The Marriage Market for Lemons: HIV Testing and Marriage in Rural Malawi

Manuela Angelucci*
Daniel Bennett†

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Abstract

Asymmetric information in the marriage market may delay marriage and cause adverse selection 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 that changes beliefs about partner's safety accelerates marriage and fertility, 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 fertility delays.

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*Department of Economics, University of Texas at Austin, mangeluc@utexas.edu.

†Center for Economic and Social Research, University of Southern California, bennettd@usc.edu

1 Introduction

In the marriage market, some aspects of partner quality are difficult to observe. People may conceal 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 the timing of marriage. Then it tests the model's predictions in the context of a high-frequency HIV testing intervention that alleviates asymmetric information on sexual safety in Malawi. This analysis is the first empirical examination of adverse selection in the marriage market and may help explain marriage and fertility patterns 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 (STDs). While partner safety is a worldwide concern, the HIV/AIDS epidemic has made partner's sexual safety 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 (e.g. through courtship), HIV/AIDS risk may foster marriage delays. 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 coincides 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 coincides 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 STD infections are often absent. 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. People who test regularly at the same clinic indirectly reveal that they continue to test 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). Moreover, 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 this potential benefit 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 MLSFH only 18 percent of respondents had ever been tested (Thornton 2012). HIV testing is inconvenient: people must travel for several hours to testing facilities and queue in public to be tested (Pinto et al. 2013). Testing is also stigmatized because it may send an unfavorable signal that

[1]This figure and other figures show 83 percent confidence intervals. Two means with non-overlapping 83 percent confidence intervals are significantly different with 95 percent confidence. 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.

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 private information 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 sufficiently patient 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 incentive for safe people to delay marriage, fostering adverse selection in the marriage market. An intervention that signals the safety of all marriage market participants in Period 1 removes this incentive and accelerates marriage and increases marital surplus for safe people. It does not affect marriage but decreases marital surplus for unsafe people in our model.

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 fertility as a proxy for marital surplus. Within the study

period, the intervention increased marriage and pregnancy by 26 and 27 percent among baseline-unmarried respondents. When we consider the full sample (which includes women already married and inframarginal for marriage), the intervention increased the probabilities of marriage and pregnancy by 6 and 16 percent. These impacts are 30-100 percent as large has 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 and pregnancy by 92 and 64 percent. We also show that the intervention, which doubled the frequency of HIV testing, alleviated asymmetric information. While treatment respondents did not differentially update their beliefs about their own status, they did revise their beliefs about the status of their partners. Women whose partners also tested became more confident in the safety of their partners, while women whose partners did not test 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 fertility (Beegle et al. 2015).[3] 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 fertility. 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

[3]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 sexually-transmitted infections (STIs) but that testing positive increases STI prevalence. Gong (2015) finds that positive test results increase STI infections and negative results decrease STI infections, 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 fertility. More generally, Delavande et al. (2016) and Wilson et al. (2014) document the behavioral response to HIV risk.

may lead people to place more emphasis on observable traits and contributes to divorce. We build upon this analysis by considering the effect of unobservable partner quality on marriage timing. Since people face asymmetric information about 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 leading empirical studies of the marriage market test equilibrium predictions using correlational evidence (e.g. Chiappori et al. 2012, Hitsch et al. 2010). We are not aware of other papers that study marriage with experimental variation. We also contribute to a discussion of the consequences of the HIV/AIDS epidemic in SSA. We argue that the epidemic has created an important information asymmetry in the marriage market, which has fostered marriage and fertility delays. This mechanism can explain the recent trend reversal in marriage and fertility in Malawi. It complements the view that pre-marital serial monogamy and other marital institutions contribute to HIV transmission (Bongaarts 2007, Magruder 2011, Greenwood et al. 2017b). Our estimates complement the simulations of the policy responses to the HIV/AIDS epidemic by Greenwood et al. (2017a).

HIV testing may become a more practical signaling technology in the future, as the individual costs of HIV testing continue to decline. The recent movement toward opt-out HIV testing has increased testing utilization (Kennedy et al. 2013). Technological changes, such as the development of 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 fertility in HIV endemic settings. Although we model early marriage and fertility as privately beneficial, the welfare implications of these changes in the marriage market are ambiguous.

2 Theory: Asymmetric Information and Marriage Timing

This section sketches a simple two-period model of bilateral asymmetric information. Under asymmetric information some safe people delay marriage and receive less marital surplus. A

screening and signaling device such as high-frequency HIV testing can reduce the information asymmetry and counteract adverse selection.

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.[4]

In each period, people decide whether and whom to marry. Since we abstract away from 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, she receives a surplus $S^{ab}_{cd} > 0$. Surplus increases with each partner's attractiveness and safety. We assume that a partner with two high traits yields the most surplus, followed by one high trait, followed by zero 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

[4]The model allows for a positive or negative correlation (but not a perfect positive or negative correlation) between attractiveness and safety.

single. Therefore, players maximize expected 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. With identical information sets and preferences, men of the same type make the same offers, which women of the same type (eventually) accept.[5] Since the distribution of preferences does not vary by gender, everyone matches by Period 2 because every man who makes an offer corresponds to a woman who accepts.

2.2 Equilibrium if Safety is Observable in Period 1

In the benchmark case, both safety and attractiveness are observable in both periods. Here, the Gale and Shapley (1962) deferred acceptance algorithm leads to a stable assignment in which everyone marries in Period 1 to a partner with the same number of high traits. Everybody marries in Period 1 since the surplus from marrying in Period 1 always exceeds the surplus from marrying in Period 2. The ability to observe both traits in Period 1 removes any incentive to postpone marriage. Expected surplus for people of attractiveness a and safety b is

$$E(S^{ab}) = S^{ab}_{ab} \cdot (1 + E(\delta)) \tag{2}$$

2.3 Equilibrium if Safety is Unobservable in Period 1

Next we consider the case in which people know the distributions of safety and the discount factor in the population but do not observe the safety of others in Period 1. In this situation, 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

[5] All men initially propose to hh women, who eventually accept the offers of hh men. The remaining men, who have at most one high trait, make offers to women with one high trait, and these women eventually accept the offers of men with one high trait. The remaining ll men make offers to the remaining ll women, who eventually accept. A stable positive assortative assignment of this type exists regardless of which gender makes the offers since the trait distribution does not differ by gender.

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, people match based on expected rather than observed surplus. Regardless of safety, marrying an attractive partner yields at least as much expected surplus as marrying an unattractive partner.[6] Among people who marry in Period 1, the Gale and Shapley (1962) algorithm leads to a stable assignment that is positively assortative in attractiveness.

Next we consider the marriage timing decision. People marry early if this choice maximizes expected surplus. Inequality (3) shows that unsafe people always prefer to marry in Period 1.

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

In this expression, p_{al} and p_{ah} are the population proportions of unsafe and safe people with attractiveness a. The inequality holds because $\delta_i > 0$ and $S^{al}_{ah} > S^{al}_{al}$. 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

$$(1+\delta_i)\,\frac{p_{al}S^{ah}_{al}+\mu_{ah}S^{ah}_{ah}}{p_{al}+\mu_{ah}} > \delta_i \cdot S^{ah}_{ah} \tag{4}$$

The parameter $\mu_{ah} \in [0, p_{ah}]$ is the population proportion of safe people of attractiveness a who marry in Period 1. The expression shows that safe people who are sufficiently patient delay marriage. Solving for δ_i yields an expression for $\bar{\delta}^a$, the threshold value for δ. Safe

[6] 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.

people of attractiveness a for whom $\delta_i < \bar{\delta}^a$ marry early.

$$\bar{\delta}^a = \frac{\mu_{ah} S^{ah}_{ah} + p_{al} S^{ah}_{al}}{p_{al}(S^{ah}_{ah} - S^{ah}_{al})} > 0 \tag{5}$$

$\bar{\delta}^a$ is always positive because the numerator and denominator of Inequality (5) 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, $\frac{\mu_{ah}}{p_{ah}} = F(\bar{\delta}^a) = \bar{\delta}^a / b$ so that $\bar{\delta}^a = b \cdot \frac{\mu_{ah}}{p_{ah}}$. We equate this expression to $\bar{\delta}^a$ in Inequality (5) to solve for $\frac{\mu^*_{ah}}{p_{ah}}$, the fraction of safe people of attractiveness a who marry early in equilibrium.

$$\frac{\mu^*_{ah}}{p_{ah}} = \frac{1}{b(r^{ah} - 1) - r^{ah} \cdot \frac{p_{ah}}{p_{al}}} \quad \forall\, b \in [b_1, b_2] \tag{6}$$

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 (6) establishes that a stable assignment exists in which all unsafe people as well as safe people of attractiveness a and discount factor $\delta_i < \bar{\delta}^a$ marry in Period 1. Expressions (3) and (4) (evaluated at μ^*_{ah} and $\bar{\delta}^a$) show that no player has an incentive to deviate in this scenario.

Values of $\mu^*_{ah} < p_{ah}$ in Equation (6) 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 the surplus loss from marrying an unsafe partner.

The prediction that asymmetric information causes adverse selection for safe people is robust to the following alternative assumptions: (i) attractiveness and safety have different relative impacts on surplus, (ii) spouses can divorce, (iii) marital surplus is negative for some people, (iv) attractiveness and patience are positively correlated, (v) safety and patience are positively correlated, and (vi) the trait distributions differ by gender. Conversely, when people are also unaware of their own safety, as well as the safety of others, both safe and

unsafe people who are sufficiently patient delay marriage. We discuss all of these alternatives in Appendix A.

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

$$E(S^{al}) = \left(\frac{p_{al}S^{al}_{al} + \mu^*_{ah}S^{al}_{ah}}{p_{al} + \mu^*_{ah}}\right)(1 + E(\delta)) \tag{7}$$

A comparison of this expression with Equation (2) shows that asymmetric information increases the expected surplus of unsafe people by allowing some of them to match with safe partners.

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

$$E(S^{ah}) = \frac{\mu^*_{ah}}{p_{ah}} \underbrace{\left(\frac{p_{al}S^{ah}_{al} + \mu^*_{ah}S^{ah}_{ah}}{p_{al} + \mu^*_{ah}}\right)(1 + E(\delta|\delta < \bar{\delta^a}))}_{\substack{\text{Surplus from Marriage} \\ \text{in Period 1}}} + \left(1 - \frac{\mu^*_{ah}}{p_{ah}}\right) \cdot \underbrace{S^{ah}_{ah} \cdot E(\delta|\delta > \bar{\delta^a})}_{\substack{\text{Surplus from Marriage} \\ \text{in Period 2}}} \tag{8}$$

In this expression, $\frac{\mu^*_{ah}}{p_{ah}}$ 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 $\bar{\delta}^a$ determines marriage timing in Equation (5). A comparison of this expression with Equation (2) shows that asymmetric information reduces expected 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

The final part of our analysis examines 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.

$$r_{hh}\left(b - \frac{p_{hh}}{p_{hl}}\right) \; > \; r_{lh}\left(b - \frac{p_{lh}}{p_{ll}}\right) \tag{9}$$

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 $r_{hh} = r_{lh}$ and $\frac{p_{hl}}{p_{hh}} > \frac{p_{ll}}{p_{lh}}$, so that unsafe people are more prevalent (i.e., there are more "lemons") in the attractive subpopulation.[8]

Next we consider the way that attractiveness influences marital surplus. In Equation (8), the terms S^{ah}_{ah} and S^{ah}_{al} are always larger for attractive people. However, expected 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 $E(\delta|\delta < \bar{\delta^a})$ and $E(\delta|\delta > \bar{\delta^a})$ in Equation (8) are also lower for attractive people. In this case, $r^{hh} \gg r^{lh}$ (so that safe partners are substantially more valuable for attractive people) for expected 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

[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 $r_{hh} = r_{lh}$.

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. This prediction sets apart a model of asymmetric information. If people are unaware of their own safety, an intervention that makes safety visible should accelerate marriage for both safe and unsafe 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 expected 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 expected 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. In the unsafe subpopulation, the negative impact on surplus is larger for attractive people under assumptions that we discuss in Appendix B.

3 Sexual Safety and HIV Testing

The cost of marrying a sexually unsafe partner increases with HIV prevalence. The HIV/AIDS epidemic emerged in Malawi around 1985. HIV prevalence peaked at 14 percent in 1998 and has 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. HIV risk is a key partner attribute for marriage market participants in this setting. HIV-positive people have a shorter life expectancy and require extra medical care (Oni et al. 2002). They may also infect their spouses, particularly since condom use is frowned upon within marriage (Chimbiri 2007, Tavory and Swindler 2009).

A marriage market participant may worry about a partner's propensity for risky sexual behavior, which in turn determines current and future HIV status (Smith and Watkins 2005).

Both factors are difficult to observe. People often conceal promiscuous behavior and HIV remains asymptomatic for several years after infection. Without a credible signal of partner quality, a safe person might 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, HIV testing can provide a signal of 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 high risk if testing is costly, whereas *not seeking* a test may signal that someone is high risk if testing is cheap.

HIV testing is costly in many parts of SSA, as it remains inconvenient and stigmatized. Providers typically follow an "opt-in" model, in which the patient initiates the test. A typical test 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. Pinto et al. (2013) find that patients in the Zomba District, which is adjacent to our study area, spend an average of 7.1 hours per visit seeking HIV care. Stigma is also an important barrier to HIV testing (Sambisa et al. 2010, Berendes and Rimal 2011, Ngatia 2011, Maughan-Brown and Nyblade 2014).[10] The inconvenience

[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.

[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.

and stigma induce people to not test regularly. Indeed, 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 out-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 is 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 follows a representative sample of women aged 15 to 25 over eight waves that are spaced four months apart. The survey covers 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 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

[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).

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 provides an indication of her HIV status in previous waves.

The study incorporates three intervention arms that were selected 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. Our primary analysis compares the treatment and control arms. 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 report 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.

Surveyors encouraged participants to invite their partners into the study, enrolling participants and partners into the same arm. Partners completed a similar questionnaire and received the same compensation as other study participants. This design feature enabled treatment respondents to screen partners according to the willingness to participate and submit to testing. Partners participated in the survey and received HIV tests 30 percent of the time. In addition, some participants and partners sought testing at other health care facilities.[12]

The intervention had a large impact on the frequency of HIV testing. Figure 2 shows that the intervention more than doubled the probability of testing within four months (either through the study or elsewhere), increasing it from 30 to 70 percent for respondents and from 30 to 55 percent for their partners. These differences are highly statistically significant, with $p < 0.001$ in both cases. This pattern suggests that the intervention substantially reduced

[12]Partners who chose to participate and receive testing were 50 percentage points more likely to be married than partners who did not participate. The participation rate did not vary by the partner's age or education.

the personal cost of HIV testing.[13]

HIV testing is strongly associated with subsequent marriage. While surveyors tested nearly all treatment participants, they only tested 35 percent of treatment partners. A comparison of the marriage rate by Wave 8 for participants with tested and untested partners shows that participants with tested partners were 46 percentage points more likely to marry. This pattern, which could also arise through selection, is consistent with the provision of safety information through HIV testing.

5 Measurement

5.1 Marriage, Fertility, 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). 44 percent of respondents were married at baseline and 63 percent were married at Wave 8.[14] Urine-based pregnancy tests in each period measure fertility. Respondents completed the tests over 95 percent of the time and most non-compliers were visibly pregnant.[15] Pregnancy, which is an important outcome in its own right, allows us to test theoretical predictions related to marital surplus. Fertility is indicative of marital surplus if the production of children is one of the gains from marriage

[13]The testing frequency varied by 8 percentage points across waves for both respondents and partners, however there were not a systematic trend for either group. Baseline-married participants were 5 percentage points more likely to test than baseline-unmarried participants. The partners of baseline-married participants were 3 percentage points more likely to test than the partners of baseline-unmarried participants.

[14]Divorce is a possible ramification of the intervention (Schatz 2005). 7 percent of respondents divorce during the study interval. However, our analysis does not focus on divorce because it is uncorrelated with treatment.

[15]Pregnancy status at Wave t does not fully reflect pregnancy completion, endline parity, or life-cycle fertility. However, the correlation between the number of positive pregnancy tests and parity in Wave 8 is 0.79 for respondents without children at baseline. This pattern suggests that observed pregnancy is indicative of subsequent childbirth. 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).

(Becker 1973, Edwards and Roff 2016).[16]

Around 42 percent of respondents were married and 13 percent of respondents were pregnant at baseline. 3.5 percent of respondents married (and fewer than 1 percent divorced) and 7 percent of respondents became newly pregnant on average between subsequent survey waves. The pregnancy rate is 3 times higher for married respondents than for singles. Over 95 percent of pregnant respondents identify their current partner as the father of the baby.

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. Angelucci and Bennett (2017) analyze the sample of baseline-married respondents and show that fertility increases in attractiveness, supporting the assumption that attractive people receive more marital surplus. These respondents have more educated husbands, which suggests positive assortative matching. These patterns also validate that our attractiveness measure is a salient marriage market attribute.

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

[16] Marriage does not necessarily precede pregnancy, however married respondents are 65 percentage points more likely to have children at baseline. Appendix D shows that the treatment effects on marriage and pregnancy are strongly correlated, and that an two-wave pregnancy lag maximizes this relationship.

[17] Oreffice and Quintana-Domeque (2014) weigh the merits of this measurement approach. Since surveyors assessed attractiveness at the end of the interview, the mannerisms of respondents may influence 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.

agree in 77 percent of cases. First, we classify participants using observable baseline risky sexual behaviors. Respondents qualify 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, 58 percent of all respondents qualify as safe, while 85 percent of unmarried respondents qualify as safe.

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). All respondents who believe their probability of having HIV is 10 percent or less are classified as safe. Under this designation, 77 percent of all respondents and 81 percent of unmarried respondents qualify as safe. For both measures, the HIV prevalence among treatment respondents (for whom we have baseline test results) is 2-3 times higher in the unsafe group than in the safe group. The subjective infection probability is 0.09 for HIV-negative people and 0.30 for HIV-positive people ($p < 0.001$). This pattern indicates that most people correctly believe that they are HIV negative and that most errors occur because HIV-positive people misjudge their status.[19]

5.3 Learning about Partner Safety

This subsection provides evidence that the HIV testing intervention helps to resolve asymmetric information about partner safety. First, a comparison of the accuracy of perceptions

[18] 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.

[19] An additional 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. An analysis that uses endline HIV status as a safety proxy yields similar results and is available from the authors.

of own status and partner status help to establish that safety information is asymmetric. We show above that own perceived HIV risk is substantially higher among HIV-positive respondents. In contrast, partner safety perceptions are not correlated with the HIV status of partners. Surveyors elicited the likelihood that the partner currently has HIV on a 5-point Likert scale.[20] At baseline, treatment respondents with HIV-positive partners ($n = 7$) perceived that their partners had an HIV likelihood of 1.43, whereas those with HIV-negative partners ($n = 115$) perceived that their partners had an HIV likelihood of 1.61. Despite the small HIV-positive sample size, the similarity of these estimates suggests that partner safety is less observable than own safety.

Next we consider whether the intervention helped to resolve asymmetric information in the marriage market. In the ideal exercise, we would examine the impact on market-wide perceptions of the safety of study participants. While these data are not available, we instead examine the impact of the intervention on perceived partner safety. Since participants and partners received the same HIV testing intervention, treatment-control comparisons of perceived partner safety indicate whether the intervention changed beliefs about partner safety. Estimating this treatment effect is difficult in practice because relationship formation is endogenous. Therefore we do not interpret these results casually but instead assess whether they are consistent with the model's predictions.

Figure 4 shows how respondents revise their beliefs about their own safety and the safety of their partners in the treatment and control groups. We distinguish between respondents who revise upward (so that they perceive less HIV risk at followup than at baseline), those who revise downward (so that they perceive more HIV risk at followup than at baseline), and those who do not revise in either direction. Panel A shows that there is no economically or statistically significant impact on own perceived safety. A handful of respondents learn from the intervention that they are HIV positive, which makes the treatment group slightly more likely to revise their own safety perceptions downward. Overall, most respondents correctly

[20]The categories for this variable are 1 (no likelihood), 2 (low), 3 (medium), 4 (high), and 5 (know he is infected). Partner HIV risk is measured differently from own HIV risk, which uses a probability scale.

perceive that they are safe at baseline and receive HIV-negative test results that confirm these perceptions.

In contrast, Panel B shows that the intervention does lead respondents to update partner safety perceptions. Treatment respondents, who mostly receive the signal that their partners are safe, are 5 percentage points (15 percent) more likely to update their beliefs upward ($p = 0.12$). This magnitude is large since 53 percent of respondents already believe their partners are safe at baseline. Panel C illustrates heterogeneity in belief updating within the treatment group. Here we distinguish between treatment respondents whose partners did and did not receive an HIV test as part of the study. People whose partners test mostly learn that partners are HIV negative and revise safety upward, while those whose partners do not test suspect that partners may be hiding something and revise downward. These differences are large and statistically significant. This pattern is consistent with HIV testing working as a screening device. However, we recognize that this comparison is not causal and could also arise if people who revise upward happen to choose partners who are willing to test for unrelated reasons.

6 Identification and Estimation

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

$$Y_{it} = \beta_0 + \beta_1 T_i + \beta_2 Y_i^b + \delta_t + \varepsilon_{it} \tag{10}$$

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.[21] We estimate this specification using OLS and cluster

[21] 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

standard errors by respondent. The coefficient of interest, β_1, identifies the average treatment effect of offering high-frequency HIV testing.[22]

This parameter is identified under two assumptions. First, one participant's treatment assignment does 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 is 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 A2 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 appear in Table 1. Column 1 shows the mean for the treatment group and Columns 2 and 4 show the treatment-control difference before and after age-reweighting. Before reweighting, treatment respondents are 12 percentage points less likely to be enrolled in school and 4 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 reweight by age. We cannot compare the baseline HIV status of treatment and control respondents because control respondents were not offered testing at baseline. However a comparison of perceived

authors.

[22]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.

HIV risk and actual HIV test results in the treatment group shows that respondents have correct HIV status beliefs on average. 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 percent of respondents have ever been tested.[23]

After estimating overall effects, we test the model predictions by examining heterogeneity in the treatment effects by safety and attractiveness. We limit the sample to baseline-unmarried respondents for safety interaction estimates and to baseline-unmarried and safe respondents for the attractiveness interaction estimates. Tables A5 and A6 provide baseline summary statistics for these subsamples. Safe respondents are younger, richer, and have higher school enrollment than unsafe respondents. Attractive people are wealthier and have a stronger future orientation than unattractive people. As we discuss below, we assess the robustness of our estimates by controlling for the interaction between treatment and baseline covariates.

7 Results

7.1 Impacts on Marriage and Fertility

Table 2 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 age-unweighted estimates while even columns reweight 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,

[23] Appendix G 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.

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 age-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.

A comparison of odd and even columns shows that estimates are similar but somewhat smaller after age reweighting. In Panel A, the overall impact on marriage is 1 percentage point lower and the overall impact on pregnancy is 0.6 percentage points lower after age reweighting. In Panel B, the baseline-unmarried impacts on marriage and pregnancy are 1.6 percentage points and 0.4 percentage points lower, respectively. Seemingly unrelated regressions (available from the authors) indicate that weighted and unweighted estimates are not significantly different for any estimates in the table. The robustness to age reweighting 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 age-unweighted results.

Figure 5 plots the probability of marriage and pregnancy by wave and intervention arm. The marriage and pregnancy gaps between the treatment and control arms increased over time and peaked around Waves 4 and 5, consistent with the idea that repetition increases the marriage market benefit of HIV testing. The gaps declined toward the end of the study period, perhaps due to intertemporal substitution in these outcomes.

Since we observe marriage and fertility only over 28 months, these effects are difficult to relate to impacts over the life cycle. Appendix D shows that the impacts on fertility and marriage are strongly correlated and explores the timing of this correlation.

7.2 Heterogeneous Effects by Safety

Next we limit the sample to baseline-unmarried respondents to test the model's predictions and examine treatment effect heterogeneity by safety. According to the model, 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 fertility proxies for marital surplus, the intervention should increase the fertility of safe people and decrease the fertility of unsafe people.

Table 3 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. Here, 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 increases 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. 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 statistically significant. The increase of the marriage and pregnancy among safe respondents, but not among unsafe ones, aligns with the predictions from our model of asymmetric information.[24]

Safety may be correlated with other characteristics that cause treatment effect heterogeneity. Appendix H 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

[24] 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.

covariates.[25] 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 3 show that estimates are robust to these controls, suggesting that the heterogeneous effect by safety is not spurious.

The findings that offering high-frequency HIV testing accelerates marriage and pregnancy for safe participants are consistent with our premise that safety information is asymmetric. Appendix A.3 clarifies that the intervention should have 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 suggest 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 (explored further in Appendix A.4) that marrying unsafe people yields negative marital surplus.

7.3 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 under both alternative safety definitions.[26]

Table 4 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,

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

[26] For unsafe people, the model predicts no differential effect on marriage timing by attractiveness, however it predicts that the intervention should differentially reduce attractive surplus. According to the "own risky behavior" definition, there are 91 unsafe and baseline-unmarried respondents in our sample. According to the "own perceived HIV risk" definition, there are 112 such respondents. These small samples do not provide adequate statistical power to identify attractiveness interactions among unsafe respondents. These estimates, which are available from the authors, show no significant differential impacts by attractiveness for marriage or pregnancy.

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.

Figures 6 and 7 show the variation underlying these patterns. For attractive respondents, the treatment-control gaps in marriage and pregnancy peak in Waves 4 and 5 and shrink later, while there are no consistent treatment-control gaps for unattractive respondents.

Since attractive people are more forward-looking and have higher socioeconomic status, Columns 2 and 4 of Table 4 repeat the exercise in Table 3 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.

7.4 Alternative Explanations

Three alternative mechanisms could contribute to our findings. The intervention may have encouraged unprotected sex, which could in turn directly affect pregnancy and marriage. However we find small and statistically insignificant effects on coital frequency, condom use, and the number of partners (estimates available from the authors).[27] These results match the conclusions of several other studies, which show small effects of HIV testing on risky sexual behavior (Thornton 2008, Baird et al. 2014, Gong 2015). Seemingly unrelated regression estimates in Appendix D show the strongest correlation between the treatment effects on marriage and *lagged* pregnancy, which is inconsistent with this mechanism.

The intervention could also have encouraged marriage and fertility by making family formation more salient. This mechanism, while plausible, does not explain the differential response for attractive respondents in Table 4. Finally, the intervention could have increased

[27] These results are consistent with the lack of an impact on own perceived HIV status (de Paula et al. 2014).

fertility by decreasing female bargaining power (Rasul 2008). In the baseline-married sample, men and women indicate similar optimal family sizes, however women prefer to have children later. We do not find effects on other bargaining power proxies, including gifts from partners and indicators of female autonomy (estimates available from the authors).

8 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) and marriage and fertility (Beegle et al. 2015). The TLT HIV testing 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.

This section attempts to reconcile our findings with the literature by assessing the importance of repeated HIV testing. As we explain in Section 4, the TLT Panel Study includes a third intervention arm ($n = 500$) 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. Table A7 in Appendix I 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 (10), reweight to balance by age, pool follow-up rounds, and cluster standard errors by respondent to match our previous empirical strategy. However this inquiry differs from our primary analysis because the follow-up period includes four rather than seven waves.

Table 5 provides treatment effect estimates for the single-test intervention. The overall estimates in Panel A are analogous to Table 2 (Panel A), the estimates by safety in Panel B are analogous to Table 3 (Panel A), and the estimates by attractiveness in Panel C

are analogous to Table 4 (Panel A). All estimates are small and many are negative. No estimates are statistically insignificant. The single-test intervention had no effect on marriage or pregnancy overall or in the safe or attractive subsamples.

Next we contrast these estimates with our main results and assess statistical significance using seemingly unrelated regressions. For a like-to-like comparison, we reestimate our main results using only Waves 2-5 and reweight to match the age distribution in the single-test sample.[28] We test whether each estimate in Table 5 is significantly different from the analogous repeated-test estimate. Daggers in Table 5 indicate significant differences between the single-test and repeated-test estimates. For marriage, repeated-test effects are significantly larger overall and for the safe and attractive subsamples. For pregnancy, the repeated-testing effect is larger in the attractive subsample (Panel C) but is not significantly larger overall ($p = 0.27$) or for the safe subsample ($p = 0.22$). These findings suggest that repeated testing is essential to the strong marriage and fertility effects above.

9 Discussion and Conclusion

The HIV/AIDS epidemic has coincided with marriage and fertility 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 emergence of the HIV/AIDS epidemic has exacerbated asymmetric information in the marriage market. The marriage and fertility effects of high-frequency testing support this hypothesis. We show that safe people responded the most to this intervention, which suggests that high-frequency testing enabled these people to signal and screen.

To gauge the size of our impacts, Figure 8 compares treatment effect estimates of high-frequency testing to the 1992-2000 increase and the 2000-2010 decrease in the age at first

[28] These estimates closely resemble our main results and are available from the authors. Age reweighting across both samples is necessary because respondents are younger in Waves 2-5 than in Waves 5-8 and this age difference could mechanically generate treatment effect differences across the interventions.

marriage and the age at first birth in Malawi.[29] The impact of offering high-frequency HIV 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 show that the provision of opt-out testing is likely to have strong effects on marriage and fertility. 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 fertility in communities with HIV. The welfare implication of this pattern is ambiguous. In our model, the resolution of asymmetric information improves welfare for safe people. However, early marriage and fertility may have other private and social costs (Jensen and Thornton 2003). We find no impact of the intervention on school enrollment, which may mitigate this concern.

[29]For this exercise, we limit the DHS sample to women aged 17-27 and reweight to match the age distribution of the 2010 DHS.

Table 1: Baseline Characteristics by Treatment Status

	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
Subjective 5-year mort. risk (percent)	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
Subjective HIV risk (percent)	0.12	0.02*	0.01	0.02	0.01
Subjective partner HIV likelihood (1-5)	1.6	0.05	0.06	0.05	0.07
Outcomes					
Married	0.43	0.03	0.03	-0.04	0.03
Pregnant	0.15	0.04**	0.02	0.03	0.02
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: 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 (10) 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 3: 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**	0.068**	0.039***	0.042***
	(0.029)	(0.029)	(0.014)	(0.014)
	[0.14]	[0.14]	[0.09]	[0.09]
Treatment · Unsafe	-0.079	-0.090	0.00074	-0.020
	(0.087)	(0.078)	(0.039)	(0.038)
	[0.41]	[0.41]	[0.13]	[0.13]
Equality of coefficients (p-value)	0.10	0.07	0.35	0.13
Significance of covariates (p-value)	-	0.00	-	0.00
B: Safety Defined by Own Perceived HIV Risk				
Treatment · Safe	0.064**	0.061**	0.048***	0.049***
	(0.032)	(0.030)	(0.014)	(0.014)
	[0.15]	[0.15]	[0.08]	[0.08]
Treatment · Unsafe	-0.019	-0.029	-0.032	-0.042
	(0.073)	(0.067)	(0.035)	(0.033)
	[0.30]	[0.30]	[0.18]	[0.18]
Equality of coefficients (p-value)	0.29	0.23	0.03	0.01
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 (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 4: 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***	0.14***	0.051***	0.056***
	(0.040)	(0.039)	(0.018)	(0.019)
	[0.12]	[0.12]	[0.08]	[0.08]
Treatment · Not Attractive	0.023	-0.021	0.015	0.012
	(0.043)	(0.044)	(0.022)	(0.021)
	[0.17]	[0.17]	[0.12]	[0.12]
Equality of coefficients (p-value)	0.16	0.01	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***	0.13***	0.062***	0.065***
	(0.039)	(0.041)	(0.018)	(0.018)
	[0.10]	[0.10]	[0.07]	[0.07]
Treatment · Not Attractive	-0.0046	-0.033	0.026	0.025
	(0.051)	(0.048)	(0.021)	(0.020)
	[0.22]	[0.22]	[0.10]	[0.10]
Equality of coefficients (p-value)	0.06	0.02	0.20	0.15
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 (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 for a Single-Test Intervention

	Currently Married	Currently Pregnant
	(1)	(2)
A: Overall Estimates		
Treatment	-0.022††	0.0020
	(0.018)	(0.014)
	[0.56]	[0.13]
Observations	3238	3238
B: Estimates by Safety (Defined by Own Risky Behavior)		
Treatment · Safe	-0.0067†	-0.0043
	(0.032)	(0.020)
	[0.15]	[0.11]
Treatment · Unsafe	-0.087	-0.012
	(0.090)	(0.051)
	[0.31]	[0.16]
Equality of coefficients (p-value)	0.40	0.89
Observations	1673	1673
C: Estimates by Attractiveness		
Treatment · Attractive	-0.010†††	-0.017†††
	(0.039)	(0.026)
	[0.13]	[0.11]
Treatment · Unattractive	-0.0023	0.022
	(0.053)	(0.030)
	[0.17]	[0.10]
Equality of coefficients (p-value)	0.91	0.33
Observations	1388	1388

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 (10). Panel B is limited to baseline-unmarried respondents and uses the same specification as Panel A of Table 3. Panel C is limited to safe, baseline-unmarried respondents and uses the same specification as Panel A of Table 4. **No estimates in the table are significantly different from zero. Daggers indicate significant differences from repeated-testing estimates based on seemingly unrelated regressions, as the text explains.** † $p < 0.1$, †† $p < 0.05$, ††† $p < 0.01$.

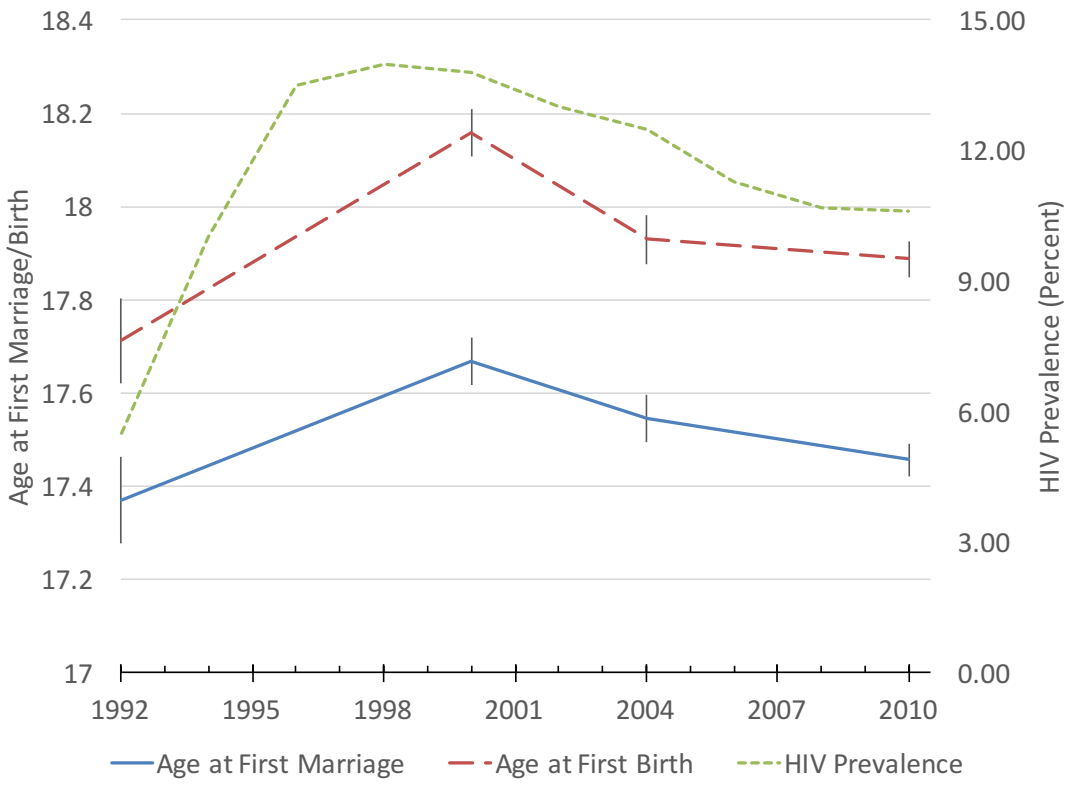


Figure 1: Marriage and Fertility for Women Aged 17-27
(with 83% Confidence Intervals)

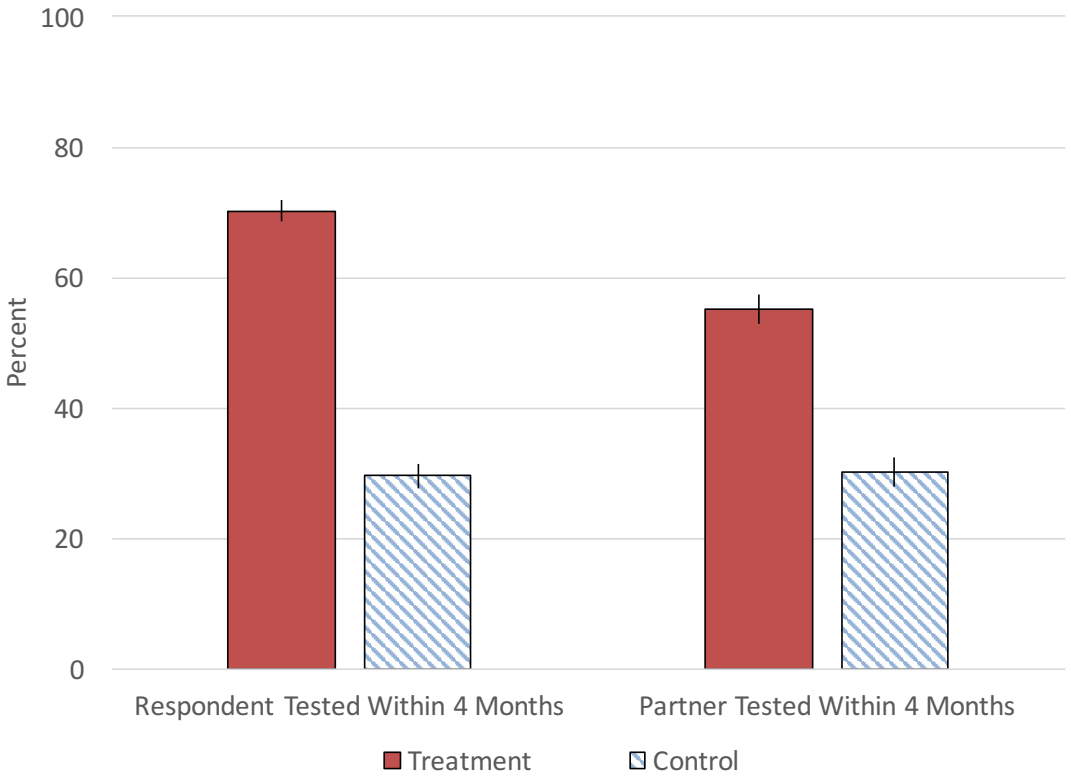
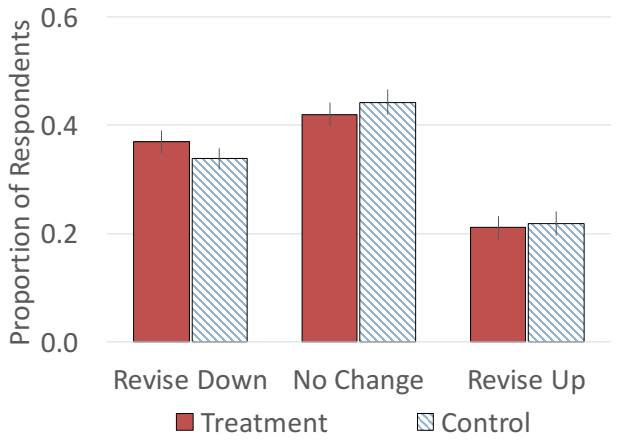
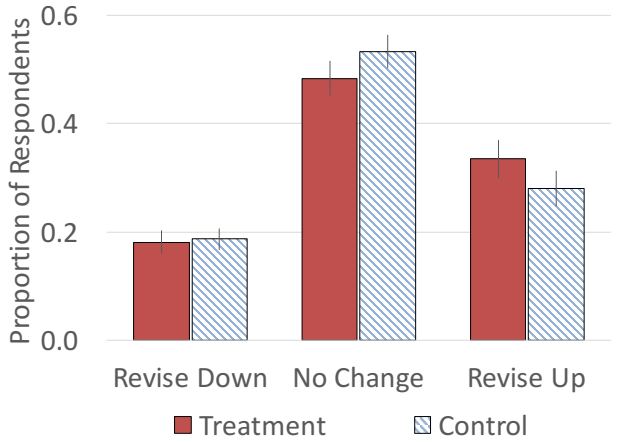


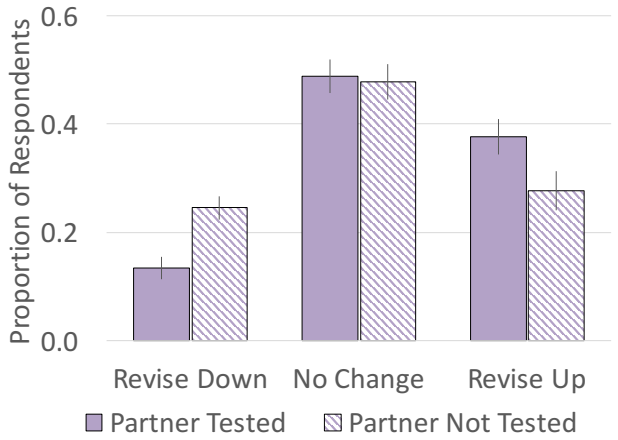
Figure 2: Probability of Testing within Four Months by Treatment Arm (with 83% Confidence Intervals)



(a) Beliefs about Own Safety, by Intervention Arm



(b) Beliefs about Partner Safety, by Intervention Arm



(c) Beliefs about Partner Safety for Treatment Respondents, by Whether Partner Tests

Figure 4: Belief Updating about Own Safety and Partner Safety
(with 83% Confidence Intervals)

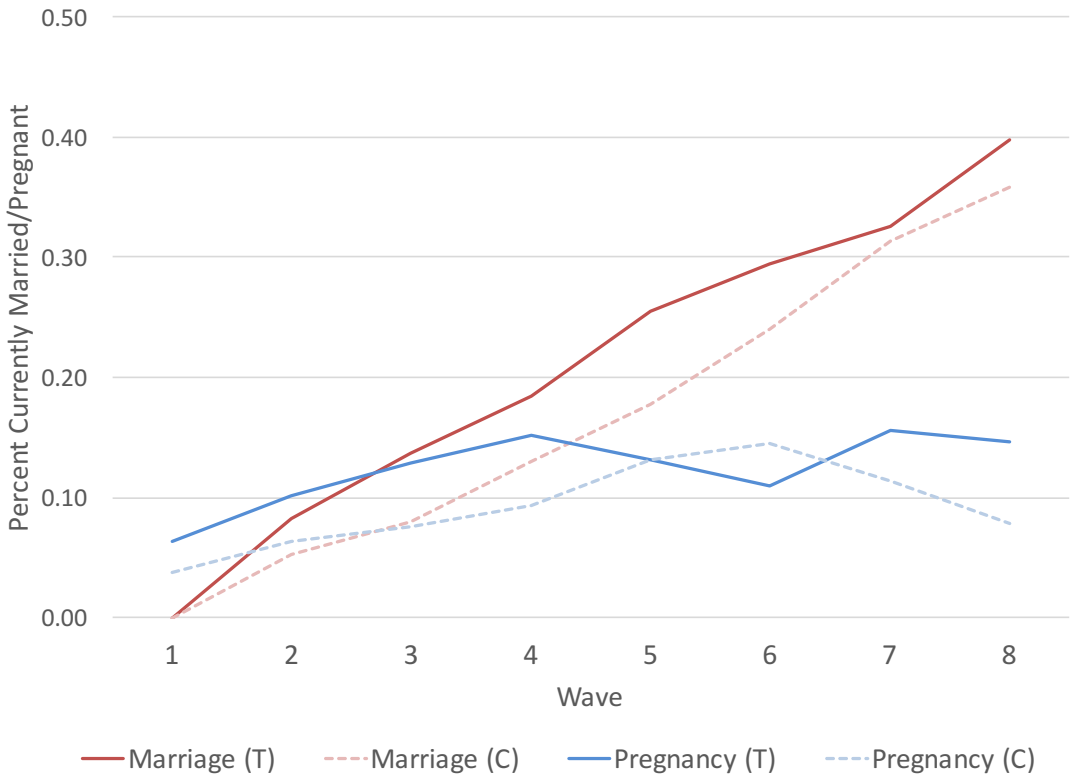


Figure 5: Marriage and Pregnancy for Baseline-Unmarried Respondents

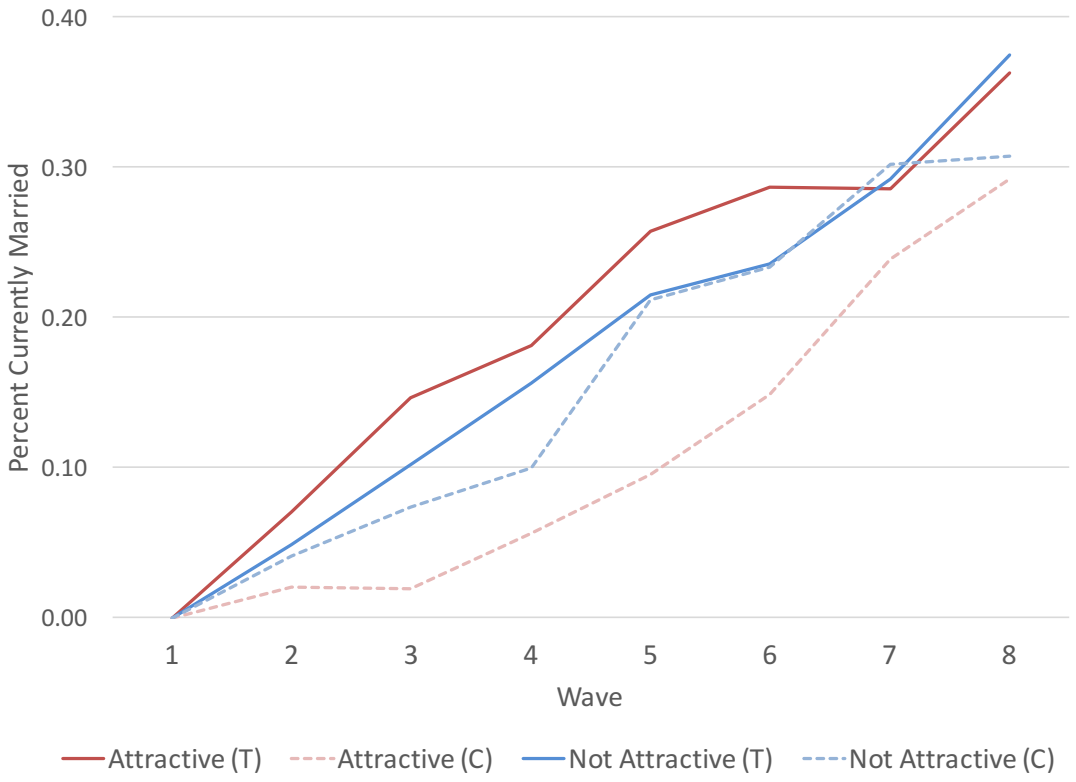


Figure 6: Marriage for Safe Baseline-Unmarried Respondents, by Attractiveness

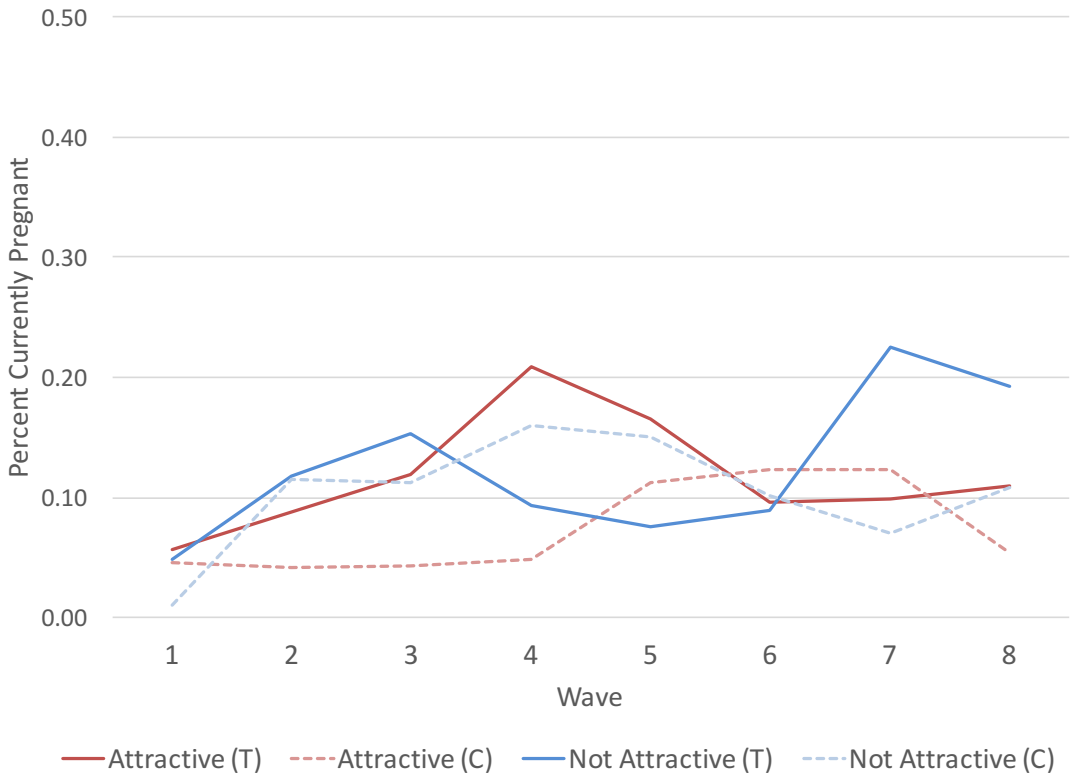


Figure 7: Pregnancy for Safe Baseline-Unmarried Respondents, by Attractiveness

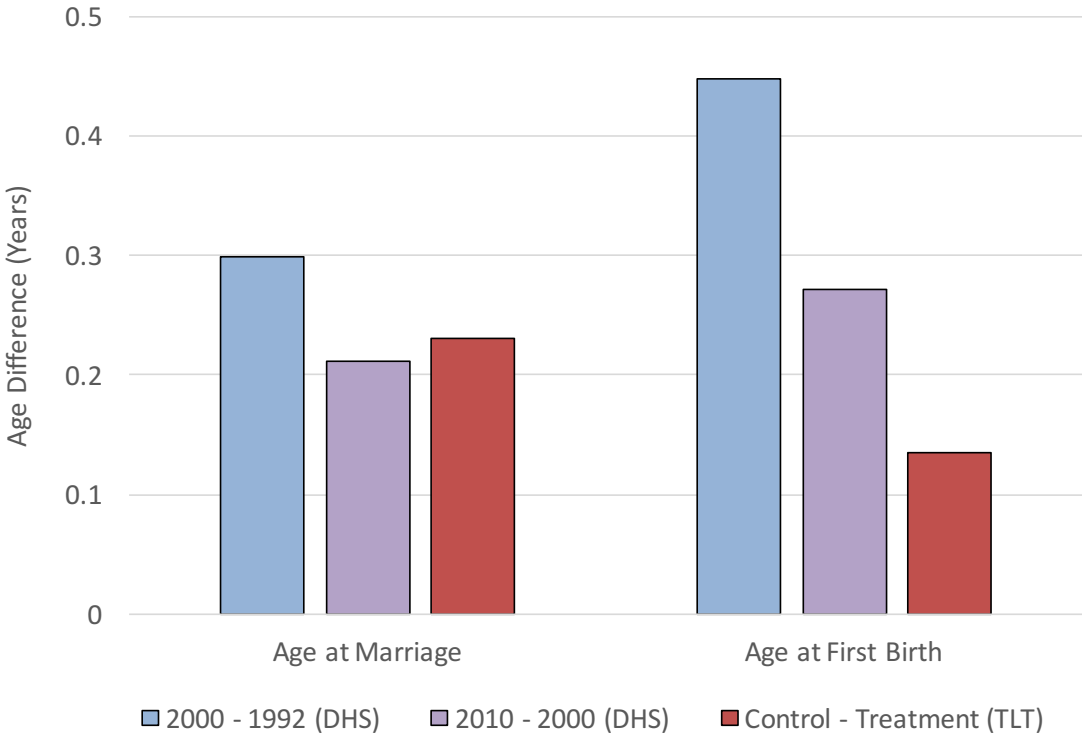


Figure 8: A Comparison of HIV Testing Impacts to National Marriage and Fertility Patterns

Online Appendix – Not for Publication

A The Model Under Alternative Assumptions

In this appendix, we explore alternative theoretical assumptions regarding the marital surplus ranking assumption, divorce, and the correlation between attractiveness and safety.

A.1 The Relative Value of Traits

Under Equation (1), attractiveness and safety make equivalent contributions to marital surplus. Alternatively, people may value one trait more than the other. If safety contributes more to marital surplus than attractiveness, an *ab* woman has the following surplus ranking (with an analogous expression for men):

$$S_{hh}^{ab} \;>\; S_{lh}^{ab} \;>\; S_{hl}^{ab} \;>\; S_{ll}^{ab} \;>\; 0. \tag{11}$$

Conversely, if attractiveness contributes more to marital surplus than safety, the surplus ranking is:

$$S_{hh}^{ab} \;>\; S_{hl}^{ab} \;>\; S_{hl}^{ab} \;>\; S_{ll}^{ab} \;>\; 0 \tag{12}$$

In contrast to our main model, both of these alternatives induce positive assortative matching on both traits, rather than the number of high traits. Working backward, people who marry in Period 2 match on both attractiveness and safety, while people who marry in Period 1 match on attractiveness. This prediction does not require a correlation between the traits.[30] We cannot distinguish between these alternative surplus ranking assumptions empirically because comprehensive HIV status information for partners is not available.

Since these alternatives preserve the matching process, marriage timing is also consistent with our main model. Unsafe people marry in Period 1 according to Equation (3) while safe people who are sufficiently patient marry in Period 2 according to Equation (5).

A.2 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

[30] Under Inequality (11), an unattractive partner may provide the most expected surplus in Period 1 if the traits are sufficiently negatively correlated. In this case, all participants prefer unattractive partners in Period 1. Unattractive people reject offers from attractive partners, so that people continue to match on attractiveness. This issue does not arise under Inequality (12) because attractive partners provide the most expected surplus, regardless of the correlation between the traits.

divorce removes the incentive to delay marriage. All participants marry partners of the same attractiveness in Period 1. At the beginning of Period 2, people who are mismatched on safety divorce and marry new partners with the same number of high traits. 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 and decreases marital surplus for unsafe people in Period 1 by allowing safe people to marry partners with the same number of high traits.

A small but positive fixed cost of divorce leads to predictions that are weaker but qualitatively similar to our main model. In this scenario, some safe people who delay in our main model marry early. This change attenuates the impact of an intervention that removes asymmetric information.

A.3 Own Safety is Unobservable

In contrast to our assumption that safety information is asymmetric, individuals may be unaware of their own safety type in Period 1. In this case, people assortatively match on the number of high traits in Period 2 and on attractiveness in Period 1. Unlike under asymmetric information, unsafe people who are patient delay marriage in this scenario. Therefore an intervention that makes safety observable in Period 1 accelerates marriage for both safe and unsafe people. This impact is the same for both groups unless other parameters differ by safety type. The intervention increases marital surplus for safe people by accelerating marriage without requiring them to match with unsafe people. It has ambiguous effect on the surplus of unsafe people since it accelerates marriage but increases matching with unsafe partners. Under some conditions, attractive people have stronger effects, as in our main model.

A.4 Negative Marital Surplus

In contrast to our assumption in Equation (1), marital surplus could be negative. One specific alternative is that only the ll surplus is negative, so that $S^{ab}_{hh} > S^{ab}_{lh} = S^{ab}_{hl} > 0 > S^{ab}_{ll}$. People with two low traits may or may not marry in this scenario. Working backward, ll people do not marry in Period 2 because matching with an ll partner yields negative surplus. However they may marry in Period 1 if expected ll surplus is positive. In this case, patient lh people delay marriage to avoid a safety mismatch.[31] Removing asymmetric information accelerates marriage for lh people and forces ll people out of the market.

[31] ll people do not marry if expected ll surplus is negative. The non-participation of ll people removes the risk of a mismatch for lh people. In one equilibrium, lh people now marry lh partners in Period 1, eliminating any effect of the intervention on lh or ll people. In another equilibrium, some lh people continue to delay marriage due to the threat of participation by ll people in Period 1. Here the intervention accelerates marriage and increases surplus for lh people.

A.5 Dependence Between Attractiveness and Patience

Section 2 assumes that patience and attractiveness are independent. A more realistic assumption is that attractiveness and patience are positively correlated since both variables are related to socioeconomic status. This assumption does not change the equilibrium outcomes or predictions of the model. In fact, a positive correlation between attractiveness and patience accentuates the difference between attractive and unattractive people in marriage timing since both attractiveness and patience encourage safe people to delay. This relationship may confound interactions between attractiveness and treatment empirically. We address this issue in Section 7.1 by controlling for the interaction of T with observable time preference variables.

A.6 Dependence Between Safety and Patience

While Section 2 assumes that patience and safety are independent, safe people may be relatively patient. This positive correlation would move the model towards a separating equilibrium in which all safe people wait to marry. In our empirical analysis in Section 7.1, we deal with this possible correlation by controlling for the interaction of T with observable time preference variables.

A.7 Dependence Between Traits and Gender

We assume that both genders have the same attractiveness and safety distributions in Section 2. This assumption ensures that a partner with the same number of high traits is available for everyone under full information. The alternative assumption that safety is imbalanced by gender does not alter our main predictions. If there are more safe men than safe women, people continue to match on the number of high traits in Period 2 and on attractiveness in Period 1. People who are safe and sufficiently patient delay marriage, as before. However some women match with men who have more high traits (if marrying in Period 2) or who are more attractive (if marrying in Period 1). Secondly, some unsafe women marry in Period 2, unlike in our main model. With fewer unsafe men around, these women cannot find partners in Period 1. A similar outcome occurs if either attractiveness or impatience is imbalanced by gender. In these scenarios, removing asymmetric information accelerates marriage but has an ambiguous effect on marital surplus for unsafe women.

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:

$$\frac{S_{ah}^{ah} - S_{al}^{ah}}{\frac{\mu_{ah}^*}{p_{al}} + 1} \geq 0$$

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 $\frac{\mu_{hh}}{p_{hh}}^* < \frac{\mu_{lh}}{p_{lh}}^*$, which are the same conditions 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

$$\frac{S_{al}^{al} - S_{ah}^{al}}{\frac{p_{al}}{\mu_{ah}^*} + 1} \leq 0$$

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.

The equilibria of this game depend on the relative magnitudes of β, γ, and μ. We

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

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 The Correlation Between Marriage and Pregnancy Effects

The interpretation of pregnancy as a proxy for marital surplus presupposes a causal relationship between marriage and fertility. A reduced-form analysis cannot identify the causal relationship between two endogenous outcomes. However a positive correlation between the treatment effects on these outcomes is a necessary condition for the outcomes to share a causal pathway. In this appendix, we use seemingly unrelated regression to estimate jointly the impacts on marriage and pregnancy in order to obtain the correlation between the treatment effects on these outcomes. We focus on the baseline-unmarried and safe sample in order to reproduce the estimates in Panel A of Table 3. The correlation between the effects on marriage pregnancy for this sample is 0.45. This pattern indicates that people who respond to the intervention by marrying also respond by becoming pregnant. The strong positive correlation is consistent with a causal pathway in which the intervention leads to marriage, which in turn leads to pregnancy. However, it does not eliminate the alternative possibility that other factors may jointly contribute to the impacts on both outcomes.[32]

This exercise also allows us to explore how the impacts on marriage and pregnancy

[32] As an alternative approach, we estimate the impacts on [pregnant · married] and [pregnant · unmarried]. If the treatment effects on marriage and pregnancy are related, the impact for the first outcome should be larger than for the second outcome. Estimates (available from the authors) show a large and statistically significant effect on [pregnant · married] and a small and statistically insignificant effect on [pregnant · unmarried]. These results are consistent with the SUR estimates but are more difficult to interpret.

are correlated temporally. We reproduce the SUR regressions above using a pregnancy of lead or lag of up to two periods. This approach requires us to drop Waves 2 and 8 for consistency across comparisons. If the treatment causes participants to first get married and then become pregnant, the correlation between the marriage and pregnancy treatment effects should be largest for lagged pregnancy. Figure A1 plots the correlation coefficient between the impact on marriage at Wave t and pregnancy at different points from Wave $t-2$ to $t+2$. The correlation is substantially stronger for post-marital pregnancy, with the largest correlation occurring between marriage at Wave t and pregnancy at Wave $t+2$. This pattern is consistent with a causal pathway from marriage to pregnancy.

E Observable Aspects of Risky Behavior

In Section 5.2, we identify "safe" respondents based on the absence of several baseline risky behaviors. To qualify as safe, a respondent must: (1) have ≤ 2 lifetime partners, (2) have ≤ 1 partners in the past year, (3) 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 receives HIV tests at baseline, HIV prevalence is 62 percent lower among respondents who exhibit no risky behaviors.

Figure A3 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 3. 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 A2 shows the unweighted age distributions of the treatment and control arms. Treatment respondents are an average of 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 reweights the control respondents so that the age distributions in the treatment and control arms are equivalent. Intuitively, it places less weight on the responses of young control respondents. Reweighting by age is not equivalent to including age as a covariate if the treatment effect varies by age. In practice, however, reweighting by age and controlling for age yield similar estimates in our regressions.

The paper incorporates age-unweighted baseline summary statistics in Table 1 and over-

all impacts in Table 2.[33] To supplement these results, Table A1 provides age-unweighted estimates by safety (following Table 3) and Table A2 provides age-unweighted estimates by attractiveness (following Table 4). 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.

G 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 A3 provides baseline summary statistics by attrition status. Non-attriters in Column 1 have completed all eight survey waves while attriters in Column 2 have completed fewer than eight waves. Attriters appear to have higher socioeconomic status than non-attriters. They are less likely to be married and more likely to be enrolled in school. Since HIV status is 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 complete all eight waves, compared to 70 percent of control respondents ($p = 0.35$).

Table A4 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.

H Baseline Covariates by Safety and Attractiveness

Section 7.1 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 A5 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 A6 cuts the safe sample (according to both definitions) by attractiveness. These samples

[33] Age-unweighted versions of Tables A5 and A6, which show baseline summary statistics by safety and attractivenes, are available from the authors.

correspond to the estimation samples in Panels A and B of Table 4. 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 3 and 4 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.

I Summary Statistics for the Single Testing Intervention

Section 8 finds no effects of a single-test intervention of marriage and fertility. This appendix offers additional background for this result. Table A7 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 complete all of Waves 4-8, compared to 79 percent of control respondents ($p = 0.37$).

Table A1: Age-Unweighted Impacts on Marriage and Fertility, 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 A2: Age-Unweighted Impacts on Marriage and Fertility, 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 $\leq$ 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 A3: Baseline Characteristics by Attrition Status

	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			
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 A4: 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: 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: Estimates by Attractiveness		
Treatment · Attractive	0.11**	0.058**
	(0.048)	(0.023)
	[0.10]	[0.07]
Treatment· Not Attractive	0.037	0.031
	(0.055)	(0.027)
	[0.18]	[0.11]
Equality of coefficients (p-value)	0.29	0.43
Observations	1902	1902

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 (10). 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 4. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: 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						
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 A6: 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						
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 A7: Wave-4 Characteristics by Treatment Status for One-Shot Testing

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

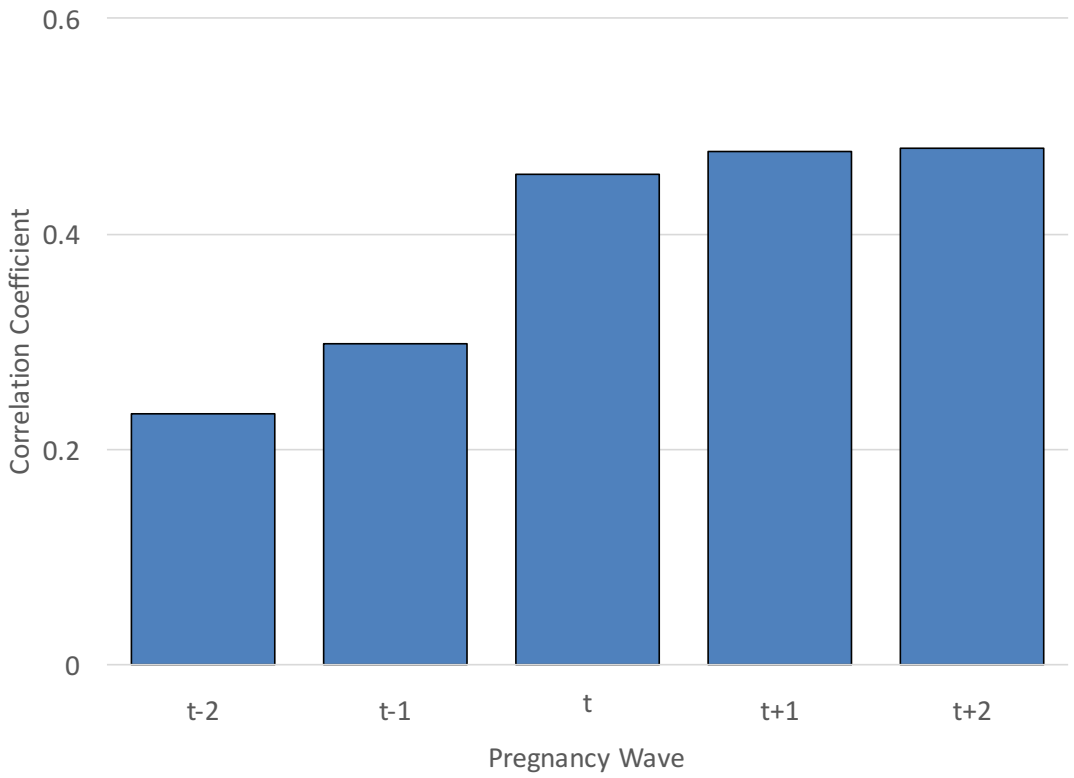


Figure A1: The Correlation Between the Treatment Effect on Marriage in Wave t and the Treatment Effect on Pregnancy in Waves $t-2$ to $t+2$

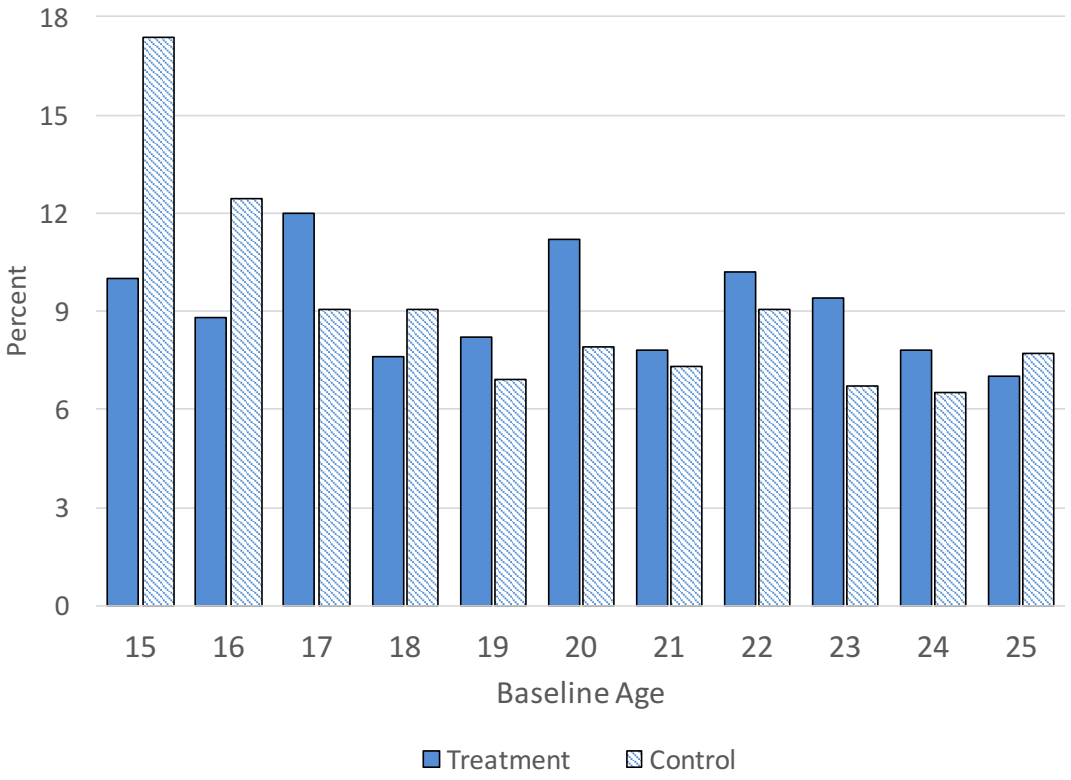


Figure A2: Age Distributions for the Treatment and Control Groups

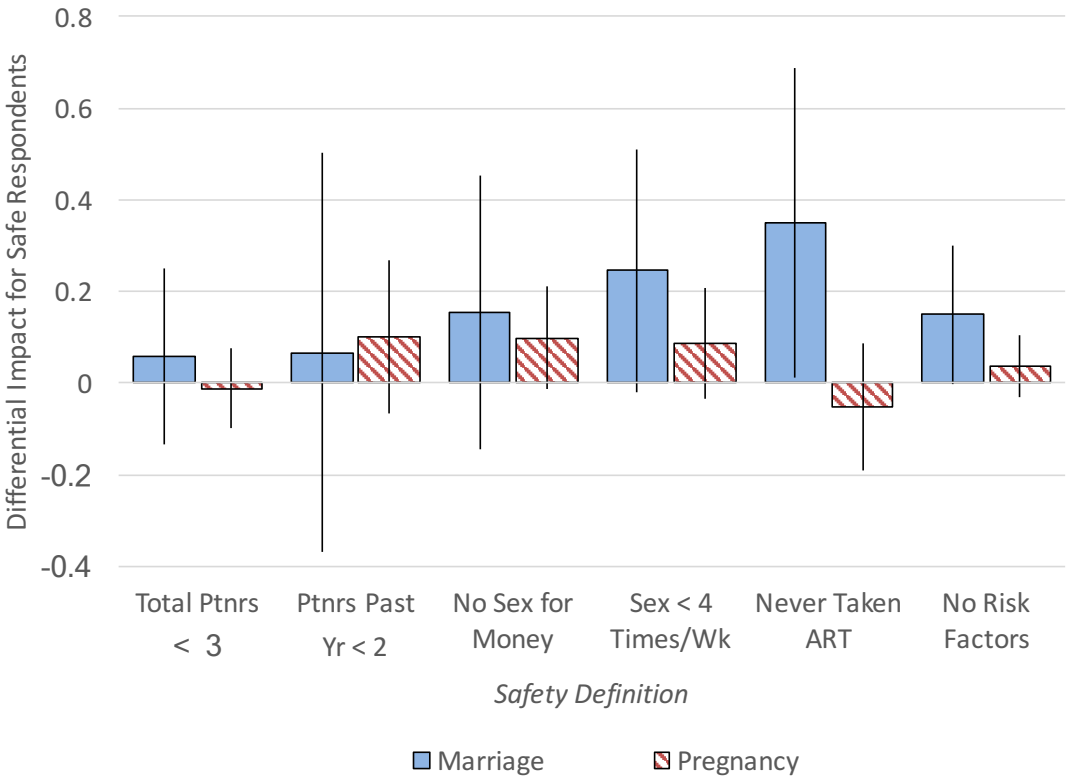


Figure A3: Differential Impacts for "Safe" Respondents According to Five Component Risky Behaviors (With 90% Confidence Intervals)

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