

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

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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% 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.

Key words:

JEL Codes: J12, J13, I15, I18

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 article 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

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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% 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 behaviour 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 signalling 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% 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% 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 because it may send an unfavourable signal that the individual is concerned about his or her HIV status (Chesney and Smith, 1999;

1. We use marriage timing data from the 1992, 1999, 2004, and 2010 rounds of the Malawi Demographic and Health Survey (National Statistical Office 1992, 2000, 2004, 2010). 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.

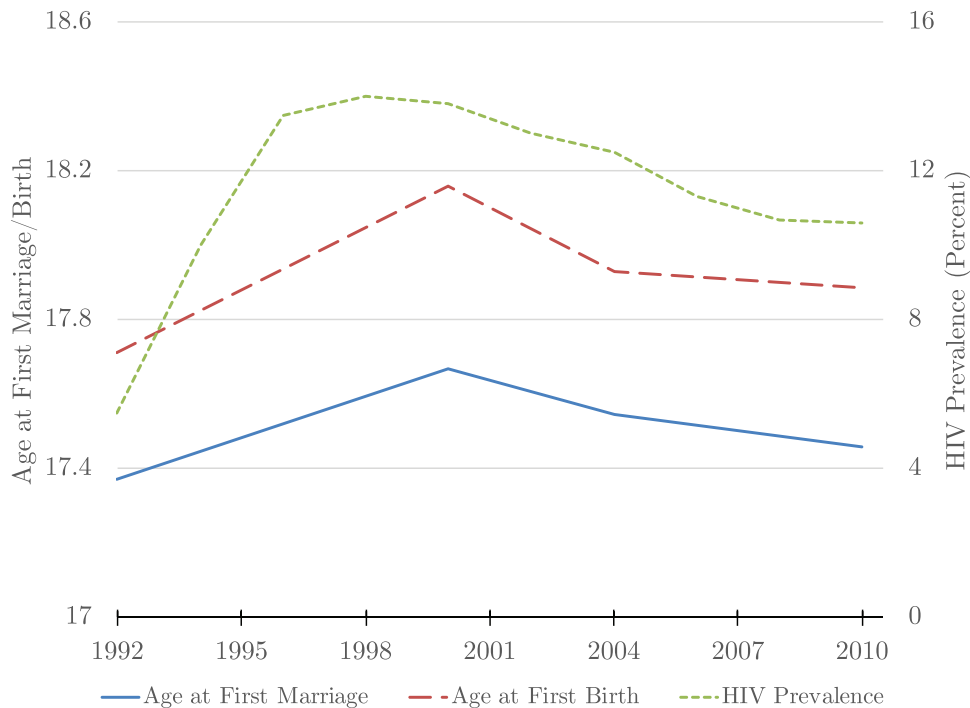


FIGURE 1

Marriage and first birth for women aged 17–27

Notes: 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.

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 unfavourable 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 1,505 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% and pregnancy by 27% among baseline-unmarried respondents. These impacts are 30–100% 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% and increased pregnancy by 64%. 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 behaviour ([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 signalling.

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

We study marriage behaviour using exogenous variation in a signalling 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 signalling 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

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 behavioural response to HIV risk.

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 $S_{cd}^{ab} > 0$. 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.

$$S_{hh}^{ab} > S_{hl}^{ab} = S_{lh}^{ab} > S_{ll}^{ab} > 0. \quad (1)$$

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.

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 $S_{ab}^{ab} \cdot (1 + \delta_i)$.

5. Supplementary Appendix A considers the implications of relaxing these assumptions.

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

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

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

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

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.

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

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

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.

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, $r^{ah} \equiv \frac{S_{ah}^{ah}}{S_{al}^{ah}} > 1$ 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.

$$\bar{s}^{al} = \left(\frac{p_{al}S_{al}^{al} + \mu_{ah}^*S_{ah}^{al}}{p_{al} + \mu_{ah}^*} \right) (1 + \bar{\delta}). \tag{6}$$

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.

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

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.

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

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_{al}^{ah} 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.

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 $S_{ah}^{ah} - \left(\frac{\mu_{ah}^*}{p_{ah}} \right) \frac{(\mu_{ah}^* S_{ah}^{ah} + p_{al} S_{al}^{ah})}{(p_{al} + \mu_{ah}^*)} \geq 0$. Conversely, the intervention decreases average marital surplus by $\frac{(\mu_{ah}^* S_{ah}^{al} + p_{al} S_{al}^{al})}{p_{al} + \mu_{ah}^*} - S_{al}^{al} \geq 0$ for unsafe people by worsening the quality of their partners. Under assumptions that we discuss in [Supplementary 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 behaviours 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

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

infect their spouses, particularly given the uncommon use of condoms within marriage (Chimbiri, 2007; Tavorly 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% 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 behaviours and current HIV status are difficult to observe. People often conceal promiscuous behaviour 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 unfavourable marriage market signal by implying that the test seeker has engaged in risky sexual behaviour: 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 seeker in rural Malawi must travel several kilometres on foot or bicycle over unimproved roads and queue in public at the health centre 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 h 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% of childless women have been tested in the past four months and only 35% 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 counselling (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 88 percent of mothers in our sample indicate that they were tested.

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 counselling, which are observable.

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. Supplementary 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.

4. SURVEY AND INTERVENTION

We evaluate a high-frequency HIV testing intervention that was embedded in the TLT Panel Study (Trinitapoli and Yeatman 2018a,b). 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, 2006).¹¹ The TLT Panel Study followed a representative sample of women aged 15–25 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).

Surveyors offered rapid HIV tests, which provide results within 30 minutes and have sensitivity and specificity of over 99% (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 behaviour 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 80% of the time and reported sharing these results with their partners 98% of the time. Participants in the treatment arm constitute 1.5% 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. Forty-two percent of treatment partners participated and 49% of control partners participated in the study ($p = 0.02$), for an average participation rate of 45%.¹³ Self-reported data are available only for the endogenous subsample of partners who 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% for respondents and from 30 to 55% for their partners ($p < 0.001$ in both cases). This pattern suggests that the intervention substantially reduced the personal cost of HIV testing.

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).

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.

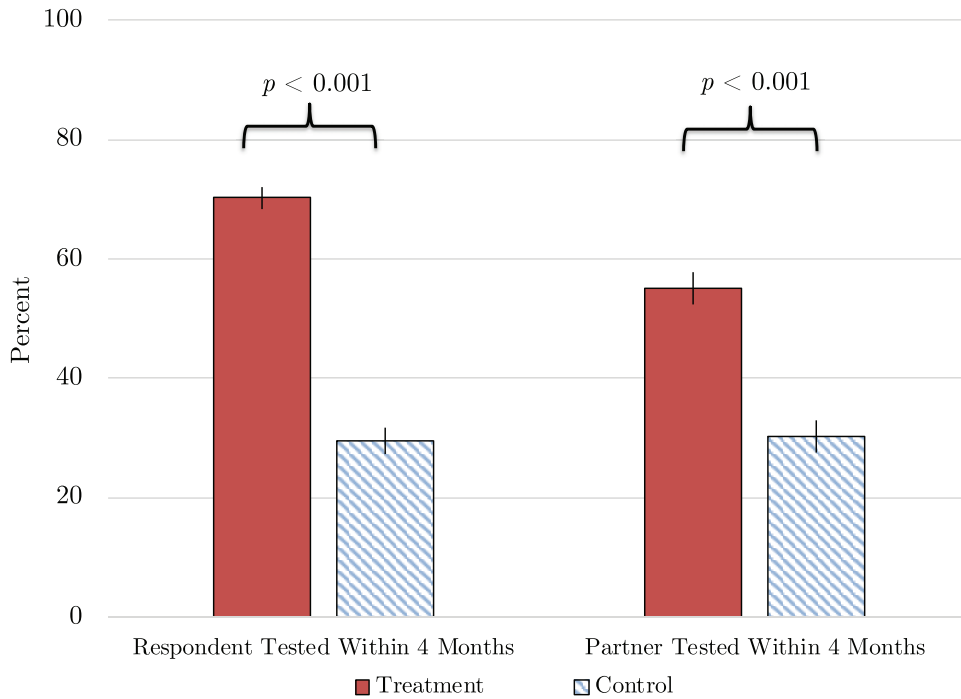


FIGURE 2

Probability of testing within four months by treatment arm

Notes: 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.

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). Forty-two percent of respondents were married at baseline (43 and 40% in treatment and control) and 60% were married at Wave 8.¹⁴ Urine-based pregnancy tests measured pregnancy in each period. Respondents completed the tests over 95% 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, 1973, 1981; Weiss and Willis, 1985). Consistent with this view, 71% of pregnancies in our sample occur within marriage, only 24% of marriages involve women who are already pregnant, and 99% of

14. Divorce was a possible ramification of the intervention (Schatz, 2005). Twenty-one percent of all respondents and 7% 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% of births in Malawi occurred to women aged 15–24 (Adebowale *et al.*, 2014).

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). Supplementary 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% of respondents were “below average,” 45% were “average,” 45% 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% of cases. Our first method uses observable baseline risky sexual behaviours. 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.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% of unmarried respondents qualify as safe. This approach has the limitation that respondents may not report sexual behaviour truthfully.

The respondent's subjective HIV status perception at baseline provides another safety indicator. Surveyors used beans to elicit responses in 10% 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 $\leq 10\%$ (the HIV prevalence at the time). This subjective probability is 9% for HIV-negative respondents and 32% for HIV-positive respondents ($p < 0.001$), indicating that most people correctly perceive that they are HIV-negative. Most errors occur because

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. Ninety-five percent of pregnancies occur among respondents who have not reached their fertility goal. Seventy-three percent of respondents say their partner has never coerced them to have sex and 95% 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. Orefice 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.

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. Supplementary 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% of young respondents and 51% 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.

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% versus 20–23%, $p < 0.01$).

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 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 6 out of 8 times over the study period.²⁰ Moreover, since 85% of participants had two or fewer total partners, most participants observed the participation and testing behaviour of the same partners multiple times. By testing repeatedly and observing the behaviour 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 behaviour. 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 behaviour. 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 (1) in the control group, or in the treatment group and (2) never tested, (3) tested only once, or (4) 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 according to partners' testing behaviour. 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% of the treatment group) were

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).

20. Among partners, 45% chose to participate while 55% 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% of respondents never tested, 12% of respondents tested once, and 85% of respondents tested multiple times. In addition, 38% of partners never tested, 13% of partners tested once, and 49% of partners tested multiple times.

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

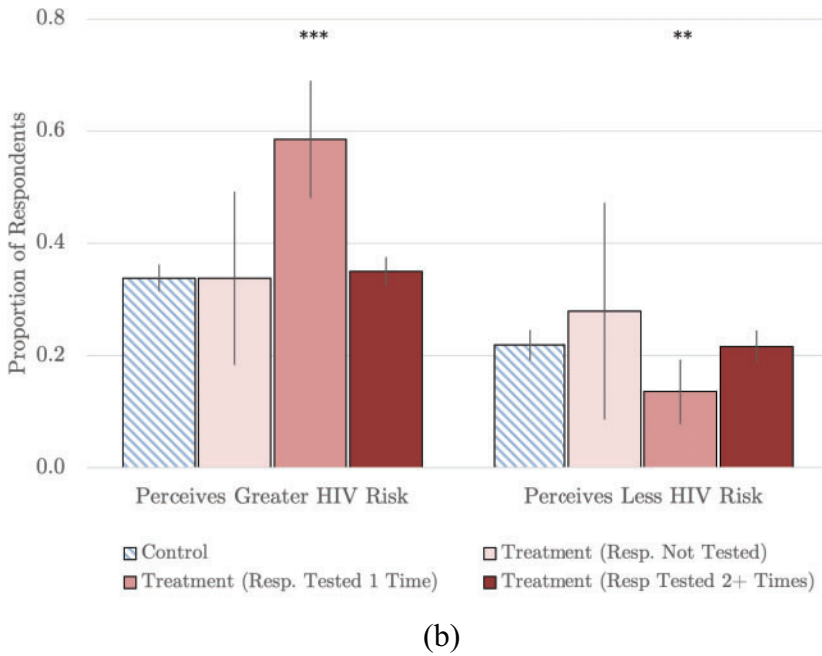
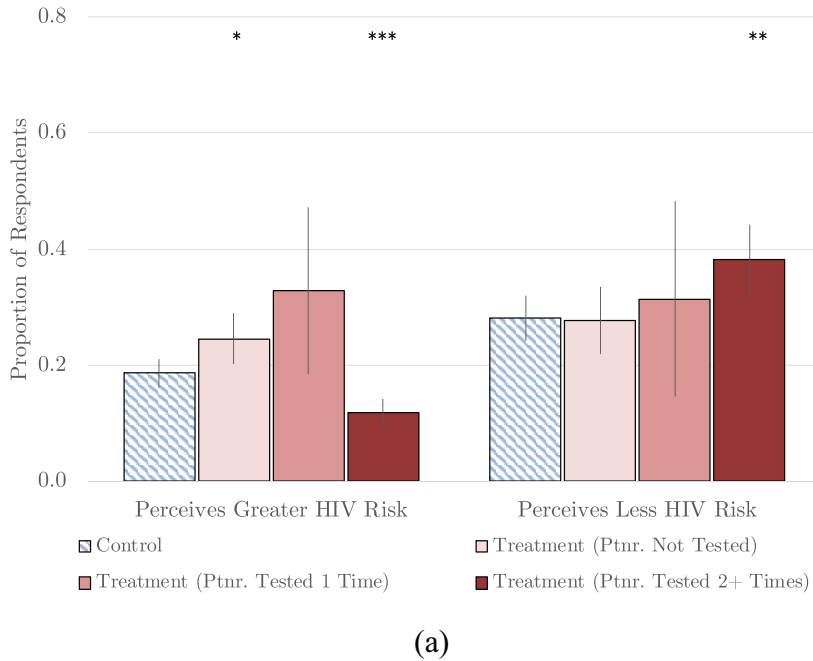


FIGURE 3

Belief updating about partner's and own HIV status

Notes: 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 (A) and themselves (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. $p < 0.1$, $** p < 0.05$, $*** p < 0.01$.

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% 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% for those whose partners ever tested, while it was 29% for those whose partners did not ($p < 0.001$). Each additional partner HIV test was associated with a 4 percentage point (9%) increase in the probability of marriage ($p = 0.02$) and a 5 percentage point (9%) 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.

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.

6.1. *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 ($p < 0.001$). [Supplementary Figure A4](#) shows that fertility increases monotonically with the number of high traits.

6.2. *Predictions about marriage timing if partner safety is unobservable*

Equations (2) and (3) show that safe people marry later than unsafe 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 ($p = 0.07$).

6.3. *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

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.

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 behaviour)		
Safe	17.5 (0.12)	0.49 (0.028)
Unsafe	17.2 (0.13)	0.46 (0.030)
Equality of means (<i>p</i> -value)	0.05	0.51
Observations	592	592
B: Means by attractiveness for safe respondents (defined as above)		
Attractive	17.8 (0.19)	0.60 (0.041)
Unattractive	17.3 (0.16)	0.40 (0.037)
Equality of means (<i>p</i> -value)	0.07	0.00
Observations	319	319

Notes: 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. $p < 0.1$, $**p < 0.05$, $***p < 0.01$.

with highly educated husbands in Column 2 of Table 1 (Panel A). Consistent with this prediction, the proportions 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% 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:

$$Y_{it} = \beta_0 + \beta_1 T_i + \beta_2 Y_i^b + \delta_t + \varepsilon_{it}. \quad (9)$$

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,

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.

Y^b , to increase precision.²⁵ We estimate this specification using OLS and cluster 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% 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. [Supplementary Figure A3](#) illustrates this imbalance by plotting the age distributions in the treatment and control groups. There are 57 additional control respondents who are 15 or 16 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](#)). [Supplementary 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 [Supplementary 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 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% of respondents have ever been tested.²⁷ [Supplementary 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. [Supplementary 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.

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.

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.

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

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 (<i>p</i> -value)	—	0.01	—	0.32	—
Observations	286	303	—	303	—

Notes: 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 is based on a regression of treatment on all covariates in the table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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%) and the probability of pregnancy by 2.7 percentage points (21%) 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%) and the probability of

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*** (0.017) [0.49]	0.035** (0.017) [0.55]	0.027*** (0.0099) [0.13]	0.021** (0.010) [0.13]
B: Estimates by baseline marital status				
Treatment · unmarried	0.072*** (0.027) [0.16]	0.056** (0.028) [0.18]	0.035*** (0.013) [0.10]	0.031** (0.013) [0.10]
Treatment · married	0.0097 (0.018) [0.93]	0.010 (0.018) [0.93]	0.017 (0.015) [0.17]	0.016 (0.015) [0.17]
Equality of coefficients (<i>p</i> -value)	0.06	0.17	0.37	0.47
Reweight by age	No	Yes	No	Yes
Observations	6,048	6,048	6,048	6,048

Notes: 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

pregnancy by 3.5 percentage points (35%) in unweighted estimates. Conversely, effects are small and 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, [Supplementary 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 behaviour 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%)

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

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 behaviour				
Treatment · safe	0.071** (0.029) [0.14]	0.050* (0.029) [0.14]	0.039*** (0.014) [0.09]	0.032** (0.014) [0.09]
Treatment · unsafe	-0.079 (0.087) [0.41]	-0.074 (0.074) [0.41]	0.00074 (0.039) [0.13]	-0.00016 (0.035) [0.13]
Equality of coefficients (<i>p</i> -value)	0.10	0.14	0.35	0.42
Significance of covariates (<i>p</i> -value)	—	0.00	—	0.00
B: Safety defined by own perceived HIV risk				
Treatment · safe	0.064** (0.032) [0.15]	0.041 (0.029) [0.15]	0.048*** (0.014) [0.08]	0.042*** (0.013) [0.08]
Treatment · unsafe	-0.019 (0.073) [0.30]	-0.020 (0.063) [0.30]	-0.032 (0.035) [0.18]	-0.034 (0.031) [0.18]
Equality of coefficients (<i>p</i> -value)	0.29	0.39	0.03	0.03
Significance of covariates (<i>p</i> -value)	—	0.00	—	0.00
Control for treatment · covariates	No	Yes	No	Yes
Observations	3,427	3,427	3,427	3,427

Notes: 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

and the probability of pregnancy by 3.9 percentage points (43%) for safe respondents. The heterogeneous response by safety is statistically significant for marriage at significance levels of 10% 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 conventional significance levels ($p = 0.29$ in Column 1) but is significant for pregnancy at significance levels of 3% 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. [Supplementary Table A1](#) examines impacts

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.

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, [Supplementary 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 [Supplementary Appendix D](#), suggest that the intervention also improved match quality.

Safety may be correlated with other characteristics that cause treatment effect heterogeneity. [Supplementary 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 pregnancy for safe participants is also consistent with our premise that safety information is asymmetric. [Supplementary 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 behaviour to define the safe subsample. Columns 1 and 3 show that the intervention increased the probability of marriage by 11 percentage points (92%) and the probability of pregnancy by 5.1 percentage points (64%) 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 socioeconomic 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.

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.

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 behaviour" definition and 112 such respondents according to the "own perceived HIV risk" definition. [Supplementary 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.

TABLE 5
Estimates by attractiveness for baseline-unmarried and safe respondents

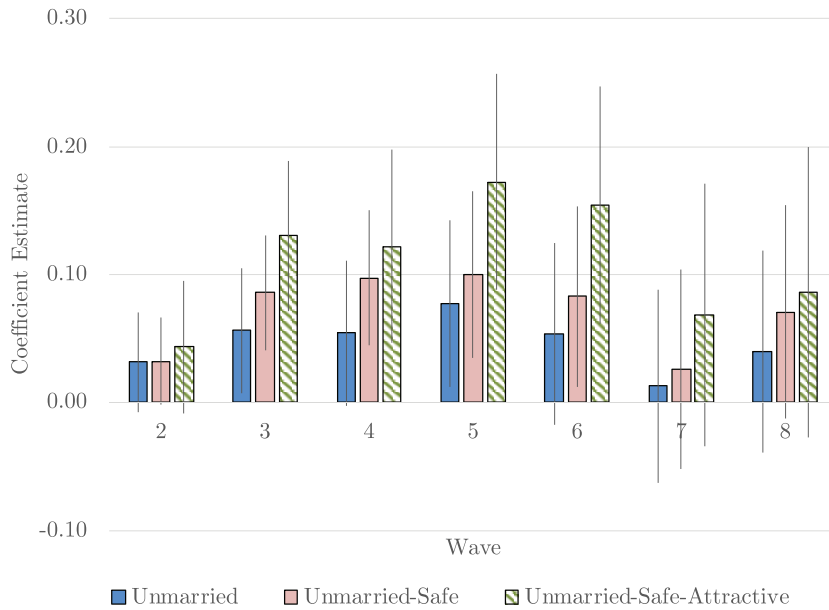
	Currently married		Currently pregnant	
	(1)	(2)	(3)	(4)
A: Safety defined by own risky behaviour				
Treatment · attractive	0.11*** (0.040) [0.12]	0.13*** (0.036) [0.12]	0.051*** (0.018) [0.08]	0.050*** (0.018) [0.08]
Treatment · not attractive	0.023 (0.043) [0.17]	-0.052 (0.042) [0.17]	0.015 (0.022) [0.12]	0.0083 (0.020) [0.12]
Equality of coefficients (<i>p</i> -value)	0.16	0.00	0.21	0.14
Significance of covariates (<i>p</i> -value)	—	0.00	—	0.00
Observations	2,881	2,881	2,881	2,881
B: Safety defined by own perceived HIV risk				
Treatment · attractive	0.12*** (0.039) [0.10]	0.11*** (0.039) [0.10]	0.062*** (0.018) [0.07]	0.056*** (0.018) [0.07]
Treatment · not attractive	-0.0046 (0.051) [0.22]	-0.052 (0.043) [0.22]	0.026 (0.021) [0.10]	0.022 (0.020) [0.10]
Equality of coefficients (<i>p</i> -value)	0.06	0.01	0.20	0.21
Significance of covariates (<i>p</i> -value)	—	0.00	—	0.00
Observations	2,753	2,753	2,753	2,753
Treatment · covariates	No	Yes	No	Yes

Notes: 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

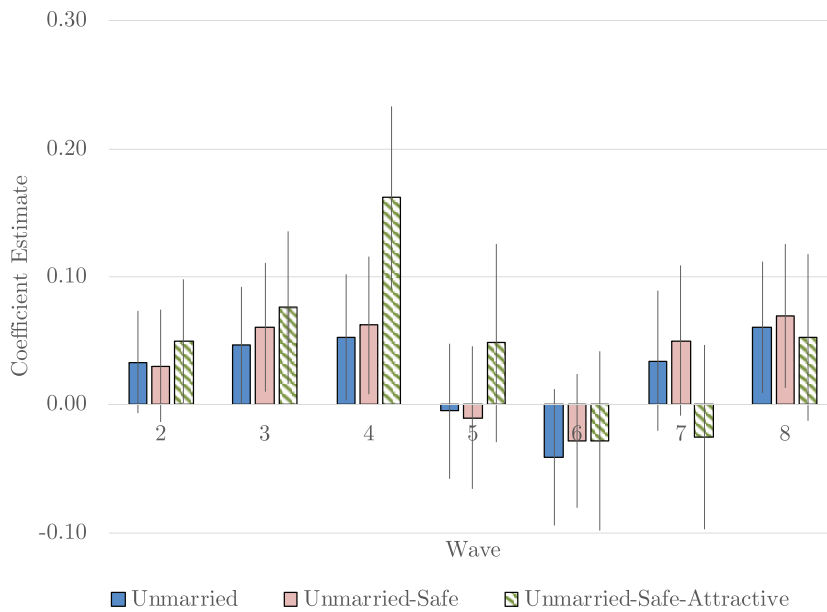
Although covariate interactions with T are jointly significant with $p < 0.001$, all estimates are robust to including these controls. This pattern suggests that our estimates reflect a heterogeneous response by attractiveness rather than a spurious correlation.

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 behaviour” 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. [Supplementary Appendix G](#) provides additional detail about the evolution of HIV testing, safety perceptions, relationship formation, marriage, and pregnancy over the study



(a)



(b)

FIGURE 4

Impacts on marriage and pregnancy by wave

Notes: The figure shows treatment effect estimates on marriage (A) and pregnancy (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.

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% of marriages involved women who were already pregnant and 71% of pregnancies occurred within marriage, rates that do not statistically differ across treatment and control arms. [Supplementary Appendix D](#) examines the impact on self-reported sexual behaviour. 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 behaviour self-reports lead us to interpret these findings cautiously ([Kelly et al., 2013](#)). As additional evidence, [Supplementary 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 [Supplementary 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 behaviour ([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 behaviour.

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. [Supplementary 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. [Supplementary 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 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 signalling 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 2000–10 decrease in the age at first marriage and the age at first birth in Malawi.³³ The impact of offering high-frequency HIV testing equals 79% of the marriage delay and 30% of the fertility delay from 1992 to 2000. It equals 110% of the marriage acceleration and 50% of the fertility acceleration from 2000 to 2010. These magnitudes suggest that the estimated impacts are large. Future work should assess whether other factors

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.

33. For this exercise, we limit the DHS sample to women aged 17–27 and weight to match the age distribution of the 2010 DHS.

TABLE 6
SUR comparison of single-test and multi-test interventions

	Currently married			Currently pregnant		
	Single-test (1)	Multi-test (2)	<i>p</i> -value (3)	Single-test (4)	Multi-test (5)	<i>p</i> -value (6)
A: Overall estimates						
Treatment	-0.018 (0.019) [0.56]	0.035** (0.016) [0.51]	0.03	0.0020 (0.014) [0.13]	0.027* (0.014) [0.13]	0.22
Observations	3238	3601	—	3238	3601	—
B: Estimates by safety (defined by own risky behaviour) for baseline-unmarried respondents						
Treatment · Safe	-0.0067 (0.032) [0.15]	0.078*** (0.025) [0.07]	0.04	-0.0043 (0.020) [0.11]	0.042** (0.019) [0.09]	0.10
Treatment · Unsafe	-0.087 (0.090) [0.31]	-0.073 (0.084) [0.31]	0.91	-0.012 (0.051) [0.17]	0.028 (0.047) [0.10]	0.57
Equality of coefficients (<i>p</i> -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 behaviour)						
Treatment · Attractive	-0.0099 (0.039) [0.13]	0.12*** (0.034) [0.05]	0.00	-0.017 (0.026) [0.11]	0.083*** (0.024) [0.06]	0.00
Treatment · Unattractive	-0.0023 (0.053) [0.17]	0.025 (0.038) [0.10]	0.48	0.022 (0.030) [0.10]	-0.027 (0.033) [0.13]	0.17
Equality of coefficients (<i>p</i> -value)	0.91	0.07	—	0.33	0.00	—
Observations	1388	1743	—	1388	1743	—

Notes: 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

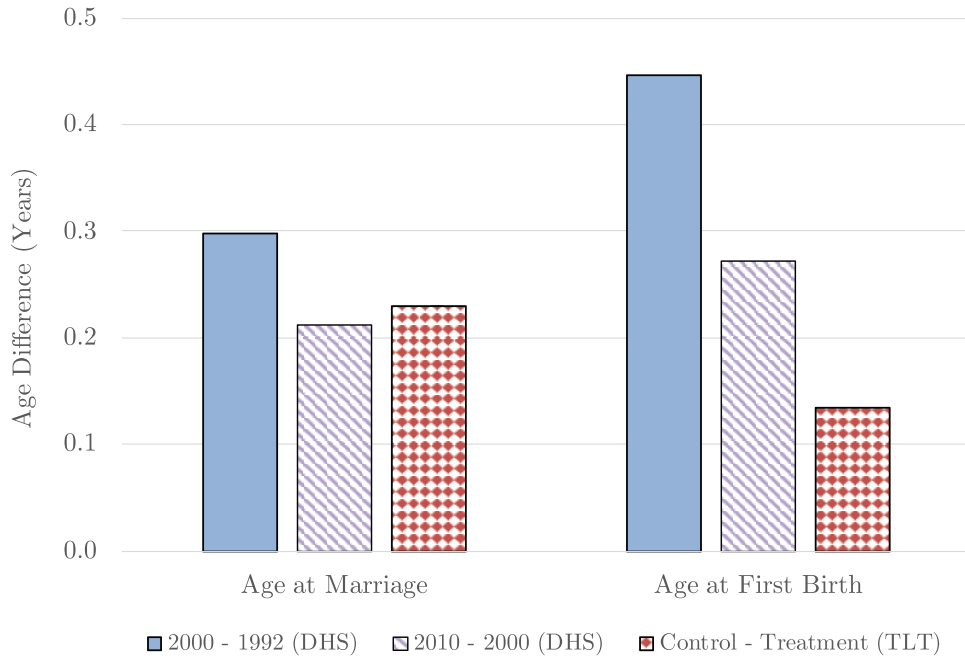


FIGURE 5

A comparison of HIV testing impacts to national marriage and fertility patterns

Notes: 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.

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.

Data availability statement: the data underlying this article are available in the ICPSR repository and can be accessed via the following URL: <https://www.icpsr.umich.edu/web/DSDR/series/767>. The replication package for this article is available at the following link: <http://doi.org/10.5281/zenodo.4290892>.

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Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

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