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DISCUSSION PAPER SERIES

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ABSTRACT

Adverse Selection in the Marriage Market: HIV Testing and Marriage in Rural Malawi*

Asymmetric information in the marriage market may cause adverse selection and delay marriage if partner quality is revealed over time. Sexual safety is an important but hidden partner attribute, especially in areas where HIV is endemic. A model of positive assortative matching with both observable (attractiveness) and hidden (sexual safety) attributes predicts that removing the asymmetric information about sexual safety accelerates marriage and pregnancy for safe respondents, and more so if they are also attractive. Frequent HIV testing may enable safe people to signal and screen. Consistent with these predictions, we show that a high-frequency, "opt-out" HIV testing intervention changed beliefs about partner's safety and accelerated marriage and pregnancy, increasing the probabilities of marriage and pregnancy by 26 and 27 percent for baseline-unmarried women over 28 months. Estimates are larger for safe and attractive respondents. Conversely, a single-test intervention lacks these effects, consistent with other HIV testing evaluations in the literature. Our findings suggest that an endogenous response to HIV risk may explain why the HIV/AIDS epidemic has coincided with systematic marriage and pregnancy delays.

JEL Classification: J12, J13, I15, I18

Keywords: adverse selection, marriage, HIV

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1 Introduction

In the marriage market, some aspects of partner quality are difficult to observe. People may hide undesirable traits such as financial, temperamental, and health characteristics. As in Akerlof (1970), the inability to observe partner traits may discourage participation by "high-quality" people, who may prefer to delay marriage until they have overcome information asymmetries (Becker 1981).

This paper studies how asymmetric information causes adverse selection in the marriage market and how removing this asymmetry affects marriage timing and surplus. Then it tests the model's predictions in the context of a high-frequency HIV testing intervention that alleviated asymmetric information on sexual safety in Malawi. This is the first empirical study of adverse selection in the marriage market. It may help explain marriage and pregnancy trends in Malawi and elsewhere in Sub-Saharan Africa (SSA).

Sexual "safety," which we define as a low propensity to engage in risky sex, is a hidden partner attribute. Sexually unsafe partners may be unfaithful to their spouses, spend less time and resources on their spouses and offspring, and contract and spread sexually-transmitted diseases. While partner safety is a worldwide concern, the HIV/AIDS epidemic has made the sexual safety of partners more salient and valuable. This trend is particularly true for HIV-endemic countries like Malawi, where HIV prevalence was 10.6 percent in 2010. An HIV-positive spouse is less productive, requires extra medical care, and may transmit HIV, particularly given norms that discourage condom use within marriage (Smith and Watkins 2005, Chimbiri 2007). HIV is also stigmatized (Ngatia 2011), increasing the social isolation of families affected by HIV.

Since marriage market participants learn about the safety of potential partners over time, an increase in the prevalence of HIV may foster marriage delays by increasing the cost of marrying an unsafe partner. Consistent with this hypothesis, Bongaarts (2007) finds a positive cross-sectional correlation between age at marriage and HIV prevalence in 33 countries in SSA. Figure 1 shows that this correlation also holds longitudinally in Malawi.[1] The rise of HIV in the 1990s coincided with an increase in the average age at first marriage of around 0.3 years. The subsequent decline in HIV prevalence in the following decade coincided with a reduction in the age at first marriage of around 0.15 years. The figure also shows a positive correlation between HIV prevalence and the age at first birth.[2]

A partner's sexual behavior is difficult to observe. Sex occurs in private and direct evidence such as pregnancy and visible sexually transmitted diseases (STDs) are often absent.[3] However, safe people may use regular HIV testing as a signaling device in HIV-endemic settings. Although test results are confidential, clinics do not repeatedly test people who are HIV positive. Therefore, people who test regularly at the same clinic indirectly reveal that they have previously tested HIV negative. One-off testing does not provide the same signal. In addition, unsafe people may prefer not to test because receiving an HIV-positive diagnosis is psychologically and socially costly (Glick 2005, Lee et al. 2002). An HIV-positive diagnosis can precipitate feelings of sadness, anger, anxiety, and depression that are difficult to conceal (Freeman et al. 2005). The widespread misperception that treatment is unavailable or ineffective may compound these feelings (Reynolds et al. 2004, Nozaki et al. 2013). Lastly, HIV positive patients may initiate antiretroviral therapy (ART) in settings where drugs are available. ART is a daily drug regimen that peers might observe.

Despite the potential benefits of HIV testing, most marriage market participants do not test frequently. In our data from southern Malawi, in 2009 only 14 percent of young childless women have tested in the past four months. In data from the 2004 Malawi Longitudinal Study of Families and Health (MLSFH), only 18 percent of respondents have ever been tested (Thornton 2012). HIV testing is inconvenient because people must travel for several hours to testing facilities and queue in public (Pinto et al. 2013). Testing is also stigmatized

[1] We use marriage timing data from the 1992, 1999, 2004, and 2010 rounds of the Malawi Demographic and Health Survey. The UNGASS Country Progress Report (2010) provides annual HIV prevalence estimates.

[2] The decline in age at first marriage and age at first birth in the 2000s occurred despite increases in female education and the supply of contraceptives.

[3] The propensity for domestic violence is another hidden partner attribute that may have similar marriage market implications.

because it may send an unfavorable signal that the individual is concerned about his or her HIV status (Chesney and Smith 1999, Ngatia 2011, Young and Zhu 2012). The relative strength of this signal (versus the signal above that testing connotes safety) depends on the convenience of testing. When testing is costly and uncommon, refusing to test does not send an unfavorable signal to potential partners (Kalichman and Simbayi 2003).

We develop a simple two-period model with positive assortative matching. We assume that attractiveness and safety are fixed traits and that attractiveness is always observable but safety is hidden until Period 2. In the model, all unsafe people marry early in order to capture additional marital surplus and possibly match with a safe spouse. However, safe people who are patient enough choose to delay marriage to avoid a mismatch. This incentive to delay is higher for attractive people under conditions that we discuss in Section 2.

Our model may explain the observed link between HIV prevalence and marriage timing in SSA: HIV risk increases the cost of marrying an unsafe partner, which magnifies the incentive for safe people to delay marriage. An intervention that enables safe marriage market participants to signal and screen in Period 1 removes this incentive, accelerating marriage and increasing the marital surplus of safe people, who can match immediately with better partners. These effects may be larger for attractive women. It does not affect marriage timing for unsafe people.

We test these predictions by evaluating the impact of a high-frequency HIV testing intervention. The Tsogolo La Thanzi (TLT) Panel Study followed a representative sample of 1505 young women in Balaka, Malawi over eight waves spanning 28 months. Surveyors offered a free HIV test after every survey wave to a randomly-assigned treatment group. They also encouraged participants to invite their partners into the study under the same intervention arm. By using an "opt-out" model in which the provider initiates testing, the intervention reduced the inconvenience and stigma of HIV testing, enabling the study participants and their partners to use testing to signal and screen.

We find large effects of high-frequency HIV testing on marriage and pregnancy. We follow Becker (1973) and consider pregnancy as a proxy for marital surplus. Within the study period, the intervention increased marriage by 26 percent and pregnancy by 27 percent among baseline-unmarried respondents. These impacts are 30-100 percent as large as the temporal changes in marriage and fertility in Malawi in recent decades. Using two complementary definitions of safety and surveyor assessments of physical appearance, we show that effects are larger for safe and attractive respondents, for whom the intervention increased marriage by 92 percent and increased pregnancy by 64 percent. We also show suggestive evidence that the intervention, which doubled the frequency of HIV testing, alleviated asymmetric information and led to belief updating. Women whose partners tested multiple times became more confident that their partners were HIV-negative, while women whose partners tested zero or one times became less confident.

These findings contrast with other studies in the literature, which show limited effects of HIV testing on risky sexual behavior (Thornton 2008, Baird et al. 2014, Gong 2015), marriage, education, and pregnancy (Beegle et al. 2015).[4] Unlike these studies, which offered testing once, the TLT study offered tests to participants and their partners eight times over 28 months. Participants in another experimental arm of our study were offered a single HIV test midway through the study period. A comparison of this group to the control group shows no statistical or economic effects of a single test offer on marriage or pregnancy. This pattern suggests that testing must be regularly available to enable marriage market signaling.

This paper provides the first empirical examination of asymmetric information in the marriage market. Becker (1981) conjectures that the inability to observe some partner traits may lead people to place more emphasis on observable traits as well as contribute to divorce. We build upon this analysis by considering the effect of unobservable partner quality on marriage timing and surplus. Since people face asymmetric information about

[4]Thornton (2008) shows that HIV testing modestly increases condom demand. Baird et al. (2014) find that testing negative in a home-based intervention does not change the prevalence of STDs but that testing positive increases STD prevalence. Gong (2015) finds that positive test results increase STD and negative results decrease STD, but only for people who are surprised by the results. Beegle et al. (2015) find no impact of a one-off testing intervention on school attendance, marriage, or pregnancy. More generally, Delavande et al. (2016) and Wilson et al. (2014) document the behavioral response to HIV risk.

several partner attributes, our conclusions may also apply to other settings.

We study marriage behavior using exogenous variation in a signaling and screening technology. Other empirical studies of the marriage market test equilibrium predictions using correlational evidence (e.g. Chiappori et al. 2012, Hitsch et al. 2010), natural experiments (Abramitzky et al. 2011), and instrumental variables (Barban et al. 2016). We are not aware of other papers that study marriage with experimental data. We also contribute to a discussion of the consequences of the HIV/AIDS epidemic in SSA. We argue that the epidemic has exacerbated the cost of asymmetric information about sexual safety, fostering marriage and pregnancy delays. This mechanism can explain the recent trend reversal in marriage and pregnancy timing in Malawi. It complements both the study of how marital norms and institutions contribute to HIV transmission (Bongaarts 2007, Magruder 2011, Greenwood et al. 2017), and the simulations of the policy responses to the HIV/AIDS epidemic by Greenwood et al. (2019).

HIV testing may soon become a more practical signaling technology, as the costs of HIV testing continue to decline. Indeed, the recent movement toward opt-out HIV testing has increased testing utilization (Kennedy et al. 2013). New technologies, such as in-home test kits, promise to make HIV testing more convenient (Low et al. 2013). Our findings suggest that these changes may further accelerate marriage and pregnancy in HIV endemic settings. Although we model early marriage and pregnancy as privately beneficial, they may have negative ramifications for female educational attainment and negative health consequences for women and children (Mirowsky 2005, Chandra-Mouli et al. 2013).

2 Theory: Asymmetric Information and Marriage Timing

This section sketches a simple two-period model to show that asymmetric information causes some safe people to delay marriage and receive less marital surplus. Removing the asymmetry can accelerate marriage and further increase surplus among safe people by enabling them to make better matches.

2.1 Setup

Consider a setting with non-transferable utility and equally sized groups of men and women who live for two periods, $t \in \{1, 2\}$. People have two fixed binary traits, attractiveness and safety, which may be either high or low (h or l). Therefore, there are four types of people, defined by their attractiveness and safety, with population shares p_{hh}, p_{lh}, p_{hl}, and p_{ll}, which sum to one and are common knowledge. Attractiveness is observable in both periods. Safety is private information in Period 1 but becomes public in Period 2. Each person has a discount factor, δ_i, which is private and is distributed uniformly between 0 and b: $\delta_i \sim U[0, b]$. We assume that attractiveness, safety, and the discount factor have the same distributions for men and women and that attractiveness and safety are independent of the discount factor.[5]

In each period, people decide whether and whom to marry. Since we rule out death and divorce, people who marry in Period 1 remain with their partners in Period 2. By marrying, a person enjoys surplus, S, defined as the additional per-period utility that accrues from being married rather than single. If a woman with attractiveness a and safety b marries a man with attractiveness c and safety d, her surplus is $S^{ab}_{cd} > 0$. We assume that surplus increases with the partner's number of high traits. The following inequality shows the surplus ranking for women of attractiveness a and safety b. An analogous inequality applies to men.

$$S^{ab}_{hh} > S^{ab}_{hl} \; = \; S^{ab}_{lh} > S^{ab}_{ll} > 0 \tag{1}$$

Since marital surplus is positive, everyone prefers to marry eventually rather than remain single. Therefore, players maximize average surplus by deciding whether to marry in Period 1 or Period 2. People of each gender make simultaneous moves based on common knowledge of the trait distributions. Given this setup, the Gale and Shapley (1962) deferred acceptance algorithm leads to positive assortative matching, as described below.

[5]Appendix A considers the implications of relaxing these assumptions.

2.2 Equilibrium if Safety is Observable in Period 1

When safety and attractiveness are observable in both periods, there is a stable assignment in which everyone marries a partner with the same number of high traits in Period 1 (Gale and Shapley 1962). Everybody marries early because they receive positive marital surplus in Period 1 and there is no reason to delay if both traits are observable. The surplus for a person of attractiveness a and safety b is $S_{ab}^{ab} \cdot (1 + \delta_i)$.

2.3 Equilibrium if Safety is Unobservable in Period 1

When people know the distributions of safety and the discount factor in the population but do not observe the safety of others in Period 1, asymmetric information causes some safe people to delay marriage, which leads to adverse selection in Period 1. We work backward from Period 2, when safety is observable. Since the distribution of traits is identical by gender, equal numbers of men and women (with the same trait distributions) postpone marriage until Period 2. As in Section 2.2, a stable assignment exists in Period 2 that is positively assortative in the number of high traits.

In Period 1, all participants prefer attractive partners. This occurs because attractive partners yield at least as much surplus as unattractive partners under Equation (1).[6] Therefore, among people who marry in Period 1, the Gale and Shapley (1962) algorithm leads to a stable assignment that is positively assortative in attractiveness. The surplus for a person of attractiveness a and safety b in this case is $\frac{p_{al} S_{al}^{ab} + \mu_{ah} S_{ah}^{ab}}{p_{al} + \mu_{ah}} \cdot (1 + \delta_i)$, where $\mu_{ah} \in [0, p_{ah}]$ is the population proportion of safe people of attractiveness a who marry in Period 1.

Next we consider the timing of marriage. People marry early if this choice maximizes

[6] Attractive people have between one and two high traits, while unattractive people have at most one high trait. Therefore, attractive partners provide (weakly) more marital surplus, regardless of the correlation between attractiveness and safety. We assume that attractiveness and safety are not perfectly negatively correlated. In that case, every attractive person is unsafe, which means that people effectively only vary in one dimension and there is no asymmetric information.

total surplus. Inequality (2) shows that unsafe people always prefer to marry in Period 1.

$$(1+\delta_i)\,\frac{p_{al}S_{al}^{al}+\mu_{ah}S_{ah}^{al}}{p_{al}+\mu_{ah}} > \delta_i \cdot S_{al}^{al} \tag{2}$$

In this expression, $\frac{p_{al}}{p_{al}+\mu_{ah}}$ and $\frac{\mu_{ah}}{p_{al}+\mu_{ah}}$ are the proportions of unsafe and safe people with attractiveness a who choose to marry in Period 1. The inequality holds because $\delta_i > 0$ and $S_{ah}^{al} > S_{al}^{al}$. For unsafe people, early marriage provides an additional period of surplus and the chance to match with a safe partner. Since the inequality does not depend on attractiveness, all unsafe people marry early.

In contrast, safe people weigh the benefit of marital surplus in Period 1 against the risk of an unsafe match. A safe person of attractiveness a marries in Period 1 if

$$(1+\delta_i)\,\frac{p_{al}S_{al}^{ah}+\mu_{ah}S_{ah}^{ah}}{p_{al}+\mu_{ah}} > \delta_i \cdot S_{ah}^{ah} \tag{3}$$

The expression shows that safe people who are sufficiently patient delay marriage. Solving for δ_i yields an expression for δ^a, the threshold value for δ. Safe people of attractiveness a for whom $\delta_i < \delta^a$ marry early.

$$\delta^a = \frac{\mu_{ah}S_{ah}^{ah}+p_{al}S_{al}^{ah}}{p_{al}(S_{ah}^{ah}-S_{al}^{ah})} > 0 \tag{4}$$

δ^a is always positive because the numerator and denominator of Inequality (4) are positive. It increases with the population proportion of safe people of attractiveness a who marry early, μ_{ah}. Under the uniform distribution of δ, $\frac{\mu_{ah}}{p_{ah}} = F(\delta^a) = \delta^a/b$ so that $\delta^a = b \cdot \frac{\mu_{ah}}{p_{ah}}$. We equate this expression to δ^a in Inequality (4) to solve for $\frac{\mu_{ah}^*}{p_{ah}}$, the fraction of safe people of attractiveness a who marry early in equilibrium.

$$\frac{\mu_{ah}^*}{p_{ah}} = \frac{1}{b(r^{ah}-1) - r^{ah} \cdot \frac{p_{ah}}{p_{al}}} \quad \forall\; b \in [b_1, b_2] \tag{5}$$

where $b_1 = \frac{p_{ah}}{p_{al}} \frac{r^{ah}}{(r^{ah}-1)}$ and $b_2 = b_1 + \frac{1}{(r^{ah}-1)}$. In this equation, $r^{ah} \equiv \frac{S^{ah}_{ah}}{S^{ah}_{al}} > 1$ is the ratio of the surplus that an ah person receives from a safe and unsafe spouse, which is greater than 1 according to Equation (1). Equation (5) establishes that a stable assignment exists in which all unsafe people as well as safe people of attractiveness a and discount factor $\delta_i < \delta^a$ marry in Period 1. Expressions (2) and (3) (evaluated at μ^*_{ah} and δ^a) show that no player has an incentive to deviate in this scenario.

Values of $\mu^*_{ah} < p_{ah}$ in Equation (5) are consistent with adverse selection on safety, and partial derivatives of μ^*_{ah} identify the factors that contribute to adverse selection. Since $\frac{\partial \mu^*_{ah}}{\partial p_{al}} < 0$ and $\frac{\partial \mu^*_{ah}}{\partial r^{ah}} < 0$, adverse selection increases in the prevalence of unsafe people and in the surplus loss from marrying an unsafe partner.

Next we compute the average two-period marital surplus in equilibrium. Since all unsafe people marry in Period 1 (when safety is hidden), their average surplus reflects uncertainty about partner safety.

$$\bar{S}^{al} = \left(\frac{p_{al} S^{al}_{al} + \mu^*_{ah} S^{al}_{ah}}{p_{al} + \mu^*_{ah}} \right) (1 + \bar{\delta}) \tag{6}$$

Asymmetric information increases the average surplus of unsafe people by allowing some of them to match with safe partners.

For safe people, average two-period surplus is a weighted average of the surpluses from marrying early and late.

$$\bar{S}^{ah} = \frac{\mu^*_{ah}}{p_{ah}} \underbrace{\left(\frac{p_{al} S^{ah}_{al} + \mu^*_{ah} S^{ah}_{ah}}{p_{al} + \mu^*_{ah}} \right) (1 + \bar{\delta}_{\delta < \delta^a})}_{\text{Surplus from Marriage in Period 1}} + \left(1 - \frac{\mu^*_{ah}}{p_{ah}} \right) \cdot \underbrace{S^{ah}_{ah} \cdot \bar{\delta}_{\delta > \delta^a}}_{\text{Surplus from Marriage in Period 2}} \tag{7}$$

In this equation, $\frac{\mu^*_{ah}}{p_{ah}}$ is the share of safe people who marry early and $1 - \frac{\mu^*_{ah}}{p_{ah}}$ is the share of safe people who marry late. People who marry early and late have different discount factors since the threshold δ^a determines marriage timing in Equation (4). Consequently, $\bar{\delta}_{\delta < \delta^a}$ is the average discount factor of people with $\delta < \delta^a$ and $\bar{\delta}_{\delta > \delta^a}$ is the discount factor of people with $\delta > \delta^a$. A comparison of this expression with the one in Section 2.2 shows that asymmetric information reduces average marital surplus for safe people. This effect arises because some safe people who marry in Period 1 match with unsafe partners and because those who marry in Period 2 forgo marital surplus in Period 1.

2.4 The Role of Attractiveness

Lastly, we examine how attractiveness, which is observable, may influence marriage timing and marital surplus. For the safe subpopulation, attractive people delay marriage more than unattractive people if $\frac{\mu_{hh}}{p_{hh}}^* < \frac{\mu_{lh}}{p_{lh}}^*$, which is equivalent to the following expression.

$$r_{hh}\left(b - \frac{p_{hh}}{p_{hl}}\right) \ > \ r_{lh}\left(b - \frac{p_{lh}}{p_{ll}}\right) \tag{8}$$

Either of two sufficient conditions may satisfy this inequality. First, the inequality holds if $r_{hh} > r_{lh}$ and $\frac{p_{hl}}{p_{hh}} \geq \frac{p_{ll}}{p_{lh}}$, so that the premium of marrying a safe partner over an unsafe one is larger for attractive people than for unattractive people.[7] Secondly, the inequality holds if $r_{hh} \geq r_{lh}$ and $\frac{p_{hl}}{p_{hh}} > \frac{p_{ll}}{p_{lh}}$, so that unsafe people are more prevalent in the attractive subpopulation.[8]

We then consider the way that attractiveness influences marital surplus. In Equation (7), the terms S^{ah}_{ah} and S^{ah}_{al} are always larger for attractive people. However, average surplus may be lower for attractive people for two reasons. Per the preceding paragraph, attractive people may disproportionately delay marriage and thereby forgo a period of marital surplus. In this scenario, the terms $E(\delta|\delta < \delta^a)$ and $E(\delta|\delta > \delta^a)$ in Equation (7) are also lower for attractive people. In this case, we require that $r^{hh} \gg r^{lh}$ (so that safe partners are substantially more valuable for attractive people) for average surplus to increase in attractiveness.

[7]For example, Fisman et al. (2006) and Hitsch et al. (2010) show that wealth is a primary determinant of male attractiveness. Under our assumption, marrying an unsafe man rather than a safe one reduces surplus proportionally more if the man is also wealthy.

[8]A surplus function such as $S^{ab}_{cd} = (ac)^{bd}$, $\forall\ l, h > 1$ leads to $r_{hh} > r_{lh}$. Surplus functions such as $S^{ab}_{cd} = abcd$ and $S^{ab}_{cd} = (a + c)(b + d)$, $\forall\ l, h > 1$ lead to $r_{hh} = r_{lh}$.

2.5 Effects of Removing Asymmetric Information

This model allows us to understand how an intervention that removes the information asymmetry may influence marriage timing and surplus. We focus this discussion on marriage and surplus in Period 1. This period represents the courtship phase when many people are considering marriage. The model has similar implications if we focus on both periods.

The model predicts that the intervention leads all safe people to marry early, which increases the marriage probability in Period 1 by $1 - \frac{\mu^*_{ah}}{p_{ah}} \geq 0$ for this group. Conversely, the intervention does not change the marriage probability in Period 1 for unsafe people, who all marry early regardless. Therefore, the impact on marriage timing should be concentrated among safe people. Within the safe subpopulation, our model predicts a differential impact on marriage timing for attractive people under the conditions in Section 2.4.

Removing asymmetric information increases Period 1 marital surplus for safe people by encouraging them to marry early and match with better partners. The increase in average marital surplus for safe people is $S^{ah}_{ah} - (\frac{\mu^*_{ah}}{p_{ah}}) \frac{(\mu^*_{ah} S^{ah}_{ah} + p_{al} S^{ah}_{al})}{(p_{al} + \mu^*_{ah})} \geq 0$. Conversely, the intervention decreases average marital surplus by $\frac{(\mu^*_{ah} S^{al}_{ah} + p_{al} S^{al}_{al})}{p_{al} + \mu^*_{ah}} - S^{al}_{al} \geq 0$ for unsafe people by worsening the quality of their partners. Under assumptions that we discuss in Appendix B, both the surplus gain for safe people and the surplus loss for unsafe people are increasing in attractiveness.

3 Sexual Safety and HIV Testing

Sexual safety is the propensity to avoid risky behaviors like sex without condoms, sex with sex workers, and extra-marital affairs. Marrying an unsafe partner reduces marital surplus: besides being unfaithful, which is costly *per se*, unsafe partners are more likely to contract STDs and infect their spouses, particularly given the uncommon use of condoms within marriage (Chimbiri 2007, Tavory and Swidler 2009). HIV magnifies the cost of marrying an unsafe partner since HIV-positive people have lower health and productivity (Smith and

Watkins 2005), may die sooner, and require extra medical care (Oni et al. 2002). Therefore, the surplus loss from marrying an unsafe partner increases with HIV prevalence.

The HIV/AIDS epidemic emerged in Malawi around 1985. HIV prevalence peaked at 14 percent in 1998 and gradually declined since then to 10.6 percent in 2010 (UNAIDS 2014). Although the provision of free HIV testing and antiretroviral treatment at public health clinics has increased throughout the country in the past decade, the HIV/AIDS epidemic remains a critical public health issue in Malawi. Therefore, HIV risk is a key aspect of partner safety for marriage market participants in this setting.

Discovering whether a prospective partner is safe may take time, since both risky behaviors and current HIV status are difficult to observe. People often conceal promiscuous behavior and HIV remains asymptomatic for several years after infection. Lacking a credible signal of partner quality, a safe person could unintentionally marry an unsafe partner. She may instead prefer to postpone marriage until she is confident that her partner is actually safe. Her partner may also wish to delay for this reason.

In an HIV-endemic setting, regular HIV testing can signal safety. However, testing may provide conflicting marriage market signals. On one hand, a safe person may communicate her type by testing frequently and revealing her results to potential partners. She may also screen partners according to their willingness to be tested.[9] Conversely, HIV testing could send an unfavorable marriage market signal by implying that the test seeker has engaged in risky sexual behavior: an observer may infer that anyone going to the trouble of being tested must have been promiscuous. The relative strength of these two mechanisms hinges upon the cost of HIV testing. Seeking a test may signal that someone is unsafe if testing is costly, whereas *not seeking* a test may signal that someone is unsafe if testing is cheap.

HIV testing remains inconvenient and stigmatized in many parts of SSA. Providers typically follow an "opt-in" model, in which the patient initiates the test. A typical test

[9] Although test results are confidential, seeking a test is observable and lying about one's HIV status may be costly in the context of a romantic relationship. Since someone who tests positive does not test further, it is difficult for an HIV-positive person to pretend to be HIV negative by retesting at the same health facility. HIV-positive people may also begin antiretroviral treatment and counseling, which are observable.

seeker in rural Malawi must travel several kilometers on foot or bicycle over unimproved roads and queue in public at the health center without being sure that the clinic will offer HIV tests that day. Patients in the Zomba District of Malawi, adjacent to our study area, spend an average of 7.1 hours per visit seeking HIV care (Pinto et al. 2013). Stigma is also a barrier to HIV testing (Sambisa et al. 2010, Berendes and Rimal 2011, Ngatia 2011, Maughan-Brown and Nyblade 2014).[10] In our sample, only 14 percent of childless women have been tested in the past four months and only 35 percent have ever been tested.

Ongoing policy changes are reducing the cost of HIV testing. Several countries, including Malawi, are introducing provider-initiated (i.e., "opt-out") HIV testing and counseling (Kennedy et al. 2013). Under this model, providers administer HIV tests during routine health care visits. Removing the need for patients to proactively request an HIV test reduces HIV testing stigma. Antenatal clinics in Malawi offered opt-out testing during the study interval, and 89 percent of mothers in our sample indicate that they were tested.

4 Survey and Intervention

We evaluate a high-frequency HIV testing intervention that was embedded in the TLT Panel Study. The study took place in the Balaka District of southern Malawi from 2009 to 2011. Polygamy is infrequent in this setting and marriage payments are uncommon. Individuals, rather than their families, decide when and whom to marry (Kaler 2001, Kaler 2006).[11] The TLT Panel Study followed a representative sample of women aged 15 to 25 over eight waves that were spaced four months apart. The survey covered socioeconomic and demographic outcomes, including HIV/AIDS perceptions, marital status, and pregnancy biomarkers. Respondents completed the questionnaires in private at the TLT clinic in Balaka Town and received US$3 per completed wave (Yeatman and Sennott 2014).

[10] Since stigma is a function of the testing take-up by others, there may be multiple equilibria with high and low levels of testing utilization. Appendix C shows that if testing exhibits a strategic complementarity, a small reduction in the cost of testing may lead to a large increase in adoption.

[11] Most families in this area practice matrilineal kinship and matrilocal marriage (Reniers 2008, Berge et al. 2014), which may reduce the importance of marriage payments and other formalities (Meekers 1992).

Surveyors offered rapid HIV tests, which provide results within 30 minutes and have sensitivity and specificity of over 99 percent (Piwowar-Manning et al. 2010). Surveyors always completed the interview before offering an HIV test. To safeguard confidentiality, surveyors provided test results verbally and in private. However, other marriage market participants could observe testing behavior and results indirectly. An observant peer could infer from the visit duration whether a study participant had received an HIV test. Surveyors discontinued testing and provided antiretroviral medication to respondents who tested HIV positive. Therefore, a participant's subsequent HIV testing provided an indication of her HIV status in prior waves.

The study incorporated three intervention arms that were assigned through simple randomization. Surveyors offered an HIV test after every wave for participants in the treatment arm ($n = 500$), only after Wave 8 for participants in the control arm ($n = 507$) and after Waves 4 and 8 for participants in the "single-test" arm ($n = 498$). Our primary analysis compares the treatment and control arms, while we use the single-test arm to examine the impact of offering an HIV test only once. Treatment participants received HIV tests 81 percent of the time and reported sharing these results with their partners 96 percent of the time. Participants in the treatment arm constitute 1.5 percent of the women aged 15-25 in Balaka, minimizing the possibility of general equilibrium effects of the intervention.[12]

Surveyors encouraged participants to invite their partners into the study, enrolling participants and their partners into the same intervention arm. This design feature enabled treatment respondents to screen partners according to their willingness to participate and submit to testing. 42 percent of treatment partners participated and 48 percent of control partners participated in the study ($p = 0.05$), for an average participation rate of 45 percent.[13] Self-reported data are available only for the endogenous subsample of partners who

[12]The gender ratio is very close to balanced in Malawi (CIA 2011). This feature, which is consistent with the assumption of gender symmetry in our model, minimizes the concern that gender imbalances could influence equilibrium marriage market outcomes.

[13]Partner statistics are based on all distinct partners as they appear in the sample. Partners who joined the study had been involved with respondents for longer and were more likely to be married. Additional comparisons of partner characteristics are available from the authors.

chose to participate. Therefore, we rely on information provided by respondents about their partners regardless of participation. The lack of reliable self-reported data from partners limits the theoretical predictions we can test.

The intervention had a large impact on the frequency of HIV testing. Figure 2 shows that the intervention increased the probability of testing within four months (either through the study or elsewhere) from 30 to 70 percent for respondents and from 30 to 55 percent for their partners ($p < 0.001$ in both cases). This pattern suggests that the intervention substantially reduced the personal cost of HIV testing.

5 Measurement

5.1 Marriage, Pregnancy, and Attractiveness

Marital status and pregnancy are the primary outcomes of our analysis. Respondents were married in Wave t if they identified a spouse or partner with whom they cohabited (marriage and cohabitation are synonymous in this setting). 42 percent of respondents were married at baseline (43 and 40 percent in treatment and control) and 60 percent were married at Wave 8.[14] Urine-based pregnancy tests measured pregnancy in each period. Respondents completed the tests over 95 percent of the time and most non-compliers were visibly pregnant.[15]

In addition to its independent interest, pregnancy may allow us to test predictions regarding marital surplus. A foundational assumption of household economics is that children are an economic product of marriage that provide union-specific utility to parents (Becker 1960, Becker 1973, Becker 1981, Weiss and Willis 1985). Consistent with this view,

[14] Divorce was a possible ramification of the intervention (Schatz 2005). 21 percent of all respondents and 7 percent of baseline-unmarried respondents divorced during the study interval. Our analysis does not focus on divorce because divorce is not correlated with treatment. The threat of domestic violence is a hidden cost that may prevent additional divorce in this setting (Bowlus and Seitz 2006).

[15] Childbirth during the late teens and early twenties (the age interval of study participants) contributes disproportionately to completed fertility. As of 2010, 44 percent of births in Malawi occurred to women aged 15-24 (Adebowale et al. 2014).

71 percent of pregnancies in our sample occur within marriage, only 24 percent of marriages involve women who are already pregnant, and 95 percent of pregnant respondents identify their current partner as the child's father.[16] Moreover, sociological research closely links marital sex (which leads to pregnancy) to marital satisfaction (Call et al. 1995, Smith et al. 2011). Appendix D shows theory-consistent impacts on the frequency of marital sex and on conception desires, further supporting this interpretation.

Physical attractiveness is an important marriage market attribute for women in this setting. Research establishes that men strongly value beauty in Malawi (Poulin 2007) and elsewhere (Fisman et al. 2006, Hitsch et al. 2010, Chiappori et al. 2012). Surveyors assessed the physical attractiveness of respondents at baseline on a four-point Likert scale.[17] Surveyors judged that 3 percent of respondents were "below average", 45 percent were "average", 45 percent were "more attractive than average", and 7 percent were "much more attractive than average". We combine the first two groups and the last two groups to create unattractive and attractive subsamples in our analysis below.

5.2 Two Proxies for Safety

Our analysis relies on the distinction between "safe" and "unsafe" marriage market participants. We designate the safety of respondents through two complementary methods, which agree in 77 percent of cases. Our first method uses observable baseline risky sexual behaviors. We classify respondents as safe if (1) they have ≤ 2 lifetime partners, (2) have ≤ 1 partners in the past year, (3) do not have multiple partners for money, (4) have sex

[16] This assumption may fail if pregnancy is coerced, exceeds either partner's predetermined optimal number of children, or occurs outside of a stable partnership. These scenarios do not characterize most pregnancies in our sample. 95 percent of pregnancies occur among respondents who have not reached their fertility goal. 79 percent of respondents sat their partner has never coerced them to have sex and 95 percent say they do not face domestic violence. Surplus may also decrease in parity if parents face a child quantity-quality tradeoff. However the elicited optimal number of children should already reflect the respondent's intended child quality investments.

[17] Oreffice and Quintana-Domeque (2016) weigh the merits of this measurement approach. Since surveyors assessed attractiveness at the end of the interview, the mannerisms of respondents may have influenced their scores. Estimates below are robust to the inclusion of surveyor fixed effects (available from the authors), which absorb surveyor-specific heterogeneity in assessments of attractiveness.

≤ 3 times per week, and (5) have never taken ART. We selected these thresholds to isolate the riskiest quartile of the distribution for each variable.[18] All these variables are positively correlated with baseline HIV infection among treatment respondents. Respondents qualify as safe if they have zero of these risk factors at baseline. Using this definition, 85 percent of unmarried respondents qualify as safe. This approach has the limitation that respondents may not report sexual behavior truthfully.

The respondent's subjective HIV status perception at baseline provides another safety indicator. Surveyors used beans to elicit responses in 10 percent increments on a probability scale, taking extra care to explain the concept of probability and maintain internal consistency across related responses (Delavande et al. 2011). We classify as safe all respondents who believe their probability of having HIV is $\leq$ 10 percent (the HIV prevalence at the time). This subjective probability is 9 percent for HIV-negative respondents and 30 percent for HIV-positive respondents ($p < 0.001$), indicating that most people correctly perceive that they are HIV-negative. Most errors occur because HIV-positive people misjudge their status.[19] Using these two proxies for safety, the HIV prevalence among treatment respondents (for whom we have baseline test results) is substantially lower in the safe subsample than in the unsafe subsample (7 percent versus 20-23 percent, $p < 0.01$).

5.3 Learning about Partner Safety

This subsection shows that the HIV testing intervention helped resolve asymmetric information about partner safety but did not generally affect beliefs about own safety, supporting

[18]In a setting with limited condom use, sexual frequency reduces sexual safety. Boileau et al. (2009) associate several of these factors with subsequent HIV infection and marital disruption. Appendix E shows treatment effects by each individual factor. Safety is correlated with age according to this definition: dividing at the median age of 20, 72 percent of young respondents and 51 percent of old respondents are safe ($p < 0.001$). As a robustness test, we construct an alternative safety index in which we isolate the riskiest quartile of each variable separately for women above and below median age. Estimates of Panel A of Table 4 under this alternative approach (available from the authors) are qualitatively similar to our primary estimates.

[19]Another approach combines these methods by including subjective HIV status as an additional HIV risk factor. Estimates using this method (available from the authors) closely resemble the results below. Baseline HIV status is not available as a safety proxy for our analysis because the control group is not tested at baseline. Using endline HIV status as a safety proxy yields similar results (available from the authors).

our assumption that people were aware of their own safety.

Study participants and their enrolled partners tested frequently: the median treatment participant was tested 7 out of 8 times, and the median enrolled partner was tested 5 out of 8 times over the study period.[20] Moreover, since 85 percent of participants had two or fewer total partners, most participants observed the participation and testing behavior of the same partners multiple times. By testing repeatedly and observing the behavior of partners over multiple periods, study participants could plausibly signal and screen.

To measure the effect of HIV testing on safety information in the marriage market, we would ideally examine the impact on market-wide perceptions of the safety of study participants. Since these data are not available, we instead examine the impact of the intervention on perceived partner HIV risk, which is one component of sexual safety. Since participants and partners received the same HIV testing intervention, we can measure whether respondents revised their HIV risk beliefs depending on own and partner testing behavior. Relationship formation and program participation are endogenous, so we do not interpret these results causally but instead assess whether they align with the model's predictions.

Figure 3 shows how respondents revised HIV risk beliefs about their partners and themselves according to partner and own testing behavior. We distinguish between respondents who perceived less HIV risk (and therefore more safety) at follow-up than at baseline and those who perceived more HIV risk (and therefore less safety) at follow-up than at baseline. We group respondents based by whether they or their partner were (a) in the control group, or in the treatment group and (b) never tested, (c) tested only once, or (d) tested more than once.[21] Stars above each bar in the figure indicate statistically significant differences from the control group.

Panel A shows that respondents updated their beliefs about the HIV risk of partners ac-

[20] Among partners, 45 percent chose to participate while 55 percent did not (forgoing $3 of compensation per survey wave). Refusing to participate may have also sent a negative signal.

[21] Within the treatment group, 3 percent of respondents never tested, 12 percent of respondents tested once, and 85 percent of respondents tested multiple times. In addition, 34 percent of partners never tested, 4 percent of partners tested once, and 62 percent of partners tested multiple times.

cording to partners' testing behavior. Compared to the control group, treatment respondents differentially perceived that partners were more likely to be HIV-negative if these partners tested repeatedly. By contrast, treatment respondents differentially perceived that partners were less likely to be HIV-negative if these partners did not test or only tested once.[22]

Conversely, Panel B shows that respondents generally had accurate beliefs about their own HIV risk. Treatment respondents who tested more than once (89 percent of the treatment group) were comparable to the control group in terms of belief revision. This pattern is understandable since most people believed they were HIV-negative and received test results that confirmed their beliefs. Belief updating only differs systematically from the control group for respondents who tested once (8 percent of the treatment group). Many of these people received news that they were HIV-positive and therefore could no longer test through the study. The contrast between the presence of updating in Panel A and the lack of updating in Panel B points to asymmetric information about safety and suggests that the intervention enabled people to signal and screen.

Lastly, we examine the association between partner testing and the marital status of baseline-unmarried treatment respondents who had partners by Wave 8 ($n = 158$). The marriage rate by Wave 8 was 75 percent for those whose partners ever tested, while it was 29 percent for those whose partners did not ($p < 0.001$). Each additional partner HIV test was associated with a 4 percentage point (9 percent) increase in the probability of marriage ($p = 0.02$) and a 5 percentage point (9 percent) increase in the probability of pregnancy or childbirth ($p < 0.001$). These patterns suggest that HIV testing may be related to the acceleration of marriage.

[22] The patterns in Panel A are robust if we limit the sample to respondents in stable relationships, who observed partner testing behavior over multiple periods. Figure A6 shows that the Panel A results are particularly strong for respondents who were initially uncertain about the HIV status of their partners.

6 Tests of Model Assumptions and Equilibrium Predictions

This section follows Chiappori et al. (2017) and Angelucci and Bennett (2017) to assess theoretical predictions about equilibrium marriage timing and matching patterns. We pool all respondents in the multiple-test, single-test, and control arms who were married at baseline ($n = 592$), for whom we observe female attractiveness, female safety, and male attractiveness (having at least median educational attainment, a proxy for earnings potential). A limitation of this exercise is that we do not observe the safety of husbands in our data.

Assumption about marital surplus. The surplus ranking assumption in Equation (1) states that marital surplus increases with the couple's number of high traits. We test this hypothesis by regressing fertility on the number of *observed* high traits.[23] To measure fertility, we divide the number of children by the marriage duration to account for variation in the number of conception opportunities. Consistent with this assumption, each extra observed high trait is associated with 0.05 additional children per year of marriage ($p < 0.001$). Figure A4 shows that fertility increases monotonically with the number of high traits.

Predictions about marriage timing if partner safety is unobservable. Equations (2) and (3) show that safe people marry later than safe people. Section 2.4 explains how, among safe people, attractiveness may lead to further delays. We test these predictions in Column 1 of Table 1, which shows age at marriage for different groups of respondents. Panel A shows that, consistent with the theory, safe respondents are 0.3 years older at marriage than unsafe respondents ($p = 0.05$). Panel B shows that, within the safe subsample, attractive respondents are 0.5 years older at marriage than unattractive respondents ($p = 0.07$).

Predictions about matching if partner safety is unobservable. We explain in Section 2.3 that people match solely on attractiveness if attractiveness is visible but safety is hidden. To test this prediction, we compare the fraction of safe and unsafe women with highly-educated husbands in Column 2 of Table 1 (Panel A). Consistent with this prediction, the proportions

[23] In the absence of information on husband's safety, we consider the three observed high traits. Couples with more observed high traits also have weakly more total high traits.

of safe and unsafe women with uneducated husbands are very similar and do not differ significantly. Panel B also shows that within the safe subsample, attractive women are 20 percentage points more likely to have highly educated husbands ($p < 0.001$). We find similar differences by attractiveness for unsafe women (estimates available upon request).

Our model also predicts that removing asymmetric information enables safe women to increase their marital surplus by marrying better partners on average. Unfortunately, our data are not well suited to test this hypothesis directly.[24] However, results suggest that the intervention led people to perceive greater concordance between own and partner safety. At baseline, the correlation between respondents' perceptions of own and partner HIV risk was 0.45 in the treatment group and 0.41 in the control group ($p = 0.19$ for this difference). By the endline, the correlation rose to 0.59 in the treatment group (a 31 percent increase) but remained 0.38 in the control group ($p = 0.08$ for this difference). This pattern is consistent with an increase in positive assortative matching on safety, but could also arise from an impact on belief accuracy or optimism.

7 Identification and Estimation

We estimate the impact of offering high-frequency HIV testing on marriage and pregnancy over 28 months. Our primary specification pools the follow-up waves (Waves 2-8) as follows:

$$Y_{it} = \beta_0 + \beta_1 T_i + \beta_2 Y_i^b + \delta_t + \varepsilon_{it} \tag{9}$$

In this equation, Y is the outcome variable, T is an indicator for assignment to the treatment arm, and δ is a set of wave indicators. All regressions control for the baseline dependent variable, Y^b, to increase precision.[25] We estimate this specification using OLS and cluster

[24] Out of 201 marriages formed during the study, we observe data on male and female attractiveness and female safety only for 97 and 73 treatment and control couples; we do not observe male safety.

[25] Controlling for additional covariates does not generally increase precision further because these variables expend degrees of freedom. Estimates that control for additional baseline covariates are available from the authors.

standard errors by respondent. The coefficient of interest, β_1, identifies the average treatment effect of offering high-frequency HIV testing.[26]

This parameter is identified under two assumptions. First, one participant's treatment assignment must not influence another participant's outcomes. Spillover effects that would violate this assumption are unlikely because the treatment group constitutes only around 1.5 percent of the local marriage market. Secondly, assignment to treatment must be uncorrelated with potential outcomes. Random assignment generally ensures that this assumption holds. However, an important caveat is that control respondents are 0.6 years younger than treatment respondents in our data. Figure A3 illustrates this imbalance by plotting the age distributions in the treatment and control groups. There are 57 additional control respondents who are fifteen or sixteen years old, while the rest of the sample is balanced. This imbalance is apparently due to chance since other orthogonal characteristics are balanced across arms. We address this issue by employing entropy weights to re-balance age in all subsequent estimates. Entropy weights, which are similar to inverse propensity weights, balance the data so that the treatment and control arms have the same mean, variance, and skewness (Hainmueller 2012, Hainmueller and Xu 2013). Appendix F discusses this issue further and shows that results are robust under alternative age corrections.

Baseline summary statistics for baseline-unmarried respondents appear in Table 2 (statistics for the full sample appear in Table A15). Column 1 shows the mean for the treatment group and Columns 2 and 4 show treatment-control differences before and after weighting by age. Before weighting, treatment respondents are 17 percentage points less likely to be enrolled in school and 3 percentage points more likely to be pregnant. They have slightly higher HIV risk perceptions, however no other covariates in the table are significantly different. In Column 4, all variables are balanced after we weight by age. After weighting, the covariates in the table do not differ significantly across intervention arms ($p = 0.41$). The

[26] The "treatment" in this context is the testing offer rather than the test itself. In this sense, all non-attriters comply with the intervention by definition, so that the "intent to treat" (ITT) and "average treatment effect on the treated" (ATT) effects are equivalent.

table also shows that few people seek HIV testing in the status quo. Excluding mothers (who are almost always tested during prenatal care), only 35 percent of respondents have ever been tested.[27] Appendix G examines the evolution of key outcomes over time.

After estimating overall effects, we test the model predictions by examining heterogeneity in the treatment effects by safety and attractiveness for baseline-unmarried respondents. Tables A10 and A11 provide baseline summary statistics for these subsamples. Safe respondents are younger, richer, and have higher school enrollment than unsafe respondents. Attractive respondents are wealthier and have a stronger future orientation than unattractive respondents. As we discuss below, we assess the robustness of our estimates by controlling for the interaction between treatment and a list of baseline covariates.

8 Effects of Offering High-Frequency HIV Tests

8.1 Impacts on Marriage and Pregnancy

Table 3 shows the impacts of high-frequency HIV testing on marriage and pregnancy. As in other regression tables, control group means appear in brackets below coefficients and standard errors. Odd columns show unweighted estimates while even columns weight to balance by age. In Panel A, which provides full-sample estimates, the intervention increased the probability of marriage 4.5 percentage points (9.2 percent) and the probability of pregnancy by 2.7 percentage points (21 percent) in unweighted regressions. Panel B distinguishes between baseline-unmarried and baseline-married respondents by interacting T with indicators for both groups. Estimates are substantially larger for unmarried women, who have a higher marriage propensity. For this group, the intervention increased the probability of marriage by 7.2 percentage points (45 percent) and the probability of pregnancy by 3.5 percentage points (35 percent) in unweighted estimates. Conversely, effects are small and

[27] Appendix H discusses attrition in more detail. Respondents completed an average of 7 survey waves, and 71 percent of respondents competed all eight waves. Attrition is balanced across intervention arms and estimates are robust if we limit the sample to non-attriters.

insignificant for baseline-married respondents (although married and unmarried estimates are significantly different only in Column 1). The lack of a negative impact on marriage for baseline-married women suggests that the intervention did not induce divorce.[28]

A comparison of odd and even columns of Table 3 shows that estimates are similar and not statistically different after weighting by age (test results available from the authors). The robustness to weighting suggests that the age imbalance is not a severe confound in practice. The rest of our analysis focuses on age-balanced estimates. However, Appendix F provides analogous unweighted results.

8.2 Heterogeneous Effects by Safety

Next we examine treatment effect heterogeneity by safety. The intervention should accelerate marriage for safe people, who can now signal and screen, but not for unsafe people, who marry early in any case. If pregnancy proxies for marital surplus, the intervention should increase the pregnancy likelihood of safe people and decrease it for unsafe people.

Table 4 distinguishes between impacts on safe and unsafe baseline-unmarried respondents. In Panel A, we define safety according to the absence of self-reported risky behavior at baseline, as we describe above. Impacts on marriage and pregnancy for safe respondents are positive and significant, while the impacts for unsafe respondents are negative and insignificant. In Columns 1 and 3, the intervention increased the probability of marriage by 7.1 percentage points (51 percent) and the probability of pregnancy by 3.9 percentage points (43 percent) for safe respondents. The heterogeneous response by safety is statistically significant for marriage at significance levels of 10 percent or higher ($p = 0.10$ in Column 1). In Panel B, we define safety using baseline subjective HIV risk. Estimates for safe respondents, which remain significant, are slightly smaller for marriage and slightly larger for pregnancy. Here the difference between the safe and unsafe impacts is not significant for marriage at

[28]Consistent with this pattern, we do not find meaningful impacts on divorce for any subgroups. Estimates are available from the authors. In addition, Table A5 in Appendix G provides the divorce rate by wave and treatment status for the subgroups in our analysis.

conventional significance levels ($p = 0.29$ in Column 1), but is significant for pregnancy at significance levels of 3 percent or higher ($p = 0.03$ in Column 3). The increase in marriage and pregnancy among safe respondents, but not among unsafe ones, aligns with the predictions from our model of asymmetric information.[29]

These pregnancy results suggest that the intervention increased marital surplus for safe participants. This increase could occur through both the acceleration of marriage and improvements in match quality. While the impact on marriage timing is evident in the table, we also find indirect evidence of match quality improvements. Table A1 examines impacts on self-reported coital frequency, and shows that the intervention increased the frequency of marital sex for safe respondents but decreased the frequency of marital sex for unsafe respondents ($p = 0.01$ for this difference). In addition, Figure A1 shows that the intervention made safe respondents and their husbands more interested in conceiving children together, while it made unsafe respondents and their husbands less interested ($p = 0.05$ for respondents and $p = 0.02$ for husbands). These patterns, which we discuss further in Appendix D, suggest that the intervention also improved match quality.

Safety may be correlated with other characteristics that cause treatment effect heterogeneity. Appendix I shows that safe respondents are younger, wealthier, and more optimistic about the future. To assess whether the heterogeneous impact by safety is robust, we control for the interaction of T with fourteen demographic, socioeconomic, and time preference covariates.[30] If our approach misattributes treatment effect heterogeneity in these variables to safety, then controlling for the interaction of T and these covariates should attenuate our estimates. Instead, Columns 2 and 4 of Table 4 show that estimates are robust to these controls, suggesting that the heterogeneous effect by safety is not spurious.

The finding that offering high-frequency HIV testing accelerated marriage and preg-

[29] An alternative approach combines these safety definitions by including baseline subjective HIV risk as a risk factor. Estimates using this approach, which are available from the authors, closely resemble the results in Panel A.

[30] These variables include tribe, religion, age, completed education, school enrollment, employment, durable roof, durable floor, electricity, telephone ownership, television ownership, future orientation, and subjective mortality risk within 1, 5, and 10 years.

nancy for safe participants is also consistent with our premise that safety information is asymmetric. Appendix A.2 clarifies that the intervention should lead to uniform effects by safety if people are unaware of their own safety in the status quo. The negative but insignificant impact on pregnancy for unsafe respondents suggests that the intervention may have reduced marital surplus for this group. The negative but insignificant impact on marriage for unsafe respondents is also consistent with the model under the alternative assumption that marrying unsafe people yields negative marital surplus.

8.3 Heterogeneous Effects by Attractiveness

Next we examine heterogeneous treatment effects by respondent attractiveness. Section 2.4 shows that among safe respondents, those who are attractive may have larger impacts on marriage and marital surplus. To test this prediction, we limit the sample to safe and baseline-unmarried respondents and show results by attractiveness under both alternative safety definitions.[31]

Table 5 shows treatment effects by baseline attractiveness. Panel A uses the absence of baseline risky behavior to define the safe subsample. Columns 1 and 3 show that the intervention increased the probability of marriage by 11 percentage points (92 percent) and the probability of pregnancy by 5.1 percentage points (64 percent) for attractive respondents, and had small and statistically insignificant impacts for unattractive respondents. Estimates are similar in Panel B, which uses baseline perceived HIV risk to define the safety and limit the sample. The effects of marriage are statistically larger for attractive and safe participants, consistent with the model's predictions.

Since attractive people in this sample are more forward-looking and have higher socioe-

[31]For unsafe people, the model predicts no differential effect on marriage timing by attractiveness, however it predicts that the intervention should differentially reduce the surplus of attractive people. There are 91 unsafe and baseline unmarried respondents according to the "own risky behavior" definition and 112 such respondents according to the "own perceived HIV risk" definition. Appendix J provides estimates by attractiveness for unsafe respondents. These small samples do not provide enough statistical power to identify attractiveness interactions. Accordingly, there are no significant differential impacts by attractiveness on marriage or pregnancy in the table.

conomic status, Columns 2 and 4 of Table 5 repeat the exercise in Table 4 and control for the interaction between T and baseline demographic, socioeconomic, and time preference covariates. Although covariate interactions with T are jointly significant with $p < 0.001$, all estimates are robust to including these controls. This pattern suggests that our estimates reflect a heterogeneous response by attractiveness rather than a spurious correlation.

8.4 Impacts by Survey Wave

An examination of treatment effects by wave provides further insight into these results. Figure 4 shows treatment effects on marriage and pregnancy by survey wave for baseline-unmarried respondents. Blue bars in the figures show impacts for all baseline-unmarried respondents, red bars limit the sample to safe respondents (based on the "own risky behavior" definition) and green bars limit the sample to safe and attractive respondents. The impact on marriage is positive in Waves 3 to 6, consistent with the need for repeated testing to overcome asymmetric information. The impact on pregnancy is positive in Waves 3 and 4 and then again in Wave 8, also consistent with the need for repeated testing to overcome asymmetric information, as well as with the cyclical nature of pregnancy. Appendix G provides additional detail about the evolution of HIV testing, safety perceptions, relationship formation, marriage, and pregnancy over the study period. Treatment respondents and their partners received HIV tests consistently throughout the study period. Relationship turnover and marriage were concentrated in the early survey waves for treatment respondents, which is consistent with the resolution of asymmetric information.

8.5 Alternative Explanations

We consider if several alternative mechanisms could contribute to our findings. First, the intervention could have encouraged unintended pregnancies that led to "shotgun marriages," contradicting our interpretation of pregnancy as a proxy for marital surplus. However, only 24 percent of marriages involved women who were already pregnant and 71 percent of pregnancies occurred within marriage, rates that do not statistically differ across treatment and control arms. Appendix D examines the impact on self-reported sexual behavior. The intervention increased the frequency of sex for safe and attractive respondents (consistent with impacts on pregnancy) but only within the context of marriage. There was no impact on self-reported sex with other people, including the primary partners of unmarried respondents. The common concern about the validity of sexual behavior self reports lead us to interpret these findings cautiously (Kelly et al. 2013). As additional evidence, Figure A1 compares the conception desires of married and non-pregnant respondents and their husbands (as expressed by respondents) at follow-up. The intervention is associated with stronger conception desires for safe respondents and husbands ($p = 0.04$ and $p = 0.02$) but with weaker conception desires for unsafe respondents and husbands ($p = 0.19$ and $p = 0.12$). These findings suggest that the intervention primarily encouraged intended pregnancies among married couples.

Secondly, the intervention could have encouraged marriage and pregnancy by making family formation more salient. While plausible, this mechanism does not explain the differential response for attractive respondents in Table 5. Finally, the intervention could have increased pregnancy by influencing intra-household bargaining power since the revelation of safety information might alter the threat points of partners (McElroy and Horney 1981). It is theoretically unclear which partner the intervention would benefit. Moreover, baseline-married men and women in our sample report similar optimal family sizes, although women prefer later childbirth. In Appendix K, we find no impact on three bargaining power proxies, including the respondent's general perception that her partner is "in charge." This finding casts further doubt on the bargaining power explanation.

9 The Effect of a One-Shot HIV Testing Intervention

Our findings contrast with other HIV testing evaluations, which find small and contingent effects on risky sexual behavior (Thornton 2008, Delavande and Kohler 2012, Baird et al. 2014, Beegle et al. 2015, Gong 2015), marriage, and pregnancy (Beegle et al. 2015). The TLT intervention was more intensive than others in the literature because surveyors offered to test participants and their partners eight times over 28 months. Delavande et al. (2016) show that repeated testing of serodiscordant couples in Malawi reduces risky sexual behavior.

To reconcile our findings with the literature, we assess the importance of *repeated* HIV testing. As explained in Section 4, the TLT Panel Study included a third arm ($n = 498$) in which participants and their partners were offered HIV tests after Waves 4 and 8. We compare this arm to the control arm over Waves 5-8 to estimate the impact of offering a single HIV test. Figure A5 in Appendix L follows our analysis in Section 5.3 to examine whether the single-test intervention led participants to revise beliefs about the HIV status of partners and themselves. Consistent with the premise that the single-test intervention provided less information about partner safety than the multiple-test intervention, this figure shows no differential patterns of belief updating by intervention arm or HIV testing utilization. Table A14 provides summary statistics for these intervention arms in Wave 4, which serves as the baseline. Characteristics are generally balanced, although single-test respondents are less future oriented and more likely to be HIV positive. We follow Equation (9), weight to balance by age, pool follow-up rounds, and cluster standard errors by respondent to match our previous empirical strategy. This inquiry differs from our primary analysis because the follow-up period includes four rather than seven waves.

Table 6 contrasts the impacts of the single-test and multiple-test interventions. For a like-to-like comparison, we estimate the multiple-test results using only Waves 2-4 and weight to match the age distribution of the single-test sample.[32] The overall estimates in Panel A are analogous to Table 3 (Panel A), the estimates by safety in Panel B are analogous to Table 4 (Panel A), and the estimates by attractiveness in Panel C are analogous to Table 5 (Panel A). The single-test intervention had no effect on marriage or pregnancy overall or in the safe or attractive subsamples. All single-test estimates (which appear in Columns 1 and 4) are

[32] Age weighting across both samples is necessary because respondents were younger in Waves 2-5 than in Waves 5-8 and this age difference could mechanically generate treatment effect differences across the interventions.

small and statistically insignificant; many estimates have an unexpected sign. These results contrast starkly with the multiple-test estimates in Columns 2 and 5, which are very similar to the results in Tables 3-5. Next we use seemingly unrelated regression to test whether the single-test and multiple-test coefficients are significantly different. Columns 3 and 6, which report p-values for these tests, show that multiple-test impacts are significantly larger overall and in the safe and attractive subsamples.

10 Discussion and Conclusion

In the marriage market, important partner attributes are difficult to observe and can only be discovered over time. Partners may disguise aspects of their health, financial circumstances, or preferences. The presence of asymmetric information may delay marriage and reduce the marital surplus of some people. These delays and losses rise with the prevalence of negative partner attributes, the cost of marrying someone with these attributes, and the difficulty of signaling and screening.

Evidence from Malawi supports this view. The HIV/AIDS epidemic has coincided with marriage and pregnancy delays in SSA. In Malawi, the age at first marriage and age at first birth loosely track the peak and subsequent abatement of HIV. Bongaarts (2007) shows that the positive correlation between age at marriage and HIV prevalence exists in many SSA countries. We hypothesize that the HIV/AIDS epidemic has increased the cost of matching with an unsafe partner and thereby strengthened the incentive to learn about partner safety. We find that offering high-frequency, opt-out HIV testing to young women and their partners accelerated marriage and increased the likelihood of pregnancy for safe people, which suggests that high-frequency testing enabled these people to signal and screen.

To gauge the size of our impacts, Figure 5 compares treatment effect estimates of high-frequency testing to the 1992-2000 increase and the 2000-2010 decrease in the age at first marriage and the age at first birth in Malawi.[33] The impact of offering high-frequency HIV

[33]For this exercise, we limit the DHS sample to women aged 17-27 and weight to match the age distribution

testing equals 79 percent of the marriage delay and 30 percent of the fertility delay from 1992-2000. It equals 110 percent of the marriage acceleration and 50 percent of the fertility acceleration from 2000-2010. These magnitudes suggest that the estimated impacts are large. Future work should assess whether other factors that moderate the impact of HIV, like the introduction of antiretroviral therapy, also influence marriage timing.

Following recent WHO guidelines, HIV testing in SSA is shifting from an opt-in to an opt-out model, resulting in substantial increases in the testing frequency (Kennedy et al. 2013). Our findings suggest that the provision of opt-out testing is likely to have strong effects on marriage and pregnancy. Recent technological changes, such as self-testing kits, may further reduce the inconvenience and stigma of HIV testing (Doherty et al. 2013) and in turn accelerate marriage and pregnancy in communities with HIV. The welfare implication of this pattern is unclear. In our model, the resolution of asymmetric information improves welfare for safe people. However, early marriage and pregnancy are associated with costs that women may not fully internalize (Jensen and Thornton 2003). For example, marriage and pregnancy are key reasons why girls drop out of school (Lloyd and Mensch 2008). We find no impact of the intervention on school enrollment, which may mitigate this concern in our setting. Further, early pregnancy is associated with health risks for women (e.g. via pregnancy complications) and children (Westendorp and Kirkwood 1998, Mirowsky 2005, Chandra-Mouli et al. 2013). In contrast to a standard model of adverse selection, the resolution of asymmetric information could have unintended negative effects in this dimension.

of the 2010 DHS.

Table 1: Tests of Equilibrium Predictions for Baseline-Married Respondents

	Age at Marriage	Husband's Schooling
	(1)	(2)
A: Means by Safety (Defined by Own Risky Behavior)		
Safe	17.5	0.49
	(0.12)	(0.028)
Unsafe	17.2	0.46
	(0.13)	(0.030)
Equality of means (p-value)	0.05	0.51
Observations	592	592
B: Means by Attractiveness for Safe Respondents (Defined as Above)		
Attractive	17.8	0.60
	(0.19)	(0.041)
Unattractive	17.3	0.40
	(0.16)	(0.037)
Equality of means (p-value)	0.07	0.00
Observations	319	319

Note: the table shows means for age at marriage (Column 1) and husband's schooling (Column 2; an indicator that the husband has above-median schooling) by safety and attractiveness for respondents from all intervention arms who are married at baseline. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Baseline Characteristics of Unmarried Respondents by Treatment Status

	Treatment	T−C (Unweighted)		T−C (Weighted)	
	Mean	Difference	SE	Difference	SE
	(1)	(2)	(3)	(4)	(5)
Demographics					
Age	18.6	1.09***	0.24	0.00	0.99
Attractiveness	3.55	-0.09	0.05	-0.09	0.06
Ngoni Tribe	0.37	0.02	0.04	0.02	0.04
Yao Tribe	0.23	-0.04	0.04	-0.03	0.04
Lomwe Tribe	0.21	0.06	0.03	0.06*	0.03
Catholic	0.41	0.05	0.04	0.04	0.04
Protestant	0.44	-0.03	0.04	-0.03	0.04
Muslim	0.15	-0.02	0.03	-0.01	0.03
Socioeconomic Status					
Enrolled in school	0.61	-0.17***	0.04	-0.08*	0.04
Employed full-time	0.10	0.05**	0.02	0.02	0.03
Any savings	0.12	0.02	0.03	-0.01	0.03
Household asset index	0.20	-0.08	0.09	-0.14	0.10
HIV					
HIV positive	0.10	-	-	-	-
HIV risk index (0-4)	0.19	0.07**	0.03	0.03	0.03
Thinks about future	3.27	-0.03	0.07	-0.08	0.07
Worried about HIV	1.54	0.03	0.06	-0.03	0.06
Subj. 5-year mort. risk (%)	0.32	0.00	0.02	-0.01	0.03
Ever tested for HIV (parity=0)	0.31	0.05	0.04	0.01	0.04
Ever tested for HIV (parity>0)	0.84	-0.09	0.06	-0.08	0.06
Subj. HIV risk (%)	0.12	0.02*	0.01	0.02	0.01
Subj. partner HIV likelihood (1-5)	1.6	0.05	0.06	0.05	0.07
Outcomes					
In a Relationship	0.47	0.09**	0.04	0.04	0.04
Married	0.00	0.00	–	0.00	–
Pregnant	0.06	0.03*	0.02	0.02	0.02
Joint signif. of covariates (p-value)	-	0.01	-	0.37	-
Observations	286	303	-	303	-

Note: the household asset index is the standardized sum of indicators that the household has a durable roof, a durable floor, electricity, a television, a telephone, and an improved toilet. Columns 2 and 3 show unweighted comparisons and Columns 4 and 5 show comparisons that are weighted to balance by age. By construction, HIV test results are only available for the treatment group at baseline. To compute p-values, we regress each variable on treatment in Wave 1 and cluster standard errors by respondent. The joint significance of covariates in based on a regression of treatment on all covariates in the table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: The Impact of High-Frequency HIV Testing on Marriage and Fertility

	Currently Married		Currently Pregnant	
	(1)	(2)	(3)	(4)
A: Overall Estimates				
Treatment	0.045***	0.035**	0.027***	0.021**
	(0.017)	(0.017)	(0.0099)	(0.010)
	[0.49]	[0.55]	[0.13]	[0.13]
B: Estimates by Baseline Marital Status				
Treatment · Unmarried	0.072***	0.056**	0.035***	0.031**
	(0.027)	(0.028)	(0.013)	(0.013)
	[0.16]	[0.18]	[0.10]	[0.10]
Treatment · Married	0.0097	0.010	0.017	0.016
	(0.018)	(0.018)	(0.015)	(0.015)
	[0.93]	[0.93]	[0.17]	[0.17]
Equality of coefficients (p-value)	0.06	0.17	0.37	0.47
Reweight by age	No	Yes	No	Yes
Observations	6048	6048	6048	6048

Note: Panel A reports $\hat{\beta}_1$ from Equation (9) in the text. Estimates in Panel B are based on the specification $Y_{it} = \beta_1[T_i \cdot U_i] + \beta_2[T_i \cdot (1 - U_i)] + \beta_3 U_i + \beta_4(1 - U_i) + \beta_5 Y_i^b + \delta_t + \varepsilon_{it}$. In this expression, Y is the dependent variable, Y^b is the baseline dependent variable, δ is a set of wave dummies, T is a treatment indicator and U is a baseline-unmarried indicator. Panel B reports $\hat{\beta}_1$ and $\hat{\beta}_2$. Standard errors are clustered by respondent and appear in parentheses. Subgroup-specific control group means appear in brackets. Even columns reweight to balance by age. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Impacts on Marriage and Fertility for Baseline-Unmarried Respondents, by Safety

	Currently Married		Currently Pregnant	
	(1)	(2)	(3)	(4)
A: Safety Defined by Own Risky Behavior				
Treatment · Safe	0.071**	0.050*	0.039***	0.032**
	(0.029)	(0.029)	(0.014)	(0.014)
	[0.14]	[0.14]	[0.09]	[0.09]
Treatment · Unsafe	-0.079	-0.074	0.00074	-0.00016
	(0.087)	(0.074)	(0.039)	(0.035)
	[0.41]	[0.41]	[0.13]	[0.13]
Equality of coefficients (p-value)	0.10	0.14	0.35	0.42
Significance of covariates (p-value)	-	0.00	-	0.00
B: Safety Defined by Own Perceived HIV Risk				
Treatment · Safe	0.064**	0.041	0.048***	0.042***
	(0.032)	(0.029)	(0.014)	(0.013)
	[0.15]	[0.15]	[0.08]	[0.08]
Treatment · Unsafe	-0.019	-0.020	-0.032	-0.034
	(0.073)	(0.063)	(0.035)	(0.031)
	[0.30]	[0.30]	[0.18]	[0.18]
Equality of coefficients (p-value)	0.29	0.39	0.03	0.03
Significance of covariates (p-value)	-	0.00	-	0.00
Control for treatment · covariates	No	Yes	No	Yes
Observations	3427	3427	3427	3427

Note: estimates are based on the specification $Y_{it} = \beta_1[T_i \cdot S_i] + \beta_2[T_i \cdot (1 - S_i)] + \beta_3 S_i + \beta_4(1 - S_i) + \beta_5 Y_i^b + \delta_t + \varepsilon_{it}$. In this expression, Y is the dependent variable, Y^b is the baseline dependent variable, δ is a set of wave dummies, and T is a treatment indicator. S_i is a 'safety' indicator, which identifies respondents with zero HIV risk factors in Panel A and ≤ 0.1 baseline subjective HIV risk in Panel B, as the text explains. The table reports $\hat{\beta}_1$ and $\hat{\beta}_2$. Standard errors are clustered by respondent and appear in parentheses. Subgroup-specific control group means appear in brackets. All regressions reweight to balance by age. Even columns also control for the interaction between treatment and demographics (tribe, religion, and age), SES (completed education, school enrollment, employment, durable roof, durable floor, electricity, telephone ownership, and television ownership), and time preferences (future orientation and subjective mortality risk within 1, 5, and 10 years). Covariates are demeaned in order to preserve the interpretation of the coefficients of interest. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Estimates by Attractiveness for Baseline-Unmarried and Safe Respondents

	Currently Married		Currently Pregnant	
	(1)	(2)	(3)	(4)
A: Safety Defined by Own Risky Behavior				
Treatment · Attractive	0.11***	0.12***	0.051***	0.050***
	(0.040)	(0.036)	(0.018)	(0.018)
	[0.12]	[0.12]	[0.08]	[0.08]
Treatment · Not Attractive	0.023	-0.052	0.015	0.0083
	(0.043)	(0.042)	(0.022)	(0.020)
	[0.17]	[0.17]	[0.12]	[0.12]
Equality of coefficients (p-value)	0.16	0.00	0.21	0.14
Significance of covariates (p-value)	-	0.00	-	0.00
Observations	2881	2881	2881	2881
B: Safety Defined by Own Perceived HIV Risk				
Treatment · Attractive	0.11***	0.11***	0.062***	0.056***
	(0.039)	(0.039)	(0.018)	(0.018)
	[0.10]	[0.10]	[0.07]	[0.07]
Treatment · Not Attractive	-0.0046	-0.052	0.026	0.022
	(0.051)	(0.043)	(0.021)	(0.020)
	[0.22]	[0.22]	[0.10]	[0.10]
Equality of coefficients (p-value)	0.06	0.01	0.20	0.21
Significance of covariates (p-value)	-	0.00	-	0.00
Observations	2753	2753	2753	2753
Treatment · covariates	No	Yes	No	Yes

Note: estimates are based on the specification $Y_{it} = \beta_1[T_i \cdot A_i] + \beta_2[T_i \cdot (1 - A_i)] + \beta_3 A_i + \beta_4(1 - A_i) + \beta_5 Y_i^b + \delta_t + \varepsilon_{it}$. In this expression, Y is the dependent variable, Y^b is the baseline dependent variable, δ is a set of wave dummies, T is a treatment indicator, and A_i is an attractive indicator. The table reports $\hat{\beta}_1$ and $\hat{\beta}_2$. Standard errors are clustered by respondent and appear in parentheses. Subgroup-specific control group means appear in brackets. Panel A includes respondents with zero baseline HIV risk factors and Panel B includes respondents with baseline subjective HIV risk ≤ 0.1, as the text explains. All regressions reweight to balance by age. Even columns also control for the interaction between treatment and demographics (tribe, religion, and age), SES (completed education, school enrollment, employment, durable roof, durable floor, electricity, telephone ownership, and television ownership), and time preferences (future orientation and subjective mortality risk within 1, 5, and 10 years). Covariates are demeaned in order to preserve the interpretation of the coefficients of interest. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: SUR Comparison of Single-Test and Multi-Test Interventions

	Currently Married			Currently Pregnant		
	Single-Test	Multi-Test	P-value	Single-Test	Multi-Test	P-value
	(1)	(2)	(3)	(4)	(5)	(6)
A: Overall Estimates						
Treatment	-0.018	0.035**	0.03	0.0020	0.027*	0.22
	(0.019)	(0.016)		(0.014)	(0.014)	
	[0.56]	[0.51]		[0.13]	[0.13]	
Observations	3238	3601	-	3238	3601	-
B: Estimates by Safety (Defined by Own Risky Behavior) for Baseline-Unmarried Respondents						
Treatment · Safe	-0.0067	0.078***	0.04	-0.0043	0.041**	0.10
	(0.032)	(0.025)		(0.020)	(0.019)	
	[0.15]	[0.07]		[0.11]	[0.09]	
Treatment · Unsafe	-0.087	-0.073	0.91	-0.012	0.028	0.57
	(0.090)	(0.084)		(0.051)	(0.047)	
	[0.31]	[0.31]		[0.17]	[0.10]	
Equality of coefficients (p-value)	0.40	0.09	-	0.89	0.78	-
Observations	1673	2064	-	1673	2064	-
C: Estimates by Attractiveness for Baseline-Unmarried and Safe Respondents (Defined by Own Risky Behavior)						
Treatment · Attractive	-0.0099	0.12***	0.00	-0.017	0.083***	0.00
	(0.039)	(0.034)		(0.026)	(0.024)	
	[0.13]	[0.05]		[0.11]	[0.06]	
Treatment · Unattractive	-0.0023	0.025	0.48	0.022	-0.027	0.17
	(0.053)	(0.038)		(0.030)	(0.033)	
	[0.17]	[0.10]		[0.10]	[0.13]	
Equality of coefficients (p-value)	0.91	0.07	-	0.33	0.00	-
Observations	1388	1743	-	1388	1743	-

Note: standard errors are clustered by respondent and appear in parentheses. Subgroup-specific control group means appear in brackets. Single-test estimates in Columns 1 and 4 cover Waves 5-8 and Multi-test estimates in Columns 2 and 5 cover Waves 2-5. Columns 3 and 6 test whether the single-test and multi-test coefficients are significantly different. All regressions control for wave dummies and the baseline dependent variable. Regressions reweight to balance by age. Panel A uses the specification in Equation (9). Panel B is limited to baseline-unmarried respondents and uses the same specification as Panel A of Table 4. Panel C is limited to safe, baseline-unmarried respondents and uses the same specification as Panel A of Table 5. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

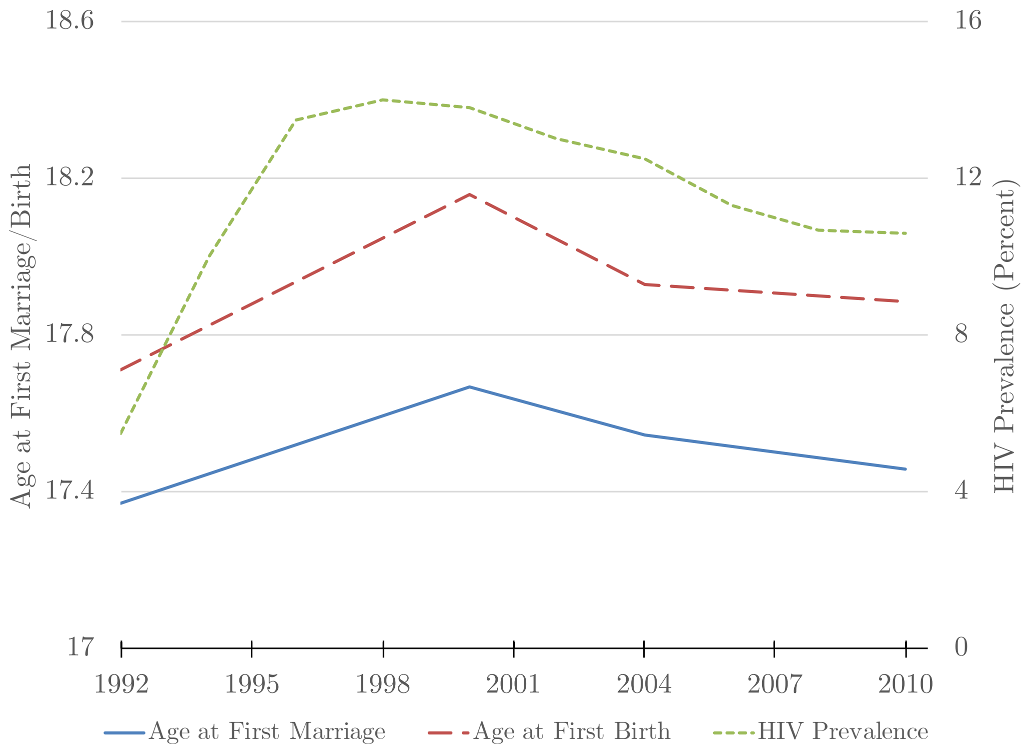


Figure 1: Marriage and First Birth for Women Aged 17-27

Note: age at first marriage and age at first birth are based on Demographic and Health Surveys of Malawi from 1992, 2000, 2004 and 2010. The UNGASS Country Progress Report (2010) provides annual HIV prevalence estimates.

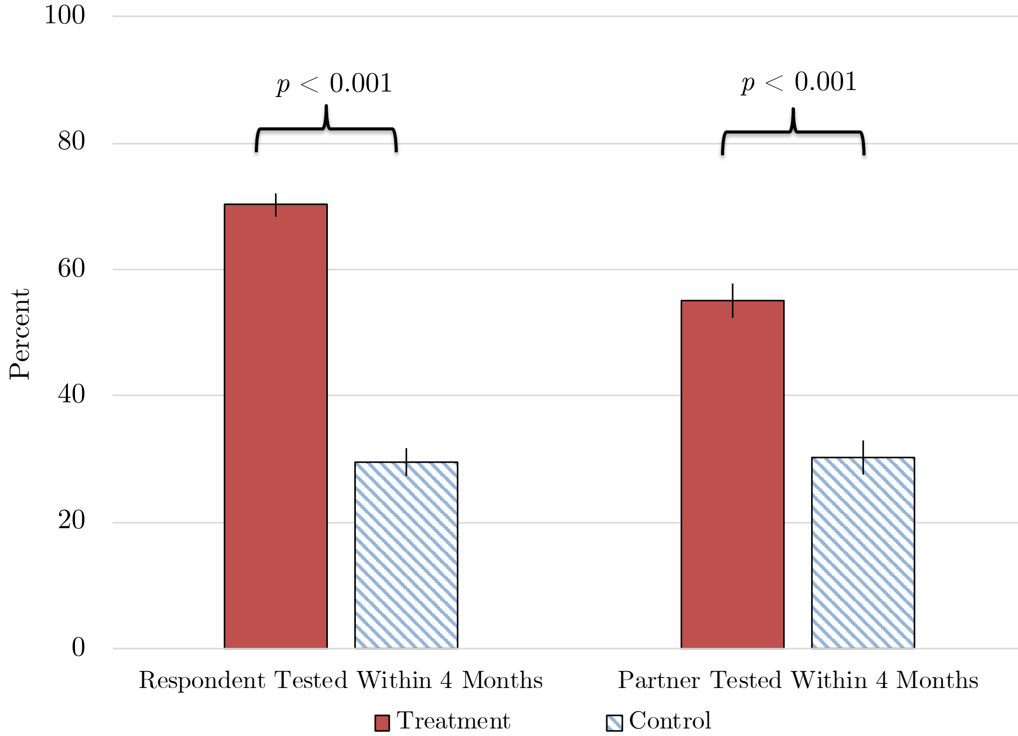
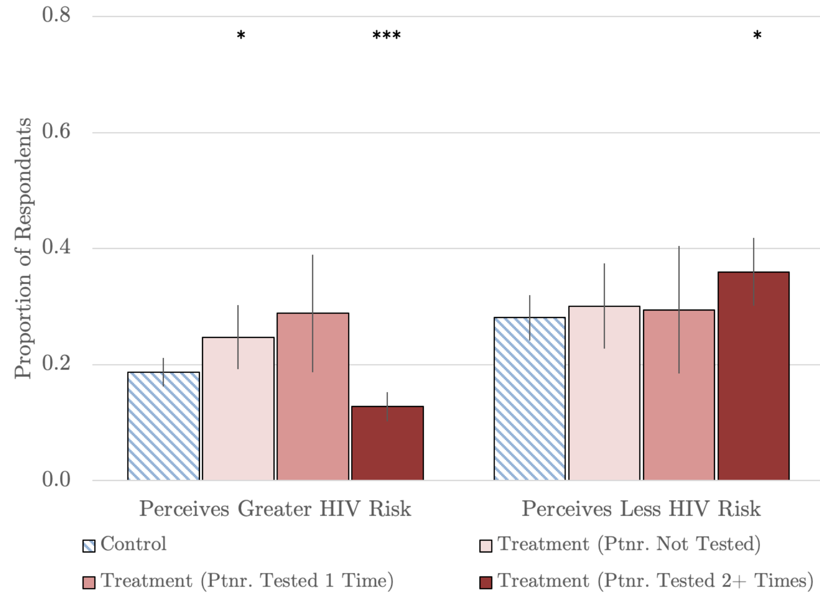
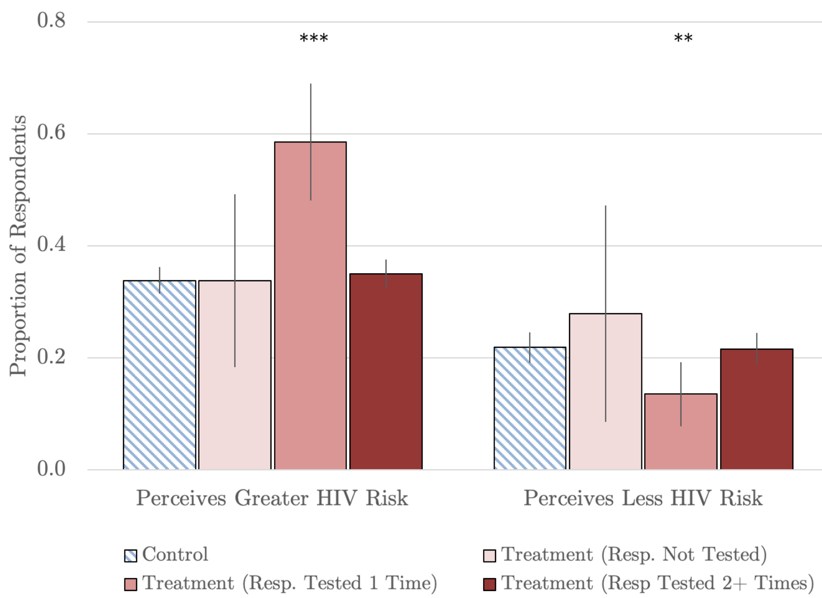


Figure 2: Probability of Testing within Four Months by Treatment Arm

Note: estimates are based on respondent reports about own testing and partner testing with any provider within the past four months. P-values and 90% confidence intervals are based on OLS regressions with respondent-clustered standard errors.



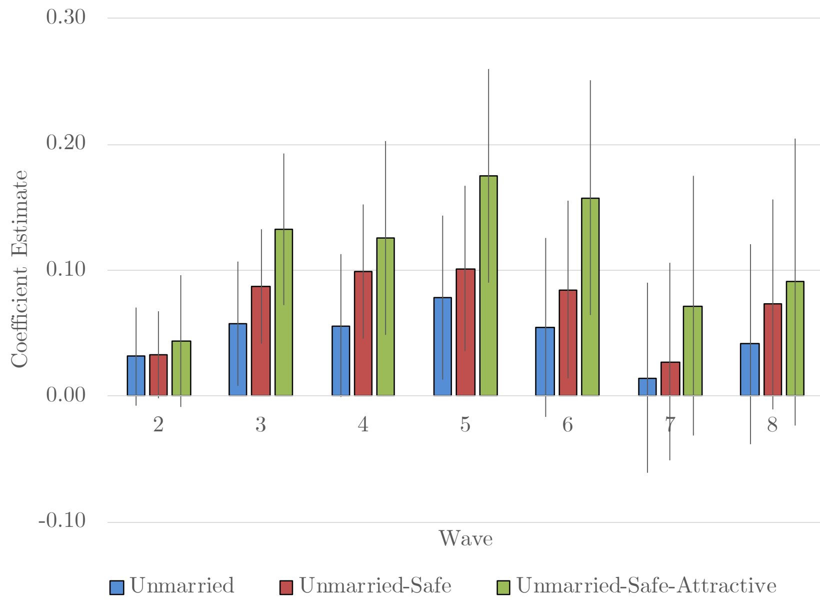
(a) Belief that Partner is HIV-Negative



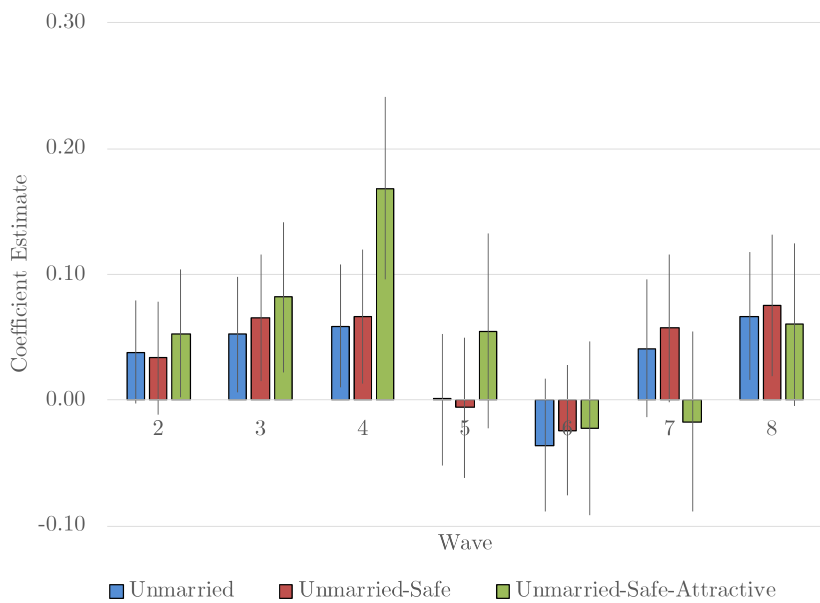
(b) Belief that Respondent is HIV-Negative

Figure 3: Belief Updating about Partner's and Own HIV Status

Note: the figure shows the proportion of respondents who perceived greater HIV risk (on the left) and less HIV risk (on the right) for their partners (Panel A) and themselves (Panel B). Respondents perceived greater risk if their risk assessment was lower in Wave 1 than in Waves 2-8. Respondents perceived less risk if their risk assessment was higher in Wave 1 than in Waves 2-8. We omit the proportion with no change in perceptions for clarity. The figure groups respondents by intervention arm and partner or own testing frequency. Partner risk perceptions are measured on a 1-5 Likert scale. Own risk perceptions are measured on probability scale. Panel A limits the sample to respondents who had a partner in Wave 1 and the follow-up wave being measured. Error bars show 90% confidence intervals and stars indicate statistically significant differences from the control group according to OLS regressions with respondent-clustered standard errors. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.



(a) Impacts on Marriage



(b) Impacts on Pregnancy

Figure 4: Impacts on Marriage and Pregnancy by Wave

Note: the figure shows treatment effect estimates on marriage (Panel A) and pregnancy (Panel B) by survey wave. Estimates are based on OLS regressions that follow Equation (9) and interact T_i with wave dummies. Estimates for unmarried (blue), unmarried-safe (red) and unmarried-safe-attractive (green) subsamples are based on separate regressions. The figure shows 90% confidence intervals based on respondent-clustered standard errors.

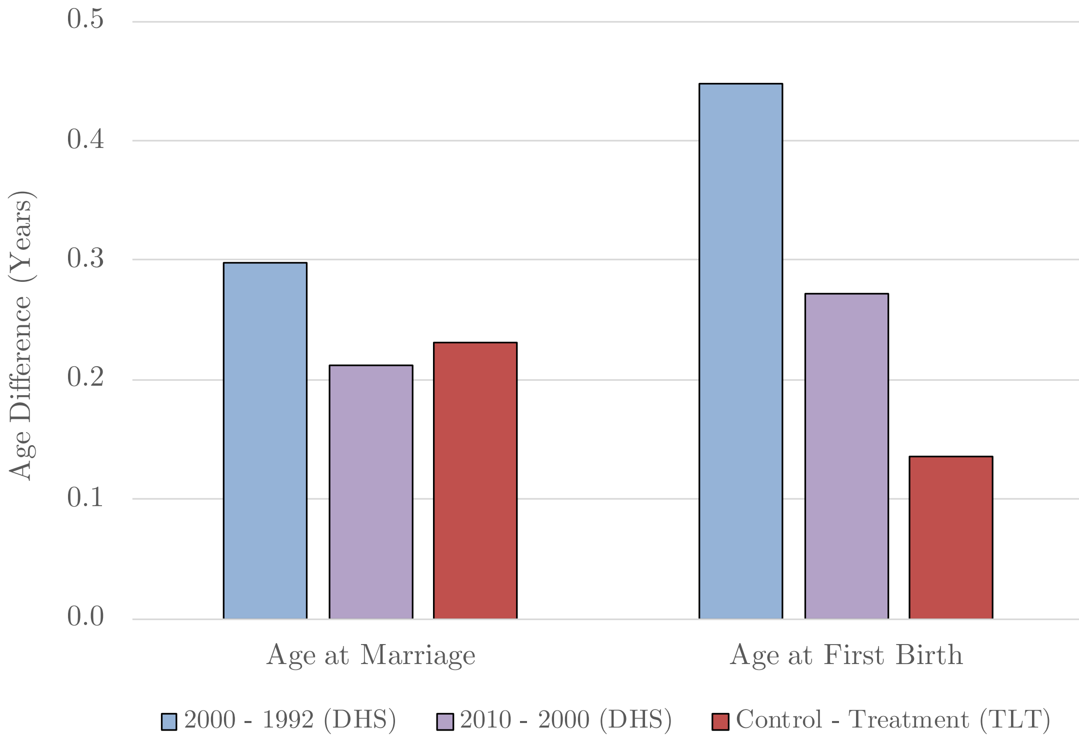


Figure 5: A Comparison of HIV Testing Impacts to National Marriage and Fertility Patterns

Note: the blue and purple bars use data on the age at first marriage and the age at first birth for women aged 18-28 in the 1992, 2000, and 2010 rounds of the Malawi Demographic and health Survey (DHS). Blue bars indicate the increase in these outcomes from 1992 to 2000 and purple bars indicate the decrease in these outcomes from 2000 to 2010. Red bars are based on treatment effect estimates from the study sample, which are reweighted to match the 2000 DHS age distribution.

Online Appendix – Not for Publication

A The Model Under Alternative Assumptions

This appendix explores alternative theoretical assumptions regarding the ranking of marital surplus, divorce, and the correlation between attractiveness and safety.

A.1 Non-Prohibitive Divorce Costs

Our main model rules out divorce by assuming that it is prohibitively costly. An alternative assumption is that people may divorce without cost at the beginning of Period 2. Costless divorce removes the incentive to delay marriage, so that everyone marries in Period 1 and perfectly assortatively matches on attractiveness. At the beginning of Period 2, people who are mismatched on safety divorce and marry new partners with the same number of high traits. In such a setting, an intervention that removes asymmetric information has no effect on marriage timing because all participants already marry early. However, the intervention increases marital surplus for safe people by allowing them to marry early (and rematch with a better partner in Period 2, if necessary). Conversely, this intervention decreases marital surplus for unsafe people by allowing their safe spouses to divorce them.

A small but positive fixed cost of divorce leads to predictions that are qualitatively similar to our main model. Suppose that divorce costs vary among the population and are independent of all other traits. In that case, all unsafe people continue to marry in Period 1. However, the option of divorce induces some safe people, who would otherwise wait, to marry in Period 1. Removing asymmetric information accelerates marriage for safe people and increases (decreases) surplus for safe (unsafe) people, but to a lower extent than in a model without divorce.

A.2 Own Safety is Unknown in Period 1

Our main model assumes that safety information is private in Period 1. Alternatively, people may be initially unaware of their own safety types, but may know the population average safety, $\bar{s} = \frac{1}{n}\sum_i s_i$. This alteration does not affect the matching process: all participants continue to prefer attractive partners, who yield weakly higher surplus, and so people positively assortatively match on attractiveness in Period 1. As in our main model, some safe people mismatch with unsafe people in Period 1. Further, participants match on the number of high traits in Period 2, since by that time (own and other's) safety becomes public information.

In a key distinction with our main model, the realization of own safety does not affect marriage timing in this model because own safety is unknown in Period 1. Only the discount factor determines marriage timing, conditional on attractiveness. That is, a person of attractiveness a chooses to marry late if she is sufficiently patient. An intervention that reveals safety in Period 1 accelerates marriage for both safe and unsafe people to the same extent insofar as the discount factor is independent of safety. Moreover, this intervention increases marital surplus for safe people by improving match quality and accelerating marriage. It also has an ambiguous effect on the marital surplus of unsafe people, since it leads some of them to accelerate marriage but also reduces their average match quality.

A.3 Dependence Between Traits and Patience

Section 2 assumes that both attractiveness and safety are independent of the discount factor. In fact, attractiveness and safety may be positively correlated with patience, since each variable is related to socioeconomic status. This correlation does not affect the matching process, which does not depend on the discount factor. It also does not affect marriage timing for unsafe people, who continue to marry early irrespective of the discount factor.

This correlation may alter marriage timing for some people. Consider first a positive correlation between safety and the discount factor. Safe people of attractiveness a delay marriage if their discount factor exceeds a threshold, $\delta_i > \delta^a$. If the positive correlation between safety and the discount factor increases the proportion of safe people whose δ_i exceeds this threshold, more safe people delay marriage. Similarly, a positive correlation between attractiveness and patience may exacerbate marriage delays by attractive and safe people (compared to unattractive and safe people) by increasing the share of safe and attractive people for whom $\delta_i > \delta^a$. As in our main model, an intervention that removes asymmetric information in Period 1 accelerates marriage for safe people (and possibly more for safe and attractive people). This correlation may confound interactions between safety (or attractiveness and safety) and treatment empirically. We address this issue in Section 8.1 by controlling for the interaction of T with observable time preference variables.

A.4 Dependence Between Traits and Gender

For simplicity, our main model assumes that the distributions of safety and attractiveness are the same for men and women. We believe that this assumption is not essential for our key predictions since the incentives for unsafe people to marry early and for safe people to marry late do not depend on the frequencies of these traits. Abstracting from attractiveness, people who marry in Period 1 perceive that safe and unsafe partners (whom they cannot distinguish) yield the same surplus on average. By contrast, people who marry in Period 2 perceive that safe partners are better than average and unsafe partners are worse.

To see this point more formally, consider an environment in which the frequencies of safety and attractiveness differ by gender in an unspecified way. In this setting, people may match with partners of different attractiveness and/or safety levels in either period. The deferred acceptance algorithm ensures that in Period 2, everyone who remains unmarried will match as follows. Men and women with two high traits match until there are no more such people in one gender. Then, some people with two high traits find partners with one high trait, and so on until everyone is matched.[34] There is a similar assignment mechanism

[34]Because there are equal numbers of men and women, nobody remains unmatched by Period 2.

in Period 1, with the difference that safety is unobserved, so people match by attractiveness only.

Within this framework, people choose when to marry based on the probabilities of marrying each type of partner in either period. These probabilities depend on the frequencies of 2, 1, or 0 high-trait partners of both genders, which are all common knowledge in equilibrium. A person of attractiveness $a = l, h$ and safety $s = l, h$ marries late if $(1+\delta_i)S^{as}(1) < \delta_i S^{as}(2)$, where $S^{as}(1)$ is the per-period surplus from marrying in Period 1, while $S^{as}(2)$ is marital surplus from marrying in Period 2. A necessary condition to marry in Period 2 is that $S^{as}(2) > S^{as}(1)$. Because of asymmetric information in Period 1, this condition is more likely to hold for safe people than for unsafe people. Under positive assortative matching on the number of high traits, attractive and safe people receive better matches in Period 2 than in Period 1 since they can demonstrate that they have two high traits in Period 2 but only that they have at least one high trait (attractiveness) in Period 1. Similar logic applies for unattractive and safe people. Conversely, marrying early yields more surplus for unsafe people, who have fewer than the average number of high traits in the Period 1 marriage pool. Therefore, in general, safe people are more likely to delay marriage than unsafe people regardless of the distributions of safety and attractiveness by gender.[35]

In contrast to our earlier approach, some unsafe people may marry in Period 2 in this more general framework. As in our main model, removing asymmetric information in Period 1 leads everyone to marry early. However, since safe people delay marriage more frequently than unsafe people in the status quo, we continue to predict a larger impact on marriage timing for safe people.

A.5 Surplus Rank Order and Period 1 Matching

In our main model, attractiveness and safety contribute equally to marital surplus, so that an attractive and unsafe partner yields the same surplus as an unattractive and safe partner in Equation (1). Under this surplus ranking, people who marry in Period 1 weakly prefer attractive partners, which leads to positive assortative matching on attractiveness in Period 1. Allowing either attractiveness or safety to make a larger contribution to surplus does not change this equilibrium. People assortatively match on attractiveness in Period 1 as long as attractive and unattractive partners do not yield the same average surplus. To illustrate this finding, we describe matching process under two alternative surplus rankings. Let $\lambda \in [0, 1]$ be the safe proportion of attractive partners and let $\theta \in [0, 1]$ be the safe proportion of unattractive partners in Period 1, so that attractive partners provide an average per-period surplus of $H = \lambda S^{ab}_{hh} + (1 - \lambda)S^{ab}_{hl}$ and unattractive partners provide an average per-period surplus of $L = \theta S^{ab}_{lh} + (1 - \theta)S^{ab}_{ll}$.

First, suppose that attractiveness contributes more to surplus than safety, so that $S^{ab}_{hh} > S^{ab}_{hl} > S^{ab}_{lh} > S^{ab}_{ll} > 0$. Under this alternative surplus ranking, people continue to prefer attractive partners because $H > L$ for all λ and θ. People who marry in Period 1 assortatively match on attractiveness as in our main model.

[35] Marriage timing is endogenous to the timing decisions of others in the marriage market. For a safe person, the incentive to marry early is stronger if other safe people also choose to marry early. However it remains the case that safe people have a stronger incentive to delay than unsafe people.

Next, suppose that safety yields more surplus than attractiveness, so that $S^{ab}_{hh} > S^{ab}_{lh} > S^{ab}_{hl} > S^{ab}_{ll} > 0$. Since $S^{ab}_{lh} > S^{ab}_{hl}$, $H \gtrless L$ depending on the values of λ and θ. If λ is sufficiently large, then $H \geq L$ and attractive partners yield weakly higher surplus in Period 1, as in our main model. However for sufficiently small values of λ, $H < L$, and attractive partners yield *less* average surplus than unattractive partners in Period 1. This situation preserves positive assortative matching on attractiveness in Period 1. Under the deferred acceptance algorithm, unattractive people first match together and then attractive people match together. Assortative matching on attractiveness only fails in the knife-edge case in which people are indifferent between attractive and unattractive partners.

Our predictions about the impact of the intervention on marriage timing and surplus are the same for each of these surplus rank orders: removing asymmetric information accelerates marriage and increases surplus for safe people. It doesn't change marriage timing, but decreases its surplus for unsafe people.

B Treatment Effects on Surplus by Attractiveness

The model predicts that removing asymmetric information decreases marital surplus for unsafe people and increases it for safe people. In the safe subpopulation, removing asymmetric information has a larger impact on attractive people if adverse selection is also stronger for attractive people. Recall that $r_{hh} \geq r_{lh}$ and $\frac{p_{hl}}{p_{hh}} \geq \frac{p_{ll}}{p_{lh}}$ are sufficient conditions for stronger adverse selection for attractive people, meaning that $\frac{\mu_{hh}}{p_{hh}}^* < \frac{\mu_{lh}}{p_{lh}}^*$. The increase in surplus from removing asymmetric information is a weighted average of two components. The first component is the benefit for people who shift marriage from Period 2 to Period 1. This amount, S^{ah}_{ah}, is always larger for attractive people. The second component is the benefit for people who continue to marry in Period 1 but are now assured of a safe match. We can rewrite this component as:

$$\frac{S^{ah}_{ah} - S^{ah}_{al}}{\frac{\mu^*_{ah}}{p_{al}} + 1} \;\geq\; 0$$

This expression is weakly greater for attractive people if $r_{hh} \geq r_{lh}$, $\frac{p_{hl}}{p_{hh}} \geq \frac{p_{ll}}{p_{lh}}$, and $\frac{\mu_{hh}}{p_{hh}}^* < \frac{\mu_{lh}}{p_{lh}}^*$, the same conditions that lead to greater adverse selection for attractive people in Section 2.4.

In the unsafe subpopulation, removing asymmetric information also has a larger (negative) impact for attractive people if adverse selection is stronger for attractive people. We rewrite the impact on unsafe surplus as

$$\frac{S^{al}_{al} - S^{al}_{ah}}{\frac{p_{al}}{\mu^*_{ah}} + 1} \;\leq\; 0$$

This expression is weakly smaller (more negative) for attractive people if $r_{hh} \geq r_{lh}$, $\frac{p_{hl}}{p_{hh}} \geq \frac{p_{ll}}{p_{lh}}$, and $\frac{\mu_{hh}}{p_{hh}}^* < \frac{\mu_{lh}}{p_{lh}}^*$, which are the same conditions in Section 2.4 and the previous paragraph.

C Multiple Equilibria and the Demand for HIV Testing

This section describes the demand for HIV testing as a coordination game. Section 2 argues that frequent HIV testing has substantial marriage market benefits for safe people. It may therefore seem paradoxical that only a minority of respondents have ever been tested at baseline. While testing is nominally free, seeking an HIV test entails substantial costs in terms of both inconvenience and stigma. The stigma cost decreases in the number of others who also seek testing. In an environment in which few people test, seeking a test may connote promiscuity and HIV risk to observers in the community. This cost is lower if seeking an HIV test is commonplace. The positive externality of seeking a test means that there may be multiple equilibria in which either many or few people seek HIV testing.

We illustrate this result through a simple, static, two-player model, although the principle easily generalizes to n players. Each player must choose whether to obtain an HIV test. Testing has benefit $\beta \geq 0$, which may represent the marriage market signaling value or the expected benefit of receiving treatment if the player tests positive. Testing entails two costs: a transportation cost, $\gamma \geq 0$, and a stigma cost $\mu \geq 0$. γ includes the monetary and time costs of traveling to the clinic and waiting in line. μ represents testing stigma, which is present only if a player tests unilaterally. The following matrix represents this game.

		Player 2	
		Test	No Test
Player 1	Test	$\beta - \gamma\ ,\ \beta - \gamma$	$\beta - \gamma - \mu\ ,\ 0$
	No Test	$0\ ,\ \beta - \gamma - \mu$	$0\ ,\ 0$

The equilibria of this game depend on the relative magnitudes of β, γ, and μ. We consider three scenarios that differ in terms of the value of γ. In Scenario 1, $\gamma > \beta$, so that HIV testing is not optimal regardless of μ. Non-testing is the dominant-strategy equilibrium in this scenario. Scenario 2, in which $\beta > \gamma > \beta - \mu$, features multiple Nash equilibria in which players either both test or both do not test. Neither player has an incentive to deviate from the non-testing equilibrium because she incurs stigma as the only tester. Finally in Scenario 3, $\beta - \mu > \gamma$, so that testing is the dominant-strategy equilibrium.

The intervention reduces γ by providing free, opt-out HIV testing. In the game, a decline in γ that moves from Scenario 1 to Scenario 2 is unlikely to increase testing because people lack the incentive to deviate from an existing non-testing equilibrium. However a decline in γ that moves from Scenario 2 to Scenario 3 may dramatically increase testing by eliminating non-testing as a Nash equilibrium. The model also shows that people may fail to test despite a large benefit of testing, β, if testing is stigmatized and the community is in a non-testing equilibrium. The demand for testing is highly elastic with respect to γ in the range for which $\gamma \approx \beta - \mu$.

D Self-Reported Sexual Behavior and Conception Desires

Since unprotected sex is necessary for pregnancy, the impact on pregnancy in the paper suggests that the intervention also increased the frequency of unprotected sex. An examination of the impact on sexual behavior may illuminate whether pregnancies occurred intentionally or as a byproduct of additional risky sexual behavior. However, the pervasive misreporting of sexual behavior is an intractable challenge. In general, many people are unwilling to disclose aspects of their sexual behavior to surveyors, particularly on sensitive topics such as extramarital affairs (Kelly et al. 2013, Fenton et al. 2001). Social desirability bias creates non-classical measurement error since misreporting is greater for respondents who are the most sexually active.

We first examine concurrency, in which people maintain multiple sexual relationships at the same time. Only 1 percent of respondents in our data admit to this practice, despite the documented prevalence of this phenomenon in Malawi (Helleringer and Kohler 2007). We also consider coital frequency within the past four weeks, which we disaggregate into sex with the respondent's husband and sex with other people. 85 percent of unmarried respondents deny being sexually active, which raises another misreporting concern. Finally, we approximate the frequency of unprotected sex by multiplying the coital frequency by an indicator for infrequent condom use.

Estimates for these outcomes appear in Table A1. Column 1 shows small and insignificant effects on concurrency, which is unsurprising given the low reported prevalence of this activity. Columns 2-5 show impacts on coital frequency overall, with the spouse, with other people, and without protection. Consistent with our model, Column 2 (Panel B) shows that the intervention increased the frequency of sex for safe respondents but decreased the frequency of sex for unsafe respondents ($p = 0.01$ for this difference). Panel C shows that within the safe subsample, the frequency of sex increased differentially for attractive respondents ($p = 0.15$). Columns 3 and 4 show that sex with husbands rather than other people drove this pattern, which supports the interpretation of pregnancy as a proxy for marital surplus. Finally, Column 5 confirms that the intervention increased unprotected sex, which is necessary for pregnancy.

Next we examine how the desire to conceive with the current partner changes by arm. Non-pregnant respondents were asked, "If you found out today that you were pregnant by [NAME], would that news be (1) very bad, (2) fairly bad, (3) neither good nor bad, (4) fairly good, and (5) very good." Respondents also indicated (on the same scale) how they perceived the partner would take the news. Figure A1 uses our standard empirical specification to examine how these outcomes vary by treatment arm for married and non-pregnant respondents and their husbands. Safe treatment respondents were more interested than safe control respondents in having children with their current partner ($p = 0.04$). By contrast, unsafe treatment respondents were less interested than unsafe control respondents in conception ($p = 0.19$). This pattern is similar for the conception desires of husbands (as expressed by respondents). These findings, which align with the predicted impacts on marital surplus in our model, suggest that the intervention primarily encouraged intended pregnancies among married couples.

E Components of the "Own Risky Behavior" Safety Measure

In Section 5.2, we identify "safe" respondents based on the absence of several baseline risky behaviors. We classify respondents as safe if (1) they have ≤ 2 lifetime partners, (2) have ≤ 1 partners in the past year, (3) do not have multiple partners for money, (4) have sex ≤ 3 times per week, and (5) have never taken ART. We selected these thresholds to isolate the riskiest quartile of the distribution for each variable. In the treatment group, which received HIV tests at baseline, HIV prevalence is 62 percent lower among respondents who exhibit no risky behaviors.

Figure A2 shows the differential treatment effects on marriage and pregnancy for baseline-unmarried, safe respondents according to each risk factor individually. Estimates by individual risk factors are similar to the overall estimates, which appear at the right of the figure and align with the difference between safe and unsafe estimates in Table 4. This pattern suggests that results are not sensitive to the particular selection of baseline risky behaviors in the safety measure.

F Age-Unweighted Estimates

This section provides additional detail regarding the age imbalance in the data. Figure A3 shows the unweighted age distributions of the treatment and control arms. Treatment respondents are 0.6 years older than control respondents. This imbalance arises because there are around 57 "extra" control respondents who are 15 or 16 years old. There are no other notable differences in the age distributions. The analysis in the paper relies on entropy weights to establish balance on the first three moments of the age distribution (Hainmueller 2012, Hainmueller and Xu 2013). This procedure re-weights the control respondents so that the age distributions in the treatment and control arms are equivalent. Intuitively, it places less weight on young control respondents. Re-weighting by age is not equivalent to including age as a covariate if the treatment effect varies by age. In practice, however, re-weighting by age and controlling for age yield similar estimates in our regressions.

The paper incorporates age-unweighted baseline summary statistics in Table 2 and overall impacts in Table 3.[36] To supplement these results, Table A2 provides age-unweighted estimates by safety (following Table 4) and Table A3 provides age-unweighted estimates by attractiveness (following Table 5). The odd columns of these tables include all respondents and the even columns only include respondents who are 17 or older, for whom age is already balanced without weighting. Results closely resemble our primary results, which provides additional evidence that the age imbalance and the weighting procedure are unlikely to change our findings.

[36] Age-unweighted versions of Tables A10 and A11, which show baseline summary statistics by safety and attractiveness, are available from the authors.

G Dynamics of Testing, Beliefs, Marriage, and Pregnancy

Tables A4-A7 show the progression of the key study outcomes over the sample period. The tables report the means of HIV testing, perceived safety, participation in romantic relationships, marriage, and pregnancy by wave and intervention arm. We show these statistics for our three key estimation samples: baseline-unmarried respondents (the "U" sample, $n = 589$), baseline-unmarried and safe respondents (the "S" sample, $n = 413$), and baseline-unmarried, safe, and attractive respondents (the "A" sample, $n = 234$). We compute means over the non-attriters in each wave and weight to balance by age. These tables reaffirm our interpretations in several key ways, as we explain below.

Table A4 shows these patterns for HIV testing in the previous four months, subjective HIV risk, and relationship participation, as reported by the respondent. Since respondents tested after answering the questionnaire, Wave 2 is the first opportunity to observe treatment effects. Partner testing data is missing from the table because this variable is not available in Round 1. We find that the intervention caused an increase in testing that was large (more so for respondents than for their partners), persistent across waves (consistent with the notion that repeated testing is important to reveal one's type), and similar across the three groups (thus ruling out that safe and attractive samples have bigger effects in marriage and pregnancy because they tested more frequently).

The table further shows the evolution of the respondent's perception of HIV risk for herself and her partner. The modestly higher HIV risk in the treatment group reflects a minority of respondents who learned from the intervention that they were HIV positive. Means for partner HIV risk by wave fail to show a clear pattern. This occurs because some women revised their beliefs about partner's safety up and others revised their beliefs down, depending on partner's behavior, as discussed in Section 5.3.

Lastly, Table A4 shows the evolution of relationship participation. Relationships include both marriages and less formal partnerships. Someone began a relationship/marriage in Wave t if she transitioned from being single in Wave $t-1$ to being attached in Wave t or if she remained attached in Waves $t-1$ and t but the identity of the partner changed. Likewise, she ended a relationship/marriage in Wave t if she transitioned from being attached in Wave $t-1$ to being single in Wave t or if she remained attached but the identity of the partner changed. These variables, which rely on changes from the previous wave, are not defined in Wave 1. In principle, one can compute the value of "in a relationship" in Wave $t+1$ from the level and change in this variable in Wave t. However, this accounting identity does not always hold in practice because the sample varies from wave to wave according to which respondents participated. Appendix H discusses attrition further.

There are three notable patterns in these data. First, the intervention caused relationship turnover: more relationships began and ended among treatment respondents in Waves 2-4. This pattern is consistent with information revelation among partners in these waves. Secondly, while relationship participation was higher in the treatment group up to Wave 6, the control group caught up substantially between Waves 7 and 8. This pattern suggests that the intervention accelerated the formation of relationships, consistent with our model. Third, the effect sizes are larger for safe (and largest for safe and attractive) participants, consistent with the model.

Table A5 shows the evolution of marriage and pregnancy. All respondents were unmarried in Wave 1 by construction. The intervention caused both higher turnover (up to Wave 5) and an increase in the share of married participants (for all waves), which was larger for safe participants (and largest for safe and attractive participants). Importantly, we do not see an immediate jump in marriage in Wave 2, consistent with our hypothesis that repeated testing is necessary to overcome asymmetric information. Pregnancy patterns generally track marriage patterns but reflect the cyclicality of pregnancy: higher rates in Waves 3-4 in the treatment group led to lower rates in Waves 5-6; likewise, higher rates in Waves 6-7 in the control group led to lower rates in Wave 8. Only 4-6 percent of unmarried respondents were pregnant at baseline, which is consistent with our interpretation of pregnancy as a proxy for marital surplus.

Finally, Tables A6 and A7 reproduce these tables for the single-test intervention. Because the single-test sample received HIV tests in Wave 4, this analysis uses Wave 4 as a baseline and Waves 5-8 as follow-up rounds. Although the single-test and multiple-test interventions have the same control group, the control group means in these tables differ from Tables A6 and A7 because the sample here is limited to respondents who were unmarried at Wave 4. There was a jump in testing at Wave 5, which is consistent with the implementation of this intervention. The modest treatment effect on partner testing in Waves 6-8 arises because the intervention led some partners to seek their own tests elsewhere. Table A7 does not show a clear positive impact on marriage or pregnancy, as Table 6 illustrates. An examination of Waves 5-8 shows that treatment respondents were not systematically more likely to be married or pregnant than control respondents. These patterns support the hypothesis that repeated testing is necessary to effect changes in the marriage market.

H Attrition

This appendix examines the impact of attrition on our analysis. The TLT Panel Study includes eight waves over 28 months. Surveyors were unable to complete 12 percent of the interviews. Respondents completed an average of 7 survey rounds, and 71 percent of respondents completed all eight rounds. Of those who completed fewer than 8 rounds, 43 percent missed only one or two rounds.

Table A8 provides baseline summary statistics by attrition status. Non-attriters in Column 1 completed all eight survey waves while attriters in Column 2 completed fewer than eight waves. Attriters apparently had higher socioeconomic status than non-attriters. They were less likely to be married and more likely to be enrolled in school. Since HIV status was measured at endline for the control group, we cannot reliably contrast the HIV status of attriters and non-attriters, however attrition is uncorrelated with HIV status in the treatment group. Attrition is also uncorrelated with treatment: 73 percent of treatment respondents completed all eight waves, compared to 70 percent of control respondents ($p = 0.35$).

Table A9 reproduces our main estimates among the sample of non-attriters. Estimates closely resemble the main results in the paper. Effects are larger for safe and attractive respondents, with magnitudes that correspond closely with our main estimates. While we cannot rule out bias in treatment effect estimates due to attrition, these results suggest that attrition is not a major confound.

I Baseline Covariates by Safety and Attractiveness

Section 8.1 shows evidence of differential treatment effects by safety and attractiveness. This appendix provides additional context for these estimates by showing how baseline respondent characteristics vary along these dimensions. Table A10 cuts the sample by safety according to "own risky behavior" in Columns 1-3 and according to "own perceived HIV risk" in Columns 4-6. The table limits the sample to baseline-unmarried respondents for consistency with our earlier estimates. Safe people are younger, more attractive, and less likely to be employed. They perceive substantially lower HIV infection risk for themselves and their partners. Table A11 cuts the safe sample (according to both definitions) by attractiveness. These samples correspond to the estimation samples in Panels A and B of Table 5. Attractive respondents are more likely to be enrolled in school rather than working. They are also wealthier and more future oriented.

Since several variables are correlated with safety and attractiveness, the even columns of Tables 4 and 5 control for the interaction of treatment with baseline covariates. Estimates are robust to the inclusion of controls, suggesting that the heterogeneous treatment effects by safety and attractiveness are not spurious.

J Estimates by Attractiveness for Unsafe Respondents

Table 5 in the paper examines impact heterogeneity by attractiveness among safe respondents. However, the model also predicts that, within unsafe people, the intervention may reduce marital surplus more for attractive people (under the conditions discussed in Appendix B). We do not study the differential effects by attractiveness for unsafe respondents because we observe only 91 or 112 of them (depending on the safety definition used). This small sample may make us underpowered to identify attractiveness interactions.

Table A12 reproduces Table 5 for unsafe respondents. As expected, there are no statistically significant attractiveness interactions for either marriage or pregnancy.[37]

K The Impact on Intra-Household Bargaining Power

The HIV testing intervention could influence intra-household bargaining power by altering the threat points of partners (McElroy and Horney 1981). It is unclear whether this phenomenon favors men or women. If the intervention increased the bargaining power of husbands, it could increase pregnancy rates because husbands generally prefer to have more children than wives (Rasul 2008). This pattern is not evident among couples who jointly participated in this study. Baseline-unmarried women with partners desire 3.32 children, while their partners desire 3.29 children ($p = 0.39$).

[37] We find a large negative effect on marriage of attractive people in Panel A, once we condition on the interaction of treatment and covariates in Column 2. However this pattern does not appear consistently across other specifications. Upon investigation, it appears that this effect arises because attractiveness is more strongly correlated with several SES characteristics in the unsafe sample than in the safe sample.

This appendix explores the impact of the intervention on three indicators of intra-household bargaining power. Respondents identified who is "generally in charge" within the relationship, including the possibility of equal control. We define an indicator that the partner is in charge, which is true for 83 percent of respondents with partners. Secondly, the survey elicited whether the respondent's partner had ever hurt her by beating her, which 5 percent of respondents answered affirmatively. Finally, the survey elicited whether the respondent's partner had ever forced her to have sex against her wishes, which 22 percent of respondents answered affirmatively.

Table A13 reproduces our primary specifications for these outcomes. Since bargaining variables are only available in Waves 3 and 5, sample sizes are reduced and these regressions do not control for the baseline dependent variable. We find no significant effects on these outcomes, either overall or for the safe and attractive sub-samples. Coefficients are small and have inconsistent signs across outcomes, which suggests that the intervention did not have a meaningful impact on bargaining power.

L Additional Summary Statistics and Results

L.1 Single-Test Intervention

Section 9 finds no effects of a single-test intervention on marriage and pregnancy. Here we offer additional background for this result. Table A14 provides summary statistics for the single-test arm and the control arm in Wave 4, which functions as a baseline in this construction. Both marriage and pregnancy are balanced across intervention arms in Wave 4. However single-test respondents are more likely to have ever been tested and more likely to be HIV positive. Attrition is balanced across intervention arms: 81 percent of single-test respondents completed all of Waves 4-8, compared to 79 percent of control respondents ($p = 0.37$). Following Figure 3 in the paper, Figure A5 examines the effect of the single-test intervention on belief updating. Unlike under the multiple-test intervention, respondents in the single-test arm did not update their beliefs about partner HIV risk.[38]

L.2 Baseline Summary Statistics for the Full Sample of Married and Unmarried Respondents

The paper's primary analysis focuses on baseline-unmarried respondents and Table 2 provides baseline summary statistics for this sample. To support the full-sample results in Panel A of Table 3, Table A15 provides baseline summary statistics for the full sample. As before, Columns 1-3 show that age is not balanced across intervention arms, however Columns 4-5

[38]Within the single-test arm, 12 percent of respondents were not tested and 88 percent of respondents were tested once. In addition, 48 percent of partners were not tested and 52 percent of partners were tested once.

show that no covariates in the table differ significantly by arm after weighting to balance by age.

L.3 Learning about Partner Safety by Baseline Beliefs

Figure A6 reproduces Figure 3A while distinguishing between respondents who, at baseline, were certain that their partners were HIV-negative (54 percent of respondents) and those who thought their partners might be HIV-positive (46 percent of respondents). This figure shows that belief updating about partner's HIV status was driven by respondents who were uncertain at baseline and whose partners tested multiple times. In Panel A, someone who at baseline was certain that her partner was HIV-negative could not revise her belief about her partner's HIV-negative status upward. Baseline-uncertain respondents whose partners tested zero or one times did not revise upward their beliefs that partners were HIV-negative. This pattern is consistent with the notion that testing once when repeated testing is available signals that a partner may be HIV-positive.

Table A1: The Impact on Self-Reported Sexual Behavior

	Multiple Partners	Coital Frequency			
		Overall	Spouse	Others	Unprot.
	(1)	(2)	(3)	(4)	(5)
A: Overall Estimates					
Treatment	0.00045	-0.14	-0.17	-0.0040	-0.27
	(0.0030)	(0.23)	(0.23)	(0.027)	(0.21)
	[0.01]	[4.20]	[4.04]	[0.16]	[2.97]
Observations	5991	5991	5991	5991	5991
B: Baseline-Unmarried Estimates by Safety (Defined by Own Risky Behavior)					
Treatment · Safe	0.0012	0.61**	0.59**	-0.004	0.36*
	(0.0041)	(0.24)	(0.24)	(0.039)	(0.20)
	[0.00]	[1.10]	[0.89]	[0.22]	[0.80]
Treatment · Unsafe	0.027	-1.36*	-1.38*	-0.13	-1.02
	(0.021)	(0.75)	(0.77)	(0.20)	(0.64)
	[0.01]	[4.02]	[3.43]	[0.60]	[2.67]
Equality of coefficients (p-value)	0.21	0.01	0.01	0.56	0.04
Observations	3405	3405	3405	3405	3405
C: Baseline-Unmarried and Safe (Defined by Own Risky Behavior) Estimates by Attractiveness					
Treatment · Attractive	-0.00041	0.93***	0.92***	-0.004	0.64**
	(0.0063)	(0.34)	(0.35)	(0.052)	(0.30)
	[0.01]	[0.94]	[0.70]	[0.24]	[0.68]
Treatment · Not Attractive	0.0044	0.21	0.13	0.005	-0.031
	(0.0034)	(0.36)	(0.35)	(0.058)	(0.26)
	[0.00]	[1.39]	[1.20]	[0.19]	[1.00]
Equality of coefficients (p-value)	0.49	0.15	0.11	0.91	0.09
Observations	2869	2869	2869	2869	2869

Note: standard errors are clustered by respondent and appear in parentheses. Subgroup-specific control group means appear in brackets. Regressions reweight to balance by age. Panel A uses the specification in Equation (9). Panel B is limited to baseline-unmarried respondents and uses the same specification as Panel A of Table 3. Panel C is limited to baseline-unmarried respondents who are safe (defined by the absence of baseline risky behavior) and uses the same specification as Panel A of Table 5. "Multiple Partners" is an indicator of ≥ 2 partners in the past four months."Coital Frequency" is the number of sexual contacts in the past four weeks. Columns 3 and 4 distinguish between contacts with the respondent's spouse and contacts with other people. Column 5 shows the frequency of unprotected sex, which we approximate by multiplying the coital frequency by an indicator for irregular condom use. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Age-Unweighted Impacts on Marriage and Fertility for Baseline-Unmarried Respondents, by Safety

	Currently Married		Currently Pregnant	
	(1)	(2)	(3)	(4)
A: Safety Defined by Own Risky Behavior				
Treatment · Safe	0.078***	0.081**	0.041***	0.045**
	(0.028)	(0.040)	(0.014)	(0.019)
	[0.13]	[0.16]	[0.09]	[0.10]
Treatment · Unsafe	-0.041	-0.10	-0.012	-0.017
	(0.081)	(0.088)	(0.037)	(0.042)
	[0.37]	[0.45]	[0.15]	[0.16]
Equality of coefficients (p-value)	0.16	0.06	0.18	0.17
B: Safety Defined by Own Perceived HIV Risk				
Treatment · Safe	0.082***	0.084**	0.045***	0.063***
	(0.029)	(0.041)	(0.014)	(0.018)
	[0.14]	[0.18]	[0.08]	[0.08]
Treatment · Unsafe	0.021	-0.077	-0.010	-0.073*
	(0.068)	(0.084)	(0.032)	(0.040)
	[0.26]	[0.35]	[0.16]	[0.21]
Equality of coefficients (p-value)	0.41	0.09	0.11	0.002
Sample	Full	Age $\geq$ 17	Full	Age $\geq$ 17
Observations	3427	1987	3427	1987

Note: estimates are based on the specification $Y_{it} = \beta_1[T_i \cdot S_i] + \beta_2[T_i \cdot (1 - S_i)] + \beta_3 S_i + \beta_4(1 - S_i) + \beta_5 Y_i^b + \delta_t + \varepsilon_{it}$. In this expression, Y is the dependent variable, Y^b is the baseline dependent variable, δ is a set of wave dummies, and T is a treatment indicator. S_i is a 'safety' indicator, which identifies respondents with zero HIV risk factors in Panel A and ≤ 0.1 baseline subjective HIV risk in Panel B, as the text explains. The table reports $\hat{\beta}_1$ and $\hat{\beta}_2$. Standard errors are clustered by respondent and appear in parentheses. Subgroup-specific control group means appear in brackets. Regressions do not reweight to balance by age. Even columns restrict the sample to respondents who are 17 or older at baseline, for whom age is uncorrelated with treatment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Age-Unweighted Impacts on Marriage and Fertility for Baseline-Unmarried and Safe Respondents, by Attractiveness

	Currently Married		Currently Pregnant	
	(1)	(2)	(3)	(4)
A: Safety Defined by Own Risky Behavior				
Treatment · Attractive	0.12***	0.095*	0.051***	0.057**
	(0.038)	(0.052)	(0.018)	(0.024)
	[0.10]	[0.15]	[0.08]	[0.09]
Treatment · Not Attractive	0.017	0.052	0.023	0.013
	(0.044)	(0.064)	(0.021)	(0.033)
	[0.18]	[0.19]	[0.11]	[0.14]
Equality of coefficients (p-value)	0.07	0.60	0.31	0.27
Observations	2881	1515	2881	1515
B: Safety Defined by Own Perceived HIV Risk				
Treatment · Attractive	0.13***	0.13**	0.060***	0.088***
	(0.037)	(0.051)	(0.018)	(0.024)
	[0.09]	[0.14]	[0.07]	[0.06]
Treatment · Not Attractive	0.015	0.0086	0.024	0.027
	(0.046)	(0.070)	(0.021)	(0.030)
	[0.21]	[0.26]	[0.10]	[0.11]
Equality of coefficients (p-value)	0.05	0.16	0.19	0.15
Observations	2753	1548	2753	1548
Sample	Full	Age $\geq$ 17	Full	Age $\geq$ 17

Note: estimates are based on the specification $Y_{it} = \beta_1[T_i \cdot A_i] + \beta_2[T_i \cdot (1 - A_i)] + \beta_3 A_i + \beta_4(1 - A_i) + \beta_5 Y_i^b + \delta_t + \varepsilon_{it}$. In this expression, Y is the dependent variable, Y^b is the baseline dependent variable, δ is a set of wave dummies, T is a treatment indicator, and A_i is an attractive indicator. The table reports $\hat{\beta}_1$ and $\hat{\beta}_2$. Standard errors are clustered by respondent and appear in parentheses. Subgroup-specific control group means appear in brackets. Panel A includes respondents with zero baseline HIV risk factors and Panel B includes respondents with baseline subjective HIV risk ≤ 0.1, as the text explains. Regressions do not reweight to balance by age. Even columns restrict the sample to respondents who are 17 or older at baseline, for whom age in uncorrelated with treatment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Means by Wave and Intervention Arm for the **Multiple-Test Intervention**

	Sample	Arm	Wave 1	2	3	4	5	6	7	8
Respondents tested in previous 4 months (%)	U	T	0.15	0.66	0.73	0.78	0.68	0.76	0.76	0.67
		C	0.12	0.16	0.23	0.27	0.29	0.31	0.28	0.25
	S	T	0.13	0.67	0.72	0.77	0.69	0.77	0.76	0.68
		C	0.10	0.14	0.21	0.24	0.26	0.30	0.28	0.26
	A	T	0.13	0.70	0.72	0.78	0.64	0.73	0.78	0.72
		C	0.13	0.15	0.23	0.24	0.25	0.36	0.29	0.27
Partners tested in previous 4 months (%)	U	T	–	0.60	0.52	0.43	0.47	0.57	0.57	0.52
		C	–	0.38	0.31	0.26	0.28	0.28	0.21	0.28
	S	T	–	0.62	0.54	0.47	0.45	0.59	0.55	0.53
		C	–	0.40	0.35	0.29	0.28	0.32	0.16	0.32
	A	T	–	0.59	0.60	0.41	0.48	0.67	0.67	0.55
		C	–	0.37	0.23	0.37	0.28	0.32	0.16	0.31
Own subjective HIV risk (prob. [0,1])	U	T	0.10	0.13	0.15	0.16	0.17	0.17	0.20	0.25
		C	0.09	0.12	0.13	0.16	0.16	0.16	0.17	0.23
	S	T	0.08	0.10	0.13	0.14	0.16	0.15	0.18	0.22
		C	0.07	0.10	0.11	0.13	0.12	0.14	0.15	0.21
	A	T	0.07	0.08	0.12	0.11	0.15	0.14	0.16	0.20
		C	0.08	0.11	0.12	0.12	0.12	0.13	0.13	0.21
Subjective HIV risk of partners (Likert 1-5)	U	T	1.5	1.4	1.4	1.4	1.4	1.5	1.5	1.5
		C	1.6	1.4	1.5	1.7	1.6	1.4	1.5	1.5
	S	T	1.5	1.3	1.3	1.4	1.4	1.4	1.4	1.4
		C	1.5	1.2	1.4	1.4	1.4	1.3	1.3	1.4
	A	T	1.4	1.4	1.3	1.3	1.3	1.4	1.3	1.4
		C	1.5	1.2	1.6	1.4	1.4	1.4	1.2	1.4
Respondents starting new relationships (%)	U	T	–	0.25	0.16	0.16	0.19	0.14	0.10	0.09
		C	–	0.20	0.16	0.11	0.20	0.15	0.06	0.16
	S	T	–	0.25	0.16	0.16	0.18	0.13	0.09	0.09
		C	–	0.19	0.17	0.10	0.19	0.14	0.07	0.17
	A	T	–	0.28	0.16	0.18	0.15	0.10	0.08	0.10
		C	–	0.21	0.12	0.11	0.19	0.13	0.06	0.23
Respondents ending prior relationships (%)	U	T	–	0.14	0.16	0.16	0.13	0.11	0.04	0.08
		C	–	0.14	0.14	0.11	0.10	0.11	0.11	0.07
	S	T	–	0.13	0.17	0.16	0.13	0.10	0.04	0.08
		C	–	0.11	0.13	0.13	0.10	0.10	0.10	0.08
	A	T	–	0.15	0.17	0.14	0.13	0.12	0.02	0.04
		C	–	0.12	0.15	0.12	0.08	0.13	0.10	0.05
Respondents in a relationship (%)	U	T	0.47	0.57	0.57	0.56	0.62	0.64	0.69	0.70
		C	0.44	0.48	0.51	0.51	0.59	0.62	0.59	0.68
	S	T	0.44	0.56	0.55	0.55	0.62	0.63	0.66	0.68
		C	0.40	0.46	0.50	0.48	0.55	0.58	0.57	0.66
	A	T	0.48	0.62	0.62	0.65	0.67	0.63	0.67	0.70
		C	0.42	0.49	0.46	0.48	0.58	0.56	0.53	0.73

Note: U = baseline unmarried sample, S = baseline unmarried and safe sample, and A = baseline unmarried, safe, and attractive sample. T = treatment arm and C = control arm. All means are computed over non-attriters in a particular wave. "Partners tested" and "Subjective HIV risk of partners" are calculated for women who have partners in Wave t. "Respondents tested" and "Partners tested" are calculated for individuals who have not previously tested positive within the study. "Partners tested" is not measured over the standard four-month interval in Wave 1. We do not observe the share of relationships that begin or end in Wave 1 because these variables rely on the difference between Wave $t-1$ and Wave t.

Table A5: Means by Wave and Intervention Arm for the **Multiple-Test Intervention**

	Sample	Arm	Wave 1	2	3	4	5	6	7	8
Respondents starting new marriages (%)	U	T	–	0.08	0.07	0.06	0.08	0.06	0.04	0.07
		C	–	0.05	0.04	0.05	0.06	0.08	0.09	0.03
	S	T	–	0.06	0.07	0.05	0.06	0.05	0.04	0.07
		C	–	0.03	0.02	0.03	0.06	0.06	0.09	0.02
	A	T	–	0.07	0.06	0.05	0.07	0.05	0.01	0.07
		C	–	0.03	0.01	0.01	0.05	0.06	0.08	0.03
Respondents ending prior marriages (%)	U	T	–	0.00	0.02	0.01	0.03	0.01	0.00	0.02
		C	–	0.00	0.01	0.01	0.02	0.01	0.02	0.01
	S	T	–	0.00	0.01	0.01	0.02	0.01	0.01	0.02
		C	–	0.00	0.00	0.01	0.00	0.01	0.02	0.01
	A	T	–	0.00	0.00	0.02	0.01	0.01	0.00	0.00
		C	–	0.00	0.00	0.00	0.00	0.01	0.02	0.02
Respondents who are married (%)	U	T	0.00	0.08	0.14	0.18	0.26	0.29	0.33	0.40
		C	0.00	0.05	0.08	0.13	0.18	0.24	0.31	0.36
	S	T	0.00	0.06	0.13	0.17	0.24	0.26	0.29	0.37
		C	0.00	0.03	0.04	0.07	0.14	0.18	0.26	0.30
	A	T	0.00	0.07	0.15	0.18	0.26	0.29	0.29	0.36
		C	0.00	0.03	0.02	0.06	0.09	0.13	0.22	0.28
Respondents starting new pregnancies (%)	U	T	–	0.06	0.07	0.08	0.06	0.07	0.10	0.05
		C	–	0.06	0.03	0.06	0.07	0.09	0.04	0.03
	S	T	–	0.07	0.07	0.08	0.04	0.06	0.11	0.06
		C	–	0.06	0.02	0.06	0.07	0.06	0.04	0.03
	A	T	–	0.05	0.08	0.12	0.06	0.05	0.07	0.05
		C	–	0.03	0.01	0.02	0.09	0.05	0.05	0.01
Respondents finishing prior pregnancies (%)	U	T	–	0.03	0.05	0.06	0.09	0.08	0.06	0.07
		C	–	0.03	0.02	0.05	0.04	0.07	0.09	0.06
	S	T	–	0.01	0.05	0.06	0.08	0.08	0.04	0.08
		C	–	0.02	0.02	0.05	0.03	0.08	0.07	0.05
	A	T	–	0.02	0.06	0.04	0.11	0.12	0.07	0.05
		C	–	0.03	0.01	0.03	0.02	0.05	0.09	0.05
Respondents who are pregnant (%)	U	T	0.06	0.10	0.13	0.15	0.13	0.11	0.16	0.15
		C	0.04	0.06	0.08	0.09	0.13	0.14	0.11	0.08
	S	T	0.05	0.10	0.14	0.15	0.12	0.09	0.16	0.15
		C	0.03	0.07	0.07	0.09	0.13	0.12	0.10	0.07
	A	T	0.06	0.09	0.12	0.21	0.16	0.11	0.10	0.11
		C	0.04	0.04	0.04	0.04	0.11	0.12	0.12	0.05

Note: U = baseline unmarried sample, S = baseline unmarried and safe sample, and A = baseline unmarried, safe, and attractive sample. T = treatment arm and C = control arm. All means are computed over non-attriters in a particular wave. We do not observe the share of marriages or pregnancies that begin or end in Wave 1 because these variables rely on the difference between Wave $t-1$ and Wave t.

Table A6: Means by Wave and Intervention Arm for the **Single-Test Intervention**

	Sample	Arm	Wave 4	5	6	7	8
Respondents tested in previous 4 months (%)	U	T	0.16	0.66	0.26	0.30	0.27
		C	0.22	0.27	0.30	0.30	0.21
	S	T	0.13	0.63	0.24	0.25	0.26
		C	0.19	0.24	0.45	0.29	0.23
	A	T	0.15	0.70	0.42	0.35	0.23
		C	0.18	0.22	0.47	0.30	0.20
Partners tested in previous 4 months (%)	U	T	0.28	0.39	0.33	0.30	0.39
		C	0.29	0.28	0.30	0.23	0.26
	S	T	0.30	0.45	0.37	0.32	0.39
		C	0.34	0.28	0.29	0.17	0.28
	A	T	0.33	0.50	0.38	0.33	0.40
		C	0.31	0.28	0.28	0.17	0.21
Own subjective HIV risk (prob. [0,1])	U	T	0.19	0.18	0.20	0.19	0.22
		C	0.14	0.13	0.13	0.14	0.21
	S	T	0.14	0.14	0.15	0.14	0.20
		C	0.13	0.11	0.10	0.13	0.19
	A	T	0.16	0.13	0.16	0.15	0.20
		C	0.12	0.11	0.12	0.13	0.20
Subjective HIV risk of partners (Likert 1-5)	U	T	1.6	1.4	1.6	1.7	1.6
		C	1.5	1.4	1.4	1.3	1.4
	S	T	1.6	1.3	1.5	1.6	1.4
		C	1.4	1.4	1.3	1.3	1.4
	A	T	1.7	1.4	1.5	1.7	1.5
		C	1.4	1.4	1.3	1.3	1.4
Respondents starting new relationships (%)	U	T	0.17	0.22	0.18	0.16	0.13
		C	0.12	0.21	0.16	0.08	0.17
	S	T	0.11	0.20	0.17	0.15	0.14
		C	0.10	0.22	0.15	0.07	0.16
	A	T	0.10	0.23	0.17	0.13	0.11
		C	0.13	0.19	0.13	0.07	0.21
Respondents endling prior relationships (%)	U	T	0.12	0.13	0.13	0.11	0.13
		C	0.12	0.10	0.11	0.11	0.08
	S	T	0.08	0.11	0.13	0.10	0.12
		C	0.12	0.09	0.10	0.11	0.08
	A	T	0.10	0.10	0.15	0.08	0.09
		C	0.11	0.09	0.12	0.11	0.05
Respondents in a relationship (%)	U	T	0.44	0.52	0.56	0.62	0.63
		C	0.42	0.51	0.55	0.52	0.61
	S	T	0.39	0.47	0.51	0.56	0.60
		C	0.39	0.51	0.54	0.52	0.59
	A	T	0.42	0.56	0.56	0.60	0.66
		C	0.45	0.55	0.54	0.51	0.67

Note: Wave 4 is the "baseline" for the single-test intervention. U = Wave-4 unmarried sample, S = Wave-4 unmarried and safe sample, and A = Wave-4 unmarried, safe, and attractive sample. T = treatment arm and C = control arm. All means are computed over non-attriters in a particular wave. "Partners tested" and "subjective HIV risk of partners" are calculated for women who have partners in Wave *t*.

Table A7: Means by Wave and Intervention Arm for the **Single-Test Intervention**

	Sample	Arm	Wave 4	5	6	7	8
Respondents starting new marriages (%)	U	T	0.00	0.07	0.07	0.05	0.08
		C	0.00	0.06	0.07	0.09	0.03
	S	T	0.00	0.06	0.05	0.05	0.07
		C	0.00	0.06	0.06	0.08	0.03
	A	T	0.00	0.06	0.03	0.05	0.09
		C	0.00	0.05	0.06	0.08	0.04
Respondents ending prior marriages (%)	U	T	0.00	0.00	0.01	0.01	0.02
		C	0.01	0.00	0.01	0.01	0.01
	S	T	0.00	0.00	0.01	0.01	0.02
		C	0.01	0.00	0.01	0.01	0.01
	A	T	0.00	0.00	0.01	0.00	0.00
		C	0.00	0.00	0.00	0.02	0.00
Respondents who are married (%)	U	T	0.00	0.07	0.12	0.15	0.21
		C	0.00	0.06	0.12	0.22	0.25
	S	T	0.00	0.06	0.09	0.13	0.18
		C	0.00	0.06	0.11	0.21	0.22
	A	T	0.00	0.06	0.06	0.11	0.22
		C	0.00	0.05	0.10	0.19	0.23
Respondents starting new pregnancies (%)	U	T	0.02	0.06	0.06	0.05	0.09
		C	0.05	0.08	0.09	0.03	0.04
	S	T	0.03	0.04	0.04	0.04	0.09
		C	0.04	0.07	0.07	0.03	0.04
	A	T	0.02	0.04	0.06	0.04	0.14
		C	0.02	0.10	0.06	0.06	0.01
Respondents finishing prior pregnancies (%)	U	T	0.00	0.04	0.02	0.06	0.07
		C	0.03	0.01	0.06	0.10	0.06
	S	T	0.01	0.04	0.01	0.04	0.07
		C	0.02	0.02	0.07	0.07	0.06
	A	T	0.00	0.06	0.00	0.05	0.04
		C	0.02	0.02	0.04	0.10	0.06
Respondents who are pregnant (%)	U	T	0.04	0.07	0.12	0.11	0.14
		C	0.06	0.13	0.15	0.11	0.07
	S	T	0.04	0.05	0.08	0.07	0.13
		C	0.05	0.11	0.11	0.09	0.08
	A	T	0.05	0.04	0.10	0.08	0.16
		C	0.04	0.12	0.14	0.13	0.06

Note: Wave 4 is the "baseline" for the single-test intervention. U = Wave-4 unmarried sample, S = Wave-4 unmarried and safe sample, and A = Wave-4 unmarried, safe, and attractive sample. T = treatment arm and C = control arm. All means are computed over non-attriters in a particular wave.

Table A8: Baseline Characteristics by Attrition Status for All Respondents

	Non-Attriters	Attriters	P-value
	(1)	(2)	(3)
Demographics			
Age	19.9	19.6	0.19
Attractiveness	3.50	3.71	0.00***
Ngoni Tribe	0.40	0.33	0.03**
Yao Tribe	0.26	0.23	0.28
Lomwe Tribe	0.17	0.19	0.38
Catholic	0.34	0.31	0.44
Protestant	0.47	0.52	0.16
Muslim	0.19	0.17	0.37
Socioeconomic Status			
Enrolled in school	0.39	0.49	0.00***
Employed full-time	0.21	0.14	0.02**
Any savings	0.14	0.16	0.42
Household asset index	-0.16	0.47	0.00***
HIV			
HIV positive (treatment group only)	0.11	0.09	0.58
Risky behavior index (0-4)	0.59	0.47	0.04**
Thinks about future	3.11	3.32	0.00***
Worried about HIV	1.04	1.03	0.95
Subjective 5-year mort. risk (percent)	0.34	0.33	0.83
Ever tested for HIV (parity=0)	0.34	0.36	0.74
Ever tested for HIV (parity>0)	0.87	0.91	0.23
Outcomes			
In a Relationship	0.70	0.69	0.70
Married	0.49	0.34	0.00***
Pregnant	0.15	0.10	0.02**
Subjective HIV risk (percent)	0.11	0.10	0.20
Subjective partner HIV likelihood (1-5)	1.61	1.63	0.85
Observations	720	287	-

Note: all means are weighted for age balance across intervention arms. Non-attriters have completed all eight survey waves while attriters have completed fewer than eight waves. To compute p-values, we regress each variable on treatment in Wave 1 and cluster standard errors by respondent. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Estimates for Non-Attriters

	Currently Married	Currently Pregnant
	(1)	(2)
A: Overall Estimates		
Treatment	0.033*	0.023**
	(0.019)	(0.011)
	[0.57]	[0.13]
Observations	5037	5037
B: Baseline-Unmarried Estimates by Safety (Defined by Own Risky Behavior)		
Treatment · Safe	0.078**	0.043***
	(0.034)	(0.016)
	[0.15]	[0.10]
Treatment · Unsafe	-0.055	0.017
	(0.096)	(0.042)
	[0.41]	[0.14]
Equality of coefficients (p-value)	0.19	0.57
Observations	2749	2749
C: Baseline-Unmarried and Safe (Defined by Own Risky Behavior) Estimates by Attractiveness		
Treatment · Attractive	0.104**	0.047**
	(0.047)	(0.023)
	[0.12]	[0.08]
Treatment· Not Attractive	0.041	0.029
	(0.050)	(0.025)
	[0.18]	[0.12]
Equality of coefficients (p-value)	0.36	0.57
Observations	2287	2287

Note: standard errors are clustered by respondent and appear in parentheses. Subgroup-specific control group means appear in brackets. All estimates cover Waves 5-8 and control for wave dummies and the baseline dependent variable. Regressions reweight to balance by age. Panel A uses the specification in Equation (9). Panel B is limited to baseline-unmarried respondents and uses the same specification as Panel A of Table 3. Panel C is limited to baseline-unmarried respondents who are safe (defined by the absence of baseline risky behavior) and uses the same specification as Panel A of Table 5. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Baseline Characteristics for Unmarried Respondents, by Safety

Safety Definition:	Own Risky Behavior			Own Perceived HIV Risk		
	Safe	Unsafe	P-value	Safe	Unsafe	P-value
	(1)	(2)	(3)	(4)	(5)	(6)
Demographics						
Age	18.1	20.8	0.00***	18.4	19.3	0.01**
Attractiveness	3.67	3.29	0.00***	3.61	3.53	0.33
Ngoni Tribe	0.36	0.34	0.72	0.38	0.28	0.03**
Yao Tribe	0.25	0.25	0.97	0.25	0.25	0.89
Lomwe Tribe	0.18	0.17	0.86	0.17	0.25	0.08*
Catholic	0.37	0.49	0.04**	0.38	0.42	0.49
Protestant	0.47	0.38	0.14	0.47	0.39	0.16
Muslim	0.16	0.13	0.33	0.15	0.19	0.37
Socioeconomic Status						
Enrolled in school	0.72	0.32	0.00***	0.69	0.50	0.00***
Employed full-time	0.06	0.23	0.00***	0.07	0.17	0.02**
Any savings	0.10	0.24	0.01***	0.12	0.14	0.62
Household asset index	0.33	0.01	0.01**	0.32	0.09	0.06*
HIV						
HIV positive (treatment group only)	0.07	0.23	0.01**	0.07	0.20	0.03**
Risky behavior index (0-4)	0.00	1.26	0.00***	0.15	0.49	0.00***
Thinks about future	3.33	3.18	0.15	3.33	3.23	0.26
Worried about HIV	1.52	1.73	0.02**	1.46	1.96	0.00***
Subjective 5-year mort. risk	0.31	0.36	0.27	0.29	0.46	0.00***
Ever tested for HIV (parity = 0)	0.28	0.53	0.00***	0.29	0.38	0.11
Ever tested for HIV (parity > 0)	0.85	0.92	0.31	0.93	0.78	0.02**
Outcomes						
In a Relationship	0.42	0.63	0.00***	0.43	0.57	0.01**
Married	0.00	0.00	-	0.00	0.00	-
Pregnant	0.04	0.10	0.08*	0.04	0.08	0.21
Subjective HIV risk (percent)	0.07	0.20	0.00***	0.02	0.41	0.00***
Subjective partner HIV likelihood (1-5)	1.46	1.86	0.02**	1.39	2.15	0.00***
Observations	498	91	-	474	115	-

Note: to compute p-values, we regress each variable on safety in Wave 1 and cluster standard errors by respondent. In Columns 1-3, respondents with no risky behaviors at baseline are classified as "safe". In Columns 4-6, respondents who perceive that their HIV risk is ≤ 0.10 are classified as "safe". * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Baseline Characteristics for Safe and Unmarried Respondents, by Attractiveness

Safety Definition:	Own Risky Behavior			Own Perceived HIV Risk		
	Attractive	Unattractive	P-value	Attractive	Unattractive	P-value
	(1)	(2)	(3)	(4)	(5)	(6)
Demographics						
Age	18.3	17.9	0.26	18.4	18.3	0.60
Attractiveness	4.14	2.97	0.00***	4.14	2.96	0.00***
Ngoni Tribe	0.37	0.35	0.67	0.39	0.37	0.67
Yao Tribe	0.23	0.27	0.23	0.24	0.26	0.64
Lomwe Tribe	0.18	0.19	0.69	0.16	0.17	0.82
Catholic	0.35	0.39	0.35	0.36	0.40	0.39
Protestant	0.49	0.43	0.18	0.49	0.44	0.34
Muslim	0.16	0.18	0.56	0.15	0.15	0.87
Socioeconomic Status						
Enrolled in school	0.74	0.68	0.19	0.74	0.63	0.02**
Employed full-time	0.03	0.10	0.02**	0.04	0.10	0.04**
Any savings	0.11	0.10	0.89	0.11	0.14	0.46
Household asset index	0.56	-0.02	0.00***	0.57	0.00	0.00***
HIV						
HIV positive (treatment group only)	0.08	0.06	0.46	0.08	0.06	0.63
Risky behavior index (0-4)	0.00	0.00	-	0.10	0.22	0.00***
Thinks about future	3.55	3.01	0.00***	3.56	3.05	0.00***
Worried about HIV	1.55	1.48	0.31	1.48	1.43	0.38
Subjective 5-year mort. risk	0.33	0.29	0.16	0.30	0.27	0.26
Ever tested for HIV (parity = 0)	0.30	0.25	0.22	0.30	0.28	0.58
Ever tested for HIV (parity > 0)	0.84	0.87	0.73	0.93	0.92	0.97
Outcomes						
In a Relationship	0.46	0.36	0.03**	0.46	0.39	0.15
Married	0.00	0.00	-	0.00	0.00	-
Pregnant	0.05	0.03	0.15	0.04	0.04	0.92
Subjective HIV risk (percent)	0.07	0.08	0.88	0.02	0.02	0.41
Subjective partner HIV likelihood (1-5)	1.45	1.47	0.87	1.36	1.44	0.35
Observations	289	207	-	260	214	-

Note: to compute p-values, we regress each variable on safety in Wave 1 and cluster standard errors by respondent. Columns 1-3 limit the sample to respondents with no risky behaviors at baseline. Columns 4-6 limit the sample to respondents who perceive that their HIV risk is ≤ 0.10 at baseline. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: A Reproduction of Table 4 for Baseline-Unmarried and Unsafe Respondents

	Currently Married		Currently Pregnant	
	(1)	(2)	(3)	(4)
A: Safety Defined by Own Risky Behavior				
Treatment · Attractive	0.0053	-0.47***	-0.036	0.0013
	(0.093)	(0.13)	(0.067)	(0.064)
	[0.19]	[0.19]	[0.14]	[0.14]
Treatment · Not Attractive	-0.093	0.23***	0.015	0.034
	(0.11)	(0.086)	(0.048)	(0.038)
	[0.49]	[0.49]	[0.14]	[0.14]
Equality of coefficients (p-value)	0.49	0.00	0.53	0.71
Significance of covariates (p-value)	-	0.00	-	0.00
Observations	546	546	546	546
B: Safety Defined by Own Perceived HIV Risk				
Treatment · Attractive	0.032	0.060	-0.029	0.035
	(0.093)	(0.13)	(0.045)	(0.047)
	[0.18]	[0.18]	[0.13]	[0.13]
Treatment · Not Attractive	-0.068	-0.096	-0.023	-0.060
	(0.11)	(0.092)	(0.054)	(0.041)
	[0.40]	[0.40]	[0.20]	[0.20]
Equality of coefficients (p-value)	0.49	0.37	0.93	0.17
Significance of covariates (p-value)	-	0.00	-	0.00
Observations	674	674	674	674
Treatment · covariates	No	Yes	No	Yes

Note: estimates are based on the specification $Y_{it} = \beta_1[T_i \cdot A_i] + \beta_2[T_i \cdot (1 - A_i)] + \beta_3 A_i + \beta_4(1 - A_i) + \beta_5 Y_i^b + \delta_t + \varepsilon_{it}$. In this expression, Y is the dependent variable, Y^b is the baseline dependent variable, δ is a set of wave dummies, T is a treatment indicator, and A_i is an attractive indicator. The table reports $\hat{\beta}_1$ and $\hat{\beta}_2$. Standard errors are clustered by respondent and appear in parentheses. Subgroup-specific control group means appear in brackets. Panel A includes respondents with one or more baseline HIV risk factors and Panel B includes respondents with baseline subjective HIV risk > 0.1, as the text explains. All regressions reweight to balance by age. Even columns also control for the interaction between treatment and demographics (tribe, religion, and age), SES (employment, completed schooling, school enrollment, durable roof, durable floor, electricity, telephone ownership, and television ownership), and time preferences (future orientation and subjective mortality risk within 1, 5, and 10 years). Covariates are demeaned in order to preserve the interpretation of the coefficients of interest. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: The Impact on Bargaining Power

	Partner in Charge	Domestic Violence	Forced Sex
	(1)	(2)	(3)
A: Overall Estimates			
Treatment	-0.011	-0.0070	-0.019
	(0.025)	(0.014)	(0.028)
	[0.83]	[0.05]	[0.22]
Observations	1122	1122	1122
B: Baseline-Unmarried Estimates by Safety (Defined by Own Risky Behavior)			
Treatment · Safe	0.022	0.020	-0.020
	(0.060)	(0.016)	(0.060)
	[0.71]	[0.00]	[0.25]
Treatment · Unsafe	0.087	-0.037	-0.046
	(0.086)	(0.082)	(0.094)
	[0.80]	[0.08]	[0.22]
Equality of coefficients (p-value)	0.54	0.50	0.82
Observations	395	395	395
C: Baseline-Unmarried and Safe (Defined by Own Risky Behavior) Estimates by Attractiveness			
Treatment · Attractive	-0.0070	0.0025	-0.082
	(0.081)	(0.013)	(0.078)
	[0.68]	[0.01]	[0.29]
Treatment · Not Attractive	0.051	0.045	0.080
	(0.082)	(0.033)	(0.088)
	[0.78]	[0.00]	[0.19]
Equality of coefficients (p-value)	0.62	0.22	0.17
Observations	308	308	308

Note: standard errors are clustered by respondent and appear in parentheses. Subgroup-specific control group means appear in brackets. Regressions reweight to balance by age. Panel A uses the specification in Equation (9). Panel B is limited to baseline-unmarried respondents and uses the same specification as Panel A of Table 3. Panel C is limited to baseline-unmarried respondents who are safe (defined by the absence of baseline risky behavior) and uses the same specification as Panel A of Table 5. Estimates use data from Waves 3 and 5, for which data are available. Estimates do not control for the baseline dependent variable because these variables are not measured at baseline. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Wave-4 Characteristics by Single-Test Treatment Status for Wave-4 Unmarried Respondents

	Treatment	T−C (Unweighted)		T−C (Weighted)	
	Mean	Difference	SE	Difference	SE
	(1)	(2)	(3)	(4)	(5)
Demographics					
Age	19.6	0.35*	0.21	0.00	0.20
Attractiveness	3.50	-0.10**	0.04	-0.09**	0.04
Ngoni Tribe	0.39	0.02	0.03	0.02	0.03
Yao Tribe	0.30	0.03	0.03	0.04	0.03
Lomwe Tribe	0.15	-0.01	0.02	-0.01	0.02
Catholic	0.31	-0.02	0.03	-0.02	0.03
Protestant	0.46	-0.02	0.03	-0.03	0.03
Muslim	0.24	0.05	0.03	0.05	0.03
Socioeconomic Status					
Enrolled in school	0.35	-0.12***	0.03	-0.08***	0.03
Employed full-time	0.19	0.03	0.03	0.02	0.03
Any savings	0.21	0.02	0.03	0.01	0.03
Household asset index	0.01	-0.05	0.07	-0.05	0.07
HIV					
HIV positive	0.13	0.06***	0.02	0.05***	0.02
HIV risk index (0-4)	0.59	0.06	0.05	0.02	0.05
Thinks about future	3.32	-0.12**	0.05	-0.12**	0.05
Worried about HIV	1.50	0.08*	0.05	0.07	0.05
Subjective 5-year mort. risk (percent)	0.49	0.03	0.02	0.02	0.02
Ever tested for HIV (parity=0)	0.41	0.10**	0.04	0.08*	0.04
Ever tested for HIV (parity>0)	0.80	0.04	0.03	0.03	0.03
Outcomes					
In a Relationship	0.72	0.03	0.03	0.01	0.03
Married	0.50	0.04	0.03	0.00	0.03
Pregnant	0.12	-0.02	0.02	-0.02	0.02
Subjective HIV risk (percent)	0.20	0.04**	0.02	0.03**	0.02
Subjective partner HIV likelihood (1-5)	1.65	0.07	0.08	0.06	0.08
Observations	498	507	-	507	-

Note: the household asset index is the standardized sum of indicators that the household has a durable roof, a durable floor, electricity, a television, a telephone, and an improved toilet. Columns 2 and 3 show unweighted comparisons and Columns 4 and 5 show comparisons that are weighted to balance by age. To compute p-values, we regress each variable on treatment in Wave 4 and cluster standard errors by respondent. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Baseline Characteristics by Treatment Status for All Respondents

	Treatment	T−C (Unweighted)		T−C (Weighted)	
	Mean	Difference	SE	Difference	SE
	(1)	(2)	(3)	(4)	(5)
Demographics					
Age	19.8	0.57***	0.20	0	0.99
Attractiveness	3.54	-0.06	0.04	-0.05	0.04
Ngoni Tribe	0.38	0.01	0.03	0.00	0.03
Yao Tribe	0.25	-0.01	0.03	-0.01	0.03
Lomwe Tribe	0.19	0.04	0.02	0.03	0.02
Catholic	0.33	0.004	0.03	0.01	0.03
Protestant	0.49	0.01	0.03	-0.004	0.03
Muslim	0.18	-0.01	0.02	-0.01	0.02
Socioeconomic Status					
Enrolled in school	0.36	-0.12***	0.03	-0.04	0.03
Employed full-time	0.18	0.003	0.02	-0.02	0.03
Any savings	0.17	0.04	0.02	0.04	0.02
Household asset index	-0.03	-0.10	0.07	-0.09	0.07
HIV					
HIV positive	0.10	-	-	-	-
HIV risk index (0-4)	0.44	0.05	0.03	-0.004	0.03
Thinks about future	3.13	-0.06	0.06	0.07	0.06
Worried about HIV	1.61	0.07	0.05	0.04	0.05
Subj. 5-year mort. risk (%)	0.34	0.01	0.02	0.01	0.02
Ever tested for HIV (parity=0)	0.35	0.05	0.04	0.00	0.04
Ever tested for HIV (parity>0)	0.87	-0.02	0.03	-0.02	0.03
Subj. HIV risk (%)	0.12	0.02*	0.01	0.02	0.01
Subj. partner HIV likelihood (1-5)	1.6	0.05	0.06	0.05	0.07
Outcomes					
In a Relationship	0.70	-0.06	0.03**	-0.01	0.03
Married	0.43	-0.03	0.03	-0.04	0.03
Pregnant	0.15	0.04**	0.02	0.03	0.02
Joint signif. of covariates (p-value)	-	0.01	-	0.11	-
Observations	500	507	-	507	-

Note: the household asset index is the standardized sum of indicators that the household has a durable roof, a durable floor, electricity, a television, a telephone, and an improved toilet. Columns 2 and 3 show unweighted comparisons and Columns 4 and 5 show comparisons that are weighted to balance by age. By construction, HIV test results are only available for the treatment group at baseline. To compute p-values, we regress each variable on treatment in Wave 1 and cluster standard errors by respondent. The joint significance of covariates in based on a regression of treatment on all covariates in the table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

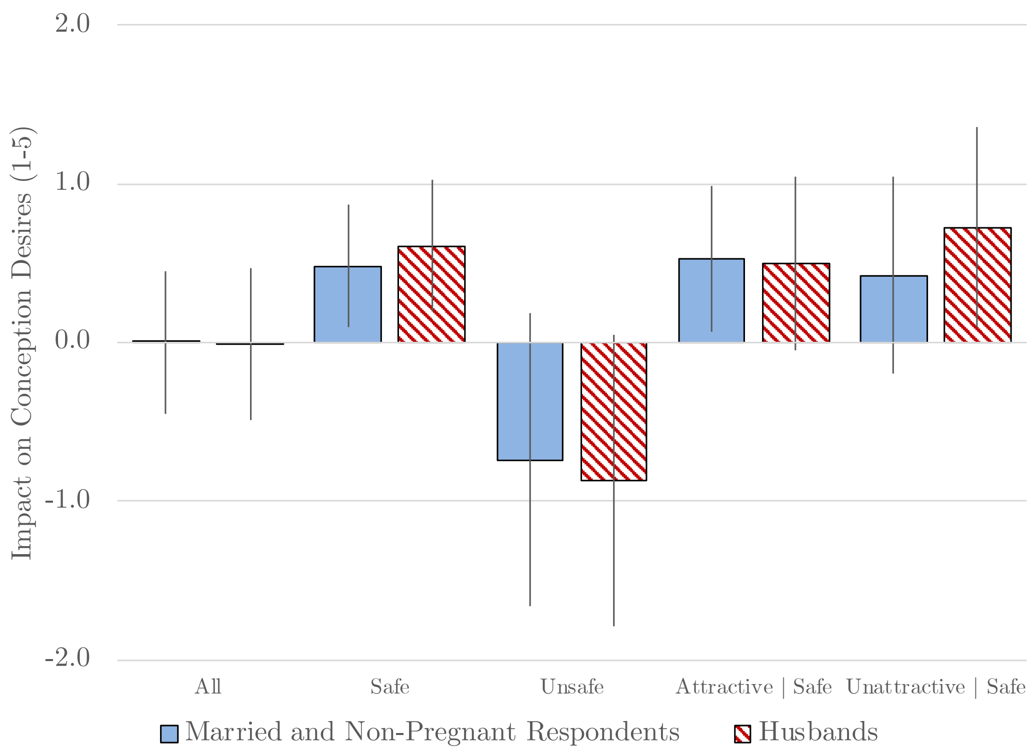


Figure A1: Differences by Treatment Arm in the Desire to Conceive for Married and Non-Pregnant Participants and Their Husbands

Note: the figure shows estimates of the impact of the multiple-testing intervention on conception desires following Equation (9) for married and non-pregnant respondents (blue) and their husbands (red stripes). Conception desires are measured on a 1-5 Likert scale. Conception desires of husbands are reported by respondents. Error bars show 90% confidence intervals based on respondent-clustered standard errors.

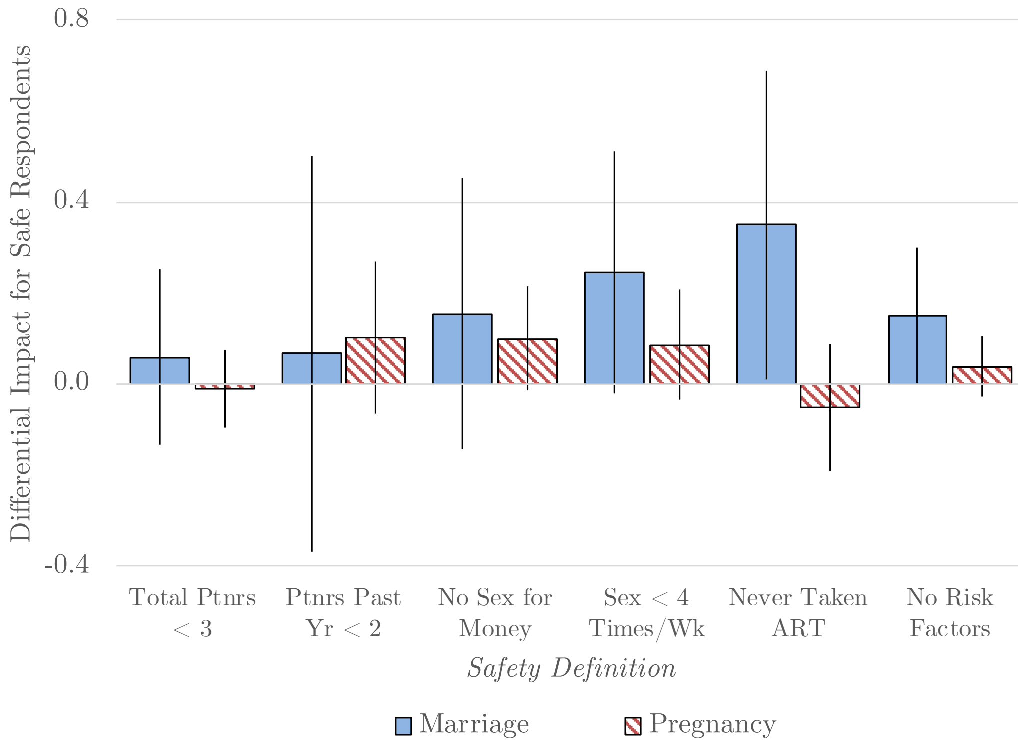


Figure A2: Difference by Safety in the Impact on Marriage and Pregnancy According to Five Components of Risky Behaviors (90% Confidence Intervals)

Note: the figure shows the differential impacts on marriage and pregnancy for safe respondents compared to unsafe respondents. We provide several alternative safety metrics, which are aspects of our primary safety definition in Table 4 (Panel A), as Appendix E describes. The final set of estimates, labeled "no risk factors", replicates our main safety definition. Error bars provide 90% confidence intervals based on respondent-clustered standard errors.

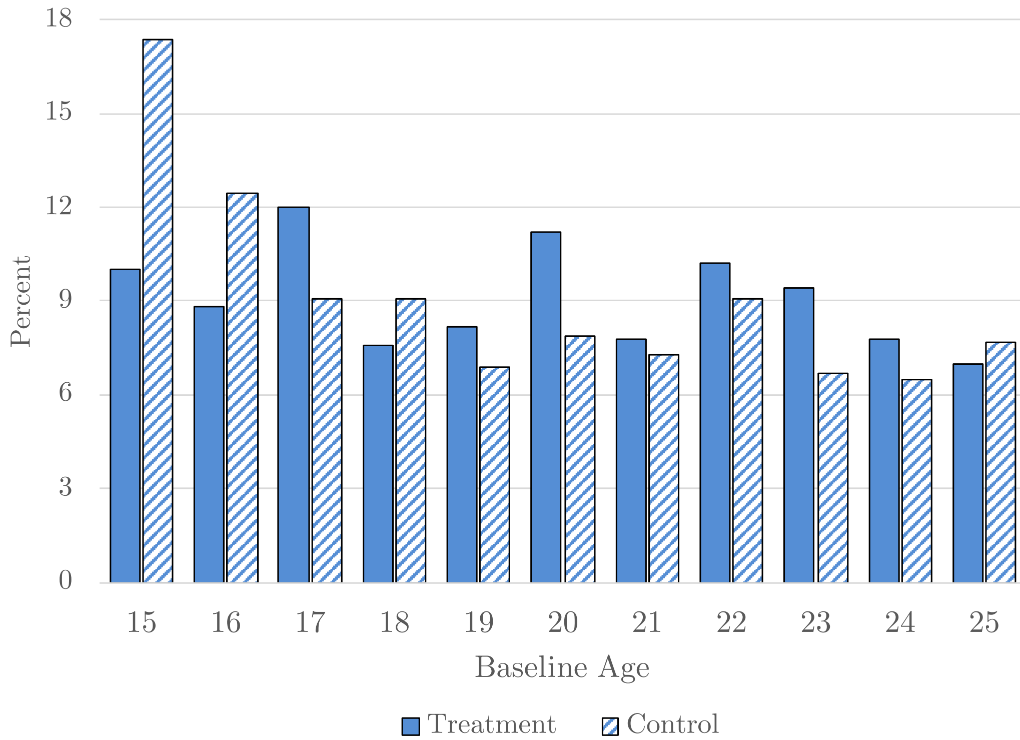


Figure A3: Age Distributions for the Treatment and Control Groups

Note: the figure shows the percent of participants of each age in the multiple-testing arm (solid) and control arm (stripes) in Wave 1.

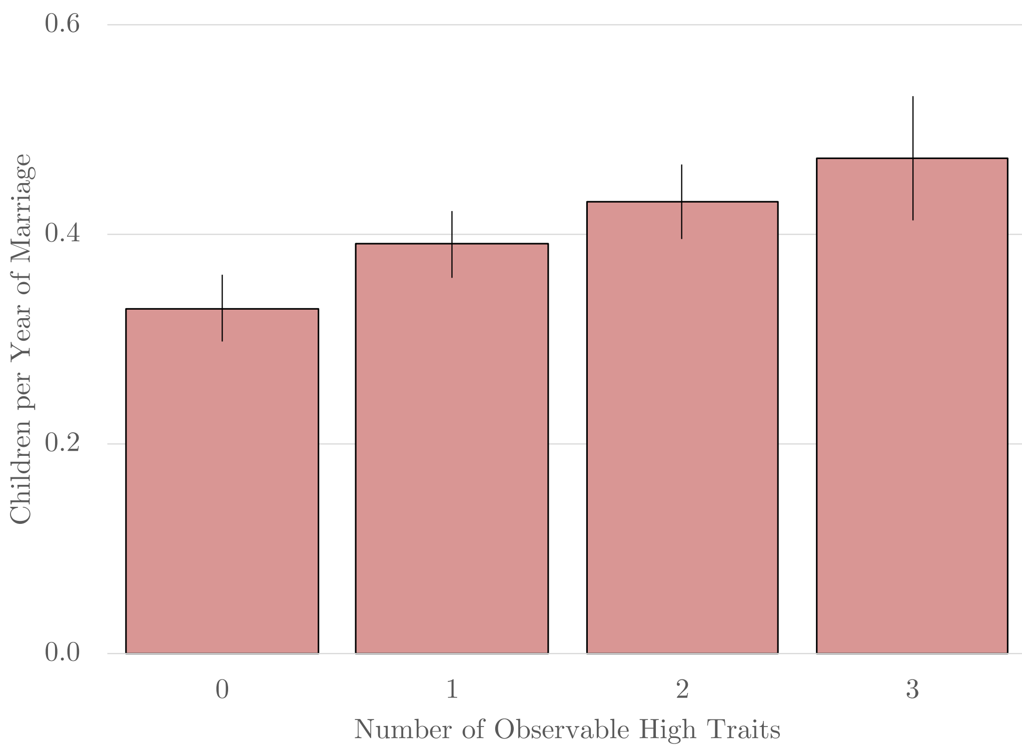
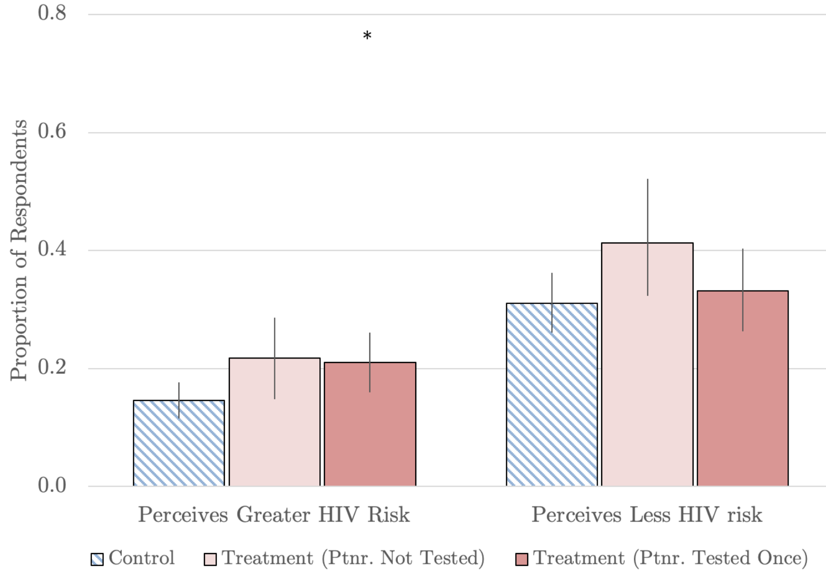
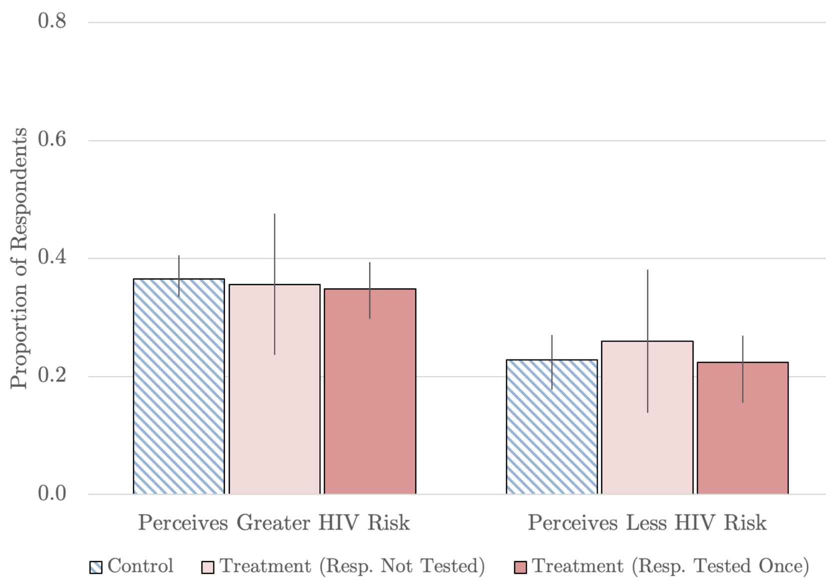


Figure A4: The Number of Observable High Traits and Fertility for Baseline-Married Couples

Note: the figure shows the average number of children per year of marriage for baseline-married respondents and their husbands. Couples are grouped by the number of observable "high traits", which include wife's attractiveness, wife's sexual safety, and husband's education. Since we do not observe the sexual safety of husbands, couples have a maximum of three observable high traits. The figure shows 90% confidence intervals based on OLS regressions.



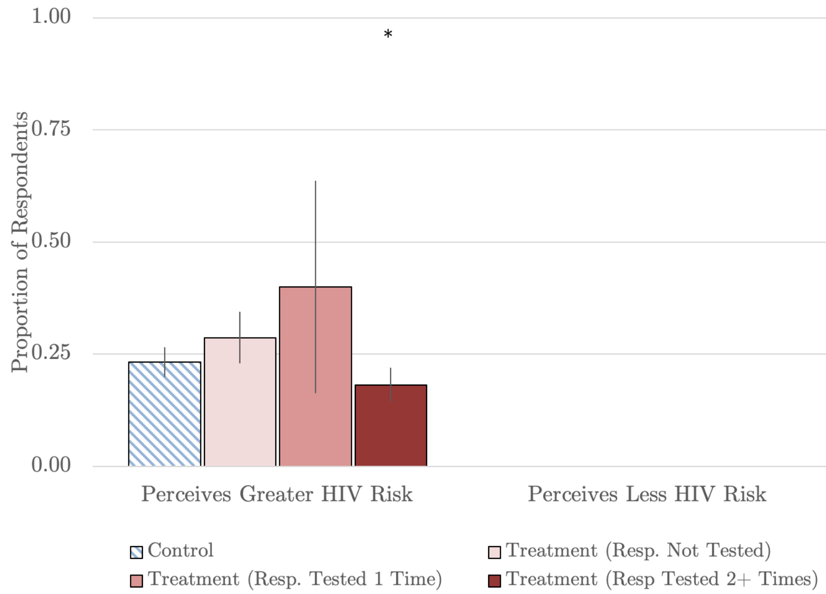
(a) Belief that Partner is HIV-Negative



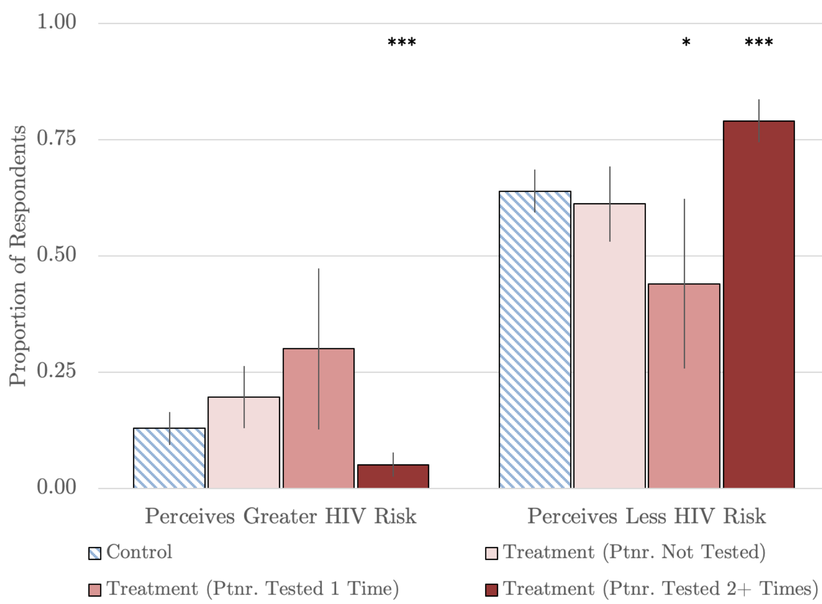
(b) Belief that Respondent is HIV-Negative

Figure A5: Belief Updating about Own and Partner's HIV Status in the **Single-Test Intervention**

Note: the figure shows the proportion of respondents who perceived greater HIV risk (on the left) and less HIV risk (on the right) for their partners (Panel A) and themselves (Panel B). Respondents perceived greater risk if their risk assessment was lower in Wave 4 than in Waves 5-8. Respondents perceived less risk if their risk assessment was higher in Wave 4 than in Waves 5-8. We omit the proportion with no change in perceptions for clarity. The figure groups respondents by intervention arm and partner or own testing frequency. Partner risk perceptions are measured on a 1-5 Likert scale. Own risk perceptions are measured on probability scale. Panel A limits the sample to respondents who had a partner in Wave 4 and the follow-up wave being measured. Error bars show 90% confidence intervals and stars indicate statistically significant differences from the control group according to OLS regressions with respondent-clustered standard errors. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.



A: Respondents Who Are Certain that Partners are HIV-Negative



B: Respondents Who Are Not Certain That Partners are HIV-Negative

Figure A6: Belief Updating About Partner HIV-Negative Status

Note: the figure shows the proportion of respondents who perceived greater HIV risk (on the left) and less HIV risk (on the right) for their partners. Respondents perceived greater risk if their risk assessment was lower in Wave 1 than in Waves 2-8. Respondents perceived less risk if their risk assessment was higher in Wave 1 than in Waves 2-8. We omit the proportion with no change in perceptions for clarity. The figure groups respondents by intervention arm and partner testing frequency. Partner risk perceptions are measured on a 1-5 Likert scale. The figure limits the sample to respondents who had a partner in Wave 1 and the follow-up wave being measured. Panel A focuses on respondents who perceived zero partner HIV risk in Wave 1 and Panel B focuses on respondents who perceived non-zero partner HIV risk in Wave 1. Error bars show 90% confidence intervals and stars indicate statistically significant differences from the control group according to OLS regressions with respondent-clustered standard errors. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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