Improving Shock-Coping with Precautionary Savings: Effects of Mobile Banking on Transactional Sex in Kenya*

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Abstract

For the most vulnerable, even small negative shocks can have significant short- and long-term impacts. Few interventions that improve shock-coping are widely available in sub-Saharan Africa. We test whether individual precautionary savings can mitigate a shock-coping behavior with potentially negative spillovers: transactional sex. Sex for money is a common shock-coping behavior in sub-Saharan Africa and is believed to be a leading driver of the HIV/AIDS epidemic. In a field experiment in Kenya, we randomly assigned half of 600+ participating, vulnerable women to a savings intervention that consists of opening a mobile banking savings account labeled for emergency expenses and individual goals. We find that the intervention led to an increase in total mobile savings, reductions in transactional sex as a risk-coping response to shocks, and a decrease in symptoms of sexually transmitted infections.

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1 Introduction

Throughout the developing world, the poor often find it difficult to cope with the financial ups and downs presented by everyday life. These contexts often feature incomplete credit markets and low saturation of other financial services such as savings and insurance. As such, the experience of a negative shock can exert both short and long term consequences. Households may sell off productive assets or withdraw children from school.[1] Families may be forced to reduce food consumption and/or forego health care, as evidenced by changes in child nutrition and health.[2] These reactions can harm the longer term development of human capital, and ultimately economic growth (Maccini and Yang, 2009; Alderman, Hoddinott and Kinsey, 2006; Dercon, 2004).

Households' responses to negative shocks, and the resulting impacts on their welfare have been studied extensively. Many of these studies have examined major shocks that are devastating but relatively infrequent, such as droughts, floods, or other adverse weather events.[3] Similarly, many studies have relied on reports of shocks from recall periods ranging from one to five years, even up to sixteen years, ensuring that only the largest household shocks are reported.[4] It is not clear what implications such studies have for understanding how households cope with the higher-frequency, smaller shocks of everyday life.

Life's most frequent shocks are less studied because they are harder to measure. The simplest and most common survey method for measuring shocks is asking a household to recall major unexpected events over the past year or few years. Collecting an accurate accounting of smaller shocks, such as moderate illness, requires high-frequency data collection. In this study we make use of a unique data set that relies on daily diaries and weekly interviews focused specifi-

[1]On selling assets, see Rosenzweig and Wolpin (1993); Udry (1995); Fafchamps, Udry and Czukas (1998); Kinsey, Burger and Gunning (1998). On school enrollment and attendance, see Jacoby and Skoufias (1997); Jensen (2000); Ferreira and Schady (2009).

[2]For impacts on child nutrition and health see Jensen (2000); Ferreira and Schady (2009); Rabassa, Skoufias and Jacoby (2014).

[3]For example, Udry (1995); Jacoby and Skoufias (1997, 1998); Fafchamps, Udry and Czukas (1998); Jensen (2000); Dercon (2004); Alderman, Hoddinott and Kinsey (2006); Ferreira and Schady (2009); Maccini and Yang (2009); Rabassa, Skoufias and Jacoby (2014); Gröger and Zylberberg (2016)

[4]For example, Kochar (1995); Carter and Maluccio (2003); Wagstaff and Lindelöw (2005); Heltberg and Lund (2009); Sun and Yao (2010); Khan, Bedi and Sparrow (2015); Dhanaraj (2016); Mitra et al. (2016)

cally on the experience of shocks and household's reactions to them.[5] We examine how households cope with a common financial burden – the illness and treatment of a child in the absence of health insurance.[6]

A variety of interventions have been tested for improving household's ability to cope with negative shocks. Certainly, insurance is the best suited intervention, though evidence on its feasibility in a poor-country context has been mixed.[7] In practice, crop, health and life insurance remain rare in poor countries. The role of credit as a coping mechanism has been explored qualitatively (see Schindler, 2010; Bylander, 2015) and tested as well, though only by relying on non-random variation in credit access (see Doocy et al., 2005; DeLoach and Smith-Lin, 2017). Similar to insurance, credit markets remain thin in many developing contexts. Increasingly common in these contexts, cash transfers may also hold promise for improving shock coping (see de Janvry et al. 2006 for the effects of conditional transfers and Goudge et al. 2009 for the effects of unconditional transfers). This study is among the first to experimentally test individual precautionary savings as a method to improve *ex-post* shock-coping.[8]

We conduct a randomized field experiment among more than 600 vulnerable women in Kenya to assess the impacts of savings on shock coping. Our intervention relies on a common mobile money platform, M-PESA, and provides existing M-PESA users with a new, second M-PESA account that is earmarked for personal, private savings. Recipients were encouraged to use the labeled account to save for personal savings goals and to accrue a buffer stock for use in the event of an emergency. The funds in the labeled account could be withdrawn at any time without penalty, and thus the intervention is effectively a soft commitment device for increasing savings.

[5] Other studies relying on data of a similar nature include Robinson and Yeh (2011, 2012).

[6] To check robustness, we also test another common and comparably sized financial burden (the payment of school fees) and find results that are broadly consistent. These are presented in Appendix C.

[7] See Dekker and Wilms (2010); Hamid, Roberts and Mosley (2011); Giesbert, Steiner and Bendig (2011); Landmann and Frolich (2015); De Janvry, Ramirez Ritchie and Sadoulet (2016); Liu (2016)

[8] The potential for savings to improve shock coping has been recognized theoretically for decades Deaton (1991). Savings may have a detrimental impact on *ex-ante* risk management by breaking down informal risk sharing arrangements Ligon, Thomas and Worrall (2000). Tests for the impact of savings on risk-sharing have mixed results (Kast and Pomeranz, 2014; Comola and Prina, 2015; Flory, 2011; Chandrasekhar, Kinnan and Larreguy, Forthcoming; Dupas, Keats and Robinson, 2017; Dizon, Gong and Jones, forthcoming).

The shock-coping behavior of focus in this study is transactional sex. This includes not only commercial sex workers but also more informal sex work, such as "bar girls," and, most commonly, the receipt of obligatory money or gifts from boyfriends, which is very common in East and Southern Africa.[9] In a context of incomplete insurance, women may cope with shocks by adding a sexual partner or engaging in riskier sexual behavior in order to increase the value of the transfers they receive from partners.[10] This is a costly shock-coping mechanism as it may involve risky sexual acts, and it typically involves multiple concurrent partnerships, which are a leading cause of the HIV/AIDS epidemic.[11] Engaging in transactional sex also carries other risks, such as unwanted pregnancy, exposure to gender-based violence, and challenges to mental health. Our sample consists both of self-identified sex workers and other vulnerable women, including widows, divorced or separated women, and single mothers. We document that not only commercial sex workers, but also other women in our sample, increase their transactional sex behavior in the week following a negative shock.

We test the impact of our intervention on savings balances and on the sexual behavior response to shocks. The literature on increasing savings among the poor has repeatedly documented the difficulties of measuring savings, due to privacy and misreporting. We address this issue by relying on high-quality administrative data for both existing and new accounts to measure savings balances among all study participants. To examine changes in the sexual behavior response to shocks we collect high-frequency data on negative shocks and sexual behavior over a three-month time frame, pairing each respondent with a single enumerator to build rapport and increase accurate reporting. Using a combination of weekly surveys for the entire sample and daily diaries for the sex workers, we are able to track both the timing of negative shocks and transactional sex over our study period. Finally, we collect additional data 8 months after the intervention to test for medium-term

[9] Much evidence exists that material support is a significant motivation for women's sexual activity in East and Southern Africa (Wojcicki, 2002*b*,*a*; Masanjala, 2007; Swidler and Watkins, 2007; Wamoyi et al., 2010; Verheijen, 2011), and that women rely on these transactional relationships as a form of insurance (Verheijen, 2011; Robinson and Yeh, 2011, 2012). Various types of transactional sex are characterized by Baird and Özler (2016).

[10] It is often the case that sexual risk is increasing in the size of the transfer (Rao et al., 2003; Gertler, Shah and Bertozzi, 2005; Luke, 2006). See LoPiccalo, Robinson and Yeh (2012) for a review of the evidence documenting income shocks and transactional sex.

[11] See Halperin and Epstein (2004); LeClerc-Madlala (2009); Mishra and Bignami-Van Assche (2009)

impacts.

We have two main findings. The first is that the intervention led to an increase in total M-PESA savings across the existing and new accounts. It is important to note that M-PESA savings is highly liquid and the increase we document may serve as a buffer for negative shocks. Our second finding is that the intervention lead to reductions in the use of transactional sex as a shock-coping behavior. When employing the weekly data and estimating within-woman, we find that sex workers increase their number of regular clients in the week following a negative shock, but that this response is entirely mitigated for those in the savings intervention. For the women who do not identify as sex workers, we find that the treatment reduces their sexual behavior generally. Specifically among those who do report engaging in transactional sex, we also find that treatment reduces their use of transactional sex as a response to shocks. When we estimate across all women who ever experienced a shock in our study period, we find that treatment reduced the probability of ever reporting symptoms of a sexually transmitted infection.

This work contributes to the substantial literature on the benefits of increasing savings for the poor.[12] Many of the previous studies focused on whether providing access to formal bank accounts increases savings and affects downstream outcomes such as income and investments.[13] An implicit motivation behind these studies is that individuals are constrained from saving more because of self-control and other-control problems.[14] Bank accounts can relax these constraints by serving as a commitment device for individual savings, as it may be costly to access funds held at banks due to travel and wait times, and a narrow window of operating hours. A chief concern however is that in exchange for this commitment, bank accounts may limit liquidity in times of negative shocks, thereby mitigating a key benefit of savings.[15] Our study examines the effects of mobile savings (M-PESA)

[12]See Karlan, Ratan and Zinman (2014); Prina (2015); Dupas et al. (2018) for comprehensive reviews.

[13]Studies will commonly encourage the opening of formal bank savings accounts by varying opening account fees, minimum balances, or interest rates (Dupas and Robinson, 2013; Prina, 2015; Schaner, Forthcoming; Karlan et al., 2016).

[14]Ashraf, Karlan and Yin (2006); Dupas and Robinson (2013); Karlan and Linden (2014); Brune et al. (2016) are studies that explicitly examine self-control and other control constraints.

[15]In Western Kenya, high withdrawal fees, unreliable availability, and lack of trust are the main barriers to bank account usage (Dupas et al., 2016). Simlarly, Dupas et al. (2018) document that liquidity concerns are also a leading reason for low take-up rates in Uganda. This may explain why providing lockboxes to Kenyan households successfully resulted in increased savings for health in

which has greater liquidity than bank savings. We provide suggestive evidence that a softer-commitment savings product can still increase savings.

Further, our findings are among the first to document how increasing savings affects households' resiliancy to shocks. In a related paper, Prina (2015) finds that Nepali households with access to free bank accounts are less likely to experience income drops when hit with a health shock. However, this finding is not about shock-coping behaviors *per se*. The author attributes this result to the improved ability of households with savings to accrue a "health capital" stock ex-ante (e.g. by consuming more meat and fish), which reduces the impact of illness on their ability to continue earning. The most closely related paper is that of Kast and Pomeranz (2014), who examine the impact of free bank accounts on reported responses to shocks in Chile. They find that major income shocks such as job loss or business downturns result in consumption cutbacks, and that the cutbacks are 44% smaller for households that received the free accounts. In contrast to these works, rather than examining major shocks, as meaured over a one or three month period, we undertake detailed measurement and analysis of coping mechanisms for smaller, more frequent shocks. Further, we go beyond income and consumption to examine other shock-coping behaviors that have significant implications for welfare and potential negative externalities. We believe our study offers new knowledge on the value that savings has as a precautionary measure.

This study also speaks to whether households can fully insure themselves against health shocks. Existing studies offer conflicting answers to this question (Gertler and Gruber, 2002; Genoni, 2012). However, a common thread is that the most vulnerable households are least likely to exhibit full insurance (Khan, Bedi and Sparrow, 2015; Mitra et al., 2016). These findings motivate the focus of this study on a sample of vulnerable women.

Finally, this work contributes to the literature documenting the use of transactional sex as a shock-coping mechanism in sub-Saharan Africa. In Kenya, which is the location of this study, about 20% of sexual partnerships are formed for the purpose of financial assistance (Luke et al., 2011). In Malawi, women have been documented to take on multiple sexual partnerships in response to income insecurity (Swidler and Watkins, 2007), while women in South Africa, Tanzania, and Western Kenya have been shown to respond to income shocks by increasing their

an experiment by Dupas and Robinsona (2013).

level of risky unprotected sex (Dinkelman, Lam and Leibbrandt, 2008; Robinson and Yeh, 2011; De Walque, Dow and Gong, 2014). This literature also offers evidence that the use of sex as shock coping is a harmful risk management strategy. Income shocks (in the form of droughts in rural areas) can explain 20% of the cross-country variation in HIV rates across Africa, a relationship for which transactional sex is the most plausible pathway (Burke, Gong and Jones, 2015).

These findings suggest that interventions targeting financial uncertainty may affect sexual behavior. Studies involving the provision of cash transfers have documented reductions in sexual activity (Kohler and Thornton, 2012) and sexually transmitted infections (HIV, HSV-2) (Baird et al., 2012). The implied mechanism is that the added liquidity of cash transfers allows women to cope with negative shocks without relying on transactional sex.

Our study advances these literatures by documenting how women can use increased savings as a safety net, allowing them to substitute away from transactional sex towards relying on their own savings to respond to negative shocks. Our findings suggest that women are better able to self-insure if given a way to accumulate precautionary savings.

In the next section of the paper, we discuss details of the sample, data collection, and the field experiment (Section 2). We then document how the intervention increased M-PESA savings (Section 3) and decreased transactional sex (Section 4). In Section 5, we examine medium-term sustainability. We discuss the implications of our findings in Section 5 and draw conclusions in Section 6.

2 Study Design

2.1 Financial Services in Kenya and M-PESA

M-PESA, operated by the leading mobile service provider in Kenya, Safaricom, is a highly successful private enterprise which provides clients with branchless banking accessed via mobile phone. Any individual with a national ID card and Safaricom SIM card can set up an M-PESA account, allowing her to make deposits, withdrawals and transfers using her mobile handset. M-PESA points for exchanging cash are ubiquitous; they are located at nearly every shop and one can be found open at nearly any time of day. The district in which this study is set has fewer than

3 formal financial institutions per 100,000 population (Kenyan average across all districts is 5.3). In contrast, the region has 38 mobile network vendors per 100,000 population.[16]

A key requirement in our study is that all study participants must have an existing M-PESA account. At the time of our experiment, over 70% of households in Kenya had adopted M-PESA due to its convenience and relatively low cost (Suri, Jack and Stoker, 2012). We found that this requirement only eliminated 12% of women who were otherwise eligible.

2.2 Sample

Our sample consists of 627 vulnerable women in both urban and rural areas in Kisumu County on the western edge of Kenya. The urban sample consisted of female sex workers (FSWs) and the rural sample consisted of widows, separated or divorced women, or never-married female heads-of-household.[17] As noted above, women in the rural sample are deemed to be at high risk for entering sex work.

The study involved two local partners that are geographically based. Our urban partner is an NGO that provides health and counseling services to FSWs in Kisumu. The NGO's operations include operating walk-in centers distributed throughout Kisumu where FSWs can access its services. Each center is staffed by peer educators who help coordinate services to FSWs. Our rural partner is a community based organization that targets vulnerable women (i.e. widows, divorced/separated women) and provides economic assistance programs. Both partners are well respected in their local communities.

Sampling activities were conducted during December 2013 and January 2014. In our urban sample, a sampling team attended scheduled meetings of peer educators, to census the FSWs who they support. Each FSW was visited individually for enrollment. For our rural sample, the sampling team visited each of the villages in the study, seeking eligible women by talking with local leaders and snowballing. The enrollment visits consisted of checking eligibility, taking verbal consent, collecting contact information, and registering existing M-PESA account details.

[16]Formal institutions are defined as Banks, Micro-finance institutions, Mortgage finance institutions, and PostaBanks; excludes cooperatives.

[17]Women in the rural sample are considered vulnerable because they lacked financial support from a male partner (i.e. husband or boyfriend).

2.3 Intervention

The unit of randomization is the individual. We first identified geographic clusters: 12 sub-locations or politically defined geographic units in the rural sub-sample, and 15 "hotspots" or specific areas within the urban sub-sample where the FSWs meet clients.[18] We stratified treatment randomization by sub-sample, geographic cluster, and age. Within each cluster, each individual was assigned into treatment or control.

The control group participated in group discussions on the importance of savings that lasted about one hour. Individuals in the Treatment 1 arm (T1) received the same group discussions as the control arm, plus a one-on-one activity eliciting savings goals, weekly SMS reminders on the savings goals, and a free M-PESA account with zero transaction costs that we define as the "Labeled M-PESA account." Individuals in the Treatment 2 arm (T2) received everything in the T1 arm, plus a 5% monthly interest rate on their labeled savings account for the first 12 weeks of the study. We are unable to reject the null that T1 and T2 have the same effect on savings outcomes, and thus we pool the T1 and T2 arms in our analysis.

Women in the treatment arm chose savings goals and were told to use the labeled M-PESA account to save for their goals. We also asked each woman in the treatment arm to think about the unexpected expenses that they face and to set aside a specific amount each week for emergencies and deposit this into the labeled M-PESA account. Women were strongly encouraged to only withdraw money from their labeled M-PESA account in the event of an emergency or when they reached their savings goal. There were no other restrictions on the labeled M-PESA account, and we thus see this account as a soft commitment device for savings.[19]

2.4 Data Collection

Figure 1 presents a timeline of the study. A baseline survey was conducted prior to the intervention in January of 2014 that collected information on demographics, in-

[18]There are 47 counties in Kenya. Kisumu county has 7 sub-counties, and our sample falls into the Nyando and Kisumu Central sub-counties.

[19]Mshwari, a savings product offered by M-PESA, was launched in 2013 just before our study. Very few women in out study had adopted Mshwari, possibly due to regressive interest rates and minimum balance requirements.

come sources, savings, and sexual behavior history. Following the random assignment to treatment and setting up the labeled M-PESA accounts, weekly surveys were conducted for twelve consecutive weeks (March through May) where high-frequency data was collected on expenses, health, and sexual behavior. In the urban sample, women were trained to keep daily diaries where they recorded basic information on each commercial sex client, including price and condom use; these were collected and checked weekly. Top-up training was provided as needed. In both samples, women reported on sexual behavior during the past week in each weekly interview (for partners not already included in the diaries).

We returned in October to collect medium-term follow up data, including retrospective reports of shocks and shock coping over the previous 4 months. We also have M-PESA administrative data for all individuals in our study. The administrative data consists of balances on all of the existing M-PESA accounts for the control and treatment arms as well as balances on the labeled M-PESA account for those in the treatment group.

2.5 Summary statistics and balance

Table 1 provides baseline summary statistics for the control group in the urban and rural samples (Columns 1 and 3). Treatment differences are reported in columns 2 & 4. Women in the rural sample tend to be younger (mean age 27) and have larger households (4.13 individuals) compared to the urban sample. A majority of the women in the rural sample are widowed (58%) compared to just 21% in the urban sample. While there are stark differences in primary income sources between the urban and rural samples (sex work dominating the urban sample, while shop keeping and agriculture are the main sources in the rural sample), food insecurity remains high in both groups. Baseline characteristics are generally balanced across treatment and control groups, with marital status in the urban sample the only unbalanced characteristic across the 15 tested, separately for rural and urban (and only at the 10% level of significance).

As we noted above, M-PESA savings is based on administrative data while all other forms of savings rely on self-reported responses, which we acknowledge are noisy measures. One concern about self-reported savings is that respondents may be reluctant to report their true savings because of concerns about theft. The rather low rates of self-reported savings at home (33% in the urban sample and 26% in the rural sample) suggest that this may be the case. We also note that while there are no statistically significant treatment/control differences in any of the savings measures, the differences are large in magnitude. Both of these concerns will motivate our use of individual-level fixed effects to estimate changes in savings within-woman.

Finally, it comes as little surprise that the urban sample is much more sexually active. Everyone in the urban sample has been sexually active over the past year and on average sees 4.1 regular clients. While in the rural sample, 58% have been sexually active over the past year, and of those who are sexually active, the mean number of partners is close to 1.

Table 2 presents a summary of the experience of negative shocks and of sexual behaviors as reported during our observation period. The first column indicates the level of observation for the sample employed for each row, as our data include multiple weeks per individual, and potentially multiple sex acts per week. The negative shocks considered in this analysis are illnesses or injuries of any other person in the respondent's household in a given week; we believe. Alternative shock measures are discussed in Appendix C. Given that 80% of the sample are heads of households, in the majority of cases these "other's illness" shocks are illnesses of the respondents own dependents. We exclude illnesses of the respondent herself because these may affect sexual behavior in non-financial ways, leading to reverse causality. In any given week, dependent illness is reported by 20% of the urban sample and 32% of the rural sample. There is no significant difference in the rate of dependent illness in the treatment group, suggesting that our intervention did not change the likelihood of our measure of negative shocks. Considering only weeks where illness occurred, the median amount spent on treatment is 350 KSh, or about one day's income in the urban sample, and 200 KSh, or about 2 days' income in the rural sample.

Turning to sexual behavior, there are several key differences between the rural and urban samples. In the control arm, only 39% of the rural sample report any sexual behavior during the 12-week period of observation, vs. 95% of the urban sample. When we examine the data at the week level, we observe sexual activity in 81% of urban woman-weeks, but only 10.5% of rural woman-weeks (control arm). It appears that women in the both the urban and rural treatment arms are less likely to have a week with any sexual activity, with a treatment difference of about 3 ppt, though this is significant only for the rural sample. Conditional on having sex, we do not observe significantly different levels of risk across treatment and control groups. Consistent with earlier evidence (Gertler, Shah and Bertozzi, 2005; Arunachalam and Shah, 2013; Shah, 2013), we do find that riskier sex acts command higher transfers, as shown in figure 2. In the same figure we also see that regular clients pay higher rates than casual clients, holding constant sex act type.[20] This is consistent with respondents' own reports that they view their regular clients as high risk interactions.

2.6 Attrition

We note that attrition is fairly minimal due to the high frequency with which we contacted respondents. Of the 627 respondents 96% were interviewed on at least 10 of the 12 weeks of data collection (82% have all 12 weeks). Of the 28 respondents with fewer than 10 weeks of data, most (20) are in the urban sample. However, assignment to treatment is not a determinant of the number of weeks of data available for an individual, nor for whether she has above or below 10 weeks of data, or whether she has all 12 weeks of data, regardless of examination by full or rural/urban subsamples (see Appendix Table B.1).

However, data is less complete for the daily diaries kept by the urban sample. We have complete diary data for 83.5% of weeks when an urban woman was interviewed. Keeping the diary of client interactions was encouraged and respondents were trained and re-trained as needed, nonetheless, only 72% of the urban sample kept the diary for at least 10 of the 12 weeks. There are 10 women in the urban sample who refused the diary exercise completely, effectively reducing the urban sample for analysis to 301 women.[21] Fortunately, it does not seem that missing diary data is correlated with treatment assignment. As shown in Appendix Table B.1, neither the weeks of complete diary data, nor the share of interview weeks with diary data vary significantly by treatment. Missing diary data seems to be driven by enumerators: 4 out of 14 enumerators working with the urban sample account

[20] Regular clients differ from casual clients in that they have a longer-term relationship with the respondent. Robinson and Yeh (2012) document that regular clients are viewed in the same way as boyfriends and that some women go on to marry their regular clients.

[21] Three of these women completed two or fewer of the planned 12 interviews.

for 67% of missing diaries. Of the seven urban respondents who refused the diary exercise completely but completed the bulk of the interviews, five were handled by a single enumerator. We note that variation in enumerator survey sucess will be controlled by the individual fixed effects, as each respondent was assigned to a single enumerator for the course of the study. Another concern might be that shocks themselves lead to changes in attrition that may drive our results. We estimate 3 using missing diary data as the outcome and find that exposure to a shock does not predict missing data for a given week, neither for treatment nor control groups (see Appendix Table B.2).

Our follow-up rate at endline, 4.5 months after the end of the weekly observation period, was 92.3% (579 women). Attrition from endline is not significantly correlated with treatment status in the full sample, nor in either sub-sample (also shown in Appendix Table B.1).

3 Effects on Savings

Before estimating the impact of the intervention on savings, we first describe the adoption of the labeled M-PESA account using the administrative data. Figure 3 presents adoption of the labeled M-PESA account (left panel) and corresponding account balances (right panel) over the 12 week period of the study. It appears that adoption was not instantaneous. Using either a single deposit or two deposits as a measure of usage, we find that cumulative adoption grew at a steady rate over time. By the end of the 12 week period, 57% (45%) of the those assigned to the treatment made at least one deposit (two deposits) into the labeled M-PESA account. A similar pattern is found with average balances in the labeled M-PESA account. Starting from a zero balance at the beginning, average balances grew to 271 KSh by week 4, and almost doubled to 493 by week 12.[22]

Do these balances represent an actual increase in savings? A natural response is that individuals in the treatment group simply moved savings from their existing M-PESA account to the labeled one. To account for this, we aggregate balances in both existing and labeled M-PESA accounts and estimate the following:

[22] These patterns are similar when we look at the rural or urban samples - see Appendix Figure A.1

$$MPESA_{it} = \alpha_i + \eta_1 Post_t + \eta_2(T_i \times Post_t) + \lambda_t + \varepsilon_{it} \quad (1)$$

where $MPESA_{it}$ is total M-PESA savings for individual i in week t, α_i is an individual fixed effect, T_i indicates whether individual i was assigned to treatment, $Post_t$ is the period after the intervention, and λ_t are week fixed effects. The coefficient of interest is η_2 (the effect of the intervention in the post period). Treatment stratification based on both geographic clusters and age is subsumed in the individual fixed effects. If treatment simply induced a substitution from the existing M-PESA account to the labeled account, we would expect estimates of η_2 to be close to zero.

As we noted earlier, adoption and balances grew over time, and we thus vary the post period in our analysis. When estimating equation 1, we begin by defining the post period as weeks 1-12 (with the baseline period being the "pre" period). We then systematically vary the beginning of the post period. For example, after our initial specification described above, we begin the post period at week 2; the post-period thus includes weeks 2-12 and week 1 is removed from the analysis. We proceed with this type of analysis until the post period consists of just week 12.

Figure 4 and Table (3) present the effects of the intervention on total M-PESA savings. We first turn our attention to Figure 4 where the x-axis defines when the post period begins and estimates of η_2 are plotted with 95% confidence intervals included in the figure. Similar to the patterns we saw with the labeled M-PESA account, we see that the treatment effect on overall M-PESA savings gradually increases over time. Estimates where the post period begins at week 5 or later are significant at the 10% level and become significant at the 5% level when the post period begins at week 8 or later. In Table (3), we present estimates for the full sample, as well as estimates split by the urban and rural samples. The estimates for the sub-samples show a similar trajectory but are less precise given the smaller sample sizes (see Appendix Figure A.2 for the plot of the estimates by rural and urban samples).

What about the effects of the intervention on other types of savings? We do not find any significant evidence of crowd out with either home savings or bank savings (Appendix Figure A.3), suggesting that the increase in M-PESA savings reflects an increase in overall savings. In fact, when when we aggregate different types of savings, we find that both liquid savings (M-PESA savings + home savings) and total savings (liquid savings + bank savings), demonstrate a similar upward trajectory of the treatment effect over time (Appendix Figure A.4). Overall, we find evidence suggesting that the savings intervention lead to an increase in M-PESA savings that is highly liquid and can be accessed during emergencies. In addition, the average increase in M-PESA savings (+416 KsH using Week 8 estimates) is probably sufficient to cover a majority of unexpected medical expenses (~350 KsH for the median health expense).

4 Effects on Transactional Sex

4.1 Woman level

This study's central analysis is whether higher precautionary savings can change the use of sexual behavior as a response to negative shocks. We begin this analysis by estimating impacts of shocks and the treatment at the woman level (aggregating the weekly data for each woman), and then proceed to the woman-by-week level (individual panel data). One reason for this inital approach is that the rural sample has a low frequency of sexual activity. Only 0.73% of rural woman-weeks record more than one sex act, and only 0.46% record more than one transactional sex act. Thus, weekly measures of both sex and transactional sex are essentially binary in nature. Yet even these simple binary outcomes offer little variation in the sample. The share of observations at zero are 91% and 94%, for any sexual acts and any transactional sex acts, respectively.

For our women-level analysis, we use the weekly data and generate a woman-level indicator for the use of sex as a response to shocks. For each occurrence of a dependent illness, we record whether or not the respondent engages in transactional sex in the following week. The woman-level indicator, C_{ic} is a continuous measure of the share of observed shocks that are followed by transactional sex ("Rate of sex coping"). Since these indicators are employed for both rural and urban, we pool the samples and include an interaction to estimate separate effects. We estimate the impact of treatment assignment, T_{ic}, on these behaviors, employing

$$C_{ic} = \alpha_0 + \beta_1 T_{ic} + \beta_2 T_{ic} \times Urb_{ic} + \beta_3 X_{ic} + \lambda_c + \varepsilon_{ic} \quad (2)$$

where the treatment effect for the rural sample is given by β_2 and for the urban sample it is $\beta_2 + \beta_3$; X_{ic} is the stratifying variable of age and λ_c are geographic cluster-fixed effects. Results are presented in Table 4.[23]

For the rural sample, we find that treatment leads to significant reductions in the rate of transactional sex as shock coping (col 1, row 1). On average, rural women in the control group use sex to cope with 8.5% of shocks; for those assigned to treatment this is reduced by 4.9 pp. The estimated treatment impact in the urban sample is 3.0 pp, which is not significantly different from zero. However, we cannot reject that the treatment effect is the same across the two samples. Finally, we use data on whether the respondent reported any STI symptom at any point during the observation period. The prevalence of STIs is remarkably similar across the two populations, at around 9%. This likely reflects the higher condom usage among the sex workers, which offsets the risk of their higher levels of transactional sex. We find that those assigned to treatment are about 5 pp less likley to report STI symptoms in both the rural and urban samples (col 2). This effect is siginificant, both statistically and economically, indicating that the sexual behavior offset by the treatment is among the riskiest sex in which women are engaging.

4.2 Woman x Week level

We now turn to the woman-by-week level analysis. A major advantage of this study is that our data are high frequency in nature and this improves our ability to detect sexual behaviors that occur as a response to a shock. Specifically, we focus on sexual behavior in the week following a shock and estimate the following:

$$B_{it} = \alpha_i + \beta_1 Shock_{it} + \beta_2 T_i \times Shock_{it} + \lambda_t + \varepsilon_{it} \quad (3)$$

where B_{it} is the sexual behavior of woman i in week t, α_i is an individual fixed effect, $Shock_{it}$, is an indicator that a dependent in individual's i household experienced any illness or injury in the week prior to t, T_i is the treatment indicator, and β_2 measures the differential in shock response between individuals assigned to the treatment rather than control arm. Given the use of woman-fixed effects, any estimate of impact derives from within-woman differences in the outcome across

[23] See table notes for explanations on variations in sample sizes.

time. Given that the rural and urban samples are very different with regards to sexual behavior, we use different outcomes for each sample, and therefore estimate impacts for these subsamples separately. For example, while an indicator for any sexual activity over the past week is relevant for the rural sample, it contains very little information for the urban sample, where virtually everyone is sexually active. Standard errors are clustered at the individual level.

We begin with an analysis of the urban sample. The weekly outcomes we use are: total number of partners, number of regular clients and number of causal clients.[24] We also acknowledge that sexual risk includes not only the number of partners, but also the types of sexual behaviors. To assess this risk, we construct a measure of weekly risk, conditional on having any sexual activity in a given week. This is a continuous risk index, generated by a principle components analysis of the numbers of regular and casual clients, the mean amount paid per act,[25] the number of risky acts (i.e. anal), and the number of unprotected acts.We take the first eigenvector as the index, which explains 38% of the variance.

Estimates of equation 3 for the urban sample are presented in Table 5.[26] Across the top row we note that the occurrence of a dependent health shock last week significantly increases a woman's number of sexual partners (col 1). However, this increase is driven by an increase in acts with regular clients and not an increase in casual clients (cols 2 and 3). This is consistent with findings by Robinson and Yeh (2012) that commercial sex workers rely on regular clients as a form of insurance. In the second row, we see the treatment impact on the use of sexual behavior as a coping strategy is decreased but the difference is not precisely estimated (col 1). However, the treatment does significantly reduce reliance on regular clients as a response to shocks (col 2). The mean number of regular client interactions in a week is 2.46; a shock increases this by 0.47, a response that is fully offset by a treatment effect of -0.50 (significant at the 5% level). While 0.5 clients per week represents about an 8% decrease on the mean number of total partners (6.48), we note that interactions with regular clients represent significant risk; one's own regular clients are estimated to be at "high risk of HIV" by 46% of sex workers.

Taking into account other factors, such as condom usage and higher-risk acts,

[24] We note that for these count variables, we also estimated negative binomial models, rather than ordinary least squares, and the results are consistent.

[25] Higher risk acts are typically better compensated, as described in section 2.5.

[26] See table notes for explanations on variations in samples sizes.

we also find that risk on the intensive margin is used as a coping strategy. Conditional on having any sexual activity, the occurrence of a health shock the previous week increases sexual risk taking by 0.14 SD and increases the chance of having higher than median risk by 7.8 percentage points (cols 4 and 5). This response is fully offset by assignment to the savings treatment, and these treatment effects are significant at the 10% and 5% levels, respectively. We note that the estimation of this type of intensive margin effect dictates the use of a subsample that may be, in part, determined by the treatment itself. However, it seems unlikely that those who reduce their sexual behavior as a result of the treatment (and as a result have fewer weeks included in this estimation) are having more risky sexual acts on average than those who do not. Barring this unlikely scenario, this estimate will not be upward biased by the sample selection.

We now turn to the rural sample. Outcomes in this analysis include a binary indicator of any sexual activity in the week following a shock, a binary indicator of any transactional sex in the week following a shock, and an analogous index for risk taken within sexual partnerships. We note that more than 2/3 of the rural sample never report any transactional sex during our 12-week observation period. This significantly reduces our power to detect changes in overall levels of transactional sex in this sample. We therefore also estimate impacts of the treatment on transactional sex among the subsample of rural women who ever report this behavior during observation. As above, we note that this subsample of observations could be determined, in part, by exposure to treatment. However, we believe the direction of bias would suggest that our estimate is a lower bound.

Table 6 presents the results from estimating equation 3 for the rural sample.[27] Across the top row we see that a dependent health shock last week increases the probability of being sexually active, though not significantly (col 1), but that increases in transactional sex are statistically different from zero. In column 2, a dependent illness increases the probability of engaging in transactional sex by 3.3 percentage points, a very large increase given that it occurs in only 6.7% of women-weeks in the sample. We find that assignment to treatment reduces the effect of shocks on transactional sex by an estimated 2.8 percentage points, nearly significant at the 10% level (p-value = 0.114). Given that only 26% of rural women ever report any transactional sex during the study period, we restrict the focus to

[27] See table notes for explanations regarding variations in samples sizes.

these women in col 3.

Among women engaging in transactional sex, the experience of a shock increases the probability of paid sex by 12.4 percentage points, on a base of 24%. Assignment to treatment offsets the impact of a shock by 10.5 percentage points, significant at the 10% level (col 3). Finally, we construct a risk index for the rural sample, employing an indicator of whether the sex was paid, and whether a condom was used. We test whether shocks and treatment affect sexual risk taking, conditional on having any sexual act (col 4). In the rural sample, 65% of reported sex acts are paid and 34% are unprotected; the mean risk index in the control group is 3.2 with a standard deviation of 2.6. The occurrence of a shock increases the risk index by 0.54 standard deviations, an effect that is more than offset by treatment, and both effects are significant at the 1% level. This suggests that in addition to a reduction in sexually active weeks, treated women also reduce the risk taken in the weeks when they are sexually active.

In order to ensure that our findings are not specific to any unique nature of illness shocks, we test whether our findings are robust to experiences of other shock types. Various shock types are considered and discussed in Appendix C. The only viable alternative measure for an alternative "shock," or periodic financial burden is the payment of school fees. Estimating equation 3 employing school fees

he results are broadly consistent with the main findings, though less preferred as school fees are not truly a shock in the unexpected sense. These results are presented and discussed in Appendix C.

5 Sustainability of Impacts

In this section we examine whether the changes in savings and shock coping observed among the treatment group over the 12-week observation period are sustained in the medium-term, relying on follow up data collected 4.5 months after the end of weekly observations.

We estimate the following:

$$MPESA_{it} = \alpha_i + \theta_1(T_i \times Week12_t) + \theta_2(T_i \times Week35_t) + \lambda_t + \varepsilon_{it} \tag{4}$$

where t= 0 (baseline), 1 (week 12), or 2 (endline at week 35). The equation includes woman- and week-fixed effects (α_i and λ_t). The coefficient θ_1 will indicate whether savings changes from baseline to week 12 are different between treatment and control groups. Similarly, θ_2 will indicate whether savings changes from baseline to week 35 are different between treatment and control groups. Results are presented in Table 7. Similar to what we previously reported in Figure 4, we find an increase in savings at week 12 ranging from 680 KSh for the rural sample to 398 KSh for the urban sample. By week 35, we still observe positive treatment differences in savings, however, these treatment effects are less precise and the mangtiude of the effect has diminished. Using either week 12 or week 35 estimates, we find that the point estimates suggest large percentage increases in savings when compared to control group means.

At week 35 we also asked respondents to report on whether their household had experienced any shocks of various types over the previous 4 months. The shocks about which we inquired included: illness, death, birth, job loss, theft, damage to property, legal issues, conflict, crop loss, or livestock illness or death. We asked whether the shock presented a financial burden, and what types of actions were taken in order to deal with the shock. Respondents reported the number of times each shock type was experienced, and the responses to each occurrence. Response types included: relying on savings (48%), taking loans (30%), reducing expenses (28%), receiving assistance (24%), engaging in transactional sex (4%), otherwise trying to increase earnings (11%), selling assets (3%), or praying (2%).

Before examining treatment impacts on shock response, we first examine whether exposure to treatment affects the probability of reporting a shock. We find that treatment is uncorrelated with reporting all types of shocks (see Appendix Table B.3).

We test whether assignment to treatment changes the probability that a woman exposed to a shock reports transactional sex as a response in months 4-8 after the start of the intervention. We estimate

$$S_{ic} = \alpha + \rho_1 T_{ic} + \rho_2 T_{ic} \times RUR_c + X_{ic} + \lambda_c + \varepsilon_{ic} \tag{5}$$

for the sample of women who report at least one shock during the period, where T_{ic}, X_{ic},λ_c, and ε_{ic} are as defined above. 48% of the sample reported at least one shock during this period. To maximize power, we pool the urban and rural samples and interact the treatment indicator with RUR_c, indicating the rural sample. The outcome S_{ic} indicates ever reporting transactional sex as a response to any shock during the period. For completeness we also show estimations employing the other most common shock coping behaviors: relying on savings, taking loans, reducing consumption, and receiving assistance.

The results are presented in Table 8. Among the urban sample exposed to shocks, assignment to treatment reduced the probability of relying on sex for shock coping by 11.4 pp, almost completely eliminating the 13.4% rate observed in the control group. In tandem, it increased the probability of relying on savings by 37.6 pp, more than doubling the 34.3% rate among the control group. It nearly halved the rate of reducing consumption in response to a shock, reducing it by 13.8 pp on a base of 34.3%. The coefficient on loan taking is also negative, though imprecisely estimated. We see no reduction in the reliance on gifts from friends and family. In contrast, we find no statistically significant results among the rural sample. In particular, no women in either the treatment or control group ever report relying on transactional sex for shock coping, so the estimated treatment effect is zero.

In order to better examine the sustainability of impacts of sexual behavior in the rural area, we additionally examine whether the women has reported any sexual activity at all during the 4 month recall period. As before, we expect treatment impacts to be limited to women that experience a shock during this period. We estimate the impact of treatment on sexual activity for the rural sample, interacting treatment with an indicator of shock exposure. That is,

$$A_{ic} = \alpha + \zeta_1 T_{ic} + \zeta_2 T_{ic} \times NoShock_{ic} + X_{ic} + \lambda_c + \varepsilon_{ic} \tag{6}$$

where A_{ic} indicates any sexual activity in the period. As before, X_{ic} includes woman's age at baseline, however we also present specifications where this includes other woman-level controls including marital status, primary school completion, and baseline reporting of transactional sex. We present equation 6 for the full rural sample, and for the subsample for whom effects were identified in Table 6: women who ever report transactional sex during the observation period.

The results are presented in Table 9. As in Table 6, we do not find statistically significant impacts for the full rural sample (though the p-value for col 2 is 0.14). However, for the sample of women who ever report engaging in transactional sex, we find that when faced with a shock, assignment to treatment reduces the probability of sexual behavior over 4 months by approximately 30 pp (on a base of 61%).

6 Discussion

In this work we have explored the use of savings for improving the ability of vulnerable women to cope with shocks. We find that even small, frequent shocks, such as the illness of a child every 3-4 weeks induces potentially harmful shock-coping behaviors, such as increased sexual risk. Not only among urban commercial sex workers, but also among widows and other female heads of household in a rural area, a high proportion of sexual acts are transactional in nature. The occurrence of an unexpected financial shock significantly increases the number of regular clients seen by sex workers as well as the riskiness of sexual acts, and increases the probability that rural women engage in transactional sex.

Given the frequency of shocks and the potential long-term health costs of transactional sex as a risk-coping behavior, we sought to reduce this behavioral response by improving access to precautionary savings. We provided beneficiaries with a new, labeled mobile banking account for saving. The intervention appears to have increased total mobile savings by 200-400 KSh over the course of 12 weeks, increasing women's access to highly liquid funds that can be easily accessed in case of an emergency. During this time, the intervention reduced overall sexual behavior in the rural sample. It also reduced the use of transactional sex as a shock coping behavior among urban sex workers, and among the 1/3 of the rural sample who were engaging at all in transactional sex. Among women who experienced shocks, those assigned to the intervention also exhibited fewer STI symptoms during this period.

Eight months afterward, we find that the savings intervention lead to positive, though imprecisely estimated, increases in M-PESA savings. At that time, we also find that sex workers exposed to the treatment are more likely to report relying on savings to cope with shocks and less likely to report relying on transactional sex. In addition, for the portion of the sample for whom earlier impacts were established, unmarried rural women exposed to treatment are also less likely to be sexually active 8 months later.

We discuss here several caveats to these findings. First, we note that while these savings amounts are comparable to the median cost of treating a dependent illness (200-350 KSh), in the 50% of cases above the median, the treatment cost would be greater than these amounts (means are 892 KSh and 410 KSh, for urban and rural respectively). This suggests that in some cases, this amount of savings may not fully cover a financial shock. Longer-term interventions that are able to raise personal savings more substantially may have the potential to exhibit greater impacts on sexual behavior than those reported here. Similarly, we note that this intervention was provided to women who were already users of this mobile money technology. In that sense it was more a nudge towards mental accounting than true provision of access to savings technology. We do not speculate on the potential magnitude of impacts if this intervention were provided to those with no previous account access, however one might imagine that impacts would also be greater in such a case.

Second, the shocks that we examine are the occurrence of a dependent illness in the previous week. We note that these shocks are very common, occurring every 3-4 weeks on average. However, they are far from the only shocks experienced by these households. We examine these shocks due to their idiosyncratic nature, affecting different households at different times, their frequency, allowing multiple observations per household during our 12-week observation period, and their cost, as discussed above. Ideally, we would also examine the impacts of other types of financial shocks in this work. There are several reasons why we do not do so.

We are not able to examine the impact of a respondent's own ill health shocks, as these may impact her sexual behavior in non-economic ways. Another common financial shock is death – not within one's household, as that is rather uncommon, but the financial cost of contributing to and participating in the funeral of a friend or family member. However, funeral participation in this context almost always requires individuals to travel away from their area of residence. This also affects sexual activity in non-economic ways, as one is far away from regular and/or potential clients or other partners. Additionally, we would ideally examine the impact of income shocks, as we know income to be highly variable in these vulnerable populations. However, given the difficulty in accurately measuring income in this context, and the difficulty in separating expected from unexpected dips in income, the reliable identification of income shocks is not feasible.

Further, we note that this experiment was designed to use high-frequency data over a short period of time to identify the impacts of small, frequent, idiosyncratic shocks. However, large, sustained, aggregate shocks can also induce the use of transactional sex as a shock-coping behavior. As documented in Burke, Gong and Jones (2015), aggregate income shocks, such as those induced by droughts in rural areas of Africa, significantly increase HIV prevalence, and transactional sex is one likely pathway of this effect. In this study, we focus on the impact of small, idiosyncratic shocks as a complement to the existing evidence on the impact of large, aggregate shocks.

Finally, we discuss the feasibility of this intervention for scaling up to prevent transactional sex as a shock coping mechanism. The cost per participant for the full intervention was about USD$7.5 in the urban area and about USD$3.8 in the rural area. However, most of this cost was in the training, including the preparation of trainers, their salaries, identification and hire of venues, time and/or airtime required to invite participants, and overhead of the managing organization. These costs could be reduced as much as $1 per participant due to economies of scale when rolling out the intervention to a larger number of participants. Only USD$1.5 of the total per-person total cost paid for the SIM card for the mobile money account, and the time of the M-PESA agent to be present at the training for account set up.

Estimations suggest that these costs prevented engagement with a regular client about once per every two shocks, or about one out of every 52 sexual acts among the urban sample. Among rural women who engage in transactional sex, the treatment prevented one partnership for every ten shocks experienced, or about one out of every seven or eight partners.[28] Given the findings that these impacts are sustained up to 8 months after the intervention, the cost/benefit ratio is at most $1.9 per partnership prevented in the urban area, or $3.8 in the rural area.[29] If benefits continue beyond 8 months, these numbers would continue to fall.

It is difficult to compare the effectiveness of this intervention to others that might

[28]The urban sample experiences dependent illness shocks about once per month. They have about 6.5 encounters per month, or 52 encounters over two months. The rural sample experiences dependent illness shocks about once every three weeks. The women who engage in transactional sex do so about once per month, or 7.5 times in 30 weeks.

[29]Partnerships prevented are 1 per 2 months in the urban sample and 1 per 7.5 months in the rural sample.

aim to reduce transactional sex or risky sex. The economic motivations for transactional sex make any attempts to "talk women out of it" likely to be ineffective. Other attempts to reduce the economic motivations might include efforts to reduce the market price by reducing demand, which is also likely to be ineffective, or direct cash transfers to women, which are far more expensive. The most comparable interventions might be those aimed at increasing women's safety in transactional sex by promoting condom usage or providing training in negotiating safer sex. However, we find an existing high rate of condom usage among the urban sample (91%), offering little room for improvement. The greatest potential impact of these would involve outreach in the rural areas where condom usage is much lower (66%), though it is not within the scope of this work to estimate the cost per partnership protected from this type of outreach.[30] Further, increasing the safety of transactional sex through condom use does nothing to reduce the other harmful impacts on mental health and exposure to gender-based violence.

7 Conclusions

From this work we draw several conclusions. First, while earlier works have examined the impacts of droughts, floods, and other major shocks, we find that the negative impacts of shock-coping are not restricted to dealing with these large, infrequent shocks. For the vulnerable women in our sample, even small shocks can lead to harmful shock-coping behaviors, such as transactional sex. Second, the use of transactional sex as a method of coping with financial shocks is not restricted to commercial sex workers but is observed among widows and single women in a rural population as well. This is consistent with findings from sociology and anthropology that document the typically transactional nature of many sexual relationships throughout East and Southern Africa. Though this study focused on a small sample in a specific context, the widespread nature of this dynamic suggests significant external validity of this trial. Third, using sex to cope with shocks implies large negative externalities, as transactional sex increases health risks for sexu-

[30] Population Service International reports that the cost per condom sold in sub-Saharan Africa is less than US$0.12 (Feldblum, Welsh and Steiner, 2003). However, this figure includes high demand populations, and high-population density areas. Achieving take-up in rural areas by a nearly invisible target population would be vastly more expensive.

ally active individuals in their communities: rural women in our sample who ever engage in paid sex have an STI symptom incidence of 15.6% over three months, versus 2.6% for those who never engage in paid sex. While we only measure self-reported symptoms of curable STIs, these findings likely have similar implications for HIV infections as well.

Our main conclusion is that the promotion of individual savings has the potential to improve the ability of the most vulnerable to cope with shocks. The intervention studied here had only modest impacts on savings over a fairly short observation period and was deemed to be fairly cost-effective. Nonetheless, we find that among women who report participating in transactional sex (both commercial sex workers and others), treated women reduced the use of transactional sex as a shock-coping mechanism. We also find suggestive evidence from the rural sample that a similar effect may have taken place among those who do not report participation in transactional sex, as treated women have lower sexual activity overall. These changes appear to add up to significantly lower sexual risk, as both populations experience a large reduction in STI prevalence in response to the savings intervention.

We find that the impacts of this intervention are modestly sustained over the medium-term (8 months). Additionally, recall data at that point offer supporing evidence for our hypothesized pathway. Urban women in the treatment group are significantly more likley to report relying on savings in the event of a shock (and less likley to report relying on sex or reductions in consumption). This supports the theory that precautionary savings act as a personal safety-net, providing needed liquidity when faced with unexpected costs, and enabling households to avoid other, less desireable ways to meet these expenses.

Larger-scale interventions, sustained over longer periods of time could potentially increase the degree to which savings affect shock-coping behaviors. In particular, longer-term savings could allow households to build the type of buffer stock needed to cope with larger, infrequent shocks as well. Increasing the availability and accessibility of savings products for the poor, and encouraging saving behavior, may offer benefits not only in terms of investment, but also in terms of risk. Widespread roll-out of such programs may have the potential to reduce not only transactional sex, but other potentially detrimental shock-coping behaviors as well. We leave the study of larger programs and other shock coping behaviors as the subject of future work.

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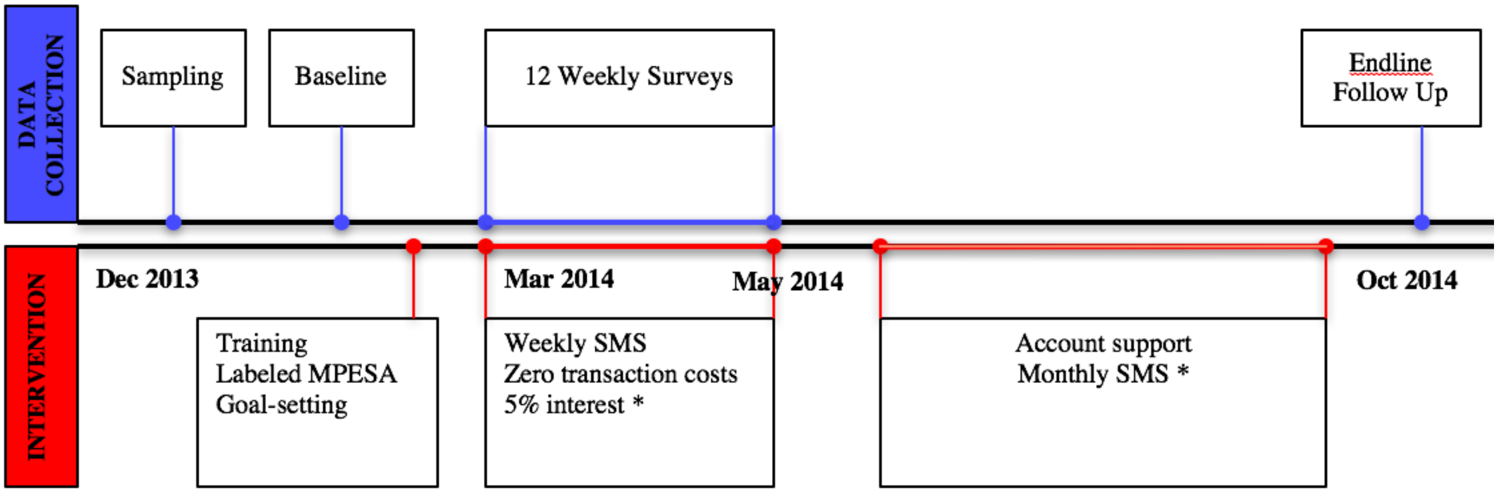
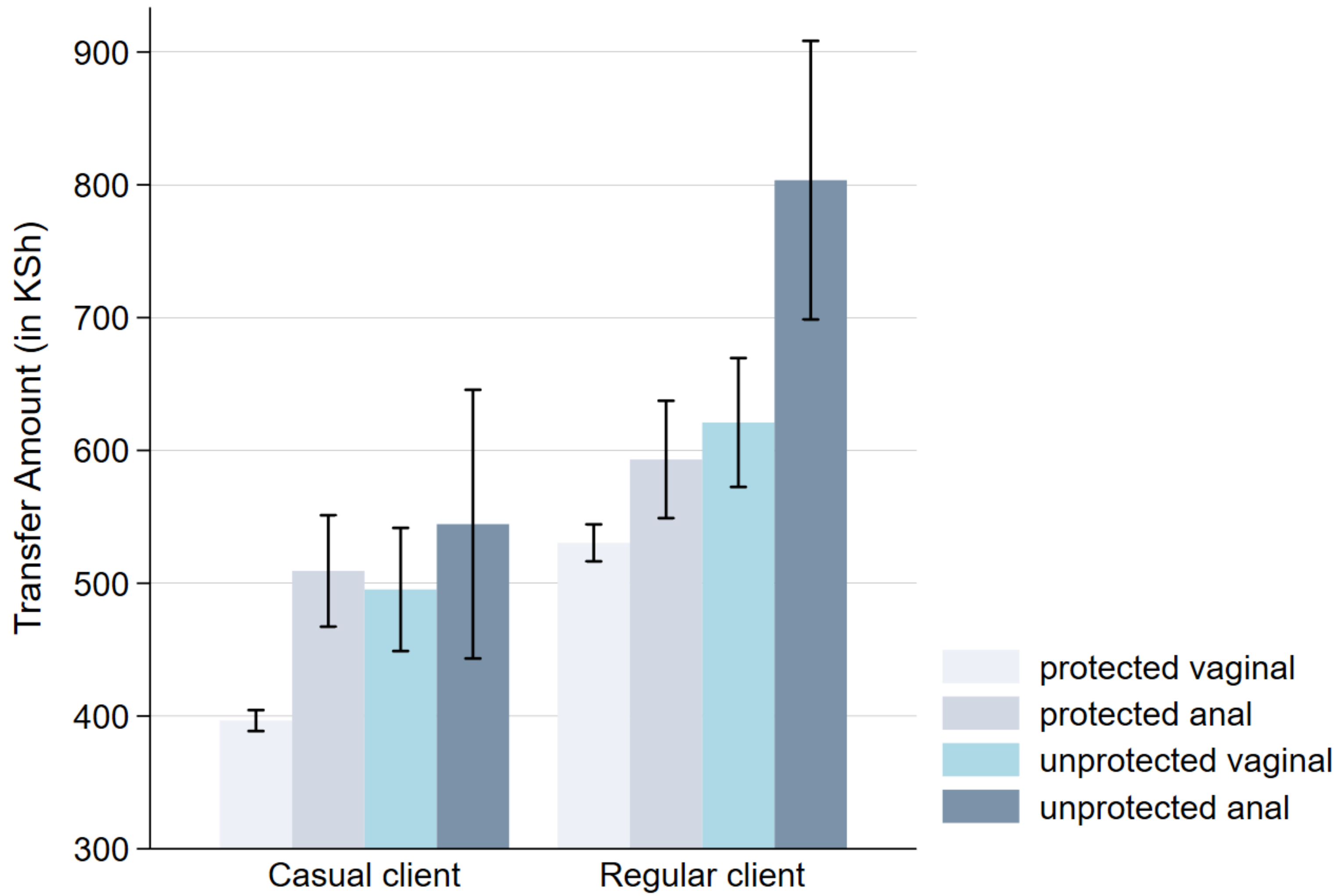
Figures

Figure 1: Study Timeline



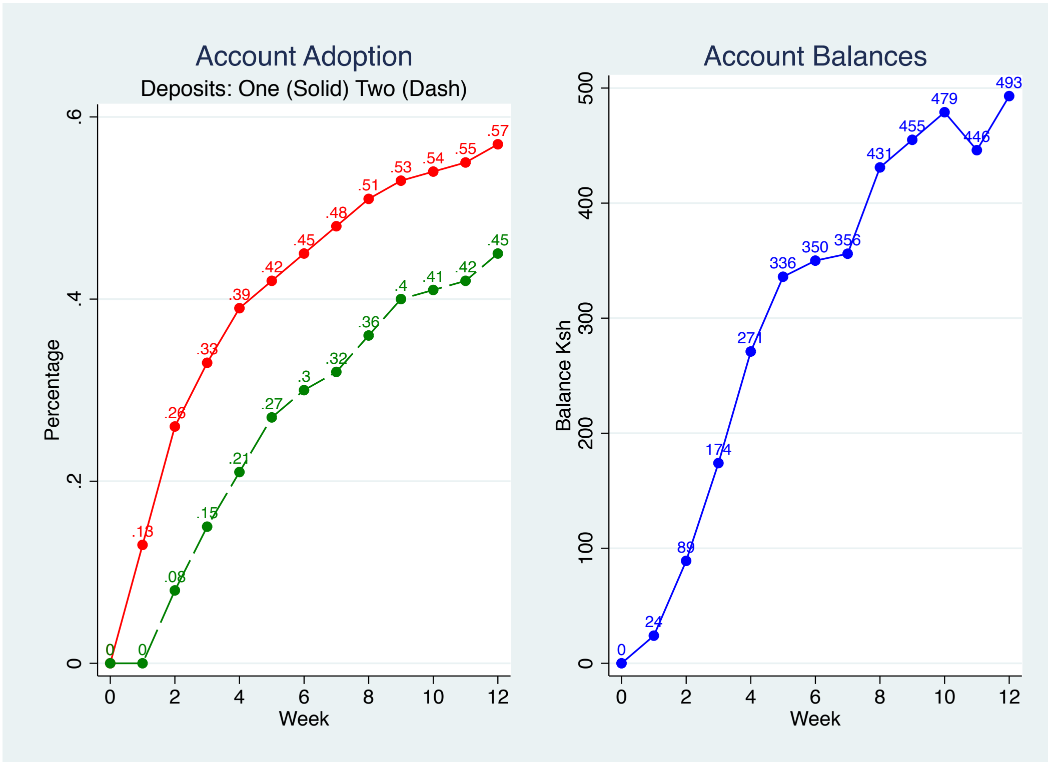
** Note: intervention arms provided to only a subsample of treatment group*

Figure 2: Price of transactional sex increases with risk taken



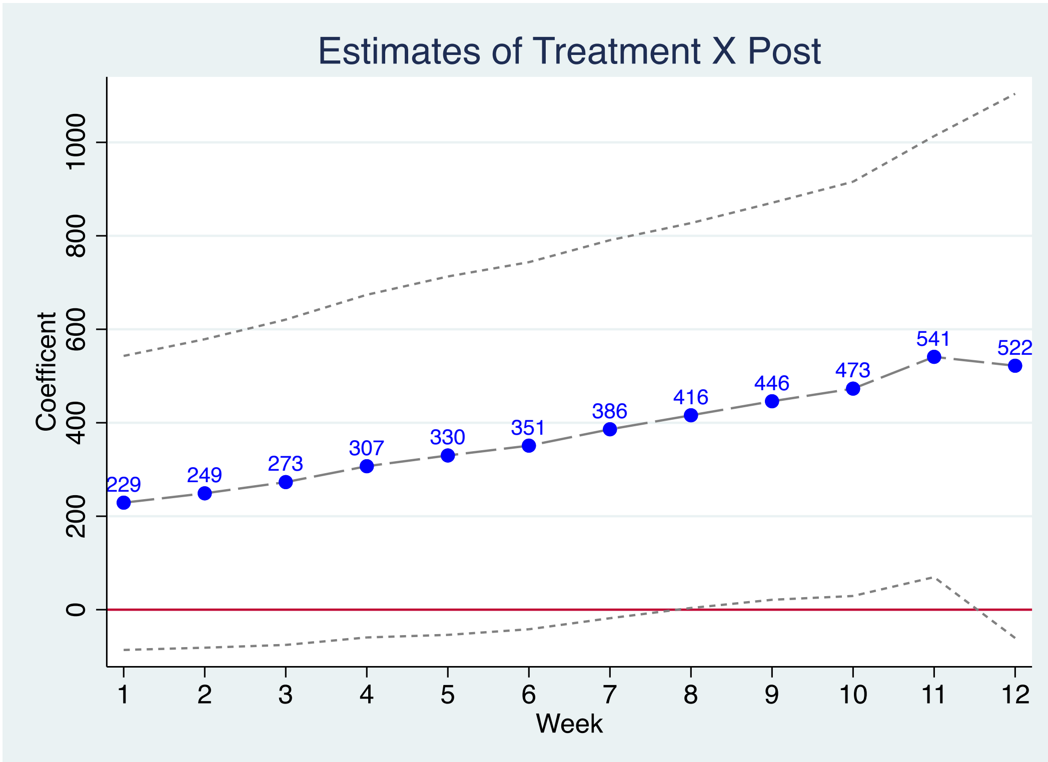
Note: Based on data from 16,738 commercial sex transactions as reported in daily diaries over the course of 12 weeks by 302 sex workers in urban Kisumu. Black bars show 95% confidence intervals.

Figure 3: Labeled M-PESA Account Adoption and Balances



The figure on the left, “Account Adoption”, reports the percentage of women assigned to the treatment group who had ever made a deposit by a given week (labeled in the x-axis) into the labeled M-PESA account. The solid red (dashed green) line shows the percentage of the treatment group that made at least one (two) deposit(s) by a given week. By week 12, 57% (45%) of women assigned to the treatment group made at least one (two) deposits into the labeled M-PESA account. The figure on the right, “Account Balances”, plots the average balance of all labeled M-PESA accounts in the treatment group in a given week. Labeled M-PESA accounts that have not been adopted (no deposits have been made into them) are coded as having a zero balance. Amounts in the labeled M-PESA accounts are winsorized at the 99th percentile.

Figure 4: Effect of Intervention on M-PESA Savings



The data used for the analysis presented above consists of weekly individual level panel data where total M-PESA savings is measured each week using administrative data. The plots represent estimates of η_2 from equation 1, indicating the treatment effect on the difference in savings balances between baseline and the post period. The coefficients plotted above are estimates of η_2 from the OLS estimation of equation 1 that includes individual and week fixed effects. The pre-period is defined as the baseline period (before the treatment intervention occurred).The x-axis indicates the start of the post period. For example, Week 1 represents a post period that consists of weeks 1-12; Week 2 represents a post period that consists of weeks 2-12. The y-axis is the size of the coefficient. The dotted line indicates the 95% confidence interval.

Tables

Table 1: Baseline Sample Characteristics & Balance

	Urban				Rural			
	Control Mean	Treatment Difference	Std. Error	N	Control Mean	Treatment Difference	Std. Error	N
Demographics								
Age	30.4	0	[0.000]	310	26.968	0	[0.000]	315
Household size	2.96	-0.271	[0.210]	310	4.167	0.076	[0.222]	315
Single	0.38	-0.005	[0.051]	310	0.167	-0.023	[0.038]	315
Widowed	0.21	-0.054	[0.038]	310	0.577	-0.054	[0.050]	315
Divorced	0.33	0.096*	[0.054]	310	0.256	0.077	[0.047]	315
Secondary school	0.48	-0.038	[0.056]	310	0.327	-0.021	[0.052]	315
Food Insecure	0.60	-0.026	[0.054]	310	0.737	-0.004	[0.050]	315
Income Generation								
Sex work	0.85	0.03	[0.039]	310	0	0	[.]	315
Shop keeping	0.38	-0.032	[0.052]	310	0.494	-0.043	[0.056]	315
Agriculture	0.00	0.015	[0.010]	310	0.385	-0.043	[0.051]	315
Savings								
Has M-PESA account	0.87	-0.002	[0.030]	310	0.987	0.012	[0.009]	315
M-PESA balance	658	-304	[189.457]	269	418	-264	[231.364]	311
Has Bank account	0.38	-0.062	[0.053]	310	0.308	-0.012	[0.052]	315
Bank balance	1729	-341	[510.864]	109	448	1094	[659.107]	92
Has Home savings	0.40	-0.068	[0.054]	310	0.128	-0.019	[0.037]	315
Home balance	6686	1615	[4735.590]	111	2282	1886	[1849.965]	37
Sexual Behavior								
Years in sex work	4.78	0.471	[0.439]	310				
Num. regular clients	4.12	-0.25	[0.300]	255				
Sexually active in past year					0.581	-0.076	[0.056]	313
Num. partners in past year					1.167	0	[0.138]	171

Note: Sample includes all women interviewed at baseline. Treatment differences are estimated controlling for age and cluster fixed effects. Statistically significant at 10% (*), 5% (**), and 1% (***).

Table 2: Dependent Illness and Sexual Behavior during observation period

Sample	**Indicator**	Urban Control Mean	Urban Treat Diff	Urban Std. Error	Urban N	Rural Control Mean	Rural Treat Diff	Rural Std. Error	Rural N
	Dependent Illness								
Weeks	% with Dependent Illness	0.20	-0.013	[0.020]	3512	0.319	0.008	[0.024]	3696
Weeks with illness	Cost of Dependent Illness (KSh)	652	250.38	[382.899]	694	334	60.13	[61.311]	1192
	Sexual Behavior								
Women	% having any acts	0.96	0.011	[0.022]	301	0.39	-0.059	[0.054]	315
Weeks	% having any acts	0.81	-0.035	[0.031]	2933	0.11	-0.031*	[0.018]	3696
Sexual acts	% remunerated	0.97	0.006	[0.011]	17392	0.65	-0.015	[0.074]	372
Sexual acts	% protected	0.92	0.001	[0.020]	17386				
Sexual acts	% higher risk (anal)	0.10	-0.026	[0.027]	16797				
Sexual acts	Mean amount paid (KSh)	500	-47.624	[38.045]	17333				

Note: Samples for each row include all available data. Nine women in the urban samples never recorded any sexual behavior diaries and are considered attrited from analyses of sexual behavior. Also among urban women, interview data is available even in weeks when sexual behavior diary data may be missing (see section 2.6). This results in different sample sizes when examining weeks with health shocks and weeks with sexual activity. Treatment differences are estimated controlling for age and cluster fixed effects. Standard errors for the weeks and sexual acts samples are clustered at the individual level. Statistically significant at 10% (*), 5% (**), and 1% (***).

Table 3: Effects of Intervention on Total M-PESA Savings

	Full Sample		Rural		Urban	
Treat X Post	Coefficient	SE	Coefficient	SE	Coefficient	SE
Post Period Consists of:						
Weeks 1-12	229	(160)	295	(234)	185	(223)
Weeks 2-12	249	(168)	325	(245)	196	(235)
Weeks 3-12	273	(178)	345	(258)	227	(248)
Weeks 4-12	307	(187)	376	(272)	266	(261)
Weeks 5-12	330*	(196)	409	(287)	278	(271)
Weeks 6-12	351*	(200)	437	(291)	293	(280)
Weeks 7-12	386*	(206)	468	(297)	335	(290)
Weeks 8-12	416**	(210)	492	(299)	368	(298)
Weeks 9-12	446**	(217)	523*	(301)	399	(316)
Weeks 10-12	473**	(226)	557*	(304)	420	(339)
Weeks 11-12	541**	(241)	654**	(305)	463	(377)
Week 12	522*	(297)	672**	(311)	408	(512)

The data used for the analysis presented above consists of weekly individual level panel data where total M-PESA savings is measured each week using administrative data. The table reports estimates of η_2 from equation 1 which represents the treatment effect on total M-PESA savings in the post period. The coefficients are estimates of η_2 from the OLS estimation of equation 1 that includes individual and week fixed effects. The pre-period is defined as the baseline period (before the treatment intervention occurred). We vary the post period, initally defining the post-period as the 12 weeks following the treatment intervention, and then systematically varying this by defining the post-period as weeks 2-12, 3-12, . . . 12. Each row above presents the coefficent that estimates η_2 depending on the defined post-period. Each column presents the estimates for the total sample, the rural sample, and the urban sample. *** indicates statistical signifigance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 4: Woman-level Estimates: Effects on the use of transactional sex for shock coping and STIs

	Rate of sex coping (1)	Any STI symptom (2)
Treatment	-0.0487***	-0.0533**
	[0.0166]	[0.0203]
Treatment x Urban	0.0186	-0.00445
	[0.0472]	[0.0364]
Observations	530	522
R-squared	0.552	0.097
Mean in rural control	0.0854	0.0890
Mean in urban control	0.5818	0.0909
Treat + Treat x Urban	-0.0300	-0.0578*
	[0.0441]	[0.0308]

Note: Estimation of β_1 (Treatment Effects) and β_2 (Differential Treatment Effects for Urban Women) in equation 2 at the woman level, controlling for age and cluster-fixed effects. Dependent variables are: cols 1- the proportion of shocks that were followed by transactional sex in the next week; col 2 - whether any STI symptom was reported at any point during the observation (queried weekly during weeks 6-12). Sample sizes in all columns are reduced by restriction to women ever reporting any shock during the observation period. Sample size in col 2 is further reduced by missing data on STIs. Robust standard errors are reported in brackets. Statistically significant at 10% (*), 5% (**), and 1% (***).

Table 5: Effects on Sexual Behavior As Shock-Coping: Urban Sample

	Number of Partners (1)	Regular Clients (2)	Casual Clients (3)	Risk Index (4)
Dependent Illness	0.810***	0.492***	0.265	0.185**
	[0.226]	[0.146]	[0.190]	[0.0798]
Treat X Dependent Illness	-0.457	-0.503**	0.0549	-0.164*
	[0.362]	[0.216]	[0.276]	[0.0974]
Observations	2634	2634	2634	2054
Individuals	294	294	294	278
R-squared	0.005	0.005	0.002	0.007
Mean Control	6.49	2.48	3.88	-0.038

Note: Estimation of β_1 (Effect of dependent illness) and β_2 (Differential Effect of dependent illness for women in the treatment arm) in equation 3 for the urban sample at the woman-week level, employing woman-fixed effects. Column headers indicate the dependent variable, which is a count for columns 1-3 and continuous in column 4. . The weeks in the sample are reduced from 2933 to 2668 as the shock indicator from last week is not available in the first week of the data. The sample is further reduced to 2634 weeks among 294 women, due to cases where the week follows a week of missing interview data (so no lagged shock is available). The sample in column 4 for the risk index is conditional on any sexual activity in the week, and is further reduced by missing data on amount paid by client for some weeks. The risk index is created using principal components analysis of number of regular and casual clients, mean amount paid per act, number of acts that were unprotected and number of acts that were anal. Standard errors are reported in brackets and are clustered at the individual level. Statistically significant at 10% (*), 5% (**), and 1% (***).

Table 6: Effects on Sexual Behavior as Shock-Coping: Rural Sample

	Any sexual activity	Transactional sex		Risk Index
	(1)	(2)	(3)	(4)
Dependent Illness	0.0166	0.0330**	0.124***	1.378***
	[0.0143]	[0.0129]	[0.0458]	[0.455]
Treat X Dependent Illness	-0.0000313	-0.0275	-0.105*	-1.820***
	[0.0217]	[0.0173]	[0.0627]	[0.665]
Sample	All	All	Woman ever reports TS	Weeks with any sex
Observations	3358	3358	899	290
Individuals	312	312	83	109
R-squared	0.002	0.003	0.011	0.049
Mean control	0.102	0.067	0.235	3.199

Note: Estimation of β_1 (Effect of dependent illness) and β_2 (Differential Effect of dependent illness for women in the treatment arm) in equation 3 for the rural sample at the woman-week level, including woman-fixed effects. Column headers indicate the dependent variable. The sample is reduced from 3696 weeks to 3390 weeks as the shock indicator from last week is not available in the first week of the data; it is further reduced to 3358 weeks among 312 women due to cases where the week follows a week of missing interview data (so no lagged shock is available). Column 3 includes only women who ever report engaging in transactional sex; column 4 includes only woman-weeks with any sexual activity. Risk Index is created with principal components analysis of whether the sex is paid and whether a condom was used. Standard errors are reported in brackets and are clustered at the individual level. Statistically significant at 10% (*), 5% (**), and 1% (***). Note that the coefficient on Treat x Dependent Illness is marginally significant in column 2 (p-value 0.114).

Table 7: Medium-term Impacts: Savings

	All	Rural	Urban
	(1)	(2)	(3)
Treat X Week 12	521*	680**	398
	(296)	(311)	(510)
Treat X Week 35	371	294	491
	(302)	(324)	(508)
Number of observations	1,716	876	840
Number of women	612	310	302
Control Mean			
Week 12	1,210	422	2,004
Week 35	919	306	1,497

Estimation of θ_1(Treatment Effect 12 weeks after intervention) and θ_2(Treatment Effect 4.5 months after interventino) in equation 4. The estimates above indicate the treatment effect on the difference in savings balances between baseline and the post period. Total observations of 612 women are greater than the number of women surveyed at endline because this analysis relies solely on adminsitrative data. Standard errors are reported in () and are clustered at the individual level. Statistically significant at) 10% (*), 5% (**), and 1% (***).

Table 8: Medium-term Impacts: Reported responses to shocks over previous 4 months

	Sex (1)	Rely on Savings (2)	Reduce Consumption (3)	Loan (4)	Gift (5)
Treatment	-0.114**	0.376***	-0.138**	-0.0536	0.0391
	[0.0545]	[0.116]	[0.0635]	[0.115]	[0.0713]
Treatment x Rural	0.113**	-0.471***	0.0699	0.0531	-0.0772
	[0.0545]	[0.139]	[0.0949]	[0.123]	[0.0996]
Observations	279	279	279	279	279
R-squared	0.044	0.075	0.015	0.007	0.014
Mean in urban control	0.134	0.343	0.343	0.358	0.209
Treat + TreatxRural	-0.001	-0.095	-0.068	-0.001	-0.038
	[0.002]	[0.075]	[0.071]	[0.0455]	[0.070]

Note: Estimation ρ_1 (Treatment Effects) and ρ_2 (Differential Treatment Effects for rural women) in equation (5) at the woman level, controlling for age and cluster-fixed effects. Samples are conditioned on experiencing a shock during the 4 months prior to data collection, which was 4-5 months after the weekly observation period. Dissaving is indicated by either reporting relying on savings or reporting that the shock was not a financial burden. Standard errors are reported in brackets, clustered at the geographic cluster level. Statistically significant at 10% (*), 5% (**), and 1% (***).

Table 9: Medium-term Impacts: Sexual activity in Rural Sample

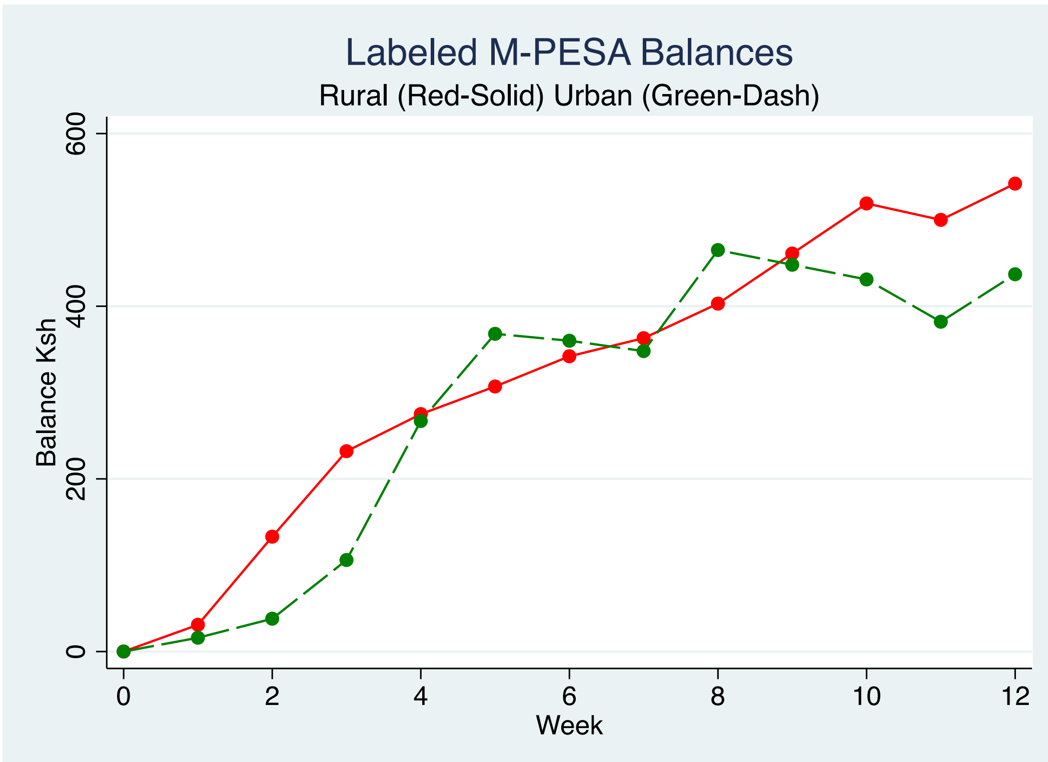
	Any sexual activity in previous 4 months			
	(1)	(2)	(3)	(4)
Treatment	-0.0857	-0.1000	-0.304*	-0.315**
	[0.0658]	[0.0628]	[0.150]	[0.131]
Treatment x No Shock	0.106	0.134	0.232	0.201
	[0.108]	[0.0944]	[0.257]	[0.299]
Sample	All	All	Ever reports	Ever reports
Controls	No	Yes	No	Yes
Observations	291	289	78	78
R-squared	0.016	0.086	0.053	0.124
Mean in control	0.336	0.336	0.610	0.610

Note: Estimation of ζ_1(Treatment Effects for women experincing a negative shock in the past 4 months) and ζ_2(Treatment Effects for women who have not experienced a shock in the pats 4 months) in equation (6) at the woman level, controlling for age and cluster-fixed effects. The sample is reduced from 315 women to 291 due to attrition at follow up (see section 2.6). The sample in column 2 is further reduced due to some missing data for the additional controls: marital status, primary school completion, and baseline reporting of transactional sex. Samples in cols 3 and 4 include only women who ever report engaging in transactional sex. Standard errors are reported in brackets, clustered at the geographic cluster level. Statistically significant at 10% (*), 5% (**), and 1% (***).

Appendix

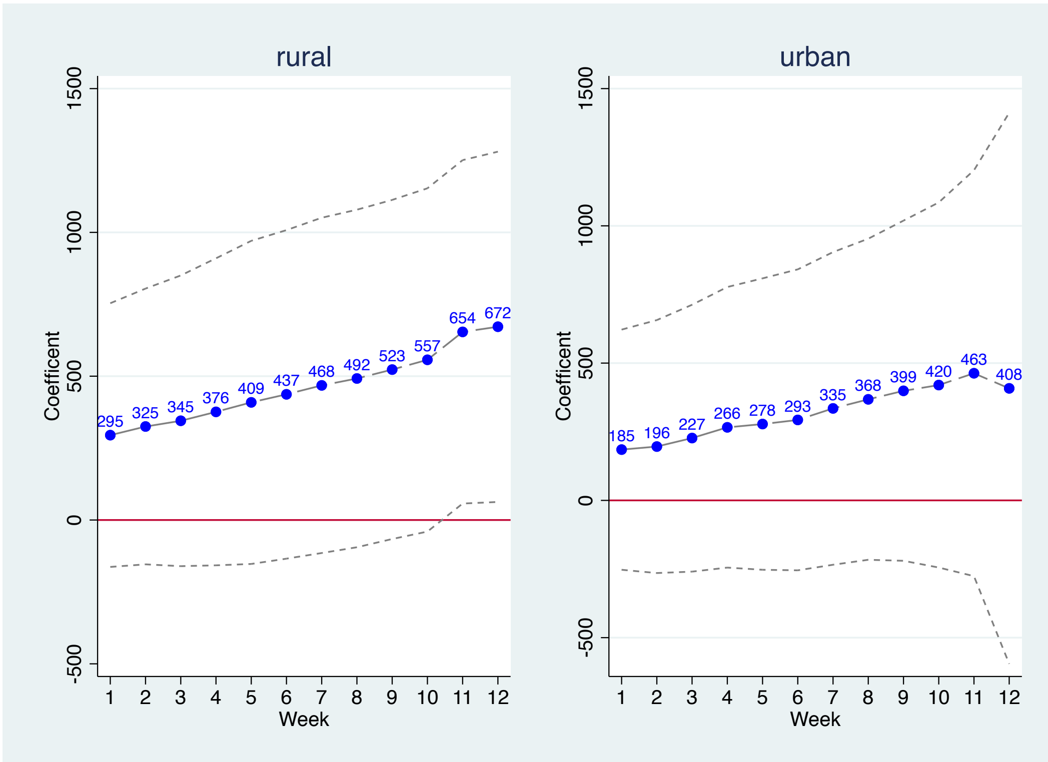
A Savings Details

Figure A.1: Labeled M-PESA Account Balances



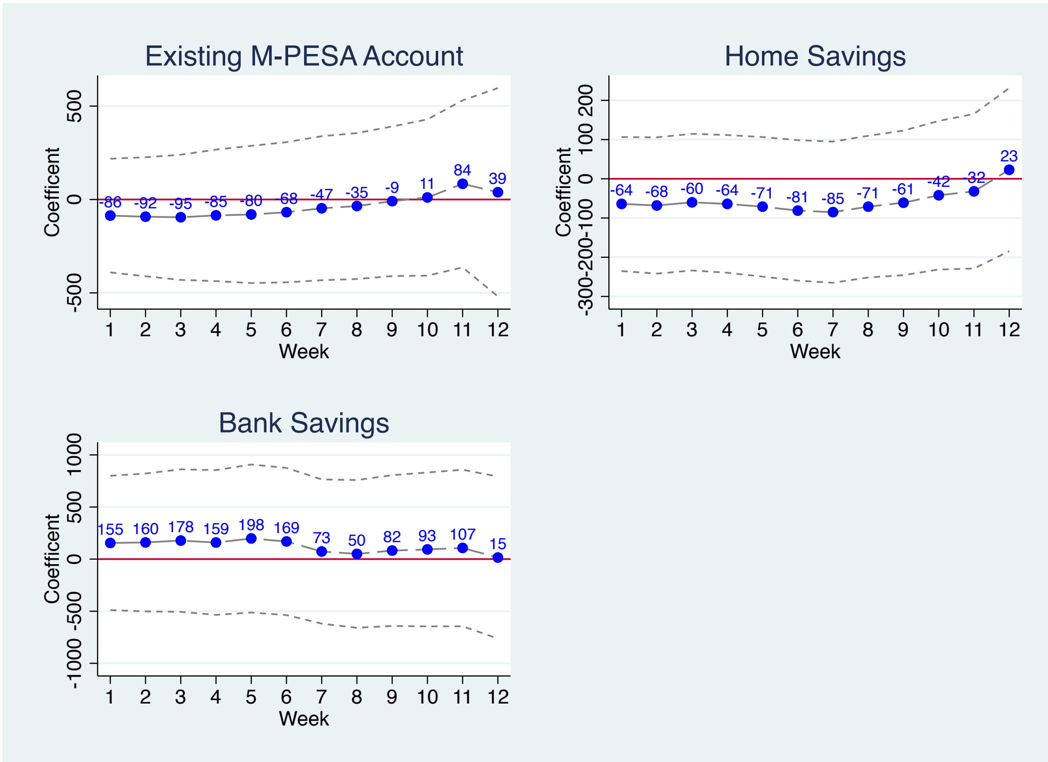
The figure above plots the average balance of all labeled M-PESA accounts in the treatment group in a given week. The solid red line represents average balances in the labeled M-PESA account for rural women in the treatment group. The dashed green line represents average balances in the labeled M-PESA account for urban women in the treatment group. Labeled M-PESA accounts that have not been adopted (no deposits have been made into them) are coded as having a zero balance. Amounts in the labeled M-PESA accounts are winsorized at the 99th percentile.

Figure A.2: Effect of Intervention on M-PESA Savings by Rural and Urban Samples



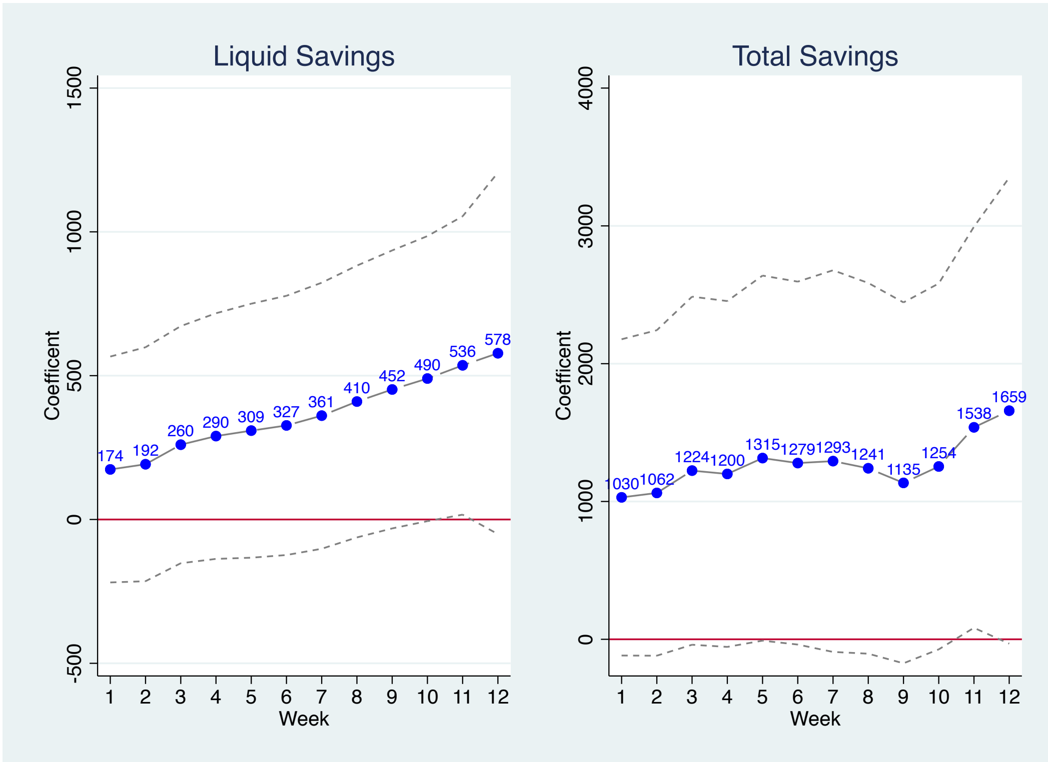
Note: Analogs to Figure 4 shown separately for rural and urban samples. The data used for the analysis presented above consists of weekly individual level panel data where total M-PESA savings is measured each week using administrative data. The plots represent estimates of η_2 from equation 1, indicating the treatment effect on the difference in savings balances between baseline and the post period. The coefficients plotted above are estimates of η_2 from the OLS estimation of equation 1 that includes individual and week fixed effects. The pre-period is defined as the baseline period (before the treatment intervention occurred).The x-axis indicates the start of the post period. For example, Week 1 represents a post period that consists of weeks 1-12; Week 2 represents a post period that consists of weeks 2-12. The y-axis is the size of the coefficient. The dotted line indicates the 95% confidence interval.

Figure A.3: Other Savings Accounts



Note: Analogs to Figure 4 but the outcome is now balance in existing M-PESA account only, home savings only, and banks savings only. These figures suggest there was no significant decline in any of these other accounts. The data used for the analysis presented above consists of weekly individual level panel data. The plots represent estimates of η_2 from equation 1, indicating the treatment effect on the difference in savings balances between baseline and the post period. The coefficients plotted above are estimates of η_2 from the OLS estimation of equation 1 that includes individual and week fixed effects. The pre-period is defined as the baseline period (before the treatment intervention occurred).The x-axis indicates the start of the post period. For example, Week 1 represents a post period that consists of weeks 1-12; Week 2 represents a post period that consists of weeks 2-12. The y-axis is the size of the coefficient. The dotted line indicates the 95% confidence interval.

Figure A.4: Treatment Effects on Liquid and Total Savings



Note: Analogs to Figure 4 but the outcome is now liquid savings (left panel) or total savings (right panel). Liquid savings includes home savings and M-PESA savings. Total savings includes liquid savings, plus bank savings. The data used for the analysis presented above consists of weekly individual level panel data. The plots represent estimates of η_2 from equation 1, indicating the treatment effect on the difference in savings balances between baseline and the post period. The coefficients plotted above are estimates of η_2 from the OLS estimation of equation 1 that includes individual and week fixed effects. The pre-period is defined as the baseline period (before the treatment intervention occurred).The x-axis indicates the start of the post period. For example, Week 1 represents a post period that consists of weeks 1-12; Week 2 represents a post period that consists of weeks 2-12. The y-axis is the size of the coefficient. The dotted line indicates the 95% confidence interval.

B Data checks

Table B.1: Analysis of Attrition

Administrative data (M-PESA)	Sample	Control	Treatment	Difference	p-value
Has baseline M-PESA data	Pooled	0.975	0.977	-0.0017	0.887
Has baseline M-PESA data	Urban	0.964	0.979	-0.015	0.419
Has baseline M-PESA data	Rural	0.987	0.975	0.0124	0.42

Weekly interview data	Sample	Control	Treatment	Difference	p-value
Weeks of interview data	Pooled	11.51	11.48	0.03	0.818
Weeks of interview data	Urban	11.31	11.26	0.05	0.815
Weeks of interview data	Rural	11.72	11.68	0.04	0.789
Has 10+ weeks interview data	Pooled	0.966	0.944	0.022	0.186
Has 10+ weeks interview data	Urban	0.952	0.917	0.035	0.216
Has 10+ weeks interview data	Rural	0.98	0.969	0.012	0.487

Sexual behavior (interview+diary)	Sample	Control	Treatment	Difference	p-value
Weeks of complete sex data	Pooled	10.6	10.55	0.048	0.837
Weeks of complete sex data	Urban	9.54	9.31	0.226	0.580
Weeks of complete sex data	Rural	11.72	11.68	0.04	0.789
%Interview weeks with sex data	Pooled	.920	0.919	0.002	0.784
%Interview weeks with sex data	Urban	.842	0.826	0.016	0.195
%Interview weeks with sex data	Rural	1	1	0	0
Woman has 10+ weeks sex data	Pooled	0.845	0.809	0.036	0.234
Woman has 10+ weeks sex data	Urban	0.717	0.634	0.0823	0.122
Woman has 10+ weeks sex data	Rural	0.98	0.969	0.012	0.487

Endline data	Sample	Control	Treatment	Difference	p-value
Has endline data	Pooled	0.922	0.924	-0.0017	0.935
Has endline data	Urban	0.922	0.924	-0.002	0.936
Has endline data	Rural	0.924	0.925	-0.0009	0.974

Table B.2: No link between shocks and missing diary data

	Missing diary data	
	(1)	(2)
Dependent Illness	0.0015	-0.013
	[.0106]	[.0200]
Treatment X Dependent Illness		0.032
		[.033]
Observations	3513	3513
R2	0.0000	0.0002

Note: Estimates control for age, cluster fixed effects and woman fixed effects. Standard errors in brackets, clustered at the geographic cluster level.

Table B.3: No link between treatment and experience of shocks

	Dependent Variable	Treatment Coefficient	SE	Observations	R-squared
(1)	Any shock	-0.0250	[0.0358]	579	0.001
(2)	Illness	0.0190	[0.0371]	579	0.001
(3)	Death	-0.0153	[0.0106]	579	0.007
(4)	Birth	-0.00254	[0.0115]	579	0.000
(5)	Job Loss	-0.00266	[0.0209]	579	0.005
(6)	Theft	0.00877	[0.0168]	579	0.002
(7)	Damage to property	-0.00535	[0.00819]	579	0.003
(8)	Legal trouble	-0.00125	[0.00731]	579	0.000
(9)	Conflict	-0.0197	[0.0119]	579	0.011
(10)	Crop Loss	-0.0121	[0.0159]	579	0.002
(11)	Livestock illness/death	-0.00137	[0.00908]	579	0.003

Note: Estimates control for age and cluster fixed effects. Standard errors in brackets, clustered at the geographic cluster level.

C Alternative shock measures

Other financial shocks, both income and expenditure, were considered for this analysis. Unfortunately, our measures of income are too noisy to construct a reliable measure of week-to-week income shocks; we do not observe in the data that our measures of income shocks predict an increase in transactional sex in the following week.

Expenditures that often present financial burdens include contributions to funerals and other social events. However, funerals are not useable for this analysis, as the travel away from home that typically occurs surrounding a funeral in this context prevents individuals from engaging in normal sexual and earning activities. Contributions to social events are not unexpected, and there may be considerable leeway regarding in which week the payment is made, so the timing of payments likely responds to income and thus transactional sex behavior.

A final consideration is the payment of school fees. While the national government mandates *de jure* free public primary education, *de facto,* most parents still pay fees to send both primary and secondary students to school. School fees are commonly reported as an acute financial burden in Kenya. While these are not unexpected, and likely exhibit some leeway in the timing of payment, participants in our training interventions unfailiingly reported these as financial shocks. Payment of school fees is not an ideal shock measure, as payment likely responds on both the extensive and intensive margins to contemporaneous or recent income. That is, while school fees are due at the start of each term, households may simply pay them (or partially pay them) when they have the money. However, we believe it is reasonable to claim that income in a given week would not affect the payment of school fees in the prior week, and that by relying on lagged payment, we can estimate a behavior response to this financial burden.

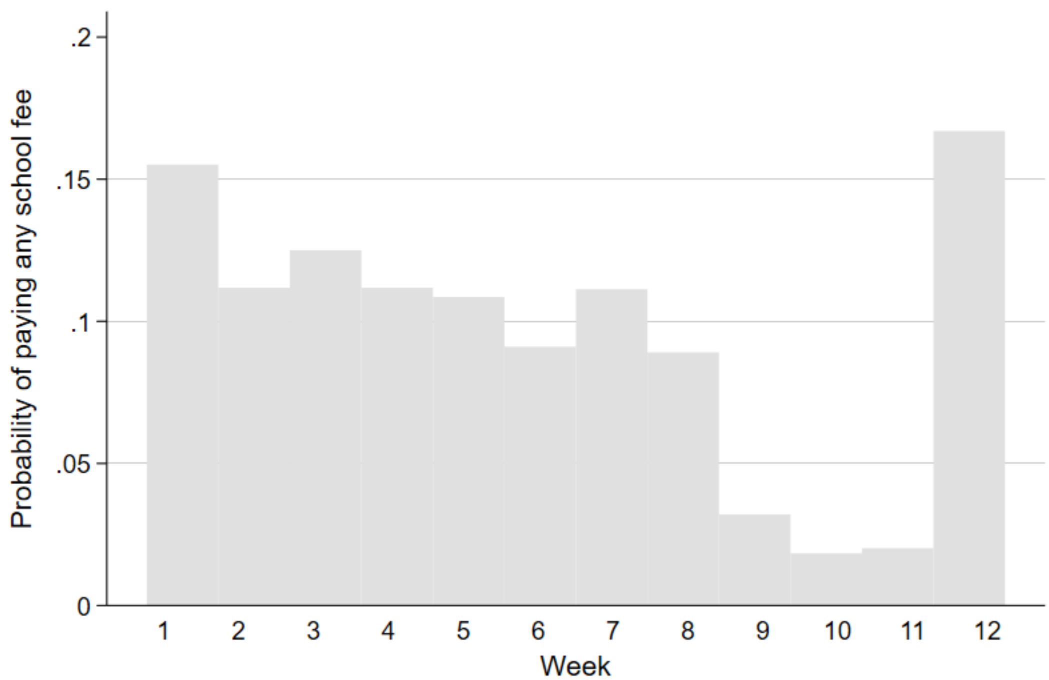
We recognize that considering school fees as a financial shock is problematic in a number of ways. Nonetheless we seek to verify that the behavior observed in this study in response to the more exogenous health shocks, and the estimated impact of savings access on this response behavior, is not tied to any unique nature of dependent health shocks.

C.1 Descriptive statistics on school fees

The payment of any school fees is relatively rare, with only 9.5% of women-week observations including the report of any school fees. This is more common at the start of a school term (weeks 1 and 12 in our data), but occur throughout our observation period, as shown in Figure C.1. Fifty-two percent of respondents paid school fees at least once during the study period; of those who ever paid, most (72%) paid only once or twice during the period. Four women paid as many as eight times, assumedly paying in weekly installments.

School fees are a significant expense. After winsorizing sample-specific outliers (top 1%), the median (mean) payment is 250 KSh (630 KSh) in the rural sample and 600 KSh (1445 KSh) in the urban sample, strikingly comparable to the cost of a dependent illness.

Figure C.1: Distribution of school fees payments over study weeks



C.2 Impact of school fees payments on sexual behavior and treament effects

We restimate equation 3 employing measures of school-based expense shocks for both the rural and urban samples. Our primary lagged expense shock is a continuous indicator for the amount of school fees paid last week (in 500 KSh). We additionally consider a subjective measure of educational expenditure burden: the number of days last week the respondent reported lacking sufficient money to pay educational expenses. Sexual behavior in the following week may indicate a woman's attempt to compensate for a large expenditure in the previous week (in the case of the first measure), or her efforts to earn the amount lacking to pay the fees (in the second measure). In both cases we may observe a sexual behavior response for the purpose of increasing earnings in response to school fees being due.

The results of these estimations are shown for the urban and rural samples in Tables C.1 and C.2, respectively. The findings are broadly consistent with those from the analog estimations presented in Tables 5 and 6. In general, shocks have a positive impact on sexual behavior and risk in the following week, and assignment to the treatment group exhibits a negative impact of comparable (and in some cases larger) size. These effects are more precisely estimated for the urban sample than for the rural sample.

In the urban sample, the treatment effects are precisely estimated when using the indicator of fee payment, but not when using the indicator of lacking funds. The effect sizes suggest a 4% increase in sexual partners and an increase in risk of 0.078 standard deviations per 500 KSh paid for school fees, a response that is fully offset by treatment assignment. You can notice the reverse causality between sexual behavior and school fees payment in the same week, as demonstrated by the positive and precisely estimated coefficients in the third row. That is, in a week with a lot of sex and a lot of income, women are better able to make payments on school fees. This is further justification for why we employ a lagged indicator of shock.

In the rural sample, the impacts of shocks among the control group are not precisely estimated. Nonetheless, the effect sizes suggest a 3% increase in the probability of any sexual behavior following a 500 KSh payment of school fees, and a 4-5% increase in transactional sex and 3% increase in risk per day of school fees deficiency. In most cases, the treatment effects are negative and of comparable or larger size, but again none of these is precisely estimated.

Table C.1: Effects on sexual behavior as shock coping using school fees shock: Urban sample

"Shock"–>	School fees paid (in 500 KSh)				Days lacked money for school (0-7)			
Outcome –>	Total Partners (1)	Regular Clients (2)	Casual Clients (3)	Risk (4)	Total Partners (5)	Regular Clients (6)	Casual Clients (7)	Risk (8)
Shock last week	0.194***	0.215***	-0.020	0.069***	0.292**	0.049	0.226**	0.056**
	(0.042)	(0.057)	(0.031)	(0.007)	(0.114)	(0.070)	(0.088)	(0.023)
Treat x Shock last week	-0.281***	-0.233***	-0.050	-0.065***	-0.221	-0.031	-0.175	-0.028
	(0.071)	(0.061)	(0.051)	(0.009)	(0.157)	(0.081)	(0.126)	(0.031)
Shock this week	0.082	0.043	0.038	0.016*	-0.009	0.002	-0.015	0.023
	(0.058)	(0.029)	(0.030)	(0.010)	(0.070)	(0.034)	(0.059)	(0.016)
Observations	2,634	2,634	2,634	2,054	2,634	2,634	2,634	2,054
Individuals	294	294	294	278	294	294	294	278
Mean Control	6.49	2.48	3.88	-0.04	6.49	2.48	3.88	-0.04

Note: Estimation of β_1, effect of school fees shock, and β_2, differential effect for women in the treatment arm, in equation 3 for the urban sample at the woman-week level, including woman-fixed effects. Column headers indicate the dependent variable. Super-column headers indicate the shock measure: The amount of school fees paid (in 1,000 KSh), and the number of days in a week that the respondent indicated lacking sufficient money to pay educational expenditures. Samples are identical to those used in Table 5. Standard errors are reported in brackets, clustered at the individual level. Statistically significant at 10% (*), 5% (**), and 1%(***).

Table C.2: Effects on sexual behavior as shock coping using school fees shock: Rural sample

"Shock"–>	School fees paid (1,000 KSh)				Days lacked money for school (0-7)			
Outcome –>	Any sex	Transactional sex	Transactional sex	Risk	Any sex	Transactional sex	Transactional sex	Risk
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Shock last week	0.004	-0.000	0.003	-0.277	0.001	0.003	0.010	0.095
	(0.005)	(0.003)	(0.043)	(0.380)	(0.006)	(0.005)	(0.017)	(0.102)
Treat x Shock last week	-0.007	-0.005	-0.018	0.562	-0.003	-0.012	-0.038	-0.254
	(0.012)	(0.011)	(0.051)	(0.447)	(0.008)	(0.008)	(0.025)	(0.230)
Shock this week	-0.007	-0.007	-0.019*	0.001	-0.003	0.001	0.005	0.184
	(0.004)	(0.004)	(0.010)	(0.447)	(0.005)	(0.004)	(0.014)	(0.191)
Observations	3,358	3,358	899	290	3,358	3,358	899	290
Individuals	312	312	83	109	312	312	83	109
Mean Control	0.102	0.067	0.235	3.199	0.102	0.067	0.235	3.199

Note: Estimation of β_1, effect of school fees shock, and β_2, differential effect for women in the treatment arm, in equation 3 for the rural sample at the woman-week level, including woman-fixed effects. Column headers indicate the dependent variable. Super-column headers indicate the shock measure: The amount of school fees paid (in 1,000 KSh), and the number of days in a week that the respondent indicated lacking sufficient money to pay educational expenditures. Samples are identical to those used in Table 6. Standard errors are reported in brackets, clustered at the individual level. Statistically significant at 10% (*), 5% (**), and 1%(***).