Affording Abstinence: Experimental Evidence on Sex as Labor in Sub-Saharan Africa*

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Abstract

Whether to engage in sex and with whom are extremely consequential decisions for young women worldwide, and particularly in sub-Saharan Africa, where HIV rates are high. Yet little is known about the extent to which these decisions are financially-motivated. Using a randomized controlled trial with university students in Zambia, where participants received financial assistance calibrated to cover housing costs, we find that becoming sexually active, partner choice, and condom use are highly income-elastic. Treated participants are 5.1 percentage points less likely to have sex during the study, and the rate of condomless sex falls by one-third. Among women who were not sexually active at baseline, the likelihood of having sex falls by 40%, and partner quality conditional on sex greatly improves. Financial assistance also reduces financial precarity, increasing food security, mental health, and cognitive capacity. Economic need is a critical driver of sexual activity and relationship formation – even in population that is not typically regarded as financially vulnerable – and transactional sex is a pervasive funding mechanism for education. Economic agency significantly increases abstinence and condom use, the two behaviors that most affect HIV risk.

1 Introduction

During late adolescence, young people make critical decisions that affect their life trajectories. They often simultaneously decide when to stop human capital investment and join the labor force, in what skills to specialize, whether and when to marry and have children, and where to locate. Decisions about sexual behavior – if, when, with whom, and how to engage – can be especially consequential for young women. This is particularly true in sub-Saharan Africa, where HIV rates are high, contraceptive use is low, young women often receive significant financial transfers from male sexual partners, and unplanned pregnancies play an important role in derailing education. Despite the importance of early sexual behavior for human capital attainment, labor force participation, marriage, and fertility, economists know very little about

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what drives women to engage in sexual relationships and with whom. Indeed, while a large literature has studied the importance of liquidity (e.g., from unemployment insurance) in allowing for longer job search, resulting in better employment matches, there has been very little attention to the importance of financial resources for delaying becoming sexually active or choice of partners. In this paper, we measure the extent to which immediate financial need drives sexual behavior and partnering in sub-Saharan Africa, even among populations that are traditionally perceived as advantaged.

To do so, we use a randomized controlled trial with undergraduates at the University of Zambia paired with rich data collection to measure the benefits of monthly cash payments to young women on financial precarity and sexual behavior. We gave treated students ∼$40 per month for two semesters—roughly a month's rent.[1] We specially designed and implemented unique high frequency (approximately every two weeks) surveys to capture sexual outcomes that would be difficult to observe with a single, retrospective endline survey. The survey was self-administered on participants' smartphones, reducing the social desirability bias that pervades interviewer-administered surveys on sensitive topics (Mensch et al., 2003), and the biweekly cadence reduces recall bias, which is known to be substantial for sexual behavior in this setting (Luke et al., 2011). We maintain extremely high response rates throughout the entire academic year through a combination of airtime incentives and frequent contact from survey staff. These data let us track women across concurrent and sequential partners and collect novel information on markers of risky and transactional sexual behavior, such as the prevalence of cash and gift transfers, the age structure of partnerships, condom use, and financial transfer size. In most past data collection in sub-Saharan Africa, this type of information would be either impossible to collect or have to be inferred from coarser endline measures. We also collect very rich, high frequency information on other economic outcomes and wellbeing.

In line with descriptive work by Bau et al. (2026), our results reveal that university students in sub-Saharan Africa live much closer to subsistence than previously thought. Financial assistance increased treatment participants' consumption on necessities such as food and housing, and their basic food security improved dramatically. Treated participants were less likely to take out short-term high-interest loans, pawn or sell belongings, or say they had avoided healthcare because they could not afford it. We also find that treatment significantly reduces depression and improves performance on tests of fluid reasoning and cognitive control. In this way, financial aid for university students had many of the same benefits typically seen in cash transfer programs to extremely poor populations (Niehaus and Suri, 2026; Haushofer and Shapiro, 2016).

Perhaps most strikingly, financial assistance significantly changed participants' sexual behavior. The transfer reduces the share of women who report ever having had sex over the study period by 5.1 percentage points, a 10.7% reduction relative to the control. This effect is entirely driven by women who had not yet had sex at baseline, a source of heterogeneity we pre-registered. Among these women, entry into sexual activity is halved relative to the control group, and conditional on an encounter, partner quality *greatly* improves. For a given encounter, these participants rank their feelings of safety with their partner 1 standard deviation better, and an overall index of partner quality improves by 2.5 sd. For women who were already sexually active at baseline, there is no effect on whether sex occurs. However, conditional on a sexual encounter, condom use improves for both groups. Treated women are 13.0 percentage points more likely to use a condom in a given encounter, a 25.1% increase relative to the control mean, consistent with both increased bargaining power and a change in the composition of partners. Both the effects on sexual activity and condom use suggest that financial support could play a key role in preventing the transmission of sexually transmitted diseases in a

[1] All monetary values are converted from Zambian Kwacha to USD using the exchange rate in effect during our first baseline survey (26.1 ZMW per USD) (Bank of Zambia, 2026).

context where 14% of women aged 15–49 are HIV positive (Zambia Statistics Agency et al., 2024). Treated women also have fewer sexual encounters with partners who provide large transfers and are 6.1 percentage points less likely to report having done something they are not proud of for money, a 34.7% reduction.

We hypothesize that this pattern of results could be explained by women who already have financially supportive partners keeping those partners in expectation of the conclusion of the program, while unpartnered women delayed making financially-motivated matches. On the other hand, women with partners valued condom use more relative to cash during the intervention period and consequently had higher condom use. Since women who were not initially sexually active had less financially motivated encounters, partner quality conditional on an encounter increased. Altogether, these results suggest that financial need is an important driver of sexual behavior and that students facing severe liquidity constraints turn to costly risky coping strategies to finance their education. Strikingly, *the timing of becoming sexually active appears to be particularly sensitive to economic desperation.* Even among college students in sub-Saharan Africa, a group that is often perceived as "elite," there is a very strong negative elasticity between income and sexual behavior.

We are also able to track academic outcomes in administrative and survey data through the beginning of the next academic year. Even though financial need and unintended pregnancies are frequently-cited reasons for dropout in Zambia, particularly at the better-studied secondary school level, we do not find any significant effects on graduation, dropout, or having to repeat a year of classes in either the administrative or survey data. It is possible that this null result is because these outcomes are too coarse or too short-term to capture important academic effects, or because this urban population has especially good access to abortions (though abortions may still be costly for psychological or health reasons). A longer period of data collection may be needed to capture the effects of unintended pregnancies since, were a pregnancy to occur during our intervention, it would only result in a birth 9 months later. Similarly, the effects of financial deprivation may also be cumulative as learning and grades erode, and effects on outcomes as rare as dropout may only emerge over subsequent years. To the extent that cash does not affect these outcomes, our results provide suggestive evidence that the young women who make it to an African flagship university – who are highly positively-selected on ability – are able to use a combination of transactional sex, high interest loans, social insurance, and reduced food consumption to stay in school. Yet, the same resourcefulness that keeps young women in school leads to more unprotected sex, putting them at risk for HIV.

This paper contributes to three literatures. First, our results speak directly to the role of financial resources for delaying sexual behavior and relationship formation during critical periods. A very large literature has considered the importance of unemployment benefits in facilitating longer job search, with potential benefits for match quality (see Card et al. (2007) and Chetty (2008) for examples, or Cohen and Ganong (2026) for a meta-analysis). Much less attention has been paid to women's relationships, even though relationship formation and sexual behavior are highly-consequential decisions with as or more important implications for life trajectories. Two exceptions are Aizer et al. (2016) and Baird et al. (2011). Aizer et al. (2016) shows that recipients of the "Mothers' Pension Program" delayed remarriage in the 20th Century in the U.S., though there are no detectable effects on partner quality. In the context of Malawi, Baird et al. (2011) show that unconditional cash transfers during adolescence reduce teenage pregnancy and early marriage. Our rich data collection highlights a likely mechanism underlying these results – that young women in sub-Saharan Africa become sexually active due to financial need – and shows that this mechanism is widespread, even outside of populations poor enough to typically be targeted for conditional cash transfers.

Second, we contribute to the literature on transactional sex. Transactional sex is a different market from

explicit sex work and typically features longer-term relationships with both financial and romantic elements. These relationships often – though not always – occur between older men who may or may not be married and young women. Public health estimates of the prevalence of transactional sex in sub-Saharan Africa frequently treat it as a binary distinction and collect self-reported information on whether sexual activity is motivated by gifts or money. These measures may underreport the degree to which sex is responsive to income, both due to social desirability bias and because women may perceive relationships that combine romantic and transactional elements as existing in a "gray" area. Nonetheless, even self-reports suggest that transactional sex markets are large and important in sub-Saharan Africa, with rates of 14% in the last year among young women in Uganda (Stamatakis et al., 2021) and of 44% in Kenya and 25% in Burkina Faso among highly-economically vulnerable populations (Machoka et al., 2026). Neither are these markets unique to sub-Saharan Africa. While measures of prevalence in the US are very limited, self-reports in survey data put rates of "sugar daddy" relationships as high as 5% on at least one American campus (Patel et al., 2026), and a variety of websites exist to match sugar daddies with potential sugar babies.[2]

Despite this, and while there are literatures on other sexual-financial relationships, such as marriage (Chiappori, 2020) and commercial sex work (Cunningham and Shah, 2016), in economics, little attention has been paid to transactional relationships. Study of these markets is challenging because the degree to which a relationship is financially-motivated is hard to infer from its observed characteristics: relationships can feature gift-giving for altruistic or cultural reasons, as well as transactional ones. One of the most prominent papers in economics related to this topic, Dupas (2011), shows that information about the relative HIV risk of older men changes Kenyan secondary school girls' partner selection. But since this paper does not observe transfers/gifts and does not shock financial need, it cannot discern the extent to which age-disparate relationships are financially-motivated vs. other considerations like status.

A smaller set of papers engages directly with economically-motivated sex, but most are focused either on sex-workers rather than transactional relationships or on very financially marginal communities. Gertler et al. (2005) document that men pay a substantial premium for condomless sex with sex workers in Mexico, providing direct evidence that the financial value of risk reduction is priced into sex markets. Robinson and Yeh (2011) use detailed daily diaries to show that sex workers in Western Kenya supply riskier sex in response to household health shocks, and Jones and Gong (2021) show that providing vulnerable women in Kenya with a mobile savings account reduces transactional sex, which is used for shock-coping. Dinkelman et al. (2008) show that local labor market conditions affect sexual behavior among youth in Cape Town. Our paper makes three contributions to this literature. First, we provide – to our knowledge – the first experimental estimate of the income elasticity of sex outside a population that is perceived to be very financially marginal. Second, we study a population that has been almost entirely absent from this literature: female undergraduates in Africa. Our finding that financial need shapes sexual behavior even among women who are typically considered highly-positively selected suggests that financially-motivated sex is far more pervasive than the existing literature, with its focus on high-risk populations and shock responses, implies. Third, and perhaps most importantly, we focus on transactional sex during the critical period of late adolescence, which allows us to show that financial need affects not just condom use or the frequency of sexual activity but also the timing of becoming sexually active and partner quality.

Third, we contribute to the literature on cash transfers and women's health outcomes. Baird et al. (2012) document that cash transfers to adolescent girls in Malawi reduce HIV and HSV-2 prevalence, Duflo et al. (2015) find that subsidizing school uniforms in Kenya reduces teenage pregnancy and dropout, and Handa et

[2]See https://chicagoreader.com/adult/sugar-daddy-sites/.

al. (2014) find that Kenya's national cash transfer program to caregivers for orphans and vulnerable children delays becoming sexually active. A separate strand of work studies conditional cash transfers and lotteries that pay individuals for maintaining HIV- or STI-negative status. Björkman Nyqvist et al. (2018) find that lotteries are particularly effective at reducing the spread of HIV in Lesotho. The evidence on conditional cash transfers for negative STI tests seems more mixed at first glance, with Walque et al. (2012) finding negative effects on STIs for sizable transfers at the 1-year mark in Tanzania, while Kohler and Thornton (2012) find no effect in Malawi. But, in Malawi, negative effects on risky sexual behavior for women emerged *after* the payments were made, while in Tanzania reduced STI rates only appeared after two rounds of payments had already been made. In both cases, in line with our experiment, which isolates the effect of liquidity as opposed to conditionality, liquidity may have played a role in reducing risky sex. Our high-frequency data on sexual behavior and partners also helps build on this literature by allowing us to characterize existing sexual and financial partnerships and measure detailed changes in sexual behavior in response to financial support. Our results highlight that transactional sex may be an important channel through which cash transfers to young women affect health and help explain the pattern of conditional cash transfer results.

Our results have direct implications for policies aimed at reducing HIV transmission and unintended pregnancy among young women in sub-Saharan Africa. The HIV burden in this population is substantial, and young women are twice as likely to be infected as their male peers (UNAIDS, 2021). Adolescent pregnancy rates are among the highest in the world (UNICEF, 2025). Age-disparate sexual relationships with older, higher-resource men have long been identified as a primary driver of HIV transmission to young women (Stoebenau et al., 2016; Vollmer et al., 2017), but the design of effective policy responses to prevent transmission has remained an open question. Information about the risks of HIV does affect women's knowledge of the risks, but often does not change the risky behaviors themselves (Duflo et al., 2015; Doyle et al., 2011; Michielsen et al., 2010). Our results help explain this apparent contradiction. If much of risky behavior is motivated by economic considerations, information may not be enough, while economic empowerment may be a powerful way to reduce HIV and pregnancy risk. Financial support also has the benefit of improving consumption, mental health, and cognitive functioning, as well as sexual health. Reducing financial pressure gives women the agency to make sexual decisions on terms other than economic necessity. Remarkably, providing enough funds for basic subsistence, without any additional information or incentives, promotes the two behaviors most associated with reducing HIV risk: abstinence and condom use.

Finally, our results speak to questions surrounding the provision of financial support during tertiary education in low-income countries. Tertiary enrollment is growing rapidly in sub-Saharan Africa, even as there has been little policy focus on how to deliver higher education in a setting where the typical student's household is increasingly poor, rural, agricultural, and living close to subsistence (Bau et al., 2026). Our results suggest that the standard model of tuition-only support – bursaries that cover fees but leave students to fund their own food, housing, and other living costs – results in severe financial deprivation and reliance on transactional sex to make ends meet. The non-tuition components of subsistence are not peripheral; they are the binding constraint on whether students can study, eat, sleep safely, and avoid risky coping strategies. Hence, relative to rich countries, financial aid in tertiary settings in sub-Saharan Africa may more closely resemble a cash transfer to a population that is both economically vulnerable and high-impact, with direct cash support at a modest scale generating sizable gains across consumption, food security, mental health, cognitive functioning, and sexual health.

2 Background

2.1 The University of Zambia

The University of Zambia (UNZA) is the oldest and most prestigious university in the country. Founded in 1966, the year after Zambian independence, it is located in the capital city of Lusaka and enrolls approximately 30,000 undergraduates across two campuses. UNZA has educated a substantial share of Zambia's political and professional elite: every Zambian president born after 1945 has attended UNZA, as have all of the country's Supreme Court justices (Judiciary of Zambia, 2025; Cabinet Office, Government of Zambia, 2024; Zambia National Assembly, 2025; Council on Foreign Relations, 2025).

Financial aid is available to Zambian students through a government program known colloquially as "bursary," administered by the Higher Education Loans and Scholarships Board. After admission, students apply for bursary, which covers the total nominal cost of attendance: tuition, a meal allowance of ZMW 750 per month, and a housing allowance of ZMW 490 per month. Students allocated to on-campus housing have their housing covered directly; those who must find off-campus accommodation receive the housing allowance as cash. The majority of students in our sample (79%) receive bursary support.

In practice, students report that the non-tuition components of bursary are insufficient to cover the cost of living in Lusaka.[3] The meal allowance does not stretch to three meals a day, on-campus housing is heavily oversubscribed, and many students are unable to secure a place (only 18% of our sample lives in allotted housing, despite 81% having applied). The off-campus housing allowance falls short of typical rents in neighborhoods near campus (the housing allowance only covers $\sim 60\%$ of costs for students in our data). Our intervention amount ($\sim$\$40 per month) is chosen to be roughly equal to out-of-pocket costs for off-campus housing. Bursary disbursement is also frequently delayed: students often report receiving bursary payments weeks or months after the start of each semester, leaving them to cover rent, food, and other expenses out of pocket or through family transfers in the interim. Students who do not receive bursary, or whose bursary is delayed or insufficient, must rely on family contributions, part-time work, or other sources of income to meet their basic needs during the academic year.

These conditions are typical of flagship universities across sub-Saharan Africa, where rapid expansion of tertiary enrollment has outpaced the growth of student support systems (Mve, 2021).

2.2 Background on Transactional Sex

Relationships between younger women and older men that involve significant cash or gift exchange have been widely documented throughout sub-Saharan Africa. Qualitative researchers report that women in these relationships often distinguish between partners who provide affection alone and partners who provide material support, with the latter often described in local idioms ("blessers," "sponsors," "ministers") that explicitly acknowledge the financial component while maintaining a frame of romantic or supportive partnership (Hunter, 2002; Luke, 2006; Stoebenau et al., 2016). These relationships are distinct from commercial sex markets in several important respects. They are typically ongoing rather than transactional at the level of individual encounters, with gifts and money flowing between partners over weeks, months, or years rather than at the point of each sexual act. The exchange is rarely priced explicitly: neither party negotiates a per-encounter rate, and the timing and magnitude of transfers are not contractually tied to specific sexual

[3] The cost of living in Lusaka is estimated to be 28% higher than the average across Zambia (Economic Research Institute, 2025)

acts. They also include romantic and social elements—dates, public acknowledgment of the relationship, introductions to friends—that are absent from commercial transactions.

These relationships have received considerable attention as a key factor in the spread of HIV in sub-Saharan Africa. Young women in the region bear a disproportionate HIV burden—they are more than twice as likely to be infected as their male peers—and a substantial epidemiological literature attributes this gap to age-disparate sexual partnerships with older, higher-resource men (Stoebenau et al., 2016; Wamoyi et al., 2016). Several features of these relationships compound HIV risk. Male partners are typically about 10 years older than their female counterparts (Luke, 2003; Luke, 2005), and older men have substantially higher HIV prevalence than men closer to the women's own age (Vollmer et al., 2017). Condom use is lower than in commercial sex work, and falls further as transfer size increases or as age gaps widen (Wamoyi et al., 2016). Transfers themselves take many forms—cash, mobile money, school fees, rent, airtime, and consumer goods are all commonly reported (Hunter, 2002; Luke, 2006)—but their role in shaping condom use and partner selection appears to be common across forms.

From an economic perspective, a natural question is whether to think of these relationships as labor: that is, whether women supply sex in exchange for compensation that they would not otherwise receive. The answer is not obvious. If women derive positive utility from the relationship itself—from companionship, attraction, aspiration, or the prospect of future commitment—then transfers may be incidental gifts within a partnership both parties value. If, on the other hand, women derive negative utility from the sexual component of the relationship and require compensation to participate, then the transfer functions as a wage, and the relationship is closer to a labor market transaction than a romantic partnership. Distinguishing between relationships as consumption vs. labor has been empirically difficult because both produce the same observed pattern of sex accompanied by gift exchange.

3 Experimental Design and Empirical Strategy

3.1 Recruitment

Participants were recruited on the UNZA campus in September and October 2024, at the end of the 2024 academic year. To be eligible, students had to be female, between the ages of 18 and 25, and full-time undergraduates not in their final year at UNZA. Recruitment was carried out by young female recruiters who were themselves recent UNZA graduates. The study operated a recruitment center in the Faculty of Arts and Sciences building; recruiters approached students across campus and brought interested students to the center for consent and to complete a baseline survey on their cell phones. The study was also advertised through posters that directed students to the recruitment center.

Once a student arrived at the recruitment center, her eligibility was verified using her student ID, and the study was explained by a recruiter before she signed informed consent documents. Recruiters emphasized that this would be a long-term recurring survey, and that participants would earn airtime each month for completing surveys, as well as data bundles if they kept up-to-date on the surveys. The consent form also informed participants that they might be randomly chosen to receive financial assistance as part of the study, but this was not emphasized in order to ensure that students would answer baseline questions honestly.

We recruited approximately 1,100 participants, anticipating that some would not return to UNZA for the 2025 academic year or would stop responding to surveys. The first three follow-up surveys, which took place pre-treatment, were sent in November 2024, December 2024, and January 2025. These follow-ups were

mainly to retain the sample but also allowed us to collected repeated pre-intervention measures. In the January 2025 survey, we asked participants whether they planned to return to campus for the 2025 academic year. Participants who completed this survey and indicated that they definitely planned to return formed the experimental sample of 1,022 women, who were randomized into treatment and control in February 2025. Biweekly surveying began once students returned to campus and the intervention started.

3.2 High-Frequency Mobile Survey

To collect data on health, well-being, and sexual behavior, we conducted a high-frequency mobile phone-based survey. After recruitment, participants were texted a link to a Qualtrics survey once every two weeks while on campus and once every four weeks while at home.

The combination of high frequency and self-administration was designed to address two well-known measurement challenges. First, retrospective reports of sexual behavior are subject to substantial recall bias: in field experiments comparing recall windows, respondents systematically underreport partners and misreport the timing of sexual encounters when asked to recall over long periods (Luke et al., 2011). By asking about the previous two weeks, we shorten the recall window to a period over which respondents can remember each partner and each encounter. Second, sensitive behaviors are systematically underreported when surveys are administered face-to-face by an interviewer, particularly in settings where the behavior carries social stigma (Mensch et al., 2003). Our survey is fully self-administered: participants complete it on their own phones, in private, with no enumerator present. We expect both features to improve the accuracy of reported sexual behavior relative to standard interviewer-administered, retrospective survey instruments.

Participants were compensated ZMW 15 for each completed survey, plus a ZMW 60 monthly data bundle if they remained up to date with their submissions. When a participant missed a survey, she was sent text reminders, followed by a phone call from the survey team if she did not respond. Attrition was low during the experimental period: 0.2% of treatment participants and 1.7% of control participants missed any given biweekly survey. Attrition increased somewhat after students left campus for term break, rising to 4.3%. We address attrition and provide bounds on our estimates in Section 5.1. In practice, even though attrition is differential, because it is very low, it has little effect on the results.

Sexual Behavior. Every survey contained questions about sexual behavior. If a participant indicated that she had had sex in the previous two weeks, she was asked to identify each partner. The first time a partner was listed, the participant was instructed to assign him an anonymous nickname that she would remember so that the same partner could be linked across future surveys. On this first listing, she was asked about the partner's characteristics, including his age, whether he was married, whether he was a student at UNZA, qualities she liked about him (attractiveness, sense of humor, financial support, etc.), whether she would be likely to introduce him to her friends and family, and the amount and value of gifts she had received from him to date. Every time a partner was reported—whether or not it was the first time—the participant was also asked about contraceptive use with that partner and gifts received from him in the past two weeks. This loop was repeated for every partner listed over the two-week period.

Other Outcomes. On odd-numbered surveys, participants were asked about consumption and earnings by category. On even-numbered surveys, they were asked three food security questions: how often they ate meat, whether they had skipped meals, and whether they had gone any days with fewer than two meals in the previous two weeks. On these same surveys, participants were also asked whether they had fallen behind

on housing, pawned or sold belongings, taken out a loan, or avoided healthcare due to costs in the previous four weeks. Finally, on these surveys, participants were asked whether they had done anything they were "not proud of" for money in the previous two weeks. This question was intended as a secondary measure of transactional sex, though it may also capture other behaviors such as borrowing, pawning, or stealing.

3.3 Intervention

In February 2025, we randomly assigned half of the experimental sample (511 participants) to a treatment group that received monthly cash transfers, stratifying on sexual activity level (i.e. has never had sex, has had sex but not in the past two weeks, and has had sex in the past two weeks), having pawned or sold items, or having ever skipped meals in the past two weeks.

We informed treatment participants of their assignment by phone in mid-February 2025. Each treatment participant was told that she would receive a stipend of ZMW 1,000 per month—approximately $40 at the prevailing exchange rate—for the four months of the first term, beginning the last week of February. Transfers were disbursed via mobile money, the standard payment infrastructure in Zambia, and were sent every four weeks. Participants in the control group were not contacted about the transfer program at this time.

In May 2025, the study received additional funding to extend the program through the second term of the 2025 academic year. Treatment participants were informed in June that they would receive four additional monthly payments of ZMW 1,000 between July and October 2025, at which point the program would end. In total, treatment participants received eight monthly payments over the course of the 2025 academic year, totaling ZMW 8,000 (approximately $300).

The transfer was unconditional. Participants were not required to maintain enrollment, complete coursework, attend campus, demonstrate any specific behavior, or report on how they spent the money in order to remain eligible. The survey was incentivized separately, and both treatment and control participants were asked to complete it on the same schedule and for the same compensation.

3.4 Balance and Sample Characteristics

Table 1 shows the sample baseline characteristics, separately for treatment and control. Baseline characteristics are generally well balanced across groups, with a small number of statistically significant differences for more granular outcomes (e.g., moderate household poverty and avoidance of healthcare), which is consistent with chance variation. The average age of participants is between 20 and 21, and almost none are married (1%) or have children (2%) at baseline.

Students report a high level of financial vulnerability. Nearly half of the students have taken out a loan in the past 4 weeks, 40% have pawned or sold belongings, and 63% are behind on housing costs.[4] Loans in this context are almost all short-term via mobile money, moneylenders, or informal sources, with extremely high interest rates (median *monthly* interest rates range from 7% to 20% , and 96% of loans are due within two months). There are also high levels of food insecurity: only 67% of participants are consistently eating two meals a day, and 26% did not eat any meat in the past two weeks.[5] Many students do not come from particularly elite backgrounds; on average, students' fathers have almost completed secondary school but

[4]The relative deprivation of female students at UNZA is discussed at more length in Bau et al. (2026), who characterize a large share of students as resembling poor rural-to-urban migrants.

[5]Vegetarianism by choice is *extremely* rare in Zambia.

have fewer years of schooling than required for tertiary education. Approximately a quarter of students grew up in a traditional mud home (commonly used as a proxy for lower-income households in this context).

Students are also relatively sexually active. About half of participants report ever having had sex at baseline, and 13% have had sex in the previous two weeks. Of those encounters, 42% were condomless and 25% involved partners that had given the participant a large gift (greater than 1,800 kwacha or ∼$100). Another 45% had characteristics of financially motivated sex (participant reports the partner is a "blesser," the primary reason for the relationship according to the participant is financial support, or the partner gives gifts above the 90th percentile of the distribution). These patterns are consistent with a setting in which students face substantial financial constraints and may engage in risky coping behaviors, including relationships involving high financial transfers and low levels of condom use.

Table 1: Balance Table

	Control Mean	Treatment Mean	Difference	P-value
Stratification Variables				
Sex Ever at Baseline	0.49	0.49	0.00	0.98
Sex in the Last 2 Weeks	0.13	0.13	0.00	0.86
Skipped Meals in the Last 2 Weeks	0.77	0.77	0.00	0.88
Pawned or Sold Items in the Last 4 Weeks	0.40	0.41	0.00	0.92
Demographics				
Age at Baseline	20.45	20.52	0.07	0.49
Married at Baseline	0.01	0.01	0.00	0.48
UNZA Year at Baseline	1.82	1.81	-0.01	0.80
Number of Children at Baseline	0.02	0.02	0.01	0.49
Number of Siblings	3.69	3.58	-0.11	0.25
Had Puberty Initiation	0.12	0.14	0.03	0.19
Traditional Home	0.25	0.25	0.01	0.79
HH Very Poor	0.14	0.11	-0.02	0.28
HH Moderately Poor	0.59	0.66	0.07**	0.02
Father's Years of Education	11.56	11.35	-0.21	0.49
Mother's Years of Education	10.38	10.26	-0.12	0.68
Economic Outcomes				
Government Bursary	0.80	0.79	0.00	0.86
In the Last 2 Weeks:				
Had At Least 2 Meals/Day	0.67	0.70	0.03	0.28
Avoided Eating Meat	0.26	0.22	-0.04	0.11
In the Last 4 Weeks:				
Took Out a Loan	0.49	0.52	0.03	0.40
Avoided Healthcare	0.35	0.41	0.07**	0.02
Behind on Housing Payments	0.63	0.65	0.01	0.65
Did Something You Were Not Proud of	0.20	0.21	0.01	0.71
Sexual Behavior				
Condomless Sex in the Past Two Weeks	0.06	0.05	-0.01	0.57
Financially Motivated Sex in the Past Two Weeks	0.06	0.07	0.01	0.69
Conditional on Sex in the Past Two Weeks: (N = 132)				
Condomless Sex	0.42	0.37	-0.05	0.57
Financially Motivated Sex	0.46	0.52	0.06	0.49
Ever Received Large Gift from Partner	0.25	0.34	0.08	0.29
Over 10 Year Age Gap with Partner	0.06	0.03	-0.03	0.43

Joint F-test = 0.89, p-value = 0.620

Notes: This table reports sample baseline characteristics for the control and treatment groups and the difference between groups. Household poverty indicators are self-reported. A "large gift" is defined as a gift valued at 1,800 ZMW (∼$100) or more ever given by the partner (75th percentile). See Table 3 for complete description of "financially motivated" sex. The joint F-test reports test of the joint predictiveness of all the characteristics for treatment (excluding the variables that are conditional on sex).

3.5 Empirical Strategy

The panel structure of our data, combined with the fact that different outcomes are measured at different frequencies, requires three distinct estimating equations.

Outcomes measured once or cumulatively. For outcomes measured only once or constructed cumulatively across survey rounds (e.g., whether a participant ever had sex), we estimate:

$$Y_{is} = \alpha + \beta_1 \tau_i + \delta_s + \epsilon_{is} \tag{1}$$

where Y_{is} is the outcome for participant i in randomization stratum s, τ_i is an indicator for assignment to treatment, and δ_s denotes stratum fixed effects. The coefficient of interest, β_1, captures the effect of the program.

Outcomes measured repeatedly. For outcomes measured in every survey or every other survey (e.g., sexual activity, consumption, food security), we exploit the panel structure and estimate:

$$Y_{iw} = \alpha + \theta_1 T_{iw} + \lambda_i + \gamma_w + \epsilon_{iw}, \tag{2}$$

where Y_{iw} is the outcome for participant i in survey wave w. The indicator T_{iw} equals one if participant i is in the treatment group *and* survey w falls within the period in which transfers are being disbursed. For our main regressions, we restrict to the periods before and during the intervention. Later, when we consider the post-treatment effects of the intervention, we extend the sample and include an indicator variable equal to 1 during the post-treatment period. We also include individual fixed effects λ_i and survey-wave fixed effects γ_w and cluster standard errors at the participant level.

Outcomes conditional on a sexual encounter. For outcomes defined only when a sexual encounter has occurred (e.g., condom use, partner characteristics), an observation is at the participant–survey–partner level: a participant reporting multiple partners in a given survey contributes multiple observations. We estimate:

$$Y_{iwp} = \alpha + \phi_1 T_{iw} + \phi_2 \tau_i + \gamma_w + \delta_s + \epsilon_{iwp} \tag{3}$$

where Y_{iwp} is the outcome for participant i in survey w with partner p. The variables T_{iw} and γ_w are defined as above. We replace individual fixed effects with a treatment-group indicator τ_i because sexual encounters in any given biweekly period are relatively infrequent in our sample. With individual fixed effects, identification would be restricted to participants who reported a sexual encounter during one of the four baseline surveys, which discards a substantial share of the relevant variation. Because treatment is randomly assigned, this will not change the interpretation of the coefficients. Standard errors are again clustered at the participant level. We note that encounter-level effects will be a combination of effects that occur conditional on a given encounter and changes in the types of encounters that occur.

4 Experimental Results

4.1 Food Security, Consumption, and Financial Well-Being

Given that this is a substantial cash grant to students, a natural first question is whether it increased consumption, and if so, what the money was spent on. Table 2 column (1) reports the effect of treatment on overall reported consumption in the past 2 weeks during the period of the intervention. They also report ZMW 377 in additional consumption per two-week period, a 35% increase over the control mean of ZMW 1082.[6] Figure 1a disaggregates the consumption response by category. Treatment effects are weakly positive across all categories, but only spending on groceries, housing, personal care, and gifts are statistically significant. The grocery (ZMW 83.3) and housing (ZMW 73.4) effects are the largest in magnitude and account for most of the total consumption increase, consistent with the gains in food security and financial well-being documented in Table 2.

Table 2: Effect of Financial Assistance on Food Security, Financial Well-Being, and Consumption

	Consumption (1)	Food Security (2)	Financial Well-Being (3)
Treated	376.786***	0.291***	0.279***
	(116.889)	(0.040)	(0.041)
Participant FE	Y	Y	Y
Survey FE	Y	Y	Y
Control Mean	1082.473	-0.138	-0.061
N	9930	11138	11140

Notes: This table reports estimates of equation 2, where the outcomes are food security, financial well-being, and consumption in the two weeks preceding a given survey. Fixed effects are included for the participant and survey. The food security index is created from students' answers to three questions: (1) whether they eat meat never, sometimes, or often, (2) whether they had eaten at least two meals every day for the past two weeks, and (3) whether they skipped meals because they could not afford to buy food never, sometimes, or often (entered inversely). The financial well-being index is constructed from binary indicators for whether respondents (1) have pawned or sold items, (2) taken out a loan, (3) avoided healthcare, (4) done something they were not proud of to obtain funds, and (5) are behind on housing payments. The index is inverted so that higher values reflect greater financial well-being. "Treated" equals one for participants in the treatment group during the period when transfers are being disbursed. The difference in sample size across columns is because consumption was asked on 1 less survey than food security. Standard errors are clustered at the participant level.

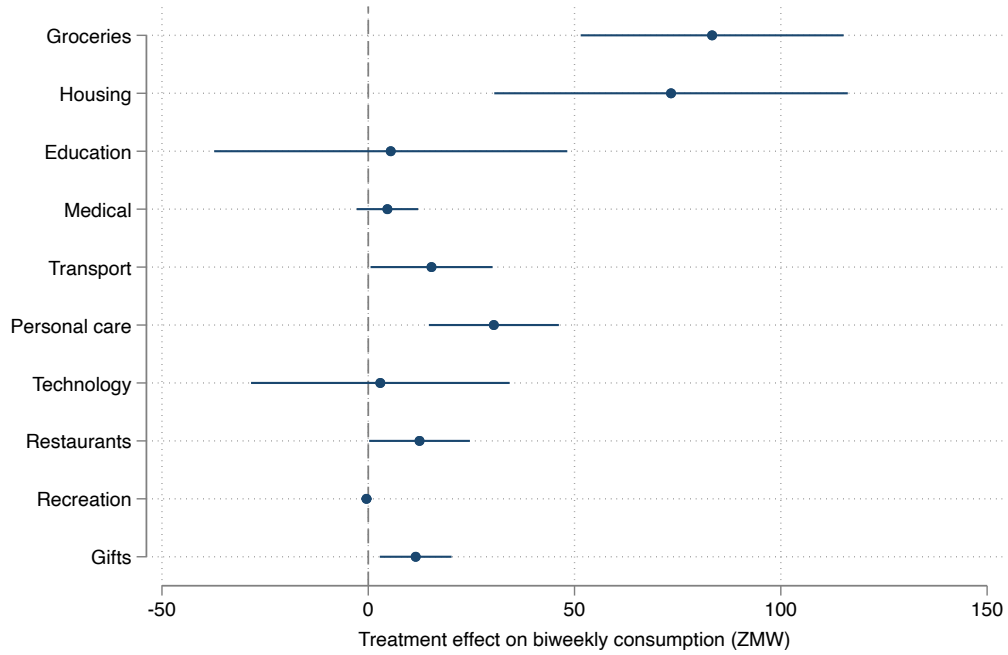
Table 2 columns (2) and (3) report estimates of equation (2) for indices of food security and financial well-being. During the treatment period, treated participants score 0.29 standard deviations higher on the food security index and 0.28 standard deviations higher on the financial well-being index, both relative to the control mean. Figure 1b disaggregates both indices into their components. All 8 index components are significantly affected by treatment: eating meat, skipping meals, and eating less than two meals a day (for food security), and avoiding healthcare, pawning and selling belongings, taking out loans, being behind on housing payments, and doing something you are "not proud of" for money (for financial well-being).

These results are consistent with the significant financial need suggested by the summary stats in Table 1. Similar to many cash transfers in settings with very poor households, the money is spent on immediate consumption support for subsistence goods – food and housing – and to avoid having to sell belongings or take out high-interest short-term loans.

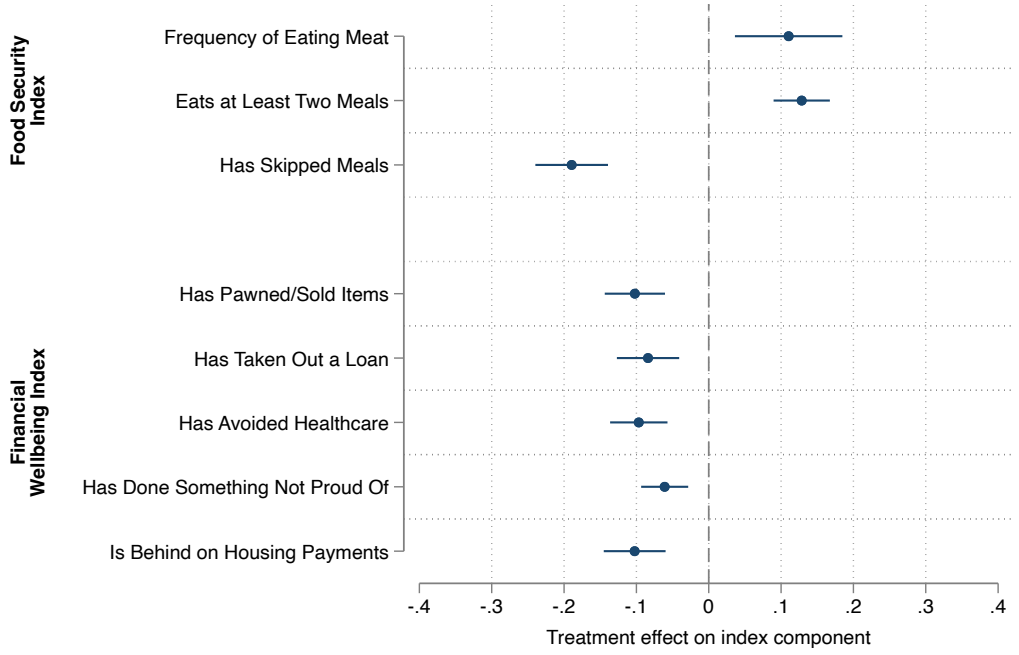
[6]The transfer is ZMW 1,000 per month, or ZMW 500 every two weeks, so the consumption response is somewhat smaller than the size of the transfer. Some of this difference may be due to the crowd out of alternative transfers (e.g., from sexual partners). The remainder may also be saved, lent, or invested in informal businesses.

Figure 1: Treatment Effects on Consumption, Food Security, and Financial Well-Being

(a) Consumption Categories



(b) Index Components



Notes: Panel A reports treatment effects on biweekly consumption (ZMW) by category. Panel B reports treatment effects on the components of the Food Security and Financial Well-Being indices. "Frequency of Eating Meat" ranges from 1 (never) to 5 (almost every day. "Eats at Least Two Meals" is a binary indicator variable for whether a student has eaten at least two meals every day. "Has Skipped meals" ranges from 1 (never) to 3 (often). The outcome variables in the financial security index are all binary indicators. All estimates use the specification outlined in equation 2, focusing on the effects of being in the treatment group during the treatment period (i.e. the "Treated" coefficient), and include individual and survey-wave fixed effects, with standard errors clustered at the individual level. Horizontal lines (whiskers) denote 95% confidence intervals.

4.2 Sex: Extensive Margin

We now turn to the effects of financial assistance on sexual behavior. Table 3 reports the effect of treatment on whether a participant *ever* reports a particular outcome on any biweekly survey conducted during the treatment period (late February through September 2025). Treated participants are 5.1 percentage points less likely to ever report having sex over this period, a 10.7% reduction relative to the control mean. The likelihood of ever reporting unprotected sex falls by 2.9 percentage points, a comparable 9.8% reduction, though the effect is not significant. The decrease in condomless sex could be due to the overall decrease in sex, if those that abstained would have had condomless sex, a separate switch to protected from unprotected sex among those having sex, or a combination of both.

In the bottom panel of the table, we separately estimate effects for participants who were and were not sexually active at baseline (a key stratification variable). The average effects in the top panel are driven almost entirely by participants who, at baseline, reported never having had sex. In the control group, 27% of these women report sex on at least one biweekly survey over the treatment period. In the treatment group, this share is 11.5 percentage points lower, a 43.0% reduction. The likelihood of ever reporting unprotected sex falls by 4.7 percentage points, and the likelihood of ever reporting "financially motivated" sex (i.e. with a partner who has given gifts at or above the 90th percentile, who has been called a "blesser", or who provides "financial support") falls by 11.3 percentage points, a 62.4% reduction. Likelihood of doing something the participant is not proud of for money falls by 18 ppt (43%). By contrast, among participants who had already had sex at baseline, only the likelihood of doing something they are not proud of for money falls (by 10 ppt). The reduction in financially motivated partnerships overall (column 3) is therefore concentrated entirely among women who would otherwise have entered sexual activity for the first time during the study period.

Figure 2 further shows that financial assistance significantly slows the rate of becoming sexually active. The figure plots the cumulative share of participants who have reported sex on at least one survey by each survey date, separately by treatment arm. The treatment and control track each other throughout the pre-intervention period (September 2024 – January 2025). Both groups begin the study with approximately 52.2% sexually active participants. In February 2025, when treatment was announced and the first transfers were disbursed, the curves diverge. Control participants become sexually active at a substantially higher rate than treated participants, and the gap widens through the treatment period. By March 2026, 65% of the control group has reported sex on at least one survey, compared with 58.5% of the treatment group.

4.3 Sex: Biweekly Data

The extensive-margin results in the previous section ask whether participants *ever* engage in a given behavior over the treatment period. We now use the panel structure of the data to ask how often these behaviors occur. Table 4 reports estimates of equation (2), where the outcome is an indicator for whether the behavior occurred in the two weeks preceding a given survey. For the pooled sample, there is an insignificant 29% reduction in having sex in any given week, a sizable and significant 2 ppt (33%) decline in ever having sex, and a 6.1 ppt (34%) reduction in participants doing something they are "not proud of" for money.

Among participants who reported never having had sex at baseline, treatment reduces the probability of reporting any sex in a given two-week period by 3.2 percentage points, a 68% reduction relative to the control mean. The likelihood of reporting unprotected sex falls by 1.7 percentage points, almost totally eliminating unprotected sex, financially motivated sex in a given survey falls by 2 ppt or two-thirds, and

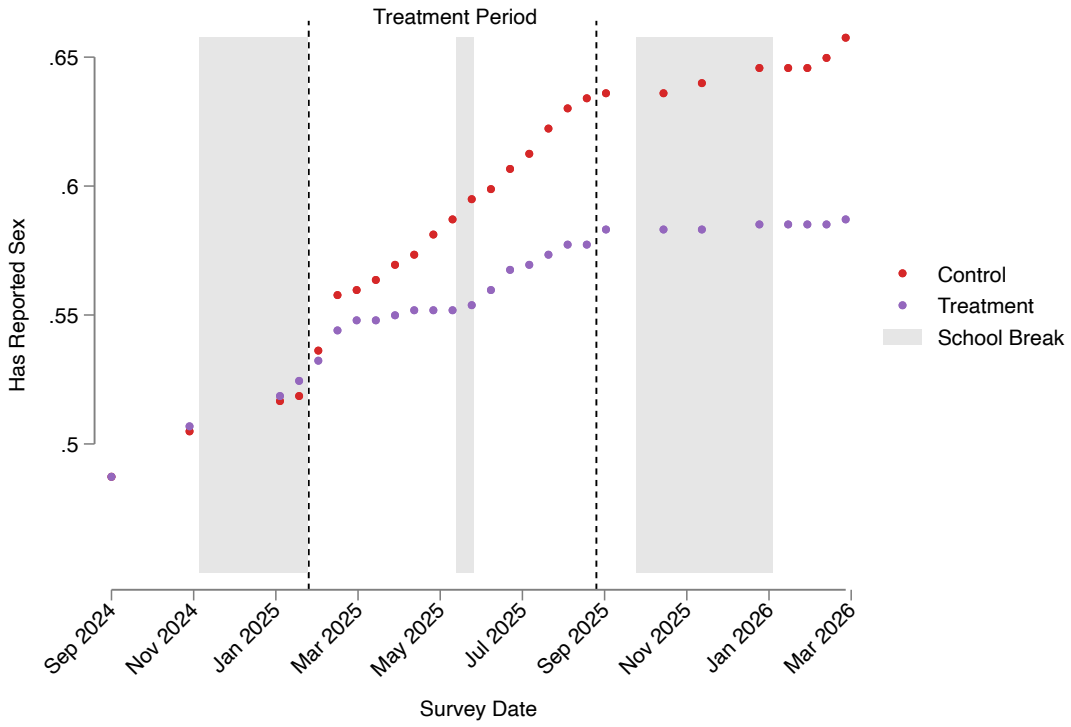
Table 3: Effect of Financial Assistance on Ever Having Sex

	All Respondents			
Since Start of Treatment:	Ever Have Sex (1)	Ever Condom-less (2)	Ever Financially Motivated (3)	Ever Not Proud (4)
Treatment	-0.05*	-0.03	-0.06**	-0.14***
	(0.03)	(0.02)	(0.03)	(0.03)
Stratum FE	Y	Y	Y	Y
Control Mean	0.48	0.29	0.32	0.47
N	1020	1020	1020	1020

	Never Sex at Baseline				Sex at Baseline			
Since Start of Treatment:	Ever Have Sex (1)	Ever Condom-less (2)	Ever Financially Motivated (3)	Ever Not Proud (4)	Ever Have Sex (5)	Ever Condom-less (6)	Ever Financially Motivated (7)	Ever Not Proud (8)
Treatment	-0.11***	-0.05*	-0.11***	-0.18***	0.02	0.00	-0.01	-0.10**
	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Stratum FE	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	0.27	0.12	0.18	0.42	0.70	0.47	0.47	0.53
N	519	519	519	519	498	498	498	498

Notes: The left panel reports estimates from regressions of an indicator for ever having sex, ever having unprotected sex, or ever having a risky partner since the start of treatment (Survey 5 onward) on treatment, for all participants. The center panel restricts to participants who reported never having had sex at baseline, and the right panel restricts to those who reported having had sex at baseline. All panels follow the specification in equation 1 and include stratum fixed effects. "Financially motivated" sex is defined as an encounter with a partner whom the participant reports is a "blesser," for whom the participant lists "He provides financial support" as a primary reason for the relationship, whose gifts of school fees, accommodation fees, medical fees, or cash are valued at or above the 90th percentile of the value distribution for that gift category (either for a single gift or on average over the course of the survey), or whose total gifts across all categories are valued at or above the 90th percentile of the total-gift distribution, either for a single gift or on average over the course of the survey. "Condomless sex" is sex without the use of a condom.

Figure 2: Cumulative Share Ever Reporting Sex, by Treatment Arm



Notes: This figure plots the cumulative share of participants who have reported having had sex on at least one biweekly survey, separately for treatment and control. Shaded regions indicate periods when students are away from campus on school break. The vertical lines mark the beginning and end of the intervention period.

the likelihood of reporting having done something they were not proud of for money falls by 8 ppt (53%). All these effects are highly significant. Among participants who were already sexually active at baseline, treatment has no significant effect on any of these outcomes; the point estimate on any sex is, if anything, positive, though not significant. This pattern is consistent with financial assistance affecting engaging in sex primarily by delaying entry into sexual activity rather than by reducing the frequency of sex among those who are already active.

Table 4: Effect of Financial Assistance on Biweekly Sexual Behavior

	All Respondents							
	Any Sex (1)	Condom-less Sex (2)	Financially Motivated (3)	Not Proud (4)				
Treated	-0.01	-0.02**	-0.01	-0.06***				
	(0.01)	(0.01)	(0.01)	(0.02)				
Participant FE	Y	Y	Y	Y				
Survey FE	Y	Y	Y	Y				
Control Mean	0.13	0.06	0.07	0.18				
N	19229	19189	19193	11135				
	Never Sex at Baseline				Sex at Baseline			
	Any Sex (1)	Condom-less Sex (2)	Financially Motivated (3)	Not Proud (4)	Any Sex (5)	Condom-less Sex (6)	Financially Motivated (7)	Not Proud (8)
Treated	-0.03***	-0.02***	-0.02***	-0.08***	0.02	-0.02	0.01	-0.04
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Participant FE	Y	Y	Y	Y	Y	Y	Y	Y
Survey FE	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	0.05	0.02	0.03	0.15	0.21	0.11	0.11	0.21
N	9771	9757	9758	5653	9404	9379	9381	5451

Notes: This table reports estimates of equation 2, where the outcome is an indicator for whether the behavior occurred in the two weeks preceding a given survey. "Treated" equals one for participants in the treatment group during the period when transfers are being disbursed. The top panel includes all participants; the bottom left panel restricts to those who reported never having had sex at baseline; the bottom right panel restricts to those who reported having had sex at baseline. All estimates include participant and survey fixed effects. "Condomless sex" is an indicator variable for sex without a condom. Standard errors are clustered at the participant level.

4.4 Sex: Encounter-Level Outcomes

We now examine how treatment affects the characteristics of the sexual encounters that do occur. Because these outcomes are defined only conditional on a sexual encounter taking place, treatment effects are driven both by the type of encounters that still occur and changes in behavior conditional on encounter type. Our estimates can be thought of as capturing the average characteristics of encounters that occur in the treatment vs. control group.

Table 5 reports estimates of equation (3), with four outcomes: an indicator for whether a condom was used in the encounter, an indicator for whether the partner has ever given a "large gift," an index of partner quality constructed from a comprehensive set of partner characteristics (see Appendix B for the list of components), and a 4-level measure of how frequently the participant felt safe with the partner (ranging

from never to always). Treated participants are 13.0 percentage points more likely to use a condom in a given encounter, a 25.1% increase over the control mean, and 8.2 percentage points less likely to report an encounter with a partner who has ever given a large gift, a 28.1% reduction. The point estimate for partner quality is positive but not statistically significant.

As before, the pooled results mask important heterogeneity. Among wome who were not sexually active at baseline, treatment increases condom use by 25.1 percentage points (a 39.6% increase) and increases the partner quality index by 0.94, significant at the 5% level. The reduction in encounters with large-gift partners (-16.4 pp) is larger in magnitude to the overall effect but not statistically significant in this smaller subsample. Among participants who were already sexually active at baseline, treatment increases condom use by 11.8 percentage points and reduces large-gift partnerships by 8.1 percentage points. The point estimate for partner quality for this subgroup is small and insignificant.

Among the women who had not had sex at baseline, the overall pattern—higher condom use, fewer large transfers, and higher partner quality—is consistent with treatment shifting women away from partners who provide financial support and demand condomless sex. For women who were sexually active at baseline, even though partner quality does not change, condom use increases substantially and large gift receipt falls. In contrast to the women who were not sexually active at baseline, the treatment effect for this group seems more likely to be driven by a change in behavior with the same partners.

4.5 Psychosocial and Cognitive Outcomes

The biweekly survey also collected a range of measures on participants' mental health and cognitive functioning. Mental health questions were asked every four weeks; the cognitive measures were administered approximately once per semester. Table 6 reports estimates of equation (2) for these outcomes.

The probability of reporting depression—measured as scoring above the standard cutoff on the depression subscale of the PHQ-4 (Kroenke et al., 2009), which has been validated for use among adolescent girls and young women in sub-Saharan Africa (Materu et al., 2020)—falls by 7.1 percentage points (14%). The financial worry measure falls by 0.19 (∼0.2 sd), and both effects are highly significant. The treatment has directionally negative but insignificant effects on the probability of reporting anxiety on the PHQ-4 and the likelihood of reporting worrying about the future.

We next turn to measures of cognitive skills, executive function, and cognitive control. Financial stress is known to decrease these measures (Mani et al., 2013), potentially inhibiting decision-making. Performance on Raven's Progressive Matrices, our measure of fluid reasoning, increases by 4.7 percentage points or 0.18 sd (see Figure A1 for example questions for this measure). Performance on the Hearts and Flowers task (Davidson et al., 2006), our measure of cognitive control, increases by 5.0 percentage points or 0.21 sd. Both effects are highly significant.[7]

Our results are consistent with a growing literature documenting that economic deprivation imposes a cognitive burden that is at least partially relieved when financial pressure is reduced (Mani et al., 2013; Schilbach et al., 2016). They also suggest that the consumption and food security gains documented in Table 2 are accompanied by improvements in mental functioning that may amplify the welfare gains from the transfer.

[7]The Hearts and Flowers task is a behavioral measure of executive function in which either a heart or a flower flash on the screen and participants must respond on the same side as the stimulus when shown a heart, and on the opposite side when shown a flower. Performance is scored as the share of trials in which the participant responded correctly. See Figure A2 for example questions.

Table 5: Effect of Financial Assistance on Encounter-Level Outcomes

	All Respondents			
	Used Condom (1)	Large Gift (2)	Partner Quality (3)	Felt Safe (4)
Treated	0.13***	-0.08*	0.07	0.08
	(0.05)	(0.04)	(0.13)	(0.09)
Treatment FE	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y
Survey FE	Y	Y	Y	Y
Control Mean	0.52	0.29	0.00	3.18
Control SD	0.50	0.45	0.96	0.91
N	2543	2549	1688	2544

	Never Sex at Baseline				Sex at Baseline			
	Used Condom (1)	Large Gift (2)	Partner Quality (3)	Felt Safe (4)	Used Condom (5)	Large Gift (6)	Partner Quality (7)	Felt Safe (8)
Treated	0.25*	-0.16	0.94**	0.70***	0.12**	-0.08*	-0.02	0.03
	(0.14)	(0.15)	(0.45)	(0.26)	(0.05)	(0.05)	(0.14)	(0.10)
Treatment FE	Y	Y	Y	Y	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y	Y	Y	Y	Y
Survey FE	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	0.63	0.28	-0.16	3.06	0.49	0.29	0.04	3.20
Control SD	0.48	0.45	1.03	0.95	0.50	0.46	0.93	0.89
N	370	371	259	370	2173	2177	1429	2174

Notes: This table reports estimates from equation (3), conditional on a sexual encounter occurring in the two-week reference period. "Used Condom" is an indicator for whether a condom was used in the encounter. "Large Gift" is an indicator for whether the partner has ever given a single gift greater than ZMW 1,800 (the 75th percentile of all partner gifts in the sample); this is asked at the first encounter with a given partner. "Partner Quality" is an index constructed from partner characteristics; see Appendix B for details. The top panel includes all encounters; the bottom left panel restricts to participants who reported never having had sex at baseline; the bottom right panel restricts to those who reported having had sex at baseline. All estimates include treatment status and survey fixed effects. Standard errors are clustered at the participant level.

4.6 Academic Outcomes

In Table 7, we draw on administrative data from UNZA to estimate the effects of the treatment on academic outcomes. Since UNZA was not able to share grades, we examine effects on graduating (an outcome most of the sample was not yet eligible for), an indicator for passing all courses during the intervention year or graduating,[8] and an indicator for dropping out (which we infer if a student does not re-register in the next academic year). Across outcomes, the point estimates are very close to zero. We caveat these null effects with the observation that these outcomes are relatively coarse – one would expect graduation and dropout to be difficult to move – and it may be too early to observe effects. To the extent the treatment did not improve academic outcomes, it suggests that participants are able to use a combination of transactional sex, high interest loans, reduced food intake, and support from family and friends to pursue high returns human capital investments during this very financially constrained period.

[8] At the end of an academic year, students are assigned a status of having passed all courses, having to retake some courses, or having to retake the year. Majors at UNZA differ from colleges in the U.S., with students in a given major taking the same set of required courses each year.

Table 6: Effect of Financial Assistance on Mental Health and Cognition

	Anxious	Depressed	Financial Worry	Future Worry	Visual Reasoning Score	Cognitive Control Score
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.034	-0.071***	-0.190***	-0.063	0.047***	0.050***
	(0.024)	(0.024)	(0.037)	(0.039)	(0.018)	(0.015)
Participant FE	Y	Y	Y	Y	Y	Y
Survey FE	Y	Y	Y	Y	Y	Y
Control Mean	0.567	0.520	2.751	2.765	0.765	0.833
Control SD			0.973	0.979	0.260	0.233
N	10087	10080	11132	11132	3040	3036

Notes: This table reports estimates of equation (2) with participant and survey fixed effects. "Anxious" and "Depressed" are indicators for scoring above the standard cutoff on the anxiety and depression subscales of the PHQ-4 (Kroenke et al., 2009). "Financial Worry" and "Future Worry" are asked on a scale of 1 to 4 (1 being "not at all" and 4 being "very much" worried) capturing self-reported worry about current financial pressures and long-term prospects, respectively. "Visual Reasoning" is the share of Raven's Progressive Matrices items answered correctly. "Cognitive Control" is the share of trials answered correctly on a Hearts and Flowers task (Davidson et al., 2006; Mani et al., 2013). The first four outcomes were measured every four weeks; Raven's Matrices and Hearts and Flowers were administered approximately once per semester. Post-treatment estimates can be found in Table **??**. Standard errors are clustered at the participant level.

Table 7: Effect of Financial Assistance on Academic Outcomes

	Graduated	Passed or Graduated	Dropped Out
	(1)	(2)	(3)
Treatment	0.001	-0.007	0.001
	(0.023)	(0.031)	(0.010)
Stratum FE	Y	Y	Y
Control Mean	0.156	0.604	0.028
N	1018	1018	1018

Notes: This table reports estimates of equation (1). Data on these outcomes are drawn from UNZA administrative data at the end of the academic year. Standard errors are heteroskedasticity robust.

5 Robustness

5.1 Attrition and Bounding

True attrition in this study was very low: of 1,022 participants, only 5 formally withdrew (4 in the control group and 1 in the treatment group), and 4 additional participants stopped responding to the survey during the treatment period (all in the control group). However, there was some non-response to individual surveys. The response rate over time is shown in Figure A3. Response rates are consistently high in both groups, although the non-response rate is greater in the control group. Non-response increases in both groups after the treatment period ends, coinciding with the exam period and subsequent school break, which is also consistent with reduced internet access as respondents leave campus.

Because non-response differs slightly between treatment and control groups over time, in Appendix Table A1, we report Lee (2009) bounds for the biweekly sexual behavior outcomes in Table 4. To generalize the approach to our panel data set, we apply the same trimming procedure as in Tauchmann (2014) separately for each survey round, using the observed difference in response rates between treatment and control groups within that round. The resulting upper and lower bounds are then re-estimated using the same regression

specification as in Table 4. The Lee bounds closely mirror the baseline estimates. Given the low rate of attrition, the bounds are tight and all estimates that were significant in the conventional regressions remain so for both bounds.

For the indicator outcomes measuring whether respondents ever had sex, had condomless sex, or had sex with a risky partner, differential non-response is less of a concern because these variables are defined as the maximum across all observed survey waves for each participant. Because we take a max over the 15 surveys asked during the intervention period for these measures, higher non-response in the control group would tend to lead us to overstate abstinence in this group, attenuating our convenient point estimates. Given the low non-response rates, even in the control group, it is likely this type of error is small, but we can think of our estimates in Table 3 as a lower bound of the effects on abstinence over the period.

5.2 Attention

Because the surveys are self-administered, there is a concern that participants clicked through the survey to answer questions as quickly as possible, without actually answering questions honestly. In particular, our survey was designed so that it first asked whether a participant had sex in the past two weeks, and if so, asks a series of follow-up questions. Students may under report having sex to complete the survey more quickly.

In order for this to drive our results, it would have to be the case that treatment participants (and particularly those who had not reported sex at baseline) were more likely to click through responses quickly. To test for this, in Appendix Table A3, we recreate Table 4, dropping the participants in the bottom 10% of survey duration for each biweekly survey. The results are reassuring. If anything, the coefficients are slightly larger and more significant than in Table 4.

6 Post-Treatment

In the penultimate section, we consider what happened after the intervention concluded. Appendix Table A2 reports the effects of the treatment on consumption and other measures of financial well-being during and after the treatment. Consumption appears to quickly revert to control levels. There is no evidence participants saved the transfers to consume later. Appendix Tables A4 and A5 re-estimate survey and encounter-level effects including the post-treatment periods. In the pooled sample, there is little evidence the effects persist for either the survey-level or encounter-level effects. However, for women who were not sexually active at baseline, reduced risky and financially motivated sex and greater partner quality conditional on encounters do persist through our 6 month post-treatment period.

7 Conclusion

Decisions about sexual activity in adolescence and young adulthood can be extremely consequential. This is especially true in sub-Saharan Africa, where the prevalence of HIV is high and unintended pregnancies are a primary driver of teen motherhood and marriage. While the qualitative and public health literatures have long described "transactional sex" relationships in this context, it is difficult to isolate the extent to which sexual behavior is *driven* by financial need. That is, it is unclear to what extent risky sexual relationships are a form of consumption vs. labor.

In this paper, we combine experimental variation in income with detailed high-frequency data collection on sex, condom use, partners, and gifts to answer this question. We find that, even in a population that is

positively selected in terms of education, sexual activity is highly income-elastic. Women, particularly those who are not initially sexually active, sharply reduce sexual activity – by 41% – when financial resources become available. When these women do engage in sex, we find that they form relationships with much higher quality partners, emphasizing the important of liquidity for waiting for better matches. Moreover, while it has long been known that men pay a premium to sex workers for condomless sex (Gertler et al., 2005), we find that casual transactional relationships also appear to trade off condom use and gifts from partners.

Our results highlight that liquidity constraints, particularly for young women in a critical period for investment, may be a key driver of risky sexual behavior in a high HIV context. Consequently, interventions that provide information about HIV or attempt to change preferences, which are popular among policymakers and NGOs, may fail to reduce risky sexual behavior. In contrast, economic empowerment reduces financially-motivated risky sex during the particularly critical period of young adulthood, when young women are financially constrained and additional funds to invest in education have high returns. Financial support during this period can help women reap those returns without resorting to risky sex, permanently lowering their exposure to HIV and improving partner match.

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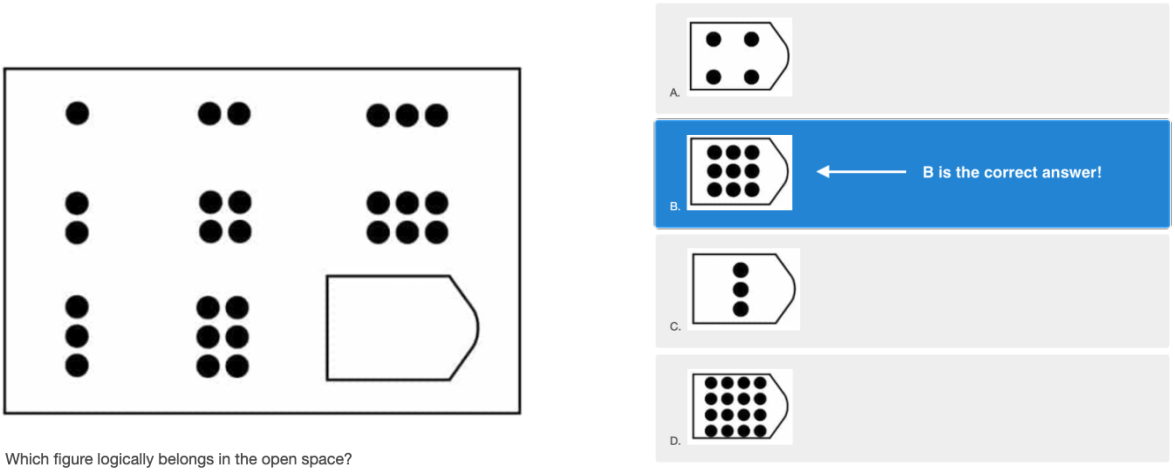
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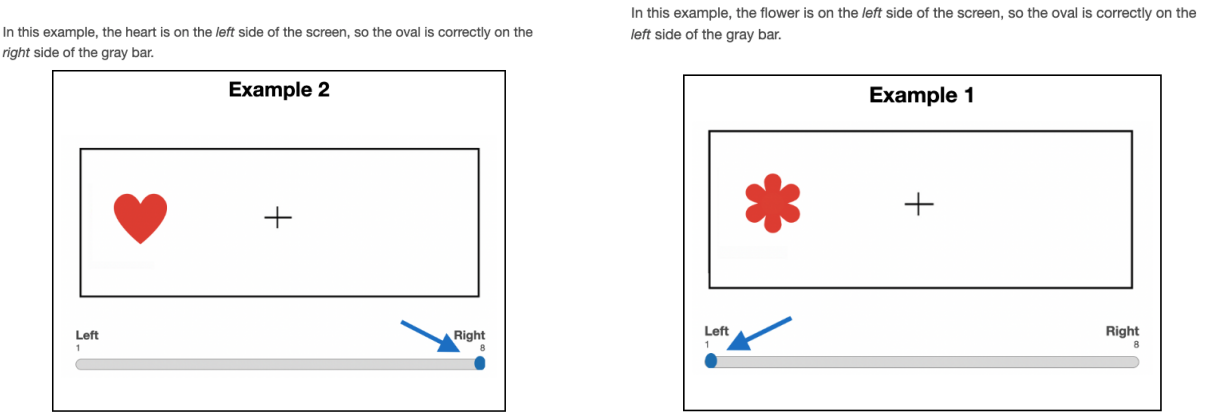
A Appendix Figures & Tables

Figure A1: Visual Reasoning Example



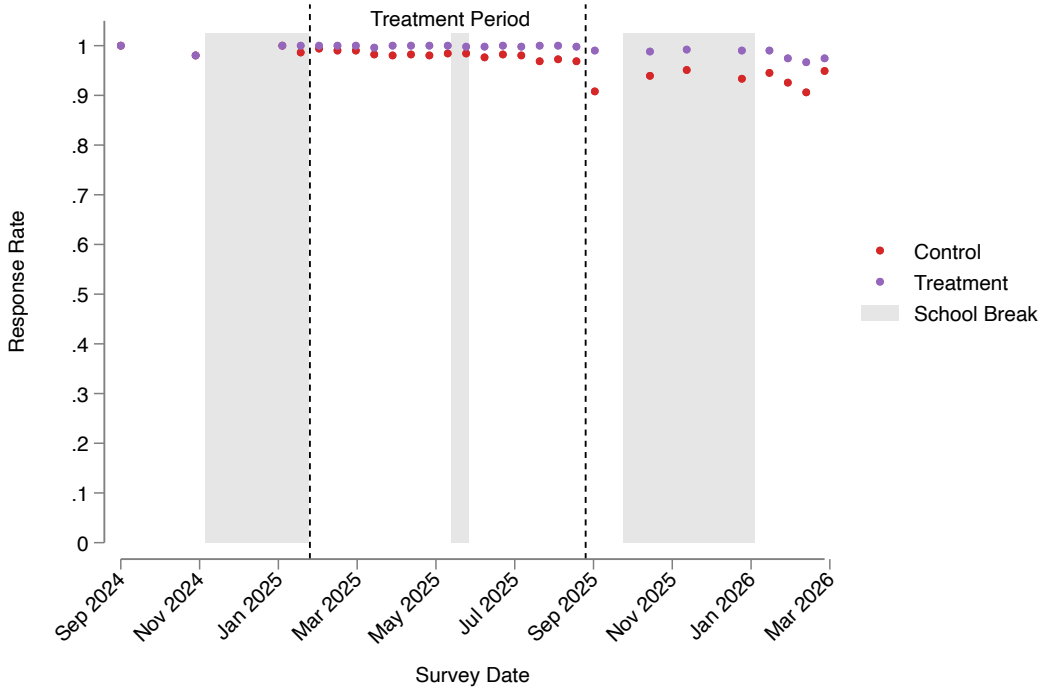
Notes: This is an example problem shown to participants to explain the Raven's Progressive Matrices questions, which were asked to measure fluid reasoning. Participants are asked to choose the most logical matrix, given the pattern laid out in the prompt.

Figure A2: Cognitive Control Example



Notes: These are the example questions given to participants to explain the Hearts and Flowers task used to measure cognitive control (Davidson et al., 2006). Participants are asked to click a certain side of the screen either aligning or opposite the heart or flower, depending on which appear on the screen.

Figure A3: Non-Response by Survey Round



Notes: This figure plots the response rate on each survey separately for treatment and control. Shaded regions indicate periods when students are away from campus on school break. The vertical lines denote the intervention period.

Table A1: Effect of Financial Assistance on Biweekly Sexual Behavior: Lee Bounds

Full Sample

	Any Sex			Condomless Sex			Not Proud		
	Baseline	Lower Bound	Upper Bound	Baseline	Lower Bound	Upper Bound	Baseline	Lower Bound	Upper Bound
Treated	-0.0073	-0.0157	-0.0058	-0.0183**	-0.0314***	-0.0178**	-0.0608***	-0.0683***	-0.0596***
	(0.012)	(0.012)	(0.012)	(0.009)	(0.008)	(0.009)	(0.017)	(0.017)	(0.017)
Post-Treated	-0.0070	-0.0397***	-0.0038	-0.0118	-0.0446***	-0.0099	-0.0266	-0.0527***	-0.0225
	(0.014)	(0.014)	(0.014)	(0.010)	(0.010)	(0.011)	(0.019)	(0.019)	(0.019)
Participant FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Survey FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	0.118	0.118	0.118	0.057	0.057	0.057	0.177	0.177	0.177
N	27051	26691	26691	26997	26631	26631	16044	15846	15846

Never Sex at Baseline

	Any Sex			Condomless Sex			Not Proud		
	Baseline	Lower Bound	Upper Bound	Baseline	Lower Bound	Upper Bound	Baseline	Lower Bound	Upper Bound
Treated	-0.0317***	-0.0414***	-0.0316***	-0.0172***	-0.0224***	-0.0168***	-0.0781***	-0.0844***	-0.0774***
	(0.010)	(0.010)	(0.010)	(0.006)	(0.005)	(0.006)	(0.021)	(0.021)	(0.021)
Post-Treated	-0.0317***	-0.0425***	-0.0310***	-0.0237***	-0.0272***	-0.0232***	-0.0511**	-0.0819***	-0.0467*
	(0.012)	(0.011)	(0.012)	(0.007)	(0.006)	(0.007)	(0.024)	(0.023)	(0.024)
Participant FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Survey FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	0.047	0.047	0.047	0.019	0.019	0.019	0.151	0.151	0.151
N	13727	13475	13475	13707	13448	13448	8137	7994	7994

Sex at Baseline

	Any Sex			Condomless Sex			Not Proud		
	Baseline	Lower Bound	Upper Bound	Baseline	Lower Bound	Upper Bound	Baseline	Lower Bound	Upper Bound
Treated	0.0179	0.0131	0.0190	-0.0199	-0.0279*	-0.0188	-0.0420	-0.0464*	-0.0409
	(0.022)	(0.022)	(0.022)	(0.017)	(0.017)	(0.017)	(0.026)	(0.026)	(0.026)
Post-Treated	0.0173	-0.0045	0.0226	-0.0003	-0.0243	0.0016	-0.0000	-0.0167	0.0052
	(0.024)	(0.024)	(0.025)	(0.020)	(0.019)	(0.020)	(0.030)	(0.030)	(0.030)
Participant FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Survey FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	0.191	0.191	0.191	0.096	0.096	0.096	0.203	0.203	0.203
N	13246	13134	13134	13213	13101	13101	7861	7800	7800

Notes: This table uses the same specification to estimate the effect of treatment on biweekly sexual behavior as Table 4 using Lee (2009) bounds to estimate a lower and upper bound of the effect. Baseline estimates (that are equivalent to what is shown in Table 4) are included for reference. We also include the "Post-treated" effect, which equals one for participants in the treatment group after transfers have ended (Survey 19 onward).

Table A2: Effect of Financial Assistance on Food Security, Financial Well-Being, and Consumption during Treatment and Post-Treatment

	Consumption (1)	Food Security (2)	Financial Well-Being (3)
Treated	371.822***	0.290***	0.277***
	(115.565)	(0.040)	(0.041)
Post-Treated	-18.406	0.009	-0.065
	(149.138)	(0.048)	(0.048)
Participant FE	Y	Y	Y
Survey FE	Y	Y	Y
Control Mean	1131.385	-0.092	-0.027
N	11845	16050	16053

Notes: This table reports the same estimates as Table 2 but includes the "Post-Treated" effect and the post-treatment period. "Post-Treated" equals one for participants in the treatment group after transfers have ended (Survey 19 onward). The post-treatment sample is included.

Table A3: Effect of Financial Assistance on Biweekly Sexual Behavior: Attention

	All Respondents			Never Sex at Baseline			Sex at Baseline		
	Any Sex (1)	Condomless Sex (2)	Not Proud (3)	Any Sex (4)	Condomless Sex (5)	Not Proud (6)	Any Sex (7)	Condomless Sex (8)	Not Proud (9)
Treated	-0.008	-0.020**	-0.067***	-0.036***	-0.018***	-0.083***	0.020	-0.021	-0.050*
	(0.013)	(0.009)	(0.017)	(0.011)	(0.006)	(0.023)	(0.023)	(0.018)	(0.026)
Post-treated	-0.006	-0.015	-0.026	-0.035***	-0.026***	-0.046*	0.023	-0.003	-0.005
	(0.015)	(0.011)	(0.020)	(0.013)	(0.007)	(0.025)	(0.026)	(0.022)	(0.032)
Participant FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Survey FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	0.131	0.065	0.177	0.051	0.022	0.151	0.213	0.109	0.205
N	23606	23555	13677	11917	11898	6878	11614	11583	6756

Notes: This table uses the same specification as Table 4 but drops respondents in the bottom 10% of survey duration for each biweekly survey. We also include the "Post-treated" effect, which equals one for participants in the treatment group after transfers have ended (Survey 19 onward). The post-treatment sample is included.

Table A4: Effect of Financial Assistance on Biweekly Sexual Behavior during Treatment and Post-Treatment

	All Respondents			
	Any Sex (1)	Condom-less Sex (2)	Financially Motivated (3)	Not Proud (4)
Treated	-0.01	-0.02**	-0.01	-0.06***
	(0.01)	(0.01)	(0.01)	(0.02)
Post-Treated	-0.01	-0.01	-0.01	-0.03
	(0.01)	(0.01)	(0.01)	(0.02)
Participant FE	Y	Y	Y	Y
Survey FE	Y	Y	Y	Y
Control Mean	0.12	0.06	0.08	0.18
N	27051	26997	27002	16044

	Never Sex at Baseline				Sex at Baseline			
	Any Sex (1)	Condom-less Sex (2)	Financially Motivated (3)	Not Proud (4)	Any Sex (5)	Condom-less Sex (6)	Financially Motivated (7)	Not Proud (8)
Treated	-0.03***	-0.02***	-0.02***	-0.08***	0.02	-0.02	0.01	-0.04
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Post-Treated	-0.03***	-0.02***	-0.03**	-0.05**	0.02	-0.00	0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Participant FE	Y	Y	Y	Y	Y	Y	Y	Y
Survey FE	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	0.05	0.02	0.03	0.15	0.19	0.10	0.12	0.20
N	13727	13707	13708	8137	13246	13213	13216	7861

Notes: This table reports the same estimates as Table 4 but includes the "Post-Treated" effect. "Post-Treated" equals one for participants in the treatment group after transfers have ended (Survey 19 onward). The post-treatment sample is included.

Table A5: Effect of Financial Assistance on Encounter-Level Outcomes during Treatment and Post-Treatment

	All Respondents			
	Used Condom (1)	Large Gift (2)	Partner Quality (3)	Felt Safe (4)
Treated	0.13**	-0.08*	0.07	0.08
	(0.05)	(0.04)	(0.13)	(0.09)
Post-Treated	0.08	-0.05	-0.18	-0.08
	(0.07)	(0.05)	(0.19)	(0.13)
Treatment FE	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y
Survey FE	Y	Y	Y	Y
Control Mean	0.52	0.28	-0.01	3.18
Control SD	0.50	0.45	0.99	0.91
N	3350	3357	2214	3352

	Never Sex at Baseline				Sex at Baseline			
	Used Condom (1)	Large Gift (2)	Partner Quality (3)	Felt Safe (4)	Used Condom (5)	Large Gift (6)	Partner Quality (7)	Felt Safe (8)
Treated	0.26*	-0.17	0.94**	0.69***	0.12**	-0.08*	-0.02	0.03
	(0.13)	(0.15)	(0.45)	(0.26)	(0.05)	(0.05)	(0.14)	(0.10)
Post-Treated	0.32**	-0.19	1.35**	0.68*	0.04	-0.04	-0.34*	-0.16
	(0.16)	(0.16)	(0.56)	(0.35)	(0.07)	(0.06)	(0.20)	(0.14)
Treatment FE	Y	Y	Y	Y	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y	Y	Y	Y	Y
Survey FE	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	0.59	0.28	-0.22	3.04	0.50	0.28	0.05	3.21
Control SD	0.49	0.45	1.11	0.95	0.50	0.45	0.95	0.90
N	512	513	352	512	2838	2843	1862	2840

Notes: This table reports the same estimates as Table 5 but includes the "Post-Treated" effect. "Post-Treated" equals one for participants in the treatment group after transfers have ended (Survey 19 onward). The post-treatment sample is included.

B Partner Quality Index

We construct a partner quality index using principal component analysis (PCA) based on a set of measures capturing partner characteristics and relationship dynamics. PCA constructs a weighted average of correlated variables that captures the largest possible variation in the data (Jolliffe, 2002).

The indicators include: partner type (indicators for boyfriend, "blesser," casual partner, or husband/fiancé), partner age (a binary indicator for whether the partner is over 35), perceived number of other partners, and whether the partner is believed to be married. We also include measures of relationship quality and stability, including likelihood of marrying the partner (1–4 scale from "not at all likely" to "very likely"), likelihood of continuing to see the partner romantically (on the same 1–4 scale), likelihood of considering this to be a serious partner (on the same 1–4 scale), likelihood of disclosing the relationship to friends (1–5 scale from "not at all likely" to "I already have"), likelihood of introducing the partner to parents (on the same 1–5 scale), and whether the respondent feels safe with the partner (1–4 scale from "never" to "always"). We additionally incorporate a set of indicators for perceived partner quality. We asked the respondent to check all of the following partner characteristics that apply: the partner has a good personality, is physically attractive, provides financial support, provides academic support, has a good sense of humor, is kind, is successful, and would make a good husband or father. Finally, we incorporate a measure of sexual negotiation within the relationship, proxied by whether the respondent would feel comfortable asking the partner to use a condom.

We use the first principal component as our baseline partner quality index.