035)	(0.014)	(0.016)	(0.015)
Reward certificate	-0.011	-0.021	-0.005	-0.019	-0.025
	(0.010)	(0.040)	(0.014)	(0.016)	(0.015)
Private commitment	0.005	-0.030	-0.006	-0.014	-0.014
	(0.010)	(0.040)	(0.013)	(0.016)	(0.016)
Public commitment	-0.006	-0.051	-0.006	-0.022	-0.015
	(0.011)	(0.039)	(0.018)	(0.020)	(0.014)
Monetary – Certificate	0.002	-0.078	0.001	-0.041	-0.038
	(0.011)	(0.041)	(0.018)	(0.021)	(0.013)
p-value	0.842	0.061	0.959	0.052	0.005
Public – Private	-0.011	-0.021	0.000	-0.009	-0.001
	(0.013)	(0.040)	(0.016)	(0.017)	(0.016)
p-value	0.382	0.606	0.986	0.611	0.964
Number of villages	84	84	84	83	84
Number of groups	978	976	978	948	978
Number of households	12,143	10,835	12,073	8,606	12,071
Omitted category mean	0.034	0.310	0.071	0.147	0.123

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

Notes: The ommited category consists of groups that received basic health message on hygienic latrines and sanitation practices. All regressions include union fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.8: Short-term Effects – Household-level Data (Comparison: Health Info. Groups)
Are Different Latrine Components Visibly Broken?
(outcome variables conditional on latrine access)

	(1) Broken or No Slab	(2) Broken or No Water Seal	(3) Broken or No Pan	(4) Broken Pit Cover	(5) Broken Pit Ring
Monetary reward	0.014	-0.058*	0.001	0.002	0.006
	(0.013)	(0.030)	(0.013)	(0.017)	(0.014)
Reward certificate	-0.013	-0.032	-0.020*	-0.007	-0.012
	(0.009)	(0.033)	(0.011)	(0.019)	(0.015)
Private commitment	-0.006	-0.005	-0.012	-0.021	-0.002
	(0.011)	(0.034)	(0.012)	(0.021)	(0.016)
Public commitment	-0.023**	-0.043	-0.036***	-0.038***	-0.034***
	(0.011)	(0.033)	(0.013)	(0.014)	(0.012)
Monetary – Certificate	0.027	-0.026	0.021	0.009	0.018
	(0.013)	(0.031)	(0.015)	(0.017)	(0.014)
p-value	0.036	0.401	0.161	0.609	0.199
Public – Private	-0.017	-0.038	-0.024	-0.017	-0.033
	(0.012)	(0.032)	(0.012)	(0.019)	(0.016)
p-value	0.181	0.232	0.054	0.386	0.041
Number of villages	84	84	84	84	84
Number of groups	975	967	975	949	964
Number of households	5,677	5,163	5,676	4,535	4,979
Omitted category mean	0.058	0.229	0.117	0.108	0.092

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

Notes: The ommited category consists of groups that received basic health message on hygienic latrines and sanitation practices. All regressions include union fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.9: Medium-term Effects – Household-level Data (Comparison: Health Info. Groups)
Are Different Latrine Components Visibly Broken?
(outcome variables conditional on latrine access)

	(1) Any Leak	(2) Major Leak	(3) Latrine Linked to Ditch/Pond
Monetary reward	-0.047***	-0.038***	-0.010
	(0.016)	(0.013)	(0.011)
Reward certificate	0.004	0.004	0.003
	(0.016)	(0.012)	(0.011)
Private commitment	-0.011	-0.008	-0.026**
	(0.016)	(0.013)	(0.010)
Public commitment	-0.040**	-0.028**	-0.022
	(0.018)	(0.014)	(0.015)
Monetary – Certificate	-0.051	-0.042	-0.013
	(0.015)	(0.011)	(0.011)
p-value	0.001	0.000	0.231
Public – Private	-0.029	-0.020	0.004
	(0.017)	(0.011)	(0.014)
p-value	0.083	0.071	0.756
Number of villages	84	84	84
Number of groups	978	978	978
Number of households	12,146	12,146	12,102
Omitted category mean	0.105	0.062	0.047

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

Notes: The outcome variables in Columns (1) and (2) are dummy variables for any observed leakages in latrine pipe, Y junction, pit or the tank. The ommited category (in all three columns) consists of groups that received basic health message on hygienic latrines and sanitation practices. All regressions include union fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.10: Short-term Effects – Household-level Data (Comparison: Health Info. Groups)
Leakages in Primary Latrine Components
(outcome variables conditional on latrine access)

	(1) Any Leak	(2) Major Leak	(3) Latrine Linked to Ditch/Pond
Monetary reward	-0.010	0.001	-0.003
	(0.016)	(0.009)	(0.007)
Reward certificate	0.003	0.002	-0.009
	(0.018)	(0.010)	(0.007)
Private commitment	-0.006	-0.002	-0.014*
	(0.020)	(0.010)	(0.007)
Public commitment	-0.039***	-0.025***	-0.016**
	(0.014)	(0.009)	(0.008)
Monetary – Certificate	-0.012	-0.002	0.006
	(0.016)	(0.009)	(0.008)
p-value	0.443	0.859	0.437
Public – Private	-0.033	-0.024	-0.002
	(0.018)	(0.011)	(0.007)
p-value	0.069	0.039	0.774
Number of villages	84	84	84
Number of groups	975	975	975
Number of households	5,677	5,677	5,676
Omitted category mean	0.094	0.043	0.020

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

Notes: The outcome variables in Columns (1) and (2) are dummy variables for any observed leakages in latrine pipe, Y junction, pit or the tank. The ommited category (in all three columns) consists of groups that received basic health message on hygienic latrines and sanitation practices. All regressions include union fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.11: Medium-term Effects – Household-level Data (Comparison: Health Info. Groups)
Leakages in Primary Latrine Components
(outcome variables conditional on latrine access)

	(1) Bad Smell	(2) Flies or Insects	(3) Slippers/ Sandals	(4) Lat. Cleaning Agent	(5) Water for Handwashing	(6) Soap/Clay for Handwashing
Monetary reward	-0.127***	-0.284***	0.052*	0.062*	0.143***	0.138***
	(0.039)	(0.042)	(0.029)	(0.035)	(0.036)	(0.036)
Reward certificate	0.032	-0.086*	0.033	0.009	0.054	0.019
	(0.040)	(0.046)	(0.040)	(0.039)	(0.037)	(0.043)
Private commitment	0.044	-0.032	0.061	0.035	0.099**	0.053
	(0.036)	(0.045)	(0.045)	(0.043)	(0.041)	(0.049)
Public commitment	-0.053	-0.042	0.036	0.001	-0.012	0.030
	(0.043)	(0.048)	(0.024)	(0.032)	(0.033)	(0.030)
Monetary – Certificate	-0.159	-0.198	0.019	0.053	0.089	0.119
	(0.047)	(0.053)	(0.030)	(0.034)	(0.033)	(0.033)
p-value	0.001	0.000	0.535	0.117	0.009	0.001
Public – Private	-0.096	-0.009	-0.025	-0.034	-0.112	-0.023
	(0.049)	(0.051)	(0.045)	(0.040)	(0.038)	(0.042)
p-value	0.050	0.859	0.588	0.397	0.005	0.583
Number of villages	84	84	84	84	84	84
Number of groups	978	978	978	978	978	978
Number of households	12,135	12,125	12,127	11,944	11,685	11,519
Omitted category mean	0.539	0.512	0.141	0.450	0.400	0.400

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

Notes: The ommited category consists of groups that received basic health message on hygienic latrines and sanitation practices. All regressions include union fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.12: Short-term Effects – Household-level Data (Comparison: Health Info. Groups)
Signs of Hygienic Behavior and Maintenance in and around Primary Latrine
(outcome variables conditional on latrine access)

	(1) Bad Smell	(2) Flies or Insects	(3) Slippers/ Sandals	(4) Lat. Cleaning Agent	(5) Water for Handwashing	(6) Soap/Clay for Handwashing
Monetary reward	0.076	0.009	0.002	0.068*	0.050	0.049
	(0.048)	(0.046)	(0.026)	(0.034)	(0.039)	(0.035)
Reward certificate	0.084**	0.120*	0.016	0.051	0.025	0.065**
	(0.037)	(0.066)	(0.023)	(0.033)	(0.036)	(0.028)
Private commitment	-0.011	0.081	0.034	0.027	-0.009	0.031
	(0.036)	(0.067)	(0.024)	(0.037)	(0.040)	(0.032)
Public commitment	-0.121**	-0.045	0.044	-0.007	0.014	0.042
	(0.048)	(0.054)	(0.027)	(0.031)	(0.033)	(0.030)
Monetary – Certificate	-0.008	-0.111	-0.014	0.017	0.025	-0.016
	(0.052)	(0.044)	(0.028)	(0.029)	(0.032)	(0.032)
p-value	0.880	0.012	0.608	0.548	0.432	0.609
Public – Private	-0.110	-0.126	0.010	-0.035	0.023	0.010
	(0.050)	(0.068)	(0.029)	(0.034)	(0.034)	(0.032)
p-value	0.031	0.065	0.734	0.310	0.505	0.754
Number of villages	84	84	84	84	84	84
Number of groups	975	975	975	975	971	975
Number of households	5,677	5,673	5,677	5,602	5,318	5,612
Omitted category mean	0.610	0.467	0.220	0.442	0.520	0.477

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

Notes: The ommited category consists of groups that received basic health message on hygienic latrines and sanitation practices. All regressions include union fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.13: Medium-term Effects – Household-level Data (Comparison: Health Info. Groups)
Signs of Hygienic Behavior and Maintenance in and around Primary Latrine
(outcome variables conditional on latrine access)

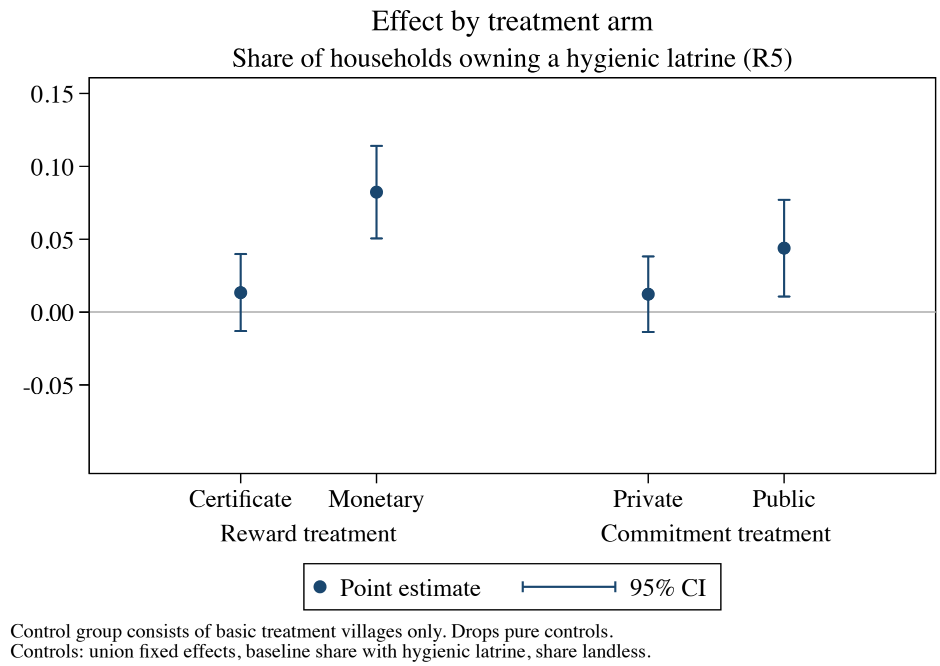

Figure 2.1: Short-term Effects (Comparison: Health Info. Groups)

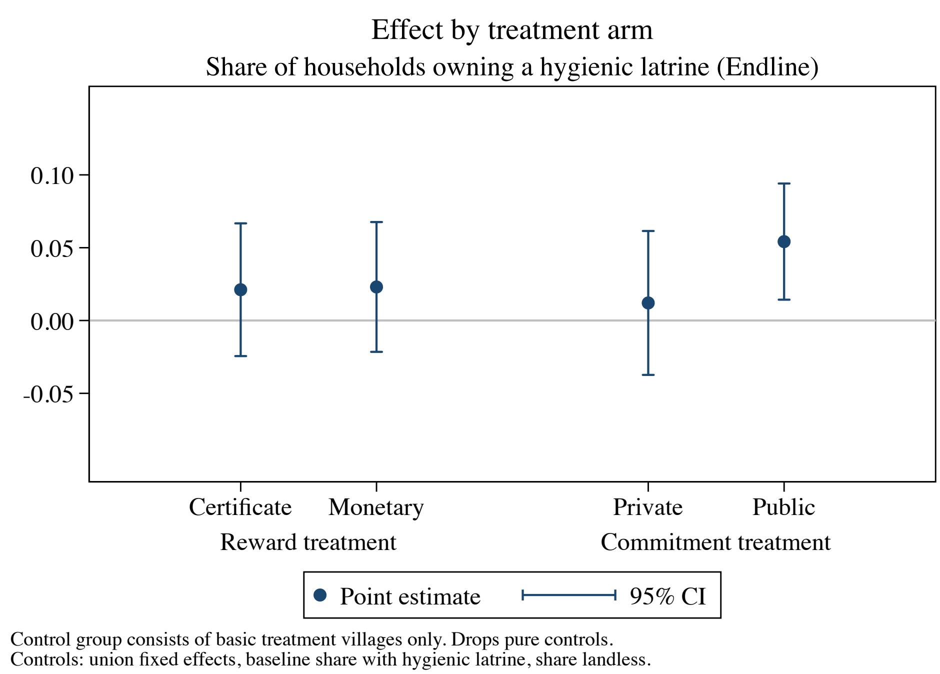

Figure 2.2: Medium-term Effects (Comparison: Health Info. Groups)

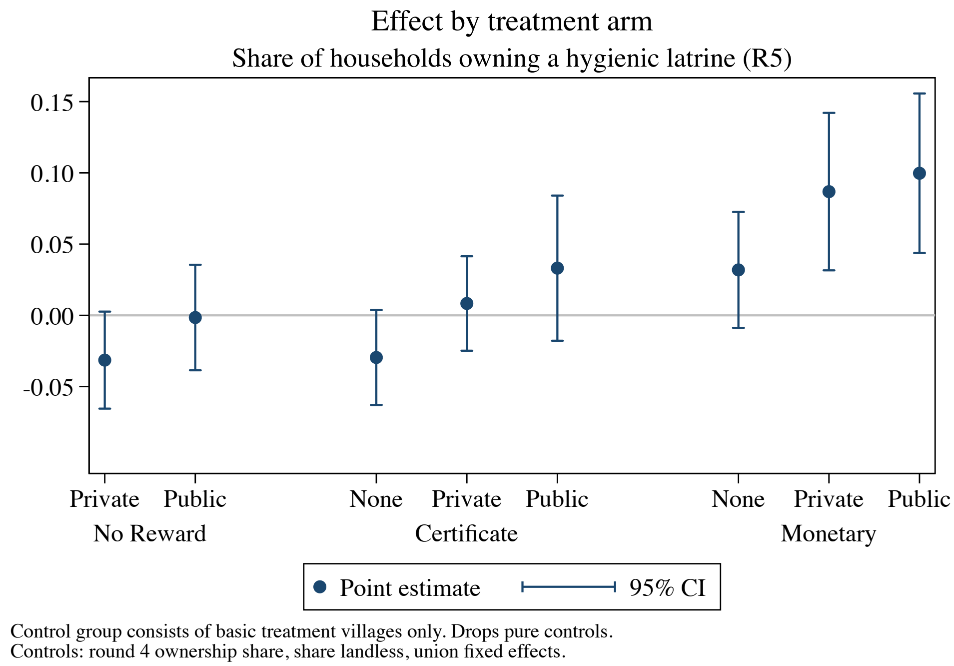

Figure 2.3: Short-term Effects - Interaction (Comparison: Health Info. Groups)

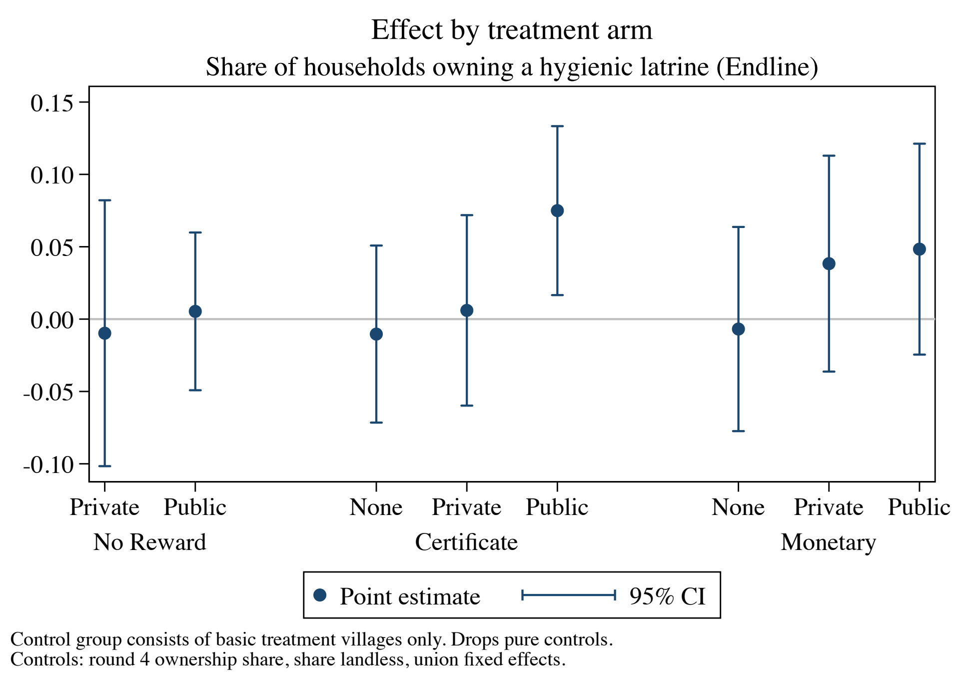

Figure 2.4: Medium-term Effects - Interaction (Comparison: Health Info. Groups)

2.9 Appendix: Outcome Variables

In this Appendix, we lay out how our outcome variables are constructed from our survey instrument. See Sections 2.3.2 and 2.4 for discussion of the concepts underlying these definitions.

- *Hygienic latrine:* Section 1, Q4; verification of the latrine's status based on other questions in Section 1 and visual verification by the survey enumerator.
- *Latrine ownership:* - Section 1, Q5 of Section 1, "What is the ownership status of the primary latrine?" (and verified by the survey enumerator)
- *Latrine ownership rate:* # of households owning a latrine in the group / # of households in the group. This is a group level outcome.
- *Latrine access:* Section 1, Q2, "Does the household have regular access to a latrine?"
- *Open defecation:* Section 1, Q4.

2.10 Appendix: Group Formation

The intervention supervisors who had also been involved with the first set of interventions – the "demand study interventions" – were tasked with the process of assigning households to groups. Because of their long stay in the survey area, the supervisors had developed a close understanding of the socio-dynamics of the sample. This helped them form groups without breaking any organic link between clusters of households in a neighborhood. The general instructions for constructing groups were:

1. Groups should consist of around 15 continuous / neighboring households;

2. Groups should generally not exceed 20 households

Generally, natural divisions such as rivers or open space between households were used to assign households into simple, continuous groups. However, some exceptions were made based on practical constraints, in particular when:

1. Households were located in isolated places. If the number of such isolated households was too few to form their own group (say, only 1-2 households) they were added to the nearest group. On the other hand, if the number of such nearby households was higher but still less than 15, these households were put together to form a group. Ultimately, only 6 groups of less than 10 households were formed.

2. At the other extreme, sometimes households were very densely packed and it proved difficult to separate them meaningfully into discrete groups. The largest group in the study area consists of 33 households. Ultimately, only 3 groups consisted of more than 23 households.

2.11 Appendix: Meeting Protocol

Followup-02/Final Cluster Meeting Training Manual

Step-01: On the day before the cluster meeting, the selected leader will be called on mobile so that s/he tells all other members to be present at the meeting on time.

Step-02: After reaching a cluster, at first the Health Motivator will go to each house of the cluster, exchange greetings and invite them to join the meeting at a selected place. Then s/he will inspect the household's latrine/s and collect the information on the electronic tabs. If the latrine is unhygienic (based on the criteria listed below) then it must be explained to the household what steps they need to take in order to make the latrine hygienic.

NOTE: Please ask the follow questions on the primary latrine which is the toilet facility that is used by the household members the majority of the time at the period during which the survey is being conducted.			
1	What kind of facility is the primary latrine used by the household?	01. Don't have any latrine/Open defecation	10. Single pit Ring-slab latrine (Offset) with water seal: intact
		02. Hanging latrine	11. Single pit ring-slab latrine (Offset) with water seal: broken
		03. Open Pit/hole without slab and lid or cover	12. Single pit Ring-slab latrine (Offset) with flip/ pollythene
		04. Pit latrine with slab but without lid or cover	13. Double pit Ring-slab latrine (Offset) with water seal: intact
		05. Pit latrine with cover	14. Double pit Ring-slab latrine (Offset) with water seal: broken
		06. Modern pit latrine with vent pipe	15. Double pit Ring-slab latrine (Offset) with flip/ pollythene
		07. Ring-slab latrine (direct) with water seal: intact	16. Eco Latrine
		08. Ring-slab latrine (direct) with water seal: broken/none	17. Sanitary latrine with septic tank
		09. Ring-slab latrine (direct) with flip/ pollythene	|___|___|
2	What is the ownership status of the primary latrine? CODE: 01=Toilet jointly owned/shared with another household, 02=private toilet solely owned by household, 03=community toilet, 04=uses someone else's latrine, 05=open spaces/bushes/hanging latrine		|___|___|
3	Where is the primary latrine located? CODE: 01=In own homestead (attached), 02=Outside own homestead (not attached), 03=Community latrine, 04=Another household/neighbor's latrine, 05=open defecation		|___|___|
4	How many other households share this latrine facility?		|___|___|
5	What is the current condition of latrine slab? CODE: 01=Fully Intact, 02=Partially Broken, 03=Completely Broken, 04=No slab, 05=N/A		|___|___|
6	Is there any visual evidence of <u>lumps of feces</u> in the toilet area? (does <u>not</u> apply for traces or floating fecal matter) *CODE: 01 = On Pan; 02 = On Slab, 03 = On both Pan and slab, 04 =No fecal matter seen, 05=N/A*		|___|___|
7	What is the current condition of the water seal? CODE: 01=Fully Intact, 02=Partially Broken, 03=Completely Broken, 04= No Water Seal, 05=N/A		|___|___|
8	(If offset) What is the current condition of delivery pipe? CODE: 01=Fully Intact, 02=Partially Broken, 03=Completely Broken, 04= No Delivery Pipe, 05=N/A		|___|___|
9	(If offset) What is the current condition of pit cover? CODE: 01=Fully Intact, 02=Partially Broken, 03=Completely Broken, 04= No Pit Cover, 05=N/A		|___|___|
10	Are there rings in the latrine pit? CODE: 01=Sufficient no. of Rings, 02=Insufficient No. of Rings, 03=No rings, 05=N/A		|___|___|

Notes for data collection:

1. Information has to be entered for hanging latrines
2. If latrine code is 03 then the response to Question 6 is 05/NA
3. If the latrine code is 08 and there is no evidence of feces lumps , then the response to Question 6 will be 04 or 'No fecal matter seen'
4. For hanging latrine (leaking latrine) even if the condition of the ring and pit cover is good, the response to Question-09 will be 02 or 'Partially broken'

5. Need to explain to the household that visible fecal matter makes the latrine unhygienic
6. We will also monitor rings to determine hygienic status – this should be explained
7. If the pit doesn't have sufficient number of rings, the latrine will be deemed unhygienic

Step-03: The meeting will start only when three-fourth of all the members of the cluster are present. The meeting will start by giving thanks to everyone. Everyone will introduce her/himself.

Step-04: Discussion on working as a group:

A. Purpose behind forming groups and solving a problem collectively:

- Unhygienic latrine - feces will be visible, spread bad smell and exposed to flies and insects. Extensive discussion later.
- Unhygienic latrines and unhygienic practices are a social/collective problem. If someone has an unhygienic latrine then it will adversely impact others as well. For example - (pointing finger to someone from the group) "Because of your unhygienic latrine someone else/someone else's children (pointing fingers to someone else) might get sick with cholera, typhoid, diarrhea, jaundice and polio. It is likely to bring economic, physical loss to you and your children."
- For this reason having hygienic latrine for one's own household is not enough, neighbors must have hygienic latrines as well.
- "Whose problem is this? Is this your problem? Does this problem need to be solved in a collective manner?" Because of the enormity of this problem, it needs to be solved in a collective manner.
- "Unity is strength" (Similar Bangla phrases to put emphasis on group work)

B. Read out the names of the members of the group and put emphasis on the importance of spreading the information to those who are absent.

Step-05: Health Motivators will start the discussion on the topics detailed below. Characteristics of a hygienic latrine needs to be repeated as many times as required. Discussion on what is on this script must end within 10-12 minutes.

A. DEFINITION, CHARACTERISTICS AND USE OF HYGIENIC LATRINES

Broad definition of hygienic latrine

1. Hygienic latrines limit the spread of diseases caused by water/feces and keep the environment pollution free.
2. Feces is enclosed in one place and it can not be seen from outside.
3. Flies or other insects cannot enter into the pit.

Most Important technical characteristics of a hygienic latrine:

(These are the characteristics which form the basis for judging if a latrine is hygienic or not. Repeat these characterisitcs often in the cluster meeting so that particiapnts can internalize these attributes.)

1. There must be a slab and it cannot be broken.
2. There must be a water-seal (gooseneck or siphon) and it cannot be broken.
3. There must be an adequate number of rings depending upon the depth of the well and those must not be broken.
4. There should be no gap between the cover of the slab and ring or between the ring and pit cover.
5. There should be no feces, flies or bad smell in or around the latrine.
6. Delivery pipe, y-junction, pit cover cannot be broken.
7. The latrine cannot be connected to a lake or any enclosed water body/ environment cannot be polluted.
8. There should be earth/cement moulded around the slab in case of direct pit latrines.

(Try to get these characteristics repeated by some of the participants)

<u>Methods of using a hygienic latrine:</u>

1. The pan should be made wet by using little water before each use. Feces will not stick on the pan if this is done
2. 1-2 pots (few liters) of water should be used after using the latrine
3. No solid object (like- stone, cloth, mud) should be thrown into the pan
4. The pan of the latrine and the slab/floor must be cleaned everyday.
5. If the latrine pit becomes full than another latrine must be built or the pit must be emptied.

B. Discuss about the number of hygienic/unhygienic latrines among the cluster members and their current condition. Disucuss what must be done to turn the current unhygienic latrines of the clusters into hygienic ones.

C. Targeted number of latrines, deadline and rewards (where applicable) as set by the office must be repeated infront of everybody present so that they can understand it clearly. Make sure that everyone has understood it by asking one or two of them.

D. Motivate each of the households to create and maintain separate hygienic latrine. It must be ensured that they understand that only one household will be rewarded (where applicable) for one hygienic latrine.

[Note: *<u>Cluster meeting participants cannot be shamed.</u>* During household latrine visit if the latrine is found to be unhygienic then the Health Motivator must explain the resaons as to why the latrine is unhygienic. But during the meeting the household member can never be pointed out and shamed. If any household member present in the meeting asks something about her latrine then her questions must be answered while visiting her home at the end of the meeting. However, if any participant asks a question which is applicable for all the participants then that answer can be given in front of everyone. For example if someone asks, "I have not built a separate cover for my offset latrine. But I have covered it up nicely with a carpet (*chatai* in Bangla). Is this hygienic?" answer to this sort of questions can be given in front of all the participants as everyone needs to know about this.]

Step-06: Like the initial meeting, the Health Motivator will conduct a group commitment session (for public commitment intervention) where people will commit to achieve the target. In case of private commitment, the Health Motivators will go to each household separately and make people commit privately.

<u>Commitment</u>

Public Commitment: During each cluster meeting, members from all the households of a cluster will commit publicly that those who do not yet have hygienic latrines will meet hygienic latrine standards as set by the project. Those with hygienic latrines would promise that they would help others reaching the goal within the time limit set by the project. The script of the pledge is as follows:

"I hereby promise before everyone present that I will do my best to set up hygienic latrines or improve existing ones into hygienic latrines for myself and for my neighbors by February 5, 2013."

Private Commitment: After organizing the cluster meeting, health motivators will visit each household in the cluster. The member of the household who had previously attended the meeting will make a commitment before the health motivator that he/she will transform their unhygienic latrines to hygienic ones within the time limit set by the project. The script of the pledge is as follows:

"I hereby promise that I will do my best to set up hygienic latrines or improve existing ones into hygienic latrines for myself and for my surrounding neighbors by February 5, 2013."

Reward Components

There are two threshold targets fixed for clusters, which is in terms of ownership of hygienic latrines. The lower threshold is 25% for Badhair while it is 33% for rest of the unions. The higher threshold is 50% for Badhair and 66% for the rest of the unions.

Monetary Incentive: A household will get a monetary reward of Tk. 250 or Tk. 500, depending on whether the household owns a hygienic latrine and the ownership of hygienic latrine at the cluster level is above the lower or the upper threshold, respectively.

Certificate: A household will get a certificate of hygiene attainment by a Member or the Chairman of the union, depending on whether the household owns a hygienic latrine and the ownership of hygienic latrine at the cluster level is above the lower or the upper threshold, respectively.

At the end of the meeting, the name of the members of the group should be repeated again and the cluster leader should be handed a complete list containing names of all the members of the cluster so that the group has a better understanding of its members.

Step-07: At the end of the meeting the Health Motivator will briefly recap the whole discussion from beginning to end and give thanks to everyone.

Decisions:

0. Every cluster must be treated the same way (except for the differences arising from the difference in treatment type).

1. Census form has to be filled up in case of new members of the cluster. But add format does not need to be filled for this round.

2. The 'add or deduct' from has to be filled up if any household migrates. Use the code ‘8888’ as before in the tab and in the tab, manually add to the cluster the household has been joined.

3. Similar to last rounds, in case of absent households, the form will be filled by using absent code 9999. Collecting information about the latrine of absent household is still a must.

4. If any new member (whose name is not in the village list) joins the meeting then enter 7777, give a space and then enter the name in the tab.

5. Cluster leader cannot be changed unless it is a special situation.

6. If any household member uses two latrines then enter information about the hygienic one. If both the latrines are used equally then collect information about the one which is closer to dwelling and contact with the field supervisor.

7. **Rewards will be given based on the total number of (hygienic) latrines in the cluster, not based on the number of households (or their access to hygienic latrines). Tell that to the household members a few times.**

8. Each field supervisor must meet two Health Motivators everyday and oversee 3 meetings.

9. The cluster meeting in which the superivisor is monitoring, has to end with a one-minute summary discussion. This has to be recorded as well.

10. Field supervisor will completely cross check the information about the latine provided by Health Motivator.

11. Information on the hardcopy filled by Health Motivator cannot be sent to the server without the concerned filed supervisor checking it.

12. Audio record of each of the meeting must be kept in the tab and submitted to the concerned field supervisor.

13. In the certificate intervention, there is no need to mention whose signature is going to be put in. Just mention that the certificates will be issued by the Union Parishad.

14. Do not use the word ‘leader’ in the meeting.

15. Consecutive meetings in the same cluster cannot happen within 18-21 days.

16. The new deadline is Feburary 5, 2013 to meet the hygienic latrine criteria. This should be mentioned instead of January 20.

17. Supervisors must monitor three meetings everyday. Cross checks of recordings of the Health Motivators will be done by the supervisors under the guidance of the Project Assistant and Project Associates.

18. Health Motivators and Field Supervisors will be rewarded based on how well the cluster meetings have been conducted according the scripts.

19. Need to finish meetings with the words that "This is the last time I have come to talk about hygienic latrines and the reward/certificate program (if applicable). The next time someone else will come to monitor your progress in achieving hygienic latrine status."

2.12 Appendix: Assessment

HHID: |___|___|___|___|___|___|

Cluster ID: |___|___|___|___|___|___|___|	Village ID: |___|___|___|	Village name: ______________	Union ID: |___|	Para name: _________________
Interviewer ID: |___|___|___|___|	Interviewer's name: ______________________	Supervisor's initial: ______________		Start Time:_______ End time: ________
HH head name: ______________________	Respondent's Name and ID: ___________________________ |____|____|			Date: _______________________

(N.B.: If you find anything out of the ordinary at this household, please take notes and contact your supervisor)

Consent

(READ TO RESPONDENT AT THE START OF THE VISIT) **Introduction:** Good morning/afternoon. My name is ___________. I am working with Innovations with Poverty Action, an international research organization. We are currently interviewing households for a study on how people make decisions about sanitation. This study covers 4 unions (Badhair, Chanduria, Pachandar, Saranjai) in the Tanore Upazilla of Rajshahi.

Procedures: We would like to invite you to participate in our study. If you choose to participate, today we will ask you some questions about the types of sanitation facilities your family uses and your family's interactions with other members of your community. This survey will take approximately 15 minutes of your time. We may also come back once in the next three-six months to conduct a longer follow-up survey that will include questions on the health of your family, the sources of water your family uses and your family's income and assets. The information collected in the follow-up surveys will be used to study how behavior in your community changes over time.

Risks and Benefits: We do not expect there to be any risk to you or your household associated with your participation in this study. There will be no direct benefit to you or your household for your participation in this survey. However, we hope that this research will result in findings that will help in improving access to sanitation facilities in communities throughout Bangladesh.

Confidentiality: All of your responses will remain confidential and will never be shared with anyone besides the researchers involved in this study. Neither your personal information nor any information linking your identity to your responses will ever be made public. Should you feel at any time that you are not comfortable answering a question or that your confidentiality is not assured, for example because someone else may be listening, please let us know.

Voluntary Participation: Participation in this study is completely voluntary. You can refuse to answer the entire survey, or you can tell us when a question makes you uncomfortable and we can skip that question. There is no need to answer any question that you do not wish to answer for any reason. If you like, you can end the interview at any time. There will be no penalty for ending or refusing to participate in the survey.

Questions and Concerns: If you have any questions, comments or concerns, you may contact Alamgir Kabir, Operations Manager, IPA Bangladesh at 01712121221. The relevant principal investigators and their contact information is as follows: 1. Mushfiq Mobarak, Yale University, +1 (203)-432-5787; 2. Raymond Guiteras, University of Maryland, +1 (301)-405-3480.

I have read (or someone has read to me) and understood the above information. I have had the opportunity to have any questions about this study answered and I agree to participate in this study. **CODE**: 01=Agree, 02=Do not agree **>> STOP SURVEY**, 03=Entire HH absent for extended period of time, 04=Migrated Household, 05=Combined Household, 06=Dwelling destroyed/perished |___|___|

<table>
<tr><th colspan="6">Section 1</th></tr>
<tr><td>1</td><td colspan="4">(ASK and OBSERVE) How many latrines does the household own? (00 if HH does not own any latrine)</td><td>|___|___|</td></tr>
<tr><td>2</td><td colspan="4">(ASK) Does the household have regular access to a latrine? CODE: 01=YES, 02= NO >> skip to Q04</td><td>|___|___|</td></tr>
<tr><td>3</td><td colspan="4">(ASK) Where is the latrine that is used most regularly by the household? CODE: 01=In own homestead, 02=Outside own homestead, 03= In neighbor's homestead</td><td>|___|___|</td></tr>
<tr><td colspan="6">(ASK if household doesn't open defecate): "Could you take me there?"</td></tr>
<tr><td rowspan="9">4</td><td rowspan="9">(OBSERVE AND ASK) What kind of facility is the latrine most regularly used (primary latrine) by the household? (if 01 then fill up Q12 to Q15 and Section 2)</td><td>01. Don't have any latrine/Open defecation</td><td colspan="3">10. Single pit Ring-slab latrine (Offset) with water seal: intact</td></tr>
<tr><td>02. Hanging latrine</td><td colspan="3">11. Single pit ring-slab latrine (Offset) with water seal: broken</td></tr>
<tr><td>03. Open Pit/hole without slab and lid or cover</td><td colspan="3">12. Single pit Ring-slab latrine (Offset) with flip/ pollythene</td></tr>
<tr><td>04. Pit latrine with slab but without lid or cover</td><td colspan="3">13. Double pit Ring-slab latrine (Offset) with water seal: intact</td></tr>
<tr><td>05. Pit latrine with cover</td><td colspan="3">14. Double pit Ring-slab latrine (Offset) with water seal: broken</td></tr>
<tr><td>06. Modern pit latrine with vent pipe</td><td colspan="3">15. Double pit Ring-slab latrine (Offset) with flip/ pollythene</td></tr>
<tr><td>07. Ring-slab latrine (direct) with water seal: intact</td><td colspan="3">16. Eco Latrine</td></tr>
<tr><td>08. Ring-slab latrine (direct) with water seal: broken/none</td><td colspan="3">17. Sanitary latrine with septic tank</td></tr>
<tr><td>09. Ring-slab latrine (direct) with flip/ polythene</td><td colspan="3">|___|___|</td></tr>
<tr><td colspan="6">For Q5 to Q13, <u>ASK</u></td></tr>
<tr><td>5</td><td colspan="4">What is the ownership status of the primary latrine? CODE: 01=Toilet jointly owned with another household, 02=private toilet solely owned by household, 03=community toilet >>Q12, 04=owned by others/neighbor>>Q08</td><td>|___|___|</td></tr>
<tr><td>6</td><td colspan="4">How much money was spent on the latrine (in total)? (in Tk.) CODE: 99=Don't remember/Don't know
NOTE: Ask about material/labor/transport cost separately before writing the total amount. If the cost is 99 taka, round it to 100 taka.</td><td>Total tk. _______________</td></tr>
<tr><td>7</td><td>a. Did you borrow any money to install the latrine? CODE: 01=Yes, 02=No>>Q08, 99= Don't Know>>Q08</td><td>|___|___|</td><td colspan="2">b) If yes, from whom? (Code below)</td><td>|___|___|</td></tr>
<tr><td colspan="6">Borrowing Code: 01=Bank, 02=NGO, 03=Cooperative/MFI, 04=Private (organization), 05= Private (Individual), 06=Bought on credit, 99=Don't Know</td></tr>
<tr><td>8</td><td colspan="2">How many <u>households</u> and <u>individuals</u> share the use of this latrine facility?</td><td colspan="2">|___|___| households</td><td>|___|___| individuals</td></tr>
<tr><td rowspan="2">9</td><td colspan="5">Identify the households who own the latrine (Use "99" if you cannot find the HH and fill up Section 03. If outside the survey area, use "55")</td></tr>
<tr><td>a. |___|___|___|___|___|___|___|</td><td>b. |___|___|___|___|___|___|___|</td><td colspan="2">c. |___|___|___|___|___|___|___|</td><td>d. |___|___|___|___|___|___|___|</td></tr>
<tr><td>10</td><td>a) How long has your primary latrine been in place? CODE: 01=0 - 6 months; 02 =6 months - 1 year; 03=1 - 2 years; 04 = 2 - 3 years; 05= 3-5 years; 06=more than 5 years</td><td>|___|___|</td><td colspan="2">b) If built since February, 2012 How many months ago was this latrine built? CODE: 99=can't remember/don't know, 77=built before February, 2012</td><td>|___|___|</td></tr>
<tr><td>11</td><td>a. Number of rings (USE "00" IF NO RINGS) (CODE: 99=Don't know) (Use 66 only for septic tanks)</td><td>|___|___|</td><td colspan="2">b. How deep is the pit? (Record answer in fet)
CODE: 99=Don't know</td><td>|___|___|</td></tr>
</table>

12	Where do the HH members usually wash their hands (henceforth, to be termed 'the hand-washing station') after going to the toilet? **CODE:** 01 = Inside toilet facility, 02=less than 3ft from toilet facility, 03=3-9ft of toilet facility, 04=more than 9ft from toilet facility, 05=No specific place **>>Q16**, 06=Do not wash**>>Q16**				|___|___|
13	Do HH members wash hands after returning from toilet? **CODE:** 01=Yes, with soap and water, 02= Yes, with only water, 03=Yes, With ash and water, 04= Yes, With clay and water (multiple responses allowed)				|___|___| |___|___|
	For Q14 to Q37, OBSERVE				
14	Is water available at the hand washing station? **CODE:**1=Yes, adequate water is available, 2=No, adequate water is not available, 3=No water available				|___|___|
15	Is soap and/or clay/ash available at the hand washing station? **CODE:** 01=Bar soap, 02=Powder/detergent, 03=Liquid soap, 04=Clay/ash, 05=No soap/clay/ash observed (multiple responses allowed)				|___|___| |___|___|
16	a. Have you (the interviewer) observed a HH member running to clean a latrine because of your presence or you suspect so? **CODE:** 1=Yes, 2=No ***(Take note if necessary)***				|___|___| ____________________________
17	What materials were used to construct the superstructures?	a. walls? |___|___| ______________		b. roofs? |___|___|____________	
CODE: 00=none, 01=mud, 02=leaves/stick/straw, 03=bamboo thatch, 04=plastic sheets/polythene, 05=CI sheets/tin, 06=wood, 07=clay tiles, 08=bricks, 09=RC/cement/concrete, 10=other (specify)					
18	Is the latrine pipe linked to any ditch/canal/pond etc.? **CODE:** 1=Yes, 2=No				|___|___|
19	**a)** (OBSERVE) Leakage of the latrine pipe, Y junction, pit or the tank? Code: 01=Major, 02=Minor, 03=No leak**>>Q21**	|___|___|	**b)** Leakage in which component? Code: 01=Pipe, 02=Pit, 03=Tank, 04=Y-junction ***(Can choose several)***		|___|___|, |___|___|, |___|___|
20	***If pit thought to be leaking*** what are the signs of leaking? **CODE:** 01=Wet soil around pit, 02=Water-logged area outside/around pit, 03=Floating feces outside pit, 04=Some odour, 05=Strong odour *(multiple responses allowed)*				|___|___|, |___|___|, |___|___|
21	***If offset*** what is the current condition of delivery pipe/Y junction? **CODE**: 01=Fully Intact, 02=Partially Broken, 03=Completely Broken, 04= No Delivery Pipe, 66=Not an offset pit latrine**>>Q23**				|___|___|
22	***If offset*** What is the current condition of pit cover? **CODE**: 01=Fully Intact, 02=Partially Broken, 03=Completely Broken, 04= No Pit Cover				|___|___|
23	Any broken ring? **CODE**: 01=Yes, 02=No, 66=No ring/Septic tank				|___|___|
24	Type of the latrine slab **CODE:** 00=No slab**>>Q28**, 01=Concrete/cement, 02=Plastic, 03=Bamboo, 04=Brick, 05=Earthen, 06=Others (mention)				|___|___|
25	What is the current condition of latrine slab? **CODE**: 01=Fully Intact, 02=Partially Broken, 03=Completely Broken				|___|___|
26	**a.** Type of pan **CODE:** 1=Ceramic commode, 02=Ceramic pan, 03=Plastic pan, 04=Concrete/Cement pan, 05=Tin, 06= Bamboo top/covered with polythene, 07=No pan**>>Q08**				|___|___|
26	**b.** What is the current condition of the pan? **CODE**: 01=Fully Intact, 02=Partially Broken, 03=Completely Broken				|___|___|
27	What is the current condition of the water seal? **CODE**: 01=Fully Intact, 02=Partially Broken, 03=Completely Broken, 04=No water seal				|___|___|
28	Presence of Vent pipe on the pit, inside the latrine or outside, condition and net? **CODE:** 01=Yes, good condition, with net, 02=Yes, good condition, without net, 03=Yes, broken, 04=No vent pipe				|___|___|
29	Has the 'slab and pan' (for direct pit) or pit cover (for off-set) been placed properly on top of pit so that flies cannot enter/exit? **CODE**: 1=Yes,				|___|___|

<table>
<tr><td></td><td>2=No, 66=No Slab/pan/pit cover</td><td></td></tr>
<tr><td rowspan="3">30</td><td>a) Any evidence of fecal matter on the latrine floor? CODE: 00=None, 01=Trace amounts only, 02=Lumps of Feces</td><td>|___|___|</td></tr>
<tr><td>c) Any fecal matter floating in the gooseneck/water-seal? CODE: 00=None, 01=Trace amounts only, 02=Lumps of Feces, 66=No water-seal</td><td>|___|___|</td></tr>
<tr><td>b) Any evidence of fecal matter on the pan? CODE: 00=None, 01=Trace amounts only, 02=Lumps of Feces, 66=No pan>>Q31</td><td>|___|___|</td></tr>
<tr><td>31</td><td>Is there a bad smell in and around the latrine? CODE: 01=Strong bad smell, 02=Some bad smell, 03=No bad smell</td><td>|___|___|</td></tr>
<tr><td>32</td><td>Is there any visibility of flies or insects inside or around the latrine? CODE: 01=Yes, 02=No</td><td>|___|___|</td></tr>
<tr><td rowspan="8">33</td><td>a) Slippers outside or inside latrine? CODE: 1=Yes, 2=No</td><td>|___|___|</td></tr>
<tr><td>b) Wet floor? CODE: 01=Yes, 02=No</td><td>|___|___|</td></tr>
<tr><td>c) Dis-coloration of pan (e.g. yellow/ green) CODE: 01=Yes, 02=No, 66=No pan>>e</td><td>|___|___|</td></tr>
<tr><td>d) Do you observe the following in the pan (that denotes lack of use) (Multiple response possible) CODE: 01= Leaves, 02= dirt, 03=spider webs, 04=Nothing</td><td>|___|___| , |___|___| , |___|___|</td></tr>
<tr><td>e) Water container in/near the latrine? CODE: 01=Yes, 02=No</td><td>|___|___|</td></tr>
<tr><td>f) Cleaning agents inside the latrine? CODE: 01=Broom/brush, 02=Harpik/Bleaching powder/detergent, 03=Ash, 04=No cleaning agent</td><td>|___|___|</td></tr>
<tr><td>g) Is there evidence that this latrine is used for storage? CODE: 01=Yes, 02=No</td><td>|___|___|</td></tr>
<tr><td>h) Does the path to the toilet suggest regular use (is it clear, well-worn, etc)? CODE: 01 = Yes; 02 = No</td><td>|___|___|</td></tr>
<tr><td>34</td><td>Level of the latrine pan: CODE: 01=Above yard height, 02=At yard height, 03=Somewhat lower than the yard, 04=Much lower than the yard</td><td>|___|___|</td></tr>
<tr><td>35</td><td>How many lumps of feces have you observed in the compound? CODE: 01=No lump of feces, 02=1-5 lumps, 03=6-10 lumps, 04=More than 10 lumps</td><td>|___|___|</td></tr>
<tr><td>36</td><td>Physical/Travel distance of latrine from main living room CODE: 00=Inside home compound, 01=01-10 ft, 02=11-20 ft, 03=21-30 ft, 04=More than 30 ft</td><td>|___|___|</td></tr>
<tr><td rowspan="2">37</td><td>a. (If the source of drinking water is TW or Dug well) Physical/Travel distance of latrine from the TW/ DW (Use next question's code)</td><td>|___|___|</td></tr>
<tr><td>b. Physical/Travel distance between the latrine and the source of water used in the latrine: Code: 00=Attached to the room, 01=01-10 ft, 02=11-20 ft, 03=21-30 ft, 04=More than 30 ft</td><td>|___|___|</td></tr>
<tr><td colspan="3">Section 02 (For Q38 to Q46 <u>ASK</u>)</td></tr>
<tr><td>38</td><td>Since November 01, 2013, (the month of Karttik in Bangla calendar) have you installed a complete latrine or any latrine parts? CODE: 01=Complete latrine>>Q40, 02=Latrine Parts, 03=Neither >> Q42</td><td>|___|___|</td></tr>
<tr><td>39</td><td>If you have installed any parts, list all such latrine parts (Use CODE B)</td><td>|___|___| , |___|___| , |___|___| , |___|___| , |___|___|</td></tr>
<tr><td colspan="3">CODE B: 01=Water seal/goose-neck/syphon, 02=Slab/Pit Cover, 03=Pit Cover with pan, 04=Pan, 05=Delivery pipe/Y Junction, 06=Ventilation pipe, 07=Rings, 08=Mud/bricks/cement/sand, 09=Door, 10=Other (detail in note)</td></tr>
<tr><td>40</td><td>Is this (for) the same latrine you have mentioned as your primary latrine? CODE: 01=Yes, 02=No</td><td>|___|___|</td></tr>
<tr><td>41</td><td>If you have installed a complete latrine or any parts Since November 01, 2013 what was the amount spent in total (In taka) (Including transportation, labor etc.)</td><td>|___|___|___|___|___|</td></tr>
</table>

42	Since November 01, 2013 ***(prompt 'in the last three months' or 'since mid-Karttik' of Bangla calendar)*** has anyone from your community assisted you ('with advice-information/material/labor/cash/transport') to make your latrine hygienic? **CODE:** 01=Yes, 02=No**>>*Q44***	|__|__|
43	Since November 01, 2013 if you have received any assistance from your community to make your latrine hygienic, list the Households and the kind of assistance. **(If cannot find HHID, use "99" and fill up section 03. If HH is outside the survey area, use "55")**	

a. HHID	b. Relationship with HH head (**Code:** 01=parents; 02=siblings; 03=children;04=near relatives;05=neighbor/community member; 06=other)	c. Assistance (**Code**: 01=Material; 02=Cash Loan **>>e**; 03=Cash help**>>e**; 04=Labor support**>>f**; 05=Advice or Information**>>g,h&i**; 06=Transport; 07=Other - Take Note)	d. What kind of material? (**Code B**);	e. Amount (in Tk.)	f. How many labor hours?	g. Frequency of mention/ discussion on latrines	h. In private or public gathering (01=private, 02=public, 03=both)	i. Did you feel pressurized? (**Code**: ***If No,*** use 00; ***If Yes***, use pressure scale 01-05; 01=least and 05=highest pressure)
a	**b**	**c**	**d**	**e**	**f**	**g**	**h**	**i**
|__|__|__|__|__|__|	|__|__|	|__|__|, |__|__|, |__|__|	|__|__| , |__|__| , |__|__|		|__|__|	|__|__|	|__|__|	|__|__|
|__|__|__|__|__|__|	|__|__|	|__|__|, |__|__|, |__|__|	|__|__| , |__|__| , |__|__|		|__|__|	|__|__|	|__|__|	|__|__|
|__|__|__|__|__|__|	|__|__|	|__|__|, |__|__|, |__|__|	|__|__| , |__|__| , |__|__|		|__|__|	|__|__|	|__|__|	|__|__|
|__|__|__|__|__|__|	|__|__|	|__|__|, |__|__|, |__|__|	|__|__| , |__|__| , |__|__|		|__|__|	|__|__|	|__|__|	|__|__|

44	a. Since November 01, 2013, have you received any assistance ('with material/cash/advice') from the government/UP or any NGO to make your latrine hygienic? **CODE:** 1=Yes, 2=No**>>Q45**					|__|__|
	b. NGO/Government/UP **Code:** 01=Government/UP; 02=BRAC; 03=World Vision; 04= IPA/VERC; 05=Other NGOs	c. Assistance Code (**Code**: 01=Material; 02=Cash Loan **>>e**; 03=Cash help**>>e**; 04=Labor support**>>f**; 05=Advice or Information**;** 06=Transport; 07=Other - Take Note)	d. What kind of material? (**Code** A); put commas in between each item	e. Amount (in Tk.)	f. Labor hours	Note
	|__|__|	|__|__|, |__|__|, |__|__|	|__|__| , |__|__| , |__|__|	|__|__|,|__|__|__|		
	|__|__|	|__|__|, |__|__|, |__|__|	|__|__| , |__|__| , |__|__|	|__|__|,|__|__|__|		
	|__|__|	|__|__|, |__|__|, |__|__|	|__|__| , |__|__| , |__|__|	|__|__|,|__|__|__|		

CODE B: 01=Water seal/goose-neck/syphon, 02=Slab/Pit Cover, 03=Pit Cover with pan, 04=Delivery pipe, 05=Ventilation pipe, 06=Rings, 07=Mud/bricks/cement/sand, 08=Transportation, 09=Labor, 10=Door, 11=Superstructure (e.g. tin), 12=Other (detail in note)

45	Did you attend or know of any meeting sessions that were conducted in your village to promote hygienic sanitation between November 01, 2013 to February 05, 2014? **CODE:** 01= Yes, attended, 02=Knows, but did not attend, 03=Neither **>>Q47 >>END**				|___|___|
46	***(Even if HH didn't attend)*** What information was covered in these meetings? ***(Do not read out the options aloud)*** **CODE:** 1=Yes; 2=No				
	a. Private Pledge	|___|___|	i. Reward (Money)		|___|___|
	b. Public Pledge	|___|___|	j. Reward (Certificate)		|___|___|
	c. Deadline (regarding hygienic latrine attainment)	|___|___|	k. Target (percentage or no. of households)		|___|___|
	d. Disadvantages of Open Defecation	|___|___|	l. Importance of washing hands		|___|___|
	e. Importance of proper disposal of feces	|___|___|	m. Installing a hygienic latrine		|___|___|
	f. Importance of hygienic sanitary habits	|___|___|	n. Assessing quality of latrine parts		|___|___|
	g. Importance of using hygienic latrine	|___|___|	o. Places where quality latrines parts are available		|___|___|
	h. Maintaining hygienic latrine	|___|___|	p. Others (Specify): ___________		|___|___|
47	**a.** Did any of your neighbor build a latrine since November 01, 2013? **CODE:** 01=Yes; 02=No **>>Q48**		|___|___|	**b.** How many households?	|___|___|
46	***(Even if HH didn't attend)*** What information was covered was covered in these meetings? ***(READ OUT the options)*** **CODE:** 1=Yes; 2=No				
	a. Private Pledge	|___|___|	i. Reward (Money)		|___|___|
	b. Public Pledge	|___|___|	j. Reward (Certificate)		|___|___|
	c. Deadline (regarding hygienic latrine attainment)	|___|___|	k. Target (percentage or no. of households)		|___|___|
	d. Disadvantages of Open Defecation	|___|___|	l. Importance of washing hands		|___|___|
	e. Importance of proper disposal of faeces	|___|___|	m. Installing a hygienic latrine		|___|___|
	f. Importance of hygienic sanitary habits	|___|___|	n. Assessing quality of latrine parts		|___|___|
	g. Importance of using hygienic latrine	|___|___|	o. Places where quality latrines parts are available		|___|___|
	h. Maintaining hygienic latrine	|___|___|	p. Others (Specify): ___________		|___|___|

Chapter 3: Training Mentors? Experimental Evidence From Female-Owned Microenterprises In Ethiopia

3.1 Introduction

Ownership of, and employment in, microenterprises account for a large fraction of the labor market in the developing world. Despite the size and potential of this sector, microenterprises face many challenges that include poor access to capital and products, high closure rates, etc. It is, therefore, not surprising that in many countries, microenterprises underperform other forms of businesses in terms of productivity and profitability (Brooks, Donovan & Johnson, 2016). One possible explanation for the dismal outcomes of microenterprises is that microenterprise owners lack managerial capital, which is the skill-set required to run a business (Bloom and Van Reenen, 2007; Bruhn et al., 2010; McKenzie and Woodruff, 2016). Policymakers have attempted to address this potential constraint that small businesses face by scaling up business training programs all over the developing world, spending over a billion dollars per year pursuing various forms of training (Blattman and Ralston, 2015). Despite this effort, the measured impact of this kind of training on business profit or operational scale appears to be modest, at best (McKenzie and Woodruff, 2013).

McKenzie and Woodruff (2016) argue that the relationship between 'better' business practices[1] and performance is causal and that the lack of effect observed for most business training programs is because firms do not change business practices enough rather than because these practices do not matter.

Moreover, a critical review of the literature by McKenzie and Woodruff (2013) suggests that many of the business training evaluations measure impacts only within a year of training. Evaluations done in such short term is potentially problematic because it may take longer than a year for business practices adopted from training programs to translate into improved business outcomes.

To address this gap in the literature, we evaluate the impact of a well-designed business training curriculum on business practices and performance of small firms both in the short term (within a year) as well as in the longer term (approximately, 3 and 4 years after the program ended). The business training program targeted high-potential female entrepreneurs in five regions of Ethiopia and was carried out as a randomized control trial (RCT). We find that formal business training does not produce any significant impact on business outcomes in the short term, but does improve the adoption of business practices taught to them in the training program.

[1] The study uses 26 questions that measure business practices in the areas of marketing (e.g., does the firm advertise? does it attempt to attract customers with a special offer? does it ask customers what other products they would like it to sell?), record keeping (e.g. does the firm record every sale and purchase? has it worked out the cost of each item it sells? does it have a written budget?), financial planning (e.g. does it have a sales target for the next year? does it have a balance sheet and profit and loss statement?), and buying and stock control (e.g. does it frequently run out of stock? does it attempt to negotiate discounts with suppliers?). These are intended to be universal best practices, in the sense that most firms should benefit from using them. They are closely based on the goals of business training programs like the ILO's Improve Your Business (IYB) program.

In the longer term,the training program appears to induce a strong positive effect on different dimensions of business outcomes, such as reported profits and revenues as well as in business diversification. This effect appears to be driven by the continued adoption of several of the 'better' business practices identified by McKenzie and Woodruff (2016) and taught in the business training program.

Given the extent of micro-entrepreneurial activities in developing countries, it is, however, important to identify low-cost programs that can lead towards the adoption of these business practices with the aim of subsequently improving business performance. Standard business training programs, like the one mentioned above, are expensive - Blattman and Ralston (2015) report that classroom training can cost over 100 USD per student. Collectively, several billion dollars have been spent behind business training all over the world involving millions of trainees.[2] This problem could be potentially addressed by a mentorship program involving trained mentors: after more experienced businesses are given formal training, they can be connected with smaller firms from their networks to pass on what they learned in the training program. This can be a promising low-cost approach to business training programs that can organically disseminate relevant information and advice on business practices to new and fledgling businesses.

To understand the potential impact of such a program, the program implementer connected the trainees of the business training program to a random subset of small firm owners within their social and business networks that they had identi-

[2]International Labor Organization's Start and Improve Your Business program has claimed to have trained more than 4 million people in 100 countries since 1977.

fied or nominated before they received the business training. We find that treatment mentees exhibit significant effects in the short term, but only for business practices.

This study is similar to the impact evaluation of a parallel training and mentorship program by Brooks, Donovan and Johnson (2016), where they directly compare the impact of one-to-one mentorship to a traditional business training program. They find that in the short term, one-to-one mentorship program results in higher business profits as a result of a transmission of local market information, such as potential low-cost suppliers. However, the effect appears to fade in the longer term. Our study departs from Brooks, Donovan and Johnson (2016) in two important ways. First, we evaluate whether firms, when given formal business training, adopt 'better' businesses practices in marketing, stock-keeping, record-keeping, and financial planning that have been identified by Mckenzie and Woodruff (2016) to improve business performance and whether such practices translate into better outcomes for the firms. We specifically look into whether the effects of the formal business training can be better captured if business outcomes are measured in the longer term. Secondly, if business performance is indeed limited by the adoption of these practices, a mere pairing with an experienced mentor (as done by Brooks, Donovan and Johnson, 2016) may not induce improved business performance in the medium and long term. However, such an effect is possible through the provision of mentorship through trained mentors – if communication of the better business practices happens effectively through the mentorship.

Together, the formal business training program and the mentoring program - which was carried out in Ethiopia - is known as the Women Agribusiness Leaders

Network (WALN) project. The evaluation of this project allows us to investigate the importance of business practices such as marketing, stock-keeping, record-keeping, and financial planning for small, female-owned firms in Ethiopia, and the longer-term consequences of adopting these practices on business performance. The paper is organized as follows: section two describes the program, section three the design of the experiment and how we isolate mentor-mentee effects, section four discusses our hypotheses and main outcomes, section five the results, and section six concludes.

3.2 Program Description

The Women in Agribusiness Leaders Network (WALN) uses a randomized controlled trial (RCT) design to assess the impact of a first-stage traditional training program and a second-stage mentorship which was carried out by the trainees of the first stage training. Half the mentors and mentees eligible to participate in the WALN program were randomly assigned to receive business training and mentoring interventions, respectively. The other halves are the control groups that did not receive the interventions. Comparing the treated groups to the control groups allows us to separately measure the impact of the business training program and the mentorship program.

3.2.1 Study Overview

This study takes place in the context of the Agribusiness and Market Development (AMDe) component of the Government of Ethiopia's Agricultural Growth

Program-1 (AGP-1), funded by USAID. The intervention is based on the idea that training, mentoring and networking could expand women's capacity to play leadership roles in sectoral organizations and manage business profitably. This is expected to improve women's participation in leadership and decision-making in the overall agriculture sector in Ethiopia.

The Africa Gender Innovation Lab of the World Bank partnered with USAID and the Government of Ethiopia to evaluate the impact of WALN. The impact evaluation of WALN was designed to capture the business, psychosocial and social-network effects of business training and mentoring on mentors and mentees.

3.2.2 Description of the Interventions

ACDI/VOCA, the program implementer, created a pool of eligible applicants based on a predetermined set of selection criteria that were applied to information that applicants provided in their application forms. Criteria included participation in higher potential agricultural value chains and having a fairly well established business. Potential mentees were also nominated at the same time that applicants (later to become potential mentors) were applying to the program.

In addition to business training, the WALN interventions included an innovative mentoring component that leveraged the mentor's existing social networks to identify mentees who were given customized business advice, aimed at increasing business performance and community leadership of participating business women by improving their business skills, self-confidence and business networks. This is thought to be a promising low-cost approach to business training that organically extends relevant information and advice to new and fledgling businesses. The following modules were developed and administered by ACDI/VOCA, during the business training intervention provided to the study participants, between May and October 2014:

Module I – Business development: The training package included sections on business planning and management; which aimed to build the participants' understanding about what business plans are, their benefits and what making business plans entails.

Module II – Leadership: with the aim to build women's confidence in leadership roles as well as give them technical skills to make them better managers, qualified international and national business specialists and leaders delivered classes, two days a month for six consecutive months. Case studies, facilitated discussions and group work were primarily employed for content delivery.

Module III – Mentoring and coaching: Women in the leadership training (mentors) were identified and given special training to assist other women (mentees) to develop specific skills and knowledge to enhance their professional and personal growth.

Module IV – Communication: the module covered topics including the meaning of commu- nication and its importance; role of communication; the process of communication; types of communication; barriers to effective communication, and related topics.

3.3 Experimental Design and Data

Randomization Process

The pool of eligible applicants became the sample for the baseline survey. Treatment was then randomly assigned to eligible applicants who responded to the baseline survey. The program operated in Ethiopia's Agricultural Growth Program's (AGP) target woredas of five regions of Ethiopia: Tigray, Amhara, Oromia, Addis Ababa and SNNPR. The impact evaluation covers the business training and mentoring activities across all regions.

Mentor randomization was stratified by region and firm-size tercile. Half of the mentors receiving formal business training covering the modules listed above. Mentors came to a training facility two days a month for six consecutive months.

Half of the mentees of treated mentors were randomly assigned to receive mentoring, while the remaining half were assigned to the control group. After the business training was concluded, mentors were asked to meet their mentees one day a month for six consecutive months. Mentors were specifically told to provide mentorship to those that were randomly selected into the mentorship program. However, since both treatment and control mentees belong to the same social/business network of the treatment mentor, there is a possibility of spillover across treatment status among the mentees. To address this issue, we include mentees nominated by control mentors in the impact evaluation. This can help us measure the spillover effect of being in a treatment mentor's network by directly comparing the outcomes of the control mentees (of the treatment mentors) and the pure control mentees (of the control mentors - who did not receive any business training). Figure A1 in Appendix A shows the WALN impact evaluation design and sample size in detail.

We conducted balance tests for a set of individual, household, and business characteristics. The tests were conducted using Ordinary Least Squares (OLS) for continuous variables and the Linear Probability Model (LPM) for binary variables. A regression of the variables of interest was run on two different treatment dummy variables representing either: (1) Treatment Mentor (TM) and Control Mentors (CM) or (2) Treated Mentees of Treated Mentors (TMTM) and Control Mentees of Treated Mentors (TMCM). Finally, to address potential spillovers, we make use of a dummy variable representing either (3) Treated Mentees of Treated Mentors (TMTM) or Control Mentees of Control Mentors (CMCM).

Baseline balance tests on observables for the mentor and mentee samples are shown in Tables 3.1, 3.2 and 3.3. Most of the variables are balanced across the different treatments. The relative size of the mentor and mentee businesses are also evident. For example, mentor monthly profits are approximately twice as large as those of their mentees. Mentors are, on average, two years older than their mentees.

Follow-up Surveys

Sections A1 and A2 in Appendix A shows the WALN impact evaluation design in detail, in particular, the number of women surveyed in each round of data collection as well as the attrition in the sample from the baseline through three post-intervention follow-up data. We start with a sample of 194 treatment and control mentors and 1126 mentees. Given the small initial sample of mentors, we placed a premium on follow-up. As observed in Section A2, overall attrition is low and does not vary by treatment status.

The WALN baseline survey was conducted from March to August 2014. Business training took place from August to December 2014 and this was followed by mentoring sessions from January to July 2015. The first follow up survey took place between August and December 2015, about 6 months after the business training was concluded and 1 month after the mentoring sessions ended. Data collection and data cleaning activities for the same were concluded by December 2015. Data collection for the second follow-up was carried out in the summer of 2017. Data for the final and the third follow up was carried out at in February-April, 2018.

3.4 Outcomes of Interest and Hypotheses

3.4.1 Outcomes of interest

Due to the fact that we have a number of variables which capture similar or identical constructs, we have regrouped the variables of interest into families of outcomes whenever it was possible to do so. For each family of outcomes, we have created an average z-score index by ensuring all variables in the outcome were coded in the same direction, calculating the z-score of each variable by subtracting the control-group mean and dividing by the control group standard deviation, and averaging the z-scores of the outcomes for each family.

The individual outcome variables that were used to create the following family of outcomes are explained in Section A3 of Appendix A:

- Profits (and revenue) index

- Index for marketing and customer service practices
- Record keeping and financial management practices index
- Stock-control index
- Business diversification

3.4.2 Hypotheses

The experimental design allows us to test the following hypotheses, that we have pre-specified in our analysis plan.

Hypothesis 1a: Getting formal business training (and being assigned as a mentor) increase profits. The experimental design allows us to only investigate the aggregate effect of receiving a formal business training as well as providing mentorship.

Hypothesis 1b: Getting formal business training (and being assigned as a mentor) improves business practices. We posit that the main channel through which businesses can perform better is the adoption of business practices, highlighted strongly in the training program and potentially internalized through a 'learning by teaching' model while mentoring other firms.

Hypothesis 2a: Receiving mentorship from trained mentors improves own profits. We hypothesize that monthly, customized mentorship sessions from more experienced and trained mentors can improve business outcome for smaller, younger firms.

Hypothesis 2b: Receiving mentorship from trained mentors improves own business practices. We hypothesize that the main channel through which mentee businesses can perform better is the adoption of business practices, highlighted in the mentorship program. It is also possible that mentees can experience higher business profits as a result of a transmission of local market information from mentors.

3.4.3 Estimating Equation: Basic Treatment Effects

Business Training

We measure the basic (intent-to-treat) program effects of the business training in the following ANCOVA specification:

$$y_{i,t} = \beta_0 + \beta_T \text{BusinessTraining}_i + \beta_1 Y_{i,0} + \sum \theta_s 1(i \in s) + \sum_{j=1}^{k} \delta_j 1(j = t) + \varepsilon_{i,t} \quad (3.1)$$

where $y_{i,t}$ is the outcome variable of interest (e.g., profit) for firm i, survey round, t; θ_s is the strata fixed effects; δ_j survey round fixed effects; $\varepsilon_{i,t}$ is the error term, which is clustered at firm level (because of firm level randomization within a strata); $Y_{i,0}$ is the lag of the dependent variable in order to maximize power (McKenzie, 2012).

Mentorship

We measure the basic (intent-to-treat) program effects of receiving mentorship in the following way:

$$y_{i,t} = \beta_0 + \beta_T \text{Mentorship}_i + \beta_1 Y_{i,0} + \sum \theta_M 1(i \in M) + \sum_{j=1}^{k} \delta_j 1(j = t) + \varepsilon_{i,t} \quad (3.2)$$

where $y_{i,t}$ is the outcome variable of interest (e.g., profit) for firm i, survey round, t; θ_M is the strata fixed effects; δ_j survey round fixed effects; $\varepsilon_{i,t}$ is the error term, which is clustered at the firm level (because of firm level randomization within a mentor); $Y_{i,0}$ is the lag of the dependent variable in order to maximize power.

Spillover

In the above estimating equation (Equation 2), we compare treatment and control mentees who belong to the social/business network of the same treatment mentor. There is, thus, the possibility of spillover across treatment and control mentees (of treatment mentors) which can bias the treatment effects downwards. To address this issue, we can directly compare treatment mentees (of treated mentors) and pure control mentees (of control mentors). The basic (Intent-to-treat) program effects of receiving mentorship (when compared against pure control mentees) is as follows:

$$y_{i,t} = \beta_0 + \beta_T \text{Mentorship}_i + \beta_1 Y_{i,0} + \sum \theta_S 1(i \in S) + \sum_{j=1}^{k} \delta_j 1(j = t) + \varepsilon_{i,t} \quad (3.3)$$

where $y_{i,t}$ is the outcome variable of interest (e.g., profit) for firm i, survey round, t; θ_S is the strata fixed effects; δ_j survey round fixed effects; $\varepsilon_{i,t}$ is the error term, which is clustered at the firm level (because of firm level randomization within a mentor); $Y_{i,0}$ is the lag of the dependent variable in order to maximize power.

3.5 Results

As indicated in Equations 1 through 3, our primary specification is an intent-to-treat analysis using all the follow-up rounds pooled together, along with the lag of the dependent variable in order to maximize power. Tables 3.1, 3.2 and 3.3 show that the treatment and control arms in the business training and mentorship programs are balanced at baseline for most of the relevant observables.

Impacts on mentors

We test the hypothesis that a business training program can improve business performance by looking into an index of profit and revenue as well as their individual components (Table 3.4). We pool impacts over the three post-treatment waves to maximize statistical power, with the coefficients then representing the average impact over 3 years after treatment ends. Point estimates suggest large and statistically significant impacts of attending the business training program on revenue and profits. The profit and sales index suggests an improvement of 0.20 standard deviations. The IHS estimates for the revenue and profit variables suggest a pooled impact of around 63-67%.

Table 3.5 shows the impact of the business training intervention on business skills. Point estimates suggest that treatment mentors fare markedly better in terms of some of the key skills identified by Mckenzie and Woodruff (2016) as well as

Bloom and Van Reenen (2007) and Bruhn et al (2010). Specifically, treatment mentors are 19 percentage points (58%) more likely to have a written business plan than control mentors. A similar magnitude of impact holds for having an annual budget. Although at a smaller scale, treatment mentors were also found to have a greater probability of keeping financial records -- 9 percentage points higher than the control group. The marketing, record-keeping and stock-control indices show strong and positive movements as a result of the business training program as well.

In addition to changes in practices, Table 3.6 documents some evidence of diversification. Treatment mentors have an average of 0.22 more businesses, indicating that the training may have spurred some business creation. For example, qualitative work indicated that one entrepreneur opened a supermarket as an outlet for the dairy products she was processing.

In Appendix tables A4-A6, we investigate the time trend of the program effects. While practices (table A5) show a big and significant effect in the first follow-up and persist in the final round, profits show a different trend. Profits show no significant impacts in the first follow up (table A4) and then show significant impact in later rounds. In terms of magnitude, Table A4 shows a clear upward movement in the average z-score index of last month's revenue and profit.

Similarly, Table A shows an upward movement in the average z-score index of last month' profits for the WALN-focused business. From this, we can see that that formal business training does not produce any significant impact on business performance in the short term despite improvements in the adoption of business practices taught to them in the training program. In the longer term, however, the training program induces a strong positive effect on different dimensions of business performance, such as reported and constructed profits as well as the number of business activities (appendix table A7).

Taken together, these results suggest that the large changes in business practices induced by the training led to higher profits, with effects growing stronger over time.

Impact on mentees

We test the hypothesis that the mentorship elements of the program can improve business performance of mentees by looking into an index of profit and revenue as well as their individual components. In Table 3.7 we compare the treatment and control mentees within a given (treated) mentor. Similar to the training program, we pool impacts over the three post-treatment waves to maximize statistical power, with the coefficients then representing the average impact over 3 years after treatment ends. Although the point estimates are mostly positive, they are quite small and the impact of the mentorship program is not statistically significant for revenues, profits, or an aggregated index of these measures.

In Table 3.8, we examine the impact of being mentored on business practices. When comparing the control and treatment mentees of the mentors who received the business training, we see small point estimates and no significant impacts. While mentors were assigned to work with a random subset of their mentees, it is possible that they either spoke about the practices they had learned with all of their potential mentees or that mentees spoke with each other, since they were part of the same pre-existing social network and we examine that possibility below. Finally, in Table 3.8, we can see a significant impact of being mentored on the total number of businesses that the mentees have.

Given the possibility of spillovers, Table 3.10 examines the impact on treatment mentees of mentors who received the business training, comparing them to the nominated (but untreated) mentees of the control mentors. As in Table 3.7, we again find no evidence of a significant impact on profits and revenue.

Table 3.11 indicates a different story. Here we find significant impacts on almost all of our measured business practices. Point estimates suggest that treatment mentees do better in terms of some of the key skills identified by Mckenzie and Woodruff (2016) as well as Bloom and Van Reenen (2007) and Bruhn et al. (2010). Effects here are more modest in magnitude than those for the mentors, for example the increase in the marketing index is 7.5 percentage points relative to 10.3 percentage points for the mentors (see Table 3.5). These results indicate widespread spillover impacts among the control mentees of treated mentors. Finally, Table 3.12 shows no significant impacts on diversification.

Given the lag in practices leading to profits for the mentors, in appendix tables A7-A9 (for control and treatment mentees of treated mentors) and A10-A12 (for treated mentees compared with nominated mentees of control mentors) we examine the trajectory for mentees. Given the evidence of spillovers, we focus our attention on comparisons with the nominated mentees of control mentees. Table A11 shows a clear and constant, but small impact of mentoring on business practices. However, from table A10, we can see that this does not translate into any increase in profits (nor is there any evident time trend).

Taken together these results show that the treatment impacted the practices, but not profits of the mentees of the treated mentors.The evaluation of the business training program suggests that business practices clearly matter for firm profits, particularly in the longer term. The lack of effect of the mentorship program could possibly be attributed to the fact that it was unable to induce enough changes in the business practices of the mentees. This is consistent with Mckenzie and Woodruff's (2016) argument that the relationship between 'better' business practices and performance is causal and that the lack of effect for existing training programs is because they do not change business practices enough rather than because these practices do not matter.

3.6 Conclusion

The Women in Agribusiness Leaders Network (WALN) program is an interesting departure from the standard business training model. Under a stratified randomized control trial framework, experienced business women who were involved in agriculture-related businesses were given formal business training. The more innovative part in this evaluation design is the inclusion of a second-stage mentorship program, where a random selection of women mentees within the social and business network of mentors, received customized mentoring from the trained mentors. There are important implications for the evaluation of this project, particularly, in the context of transmission of business knowledge and best practices across social and business networks and the efficacy of informal mentorship programs.

We find that formal business training does not produce any significant impact on business performance in the short term but does improve the adoption of business practices taught to them in the training program. In the longer term, however, the training program induces a strong positive effect on different dimensions of business performance, such as reported and constructed profits as well as the number of business activities. Pooled together, the business training program appears to have a strong effect on business performance over the approximately 4-year post training period.

This effect appears to be driven by the continued adoption of several of the 'better' business practices identified by Mckenzie and Woodruff (2016). This result also confirms the criticism by McKenzie and Woodruff (2013) that many of the business training evaluations measure impacts only within a year of training, which may be one explanation for the prevalence of null results in the literature.

Given the extent of micro-entrepreneurial activities in developing countries, it is also important to identify low-cost programs that can lead towards the adoption of these business practices that can potentially improve performance for small businesses. Mentorship could be a relatively low-cost solution to this problem. We tested this here by connecting trainees of the above-mentioned formal business training program to a random subset of small firm owners within their social and business networks and evaluate the outcome of the treatment mentees compared to control mentees (who did not receive the mentorship). We find that the treatment mentees exhibit some effects in the short term, but only in the adoption of some business practices. It is possible that this adoption of practices is not of a large enough magnitude to change profits, or that these businesses are not yet of the size for these practices to matter for profits. These results are consistent with the lackluster results from other evaluations of business mentoring programs such as Brooks, Donovan & Johnson (2016).

3.7 Tables

Variable	(1) Control Mentor N	Mean/SE	(2) Treatment Mentor N	Mean/SE	T-test Difference (1)-(2)
Yearly Earnings (Raw)	75	1.01e+05	81	63304.988	37363.346
		(41659.524)		(12789.753)	
Last Month's Bus. Profit (Raw)	97	4615.093	95	2251.368	2363.724
		(1551.791)		(439.286)	
Last Month's Bus. Rev. (Raw)	96	72306.990	95	35869.453	36437.537
		(52534.630)		(16468.652)	
Last Month's Bus. Cost (Raw)	94	18535.532	82	53018.951	-3.45e+04
		(6710.129)		(35299.903)	
Total Num. of Businesses	97	1.536	97	1.701	-0.165
		(0.076)		(0.092)	
Owner's age	97	37.423	97	37.289	0.134
		(0.925)		(0.936)	
Operational years of business	95	6.726	92	7.522	-0.795
		(0.515)		(0.617)	
Owner's years of bus. experience	97	10.948	97	11.546	-0.598
		(0.801)		(0.809)	
Number of products	97	5.557	97	5.577	-0.021
		(0.849)		(0.910)	
Value of input	97	1.14e+05	97	2.90e+05	-1.76e+05
		(65873.525)		(1.44e+05)	
Has Bus. Plan	97	0.309	97	0.402	-0.093
		(0.047)		(0.050)	
Has Annual Budget	97	0.227	97	0.299	-0.072
		(0.043)		(0.047)	
Has Financial Record	97	0.309	97	0.309	0.000
		(0.047)		(0.047)	

Notes: The value displayed for t-tests are the differences in the means across the groups. Fixed effects using variable strata_second are included in all estimation regressions. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 3.1: Mentor Baseline Balance (comparing Treatment vs. Control Mentors)

Variable	(1) Control Mentee of Treatement Mentor N	Mean/SE	(2) Treatement Mentee N	Mean/SE	T-test Difference (1)-(2)
Yearly Earnings (Raw)	282	29000.589 (5268.778)	282	40253.972 (9271.947)	-1.13e+04
Last Month's Bus. Profit (Raw)	289	1914.599 (329.922)	291	2028.460 (694.106)	-113.862
Last Month's Bus. Rev. (Raw)	290	17283.166 (5044.704)	291	26813.186 (18566.880)	-9530.020
Last Month's Bus. Cost (Raw)	268	15016.578 (3059.217)	271	22961.118 (13404.519)	-7944.540
Total Num. of Businesses	293	1.375 (0.035)	294	1.395 (0.037)	-0.019
Owner's age	292	35.219 (0.604)	294	34.769 (0.556)	0.450
Operational years of business	267	6.719 (0.535)	271	7.018 (0.516)	-0.299
Owner's years of bus. experience	293	9.932 (0.634)	294	9.765 (0.541)	0.166
Number of products	293	6.526 (1.137)	294	5.017 (0.423)	1.509
Value of input	293	16089.863 (4018.034)	294	23104.320 (12096.397)	-7014.456
Has Bus. Plan	293	0.140 (0.020)	294	0.173 (0.022)	-0.034
Has Annual Budget	293	0.061 (0.014)	294	0.126 (0.019)	-0.064***
Has Financial Record	293	0.102 (0.018)	294	0.146 (0.021)	-0.044

Notes: The value displayed for t-tests are the differences in the means across the groups. Fixed effects using variable strata_second are included in all estimation regressions. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 3.2: Mentor Baseline Balance (comparing Treatment vs. Control Mentors)

Variable	(1) Pure control mentee N/[Clusters]	Mean/SE	(2) Treatment mentee N/[Clusters]	Mean/SE	T-test Difference (1)-(2)
Yearly Earnings (Raw)	531	29202.565	282	40253.972	-1.11e+04
	[90]	(6131.456)	[94]	(13352.843)	
Last Month's Bus. Profit (Raw)	531	1224.341	291	2028.460	-804.120
	[90]	(196.780)	[94]	(690.093)	
Last Month's Bus. Rev. (Raw)	531	15236.655	291	26813.186	-1.16e+04
	[90]	(6666.054)	[94]	(18539.125)	
Last Month's Bus. Cost (Raw)	495	13422.846	271	22961.118	-9538.272
	[90]	(6003.866)	[93]	(13355.289)	
Total Num. of Businesses	539	1.468	294	1.395	0.073
	[90]	(0.038)	[94]	(0.044)	
Owner's age	538	35.626	294	34.769	0.858
	[90]	(0.586)	[94]	(0.686)	
Operational years of business	493	7.473	271	7.018	0.454
	[90]	(0.713)	[93]	(0.497)	
Owner's years of bus. experience	539	10.477	294	9.765	0.712
	[90]	(0.706)	[94]	(0.553)	
Number of products	539	4.824	294	5.017	-0.193
	[90]	(0.510)	[94]	(0.551)	
Value of input	539	41920.510	294	23104.320	18816.190
	[90]	(33674.393)	[94]	(12070.335)	
Has Bus. Plan	539	0.113	294	0.173	-0.060
	[90]	(0.020)	[94]	(0.028)	
Has Annual Budget	539	0.089	294	0.126	-0.037
	[90]	(0.016)	[94]	(0.025)	
Has Financial Record	539	0.078	294	0.146	-0.068***
	[90]	(0.013)	[94]	(0.023)	

Notes: The value displayed for t-tests are the differences in the means across the groups. Standard errors are clustered at variable mentor_ID_bl. Fixed effects using variable strata_second are included in all estimation regressions. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 3.3: Mentor Baseline Balance (comparing Treatment vs. Control Mentors)

	(1) Profit & Rev. Index	(2) Profit (Raw)	(3) Profit (IHS)	(4) All Profit (Wins.)	(5) Revenue (Raw)	(6) Revenue (IHS)	(7) Revenue (Wins.)
Bus. Training	0.20***	6142.57*	0.63	2554.48*	29159.24***	0.67**	18376.58**
	(0.06)	(3527.56)	(0.48)	(1483.19)	(9920.45)	(0.31)	(8803.44)
Omitted category mean	-0.12	2431.94	6.12	2857.64	25187.69	8.23	21264.10
Number of firms	511	416	416	416	500	500	502
Number of distinct firms	189	154	154	154	187	187	188
Number of strata	18	18	18	18	18	18	18

Notes: The outcome variable is the mean of the standardized z-scores of various profits and revenue measures (raw, IHS and winsorized profits and revenue) for the last 30 days. ANCOVA specification. 3 rounds of follow-up data pooled. Strata Fixed effects. SEs clustered at firm level.

Table 3.4: Reduced Form Effects of Business Training (and being a Mentor) on Last Month's Profits and Revenue (comparing Treatment vs. Control Mentors)

	(1) Marketing Index	(2) Recordkeeping Index	(3) Stock Control Index	(4) Has a written Bus. Plan?	(5) Has a written Annual Budget?	(6) Keeps Fin. Rec. of each Transac.?	(7) Bus. Prac. I (Mean: 4,5 &
Bus. Training	0.103**	0.132***	0.069*	0.195***	0.144***	0.092**	0.143***
	(0.041)	(0.041)	(0.038)	(0.045)	(0.039)	(0.040)	(0.032)
Omitted category mean	0.432	0.388	0.648	0.336	0.205	0.238	0.260
Number of firms	323	323	323	506	506	506	506
Number of distinct firms	175	175	175	189	189	189	189
Number of strata	18	18	18	18	18	18	18

Notes: 3 rounds of follow-up data pooled. Strata Fixed effects. SEs clustered at firm level. Controlled for baseline index of business practices. The outcome varible in Col. (7) is the mean of Col. (4), (5) and (6).

Table 3.5: Reduced Form Effects of Business Training (and being a Mentor) on Bus. Practices (comparing Treatment vs. Control Mentors)

	(1) No. of New Businesses	(2) Total No. of Businesses
Treatment mentor	0.110	0.222**
	(0.069)	(0.103)
Omitted category mean	0.469	1.634
Number of firms	554	523
Number of distinct firms	192	191
Number of strata	18	18

Notes: ANCOVA specification for Col. 2. 3 rounds of follow-up data pooled. Strata Fixed effects. SEs clustered at firm level.

Table 3.6: Reduced Form Effects of Business Training (and being a Mentor) on No. of Businesses/IGAs (comparing Treatment vs. Control Mentors)

	(1) Profit & Rev. Index	(2) Profit (Raw)	(3) Profit (IHS)	(4) All Profit (Wins.)	(5) Revenue (Raw)	(6) Revenue (IHS)	(7) Revenue (Wins.)
Received mentorship	0.04	264.20	0.12	83.61	16082.43	0.18	-278.68
	(0.04)	(1566.63)	(0.22)	(297.71)	(12694.03)	(0.17)	(1821.24)
Omitted category mean	0.00	3057.82	6.07	2056.73	14005.96	7.93	11968.69
Number of firms	1,469	1,469	1,469	1,469	1,425	1,425	1,425
Number of distinct firms	553	553	553	553	545	545	545
Number of strata	18	18	18	18	18	18	18

Notes: The outcome variable is the mean of the standardized z-scores of various profits and revenue measures (raw, IHS and winsorized profits and revenue) for the last 30 days. 3 rounds of follow-up data pooled. Mentor Fixed effects. SEs clustered at firm level.

Table 3.7: Reduced Form Effects of Receiving Mentorship on Last Month's Profits and Revenue (comparing Treatment vs. Control Mentee - both of Treatment Mentor)

	(1) Marketing Index	(2) Recordkeeping Index	(3) Stock Control Index	(4) Has a written Bus. Plan?	(5) Has a written Annual Budget?	(6) Keeps Fin. Rec. of each Transac.?	(7) Bus. Prac. I (Mean: 4,5 &
Received mentorship	-0.004	0.009	0.020	0.004	0.019	0.018	0.014
	(0.018)	(0.021)	(0.019)	(0.021)	(0.017)	(0.017)	(0.015)
Omitted category mean	0.415	0.335	0.650	0.208	0.148	0.128	0.161
Number of firms	949	949	949	1,465	1,465	1,465	1,465
Number of distinct firms	517	517	517	553	553	553	553
Number of strata	18	18	18	18	18	18	18

Notes: 3 rounds of follow-up data pooled. Mentor Fixed effects. SEs clustered at firm level. Controlled for baseline index of business practices. The outcome varible in Col. (7) is the mean of Col. (4), (5) and (6).

Table 3.8: Reduced Form Effects of Receiving Mentorship on Bus. Practices (comparing Treatment vs. Control Mentee - both of Treatment Mentor)

	(1) No. of New Businesses	(2) Total No. of Businesses
Treatment mentee (Vs TMCM)	0.029	0.138***
	(0.032)	(0.045)
Omitted category mean	0.449	1.422
Number of firms	1,662	1,540
Number of distinct firms	576	566
Number of strata	18	18

Notes: 3 rounds of follow-up data pooled. Mentor Fixed effects. SEs clustered at firm level.

Table 3.9: Reduced Form Effects of Receiving Mentorship on No. of Businesses/IGAs (comparing Treatment vs. Control Mentee - both of Treatment Mentor)

	(1) Profit & Rev. Index	(2) Profit (Raw)	(3) Profit (IHS)	(4) All Profit (Wins.)	(5) Revenue (Raw)	(6) Revenue (IHS)	(7) Revenue (Wins.)
Received mentorship	0.08	-4578.18	0.26	425.71	-38905.15	0.58**	2436.08
	(0.05)	(5948.02)	(0.24)	(288.42)	(59570.81)	(0.24)	(1859.36)
Omitted category mean	-0.03	4872.94	5.99	1707.78	41750.97	7.60	8863.67
Number of firms	2,048	2,048	2,048	2,048	1,984	1,984	1,984
Number of distinct firms	777	777	777	777	758	758	758
Number of strata	18	18	18	18	18	18	18

Notes: The outcome variable is the mean of the standardized z-scores of various profits and revenue measures (raw, IHS and winsorized profits and revenue) for the last 30 days. rounds of follow-up data pooled. Strata Fixed effects. SEs clustered at mentor level.

Table 3.10: Reduced Form Effects of Receiving Mentorship on Last Month's Profits and Revenue (comparing Treatment Mentee (of T. Mentor) vs. Pure Control Mentee (of C. Mentor))

	(1) Marketing Index	(2) Recordkeeping Index	(3) Stock Control Index	(4) Has a written Bus. Plan?	(5) Has a written Annual Budget?	(6) Keeps Fin. Rec. of each Transac.?	(7) Bus. Prac. I (Mean: 4,5 &
Received mentorship	0.075***	0.062**	0.099***	0.068***	0.050**	0.030	0.049***
	(0.024)	(0.027)	(0.025)	(0.025)	(0.021)	(0.019)	(0.018)
Omitted category mean	0.340	0.283	0.582	0.141	0.114	0.117	0.124
Number of firms	1,331	1,331	1,331	2,052	2,052	2,052	2,052
Number of distinct firms	722	722	722	778	778	778	778
Number of strata	18	18	18	18	18	18	18

Notes: rounds of follow-up data pooled. Strata Fixed effects. SEs clustered at mentor level. Controlled for baseline index of business practices. The outcome varible in Col. (7) is the mean of Col. (4), (5) and (6).

Table 3.11: Reduced Form Effects of Receiving Mentorship on Bus. Practices (comparing Treatment Mentee (of T. Mentor) vs. Pure Control Mentee (of C. Mentor))

	(1) No. of New Businesses	(2) Total No. of Businesses
Treatment mentee (Vs Pure control mentees)	0.043 (0.034)	0.086 (0.062)
Omitted category mean	0.428	1.452
Number of firms	2,355	2,168
Number of distinct firms	818	802
Number of strata	18	18

Notes: rounds of follow-up data pooled. Strata Fixed effects. SEs clustered at mentor level.

Table 3.12: Reduced Form Effects of Receiving Mentorship on No. of Businesses/IGAs (comparing Treatment Mentee (of T. Mentor) vs. Pure Control Mentee (of C. Mentor))

3.8 Appendix

3.8.1 Survey Attrition Rates

	(1) Follow-up 1	(2) Follow-up 2	(3) Follow-up 3
Bus. Training	-0.00	0.03	0.05
	(0.02)	(0.03)	(0.04)
Omitted category mean	0.98	0.93	0.91
Number of firms	194	194	194
Number of distinct firms	194	194	194
Number of strata	18	18	18

Notes: Regressions for 3 rounds of follow-up data separately carried out. Strata Fixed effects. SEs not clustered.

Table A.1: Reduced Form Effects of Business Training (and being a Mentor) on Survey Response Rates - Trajectory (comparing Treatment vs. Control Mentors)

	(1) Follow-up 1	(2) Follow-up 2	(3) Follow-up 3
Received mentorship	0.02	0.02	0.02
	(0.02)	(0.02)	(0.02)
Omitted category mean	0.94	0.92	0.94
Number of firms	587	587	587
Number of distinct firms	587	587	587
Number of strata	18	18	18

Notes: Regressions for 3 rounds of follow-up data separately carried out. Mentor Fixed effects. SEs not clustered.

Table A.2: Reduced Form Effects of Receiving Mentorship on Survey Response Rates - Trajectory (comparing Treatment vs. Control Mentee - both of Treatment Mentor)

	(1) Follow-up 1	(2) Follow-up 2	(3) Follow-up 3
Received mentorship	0.02	0.01	0.03
	(0.02)	(0.02)	(0.02)
Omitted category mean	0.95	0.93	0.93
Number of firms	833	833	833
Number of distinct firms	833	833	833
Number of strata	18	18	18

Notes: Regressions for rounds of follow-up data separately carried out. Strata Fixed effects. SEs clustered at mentor level.

Table A.3: Reduced Form Effects of Receiving Mentorship on Survey Response Rates - Trajectory (comparing Treatment Mentee vs. Pure Control Mentee (of C. Mentor))

3.8.2 Outcomes of interest

The outcome variables discussed in this paper are defined in the following way:

- Profits (and revenue) index: This index averaged the z-scores of the following variables according to the procedure described above:
  - Raw last month's profits: We asked business owners for profits directly rather than attempting to have them match up revenue and expenses for the same period. Entrepreneurs were asked "what were the profits of your business in the last full month? That is, your revenue after having paid all expenses including the salaries of employees, but before paying your own salary"
  - Last month's profits, transformed using the inverse hyperbolic sine
  - Last month's profits, winsorized[3] at the 99th and 1st percentiles
  - Raw last month's revenue: Entrepreneurs were asked "what was the revenue of your business in the last full month?"
  - Last month's revenue, transformed using the inverse hyperbolic sine
  - Last month's revenue, winsorized at the 99th percentile

The outcome variables in Table 3.5 and 3.11 are different measures of business practices. For most of the business practices, the entrepreneur was asked over the past 6 months how often they used the practice, with four possible response options: never, about once per month, about once per week, every day. These practices were

[3]Winsorization was done separately for the mentors and mentees' distributions

first recoded to binary, with only those saying they never used the practice being coded to 0. The business practices are defined in the following way:

- Index for Marketing and customer service practices: This index averaged the z-scores of the following variables
  - Asks customers what products or services they would like to see
  - Asks clients if satisfied with their products or services
  - Offers promotions
  - Changes the presentation of products or services to make them more attractive
  - Used at least one form of publicity - constructed from a question asking about whether they used the following forms of advertising:
    - Written press
    - Radio or television
    - Classified ads through professional, trade or religious associations
    - Trade fair
    - Posters/flyers/business cards
  - Asks customers who do not come back why they did not return
- Record keeping and financial management practices index
  - Keeps accounting books
  - Keeps all types of accounting books, constructed from a question asking whether entrepreneurs keep a record book only for:

        - Purchases
        - Sales
        - Cash register operations
        - Inventory
    - Has a written budget
    - Has a budget that shows monthly expenses
    - Has a budget that shows yearly expenses
    - Gives receipts to customers systematically
    - Keeps receipts from suppliers
    - Has a business bank account
    - Pays self a fixed salary
- Stock Control index
    - Negotiated price with supplier in the last 3 months
    - Compared price with other supplier in the last 3 months
    - Don't run out of stock frequently
        - this is measured from the question on how often the business runs out of stock; any time interval less than 6 months is coded as 1 and the rest as 0
    - Don't take long to resupply
        - this is measured from the question on how long it takes to resupply; any time interval less than a week is coded as 1 and the rest as 0

  - Business has a record-keeping system which allows it to know how much stock it has on hand

- Because of their importance, the following business practices are reported separately:

  - Has a written business plan

  - Has a written annual budget

  - Keeps financial record for each transaction

The outcome variables in Tables **??** and 3.12 are defined in the following way:

- Number of businesses: total number of independent income-generating activities the business-owner has; this offers a measure of business diversification

- Total number of new businesses: this denotes the total number of new income-generating activities since the previous survey

3.8.3 Trajectories

	(1) Follow-up 1	(2) Follow-up 2	(3) Follow-up 3
Bus. Training	0.06	0.30***	0.25**
	(0.07)	(0.10)	(0.11)
Omitted category mean	-0.04	-0.18	-0.15
Number of firms	183	167	161
Number of distinct firms	183	167	161
Number of strata	18	18	18

Notes: ANCOVA specifications. Regressions for 3 rounds of follow-up data separately carried out. Strata Fixed effects. SEs not clustered.

Table A.4: Reduced Form Effects of Business Training (and being a Mentor) on Last Month's Profit and Revenue Index - Trajectory (comparing Treatment vs. Control Mentors)

	(1) Follow-up 1	(2) Follow-up 2	(3) Follow-up 3
Bus. Training	0.21***	0.06	0.17***
	(0.05)	(0.05)	(0.06)
Omitted category mean	0.25	0.30	0.23
Number of firms	183	162	161
Number of distinct firms	183	162	161
Number of strata	18	18	18

Notes: ANCOVA Specifications. The outcome variable is the mean of the variables: "Has a written annual budget?", "Keeps financial record of each transaction" and "Has a written business plan?". Regressions for 3 rounds of follow-up data separately carried out. Strata Fixed effects. SEs not clustered.

Table A.5: Reduced Form Effects of Business Training (and being a Mentor) on Bus. Practices Index - Trajectory (comparing Treatment vs. Control Mentors)

	(1) (Follow-up 1)	(2) (Follow-up 2)	(3) (Follow-up 3)
Bus. Training	0.03	0.34*	0.30*
	(0.05)	(0.18)	(0.18)
Omitted category mean	1.05	2.09	1.87
Number of firms	190	168	165
Number of distinct firms	190	168	165
Number of strata	18	18	18

Notes: ANCOVA specifications. Regressions for 3 rounds of follow-up data separately carried out. Strata Fixed effects. SEs not clustered.

Table A.6: Reduced Form Effects of Business Training (and being a Mentor) on Total No. of Businesses - Trajectory (comparing Treatment vs. Control Mentors)

	(1) Follow-up 1	(2) Follow-up 2	(3) Follow-up 3
Received mentorship	0.08	0.09*	-0.00
	(0.05)	(0.05)	(0.07)
Omitted category mean	-0.01	-0.04	0.07
Number of firms	515	496	458
Number of distinct firms	515	496	458
Number of strata	18	18	18

Notes: Regressions for 3 rounds of follow-up data separately carried out. Mentor Fixed effects. SEs not clustered.

Table A.7: Reduced Form Effects of Receiving Mentorship on Last Month's Profit and Revenue Index - Trajectory (comparing Treatment vs. Control Mentee - both of Treatment Mentor)

	(1) Follow-up 1	(2) Follow-up 2	(3) Follow-up 3
Received mentorship	0.05**	0.01	0.01
	(0.02)	(0.02)	(0.02)
Omitted category mean	0.15	0.15	0.18
Number of firms	516	467	482
Number of distinct firms	516	467	482
Number of strata	18	18	18

Notes: The outcome variable is the mean of the variables: "Has a written annual budget?", "Keeps financial record of each transaction" and "Has a written business plan?". Regressions for 3 rounds of follow-up data separately carried out. Mentor Fixed effects. SEs not clustered.

Table A.8: Reduced Form Effects of Receiving Mentorship on Bus. Practices Index - Trajectory (comparing Treatment vs. Control Mentee - both of Treatment Mentor)

	(1) (Follow-up 1)	(2) (Follow-up 2)	(3) (Follow-up 3)
Received mentorship	0.03	0.18**	0.22***
	(0.03)	(0.08)	(0.08)
Omitted category mean	0.99	1.72	1.61
Number of firms	560	496	484
Number of distinct firms	560	496	484
Number of strata	18	18	18

Notes: Regressions for 3 rounds of follow-up data separately carried out. Mentor Fixed effects. SEs not clustered.

Table A.9: Reduced Form Effects of Receiving Mentorship on Total No. of Businesses - Trajectory (comparing Treatment vs. Control Mentee - both of Treatment Mentor)

	(1) Follow-up 1	(2) Follow-up 2	(3) Follow-up 3
Received mentorship	0.09	0.05	0.11
	(0.07)	(0.06)	(0.06)
Omitted category mean	-0.04	-0.01	-0.05
Number of firms	719	700	629
Number of distinct firms	719	700	629
Number of strata	18	18	18

Notes: Regressions for rounds of follow-up data separately carried out. Strata Fixed effects. SEs clustered at mentor level.

Table A.10: Reduced Form Effects of Receiving Mentorship on Last Month's Profit and Revenue Index - Trajectory (comparing Treatment Mentee vs. Pure Control Mentee (of C. Mentor))

	(1) Follow-up 1	(2) Follow-up 2	(3) Follow-up 3
Received mentorship	0.05**	0.09***	0.05*
	(0.03)	(0.03)	(0.03)
Omitted category mean	0.14	0.10	0.13
Number of firms	721	661	670
Number of distinct firms	721	661	670
Number of strata	18	18	18

Notes: The outcome variable is the mean of the variables: "Has a written annual budget?", "Keeps financial record of each transaction" and "Has a written business plan?". Regressions for rounds of follow-up data separately carried out. Strata Fixed effects. SEs clustered at mentor level.

Table A.11: Reduced Form Effects of Receiving Mentorship on Bus. Practices Index - Trajectory (comparing Treatment Mentee vs. Pure Control Mentee (of C. Mentor))

	(1) (Follow-up 1)	(2) (Follow-up 2)	(3) (Follow-up 3)
Received mentorship	0.05	0.10	0.12
	(0.03)	(0.10)	(0.09)
Omitted category mean	0.97	1.78	1.69
Number of firms	796	700	672
Number of distinct firms	796	700	672
Number of strata	18	18	18

Notes: Regressions for rounds of follow-up data separately carried out. Strata Fixed effects. SEs clustered at mentor level.

Table A.12: Reduced Form Effects of Receiving Mentorship on Total No. of Businesses - Trajectory (comparing Treatment Mentee vs. Pure Control Mentee (of C. Mentor))

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