# Sex and Subsistence: Experimental Evidence on Risky Sexual Behavior as Labor in Sub-Saharan Africa*

Natalie Bau† Corinne Low‡ Bryce Millett Steinberg§

September 11, 2026

**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, condom use, and partner choice are highly income-elastic. Treated participants are 5.1 percentage points less likely to have sex during the treatment period, and the rate of condomless sex falls by 30%. Among women who were not sexually active at baseline, the likelihood of having sex falls by 43%, 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 a 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 leave school 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 on what terms – can be especially consequential for young women. This is particularly true in sub-Saharan Africa (SSA), where HIV prevalence is high, contraceptive use is low, and unplanned pregnancies play an important role in derailing education. Despite the importance of early sexual behavior for schooling, work, marriage, and fertility, economists know relatively little about its determinants. A large literature shows that liquidity, such as that provided by unemployment insurance, allows liquidity-constrained workers to search longer rather than take the first available job (Card et al., 2007; Chetty,

*Bau, Low, and Steinberg gratefully acknowledge funding from NSF grant SES2343873, the J-PAL Gender and Economic Agency Initiative, IGC, and CEGA. Richa Mukerjee and Corinne Williams provided outstanding research assistance. IPA Zambia oversaw data collection for this study. We thank Jack Fanning for helpful comments. The study was approved by the National Bureau of Economic Research IRB and UNZABREC, the local Zambian IRB. The RCT and pre-analysis plan were pre-registered in the AEA RCT Registry AEARCTR-0014078.

†UCLA, NBER, & BREAD. Contact: nbau@ucla.edu

‡Wharton (UPenn) & NBER. Contact: corlow@wharton.upenn.edu

§Brown & NBER. Contact: bryce_steinberg@brown.edu

2008). There has been far less attention to whether financial resources play the same role for young women choosing partners, allowing them to delay becoming sexually active or to decline relationships they would otherwise enter out of financial need. In this paper, we measure the extent to which immediate financial need drives sexual behavior and partner choice, even among university students in Zambia, a population that is typically perceived as advantaged.

To do so, we combine a randomized controlled trial with a high-frequency panel survey among 1,022 female undergraduates at the University of Zambia. Treated students received approximately $40 per month for two semesters, roughly a month's rent.[1] Rather than measuring outcomes in a single retrospective endline survey, we follow participants for 18 months with a survey they complete on their own smartphones approximately every two weeks, before, during, and after the transfers. Self-administration reduces the social desirability bias that affects interviewer-administered surveys on sensitive topics (Mensch et al., 2003; Luke et al., 2011). The two-week recall window limits recall bias, which is severe for retrospective self-reports even of the most salient events: Duflo et al. (2024) find that, of Ghanaian parents who reported the death of a child in one survey round, nearly half answered no when asked the same question two years later. Response rates averaged over 98% across the 15 surveys sent during the intervention period, which we achieved through a combination of airtime incentives and frequent contact from survey staff.

These data allow us to observe women's sexual behavior and partnerships in far greater detail than existing studies. Each time a participant reports sex, she identifies the partner by a nickname she chooses, so that the same partner can be followed across surveys. The resulting data cover 932 distinct partnerships. The first time a partner appears, she reports his age, whether he is married, whether he is a student, what she likes about him (e.g., sense of humor, attractiveness, financial support), whether she sees him as a potential marriage partner, and whether she feels safe with him, among other characteristics. Every time he appears, she reports whether a condom was used and the gifts she received from him in the previous two weeks, by category (cash, meals, material goods, school fees). Tracking partners over time turns out to matter, because condom use is not a fixed attribute of a woman or even of a relationship: among control-group partnerships that we observe more than once, half use a condom at some encounters but not others. It also means that, for a given partnership, we observe the transfers, the partner's characteristics, and condom use together and over time. The same survey measures consumption by category, food security, borrowing, pawning, and avoidance of healthcare every four weeks, mental health every four weeks, and cognitive functioning each semester, which allows us to measure impacts more granularly than coarser endline measures would (Poll, 2026).

Our results show that these students live much closer to subsistence than their place at the country's flagship university would suggest. At baseline, nearly half of participants had taken out a loan in the previous four weeks, typically at monthly interest rates of 7 to 20%; 40% had pawned or sold belongings; and one-third were not consistently eating two meals a day. Financial assistance raises reported consumption by ZMW 377 ($14) per two-week period, a 35% increase over the control mean, concentrated in groceries and housing. Treated participants score .29 SD higher on an index of food security and .28 SD higher on an index of financial well-being, with significant improvements in every component of both: they are less likely to skip meals, take out loans, pawn or sell belongings, fall behind on housing payments, or forgo healthcare because they cannot afford it. Treatment also reduces the share of participants screening positive for depression by 7.1 percentage points (14%) and raises performance on tests of fluid reasoning and cognitive control by .18

[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).

and .21 SD. In this way, financial aid for university students has many of the same benefits typically seen in cash transfer programs to extremely poor populations (Niehaus and Suri, 2026; Haushofer and Shapiro, 2016). Financial assistance did not, however, change academic outcomes over the horizon we observe. In administrative and survey data through the start of the following academic year, treatment has no effect on passing all courses, graduating, or dropping out. The estimates are precise enough to rule out effects on passing larger than 7 percentage points (11% of the control mean), though dropout is rare enough (2.8% in the control group) that we cannot rule out proportionally large effects on it. These outcomes are coarse and short-term, and effects of a pregnancy, or of cumulative deprivation on learning, may take longer to appear.

Financial assistance also changes sexual behavior, on two margins. Perhaps most strikingly, among women who had not had sex at baseline,[2] treatment reduces entry into sexual activity over the treatment period by 11.5 percentage points, a 43% reduction relative to the control group, and the encounters that do occur are with different partners: conditional on an encounter, an index of partner quality is .9 SD higher, participants report feeling safe with their partner .74 SD more often, and condom use is 25 percentage points (40%) higher. On the intensive margin, among women who were already sexually active at baseline, treatment has no effect on whether sex occurs, but condom use in a given encounter rises by 11.8 percentage points (24%) and encounters with partners who have given a large gift fall by 8.1 percentage points (28%), a pattern consistent with the same partners bargaining to different terms. Pooling both groups, the transfer reduces the share of women who report any sex over the treatment period by 5.1 percentage points (10.7%), raises condom use in a given encounter by 13.0 percentage points (25.1%), and reduces the biweekly rate of condomless sex by 1.8 percentage points, a 30% reduction relative to the control mean. Treated women are also 6.1 percentage points less likely to report having done something they are not proud of for money, a 34.7% reduction. Because abstinence and condom use are the two behaviors that most directly affect HIV transmission, these effects suggest that financial support could reduce HIV risk in a context where 14% of women aged 15–49 are HIV positive (Zambia Statistics Agency et al., 2024).

These results are consistent with a large literature outside economics documenting "transactional sex" relationships between young women and (often) older men in SSA, in which cash and gifts flow from the male to the female partner (Luke, 2003; Hunter, 2002; Stoebenau et al., 2016). These relationships are distinct from sex work: they are typically longer-term arrangements that combine financial and romantic elements, and neither party negotiates a price. They attracted particular attention from the public health community during the HIV/AIDS epidemic as an important channel for the spread of HIV from older men to young women (Gregson et al., 2002; Leclerc-Madlala, 2008). Measuring them, however, is difficult. The degree to which a relationship is financially motivated is hard to infer from its observable features, since gifts flow within relationships for altruistic and cultural reasons as well as transactional ones. Estimates therefore rely on young women's self-reports of whether sex was financially motivated, treating the distinction as binary. Such reports are likely to understate the degree to which sex responds to income, both because of social desirability bias and because women may not classify relationships that combine romantic and financial elements as transactional at all. Even so, self-reported rates are high: 14% of young women in Uganda report transactional sex in the past year (Stamatakis et al., 2021), as do 44% of pregnant and parenting adolescents in Kenya and 25% in Burkina Faso (Machoka et al., 2026). Nor are these arrangements unique to sub-Saharan Africa: self-reports put the rate of "sugar daddy" relationships at 5% on at least one American campus (Patel et al., 2026), and a variety of websites exist to match sugar daddies with sugar babies.[3]

[2] Randomization was stratified on baseline sexual activity, and heterogeneity by baseline sexual activity was pre-registered (AEARCTR-0014078).

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

To interpret these patterns, we develop a model of partner search and bargaining in which sex can be either consumption or labor. Women derive utility from consumption and from sex, and the utility from sex with a given partner can be positive or negative, depending on his quality and her aversion to premarital sex. This allows us to define transactional sex precisely: sex that a woman would refuse absent compensation, but that occurs in equilibrium because the man compensates her for it with transfers. Each period, a single woman chooses whether to search at no cost among ordinary men, who cannot afford transfers, or to pay a cost to search among rich men, who can; a matched couple bargains over transfers and condom use, with men paying a premium for condomless sex. A temporary income shock enters the model through a single channel, the woman's marginal utility of consumption, and that channel produces the two-margin pattern in the data. For a woman already matched to a paying partner, the shock is too short-lived to make leaving worthwhile, but it lowers the value she places on his money relative to condom use, so the bargain shifts toward condoms and away from transfers. For a single woman, the shock lowers the value of searching for a paying partner, so fewer women enter the market for "sugar daddies"; because the matches this eliminates are exactly those she would have refused without compensation, the encounters that remain are with "boyfriends," and partner quality and condom use conditional on an encounter rise. Because the shock is temporary, the decline in matching falls entirely on women who are single when it arrives.

Altogether, these results suggest that financial need is an important driver of sexual behavior. Taken with the academic nulls, they also suggest that the young women who reach an African flagship university, who are highly positively selected on ability, stay in school regardless, using a combination of transactional sex, high-interest loans, and reduced food consumption to do so; the same resourcefulness that keeps them in school exposes them to HIV. The timing of becoming sexually active is the margin most sensitive to income: read through the model, at least 43% of the first sexual relationships that women in the control group form are ones they would have refused without compensation. Even among college students in sub-Saharan Africa, risky sexual behavior falls sharply when income rises. Transactional sex may be an important part of the economic lives of young women, and a large labor market in its own right.

Economists studying financially motivated sex have largely focused on sex work, in which women identify as sex workers and exchange sex for money at clear prices, with no pretense of dating or romantic partnership.[4] 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. However, the public health estimates suggest that economically motivated sex may be relevant for a much wider swath of the population than sex workers or those on the margins of sex work. Indeed, Dinkelman et al. (2008) find that risky sex among young adults in Cape Town, South Africa rises in the wake of economic shocks. One of the most prominent papers in economics on this topic, Dupas (2011), shows that information about the relative HIV risk of older men changes the partner choices of teenage girls in Kenya, and that their sexual behavior responds on the intensive margin (which partner) rather than the extensive margin (whether to have sex). Because that paper does not observe transfers and does not shock financial need, it cannot speak to whether age-disparate relationships are financially motivated or reflect other considerations such as status. Our rich data on partners, transfers, and condom use, combined with experimental variation in income, allow us to measure the income elasticity of sex in this population directly.

[4]See Cunningham and Shah (2016) for an overview of this literature.

Whether sex, especially risky sex, is financially motivated matters directly for policy. Many campaigns to reduce risky sexual behavior in SSA have relied on information about risks or on incentives to remain STI- or HIV-negative (Duflo et al., 2015; Doyle et al., 2011; Michielsen et al., 2010). If risky sex is better understood as labor, an activity with negative utility undertaken for money, than as a consumption good, then liquidity or income support may be more effective than information or incentives. Some findings from the conditional cash transfer literature are consistent with this reading. Björkman Nyqvist et al. (2018) find that lottery-based incentives conditional on negative STI tests reduced HIV incidence in Lesotho, but the evidence on conditional cash transfers for negative STI tests is more mixed at first glance: Walque et al. (2012) find that sizable transfers reduced STI prevalence after one year in Tanzania, while Kohler and Thornton (2012) find no effect on HIV status in Malawi. In Malawi, however, women's risky sexual behavior fell *after* the payments were made, and in Tanzania the reduction in STIs appeared only after two rounds of payments. In both cases, then, liquidity rather than conditionality may have been what reduced risky sex, a possibility our experiment, which provides liquidity with no conditions attached, tests directly.

There is considerable evidence that increasing young women's income affects downstream 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 al. (2014) find that Kenya's national cash transfer program for orphans and vulnerable children delays sexual debut. Baird et al. hypothesize that transactional sex may explain their findings, but do not have the data to test whether this is the case. Our high-frequency data on partners, transfers, and condom use allow us to build on this literature by characterizing the sexual and financial partnerships that women form and by measuring how they change, encounter by encounter, in response to financial support. Our results highlight that transactional sex may be an important channel through which cash transfers to young women affect their health.

The consequences of risky sex extend beyond HIV. Unintended pregnancies are common among young women in SSA, and often lead to marriage,[5] derail education, or end in unsafe abortions. If women enter partnerships out of economic need, then liquidity may allow them to hold out for better matches, in the same way that unemployment benefits allow workers to hold out for better jobs. Whether longer search actually delivers better matches is an open question even in the labor market: Card et al. (2007) find that the longer spells induced by severance pay and benefit extensions have little effect on the quality of the next job, while Nekoei and Weber (2017) find that benefit extensions raise reemployment wages (see Cohen and Ganong (2026) for a meta-analysis of the duration effects). Far less attention has been paid to the analogous question for women's relationships, even though relationship formation and sexual behavior have implications for life trajectories at least as large. One prominent exception is Aizer et al. (2024), who show that recipients of the Mothers' Pension Program in the early twentieth-century United States delayed remarriage, with no detectable effect on the quality of the eventual spouse. Our results speak directly to this question during the period when it matters most: financial support both delays entry into sexual relationships and raises the quality of the partners chosen by the women who do enter.

The income elasticity we estimate also bears on HIV policy. Young women in sub-Saharan Africa are twice as likely to be infected as their male peers (UNAIDS, 2021), and age-disparate relationships with older, higher-resource men have long been identified as a key driver of that gap (Stoebenau et al., 2016; Vollmer et al., 2017). Prevention efforts have understandably focused on the information and incentives that shape

[5] In the 2024 Zambia DHS, among women whose first birth was conceived before their first union, 40% were married within one year of the child's birth (Zambia Statistics Agency et al., 2024).

sexual choices, and these have raised knowledge of risk more reliably than they have changed behavior. Our results point to one reason the link between knowledge and behavior may be weak: for a substantial share of young women, risky sex is a source of income, and a woman who knows the risks may still accept them if the alternative is going without rent or food. Economic circumstances are therefore a margin that prevention policy can act on alongside information, and in our setting a modest improvement in those circumstances increased abstinence and condom use, the two behaviors most closely tied to transmission risk.

Finally, our results speak to 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). The standard model of support, bursaries that cover fees but leave students to fund their own food, housing, and other living costs, leaves students in severe financial deprivation and reliant 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. Support for living costs, at a modest scale, relaxes that constraint, with 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. Opened in 1966, two years 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 (29 USD), and a housing allowance of ZMW 490 per month (19 USD). 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.[6] 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

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

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 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, January 2025, and February 2025. These follow-ups were mainly to retain the sample but also allowed us to collect 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 ($0.57), plus a ZMW 60 ($2.30) 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.

True attrition in the study was negligible (9 of 1,022). Periodic non-response was low during the experimental period: 0.2% of treatment participants and 1.7% of control participants missed any given biweekly survey. Non-response 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 non-response 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 four 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, and 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[7], 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).

[7]The actual payments were every four weeks, and went from early July to the end of September 2025.

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.[8] 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.[9] Many students do not come from particularly elite backgrounds; on average, students' fathers have almost completed secondary school but 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 ZMW 1,800 or ∼$70). Another 24% had characteristics of financially motivated sex (participant reports the partner is a "blesser," one of the answers to "what do you like about this person?" was "he gives me financial support"). 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.

### 3.5 Descriptive Evidence on Partnerships

Our high-frequency data allow us to characterize partnerships in detail over time. In Figure A3 we report Pearson correlations between our measured partner characteristics. We include the reported relationship type (blesser, boyfriend, husband/fiance, or casual partner), a set of questions asked the first time a partner is listed, including a binary variable for each characteristic listed from the question "What do you like about this partner?" (attractive, good personality, provides financial support, etc.), and whether she plans to introduce him to friends, parents, etc. We also include averages over questions asked every time a partner is reported, including whether she feels safe, whether a condom was used, and gifts in the past 2 weeks. The correlations indicate that both blessers and casual partners are negatively correlated with safety, and many of the "positive" characteristics, such as personality, physical attractiveness, and whether she sees him as a

[8] 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.

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

**Table 1: Balance Table**

| | Control Mean | Treatment Mean | Difference | P-value |
|---|---|---|---|---|
| **Stratification Variables** | | | | |
| Sex Ever at Baseline | 0.49 | 0.49 | 0.0010 | 0.98 |
| Sex in the Last 2 Weeks | 0.13 | 0.13 | -0.0037 | 0.86 |
| Skipped Meals in the Last 2 Weeks | 0.77 | 0.77 | -0.0039 | 0.88 |
| Pawned or Sold Items in the Last 4 Weeks | 0.40 | 0.41 | 0.0031 | 0.92 |
| **Demographics** | | | | |
| Age at Baseline | 20.45 | 20.52 | 0.070 | 0.49 |
| Married at Baseline | 0.0059 | 0.010 | 0.0039 | 0.48 |
| UNZA Year at Baseline | 1.82 | 1.81 | -0.014 | 0.80 |
| Number of Children at Baseline | 0.016 | 0.022 | 0.0059 | 0.49 |
| Number of Siblings | 3.69 | 3.58 | -0.11 | 0.25 |
| Had Puberty Initiation | 0.12 | 0.14 | 0.027 | 0.19 |
| Traditional Home | 0.25 | 0.25 | 0.0073 | 0.79 |
| HH Very Poor | 0.14 | 0.11 | -0.023 | 0.28 |
| HH Moderately Poor | 0.59 | 0.66 | 0.071** | 0.022 |
| 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 | -0.0043 | 0.86 |
| *In the Last 2 Weeks:* | | | | |
| Had At Least 2 Meals/Day | 0.67 | 0.70 | 0.031 | 0.28 |
| Avoided Eating Meat | 0.26 | 0.22 | -0.043 | 0.11 |
| *In the Last 4 Weeks:* | | | | |
| Took Out a Loan | 0.49 | 0.52 | 0.026 | 0.40 |
| Avoided Healthcare | 0.35 | 0.41 | 0.068** | 0.024 |
| Behind on Housing Payments | 0.63 | 0.65 | 0.014 | 0.65 |
| Did Something You Were Not Proud of | 0.20 | 0.21 | 0.0094 | 0.71 |
| **Sexual Behavior** | | | | |
| Condomless Sex in the Past Two Weeks | 0.055 | 0.047 | -0.0078 | 0.57 |
| Reported Financially Motivated Sex in the Past Two Weeks | 0.031 | 0.047 | 0.016 | 0.20 |
| *Conditional on Sex in the Past Two Weeks:* (N = 132) | | | | |
| Condomless Sex | 0.42 | 0.37 | -0.049 | 0.57 |
| Reported Financially Motivated Sex | 0.24 | 0.37 | 0.13 | 0.11 |
| Ever Received Large Gift from Partner | 0.25 | 0.34 | 0.085 | 0.29 |
| Over 10 Year Age Gap with Partner | 0.060 | 0.031 | -0.029 | 0.43 |

Joint F-test = 0.92, p-value = 0.57

**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 (∼$70) or more ever given by the partner (75th percentile). See Table 3 for complete description of "reported 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).

likely marriage partner. Blessers, in particular, are more likely to be older, married, see other people, and provide financial support. The full list of partner characteristic questions is listed in Appendix D.

In Figure A4, we show condom use within partnerships over time in the control group. We restrict to partnerships that are reported at least twice, which leaves us with 263 unique partnerships. We find that condom use is often inconsistent. While 31% report using a condom every time that partner is entered, a plurality, 49% report condom use some of the time that partner is reported. The remaining 20% never use a condom within the partnership. This indicates that condom use is not a fixed attribute of a woman, or even a partnership, but rather is decided within partnership by encounter.

### 3.6 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 $s$, $\tau_i$ is an indicator for assignment to treatment, and $\delta_s$ denotes stratum fixed effects. The coefficient of interest, $\beta_1$, 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 for treatment participants during the post-treatment period. We also include individual fixed effects $\lambda_i$ and survey-wave fixed effects $\gamma_w$ 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 $\gamma_w$ are defined as above. We replace individual fixed effects with a treatment-group indicator $\tau_i$ 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. Treated participants also report ZMW 377 (14 USD) in additional consumption per two-week period, a 35% increase over the control mean of ZMW 1082 (41 USD).[10] 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 or 3.2 USD) and housing (ZMW 73.4 or 2.8 USD) effects are the largest in magnitude and account for the largest share 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 and consumption in the last two weeks, and financial well-being in the last four 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. Both indices are constructed using generalized least squares weighting following the inverse covariance weighting approach proposed by Anderson (2008). "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.

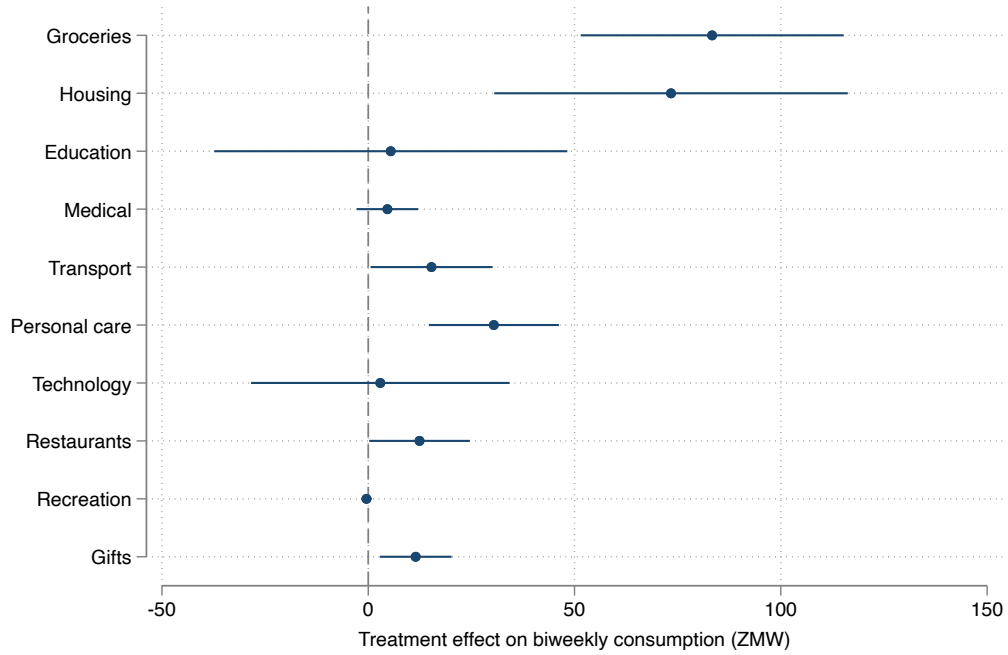
Table 2 columns (2) and (3) report estimates of equation (2) for indices of food security and financial well-being. During the treatment period, treated participants score 0.29 standard deviations higher on the food security index and 0.28 standard deviations higher on the financial well-being index, 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 statistics 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.

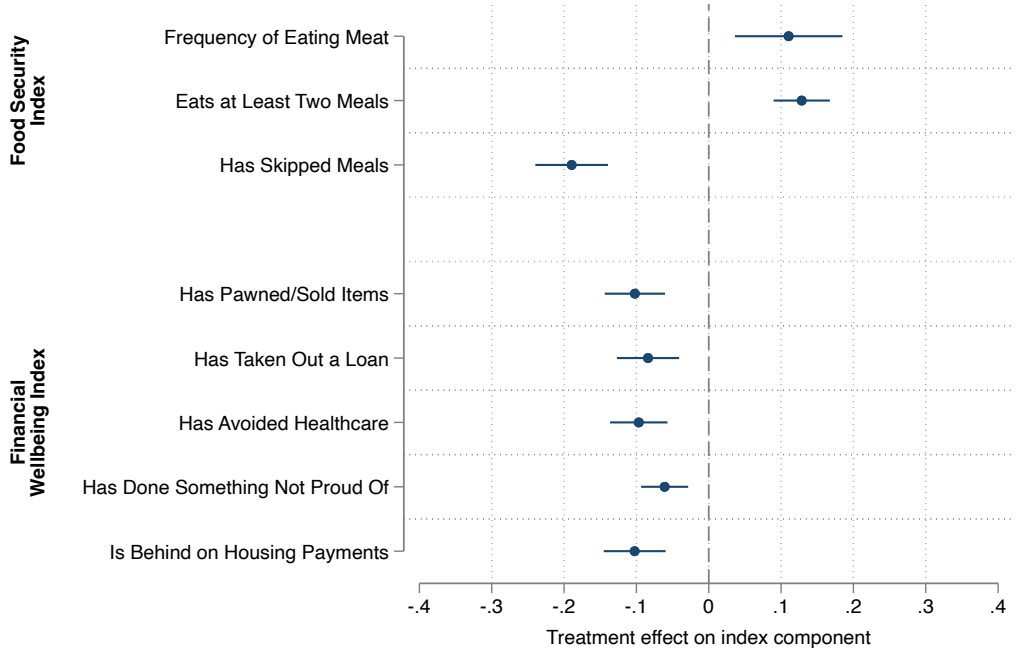
[10]The transfer is ZMW 1,000 per month (38 USD), or ZMW 500 (19 USD) every two weeks, so the consumption response is somewhat smaller than the size of the transfer. Some of this difference may be due to the crowd out of alternative transfers (e.g., from sexual partners). The remainder may also be saved, 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 well-being 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 Table 3, 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 been called a "blesser" or whom the participant reports provides "financial support") falls by 7.4 percentage points, a 74% 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.

## 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 can use the biweekly data to examine the *timing* of entry into sexual activity. Figure 2 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 49% sexually active participants, rising to around 52% by the beginning of treatment. 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.

We can also 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 a small and insignificant reduction in having sex in any given two-week period, a sizable and significant 1.8 ppt (30%) decline in condomless 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

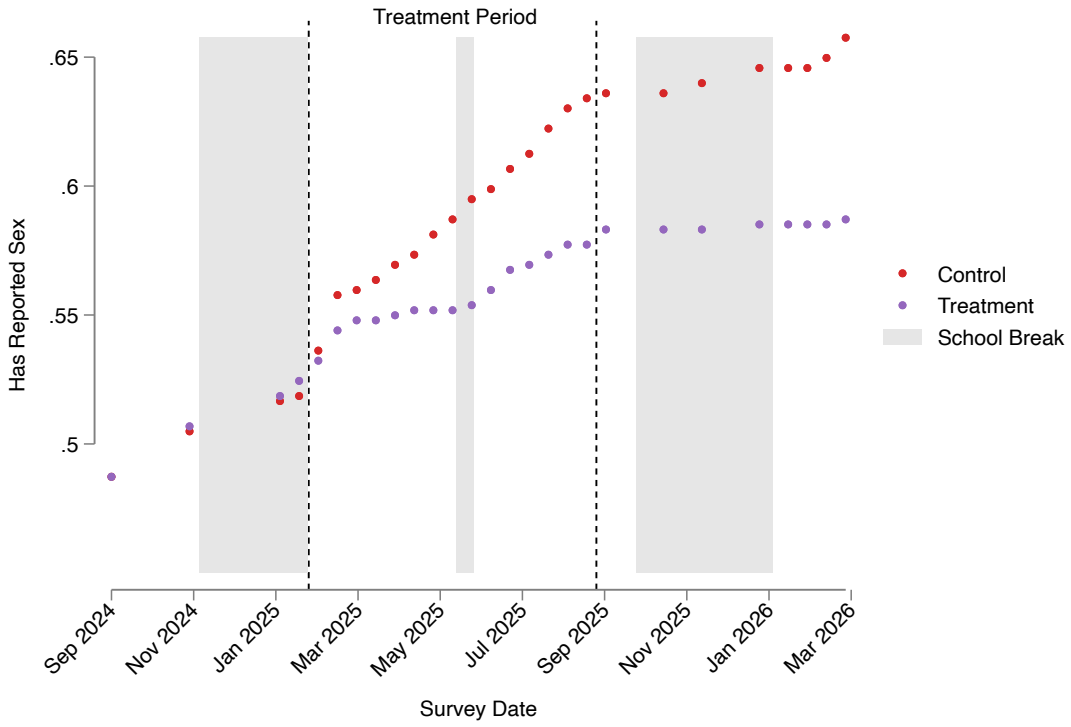
**Table 3: Effect of Financial Assistance on Ever Having Sex**

| | All Respondents | | | |
|---|---|---|---|---|
| Since Start of Treatment: | Ever Have Sex (1) | Ever Condom-less (2) | Ever Reported Financially Motivated (3) | Ever Not Proud (4) |
| Treatment | -0.051* | -0.029 | -0.029 | -0.14*** |
| | (0.026) | (0.025) | (0.022) | (0.029) |
| Stratum FE | Y | Y | Y | Y |
| Control Mean | 0.48 | 0.29 | 0.19 | 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 Reported Financially Motivated (3) | Ever Not Proud (4) | Ever Have Sex (5) | Ever Condom-less (6) | Ever Reported Financially Motivated (7) | Ever Not Proud (8) |
| Treatment | -0.11*** | -0.047* | -0.074*** | -0.18*** | 0.022 | -0.0042 | 0.017 | -0.097** |
| | (0.035) | (0.025) | (0.022) | (0.039) | (0.039) | (0.043) | (0.040) | (0.044) |
| Stratum FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Control Mean | 0.27 | 0.12 | 0.10 | 0.42 | 0.70 | 0.47 | 0.28 | 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. "Reported financially motivated" sex is defined as an encounter with a partner whom the participant reports is a "blesser" or for whom the participant lists "He provides financial support" as a primary reason for the relationship. "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.

eliminating unprotected sex, financially motivated sex in a given survey falls by 1.4 ppt or three-fourths, and 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 | Condom-less Sex | Reported Financially Motivated | Not Proud |
| | (1) | (2) | (3) | (4) |
| Treated | -0.0072 | -0.018** | -0.0013 | -0.061*** |
| | (0.012) | (0.0088) | (0.0084) | (0.017) |
| Participant FE | Y | Y | Y | Y |
| Survey FE | Y | Y | Y | Y |
| Control Mean | 0.13 | 0.061 | 0.045 | 0.18 |
| N | 19229 | 19189 | 19193 | 11135 |
| Clusters | 1022 | 1022 | 1022 | 1022 |

| | Never Sex at Baseline | | | | Sex at Baseline | | | |
|---|---|---|---|---|---|---|---|---|
| | Any Sex | Condom-less Sex | Reported Financially Motivated | Not Proud | Any Sex | Condom-less Sex | Reported Financially Motivated | Not Proud |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treated | -0.032*** | -0.017*** | -0.014** | -0.080*** | 0.018 | -0.020 | 0.012 | -0.042 |
| | (0.010) | (0.0056) | (0.0066) | (0.021) | (0.022) | (0.017) | (0.016) | (0.026) |
| Participant FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Survey FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Control Mean | 0.047 | 0.017 | 0.018 | 0.15 | 0.21 | 0.11 | 0.072 | 0.21 |
| N | 9771 | 9757 | 9758 | 5653 | 9404 | 9379 | 9381 | 5451 |
| Clusters | 521 | 521 | 521 | 521 | 498 | 498 | 498 | 498 |

**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 D 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 women 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. Treated participants also reported an increase of .7 on the 1-4 scale of whether they felt safe with their partner (1="never", 2 = "sometimes", 3 = "most of the time", 4 = "always"), an increase of .74 SD. The reduction in encounters with large-gift partners (-16.4 pp) is larger in magnitude than 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 A5 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.[11]

[11]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 opposite side as the stimulus when shown a heart, and on the same side when shown a flower. Performance is scored as the share of trials in which the participant responded correctly. See Figure A6 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 |
| Clusters | 489 | 493 | 404 | 490 |

| | 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 (or 69 USD, 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 D 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.

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.

## 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,[12] and an indicator for dropping out (which we infer if a student does not re-register in the next

[12]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 | 0.184*** | 0.210*** |
| | (0.024) | (0.024) | (0.037) | (0.039) | (0.070) | (0.062) |
| 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 | 0.010 | 0.042 |
| Control SD | | | 0.973 | 0.979 | 1.008 | 0.976 |
| 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 visual reasoning and cognitive control scores have been standardized to have mean 0 and SD 1; coefficients represent effects in standard deviation units. 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 B1. Standard errors are clustered at the participant level.

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.

**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.

## 4.7 Post-Treatment

Transfers were given out to the treatment group through the end of the 2025 academic year. The final transfer was issued in late September, 2025. We conducted several surveys after treatment ended, roughly every month after the end of term, and biweekly in February and March when students had returned to campus. In this section, we examine whether there were any lasting changes to behavior after the end of the treatment period. We note that non-response, particularly by the control group, was higher during this period, especially when students were at home (both electricity outages and spotty service are common

outside Lusaka). Still, response rates remained above 90% for most surveys for both treatment and control.

Appendix Table B3 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 Table B5 re-estimate survey and encounter-level effects including the post-treatment periods. 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 six-month post-treatment period.

# 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 A7. 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 B2, 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 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, asked 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 B4, 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.

### 5.3 Reporting Bias

The primary analysis in this paper relies on self-reported survey measures. It is natural to ask whether these measures were reported truthfully, and whether treatment could have affected participants' truthfulness in reporting. It is important to note that our survey was designed to elicit truthful responses even on sensitive questions by using self-administered surveys with short recall windows. In a previous paper on contraceptive use in this population, self-reports of contraceptive use, another stigmatized behavior, closely mirrored administrative data from clinic records (Bau et al., 2024).

In order for differential reporting to drive our findings, participants would have to be differentially misreporting (more sex, less condom use, and more financial deprivation) in the control group relative to the treatment group. There are two potential factors that could lead to this. First, either the control or treatment group might want to strategically answer the survey in order to be eligible (or continue to be eligible) for the cash transfers. It is important to note that both control and treatment groups were told at baseline that they might be eligible for cash assistance in the future, but the amount and timing was left intentionally vague. Thus, the announcement of monthly cash assistance was a surprise to both groups. Second, this experiment was done on a single university campus, with randomization at the individual level. The control group was aware that cash was being given to treatment participants: our office fielded many calls from control participants asking whether they would receive funds, and it was frequently mentioned in the "free response" section of the survey among control participants. Thus, it is likely that strategic responses would be equally likely from both groups. All information from the research team made clear that the cash assistance was allocated randomly, and was being studied for research purposes. However, to the extent that participants were inclined to report what they thought researchers would want to hear, it is likely that both treatment and control participants were equally inclined to do so.

Because our organization was giving out cash, the most natural strategic response would be that participants would want to appear more financially deprived than they actually are, in order to increase the likelihood that they would receive a grant. To address this, we can take advantage of the fact that we asked participants some demographic questions about their natal household poverty in August 2025, after treatment had been underway for several months. Because these questions are about household poverty, they should not be affected by treatment status. But if either treatment or control participants were differentially over-reporting poverty indicators in hopes of being allocated financial assistance, we could see a placebo effect of treatment status. In Appendix Table B6, we report the effect of treatment status on four pre-determined but highly subjective measures of household poverty: whether the household is not poor, moderately poor or very poor (scaled 1-3), and the economic standing of the student's natal household relative to their classmates, their own village, and Zambia as a whole on a scale from 1 ("among the poorest") to 5 ("among the richest"). Reassuringly, we do not find any treatment effects on these outcomes, and the coefficients are close to zero.

The second possible type of reporting bias that could impact our results is if participants (and treatment participants specifically) wanted to report "moral" behavior to us, in order to seem deserving of grants, which led them to under-report sexual activity. We note that this under-reporting would have to be very particular to explain our results: contained (on reporting sex at all) to participants who did not report being

sexually active at baseline, and also affecting reported condom use among all participants. However, to test for this, we can exploit the timing of the announcement of treatment, to see if this affected reported sexual activity (for instance, if the treatment effects were driven by control participants significantly *reducing* sexual activity at the onset of treatment, that might be concerning). In Figure 2, we show the timing of *new* reports of sexual activity actually increase for both treatment and control in the two surveys immediately after the announcement of treatment. This is consistent with sexual activity increasing as students return to school, and inconsistent with students strategically reporting "virtuous" behavior on surveys to try to qualify for financial assistance.

Lastly, we can test whether treatment status affected self-reports of other morally coded behavior, either stigmatized or "virtuous" behavior. In Appendix Figure A8 we report the impact of treatment on reported time use. Participants were asked about total hours spent on various activities on the previous day. While it is certainly plausible that treatment could have affected time use (freeing up time working for studying, for instance, or spending less time with sexual partners), we do not find any significant effects on reported hours spent on any activity. Importantly, we do not see that treatment affected reporting on time spent in religious services, for instance, or in school/studying, which may be seen as "virtuous" behaviors, and also did not affect reported time with partners or "relaxing/watching TV" which might be seen as less virtuous. In Appendix Table B7, we test whether treatment status affected reporting the most stigmatized behavior in our survey: abortion. We find that treatment has no significant effect on reported abortions, whether we condition on pregnancy or not. In fact, in four of the six columns, the coefficient on treatment is positive, indicating that, if anything, treatment participants are *more* likely to report abortions, even as they are reporting less sex.

Taken together, these results give us reassurance that our primary findings are not the results of social desirability or reporting bias among the treatment group.

# 6 Model

In Section 4, we show that a temporary increase in income changes women's sexual behavior, increasing abstinence and condom use. We show that the decrease in sexual activity is driven by delayed entry into sexual activity, and that the partners who are crowded out by the experimental financial transfers were particularly low quality – driving up the average quality of partners who remain. In this section, we consider whether a model in which women are engaging in both transactional sex (with "blessers" or "sugar daddies") *and* non-transactional sex (with boyfriends) could explain these findings.

We introduce a dynamic model of dating in which women can either search costlessly for low-income partners (likely other university students) or pay a cost to search for high-income partners (who are older and working). Each partner has a quality, $q$, and women get utility from sex $s(q)$ with a partner of quality $q$, where $s(q)$ can be positive or negative, and men can compensate women with financial transfers when $s(q) < 0$. Women can choose whether or not to have sex with that partner if the overall utility from sex (including transfers) is positive. Women and men Nash bargain over both the financial transfers and whether a condom is used.

## 6.1 Environment

Consider a continuum of infinitely-lived *women*, endowed with a *baseline income* $y^0 \sim Y$, and another of *men*, endowed with an *income* $i \in \{H, L\}$ and *quality* $q \sim Q$. Let time be discrete. In each period a woman

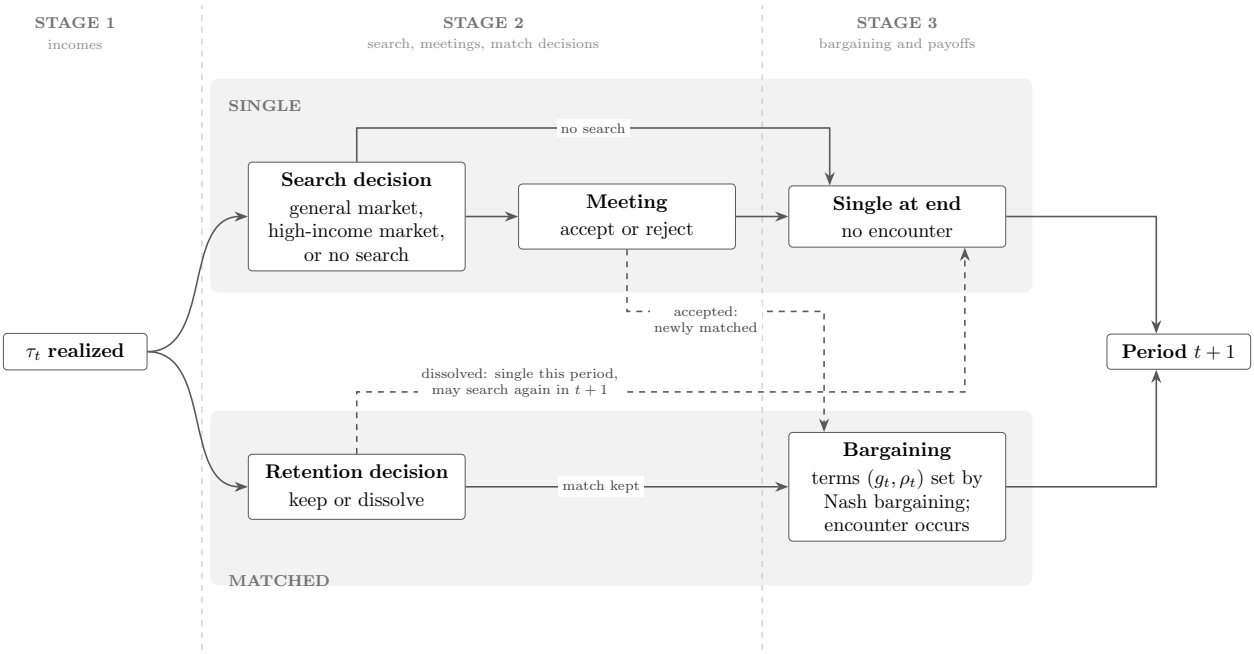



**Figure 3: Model Timing**

is either single or matched to one man, and a match persists until one party dissolves it. A matched couple has a sexual encounter in every period the match persists.

*Timing.* Each woman enters each period with her baseline income and a match status, single or matched. The period then unfolds as follows. First, each woman's *period income* $y_t = y^0 + \tau_t$ is realized, where $\tau_t \in \{0, \bar{\tau}\}$ is her *treatment status* in period $t$. Second, a matched woman decides whether to keep or dissolve her match, and a single woman chooses whether to search. If she chooses to search, she can search in the *general market*, consisting of men with income $i = L$, at no cost or in the *high-income market*, consisting of men with income $i = H$, with positive utility cost. Then, for each woman, a meeting occurs and she chooses to accept or reject. Third, with perfect information, matched couples bargain over the *transfer* $g_t$ and *condom use rate* $\rho_t \in [0, 1]$. Finally, payoffs are realized. A woman who dissolves a match, or who rejects a meeting, is single in the current period and can search again only next period.

*Women.* A woman is characterized by her period income $y_t$. Her income is made up of a baseline endowment $y^0 \sim Y$ where $Y$ has support on $[\underline{y}, \overline{y}]$, with $\underline{y} > 0$, and treatment status $\tau_t$, drawn i.i.d. in a single period $t$ from a distribution with support $\{0, \bar{\tau}\}$. Hence, we model treatment as a one-time, temporary transfer. We refer to $\tau_t = 0$ as *baseline* and to $\tau_t = \bar{\tau}$ as *treatment*. Hence, the financial assistance treatment is a temporary income shock which increases period income but not the expectation over future income. If she is single, her period utility is $v(y_t)$, where $v$ is continuously differentiable with $v' > 0$, $v'' < 0$, and $v'(c) \to \infty$ as $c \to 0$. If she is matched to a man of quality $q_t$ (we slightly abuse notation throughout by using a common subscript $t$ for the characteristics of whichever man she meets or is matched to and vice versa) who pays her a transfer $g_t \geq 0$, her period utility is

$$v(y_t + g_t) + s(q_t) + \mu\rho_t, \tag{4}$$

where $s$ is continuous and increasing in $q_t$ (the quality of her match in period $t$) and may be positive or negative, $\rho_t \in [0, 1]$ is the fraction of the couple's sexual encounters in which a condom is used, and $\mu > 0$ is

her utility per unit of condom use. Women, then, get utility from income and condom usage. They also get utility or disutility from sex, depending on the quality of the man.

*Men.* A man is characterized by his income $i \in \{L, H\}$ (with $H > L$) and his quality $q$, distributed according to $Q$.[13] We refer to men with $i = L$ as *ordinary men* and to men with $i = H$ as *rich men*. Assume $Q$ has bounded support, is atomless, and that income and quality are drawn independently. A single man's period utility is $v(i)$. A matched man's period utility is

$$v(i - g_t) + S - \lambda\rho_t, \tag{5}$$

where $S > 0$ is his utility from sex and $\lambda > 0$ is his disutility per unit of condom use. That is, men pay a premium for condomless sex (Gertler et al., 2005). We do not model men's search: a single man receives $v(i)$ in every period he remains single.

*Search.* In each period a single woman chooses one of three options. She may search the *general market* at no cost, search the *high-income market* at a utility cost $\kappa > 0$, or not search. The two markets differ only in the income distribution from which men are drawn: the general market includes only men with $i = L$ and the high-income market includes only men with $i = H$. In either market she meets one man at random; the pair observes each other's type—$y_t$ for her and $(i, q)$ for him—and each decides whether to accept. Because $i$ and $q$ are independent, paying $\kappa$ buys a better income draw, not a better quality draw.

*Bargaining.* A matched couple bargains in every period over the terms $(g, \rho)$ according to the *symmetric Nash solution*. The disagreement point is that both parties are single in the current period and enter the next period single. Let $W^S(y^0)$ denote the woman's value of entering the next period single and $W^M(y^0; i, q)$ her value of entering the next period matched to $(i, q)$, and define her continuation gain from remaining matched

$$\Delta_f(y^0; i_t, q_t) \equiv \beta \left[W^M(y^0; i_t, q_t) - W^S(y^0)\right],$$

where $\beta \in (0, 1)$ is the discount rate. Define $\Delta_m(i, q; y^0)$ analogously for the man. Because $\tau_t$ is drawn i.i.d., its current realization carries no information about future income: the continuation values depend on the constant baseline income but not on $\tau_t$. So the current treatment status does not impact the continuation gain of remaining matched, and the value of remaining or becoming single is a constant. The woman's surplus from terms $(g, \rho)$, relative to disagreement, is

$$\sigma_f(g_t, \rho_t) = v(y_t + g_t) - v(y_t) + s(q_t) + \mu\rho_t + \Delta_f, \tag{6}$$

and the man's is

$$\sigma_m(g_t, \rho_t) = v(i - g_t) - v(i) + S - \lambda\rho_t + \Delta_m. \tag{7}$$

The bargained terms $(g^*, \rho^*)$ solve

$$\max_{g_t \geq 0,\ \rho_t \in [0,1]} \sigma_f(g_t, \rho_t)\, \sigma_m(g_t, \rho_t) \quad \text{subject to} \quad \sigma_f(g_t, \rho_t) \geq 0,\ \sigma_m(g_t, \rho_t) \geq 0. \tag{8}$$

A match forms, or persists, if and only if both parties receive nonnegative surplus. Because a woman who rejects a meeting and a woman who dissolves a match face the same continuation, she accepts a meeting and

[13] Note that all results in this model hold if we allow for a distribution of incomes, as long as the supports of the general market and high-income markets have an empty intersection. In other words, rich men need to be strictly richer than ordinary men.

retains a match by the same rule. We write $\sigma(y_t; i_t, q_t)$ for the woman's bargained surplus, so that this rule is $\sigma \geq 0$.

## 6.2 Assumptions

We make two assumptions to simplify the matching and bargaining problems and to restrict attention to the interesting cases.

**Assumption 1** (Rich enough men)**.** Rich men *($i = H$) are rich enough to compensate any woman: there exists $g \in (0, H)$ such that $\sigma_f(g, 0) > 0$ and $\sigma_m(g, 0) > 0$ for every $y_t \in [\underline{y},\, \overline{y} + \bar{\tau}]$ and every $q \in \operatorname{supp} Q$.*

First, Assumption 1 says that a rich man's income is large relative to the worst match a woman could draw: even facing the lowest-quality man, at no condom use, and at any income she might have under treatment, he can transfer enough to leave both parties with strictly positive surplus. The transfer is bounded only by his income $H$, so his capacity to compensate scales with the income gap between rich and ordinary men, which is large in our setting. What the assumption buys is that entry into the high-income market is a decision about search, not about matching: a woman who pays $\kappa$ meets a rich man and matches with him, whatever his quality, as we verify in Proposition 1(b).

**Assumption 2** (Transfer sizes)**.** *(a)* Ordinary men. *$\mu\, v'(L) \geq \lambda\, v'(\underline{y})$. In any match with an ordinary man in which the bargained condom share is one, the bargained transfer is zero.*

*(b)* Rich men. *$\lambda\, v'(\overline{y} + \bar{\tau}) > \mu\, v'(H)$. In any match with a rich man in which the bargained condom share is zero, the bargained transfer is strictly positive.*

Assumption 2 first ensures that money is the *expensive* bargaining tool for ordinary men, so much so that the transfer is zero even when the pair always use a condom. This is a reasonable assumption in a world where most ordinary men live relatively close to subsistence. Part (b) ensures that money is the *cheap* bargaining tool for rich men. Because a rich man's marginal utility of consumption is low, a small reduction in condom use compensated by a small transfer raises the total surplus. When condoms are always used, we impose that rich men are still willing to make a transfer. This is reflected in the data; in "sugar daddy"-type arrangements where condoms are used we still see transfers being made.

## 6.3 Equilibrium

Under the above assumptions, the symmetric Nash equilibrium can be characterized as follows.

**Proposition 1** (Equilibrium)**.** *(a)* General market. *In a match formed in the general market: (i) there is a threshold $\bar{q}$ such that the woman accepts the man if and only if $q \geq \bar{q}$; (ii) the bargained condom share does not depend on her income.*

*(b)* High-income market. *In a match formed in the high-income market: (i) every meeting is accepted, regardless of $q$; (ii) for $\rho^* \in (0, 1)$, the transfer satisfies $\lambda v'(y_t + g^*) = \mu v'(H - g^*)$ and the condom share divides the surplus at rate $\lambda/\mu$, that is, $\mu\, \sigma_m = \lambda\, \sigma_f$.*

*(c)* Market choice. *There is a cutoff $\tilde{y}(y^0)$ in current income such that a single woman pays $\kappa$ and searches the high-income market if $y_t \leq \tilde{y}(y^0)$, and searches the general market at no cost if $y_t > \tilde{y}(y^0)$.*

*Proof.* See Appendix C. □

Part (a) describes matches in the general market. Since an ordinary man cannot compensate a woman via a transfer (by Assumption 2), she matches with him only when the man's quality is sufficiently high that she is willing to do so without monetary compensation. We call such matches *love matches*. Notably, since no transfer will or can take place, her income is irrelevant both to whether she matches and to the terms of the match; the terms depend only on the man's quality.

**Definition 1** (Transactional match)**.** *A high-income match is transactional if the woman would refuse the man in the absence of the transfer at the (already) bargained $\rho^*$.*

Part (b) describes matches in which rich men can pay, and do. Every woman who searches the high-income market is compensated enough to accept any draw. Compensation takes the form of money, plus condom use where the division of the surplus calls for it. Even though a transfer always occurs in these matches, they are not all *transactional*, as defined in Definition 1. For some matches the quality of the men is sufficiently high and the bargained condom share sufficiently large that the woman would remain. The transfer, in these cases, exists to maximize the pair's joint surplus in an already agreeable match.

Part (c) follows from the first two. Write the gain from searching the high-income market rather than the general market as

$$\Pi(y_t; y^0) \;\equiv\; \mathbb{E}_R\, \sigma^R(y_t; H, q_t) \;-\; \mathbb{E}_G \max\{\sigma^G(L, q_t), 0\} \tag{9}$$

where the expectations are over the quality draw and continuation values are held at their baseline values. A single woman pays $\kappa$ if and only if $\Pi(y_t; y^0) \geq \kappa$, and $\Pi$ is strictly decreasing in $y_t$, which is what delivers the cutoff $\tilde{y}(y^0)$. Intuitively, the value of searching the general market for a single woman does not depend on her current income: no general-market match carries a transfer, so her income enters her payoff identically whether or not she matches. The value of searching the high-income market does depend on her current income because every high-income match carries a transfer, and a transfer is worth more when the marginal utility of consumption is high. An increase in current income therefore moves a woman out of the high-income market. Since general-market search is costless and she may reject any meeting, a single woman never strictly prefers not to search and we therefore restrict attention to the two search options.

So, in equilibrium, women sort across three states. Those with low enough current income pay $\kappa$, meet a rich man, and match with him whatever his quality; some of these matches are transactional and some are not. Those with higher income search the general market and match only when they draw a man above $\bar{q}$, so they end the period either in a love match or single, and single with probability $\Pr(q < \bar{q})$. Which of the two markets a woman searches is determined by her income relative to $\tilde{y}(y^0)$.

## 6.4 Comparative Statics of a Temporary Income Shock

We now ask what a temporary increase in income does in this environment. Because the transfer is small and lasts one period, it cannot change what a match is worth going forward; it changes only what a woman gives up today by matching, and hence only her decision about where to search. The results below trace that single margin through to sexual activity, partner quality, who responds, and the terms of the encounters that still occur.

The comparative statics below are interesting only if the treatment actually changes some women's market choice. We therefore restrict the search cost as follows.

**Assumption 3** (Baseline)**.** *$Y$ admits a positive density on $[\underline{y}, \overline{y}]$, and the search cost satisfies $\Pi(\overline{y} + \bar{\tau};\, \overline{y}) \;<\; \kappa \;\leq\; \Pi(\underline{y};\, \underline{y})$.*

In other words, we assume that $\kappa$ is sufficiently small that the poorest woman pays to search the high-income market at baseline, but sufficiently large that the richest does not pay it under treatment, reflecting the fact that we see different search and match behavior in the data: some women only have love matches and some only with richer men. By the continuity of $\Pi$ in both of its arguments and its strict monotonicity in current income (Lemma 2), a positive measure of $y^0$ then satisfies $y^0 \leq \tilde{y}(y^0) < y^0 + \bar{\tau}$, meaning that the treatment moves some women from the high-income market to the general market.

First, the model predicts that we should see an overall decrease in sexual activity.

**Proposition 2** (Sexual activity)**.** *For every woman and every initial state—single, or matched to any* $(i_t, q_t)$*—the probability of a sexual encounter in the current period is weakly lower under treatment than at baseline. Moreover, the decline in sexual activity occurs entirely among women who are single at baseline.*

*Proof.* See Appendix C. □

This decline comes from switches across markets. A woman who begins period $t$ single and receives the transfer is more likely to search the general market, since a transfer is worth less to her when her income is already higher, and $\kappa$ buys nothing but access to men who pay. Having switched, she matches only when she draws a man above $\bar{q}$, so with probability $\Pr(q < \bar{q})$ she has no encounter at all. Already matched women, however, continue to have sexual encounters. Since the treatment is a one-time shock, it has no impact on a woman's expected future income. Dissolving a match means searching again, and potentially paying $\kappa$ again, after income has returned to baseline. So, a matched woman keeps her match but may be compensated by higher condom usage. This reflects the fact that the decrease in sexual activity happens primarily among women who had not reported previous sexual activity, while those already engaged in relationships of either kind (with ordinary or rich men) see no change in either direction.

This movement across markets, then, also impacts the encounter-level outcomes in terms of quality of matches and condom use rate. First, the composition of matched men changes due to the treatment, since women follow different acceptance rules in each market.

**Proposition 3** (Partner quality)**.** *Consider the matches formed in the current period by women who are single at the start of the period. Treatment (a) weakly reduces the measure of such matches, and every match that is eliminated has* $q_t < \bar{q}$*; (b) weakly raises the share of such matches with quality above any given level, and hence weakly raises average partner quality among them; and (c) decreases the number of transactional matches. Part (a) is strict if* $\Pr(q < \bar{q}) > 0$*, and part (b) is strict if* $0 < \Pr(q < \bar{q}) < 1$*.*

*Proof.* See Appendix C. □

Women match only with a man of quality at least $\bar{q}$ in the general market; the rest of their draws produce no match. Because a meeting occurs in both markets and the quality draw is independent of the income draw, switching markets deletes exactly the matches with $q < \bar{q}$ which would exist otherwise and leaves the rest unchanged. Hence, treatment makes transactional matches less likely; women are less likely to search in the high-income market and are thereby less likely to match with a low-quality man.

Second, the treatment makes condom use more likely in high-income relationships, since the value of an additional dollar decreases relative to condom usage.

**Proposition 4** (Condom use)**.** *Under treatment, in every match with a rich man, the bargained condom share is weakly higher at income* $y^0 + \bar{\tau}$ *than at* $y^0$ *(strictly so if it is interior at* $y^0$*). For* $\rho^* \in (0, 1)$ *the transfer falls with income, with* $-1 < \partial g^*/\partial y_t < 0$*. In general-market matches the bargained terms are unchanged.*

*Proof.* See Appendix C. □

Since a temporary increase in income lowers the woman's marginal utility of consumption in the treated period, a woman who remains matched to a rich man therefore values compensation in the form of condom use relatively more than compensation in the form of money, and the bargain shifts toward condoms. The same result is not expected in matches with ordinary men since no money changes hands; the additional income does not change the bargained agreement.

While Assumption 1 makes the statement of these results cleaner, the heart of the result remains even if this is relaxed. Suppose instead rich men were sometimes income-constrained and unable to always increase $g_t$ to make surplus positive, but were still richer than poor men. Then, search in the high-income market would be less attractive and the behavior of singles would not change from our predictions except that more women overall search the general market. There is, though, the possibility that matched women will dissolve their matches, but this would only occur for the (relatively small) set of women who are very close to the acceptance margin; i.e., women for whom the treatment makes the man unwilling to further compensate them either by increasing $g_t$ or $\rho_t$. The effect on condom use would remain as well. Consistent with few women lying near this margin, we find no significant change in sexual activity among women who were already sexually active at baseline in the data.

Propositions 2–4 therefore reflect patterns documented in Section 4: a temporary increase in income weakly decreases sexual activity, the decline is concentrated among women unmatched at baseline, partner quality conditional on matching improves, and condom use rises.

# 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 43% – 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 importance 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.

## References


Aizer, Anna, Sungwoo Cho, Shari Eli, and Adriana Lleras-Muney (2024). "The Impact of Cash Transfers to Poor Mothers on Family Structure and Maternal Well-Being". In: *American Economic Journal: Applied Economics* 16.2, pp. 492–529.

Anderson, Michael L. (2008). "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects". In: *Journal of the American Statistical Association* 103.484, pp. 1481–1495. DOI: 10.1198/016214508000000841. URL: https://doi.org/10.1198/016214508000000841.

Baird, Sarah, Richard S. Garfein, Craig T. McIntosh, and Berk Özler (2012). "Effect of a Cash Transfer Programme for Schooling on Prevalence of HIV and Herpes Simplex Type 2 in Malawi: A Cluster Randomised Trial". In: *The Lancet* 379.9823, pp. 1320–1329.

Bank of Zambia (2026). *Historical Series of Daily ZMW/USD Exchange Rates*. https://www.boz.zm/historical-series-of-daily-zmw-usd-exchange-rates-zmw.htm. Historical daily exchange rate data for the Zambian Kwacha (ZMW) to US Dollar (USD), accessed 2026-01-22.

Bau, Natalie, David J. Henning, Corinne Low, and Bryce Millett Steinberg (2024). "Family Planning, Now and Later: Infertility Fears and Contraceptive Take-Up". NBER Working Paper 32735.

Bau, Natalie, Corinne Low, Simona Simona, and Bryce Steinberg (2026). "The Precarious Financial Lives of College Students in Sub-Saharan Africa". In: *NBER Working Paper* 34989.

Björkman Nyqvist, Martina, Lucia Corno, Damien de Walque, and Jakob Svensson (2018). "Incentivizing Safer Sexual Behavior: Evidence from a Lottery Experiment on HIV Prevention". In: *American Economic Journal: Applied Economics* 10.3, pp. 287–314.

Cabinet Office, Government of Zambia (2024). *Levy Patrick Mwanawasa BIO*. https://www.cabinet.gov.zm/newsite/wp-content/uploads/2024/05/LEVY-PATRICK-MWANAWASA-BIO.pdf. Accessed: 2025-12-09.

Card, David, Raj Chetty, and Andrea Weber (2007). "Cash-on-Hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market". In: *Quarterly Journal of Economics* 122.4, pp. 1511–1560.

Chetty, Raj (2008). "Moral hazard versus liquidity and optimal unemployment insurance". In: *Journal of political Economy* 116.2, pp. 173–234.

Cohen, Jonathan P and Peter Ganong (2026). "Disemployment Effects of Unemployment Insurance: A Meta-Analysis". In: *American Economic Review: Insights* 8.1, pp. 1–18.

Council on Foreign Relations (2025). *Biography President Hakainde Hichilema*. Accessed: 2025-12-09.

Cunningham, Scott and Manisha Shah, eds. (2016). *The Oxford Handbook of the Economics of Prostitution*. Oxford: Oxford University Press.

Davidson, Matthew C., Dima Amso, Loren Cruess Anderson, and Adele Diamond (2006). "Development of Cognitive Control and Executive Functions from 4 to 13 Years: Evidence from Manipulations of Memory, Inhibition, and Task Switching". In: *Neuropsychologia* 44.11, pp. 2037–2078.

Dinkelman, Taryn, David Lam, and Murray Leibbrandt (2008). "Linking Poverty and Income Shocks to Risky Sexual Behaviour: Evidence from a Panel Study of Young Adults in Cape Town". In: *South African Journal of Economics* 76.S1, S52–S74.

Doyle, Aoife M, Helen A Weiss, Kaballa Maganja, Saidi Kapiga, Sheena McCormack, Deborah Watson-Jones, John Changalucha, Richard J Hayes, and David A Ross (2011). "The long-term impact of the MEMA kwa Vijana adolescent sexual and reproductive health intervention: effect of dose and time since intervention exposure". In: *PLoS One* 6.9, e24866.

Duflo, Esther, Pascaline Dupas, and Michael Kremer (2015). "Education, HIV, and Early Fertility: Experimental Evidence from Kenya". In: *American Economic Review* 105.9, pp. 2757–2797.

Duflo, Esther, Pascaline Dupas, Elizabeth Spelke, and Mark P Walsh (2024). "Intergenerational Impacts of Secondary Education: Experimental Evidence from Ghana". In: *NBER Working Paper* 32742.

Dupas, Pascaline (2011). "Do Teenagers Respond to HIV Risk Information? Evidence from a Field Experiment in Kenya". In: *American Economic Journal: Applied Economics* 3.1, pp. 1–34.

Economic Research Institute (2025). *Cost of Living in Lusaka, Zambia*. https://www.erieri.com/cost-of-living/zambia/lusaka. Accessed: 2025-12-09.

Gertler, Paul, Manisha Shah, and Stefano M. Bertozzi (2005). "Risky Business: The Market for Unprotected Commercial Sex". In: *Journal of Political Economy* 113.3, pp. 518–550.

Gregson, Simon, Constance A. Nyamukapa, Geoffrey P. Garnett, Peter R. Mason, Tom Zhuwau, Michel Caraël, Stephen K. Chandiwana, and Roy M. Anderson (2002). "Sexual Mixing Patterns and Sex-Differentials in Teenage Exposure to HIV Infection in Rural Zimbabwe". In: *The Lancet* 359.9321, pp. 1896–1903.

Handa, Sudhanshu, Carolyn Tucker Halpern, Audrey Pettifor, and Harsha Thirumurthy (2014). "The Government of Kenya's Cash Transfer Program Reduces the Risk of Sexual Debut among Young People Age 15–25". In: *PLOS ONE* 9.1, e85473.

Haushofer, Johannes and Jeremy Shapiro (2016). "The short-term impact of unconditional cash transfers to the poor: experimental evidence from Kenya". In: *The Quarterly Journal of Economics* 131.4, pp. 1973–2042.

Hunter, Mark (2002). "The Materiality of Everyday Sex: Thinking Beyond 'Prostitution'". In: *African Studies* 61.1, pp. 99–120.

Jolliffe, I. T. (2002). *Principal Component Analysis*. New York: Springer.

Jones, Kelly and Erick Gong (2021). "Precautionary Savings and Shock-Coping Behaviors: Effects of Promoting Mobile Bank Savings on Transactional Sex in Kenya". In: *Journal of Health Economics* 78.

Judiciary of Zambia (2025). *Supreme Court Judges*. https://judiciaryzambia.com/category/adjudicators/supreme-court-judges/. Accessed: 2025-12-09.

Kohler, Hans-Peter and Rebecca L. Thornton (2012). "Conditional Cash Transfers and HIV/AIDS Prevention: Unconditionally Promising?" In: *World Bank Economic Review* 26.2, pp. 165–190.

Kroenke, Kurt, Robert L. Spitzer, Janet B. W. Williams, and Bernd Löwe (2009). "An Ultra-Brief Screening Scale for Anxiety and Depression: The PHQ-4". In: *Psychosomatics* 50.6, pp. 613–621.

Leclerc-Madlala, Suzanne (2008). "Age-Disparate and Intergenerational Sex in Southern Africa: The Dynamics of Hypervulnerability". In: *AIDS* 22.Suppl 4, S17–S25.

Lee, David S. (2009). "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects". In: *The Review of Economic Studies* 76.3, pp. 1071–1102. URL: http://www.jstor.org/stable/40247633 (visited on 04/29/2026).

Luke, Nancy (2003). "Age and Economic Asymmetries in the Sexual Relationships of Adolescent Girls in Sub-Saharan Africa". In: *Studies in Family Planning* 34.2, pp. 67–86.

— (2005). "Confronting the "Sugar Daddy" Stereotype: Age and Economic Asymmetries and Risky Sexual Behavior in Urban Kenya". In: *International Family Planning Perspectives* 31.1, pp. 6–14.

— (2006). "Exchange and Condom Use in Informal Sexual Relationships in Urban Kenya". In: *Economic Development and Cultural Change* 54.2, pp. 319–348.

Luke, Nancy, Shelley Clark, and Eliya M. Zulu (2011). "The Relationship History Calendar: Improving the Scope and Quality of Data on Youth Sexual Behavior". In: *Demography* 48.3, pp. 1151–1176.

Machoka, Beryl Nyatuga, Caroline Kabiru, Jalisa Stanislaus, Catherine Asego, and Anthony Idowu Ajayi (2026). "Transactional sex and intersecting vulnerabilities: a multicountry cross-sectional study of pregnant and parenting adolescents in sub-Saharan Africa." In: *BMJ Public Health* 4.2, e003966–e003966.

Mani, Anandi, Sendhil Mullainathan, Eldar Shafir, and Jiaying Zhao (2013). "Poverty Impedes Cognitive Function". In: *Science* 341.6149, pp. 976–980.

Materu, Jacqueline, Evodius Kuringe, Daniel Nyato, Anthony Galishi, Amasha Mwanamsangu, Maligo Katebalila, Amani Shao, John Changalucha, Soori Nnko, and Mwita Wambura (2020). "The Psychometric Properties of PHQ-4 Anxiety and Depression Screening Scale among Out of School Adolescent Girls and Young Women in Tanzania: A Cross-Sectional Study". In: *BMC Psychiatry* 20.1, p. 321.

Mensch, Barbara S., Paul C. Hewett, and Annabel S. Erulkar (2003). "The Reporting of Sensitive Behavior by Adolescents: A Methodological Experiment in Kenya". In: *Demography* 40.2, pp. 247–268.

Michielsen, Kristien, Matthew F Chersich, Stanley Luchters, Petra De Koker, Ronan Van Rossem, and Marleen Temmerman (2010). "Effectiveness of HIV prevention for youth in sub-Saharan Africa: systematic review and meta-analysis of randomized and nonrandomized trials". In: *Aids* 24.8, pp. 1193–1202.

Mve, Jean Patrick (2021). "Revisiting the Causes and Meaning of Higher Education Massification in Sub-Saharan Africa: Evidence from Cameroon". In: *Open Journal of Social Sciences* 9.4, pp. 188–211. DOI: 10.4236/jss.2021.94015. URL: https://doi.org/10.4236/jss.2021.94015.

Nekoei, Arash and Andrea Weber (2017). "Does Extending Unemployment Benefits Improve Job Quality?" In: *American Economic Review* 107.2, pp. 527–561.

Niehaus, Paul and Tavneet Suri (2026). "Cash Transfers". In: *The Handbook of Social Protection*. Ed. by Benjamin A. Olken and Rema Hanna. Forthcoming. Cambridge, MA: MIT Press.

Patel, Grishma, Mirella Torresan, Shirley Eng, Nicole Acero, Anna Chessky, and Veronica Ades (2026). "Undergraduate students sugar dating in the US demonstrate greater financial need and a history of several adverse childhood experiences". In: *Journal of American College Health*, pp. 1–14.

Poll, Moritz (2026). "Poverty Alleviation at High Frequency". Job Market Paper, Brown University.

Robinson, Jonathan and Ethan Yeh (2011). "Transactional Sex as a Response to Risk in Western Kenya". In: *American Economic Journal: Applied Economics* 3.1, pp. 35–64.

Schilbach, Frank, Heather Schofield, and Sendhil Mullainathan (2016). "The Psychological Lives of the Poor". In: *American Economic Review* 106.5, pp. 435–440.

Stamatakis, Caroline, Ashleigh Howard, Laura Chiang, Greta M Massetti, Rose Apondi, Kirsten Stoebenau, Jennifer Hegle, Lydia Wasula, and Pragna Patel (2021). "Regional heterogeneity in violence and individual characteristics associated with recent transactional sex among Ugandan girls and young women: A national and regional analysis of data from the Violence Against Children and Youth Survey". In: *PLoS one* 16.9, e0257030.

Stoebenau, Kirsten, Lori Heise, Joyce Wamoyi, and Natalia Bobrova (2016). "Revisiting the Understanding of "Transactional Sex" in Sub-Saharan Africa: A Review and Synthesis of the Literature". In: *Social Science & Medicine* 168, pp. 186–197.

Tauchmann, Harald (2014). "Lee (2009) treatment-effect bounds for nonrandom sample selection". In: *The Stata Journal* 14.4, pp. 884–894.

UNAIDS (Joint United Nations Programme on HIV/AIDS) (2021). *UNAIDS Data 2021*. Tech. rep. Geneva: UNAIDS. URL: https://www.unaids.org/sites/default/files/media_asset/JC3032_AIDS_Data_book_2021_En.pdf (visited on 07/29/2026).

Vollmer, Sebastian, Kenneth Harttgen, Tobias Alfven, Jaineetha Padayachy, Peter Ghys, and Till Barnighausen (2017). "The HIV Epidemic in Sub-Saharan Africa is Aging: Evidence from the Demographic and Health Surveys in Sub-Saharan Africa". In: *AIDS and Behavior* 21, pp. 101–113.

Walque, Damien de, William H. Dow, Rose Nathan, Ramadhani Abdul, Faraji Abilahi, Erick Gong, Carol Medlin, Andy Stergachis, Michael D. Sweat, Catharina Hjortsberg, and Uma Ramakrishnan (2012). "Incentivising Safe Sex: A Randomised Trial of Conditional Cash Transfers for HIV and Sexually Transmitted Infection Prevention in Rural Tanzania". In: *BMJ Open* 2.1, e000747.

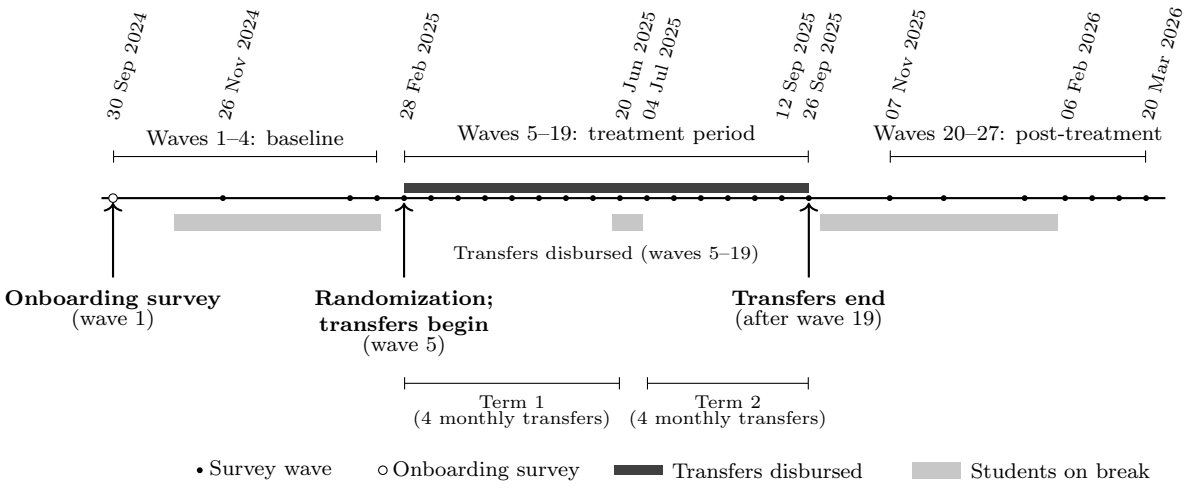
Wamoyi, Joyce, Kirsten Stoebenau, Natalia Bobrova, Tanya Abramsky, and Charlotte Watts (2016). "Transactional Sex and Risk for HIV Infection in Sub-Saharan Africa: A Systematic Review and Meta-Analysis". In: *Journal of the International AIDS Society* 19.1, p. 20992.

Zambia National Assembly (2025). *Profile (Node 297)*. https://www.parliament.gov.zm/node/297. Accessed: 2025-12-09.

Zambia Statistics Agency, Ministry of Health (MoH) [Zambia], and ICF (2024). *Zambia Demographic and Health Survey 2024*. Tech. rep. Lusaka, Zambia and Rockville, Maryland, USA: Zambia Statistics Agency, MoH, and ICF. URL: https://www.zamstats.gov.zm/wp-content/uploads/2025/11/Zambia-Demographic-and-Health-Survey-2024.pdf.

# A Appendix Figures

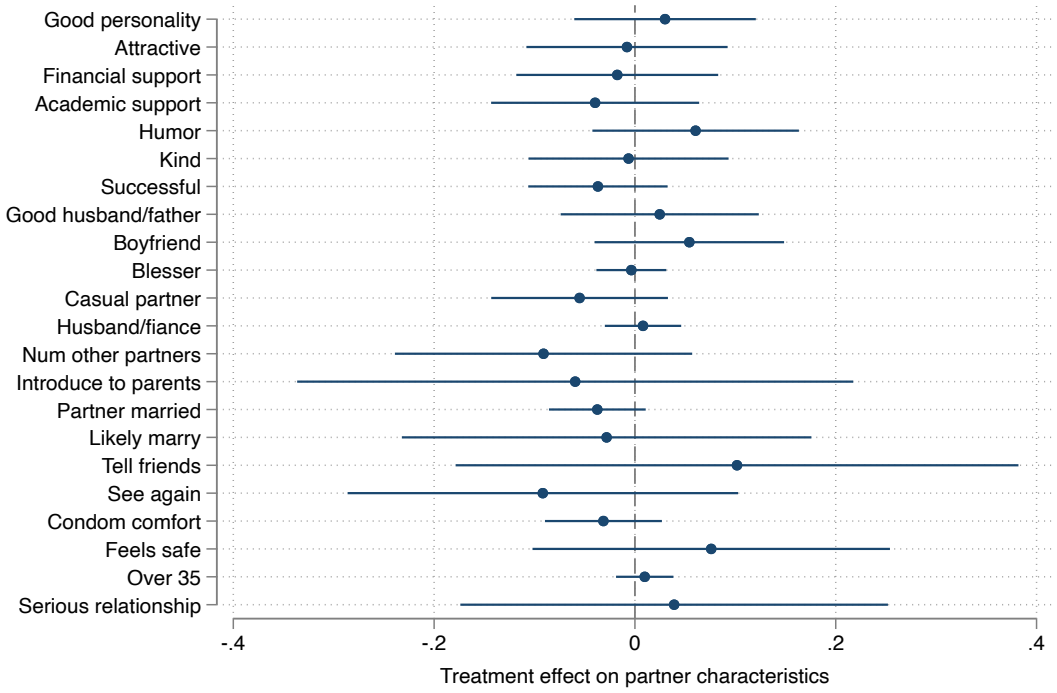
**Figure A1: Study Timeline**



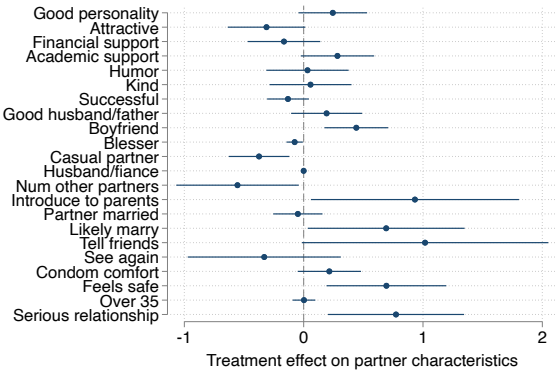

**Notes:** Each dot marks one of the 27 survey waves, sent roughly every two weeks while students were on campus and less frequently during school breaks. Waves 1–4 (September 2024 – February 2025) were completed before randomization and serve as baseline surveys. Waves 5–19 (late February – mid-September 2025) fall within the period in which transfers were disbursed; the dark bar marks the eight monthly transfers, four in each term. Waves 20–27 (late September 2025 – March 2026) are post-treatment. Grey bars mark periods when students were away from campus on school break.

## Figure A2: Partner Quality Components

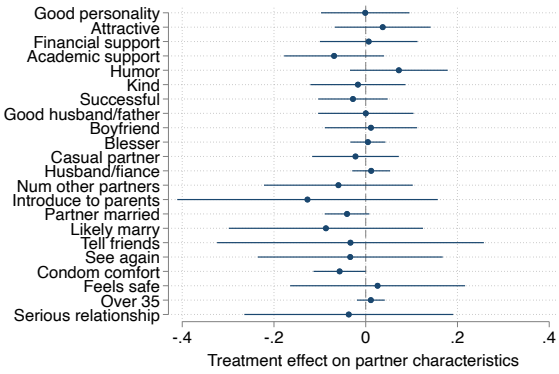
### (a) Full Sample




### (b) No Sex at BL

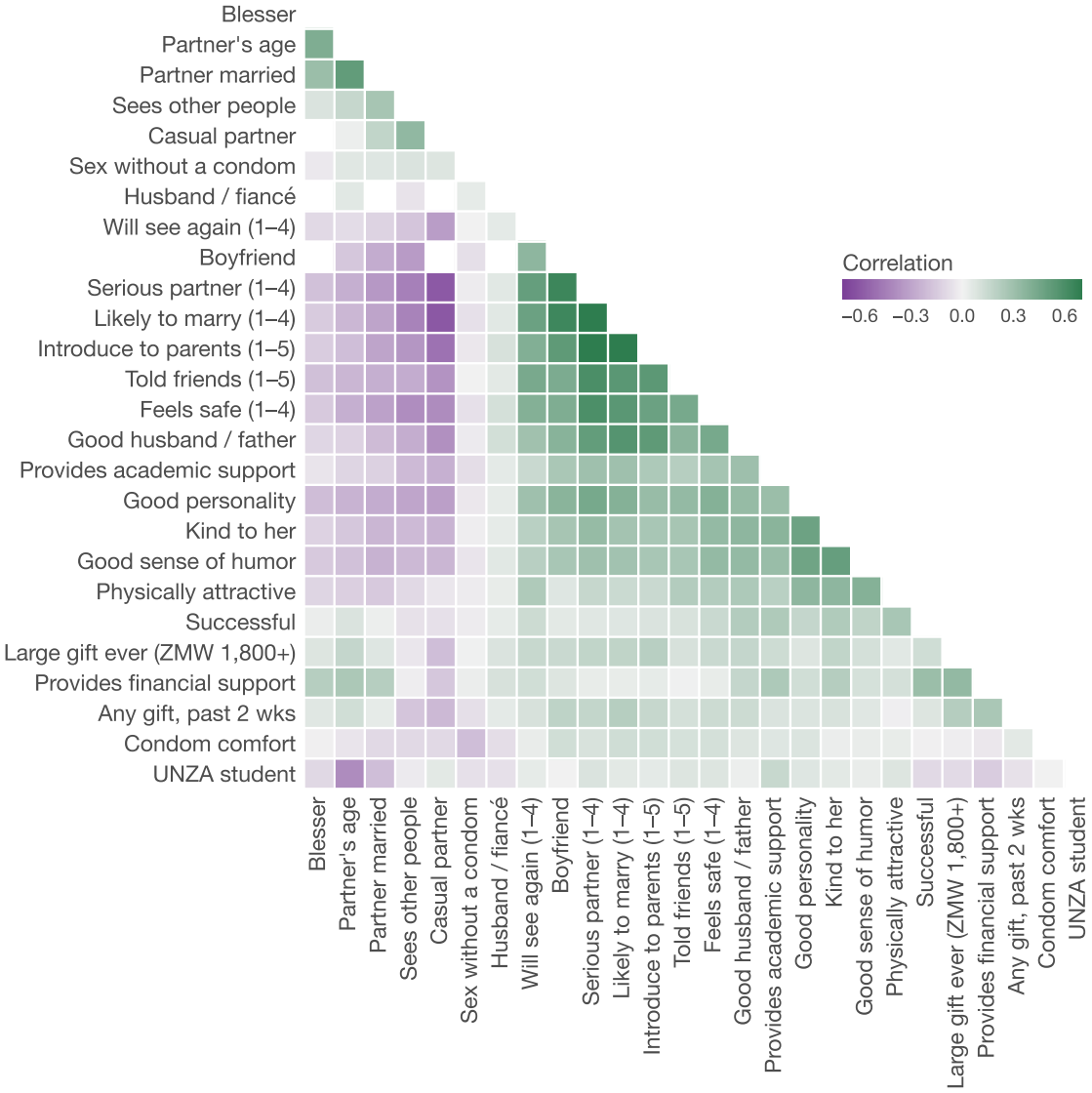



### (c) Sex at BL



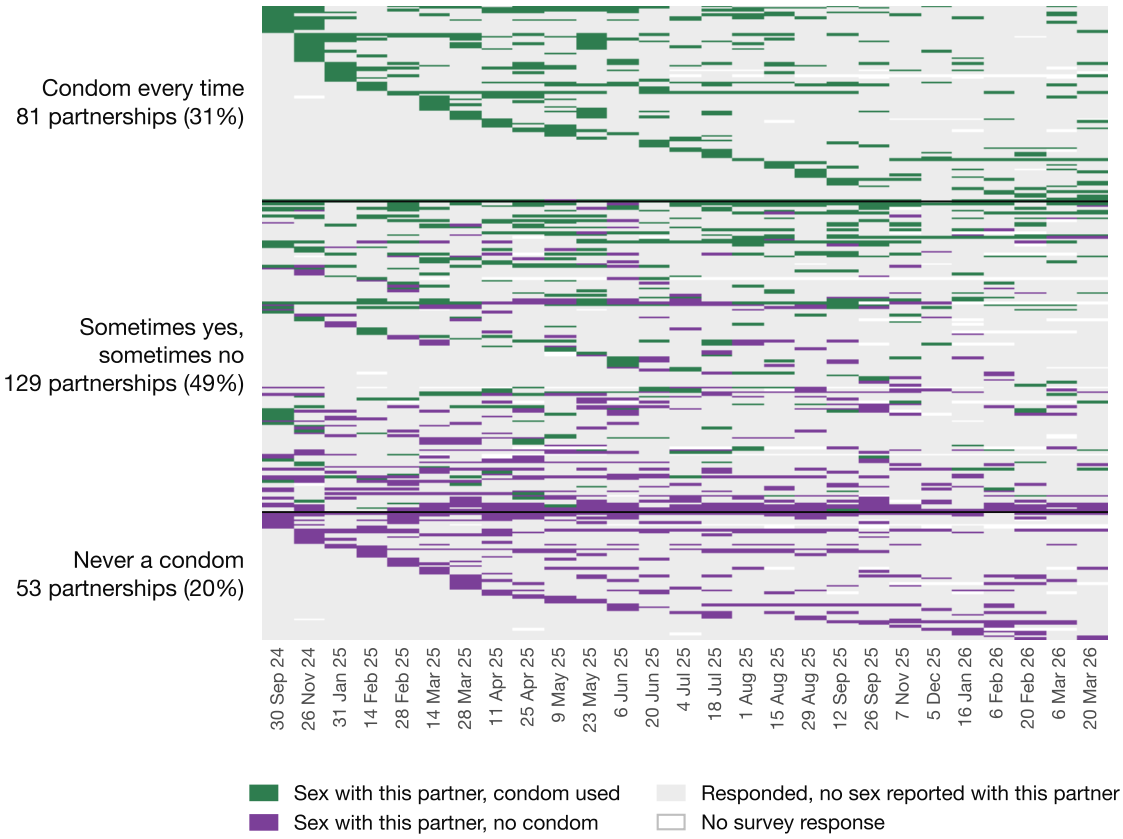

**Notes:** This figure reports the impact of treatment on each component in the partner quality index (each component is fully described in Appendix D), conditional on encounter and broken out by participants who had not had sex at baseline and those who had.

**Figure A3: Partner Characteristic Correlations**



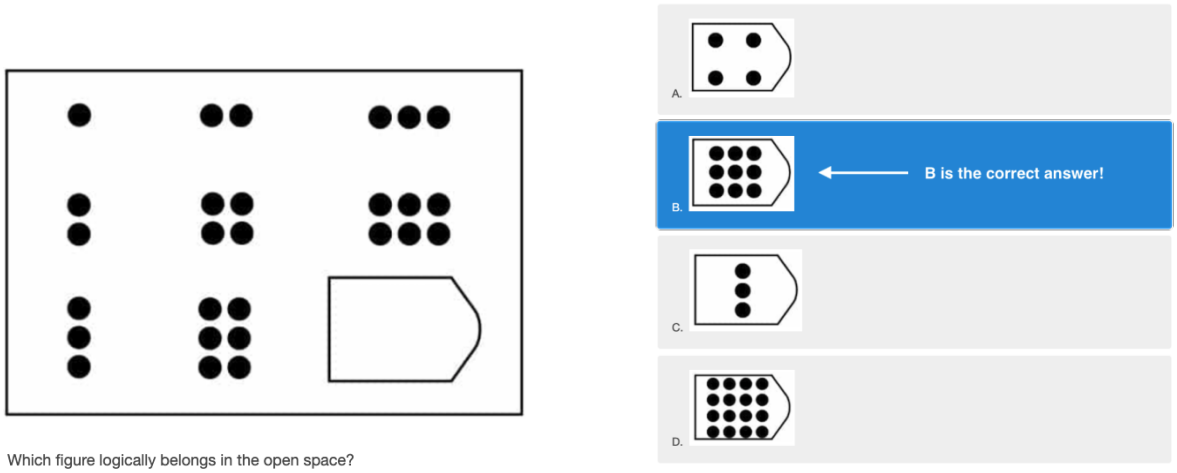

**Notes:** This figure reports pairwise Pearson correlations among partner characteristics, with one observation per partnership (a participant–partner pair linked across surveys by the partner's nickname), pooling treatment and control. The sample is the 932 partnerships for which the participant reported partner characteristics. Partner type, age, marital status, student status, whether he sees other people, the qualities the participant likes about him, and her expectations for the relationship are taken from the first survey in which the partner was listed. Feeling safe, receipt of a gift in the past two weeks, and condomless sex are averaged over all of that partner's encounters. "Serious partner," "likely to marry," "will see again," and "feels safe" are coded 1 to 4; "told friends" and "introduce to parents" are coded 1 to 5, where 5 is "I already have." "Partner married" includes "I don't know." "Large gift ever" indicates that the partner had given a single gift worth ZMW 1,800 or more (the 75th percentile of partner gifts) as of his first listing. Rows and columns are ordered by hierarchical clustering (average linkage on one minus the correlation) so that related characteristics appear together; only the lower triangle is shown. Blank cells are pairs for which the correlation is not meaningful: the four partner-type indicators are mutually exclusive by construction, and marital status is not asked about husbands and fiancés. Green denotes positive and plum negative correlations.

**Figure A4: Condom Use Within Partnerships (Control Group)**



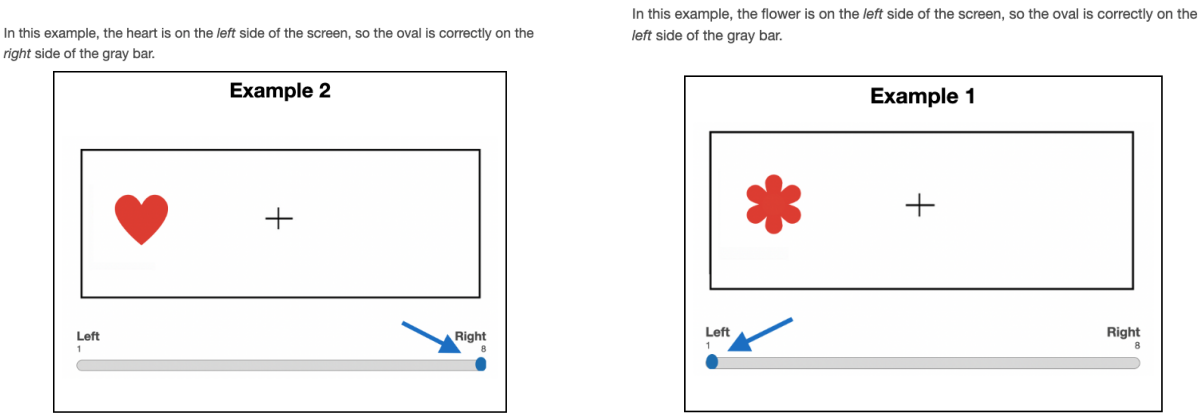

**Notes:** This figure shows condom use over time within partnerships for control-group participants. Each row is one partnership reported in at least two survey waves (N = 263); each column is one of the 27 survey waves, labeled by the date the survey was sent. Surveys were sent every two weeks while students were on campus and every four weeks during breaks. A green cell indicates that the participant reported sex with that partner in the two weeks before the survey and that a condom was used; a plum cell indicates sex with that partner without a condom, based on the contraceptive methods reported for that partner in that wave. A light grey cell indicates that the participant completed the survey but did not report sex with that partner; a white cell indicates no survey response. Partnerships are grouped by whether a condom was used at every reported encounter (81 partnerships, 31 percent), at some but not all (129, 49 percent), or at none (53, 20 percent); within the middle group, rows are sorted by the share of encounters without a condom. Encounters with missing condom information are excluded.

## Figure A5: Visual Reasoning Example



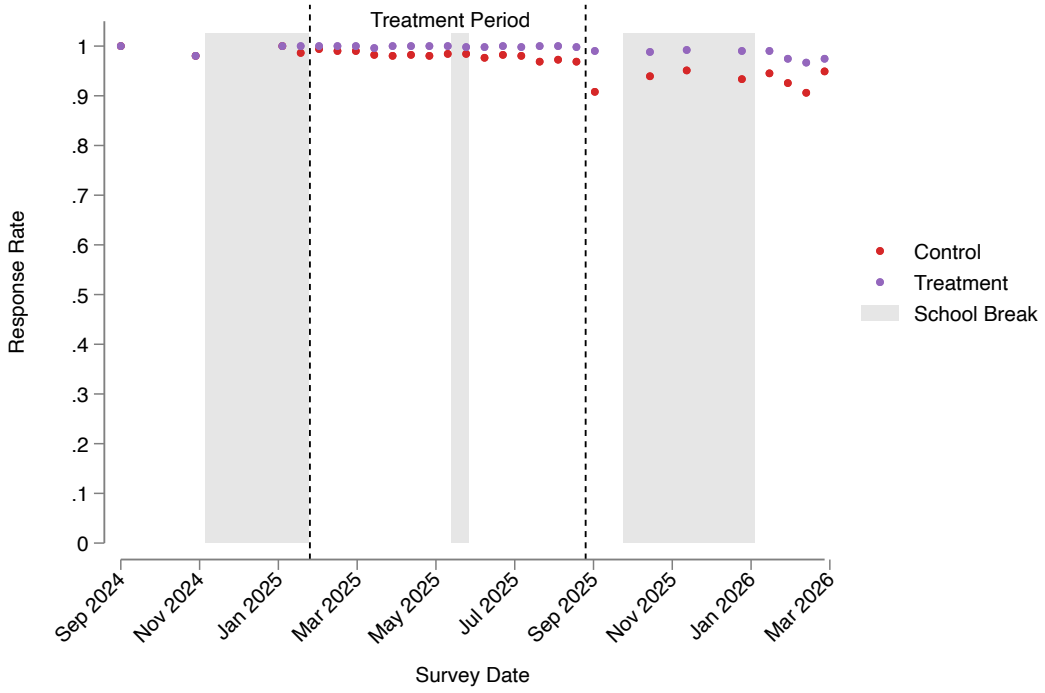

**Notes:** This is an example problem shown to participants to explain the Raven's Progressive Matrices questions, which were asked to measure fluid reasoning. Participants are asked to choose the most logical matrix, given the pattern laid out in the prompt.

## Figure A6: Cognitive Control Example



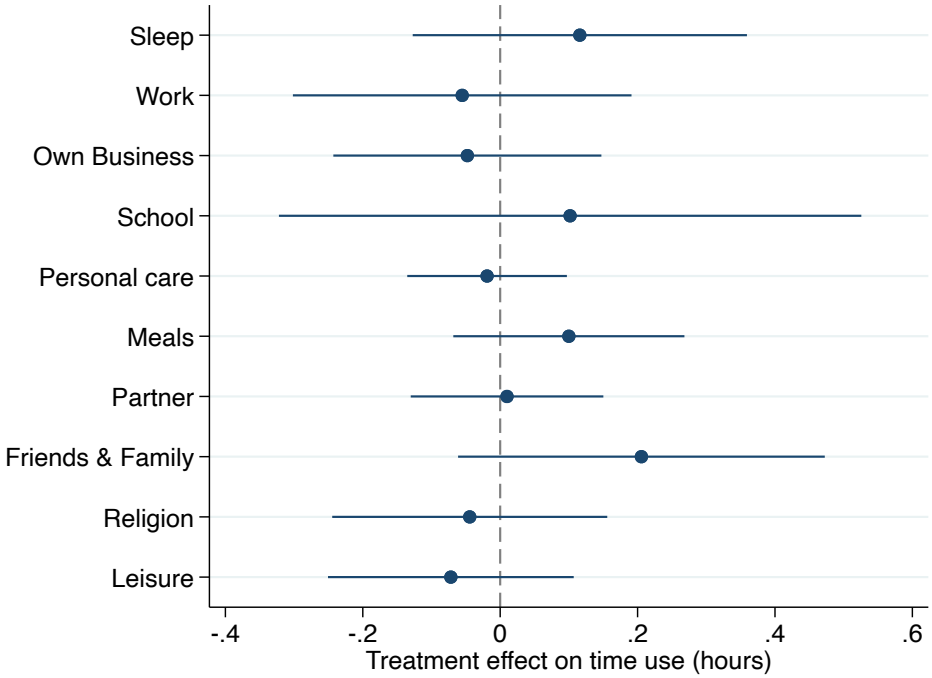

**Notes:** These are the example questions given to participants to explain the Hearts and Flowers task used to measure cognitive control (Davidson et al., 2006). Participants are asked to click a certain side of the screen either aligning or opposite the heart or flower, depending on which appear on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

**Figure A7: 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.

**Figure A8: Effect of Treatment on Time Use**




**Notes:** This figure reports the impact of treatment on time use (in hours) by category.

# B Appendix Tables

**Table B1: 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*** | -0.189*** | -0.062 | 0.184*** | 0.210*** |
| | (0.024) | (0.024) | (0.037) | (0.039) | (0.070) | (0.062) |
| Post-Treated | 0.033 | -0.014 | -0.050 | 0.013 | | |
| | (0.027) | (0.026) | (0.045) | (0.045) | | |
| Participant FE | Y | Y | Y | Y | Y | Y |
| Survey FE | Y | Y | Y | Y | Y | Y |
| Control Mean | 0.565 | 0.523 | 2.765 | 2.765 | 0.010 | 0.042 |
| Control SD | | | 0.981 | 0.989 | 1.008 | 0.976 |
| N | 14973 | 14963 | 16034 | 16035 | 3040 | 3036 |

**Notes:** This table reports the same estimates as Table 6 but includes the "Post-Treated" effect for mental health outcomes (visual reasoning and cognitive control were not measured in the post-treatment period). "Post-Treated" equals one for participants in the treatment group after transfers have ended (Survey 20 onward).

**Table B2: Effect of Financial Assistance on Biweekly Sexual Behavior: Lee Bounds**

| | Any Sex | Condomless Sex | Reported Financially Motivated | Not Proud |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Full Sample** | | | | |
| Treated | -0.0073 | -0.018** | -0.0012 | -0.061*** |
| | (0.012) | (0.0088) | (0.0084) | (0.017) |
| | [-0.016, -0.0060] | [-0.032, -0.018] | [-0.013, -0.00080] | [-0.069, -0.060] |
| Post-treated | -0.0070 | -0.012 | -0.0051 | -0.027 |
| | (0.014) | (0.010) | (0.0093) | (0.019) |
| | [-0.039, -0.0035] | [-0.045, -0.010] | [-0.029, -0.0040] | [-0.053, -0.023] |
| Control Mean | 0.118 | 0.057 | 0.042 | 0.177 |
| N | 27051 | 26997 | 27002 | 16044 |
| **Never Sex at Baseline** | | | | |
| Treated | -0.032*** | -0.017*** | -0.014** | -0.078*** |
| | (0.010) | (0.0056) | (0.0066) | (0.021) |
| | [-0.041, -0.031] | [-0.022, -0.017] | [-0.017, -0.014] | [-0.086, -0.078] |
| Post-treated | -0.032*** | -0.024*** | -0.018** | -0.051** |
| | (0.012) | (0.0068) | (0.0078) | (0.024) |
| | [-0.043, -0.031] | [-0.027, -0.023] | [-0.018, -0.018] | [-0.081, -0.047] |
| Control Mean | 0.047 | 0.019 | 0.019 | 0.151 |
| N | 13727 | 13707 | 13708 | 8137 |
| **Sex at Baseline** | | | | |
| Treated | 0.018 | -0.020 | 0.012 | -0.042 |
| | (0.022) | (0.017) | (0.016) | (0.026) |
| | [0.013, 0.019] | [-0.027, -0.019] | [0.0058, 0.013] | [-0.047, -0.041] |
| Post-treated | 0.017 | -0.00027 | 0.0082 | -0.000015 |
| | (0.024) | (0.020) | (0.017) | (0.030) |
| | [-0.0024, 0.022] | [-0.023, 0.0014] | [-0.013, 0.010] | [-0.015, 0.0042] |
| Control Mean | 0.191 | 0.096 | 0.067 | 0.203 |
| N | 13246 | 13213 | 13216 | 7861 |
| Participant FE | Y | Y | Y | Y |
| Survey FE | Y | Y | Y | Y |

**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 20 onward).

## Table B3: 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 20 onward). The post-treatment sample is included.

## Table B4: Effect of Financial Assistance on Biweekly Sexual Behavior: Attention

| | All Respondents | | | |
|---|---|---|---|---|
| | Any Sex | Condom-less Sex | Reported Financially Motivated | Not Proud |
| | (1) | (2) | (3) | (4) |
| Treated | -0.0085 | -0.020** | -0.0021 | -0.067*** |
| | (0.013) | (0.0092) | (0.0089) | (0.017) |
| Post-treated | -0.0081 | -0.013 | -0.0073 | -0.025 |
| | (0.014) | (0.011) | (0.010) | (0.020) |
| Participant FE | Y | Y | Y | Y |
| Survey FE | Y | Y | Y | Y |
| Control Mean | 0.13 | 0.063 | 0.047 | 0.18 |
| N | 24497 | 24445 | 24450 | 14569 |

| | Never Sex at Baseline | | | | Sex at Baseline | | | |
|---|---|---|---|---|---|---|---|---|
| | Any Sex | Condom-less Sex | Reported Financially Motivated | Not Proud | Any Sex | Condom-less Sex | Reported Financially Motivated | Not Proud |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treated | -0.036*** | -0.018*** | -0.018** | -0.083*** | 0.020 | -0.021 | 0.014 | -0.050* |
| | (0.011) | (0.0058) | (0.0073) | (0.023) | (0.023) | (0.018) | (0.016) | (0.026) |
| Post-treated | -0.037*** | -0.026*** | -0.022*** | -0.053** | 0.021 | 0.00052 | 0.0079 | 0.0062 |
| | (0.013) | (0.0072) | (0.0086) | (0.025) | (0.026) | (0.021) | (0.018) | (0.031) |
| Participant FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Survey FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Control Mean | 0.052 | 0.022 | 0.021 | 0.15 | 0.21 | 0.11 | 0.073 | 0.20 |
| N | 12368 | 12348 | 12349 | 7329 | 12051 | 12020 | 12023 | 7194 |

**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 20 onward). The post-treatment sample is included.

## Table B5: Effect of Financial Assistance on Encounter-Level Outcomes during Treatment and Post-Treatment

| Panel A: Biweekly Outcomes | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Never Sex at Baseline | | | | Sex at Baseline | | | |
| | Any Sex | Condom-less Sex | Reported Financially Motivated | Not Proud | Any Sex | Condom-less Sex | Reported Financially Motivated | Not Proud |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treated | -0.032*** | -0.017*** | -0.014** | -0.078*** | 0.018 | -0.020 | 0.012 | -0.042 |
| | (0.010) | (0.0056) | (0.0066) | (0.021) | (0.022) | (0.017) | (0.016) | (0.026) |
| Post-Treated | -0.032*** | -0.024*** | -0.018** | -0.051** | 0.017 | -0.00027 | 0.0082 | -0.000015 |
| | (0.012) | (0.0068) | (0.0078) | (0.024) | (0.024) | (0.020) | (0.017) | (0.030) |
| Participant FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Survey FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Control Mean | 0.047 | 0.019 | 0.019 | 0.15 | 0.19 | 0.096 | 0.067 | 0.20 |
| N | 13727 | 13707 | 13708 | 8137 | 13246 | 13213 | 13216 | 7861 |
| Clusters | 521 | 521 | 521 | 521 | 498 | 498 | 498 | 498 |

| Panel B: Encounter-Level Outcomes | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Never Sex at Baseline | | | | Sex at Baseline | | | |
| | Used Condom | Large Gift | Partner Quality | Felt Safe | Used Condom | Large Gift | Partner Quality | Felt Safe |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (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 20 onward). The post-treatment sample is included.

## Table B6: Effect of Financial Assistance on Self-Reported Household SES

| | | SES Relative to... | | |
|---|---|---|---|---|
| | HH Poverty | Classmates | Own Village | Zambia |
| | (1) | (2) | (3) | (4) |
| Treatment Status | 0.027 | -0.053 | -0.028 | -0.058 |
| | (0.037) | (0.052) | (0.044) | (0.049) |
| Strata FE | Y | Y | Y | Y |
| Control Mean | 1.867 | 2.499 | 2.740 | 2.527 |
| N | 965 | 963 | 963 | 963 |

**Notes:** This table reports coefficients of a regression of self-reported household SES on treatment status. All four of these questions were asked on survey 18, which occurred in August 2025. Household poverty (column 1) ranges from 1 (not poor) to 3 (very poor). Relative SES (columns 2-4) ranges from 1 (among the poorest) to 5 (among the richest). All regressions include strata fixed effects. Robust standard errors reported in parentheses.

**Table B7: Effect of Financial Assistance on Reported Abortions**

| | All Respondents | | Never Sex at Baseline | | Sex at Baseline | |
|---|---|---|---|---|---|---|
| | Abortion | Abortion (if Pregnant) | Abortion | Abortion (if Pregnant) | Abortion | Abortion (if Pregnant) |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treatment Status | -0.0002 | 0.1019 | -0.0015 | 0.0825 | 0.0011 | 0.1059 |
| | (0.0009) | (0.0773) | (0.0012) | (0.1588) | (0.0013) | (0.0874) |
| Strata FE | Y | Y | Y | Y | Y | Y |
| Control Mean | 0.0022 | 0.2397 | 0.0019 | 0.3611 | 0.0024 | 0.1882 |
| N | 27072 | 221 | 13739 | 51 | 13252 | 170 |

**Notes:** This table measures the effect of treatment status on reporting an abortion, which was only asked about if participants reported a pregnancy. Columns 1, 3, and 5 report unconditional treatment effects, and 2, 4, and 6 are conditional on pregnancy. All regressions include strata fixed effects. Robust standard errors reported in parentheses.

# C Proofs

## A.1 Notation

Before presenting the proofs, we introduce some additional notation used throughout this appendix section. First, write the flow surpluses in a match with a man of income $i \in \{L, H\}$ and quality $q_t$ as

$$x(g_t, \rho_t) \equiv v(y_t + g_t) - v(y_t) + s(q_t) + \mu\rho_t, \qquad z(g_t, \rho_t) \equiv v(i - g_t) - v(i) + S - \lambda\rho_t,$$

so that $\sigma_f = x + \Delta_f$ and $\sigma_m = z + \Delta_m$. Second, define $N(y_t) \equiv \sigma_f(g^*, \rho^*)\,\sigma_m(g^*, \rho^*)$ for the maximized Nash product. Then, the Kuhn–Tucker conditions of the bargaining problem (8) are

$$(g) \qquad v'(y_t + g_t)\,\sigma_m - v'(i - g_t)\,\sigma_f \ \{\leq, =, \geq\}\ 0 \quad \text{as } g_t \ \{= 0,\ \in (0, i),\ = i\}, \tag{10}$$

$$(\rho) \qquad \mu\,\sigma_m - \lambda\,\sigma_f \ \{\leq, =, \geq\}\ 0 \quad \text{as } \rho_t \ \{= 0,\ \in (0, 1),\ = 1\}. \tag{11}$$

Third, let $m(y^0) \geq 0$ denote the value of her best search option at baseline,

$$m(y^0) \equiv \max\left\{\mathbb{E}_G \max\{\sigma^G, 0\},\ \ \mathbb{E}_R \max\{\sigma^R, 0\} - \kappa,\ \ 0\right\},$$

where $\sigma^G$ and $\sigma^R$ are her bargained surpluses in general-market and high-income matches at baseline, and the expectations are taken over the quality draw $q$. The second branch carries no $\max\{\cdot, 0\}$ because every high-income meeting is accepted under Assumption 1, and there is no third branch because general-market search is costless and any meeting may be rejected, so not searching is never strictly preferred.

Finally, for a match between a woman with baseline income $y^0$ and a man $(i, q)$, let $(g^0, \rho^0)$ denote the *baseline terms*: the terms the couple bargains to in a period in which the woman is untreated, so that $y_t = y^0$. Equivalently, these are the terms a couple would bargain in the untreated counterfactual.

## A.2 Preliminaries

**Lemma 1** (Properties of the bargaining problem)**.** *Fix a match $(i, q_t)$ and an income $y_t$.*

*(i)* Uniqueness. *The log objective is strictly concave on $\{\sigma_f > 0,\ \sigma_m > 0\}$, so (8) has a unique solution $(g^*, \rho^*)$, and (10)–(11) are necessary and sufficient for it.*

*(ii)* Envelope. *On any interval of $y_t$ over which the set of binding constraints is unchanged, $N$ is differentiable and*

$$N'(y_t) = \sigma_m(g^*, \rho^*)\left[v'(y_t + g^*) - v'(y_t)\right]. \tag{12}$$

*Proof.* (i) Each $\sigma$ is concave: it is strictly concave in $g_t$ because $v'' < 0$, and affine in $\rho_t$ (and this remains true under the log). Hence the log objective is strictly concave, so the objective is strictly concave on the region and (8) has at most one solution. Existence holds because $N$ is continuous on the compact set $[0, i] \times [0, 1]$. By Assumptions 1 and since $Q$ is atomless, $N > 0$ at any maximizer, so both surplus constraints are slack and only the constraints $g \in [0, i]$ and $\rho \in [0, 1]$ can bind. These are affine, so the linear constraint qualification holds and (10)–(11) are necessary and sufficient (by concavity of $\sigma$).

(ii) Fix an interval of $y_t$ on which the same constraints bind. Income enters the problem only through

$\sigma_f$, and only through the term $v(y_t + g_t) - v(y_t)$, so

$$\left.\frac{\partial \sigma_f}{\partial y_t}\right|_{g_t} = v'(y_t + g_t) - v'(y_t), \qquad \left.\frac{\partial \sigma_m}{\partial y_t}\right|_{g_t} = 0.$$

Differentiating $N = \sigma_f \sigma_m$ and applying the envelope theorem, the terms in $\partial g^*/\partial y_t$ and $\partial \rho^*/\partial y_t$ drop out: for a choice variable that is interior, the corresponding first-order condition in (10)–(11) holds with equality, and for one at a corner, it is locally constant in $y_t$ on the interval. What remains is (12). □

**Lemma 2** (Existence, Uniqueness, Continuity, and Stationarity)**.** *The values $W^S(y^0)$ and $W^M(y^0; i, q)$ exist, are unique, and are continuous in $y^0$, and the optimal retention and search policies are stationary. Consequently $m(y^0)$, $\Delta_f$, $\Delta_m$, $\sigma^G$, $\sigma^R$, and $\Pi(y_t; y^0)$ are well defined and continuous in all of their arguments. Moreover, $\Pi(y_t; y^0)$ is strictly decreasing in $y_t$.*

*Proof.* Since $v$ is continuously differentiable, $s$ is continuous on $\operatorname{supp} Q$, and $Q$ and $Y$ have compact support, the per-period payoffs are continuous and bounded on a compact domain. Hence the Bellman operator $T$, defined by

$$(TW)(y^0) \;=\; v(y^0) \;+\; \max\Big\{\mathbb{E}_G \max\{\sigma^G, 0\},\;\; \mathbb{E}_R\, \sigma^R - \kappa\Big\} \;+\; \beta\, W(y^0),$$

maps the space of bounded continuous functions of $y^0$ into itself. It is monotone —the value enters the bargained surpluses only through the difference $\Delta_f = \beta(W^M - W^S)$, which a uniform increase in $W$ leaves unchanged—and discounts at rate $\beta < 1$, hence is a contraction by Blackwell's sufficient conditions. Its fixed point is therefore unique and continuous, and since the problem is stationary—the environment, the distributions $Q$ and $Y$, and $\tau_t$ do not depend on calendar time—the optimal policies attaining it are stationary as well. Uniqueness of the fixed point delivers a unique $m(y^0)$. Continuity of $m(y^0)$ follows because the maximum of finitely many continuous functions is continuous and because $\mathbb{E}\max\{\sigma, 0\}$ is continuous by dominated convergence, the integrand being continuous and bounded. Continuity of $\sigma^G$ and $\sigma^R$ in the remaining arguments follows from the maximum theorem applied to (8), whose constraint correspondence is continuous and compact-valued—nonempty-valued and lower hemicontinuous by Assumption 1 for rich matches and by strict feasibility for ordinary matches that form—and whose objective is continuous; $\Pi$ is then a continuous transformation of continuous functions.

Finally, $\Pi$ is strictly decreasing in $y_t$: $\sigma^G$ does not depend on $y_t$ by Proposition 1(a), while $\sigma^R$ is strictly decreasing in $y_t$ by Lemma 6(ii), so the difference in (9) is strictly decreasing and the expectation over $q$ preserves this. □

**Lemma 3** (Stationary values at baseline)**.** *Fix a woman with baseline income $y^0$ and a match $(i, q)$. The match is retained at baseline if and only if $x^0 \geq \beta m(y^0)$, where $(g^0, \rho^0)$ solve*

$$\max_{g \geq 0,\ \rho \in [0,1]} \big[x(g,\rho) - \beta m(y^0)\big]\, z(g,\rho) \quad \textit{subject to} \quad x(g,\rho) \geq \beta m(y^0),\ z(g,\rho) \geq 0, \tag{13}$$

*and $x^0 \equiv x(g^0, \rho^0)$, $z^0 \equiv z(g^0, \rho^0)$. In that case the continuation gains are*

$$\Delta_f = \frac{\beta\left[x^0 - m(y^0)\right]}{1-\beta}, \qquad \Delta_m = \frac{\beta z^0}{1-\beta},$$

*and the woman's baseline bargained surplus is $\left[x^0 - \beta m(y^0)\right]/(1-\beta)$.*

*Proof.* A woman who enters a period single receives $v(y^0)$ this period. If she matches, she receives in addition her surplus $\sigma_f = x + \Delta_f$ this period, where $\Delta_f$ already accounts for the difference in continuation values; if she does not, she enters the next period single. Hence $W^S = v(y^0) + \beta W^S + m(y^0)$, so that

$$W^S(y^0) = \frac{v(y^0) + m(y^0)}{1-\beta}. \tag{14}$$

If the match is retained in every period, stationarity gives $W^M = v(y^0) + x^0 + \beta W^M$, so $W^M = \left[v(y^0) + x^0\right]/(1-\beta)$. If instead she would dissolve the match next period, $W^M = W^S$. Retention next period is worth $\left[v(y^0) + x^0\right]/(1-\beta)$ and dissolution is worth $v(y^0) + \beta W^S = \left[v(y^0) + \beta m(y^0)\right]/(1-\beta)$ by (14), so she retains if and only if $x^0 \geq \beta m(y^0)$. Subtracting (14) gives $\Delta_f$. For the man, $W_m^M = \left[v(i) + z^0\right]/(1-\beta)$ and $W_m^S = v(i)/(1-\beta)$, since he does not search; subtracting gives $\Delta_m$.

Substituting into (6) and (7), at the baseline terms the surpluses are $\sigma_f = \left[x^0 - \beta m(y^0)\right]/(1-\beta)$ and $\sigma_m = z^0/(1-\beta)$. The Kuhn–Tucker conditions (10)–(11) of (8) at $(g^0, \rho^0)$ are homogeneous of degree one in $(\sigma_f, \sigma_m)$, so they coincide with the Kuhn–Tucker conditions of (13) at $(g^0, \rho^0)$. Both problems are strictly log-concave, so $(g^0, \rho^0)$ is the unique solution of each by Lemma 1(i). The feasibility conditions coincide by the same substitution. □

**Lemma 4** (Treated periods)**.** *In a treated period, $\Delta_f$ and $\Delta_m$ in (6)–(7) are the baseline objects of Lemma 3, and a meeting accepted in the current period is retained from the next period on at the baseline terms.*

*Proof.* Treatment enters the bargaining problem only through the flow term $v(y_t + g_t) - v(y_t)$ in $\sigma_f$; because the shock is unanticipated and lasts one period, the continuation values are those of an untreated match, so $\Delta_f$ and $\Delta_m$ are unchanged. From the next period on the couple faces the untreated problem, whose unique solution is $(g^0, \rho^0)$ by Lemma 1(i). □

## A.3 Equilibrium

**Lemma 5** (Ordinary men)**.** *In a match with an ordinary man, $g^* = 0$ always.*

*Proof.* Suppose $g^* > 0$ and $\rho^* < 1$. Reduce the transfer by $\epsilon > 0$ and raise the condom share by $v'(L - g^*)\,\epsilon/\lambda$; to first order this holds $\sigma_m$ fixed and remains feasible for small $\epsilon$. The change in $\sigma_f$ is

$$\left[-v'(y_t + g^*) + \frac{\mu}{\lambda} v'(L - g^*)\right]\epsilon.$$

Since $L - g^* < L$ and $y_t + g^* > y_t \geq \underline{y}$, concavity gives $\mu v'(L - g^*) > \mu v'(L) \geq \lambda v'(\underline{y}) > \lambda v'(y_t + g^*)$, so the bracket is strictly positive by Assumption 2(a). The Nash product rises, contradicting optimality. □

**Lemma 6** (Rich men)**.** *Consider a match with a rich man ($i = H$) at a current income $y_t$. Let $(\hat{g}, \hat{\rho})$ solve (8) with the constraint $\rho \in [0,1]$ removed. Then,*

*(i) $\hat{g} > 0$ is the unique solution of $\lambda v'(y_t + g) = \mu v'(H - g)$. Also $\rho^* = \min\{1, \max\{0, \hat{\rho}\}\}$ and $g^* = \hat{g}$ when $\hat{\rho} \in (0,1)$.*

*(ii) The woman's bargained surplus $\sigma^R(y_t) \equiv \sigma_f(g^*, \rho^*)$ is strictly decreasing in $y_t$. When $\rho^* \in (0,1)$,*

$$\frac{\partial \sigma^R}{\partial y_t} = \frac{1}{2}\left[v'(y_t + g^*) - v'(y_t)\right] < 0. \tag{15}$$

*(iii) When $\rho^* \in (0,1)$, $-1 < \partial g^*/\partial y_t < 0$.*

*(iv) $\hat{\rho}$ is strictly increasing in $y_t$.*

*Proof.* (i) Removing the constraint on $\rho$ leaves a strictly log-concave problem whose objective tends to $-\infty$ as $\sigma_f \to 0$ or $\sigma_m \to 0$, so it has a unique solution, characterized by the first-order conditions $v'(y_t+g)\,\sigma_m = v'(H-g)\,\sigma_f$ and $\mu\,\sigma_m = \lambda\,\sigma_f$. Dividing, $\lambda v'(y_t+g) = \mu v'(H-g)$. At $g=0$ the left side exceeds the right by Assumption 2(b). Hence the solution $\hat{g} > 0$ exists (since $v'(c) \to \infty$ as $c \to 0$) and is unique. Because the constrained problem is a concave program with a single interval constraint on $\rho$, its solution is $\rho^* = \hat{\rho}$ if $\hat{\rho} \in [0,1]$, $\rho^* = 0$ if $\hat{\rho} < 0$, and $\rho^* = 1$ if $\hat{\rho} > 1$. When $\hat{\rho} \in (0,1)$ the two problems coincide, so $g^* = \hat{g}$.

(ii) From (11), $\sigma_f = (\mu/\lambda)\,\sigma_m$ whenever $\rho^* \in (0,1)$, so $N = (\lambda/\mu)\,\sigma_f^2$ and $\sigma_f = \sqrt{\mu N/\lambda}$. Differentiating and using (12),

$$\frac{\partial \sigma_f}{\partial y_t} = \frac{1}{2}\sqrt{\frac{\mu}{\lambda N}}\,N'(y_t) = \frac{1}{2}\sqrt{\frac{\mu}{\lambda N}}\,\sigma_m\left[v'(y_t+g^*) - v'(y_t)\right] = \frac{1}{2}\left[v'(y_t+g^*) - v'(y_t)\right],$$

since $\sqrt{\mu/(\lambda N)} = 1/\sigma_m$ when $\sigma_f = (\mu/\lambda)\sigma_m$. The bracket is negative by concavity and $g^* > 0$ (which holds by Assumption 2(b) and Lemma 6(i) covering the interior case).

Now fix $\rho^* \in \{0,1\}$. (10) still holds with equality: $v'(y_t+g)\,\sigma_m = v'(H-g)\,\sigma_f$. Write $D \equiv v'(y_t+g^*) - v'(y_t) < 0$ and differentiate the equality with respect to $y_t$, using $\partial\sigma_f/\partial y_t|_g = D$ and $\partial\sigma_m/\partial g = -v'(H-g)$, to solve for $\partial g^*/\partial y_t$. Substituting into $\partial\sigma^R/\partial y_t = D + v'(y_t+g^*)\,\partial g^*/\partial y_t$ gives

$$\frac{\partial \sigma^R}{\partial y_t} = D\left[1 - \frac{v'(y_t+g^*)\,v'(H-g^*)}{K}\right] - \frac{v'(y_t+g^*)\,|v''(y_t+g^*)|\,\sigma_m}{K},$$

where $K \equiv |v''(y_t+g^*)|\,\sigma_m + 2v'(y_t+g^*)\,v'(H-g^*) + \sigma_f\,|v''(H-g^*)| > 0$. Since $K > 2v'(y_t+g^*)\,v'(H-g^*)$, the bracket lies in $(1/2,1)$, so the first term is strictly negative and the second is nonpositive.

(iii) In the interior region $g^* = \hat{g}$ solves $\lambda v'(y_t+g) = \mu v'(H-g)$. Differentiating,

$$\frac{\partial g^*}{\partial y_t} = \frac{-\lambda v''(y_t+g^*)}{\lambda v''(y_t+g^*) + \mu v''(H-g^*)} \in (-1,0).$$

since $v'' < 0$.

(iv) Along the unconstrained solution $(\hat{g},\hat{\rho})$, differentiate $\mu\,\sigma_m = \lambda\,\sigma_f$ with respect to $y_t$:

$$\mu\left[-v'(H-\hat{g})\,\frac{\partial \hat{g}}{\partial y_t} - \lambda\frac{\partial \hat{\rho}}{\partial y_t}\right] = \lambda\left[v'(y_t+\hat{g})\left(1+\frac{\partial \hat{g}}{\partial y_t}\right) - v'(y_t) + \mu\frac{\partial \hat{\rho}}{\partial y_t}\right].$$

Using $\mu v'(H-\hat{g}) = \lambda v'(y_t+\hat{g})$ and collecting terms,

$$2\lambda\mu\,\frac{\partial \hat{\rho}}{\partial y_t} = \lambda\left[v'(y_t) - v'(y_t+\hat{g})\right] - 2\lambda v'(y_t+\hat{g})\,\frac{\partial \hat{g}}{\partial y_t}.$$

The first term is strictly positive by concavity and $\hat{g} > 0$; the second is strictly positive by (iii). Hence $\partial\hat{\rho}/\partial y_t > 0$.

*Remark.* In the corner regions the sign of $\partial g^*/\partial y_t$ depends on the relative size of $|v''(y_t+g^*)|\,\sigma_m$ and $|D|\,v'(H-g^*)$ and is not determined by the argument above; Proposition 4 accordingly signs the transfer only in the interior region. □

*Proof of Proposition 1(a).* (ii) With $g = 0$, simplifying (11) gives

$$\rho^* = \min\left\{1,\ \max\left\{0,\ \frac{\mu(S+\Delta_m) - \lambda(s(q_t)+\Delta_f)}{2\lambda\mu}\right\}\right\}, \tag{16}$$

in which $y_t$ and $i$ appear nowhere: the transfer is zero, and by Lemma 3 the continuation gains are the baseline objects $\Delta_f = \beta[x^0 - m(y^0)]/(1-\beta)$ and $\Delta_m = \beta z^0/(1-\beta)$, with $x^0 = s(q_t) + \mu\rho^0$ and $z^0 = S - \lambda\rho^0$.

(i) By Lemma 3, a general-market match is feasible at baseline if and only if there is $\rho \in [0,1]$ with $s(q_t) + \mu\rho \geq \beta m(y^0)$ and $S - \lambda\rho \geq 0$; since $g^* = 0$ and the continuation gains are unchanged in a treated period (Lemma 4), the same condition applies at any $y_t$. The first condition is increasing in $\rho$ and the second decreasing, so feasibility is equivalent to

$$s(q_t) + \mu \min\{1, S/\lambda\} \geq \beta m(y^0). \tag{17}$$

Define $\bar{q}(y^0) \equiv \inf\{q : (17) \text{ holds}\}$; she accepts if and only if $q \geq \bar{q}(y^0)$. □

*Proof of Proposition 1(b).* (i) By Assumption 1, for every $y_t \in [\underline{y}, \overline{y} + \bar{\tau}]$ and every $q$ there is a transfer $g \in (0, H)$ at which $\sigma_f(g,0) > 0$ and $\sigma_m(g,0) > 0$. The constraint set in (8) is therefore nonempty at any income a woman may have, so both parties accept regardless of $q$.

(ii) When $\rho^* \in (0,1)$, Lemma 6(i) gives $g^* = \hat{g}$, which solves $\lambda v'(y_t + g) = \mu v'(H - g)$, and (11) holds with equality, that is, $\mu\,\sigma_m = \lambda\,\sigma_f$. □

*Proof of Proposition 1(c).* Consider a single woman with baseline income $y^0$ at current income $y_t$. In the general market, her current-period payoff plus continuation is $v(y_t) + \beta W^S(y^0) + \mathbb{E}_G \max\{\sigma^G(L, q_t), 0\}$, where $\sigma^G$ is independent of $y_t$ by part (a). If she searches the high-income market, every meeting is accepted by part (b)(i), and her payoff is $v(y_t) - \kappa + \beta W^S(y^0) + \mathbb{E}_R\,\sigma^R(y_t; H, q_t)$. She therefore pays $\kappa$ if and only if $\Pi(y_t; y^0) \geq \kappa$, breaking ties in favor of paying. By Lemma 2, $\Pi$ is continuous and strictly decreasing in $y_t$. The set of incomes at which she pays is therefore an interval $\{y_t \leq \tilde{y}(y^0)\}$, with $\tilde{y}(y^0) \equiv \sup\{y_t : \Pi(y_t; y^0) \geq \kappa\}$ (and $\tilde{y}(y^0) = -\infty$ if the set is empty). □

## A.4 Comparative statics

*Proof of Proposition 2.* A matched woman has a sexual encounter in the current period if and only if she retains her match. In a general-market match, $\sigma$ does not depend on $y_t$ (Proposition 1(a)), so retention is unchanged. In a high-income match, $\sigma^R$ is strictly decreasing in $y_t$ (Lemma 6(ii)), so retention is unchanged by the argument in the proof of Proposition 1(b)(i). A single woman who searches the high-income market has a sexual encounter with probability one; one who searches the general market, with probability $\Pr(q \geq \bar{q}(y^0)) \leq 1$; one who does not search, with probability zero. By Proposition 1(c), an increase in $y_t$ can move a woman from the high-income market to the general market but not the reverse.

Finally, by Proposition 1(a) and the proof of Proposition 1(b)(i)), existing matches persist so the only change in matching is among women single at baseline. □

*Proof of Proposition 3.* Fix a single woman with baseline income $y^0$. A meeting occurs in both markets and the quality draw is $Q$ in both, by independence of $i$ and $q$. In the high-income market every meeting is accepted, but in the general market she accepts if and only if $q \geq \bar{q}(y^0)$. Switching markets therefore deletes exactly the draws with $q < \bar{q}(y^0)$ and leaves the rest unchanged, and by Proposition 1(c) treatment moves women only in that direction.

(a) For each woman moved across $\tilde{y}(y^0)$ the measure of her matches falls by $\Pr(q < \bar{q}(y^0))$, and every eliminated match has $q < \bar{q}(y^0)$.

(b) For a woman who switches, her post-treatment match quality is distributed as $Q$ conditional on $q \geq \bar{q}(y^0)$, and for every $c$, $\Pr\big(q \geq c \mid q \geq \bar{q}(y^0)\big) \geq \Pr(q \geq c)$, with equality for $c > \bar{q}(y^0)$ and the left side equal to one otherwise. Her match quality therefore improves in the sense of first-order stochastic dominance, and averaging over women—those who do not switch contribute unchanged distributions—the same holds for the pool of matches formed by single women.

(c) Transactional matches do not exist in the general market, so the result follows from the fact that no matches are broken and woman can only be moved from the high-income market to the general market.

Under Assumption 3 a positive measure of women are moved, so part (a) is strict when $\Pr(q < \bar{q}(y^0)) > 0$, and part (b) is strict when in addition $\Pr(q < \bar{q}(y^0)) < 1$, since the inequality above is then strict at some $c$. □

*Proof of Proposition 4.* In high income matches, observe that, by Lemma 6(i), $\rho^* = \min\{1, \max\{0, \hat{\rho}(y_t)\}\}$, and, by Lemma 6(iv), $\hat{\rho}$ is strictly increasing in $y_t$. Hence $\rho^*$ is nondecreasing in $y_t$, and strictly increasing at any $y_t$ with $\hat{\rho}(y_t) \in (0, 1)$, that is, whenever $\rho^*$ is interior. The transfer in the interior region is Lemma 6(iii). In a general-market match the bargained terms do not depend on either partner's income by Proposition 1(a)(ii), so treatment leaves them unchanged. □

# D Partner Characteristics and 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)
- Number of other partners this partner has
- Whether the partner is believed to be married
- 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)
- Whether the respondent feels safe with the partner (1–4 scale from "never" to "always")

- Indicators for perceived partner quality (respondent checked all that apply): good personality, physically attractive, provides financial support, provides academic support, good sense of humor, kind, successful, and would make a good husband or father
- Whether the respondent would feel comfortable asking the partner to use a condom (a proxy for sexual negotiation within the relationship)

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