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

Manuela Angelucci*
Daniel Bennett†

February 3, 2017

Abstract

Asymmetric information in the marriage market may delay marriage and cause adverse selection if partner quality is revealed over time. HIV risk is an important but hidden partner attribute in many parts of Sub-Saharan Africa (SSA), however frequent HIV testing may enable low-risk people to signal and screen. This paper shows that a high-frequency, "opt-out" HIV testing intervention accelerates marriage and fertility, increasing probabilities of marriage and pregnancy by 26 and 27 percent respectively for baseline-unmarried women over 28 months. We test the predictions of a model of positive assortative matching with both observable (attractiveness) and hidden (HIV risk) attributes. As predicted, estimates are larger for low-risk and attractive respondents. We also show that, consistent with other HIV testing evaluations in the literature, a single-test intervention lacks these effects. Our findings suggest that an endogenous response to HIV risk may largely explain why the HIV/AIDS epidemic has coincided with systematic marriage and fertility delays.

JEL: J12, J13, I15, I18

We received helpful feedback from V. Bhaskar, Dan Black, Abdul Chilungo, Costas Meghir, Adam Szeidl, Caroline Thomas, Jenny Trinitapoli, Alessandra Voena, and Sara Yeatman. Hannah Furnas, Ashley Larsen Gibby, and Ngoc Nguyen provided excellent research assistance. This research uses data from Tsogolo la Thanzi, a research project designed by Jenny Trinitapoli and Sara Yeatman, and funded by grants R01-HD058366 and R01-HD077873 from the National Institute of Child Health and Human Development. Persons interested in obtaining data files TLT should contact Tsogolo la Thanzi, University of Chicago Sociology Department, 1126 E. 59th St., Chicago, IL 60637.

*Department of Economics, University of Michigan, mangeluc@umich.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 from their partners, 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 evaluates a high-frequency HIV testing intervention and argues that asymmetric information about HIV risk has caused adverse selection in the marriage market in Malawi. This analysis, which is the first empirical examination of adverse selection in the marriage market, may help explain recent marriage and fertility patterns in Malawi and elsewhere in Sub-Saharan Africa (SSA).

HIV risk is an important but hidden partner attribute in HIV-endemic settings, including Malawi, where HIV prevalence was 10.6 percent in 2010. Marriage market participants may worry about both the current HIV status and future HIV infection risk of potential spouses. An HIV-positive spouse is less productive and requires extra medical care. The spouse may also transmit HIV, particularly given norms that discourage condom use within marriage (Smith and Watkins 2005, Chimbiri 2007). However, HIV infection remains asymptomatic for several years. An individual's HIV status and propensity for risky sexual behavior are difficult for other marriage market participants to observe.

The existence of HIV creates an incentive to delay marriage by increasing the cost of marrying a partner whose type is unknown. Consistent with this hypothesis, Bongaarts (2007) finds a positive cross sectional correlation between age at marriage and HIV prevalence in 33 SSA countries. Figure 1Figure 0: Marriage and Fertility for Women Aged 17-27figure.caption.13 shows that this correlation holds also longitudinally in Malawi. 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 an acceleration of marriage of around 0.15 years. This pattern does not align with a trend reversal in the supply of contraceptives or female education. The figure also shows a positive correlation between HIV prevalence and the age at first birth.

Testing frequently and observing whether others also test may enable low-risk people to signal and screen. Nevertheless, people do not test for HIV frequently. In our data from southern Malawi in 2009, only 14 percent of young childless women have tested in the past four months. Testing remains inconvenient and stigmatized (Chesney and Smith 1999, Ngatia 2011, Young and Zhu 2012). People must travel and queue in public for several hours to be tested (Pinto et al. 2013). Since testing is costly, the failure to test sends no negative signal to potential partners, as it would if testing were easier (Kalichman and Simbayi 2003).

We begin by deriving predictions from a simple two-period model with positive assortative matching. We assume that attractiveness and health are fixed traits and that attractiveness is always observable but health is revealed in Period 2. As a result, all unhealthy people marry early while some healthy people wait to avoid a mismatch. Under conditions that we discuss in Section 2Theorysection.2, attractive people have the strongest incentive among the healthy to delay marriage. An intervention that reveals health in Period 1 removes the incentive to delay, accelerating marriage for healthy people and increasing their marital surplus. It has no impact on marriage timing and decreases marital surplus for unhealthy people.

We test these predictions within the context of an intensive HIV testing intervention. The Tsogolo La Thanzi (TLT) Panel Study follows 1505 young women in Balaka, Malawi over eight waves spanning 28 months. Surveyors offer a free HIV test after every survey wave to a randomly-assigned treatment group. They also encourage participants to invite their partners into the study under the same intervention arm, enabling treatment respondents to both signal and screen. By using an "opt-out" model in which the provider initiates testing, the intervention dramatically reduces the inconvenience and stigma of HIV testing.

We show that high-frequency HIV testing increases the likelihood of marriage and pregnancy. Within the study period, the intervention increases the probabilities of marriage and pregnancy by 6 and 16 percent respectively in the full sample. The marriage and pregnancy effects are 26 and 27 percent respectively among baseline-unmarried respondents. Consistent with our model, effects are even larger for low-risk and attractive respondents. Treatment effects are 30-100 percent as large as the temporal changes in marriage and fertility in Malawi in recent decades. We support the argument that HIV testing alleviates asymmetric information by showing that this intervention, which doubles the HIV testing frequency for women and their partners, has a minimal effect on own HIV status beliefs but makes beliefs about partner HIV status more precise.

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).[1] Unlike these studies, which offer testing once, the TLT study repeatedly offered tests to participants and their partners over 28 months. In our study, another experimental arm received a single HIV test offer midway through the study period. A comparison of this group to the control group shows that the single HIV test offer has no statistical or economic effects on marriage or fertility. This pattern suggests that testing must be regularly available to enable signaling and screening.

This paper provides the first examination of asymmetric information in the marriage market using experimental variation. Most studies in this field rely on testing equilibrium predictions using correlational evidence (e.g. Chiappori et al. 2012, Hitsch et al. 2010). Becker (1981) conjectures that the inability to observe some partner traits may lead people to place more emphasis on observable traits and contribute to divorce. We build upon this analysis by considering the effect of unobservable partner quality on marriage timing. Since people face asymmetric information about a variety of partner attributes, our conclusions may also apply to other settings.

We also contribute to a discussion of the causes and consequences of the HIV/AIDS

[1] 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, fertility.

epidemic in SSA. Researchers have attributed higher HIV prevalence in SSA to delayed marriages (Bongaarts 2007, Magruder 2011). We provide the complementary explanation that increases in HIV prevalence or in asymmetric information delay marriage and fertility. This channel can explain the recent trend reversal in marriage and fertility in Malawi.

Recent increases in the supply of opt-out HIV testing in SSA have increased testing utilization (Kennedy et al. 2013). Technological changes, such as the development of in-home test kits, promise to make HIV testing more private and convenient (Low et al. 2013). Our findings suggest that these changes may cause further reductions in age at first marriage and birth. While we have modeled early marriage and fertility as privately beneficial, these choices may be socially costly if they limit the human capital accumulation of women and children.

2 Theory

2.1 Setup

Consider a setting with non-transferable utility and a continuum of people who live for two periods, $t \in \{1, 2\}$. People have two fixed binary traits, attractiveness and health, which may be either high or low (h or l). Therefore, there are four types of people, defined by their attractiveness and health, with population shares p_{hh}, p_{lh}, p_{hl}, and p_{ll}, which sum to one and are common knowledge. Attractiveness, which represents any visible partner attribute such as physical appearance or education, is observable in both periods. Health 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, health, and the discount factor are independent of gender and that attractiveness and health are independent of the discount factor. However the model allows for a positive or negative correlation between attractiveness and health.

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 health b marries a man with attractiveness c and health d, they each receive a surplus $S^{ab}_{cd} > 0$. Surplus increases with each partner's attractiveness and health. We assume for tractability that a partner with two high traits yields the most surplus, followed by one high trait, followed by zero high traits.[2] The following expression shows the surplus ranking for a woman of attractiveness a and health b. There is an equivalent expression for men.

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

Since marital surplus is strictly positive, everyone prefers to marry eventually rather than remain single. Participants decide when to make and accept marriage offers in order to maximize expected surplus. With identical information sets and preferences, men of the same type make the same offers, which women of the same type (eventually) accept. 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 Health is Observable in Period 1

In the benchmark case, both health 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.[3] Every

[2]In general, attractiveness and health may make different contributions to surplus. People may prefer a healthy but unattractive partner over an attractive but unhealthy one. Marital surplus could also be negative, which would lead people to remain single. We discuss these cases and other alternative assumptions in Appendix AThe Model Under Alternative Assumptionsappendix.A.

[3]All men initially propose to women with two high traits, who eventually accept the offers from men with two high traits. The remaining men make offers to women with one high trait, who eventually accept the offers from men with one high trait. The remaining men, who have zero high traits, then make offers to the remaining women, who eventually accept. A stable positive assortative assignment exists regardless of which gender makes the offers since the trait distribution does not differ by gender.

man makes an offer and every woman accepts 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.

2.3 Equilibrium if Health is Unobservable in Period 1

Next we consider the case in which health is private in Period 1. People are aware of their own health and the distributions of health and the discount factor in the population but do not observe the health of others. In this situation, asymmetric information causes some healthy people to delay marriage, which leads to adverse selection in Period 1. We work backward from Period 2, when health is observable. Since the distribution of traits is identical by gender, an equal number of men and women (with the same distributions of traits) decide to postpone marriage until Period 2. As in Section 2.2Equilibrium if Health is Observable in Period 1subsection.2.2, a stable assignment exists that is positively assortative in the number of high traits.

In Period 1, people maximize expected surplus to decide whether and whom to marry. Attractive partners have more high traits than unattractive partners in expectation.[4] Among people who marry in Period 1, the Gale and Shapley (1962) algorithm leads to a stable assignment that is positively assortative in attractiveness. Since health is unobservable, some healthy people mismatch and marry unhealthy partners.

Next we consider the marriage timing decision. People marry in Period 1 rather than Period 2 if this choice leads to more expected surplus. Equation (2Equilibrium if Health is Unobservable in Pe shows that marrying in Period 1 is always advantageous for unhealthy people.

$$(1+\delta_i)\frac{p_{al}S^{al}_{al}+p_{ah}S^{al}_{ah}}{p_{al}+p_{ah}} > \delta_i S^{al}_{al} \tag{2}$$

In this expression, p_{al} and p_{ah} are the population proportions of unhealthy and healthy types

[4]Attractiveness and health may be negatively correlated but not *perfectly* negatively correlated. In that case, every attractive person is unhealthy, which means that partners effectively only vary in one dimension.

with attractiveness a. The inequality holds because $\delta_i > 0$ and $S^{al}_{ah} > S^{al}_{al}$. Unhealthy people benefit from early marriage in two ways. Marrying early leads to more time in marriage, which is beneficial by definition. Secondly, marrying early might allow them to obtain additional surplus by marrying a healthy spouse. Since the inequality does not depend on an individual's attractiveness, all unhealthy people marry early regardless of attractiveness.

Unlike unhealthy people, who strictly prefer to marry early, healthy people trade off the benefit of Period 1 marital surplus against the risk of mismatching with an unhealthy person. Expression (3Equilibrium if Health is Unobservable in Period 1equation.2.3) shows that a healthy person of attractiveness a marries in Period 1 if doing so maximizes her expected surplus.

$$(1 + \delta_{ia})\frac{p_{al}S^{ah}_{al} + \mu_{ah}S^{ah}_{ah}}{(p_{al} + \mu_{ah})} > \delta_{ia}S^{ah}_{ah}. \tag{3}$$

The parameter $\mu_{ah} \in [0, p_{ah}]$ is the population proportion of healthy people of attractiveness a who marry in Period 1. The expression shows that healthy people who are sufficiently patient delay marriage. Solving for δ_{ia} yields an expression for $\bar{\delta}_a$, the threshold value for δ. Healthy people for whom $\delta_i < \bar{\delta}_a$ marry early.

$$\bar{\delta}_a = \frac{\mu_{ah}S^{ah}_{ah} + p_{al}S^{ah}_{al}}{p_{al}(S^{ah}_{ah} - S^{ah}_{al})} > 0 \tag{4}$$

$\bar{\delta}_a$ is always positive because both its numerator and its denominator are positive. It is a function of μ_{ah}, the share of other healthy people of attractiveness a who marry early.

Under the uniform distribution of δ, $\mu_{ah} = F(\bar{\delta}) = \bar{\delta}_a/b$ so that $\bar{\delta}_a = \mu_{ah}b$. We equate this expression to $\bar{\delta}_a$ in Equation (4Equilibrium if Health is Unobservable in Period 1equation.2.4) to solve for μ^*_{ah}.

$$\mu^*_{ah} = \frac{1}{b(r_{ah} - 1) - \frac{r_{ah}}{p_{al}}}, \tag{5}$$

In this expression, r_{ah} is the ratio of the marital surplus from a healthy spouse to surplus from an unhealthy spouse for an ah woman, $r_{ah} = \frac{S^{ah}_{ah}}{S^{ah}_{al}}$, which is greater than 1 by assumption.[5] Equation (5Equilibrium if Health is Unobservable in Period 1equation.2.5) establishes that a stable assignment exists in which all unhealthy people and healthy people of attractiveness a with a discount factor $\delta_i < \bar{\delta}_a$ marry in Period 1. Equations (2Equilibrium if Health is Unobservable in Peri and (3Equilibrium if Health is Unobservable in Period 1equation.2.3) (evaluated at μ^*_{ah} and $\bar{\delta}_a$) demonstrate that no player has an incentive to deviate in this scenario.

Values of $\mu^*_{ah} < 1$ in Equation (5Equilibrium if Health is Unobservable in Period 1equation.2.5) connote adverse selection in Period 1, and partial derivatives of this expression identify the factors that contribute to adverse selection. $\frac{\partial \mu^*_{ah}}{\partial p_{al}} < 0$, which indicates that the percent of healthy people who marry early decreases in the unhealthy proportion. The HIV/AIDS epidemic may have fostered delays in marriage through this mechanism. Secondly, r_{ah} reflects the degree to which the spouse's health matters for surplus. $\frac{\partial \mu^*_{ah}}{\partial r_{ah}} < 0$, which means that adverse selection increases as the utility consequence of marrying an unhealthy spouse becomes more severe.

2.4 The Role of Attractiveness

The final part of our analysis examines how attractiveness, which is observable, may influence the extent of adverse selection. Among the healthy, attractive people delay marriage more than unattractive people if $\mu^*_{hh} < \mu^*_{lh}$, which is equivalent to the following expression.

$$r_{hh}(b - \frac{1}{p_{hl}}) \ > \ r_{lh}(b - \frac{1}{p_{ll}}) \tag{6}$$

Either of two sufficient conditions may satisfy this inequality. First, the inequality holds if $r_{hh} > r_{lh}$ and $p_{hl} = p_{ll}$. In this case, the relative gain from marrying a healthy partner is larger for attractive people than for unattractive people. This assumption is reasonable in

[5] A sufficiently large value of b ensures that $mu^*_{ah} > 0$. b may be large if Period 2 is much longer than Period 1, which is reasonable since people expect to live for many years after marriage.

the context of HIV, which reduces income further for a wealthy spouse than a poor spouse. Secondly, the inequality holds if attractive people are more likely to be unhealthy, so that $p_{hl} > p_{ll}$ and $r_{hh} = r_{lh}$.[6] In this case, attractive people are more likely to be "lemons," which strengthens the incentive for healthy and attractive people to delay.

2.5 Treatment Effects on Marriage and Marital Surplus in Theory

This subsection relates the model to our empirical analysis below by considering the impact of an intervention that makes health observable in Period 1. We consider the effects on marriage and marital surplus during Period 1 since our data cover a short interval of early adulthood.

The intervention increases the marriage probability of healthy people by $1 - \frac{\mu^*_{ah}}{p_{ah}} \geq 0$. Under the intervention, all healthy people marry early and match with healthy partners. Conversely, the intervention does not alter the marriage probability of unhealthy people, who marry early regardless of whether health is observable.

The intervention also increases Period 1 marital surplus of healthy people by $S^{ah}_{ah} - (\frac{\mu^*_{ah}}{p_{ah}})\frac{(\mu^*_{ah}S^{ah}_{ah}+p_{al}S^{ah}_{al})}{(p_{al}+\mu^*_{ah})} \geq 0$. This increase in surplus is generally higher for attractive people ($a = h$) because the difference $S^{ah}_{ah} - S^{ah}_{al}$ grows with attractiveness when $r_{hh} > r_{lh}$.[7] Conversely, the intervention decreases expected marital surplus of unhealthy people by $S^{al}_{al} - \frac{(\mu^*_{ah}S^{al}_{ah}+p_{al}S^{al}_{al})}{p_{al}+\mu^*_{ah}} \leq 0$.

3 HIV Testing

The HIV/AIDS epidemic emerged in Malawi around 1985 and HIV prevalence peaked at 14 percent in 1998. Since then, HIV prevalence has gradually declined to 10.6 percent in 2010 (UNAIDS 2014). In the past decade, an international initiative led by the Global Fund

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

[7] Moreover, the more marked the adverse selection of attractive people, i.e., $\mu^*_{ah} < \mu^*_{ah}$, the bigger the relative surplus gain for attractive people, as a bigger μ^* makes the second term of the expression larger.

to Fight AIDS, TB, and Malaria has helped to provide free HIV services, including testing and antiretroviral treatment at public health clinics throughout the country. Despite this improvement, 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 is primarily transmitted through unprotected heterosexual sex. The infection remains asymptomatic for several years before AIDS symptoms develop and health declines rapidly. Marital surplus is a function of the partner's current HIV status and future infection risk, which depends upon the partner's propensity for risky behavior (Smith and Watkins 2005). An HIV-positive partner may transmit HIV within the marriage, particularly since post-marital condom use is frowned upon (Chimbiri 2007, Tavory and Swindler 2009). He or she is also substantially less productive and requires extra medical care in expectation (Oni et al. 2002). Both current HIV status and the propensity for risky behavior are difficult for marriage market participants to observe, and may be discovered over time. Without a credible signal of partner quality, a low-risk person faces the chance of marrying a high-risk partner. He or she may prefer to delay marriage until partner HIV risk attributes become visible.

The provision of HIV testing may enable signaling and screening in the marriage market. A low-risk person can signal her type by testing frequently and can screen potential partners by their willingness to test. Although test results are confidential, seeking a test is observable and misrepresenting test results can be costly in the context of a relationship. Moreover, someone who tests positive normally discontinues testing and begins antiretroviral treatment, both of which are observable in a small community. However, seeking an HIV test could also send an unfavorable marriage market signal by implying that the test seeker has engaged in risky behavior and is worried about her HIV status. The relative strength of these two mechanisms hinges upon the cost of HIV testing. If testing is costly, refusing to be tested may not connote high risk. The stigma of seeking an HIV test increases with the cost of testing since observers may reason that only someone who has been very risky would seek such an expensive HIV test. Conversely, if testing is cheap, *not* being tested may send a negative signal.

HIV testing remains costly in many places. Providers typically follow at "opt-in" model, in which the patient initiates the test. A "rapid" HIV test yields results in about 30 minutes. A typical test seeker in rural Malawi must ravel several kilometers on foot or bicycle over unimproved roads and queue 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. Research also documents the importance of stigma as a barrier to HIV testing (Sambisa et al. 2010, Berendes and Rimal 2011, Ngatia 2011, Maughan-Brown and Nyblade 2014).[8] In our sample, only 14 percent of childless women have been tested in the past four months, and only 34 percent have ever been tested.

Recent policy and technological changes have reduced the cost of HIV testing. Following WHO guidelines, several countries (including Malawi) plan to introduce provider-initiated (i.e., "opt-out") HIV testing and counseling (Kennedy et al. 2013). Under this model, providers routinely test during many health care visits, which dramatically reduces stigma by removing the implication that the person being tested has been risky. Antenatal clinics in Malawi provide opt-out testing, and 89 percent of the mothers in our sample report that they have been tested. The dissemination of home-based HIV testing kits also promises to increase the privacy and convenience of HIV testing, which may also facilitate the use of HIV testing in the marriage market.

[8]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 BMultiple Equilibria and the Demand for HIV Testingappendix.B shows that in a "strategic complements" framework, a small reduction in the cost of testing may lead to a large increase in adoption.

4 Intervention

We evaluate an HIV testing intervention that is embedded in the Tsogolo la Thansi (TLT) Panel Study, which 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).[9] 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).

At the end of each wave, surveyors offered HIV tests to all participants in the randomly-selected treatment arm ($n = 500$), while the offered HIV tests only after Wave 8 to participants in the control arm ($n = 507$). They offered HIV tests to a third "single-test" arm ($n = 500$) after Waves 4 and 8. Our primary analysis compares the treatment and control arms. However, Section 7The Effect of a One-Shot HIV Testing Interventionsection.7 uses the single-test arm to examine the impact of offering an HIV test only once. Participants in the treatment arm make up 1.5 percent of the 32,300 women aged 15-25 in Balaka, minimizing the size of any general equilibrium effects of the intervention. By incorporating testing into the survey and following an opt-out model, surveyors dramatically reduced the stigma and inconvenience of being tested. To safeguard confidentiality, surveyors followed the standard Voluntary Counseling and Testing (VCT) protocol and provided test results verbally in private without giving written documentation. However peers could infer from the duration of the interview whether a respondent received an HIV test.

Surveyors encouraged participants to invite their partners into the study. Partners completed a similar questionnaire and received the same compensation as other study par-

[9]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).

ticipants. Surveyors also enrolled partners into the same intervention arm as the referring respondent. This design feature enabled treatment respondents to screen partners according to the willingness to participate and submit to 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.[10] 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.[11] 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 net benefit of a child is function of each spouse's utility (Becker 1973, Edwards and Roff 2016).[12]

Beauty is an indicator of observable female attractiveness. Several studies validate the male preference for physically attractive partners (Fisman et al. 2006, Hitsch et al. 2010, Chiappori et al. 2012), including in Malawi (Poulin 2007). Surveyors assessed the physical beauty of respondents at baseline on a four-point Likert scale.[13] Figure 2Figure 0: The Distribu-

[10]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.

[11]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 0.52 overall and 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).

[12]Marriage does not necessarily precede pregnancy, however married respondents are 65 percentage points more likely than unmarried respondents to have children at baseline. Appendix CThe Correlation Between Marriage and Pregnancy Effectsappendix.C shows that the treatment effects on marriage and pregnancy are strongly correlated, and that an two-wave pregnancy lag maximizes this relationship.

[13]Oreficce and Quintana-Domeque (2014) weigh the merits of this measurement approach. Since surveyors

tion of Baseline Attractiveness (with 83% confidence intervals)figure.caption.14 shows the baseline frequency distribution of this variable.[14] Angelucci and Bennett (2017) shows that among respondents who are married at baseline, attractiveness is positively correlated with partner education and parity conditional on marriage duration. This pattern suggests that women assortatively match on attractiveness and that attractive women receive more marital surplus.

5.2 HIV Status, HIV Risk, and Subjective Perceptions

HIV status and the propensity for risky behavior are distinct but related marriage market attributes. Since HIV is incurable, current non-infection is necessary but not sufficient for future non-infection. The HIV testing intervention directly provides information about HIV status and indirectly signals HIV risk according to the willingness to be tested and share results. As we describe above, surveyors offered participants up to eight HIV tests, depending on the intervention arm. They administered the Trinity Unigold Recombinigen Test, which has sensitivity and specificity of over 99 percent and returns results within 30 minutes (Piwowar-Manning et al. 2010). Surveyors did not continue to test people who had previously tested positive as part of the study.

The use of HIV status as a fixed aspect of "quality" in the marriage market marriage market raises a concern about the potential endogeneity of this variable. The use of endline HIV status information is unavoidable in this context, and other HIV testing evaluations use a similar approach. The HIV testing intervention may lead people to alter their HIV risk exposure, which could in turn influence the probability of seroconversion. This issue is a concern in principle but not in practice. First, HIV testing appears to have only small effects on risky sexual behavior (Thornton 2008, Baird et al. 2014, Gong 2015). Secondly, HIV is difficult to transmit. The HIV transmission probability per coital act is around

assessed attractiveness at the end of the interview, the mannerisms of respondents may have influenced their scores.

[14]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.

0.11 percent (Gray et al. 2001) and the annual infection probability among serodiscordant couples without ART is approximately 5.6 percent (Attia et al. 2009). Among treatment respondents, who are tested repeatedly, only 13 out of 500 people (2.6 percent) seroconvert over the 28 month study interval.[15] Given these magnitudes, no more than a handful of people could have contracted or avoided HIV due to the intervention.

Missing HIV status information in the control group creates another practical issue. While surveyors offered to test treatment respondents eight times, they only offered to test control respondents once in Wave 8. As a result, we do not observe HIV status for 25 percent of control respondents, compared to 3 percent of treatment respondents. Statistically, most untested respondents are likely to be negative. The HIV prevalence among tested respondents is 10.5 percent in the treatment group and 4.9 percent in the control group. Hypothetically, a prevalence of 20 percent among untested respondents would equate the overall prevalence across the treatment and control groups. In our main analysis, we code untested respondents HIV negative because this choice causes less measurement error than coding them as HIV positive. This approach is conservative under the hypothesis that HIV-negative people respond more to the intervention since it biases against finding a response for this group. As an alternative strategy, we impute the HIV status of untested respondents according to their subjective HIV status beliefs. Estimates using this method, which are available from the authors, closely resemble our main results.

To complement HIV status, we define a broader "HIV risk" measure that incorporates additional risk factors. Respondents qualify as "low-risk" if they are HIV-negative (or untested) and meet the following six criteria: (1) has ≤ 2 lifetime partners, (2) has ≤ 1 partners in the past year, (3) has ≤ 10 percent subjective infection risk, (4) does not have multiple partners for money, (5) has sex ≤ 3 times per week, and (6) has never taken ART. We selected these thresholds to isolate the riskiest quartile of the distribution for each vari-

[15]This increase is consistent with data from the Malawi DHS, which show that, in 2010, infection rates are approximately 4 and 14 percent among women aged 15-19 and 25-29, and thus suggest an increase in HIV prevalence of about one percentage point per year, which is what we find in our treatment group.

able. Boileau et al. (2009) associate several of these factors with subsequent HIV infection and marital disruption. 43 percent of all respondents qualify as low-risk, compared to 92 percent who are HIV negative. 67 percent of baseline-unmarried respondents qualify as low-risk, compared to 93 percent who are HIV negative. Appendix GEstimates by the Components of HIV Riskappendix.G shows treatment effect heterogeneity for each individual risk factor.

The survey includes the respondent's perceptions of her own HIV status and the status of her partners. 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). At baseline, 76 percent of respondents provide an infection probability of either 0 or 0.1. The survey also elicits the likelihood that each partner of the respondent has HIV. Unlike own HIV status, this variable is measured on a five-point Likert scale. It is available for respondents who report having at least one partner in Wave t (65 percent of observations), regardless of whether the partner chooses to join the study. We simplify the interpretation of this variable by defining a binary version that equals 1 if the respondent believes her partner "may have HIV." Estimates using the full scale are available from the authors.

Baseline HIV status perceptions for the respondents and their partners support the premise that HIV status information is asymmetric. Among treatment respondents, who surveyors tested at baseline, the subjective infection probability is 0.09 for HIV-negative people and 0.30 for HIV-positive people. This pattern indicates that most people have accurate HIV status perceptions (94 percent of respondents are negative) and that most errors occur because HIV-positive people are unaware of or do not acknowledge their status. In contrast, the belief that a partner may be HIV-positive is only weakly associated with the partner's actual status. We compare the perceptions of partners who are actually HIV negative to the perceptions of partners who are untested or HIV positive. At baseline, respondents believe that 37 percent of HIV-negative partners and 41 percent of HIV-positive or untested partners may be HIV positive. These patterns suggest that people are more informed about their own status than the status of their partners.

6 Identification and Estimation

6.1 Empirical Approach

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

$$Y_{it} = \beta_0 + \beta_1 T_i + \beta_2 Y_i^b + \delta_t + \varepsilon_{it} \tag{7}$$

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, in order to increase precision. We estimate this specification using OLS and cluster standard errors by respondent. The coefficient of interest, β_1, identifies the average treatment effect of offering high-frequency HIV testing.[16]

This parameter is identified under two assumptions. First, we must assume that 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 only constitutes around 1.5 percent of the local marriage market. Secondly, we must assume that 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. Since marriage and fertility increase with age, the age imbalance could spuriously suggest that the intervention leads to marriage and fertility. Figure A3Figure A0: Age Distributions for the Treatment and Control Groupsfigure.caption.38 illustrates this imbalance by plotting the age distribu-

[16]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.

tions in the treatment and control groups. There are approximately 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 DAge-Unweighted Estimatesappendix.D discusses this issue further and shows that results are robust under alternative age corrections.

Table 1 Table 0: Baseline Characteristics by Treatment Statustable.caption.2 provides baseline summary statistics for the treatment and control arms after reweighting by age. Demographic and socioeconomic variables appear balanced, including tribe, religion, HIV status, school enrollment, employment, and household assets.[17] Treatment and control respondents have similar HIV and mortality risk perceptions, which is important because time preferences may influence marriage timing. The table shows that the frequencies of marriage and pregnancy do not differ statistically across treatment arms at baseline. Finally, the table affirms that few people seek HIV testing in the status quo. While most mothers have been tested at least once (under compulsory antenatal testing), only 35 percent of childless women have ever been tested.

After estimating overall effects, we assess the model by examining heterogeneity in the treatment effects by HIV status, HIV risk, and attractiveness. Omitted variables that are correlated with these characteristics may confound our estimates. In these regressions, we limit the sample to baseline-unmarried respondents (for the HIV status and HIV risk interactions) and by HIV status or HIV risk (for the attractiveness interaction). Tables A5 Table A0: Baseline Characteristics by HIV Status and HIV Risk for Unmarried Respondentstable.caption.31 and A6 Table A0: Baseline Characteristics by Attractiveness for Unmarried Respondentstable.caption.33 provide baseline summary statistics for these

[17]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.

subsamples. HIV-positive and high-risk people are older, poorer, and have lower school enrollment than HIV-negative and low-risk people. Attractive people are wealthier and have a stronger future orientation than unattractive people. As we discuss below, we assess the robustness of our heterogeneity estimates by controlling for the interaction between treatment and baseline covariates.

6.2 The Impact on HIV Status Perceptions

This subsection examines the impact of high-frequency testing on HIV status perceptions. To begin, Figure 3Figure 0: Probability of Testing within Four Months by Treatment Arm (with 83% confidence intervals)figure.caption.15 shows that the intervention more than doubles the probability of testing within four months, increasing it from 30 to 70 percent for respondents and from 25 to 48 percent for their partners. These differences are highly statistically significant ($p < 0.001$ in both cases). This pattern is consistent with our interpretation that the intervention dramatically reduced the cost of HIV testing.

Table 2 Table 0: The Impact of HIV Testing on HIV Perceptionstable.caption.4 shows the impact of high-frequency testing on respondents' beliefs about their own HIV status and the HIV status of their partners. If HIV status information is asymmetric, the intervention should have a larger effect on perceptions of partner HIV status than perceptions of own status.[18] Regressions distinguish between HIV-negative and HIV-positive respondents since these subgroups receive fundamentally different messages from the intervention. The subgroup-specific control group means appear in brackets in this table and all subsequent tables. Column 1 shows that high-frequency testing does not change the HIV status perceptions of people who test negative but increases subjective HIV-positive probability of people who test positive, while Column 2 provides a similar estimate for the subset of respondents who have partners. The impact for HIV-positive people arises because there are 26 addi-

[18] While we would prefer to observe market-wide perceptions of the HIV status and HIV risk of respondents, these data are not available. As we explain above, surveyors also offered high-frequency testing to the partners of treatment respondents. Respondent perceptions of the HIV status of partners proxy for market-wide perceptions of HIV status.

tional people in the treatment group who perceive with certainty that they are HIV positive. However the overall effect on own HIV status perceptions is negligible because 94 percent of respondents are HIV negative. However, since 94 percent of respondents are HIV negative, the overall effect on own HIV status perceptions is negligible. This finding is consistent with other HIV testing evaluations, which find minimal effects on on own HIV status perceptions (e.g. Baird et al. 2014).

In contrast, Column 3 shows the impact on perceptions that partners may be HIV positive. Unlike own HIV status, which is measured on a probability scale, this variable is a binary indicator that the respondent believes her partner "may have HIV." Among HIV-negative women, the intervention attenuates this belief by 4.8 percentage points (14 percent). Since own status perceptions do not change, this effect seems to operate via partner testing behavior, which sends a signal of HIV risk. The intervention increases the frequency of partner testing and since most partners test negative, respondents revise downward their perceptions that partners may be HIV positive. Conversely, the intervention leads HIV-positive respondents to revise upward their beliefs about the HIV status of partners by 5.4 percentage points (7.5 percent) although this estimate is not statistically significant. This effect could operate through the upward revision of their own HIV status perceptions.

An examination of endline partner status perceptions also suggests that perceptions depend on the willingness to be tested and the test results. Figure 4Figure 0: Perceptions of Partner HIV Status at Wave 8 (with 83% confidence intervals)figure.caption.16 considers respondents with partners in Wave 8 and distinguishes between those whose partners (1) have tested positive, (2) have never been tested, and (3) have only tested negative. Unsurprisingly, respondents are most likely to believe their partners are positive if these partners have tested positive, and they are most likely to be believe their partners are negative if these partners have tested negative at least once. They hold intermediate beliefs about partners who have never been tested, suggesting that the willingness to be tested sends a signal about HIV status. This comparison is non-causal, and we cannot rule out that this pattern arises through the selection of respondents into categories rather than a causal effect of partner testing. Nevertheless these results suggest that HIV testing allows marriage market participants to signal and screen.

6.3 Impacts on Marriage and Fertility

Table 3 Table 0: The Impact of HIV Testing on Marriage and Fertility table.caption.6 shows treatment effects on marriage and fertility. As in other regression tables, subgroup-specific control group means appear in brackets below coefficients and standard errors. In Panel A, which shows overall estimates, the intervention increases the probability of marriage by 3.5 percentage points (6.3 percent) and the probability of pregnancy by 2.1 percentage points (16.2 percent) across the follow-up period. Panel B shows separate effects for women who are married and unmarried at baseline by interacting T with indicators for both groups. Estimates are substantially larger for unmarried women, who have a higher marriage propensity. The intervention increases the probability of marriage by 5.6 percentage points (27 percent) and the probability of pregnancy by 3.1 percentage points (26 percent) for this group. Since we only observe marriage and fertility over the 28-month study interval, these effects are difficult to relate to impacts over the life cycle. Conversely, estimates are small and insignificant for baseline-married women (although these estimates are not statistically different from the baseline-unmarried estimates). The lack of a negative treatment effect for baseline-married women suggests that the intervention does not induce divorce.

Figure 5Figure 0: Marriage and Pregnancy for Baselined-Unmarried Respondentsfigure.caption.17 illustrates these treatment effects by plotting the probability of marriage and pregnancy by wave and intervention arm. The marriage and pregnancy gaps between the treatment and control arms increase over time and peak around Waves 4 and 5, consistent with the idea that repetition maximizes the marriage market benefit of HIV testing. The marriage and pregnancy gaps decline toward the end of the study period, perhaps due to intertemporal substitution in these outcomes. Appendix CThe Correlation Between Marriage and Pregnancy

Effectsappendix.C shows that the impacts on marriage and fertility are strongly correlated and explores the timing of this correlation in more detail.

Next we examine treatment effect heterogeneity by HIV status and HIV risk for baseline-unmarried respondents. According to the model, the intervention should accelerate marriage for HIV-negative and low-risk people by enabling them to signal and screen. The intervention should have no effect on HIV-positive and high-risk respondents, who marry early in the status quo.[19] If fertility proxies for marital surplus, the intervention should increase fertility for HIV-negative and low-risk respondents and reduce fertility for HIV-positive and high-risk respondents.

Table 4 Table 0 : Baseline-Unmarried Estimates by HIV Risk table.caption.8 shows separate effects by HIV status and HIV risk. In Panel A, the impacts on marriage and pregnancy are positive and significant for HIV-negative respondents but are negative and insignificant for HIV-positive respondents. These differences are not statistically significant ($p = 0.13$) due to the small sample size ($n = 73$) and large standard errors in the HIV-positive group. Panel B distinguishes between low-risk and high-risk respondents. As we discuss in Section 5.2HIV Status, HIV Risk, and Subjective Perceptionssubsection.5.2, "low-risk" is a more restrictive category than "HIV-negative" since it only includes people who are both HIV-negative and lack six additional risk factors. The intervention increases the probability of marriage by 11 percentage points (92 percent) and increases the probability of pregnancy by 6 percentage points (81 percent) for low-risk respondents. Effects are negative and insignificant for the high-risk sample. The low-risk and high-risk effects are significantly different with p-values below 0.02.

HIV status and HIV risk may be correlated with other characteristics that cause treatment effect heterogeneity. Appendix FBaseline Covariates by HIV Status, Risk, and Attractivenessappendix shows that HIV-positive and high-risk respondents are older, poorer, and have more pessimistic views of the future. We assess whether the HIV status and HIV risk interactions are

[19] This prediction relies on the assumption that the marriage market clears. In an alternative model, the intervention may reduce the marriage probability for HIV-positive and high-risk people by making it harder for them to find partners.

robust by controlling for the interaction of T with fourteen demographic, socioeconomic, and time preference covariates.[20] If our strategy misattributes treatment effect heterogeneity in these dimensions to HIV status or HIV risk, then controlling for the interaction of T with these covariates should attenuate our estimates. Instead, Columns 2 and 4 of Table 4 Table 0 : Baseline-Unmarried Estimates by HIV Risk table.caption.8 show that HIV status and HIV risk interactions are robust to these controls. The significant difference between low-risk and high-risk effects in Panel B is also robust ($p = 0.02$). These findings suggest that heterogeneous effects by HIV status and HIV risk do not arise spuriously through another dimension of heterogeneity.

Table 5 Table 0: Baseline-Unmarried Estimates by Attractiveness table.caption.10 explores treatment effect heterogeneity by baseline attractiveness. As Section 2.4The Role of Attractivenesssubsection.2.4 explains, we expect that among the healthy, attractive people may respond more to the intervention than unattractive people. To test this prediction, we limit the sample to HIV-negative respondents in Panel A ($n = 552$) and low-risk respondents in Panel B ($n = 401$).[21] In Panel A, the intervention increases the probability of marriage by 10 percentage points (91 percent) and the probability of pregnancy by 4.6 percentage points (58 percent) for attractive respondents but has small and statistically insignificant effects for unattractive respondents. The impact on attractive respondents is even larger in Panel B. The intervention increases the probability of marriage by 14 percentage points (156 percent) and the probability of pregnancy by 6.9 percentage points (138 percent) in the low-risk sample. Figures 6Figure 0: Marriage by Attractiveness for Low-Risk Baseline Singlesfigure.caption.18 and 7Figure 0: Pregnancy by Attractiveness for Low-Risk Baseline Singlesfigure.caption.19 show the variation underlying these patterns. In the attractive subsample, the treatment-control marriage and pregnancy gaps peak at Waves 4 and 5 and shrink later, while there is no consistent treatment-control gap for unattractive respondents.

[20] 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.

[21] Attractiveness interaction estimates (available from the authors) are small and statistically insignificant for HIV-positive and high-risk people.

Since Appendix FBaseline Covariates by HIV Status, Risk, and Attractivenessappendix.F shows that attractive people are more forward-looking and have higher socioeconomic status, Columns 2 and 4 of Table 5 Table 0: Baseline-Unmarried Estimates by Attractiveness table.caption.10 repeat the exercise in Table 4 Table 0 : Baseline-Unmarried Estimates by HIV Risk table.caption.8 and control for the interaction between T and baseline demographic, socioeconomic, and time preference covariates. As before, the estimates are robust to including these controls and the covariate interactions with T are jointly significant with $p < 0.001$. These results suggest that our estimates reflect a heterogeneous response by attractiveness rather than a spurious correlation.

Three alternative explanations could contribute to our findings. First, the intervention may spur marriage and fertility by making family formation more salient. This explanation is plausible but cannot explain the differential effect for attractive respondents in Table 5 Table 0: Baseline-Unmarried Estimates by Attractiveness table.caption.10. Secondly, the intervention may encourage unprotected sex, which could in turn prompt pregnancy and then marriage. We find no effect on self-reported sexual behavior (estimates available from the authors), which aligns with other results in the literature (Thornton 2008, Baird et al. 2014, Beegle et al. 2015, Gong 2015). Moreover, bootstrap estimates in Appendix CThe Correlation Between Marriage and Pregnancy Effectsappendix.C show the strongest correlation between marriage and lagged pregnancy, which is not consistent with this mechanism. Finally, fertility results could arise through a decrease in female bargaining power (Rasul 2008). Baseline-married women in our sample desire the same number of children as their husbands but prefer to have children later. However we do not find effects on other bargaining power proxies, including gifts from partners and decision-making autonomy (estimates are available from the authors).

7 The Effect of a One-Shot HIV Testing Intervention

Our findings contrast with other evaluations of HIV testing, 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). A distinctive feature of the TLT HIV testing intervention is that respondents and their partners were repeatedly offered HIV tests. While a single test may not reveal one's risk type, repeated offers to test may help people screen by observing others' testing behavior and results.[22]

This section attempts to reconcile our findings with the literature by assessing the importance of repeated HIV testing. As we explain in Section 4Interventionsection.4, the TLT Panel Study includes a third intervention arm ($n = 500$) that offered participants and their partners HIV tests after Waves 4 and 8. By comparing this arm to the control arm over Waves 5-8, we estimate the impact of offering a single HIV test. We estimate Equation (7Empirical Approachequation.6.7), reweight to balance by age, pool follow-up rounds, and cluster standard errors by respondent to match our previous empirical strategy. This approach differs from our primary analysis because the follow-up period includes four rather than seven waves.[23]

Table 6 Table 0: The Impact of an Alternative Single-Test Interventiontable.caption.12 provides treatment effect estimates for the single-test intervention. The overall estimates in Panel A are analogous to Table 3 Table 0: The Impact of HIV Testing on Marriage and Fertility table.caption.6 (Panel A), the estimates by HIV risk in Panel B are analogous to Table 4 Table 0 : Baseline-Unmarried Estimates by HIV Risk table.caption.8 (Panel B), and the estimates by attractiveness in Panel C are analogous to Table 5 Table 0: Baseline-Unmarried Estimates by Attractiveness table.caption.10 (Panel B). All estimates are small and statistically insignificant. The single-test intervention has no effect on marriage or pregnancy overall or in the low-risk and attractive subsamples. For a like-to-like comparison with repeated testing, we limit the repeated-test sample to Waves 2-5 and use seemingly unrelated regression to test whether treatment effects are statistically different.[24] Daggers in Table 6 Table 0: The Impact of an Alternative Single-Test Interventiontable.caption.12

[22]Delavande et al. (2016) show that repeated HIV testing of serodiscordant couples in Malawi reduces risky sexual behavior.

[23]Table A7 Table A0: Wave-4 Characteristics by Treatment Status for One-Shot Testingtable.caption.35 provides summary statistics for these intervention arms at Wave 4. Characteristics are generally balanced, although respondents in the single-test arm are less future-oriented and more likely to be HIV positive.

[24]Repeated-test estimates over Waves 2-5 closely resemble our primary estimates and are available from the authors.

indicate statistically significant differences between single-test and repeated-test estimates. Effects are nearly always significantly different. The lack of an effect of the single-test intervention in this sample suggests that repeated testing is an essential factor in the strong marriage and fertility effects above.

8 Discussion and Conclusion

The HIV/AIDS epidemic has coincided with marriage and fertility delays in SSA. In Malawi, age at first marriage and age at first birth loosely track the peak and subsequent abatement of HIV in Malawi. 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 finding that high-frequency testing (offered for free on an opt-out basis) accelerates marriage and fertility supports this hypothesis. We show that HIV-negative, low-risk people respond the most to this intervention, which suggests that high-frequency testing enables these people to signal and screen.

Figure 1Figure 0: Marriage and Fertility for Women Aged 17-27figure.caption.13 shows that trends in marriage and fertility in Malawi coincide with the emergence and subsequent abatement of the HIV/AIDS epidemic in Malawi. To gauge whether adverse selection is a plausible factor in these trends, Figure 8Figure 0: A Comparison of HIV Testing Impacts to National Marriage and Fertility Patternsfigure.caption.20 compares treatment effect estimates of high-frequency testing to the 1992-2000 increase and the 2000-2010 decrease in the age at first marriage and the age at first birth in Malawi.[25] The impact of 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 from and 50 percent of the fertility acceleration from 2000-2010. These magnitudes suggest that endogenous marriage timing

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

may contribute substantially to the observed demographic trend. 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. Besides helping HIV diagnosis and treatment, our findings show that the provision of opt-out testing is likely to have strong effects on marriage and fertility. Recent technological changes have reduced the cost and ease of HIV testing. Self-testing kits, which continue to gain prevalence, 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 a standard model, the resolution of asymmetric information should improve welfare for low-risk people. However early marriage and fertility are associated with the cessation of schooling, which may have other private and social costs (Jensen and Thornton 2003). We find no impact of the intervention on school enrollment, which may temper this concern.

Table 1: Baseline Characteristics by Treatment Status

	Treatment (1)	Control (2)	P-value (3)
Demographics			
Age	19.8	19.8	1.00
Attractiveness	3.54	3.59	0.21
Ngoni Tribe	0.38	0.38	0.99
Yao Tribe	0.25	0.26	0.83
Lomwe Tribe	0.19	0.16	0.15
Catholic	0.33	0.32	0.71
Protestant	0.49	0.49	0.89
Muslim	0.18	0.19	0.78
HIV positive (endline)	0.10	0.08	0.14
High HIV risk	0.59	0.56	0.25
Socioeconomic Status			
Enrolled in school	0.36	0.40	0.14
Employed full-time	0.18	0.20	0.43
Any savings	0.17	0.13	0.12
Household asset index	-0.02	0.06	0.16
Preferences and Perceptions			
Thinks about future	3.12	3.19	0.28
Subjective 5-year mort. risk	0.34	0.33	0.74
Subjective probability HIV positive	0.12	0.10	0.17
Worried about HIV	1.04	1.03	0.85
Outcomes			
Married	0.43	0.46	0.26
Pregnant	0.15	0.12	0.15
Ever tested for HIV (parity=0)	0.35	0.35	0.87
Ever tested for HIV (parity>0)	0.90	0.88	0.62
Partner may be HIV positive	0.48	0.44	0.44
Observations	500	507	-

Note: All means are weighted for age balance. 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 HIV Testing on HIV Perceptions

	Respondent pr(HIV+)		Partner May Have HIV
	(1)	(2)	(3)
Treatment · HIV Negative	0.0046	0.011	-0.048**
	(0.0092)	(0.010)	(0.021)
	[0.14]	[0.15]	[0.34]
Treatment · HIV Positive	0.17***	0.17**	0.054
	(0.064)	(0.072)	(0.062)
	[0.57]	[0.60]	[0.72]
Equality of coefficients (p-value)	0.01	0.03	0.12
Observations	6048	4456	4456

Note: Clustered standard errors appear in parentheses. Subgroup-specific control group means appear in brackets. All regressions cover Waves 2-8 and control for wave fixed effects and the baseline dependent variable. Column 1 includes the full sample while Columns 2-3 only include respondents with partners. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

	Currently Married	Currently Pregnant
	(1)	(2)
A: Overall Estimates		
Treatment	0.035**	0.021**
	(0.017)	(0.010)
	[0.55]	[0.13]
B: Estimates by Baseline Marital Status		
Treatment · Unmarried	0.056**	0.031**
	(0.028)	(0.013)
	[0.21]	[0.12]
Treatment · Married	0.010	0.016
	(0.018)	(0.015)
	[0.93]	[0.17]
Equality of coefficients (p-value)	0.17	0.47
Observations	6048	6048

Note: 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 and include wave indicators and the baseline dependent variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 : Baseline-Unmarried Estimates by HIV Risk

	Currently Married		Currently Pregnant	
	(1)	(2)	(3)	(4)
A: Estimates by HIV Status				
Treatment · HIV Negative	0.056*	0.049*	0.038***	0.039***
	(0.030)	(0.029)	(0.014)	(0.014)
	[0.17]	[0.17]	[0.09]	[0.09]
Treatment · HIV Positive	-0.15	-0.053	-0.057	-0.071
	(0.13)	(0.13)	(0.061)	(0.057)
	[0.41]	[0.41]	[0.15]	[0.15]
Equality of coefficients (p-value)	0.13	0.45	0.13	0.07
Significance of covariates (p-value)	-	0.00	-	0.00
B: Estimates by HIV Risk				
Treatment · Low Risk	0.11***	0.094***	0.057***	0.054***
	(0.034)	(0.034)	(0.015)	(0.015)
	[0.12]	[0.12]	[0.07]	[0.07]
Treatment · High Risk	-0.060	-0.046	-0.013	-0.011
	(0.053)	(0.046)	(0.025)	(0.024)
	[0.30]	[0.30]	[0.15]	[0.15]
Equality of coefficients (p-value)	0.01	0.02	0.02	0.02
Significance of covariates (p-value)	-	0.00	-	0.00
Control for treatment · covariates	No	Yes	No	Yes
Observations	3437	3437	3437	3437

Note: 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 and include wave indicators and the baseline dependent variable. The "low risk" group includes respondents who are HIV-negative and also lack six additional risk factors, as explained in the text. The "high risk" group includes respondents who are HIV-positive or have any of these risk factors. 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: Baseline-Unmarried Estimates by Attractiveness

	Currently Married		Currently Pregnant	
	(1)	(2)	(3)	(4)
A: HIV-Negative Subsample				
Treatment · Attractive	0.10***	0.14***	0.046***	0.056***
	(0.038)	(0.037)	(0.018)	(0.018)
	[0.11]	[0.11]	[0.08]	[0.08]
Treatment · Not Attractive	-0.009	-0.050	0.021	0.013
	(0.048)	(0.046)	(0.022)	(0.020)
	[0.25]	[0.25]	[0.12]	[0.12]
Equality of coefficients (p-value)	0.06	0.00	0.35	0.12
Significance of covariates (p-value)	-	0.00	-	0.00
Observations	3200	3200	3200	3200
B: Low-Risk Subsample				
Treatment · Attractive	0.14***	0.15***	0.069***	0.064***
	(0.045)	(0.045)	(0.020)	(0.020)
	[0.09]	[0.09]	[0.05]	[0.05]
Treatment · Not Attractive	0.053	0.014	0.030	0.031
	(0.054)	(0.055)	(0.024)	(0.023)
	[0.16]	[0.16]	[0.10]	[0.10]
Equality of coefficients (p-value)	0.21	0.08	0.21	0.29
Significance of covariates (p-value)	-	0.00	-	0.00
Observations	2206	2206	2206	2206
Treatment · covariates	No	Yes	No	Yes

Note: Standard errors are clustered by respondent and appear in parentheses. Subgroup-specific control group means appear in brackets. The "low risk" group includes respondents who are HIV-negative and also lack six additional risk factors, as explained in the text. All regressions reweight to balance by age and include wave indicators and the baseline dependent variable. 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 6: The Impact of an Alternative Single-Test Intervention

	Currently Married	Currently Pregnant
	(1)	(2)
A: Overall Estimates		
Treatment	-0.020††	-0.001
	(0.018)	(0.014)
	[0.56]	[0.14]
Observations	3308	3308
B: Estimates by HIV Risk		
Treatment · Low Risk	-0.013†††	-0.018†††
	(0.034)	(0.021)
	[0.14]	[0.10]
Treatment · High Risk	-0.039	0.016
	(0.061)	(0.036)
	[0.25]	[0.15]
Equality of coefficients (p-value)	0.72	0.42
Observations	1649	1649
C: Estimates by Attractiveness		
Treatment · Attractive	-0.0018†††	-0.030†††
	(0.037)	(0.027)
	[0.10]	[0.10]
Treatment · Unattractive	-0.032†††	-0.0019
	(0.060)	(0.033)
	[0.18]	[0.10]
Equality of coefficients (p-value)	0.67	0.51
Observations	1098	1098

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. Consistent with earlier results, Panel B is limited to baseline-unmarried respondents and Panel C is limited to low-risk, baseline-unmarried respondents. No estimates in the table are significantly different from zero. Daggers indicate significant differences from repeated testing estimates based on seemingly unrelated regressions, as we explain in the text. † $p < 0.1$, †† $p < 0.05$, † † † $p < 0.01$.

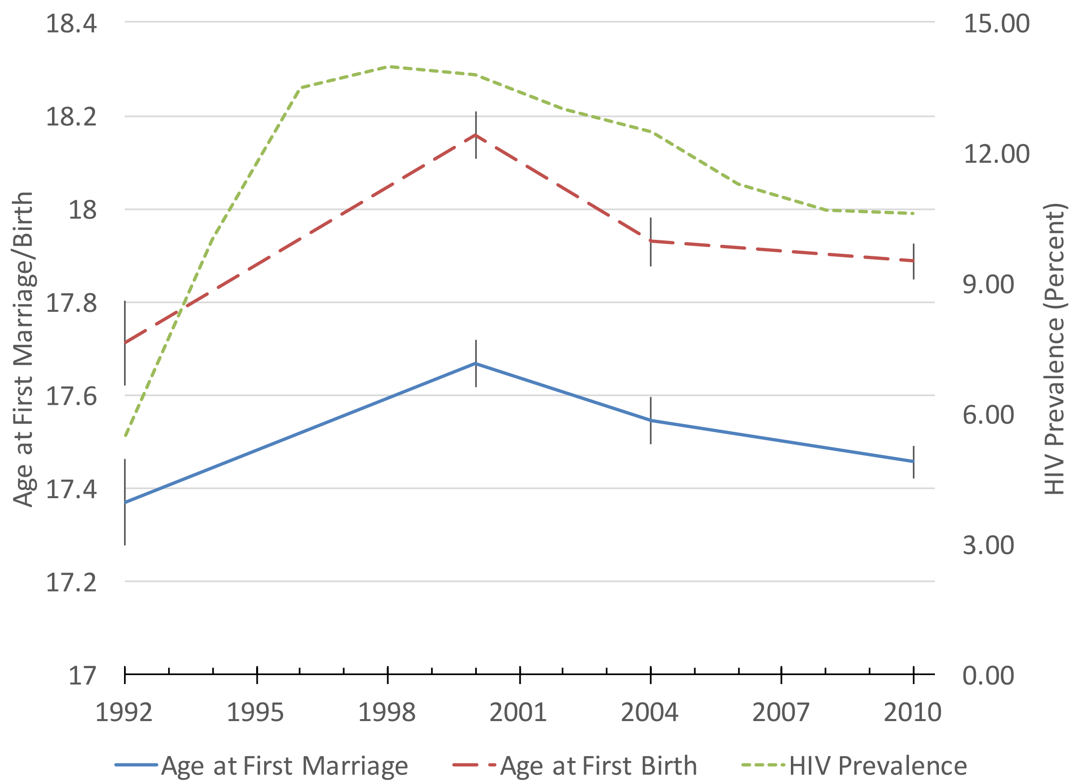

Figure 1: Marriage and Fertility for Women Aged 17-27

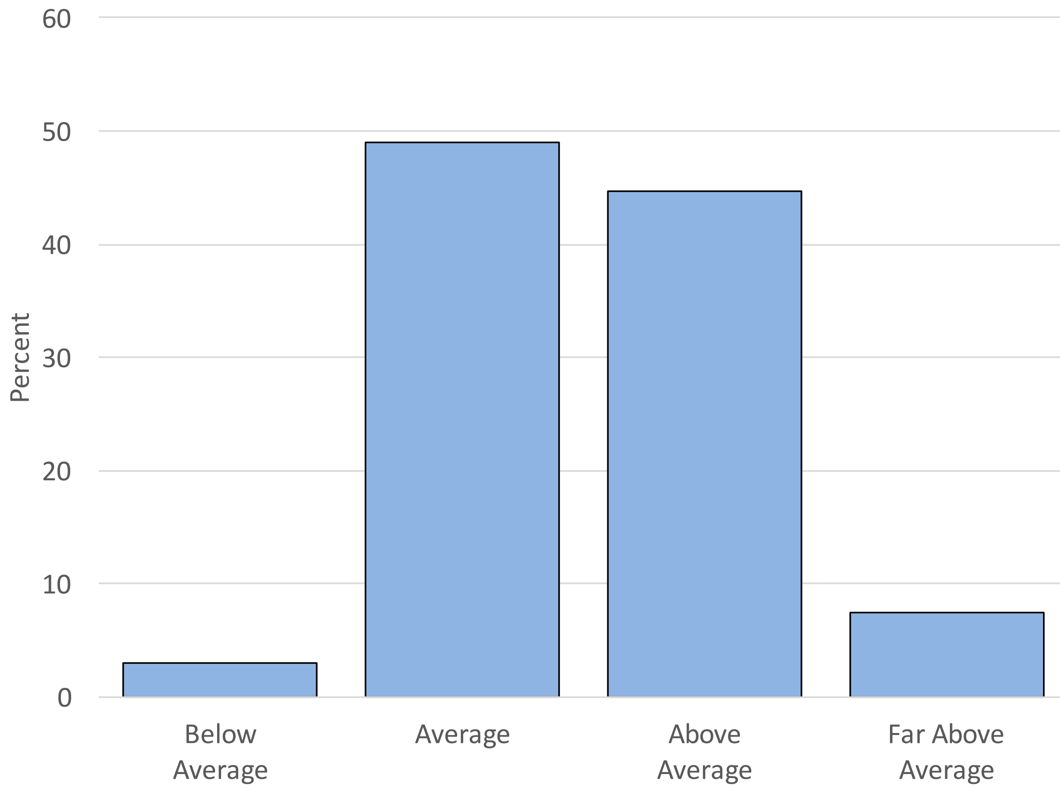

Figure 2: The Distribution of Baseline Attractiveness (with 83% confidence intervals)

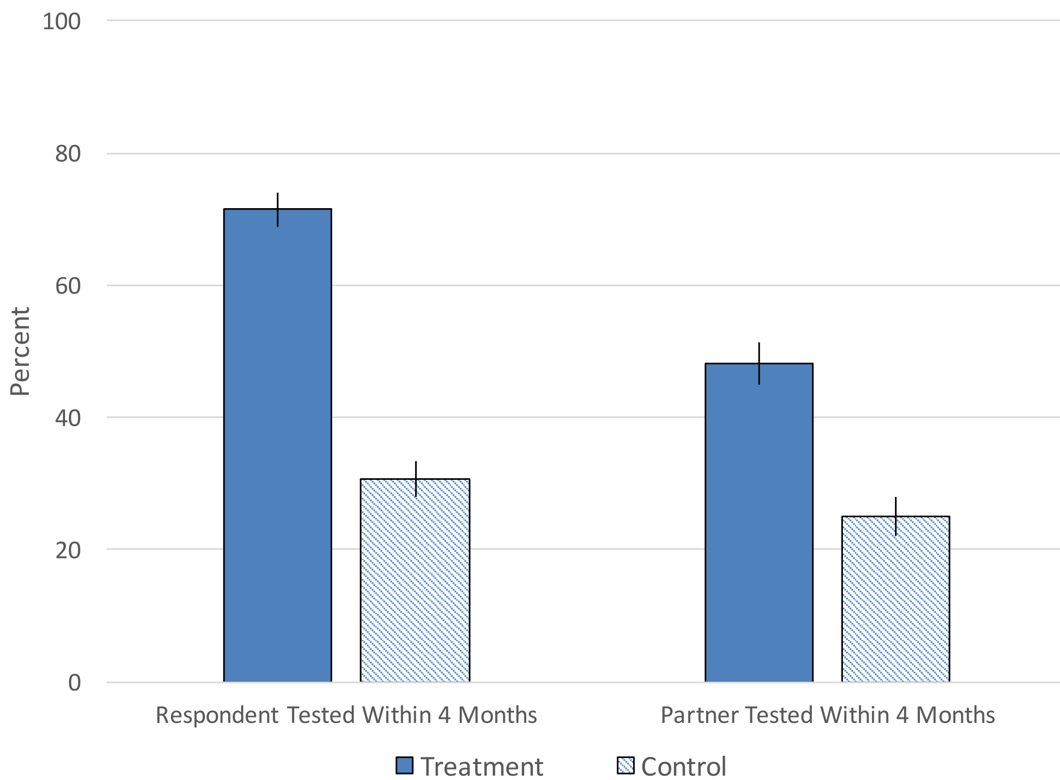

Figure 3: Probability of Testing within Four Months by Treatment Arm (with 83% confidence intervals)

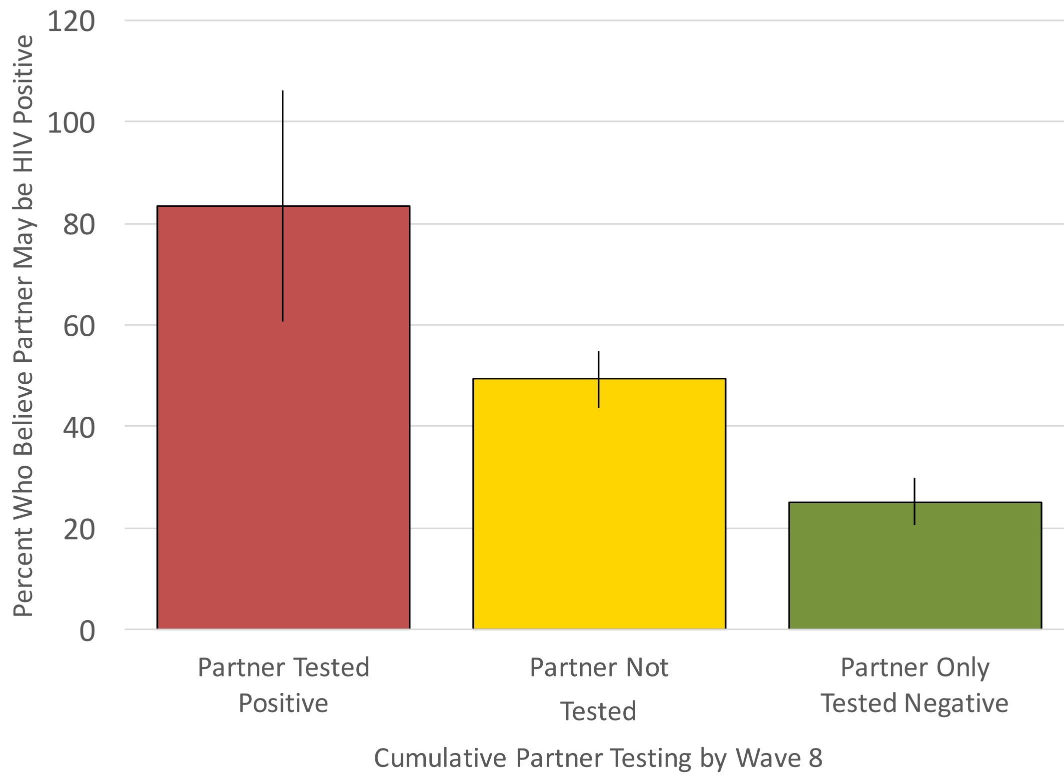

Figure 4: Perceptions of Partner HIV Status at Wave 8 (with 83% confidence intervals)

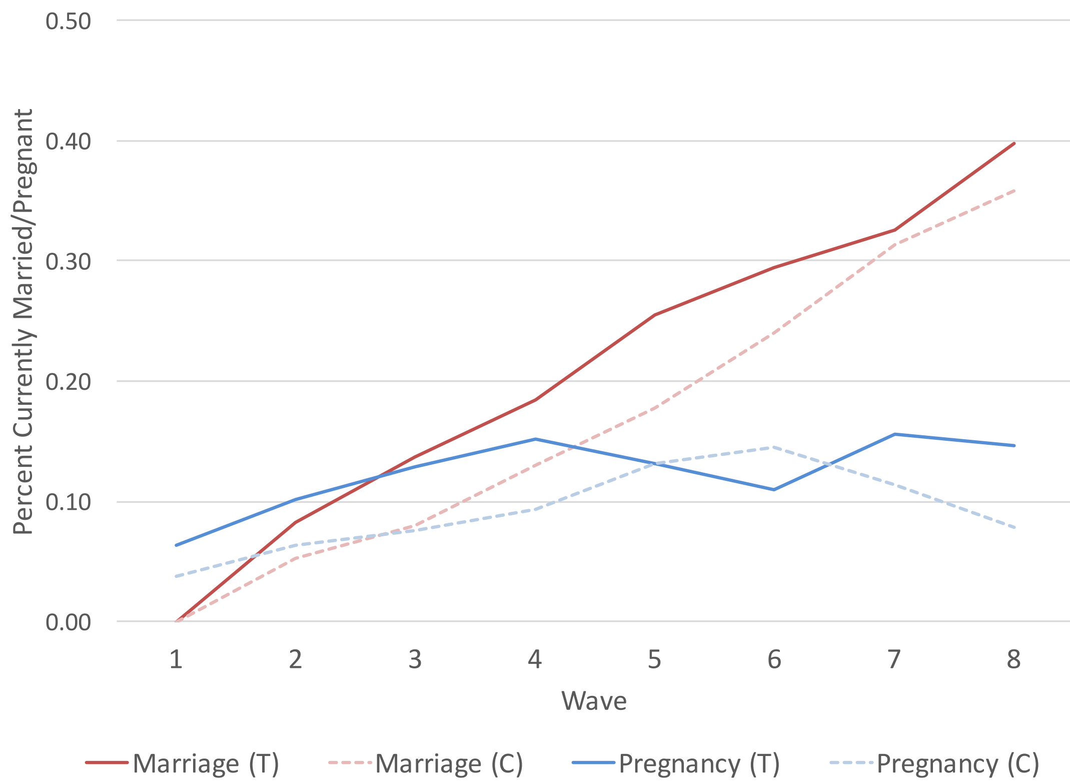

Figure 5: Marriage and Pregnancy for Baselined-Unmarried Respondents

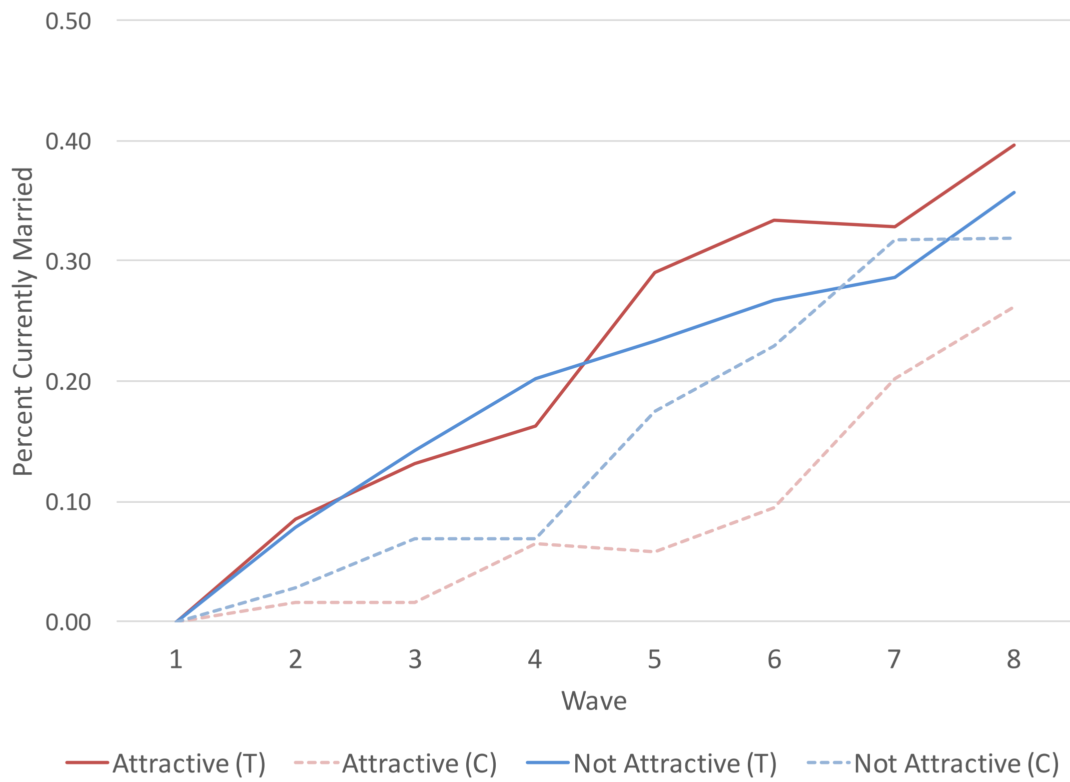

Figure 6: Marriage by Attractiveness for Low-Risk Baseline Singles

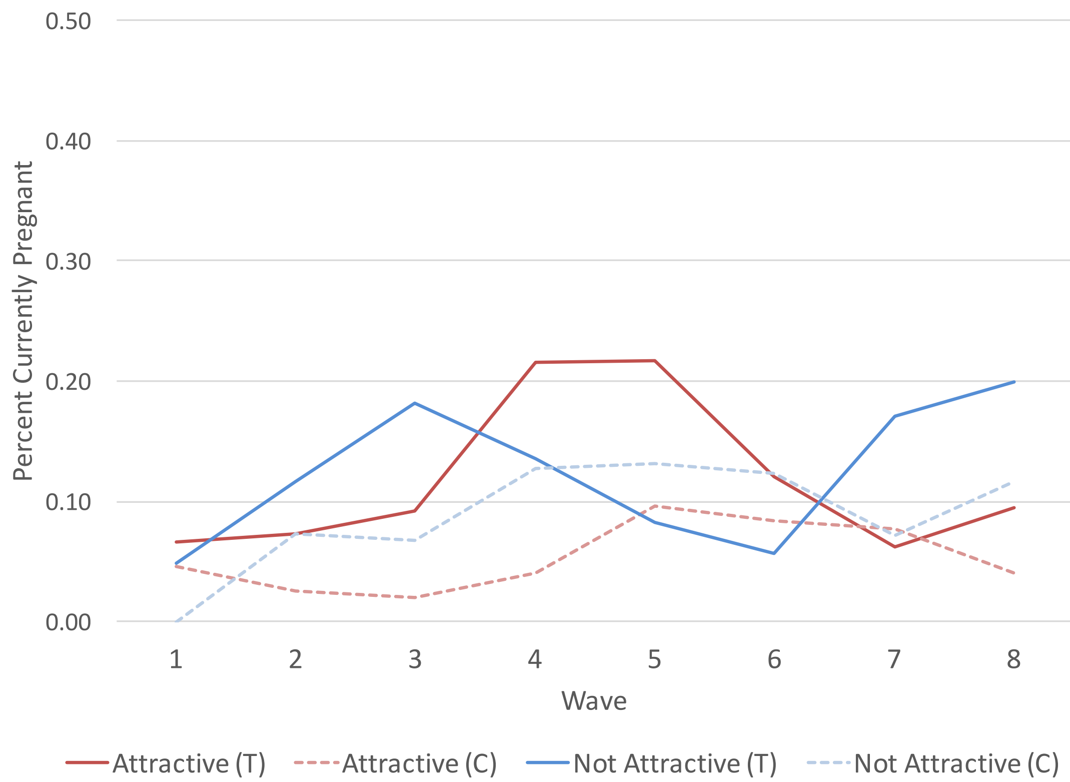

Figure 7: Pregnancy by Attractiveness for Low-Risk Baseline Singles

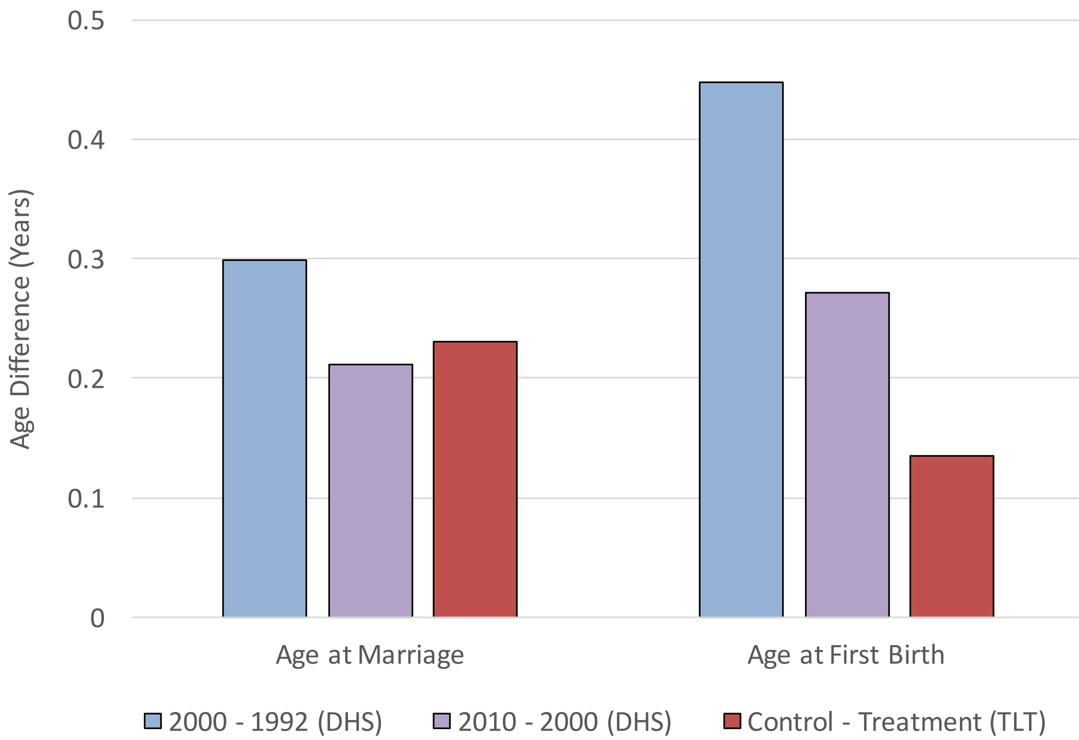

Figure 8: A Comparison of HIV Testing Impacts to National Marriage and Fertility Patterns

Appendix – Not for Publication

A The Model Under Alternative Assumptions

This section considers the implications of alternative assumptions about the relative values of attractiveness and health, divorce, and the correlation between attractiveness and health.

A.1 Relative Value of Traits

In our empirical setting, health may be a larger determinant of surplus than attractiveness. In this case, the surpluses would be ranked as follows: $S^{ab}_{hh} > S^{ab}_{lh} > S^{ab}_{hl} > S^{ab}_{ll} > 0$. This different ranking of surpluses would cause people to marry a type with their exact traits in Period 2, rather than a type with the same number of high traits. Conversely, there would still be assortative matching in attractiveness in Period 1, regardless of the correlation between health and attractiveness. In Period 1, people match to maximize expected, rather than observed surplus, because of the two-sided nature of the asymmetric information. Suppose that attractiveness and health are sufficiently negatively correlated that the expected surplus from marrying an attractive partner is lower than marrying an unattractive partner. In Period 1, an attractive and healthy person would want to marry an unattractive person, because that maximizes her expected surplus. However, since she cannot signal that she is healthy, unattractive people do not want to marry her. Regarding marriage timing, unhealthy people continue to prefer to marry in Period 1, as they have more to gain, while the incentives for delaying marriage are now higher for healthy people, as the cost of marrying an unhealthy partner in Period 1 is greater in this version of the model. Therefore, making marrying unhealthy men less appealing exacerbates the adverse selection in the marriage market in Period 1. Making health observable in Period 1 does not change the signs of the predictions. If anything, it makes the Period 1 increases in marriage rates larger for healthy women, as more of them delayed marriage in Period 2 when health is unobserved.

A.2 Negative Marital Surplus

If marriage surplus from marrying a person with two low traits were negative, the main intuitions of the model would not change. However, the adverse selection of healthy types would be exacerbated and making health observable in Period 1 would result in no marriages for people with two low traits.

A.3 Divorce Costs

We modeled divorce costs as being sufficiently high to prevent married people from divorcing. If we relaxed this assumption and, for simplicity, made divorce costless, everybody would marry a person with the same attractiveness in Period 1, because doing that gives a strictly positive surplus. In Period 2, people who are mismatched would divorce and marry a partner with the same number of high traits.

In this case, making health observable in Period 1 does not change the likelihood of being married in Period 1 (as everybody marries in Period 1 anyway). Conversely, as before, making health observable increases Period 1 surplus for healthy women and decreases Period 1 surplus for unhealthy women. This occurs because now all women marry a partner with the same number of high traits, while, when health is unobserved, healthy women marry partners with fewer high traits on average, and unhealthy women marry partners with more high traits on average.

Making health observable in Period 1 also increases divorce rates for women already married to a spouse with mismatched traits in Period 1. Those women would have divorced in Period 2, after finding out their spouses' health, but now do so in Period 1. If re-marriage is instantaneous, then we also expect the same effects on surplus for married women as the ones described for singles. That is, surplus increases for healthy women and decreases for unhealthy ones through divorce and re-marriage with a partner who has the same number of high traits.

With low but positive divorce costs, the effects of making health observable in Period

1 on marriage timing and surplus would have the same sign as in the main version of the model, by interpolating from the two extreme cases of zero and "too-high" divorce costs.

A.4 Dependence Between Attractiveness and Patience

To simplify our notation, we assumed that patience and attractiveness are independent of each other. If we assume that attractive people are more patient, which is more realistic, there are no major changes in equilibrium behavior and predictions. If anything, now a higher fraction of attractive than unattractive women wait to marry in Period 2, and, therefore, making health observable in Period 1 causes an even bigger increase in the likelihood of marrying in Period 1 for attractive than unattractive women.

A.5 Dependence Between Traits and Gender

To simplify our notation, we also assumed that attractiveness and health are independent of gender. Making this assumption results in a Period 2 equilibrium in which each woman is matched with a partner with the same number of high traits, because each type has equal size for men and women. If we relax this assumption and, for example, have a higher proportion of unhealthy women than men, the essence of the model does not change - we still have positive assortative matching in the number of high traits in Period 2 and in attractiveness in Period 1, and sufficiently patient healthy people who wait to marry in Period 2. However, in this case there are two differences. First, some women "marry down," that is, marry a man with fewer high traits in Period 2 and marry a man of lower attractiveness in Period 1. Second, some unhealthy women may remain unmarried in Period 1. This is because there are more men than women who want to wait and marry in Period 2. Something similar would also occur if men are more patient than women. In this setting, making health observable in Period 1 increases the marriage likelihood in Period 1 also for unhealthy women and may increase or decrease their Period 1 surplus.

A.6 Own Health is Unobservable

We also consider a scenario in which people do not know their own health in Period 1. In that case, marriage is still assortative in the number of high traits in Period 2 and in attractiveness in Period 1. Marriage timing would change, though, being determined by impatience only, and not by health (since all have the same beliefs about their own health). Therefore, sufficiently patient (healthy and unhealthy) people marry in Period 2, while impatient people marry in Period 1. Under some conditions, attractive women continue to be more likely to marry in Period 2 than unattractive women.

If health becomes observable in Period 1, marriage likelihood increases for both healthy and unhealthy unmarried women, as patient women no longer have to wait, and, under some conditions, more for attractive than unattractive women. The average surplus increases for healthy women because (i) all, rather than some, marry in Period 1 and (ii) none of them marries an unhealthy man. Conversely, the effect of making health observable in Period 1 on unhealthy women's surplus is unclear. This occurs because patient unhealthy women now marry in Period 1, generating a positive marriage surplus. However, the average surplus for impatient unhealthy women decreases, as now they all marry unhealthy men, while, with health unobserved, some of them would have married healthy men.

B Multiple Equilibria and the Demand for HIV Testing

This section describes the demand for HIV testing as a coordination game. Section 2Theorysection.2 argues that frequent HIV testing has substantial marriage market benefits for healthy people. It may therefore seem paradoxical that only a minority of respondents have ever been tested at baseline. While it is nominally free, seeking an HIV test entails substantial costs in terms of both convenience 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. This positive externality of seeking a test means 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 cost of receiving treatment if positive. Testing entails two costs: a transportation cost, $\gamma \geq 0$, and a stigma cost $\mu \geq 0$. γ includes the monetary and time costs of traveling to the clinic and waiting in line. μ represents testing stigma, which is present only if a player tests unilaterally. The following matrix represents this game.

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

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

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

$\gamma \approx \beta - \mu$.

C The Correlation Between Marriage and Pregnancy Effects

The interpretation of our results depends in part on the causal link between marriage and pregnancy. For instance, the use of pregnancy as a proxy for marital surplus presupposes that pregnancies primarily occur within marriage. While a reduced-form analysis cannot directly identify the causal relationship between endogenous outcomes, 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 jointly bootstrap the marriage and pregnancy estimates for the baseline-unmarried, low-risk sample. Panel B of Table 4 Table 6 : Baseline-Unmarried Estimates by HIV Risk table.caption.8 shows that the intervention increases the probability of marriage by 11 percentage points and the probability of pregnancy by 5.4 percentage points for this group. The bootstrap exercise allows us to estimate the correlation between the treatment effects on these outcomes in order to provide suggestive evidence of a causal relationship. Figure A1Figure A8: Effects on Marriage and Fertility for Baseline-Unmarried Low-Risk Respondents Across Bootstrap Replicationsfigure.caption.36 provides a scatterplot of coefficient estimates for marriage and pregnancy regressions across bootstrap replications. The correlation coefficient for these treatment effects is 0.54, indicating that replication samples with strong marriage effects also have strong pregnancy effects. Although this finding is consistent with a common causal pathway, we cannot rule out the possibility that another factor jointly contributes to the impact on both outcomes.[26]

The bootstrap approach also allows us to explore the timing of the treatment effects on marriage and pregnancy. We jointly bootstrap the marriage and pregnancy regressions using

[26] 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 significant effect on [pregnant · married] and a small and statistically insignificant effect on [pregnant · unmarried]. These results are consistent with our bootstrap estimates but are more difficult to interpret.

a lead or lag of up to two periods between marriage and fertility. This approach requires us to drop Waves 2 and 8 for consistency across comparisons, which has a minimal effect on our estimates. If the treatment effect on pregnancy primarily operates via marriage, the correlation between the marriage and pregnancy treatment effects should be the strongest for pregnancy *after* Wave t. Figure A2Figure A8: 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.caption.37 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.

D Age-Unweighted Estimates

This section provides additional detail regarding the age imbalance in the data. Figure A3Figure A8: Age Distributions for the Treatment and Control Groupsfigure.caption.38 shows the unweighed 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).

Table A1 Table A6: Age-Unweighted Baseline Characteristics by Treatment Statustable.caption.23 reproduces Table 1 Table 6: Baseline Characteristics by Treatment Statustable.caption.2 from the paper without reweighing to balance by age.[27] The table shows that several additional variables, including pregnancy, school enrollment, and HIV status, are unbalanced across intervention arms. This pattern is understandable in terms of the age imbalance since

[27] Age-unweighed versions of Tables A5 Table A6: Baseline Characteristics by HIV Status and HIV Risk for Unmarried Respondentstable.caption.31 and A6 Table A6: Baseline Characteristics by Attractiveness for Unmarried Respondentstable.caption.33, which cut by HIV status, HIV risk, and attractiveness, are available from the authors.

these variables are strongly correlated with age. Several other variables, including marriage, attractiveness, employment, and household assets do not differ significantly by intervention arm. The research team cannot identify a probem with the sampling methodology that could have caused this issue. The balance across age-orthogonal characteristics, including religion, tribe, and household assets, suggests that the age imbalance is due to chance rather than a flaw in the randomization.

Next we provide age-unweighed estimates in order to assess further the impact of age weighting. Table A2 Table A6: Age-Unweighted Estimates table.caption.25 reproduces our main results without reweighting by age. Columns 1 and 3 include respondents of all ages, while Columns 2 and 4 limit the sample to respondents who are 17 or older at baseline, for whom age is already balanced without reweighing. In Panel A, which provides overall estimates, the impact on marriage increases from 0.035 to 0.045 and the impact on pregnancy increases from 0.021 to 0.025, compared to Table 3 Table 6: The Impact of HIV Testing on Marriage and Fertility table.caption.6. The interactions with HIV risk and attractiveness in Panels B and C closely resemble our earlier estimates, as do estimates (available from the authors) that control for age rather than reweight. The estimates in Columns 2 and 4 show that results are robust in the age-limited sample. These findings indicate that the age imbalance and the weighting procedure are unlikely to change our findings.

E 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 these interviews. Respondents completed an average of 7 survey rounds, and 71 percent of respondents completed all eight rounds. Table A3 Table A6: Baseline Characteristics by Attrition Statustable.caption.27 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, we cannot reliably contrast the HIV status or HIV risk of attriters and non-attriters. Attrition is not correlated with treatment: 73 percent of treatment respondents complete all eight waves, compared to 70 percent of control respondents ($p = 0.35$).

Table A4 Table A6: Estimates for Non-Attriterstable.caption.29 reproduces our main estimates among the sample of non-attriters. Estimates closely resemble the main results in the paper. Effects are larger for low-risk and attractive respondents, with magnitudes that correspond closely with the results in the paper. While we cannot definitively rule out bias in treatment effect estimates due to attrition, these results suggest that attrition is not a major confound.

F Baseline Covariates by HIV Status, Risk, and Attractiveness

Section 6.3Impacts on Marriage and Fertilitysubsection.6.3 of the paper shows evidence of differential treatment effects by HIV status, HIV risk, and attractiveness. This appendix provides additional context for these estimates by showing how baseline respondent characteristics vary along these dimensions. Table A5 Table A6: Baseline Characteristics by HIV Status and HIV Risk for Unmarried Respondentstable.caption.31 cuts the sample by HIV status (Columns 1-3) and HIV risk (Columns 4-6). The table limits the sample to baseline-unmarried respondents for consistency with our earlier estimates. HIV-positive and high-risk people are older, less attractive, and more likely to be employed. They perceive substantially higher HIV infection probabilities for themselves and their partners. Table A6 Table A6: Baseline Characteristics by Attractiveness for Unmarried Respondentstable.caption.33 limits the sample further to HIV-negative respondents in Columns 1-3 and low-risk respondents (as defined in Section 5.2HIV Status, HIV Risk, and Subjective Perceptionssubsection.5.2) in Columns 4-6. These samples correspond to the estimation samples in Panels A and B of Table 5 Table 6: Baseline-Unmarried Estimates by Attractiveness table.caption.10. In the HIV-negative sample, attractive respondents are more likely to be enrolled in school rather than working, although these differences are not statistically significant in the low-risk sample. Attractive respondents are also wealthier and more future oriented.

Since several variables are correlated with HIV status, HIV risk, and attractiveness, the even columns of Tables 4 Table 6 : Baseline-Unmarried Estimates by HIV Risk table.caption.8 and 5 Table 6: Baseline-Unmarried Estimates by Attractiveness table.caption.10 control for the interaction of treatment with baseline covariates. Estimates are robust to the inclusion of controls, suggesting that the heterogeneous treatment effects along these dimensions are not spurious.

G Estimates by the Components of HIV Risk

The HIV risk index incorporates six HIV risk factors in addition to HIV status. A person qualifies as "low-risk" if she is HIV-negative and lacks *all* of these risk factors. These characteristics are: (1) has ≤ 2 lifetime partners, (2) has ≤ 1 partners in the past year, (3) has ≤ 10 percent subjective infection risk, (4) does not have multiple partners for money, (5) has sex ≤ 3 times per week, and (6) has never taken ART. We selected these thresholds to isolate the riskiest quartile of the distribution for each variable.

Figure A4Figure A8: Estimated Treatment Effects on Marriage and Fertility for "Low-Risk" Respondents According to Seven Risk Factorsfigure.caption.39 shows the treatment effect for baseline-unmarried, low-risk respondents according to each risk factor individually. Impacts are moderate and magnitudes are comparable to the HIV-negative estimates on the left of the figure. Estimates using the composite low-risk definition, which appear at the far right, are substantially larger than the estimates by any individual risk factor. This pattern suggests that HIV risk is a multidimensional concept, so that there are several different characteristics that may indicate that someone is high-risk.

H Summary Statistics for the Single Testing Intervention

Section 7The Effect of a One-Shot HIV Testing Interventionsection.7 finds no effects of a single-test intervention of marriage and fertility. This appendix provides additional background for this result. Participants in the "single test" intervention arm were offered testing after Wave 4 and Wave 8. To examine the impact of offering a single test, we compare these respondents to the control group over Waves 5-8. Wave 4 functions as a baseline in this construction. Table A7 Table A6: Wave-4 Characteristics by Treatment Status for One-Shot Testingtable.caption.35 provides summary statistics for the single-test arm and the control arm in Wave 4. 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 Baseline Characteristics by Treatment Status

	Full Sample		
	Treatment	Control	P-value
	(1)	(2)	(3)
Demographics			
Age	19.8	19.2	0.01***
Attractiveness	3.54	3.59	0.18
Ngoni Tribe	0.38	0.37	0.76
Yao Tribe	0.25	0.27	0.60
Lomwe Tribe	0.19	0.16	0.12
Catholic	0.33	0.33	0.88
Protestant	0.49	0.48	0.83
Muslim	0.18	0.19	0.64
HIV positive (endline)	0.10	0.07	0.04**
High HIV risk	0.59	0.51	0.01***
Socioeconomic Status			
Enrolled in school	0.36	0.47	0.00***
Employed full-time	0.18	0.18	0.90
Any savings	0.17	0.12	0.05**
Household asset index	-0.03	0.07	0.12
Preferences and Perceptions			
Thinks about future	3.12	3.18	0.35
Subjective 5-year mort. risk	0.34	0.33	0.58
Subjective probability HIV positive	0.12	0.10	0.10
Worried about HIV	1.04	0.92	0.05*
Outcomes			
Married	0.43	0.40	0.41
Pregnant	0.15	0.11	0.04**
Ever tested for HIV (parity= 0)	0.35	0.30	0.20
Ever tested for HIV (parity> 0)	0.88	0.90	0.48
Partner may be HIV positive	0.48	0.44	0.45
Observations	500	507	-

Note: means are not weighted by age. To compute p-values, we regress each variable on treatment in Wave 1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Age-Unweighted Estimates

	Currently Married		Currently Pregnant	
	(1)	(2)	(3)	(4)
A: Overall Estimates				
Treatment	0.045***	0.028	0.027***	0.022*
	(0.017)	(0.019)	(0.0099)	(0.012)
	[0.49]	[0.65]	[0.13]	[0.14]
Observations	6048	4563	6048	4563
B: Baseline-Unmarried Estimates by HIV Risk				
Treatment · Low Risk	0.094***	0.14***	0.049***	0.072***
	(0.031)	(0.044)	(0.015)	(0.021)
	[0.11]	[0.13]	[0.08]	[0.08]
Treatment · High Risk	-0.013	-0.11*	-0.0051	-0.036
	(0.054)	(0.065)	(0.024)	(0.030)
	[0.29]	[0.37]	[0.14]	[0.17]
Equality of coefficients (p-value)	0.09	0.00	0.06	0.00
Observations	3437	1996	3437	1996
C: Low-Risk Baseline-Unmarried Estimates by Attractiveness				
Treatment · Attractive	0.14***	0.15***	0.063***	0.094***
	(0.041)	(0.057)	(0.020)	(0.026)
	[0.08]	[0.12]	[0.06]	[0.06]
Treatment · Not Attractive	0.035	0.11	0.025	0.028
	(0.048)	(0.068)	(0.024)	(0.036)
	[0.16]	[0.15]	[0.10]	[0.12]
Equality of coefficients (p-value)	0.11	0.60	0.22	0.14
Observations	2306	1189	2306	1189
Sample	Full	Age ≥ 17	Full	Age ≥ 17

Note: Clustered standard errors appear in parentheses. Subgroup specific control group means appear in brackets. Estimate are not reweighted to balance by age. All regressions include Waves $2-8$. Estimates control for the baseline dependent variable and wave indicators. Columns 1 and 3 show estimates for the all ages while Columns 2 and 4 limit the sample to respondents over age 16, for whom age is already balanced. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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***
Preferences and Perceptions			
Thinks about future	3.11	3.32	0.00***
Subjective 5-year mort. risk	0.34	0.33	0.83
Subjective probability HIV positive	0.11	0.10	0.20
Worried about HIV	1.04	1.03	0.95
Outcomes			
Married	0.49	0.34	0.00***
Pregnant	0.15	0.10	0.02**
Ever tested for HIV (parity=0)	0.34	0.36	0.64
Ever tested for HIV (parity>0)	0.88	0.92	0.20
Partner may be HIV positive	0.46	0.45	0.79
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 (1)	Currently Pregnant (2)
A: Main Estimates		
Treatment	0.033*	0.023**
	(0.019)	(0.011)
	[0.57]	[0.14]
Observations	5037	5037
B: Baseline Unmarried Estimates by HIV Risk		
Treatment · Low Risk	0.095***	0.054***
	(0.036)	(0.018)
	[0.12]	[0.09]
Treatment · High Risk	-0.034	0.0083
	(0.067)	(0.029)
	[0.34]	[0.14]
Equality of coefficients (p-value)	0.09	0.18
Observations	2749	2749
C: Low-Risk Baseline-Unmarried Estimates by Attractiveness		
Treatment · Attractive	0.12**	0.060***
	(0.048)	(0.023)
	[0.09]	[0.07]
Treatment · Not Attractive	0.043	0.032
	(0.056)	(0.028)
	[0.18]	[0.12]
Equality of coefficients (p-value)	0.28	0.43
Observations	1818	1818

Note: Clustered standard errors appear in parentheses. Subgroup-specific control means appear in brackets. We limit the sample to respondents who are present in all survey waves. All regressions include Waves 2-8. Estimates control for the baseline dependent variable and wave indicators. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Baseline Characteristics by HIV Status and HIV Risk for Unmarried Respondents

	By HIV Status			By HIV Risk		
	HIV−	HIV+	P-value	Low-Risk	High-Risk	P-value
	(1)	(2)	(3)	(4)	(5)	(6)
Demographics						
Age	18.4	21.2	0.00***	18.1	19.6	0.00***
Ngoni Tribe	0.35	0.40	0.56	0.37	0.32	0.31
Yao Tribe	0.25	0.21	0.60	0.25	0.24	0.94
Lomwe Tribe	0.18	0.14	0.47	0.17	0.20	0.42
Catholic	0.38	0.56	0.03**	0.36	0.45	0.04**
Protestant	0.48	0.27	0.01***	0.48	0.39	0.05*
Muslim	0.16	0.17	0.84	0.16	0.15	0.84
Attractiveness	3.61	3.38	0.15	3.66	3.48	0.00***
Socioeconomic Status						
Enrolled in school	0.68	0.23	0.00***	0.80	0.50	0.00***
Employed full-time	0.07	0.36	0.00***	0.05	0.18	0.00***
Any savings	0.12	0.22	0.17	0.10	0.14	0.26
Household asset index	0.30	-0.14	0.01**	0.36	0.10	0.01**
Preferences and Perceptions						
Thinks about future	3.32	3.04	0.15	3.34	3.24	0.22
Subjective 5-year mort. risk	0.31	0.43	0.04**	0.29	0.39	0.00***
Subjective probability HIV+	0.08	0.34	0.00***	0.02	0.25	0.00***
Worried about HIV	0.73	1.18	0.03**	0.58	1.11	0.00***
Outcomes						
Pregnant	0.05	0.07	0.55	0.03	0.08	0.04**
Ever tested for HIV	0.44	0.58	0.09*	0.37	0.59	0.00***
Partner may be HIV positive	0.40	0.75	0.00***	0.33	0.57	0.00***
Respondents	552	37	-	401	188	-

Note: To compute p-values, we regress each variable on HIV status or HIV risk in Wave 1 and cluster standard errors by respondent. The "low-risk" group includes respondents who are HIV negative and also lack six additional risk factors, as explained in the text. The "high-risk" group includes respondents who are HIV-positive or have any of these risk factors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Baseline Characteristics by Attractiveness for Unmarried Respondents

	HIV-Negative Sample			Low-Risk Sample		
	Attractive	Unattractive	P-value	Attractive	Unattractive	P-value
	(1)	(2)	(3)	(4)	(5)	(6)
Demographics						
Age	18.3	18.4	0.75	18.2	17.7	0.17
Ngoni Tribe	0.36	0.35	0.98	0.38	0.36	0.61
Yao Tribe	0.23	0.27	0.36	0.23	0.28	0.27
Lomwe Tribe	0.19	0.19	0.98	0.17	0.18	0.80
Catholic	0.36	0.40	0.31	0.35	0.38	0.54
Protestant	0.50	0.43	0.17	0.51	0.45	0.28
Muslim	0.15	0.17	0.59	0.15	0.17	0.51
Socioeconomic Status						
Enrolled in school	0.76	0.59	0.00***	0.82	0.77	0.22
Employed full-time	0.04	0.10	0.02**	0.03	0.06	0.20
Any savings	0.10	0.14	0.26	0.09	0.09	0.89
Household asset index	0.58	-0.05	0.00***	0.59	0.01	0.00***
Preferences and Perceptions						
Thinks about future	3.54	3.04	0.00***	3.54	3.04	0.00***
Subjective 5-year mort. risk	0.33	0.30	0.24	0.30	0.26	0.23
Subjective probability HIV+	0.08	0.08	0.65	0.02	0.02	0.49
Worried about HIV	0.70	0.73	0.78	0.59	0.53	0.52
Outcomes						
Pregnant	0.05	0.04	0.68	0.05	0.01	0.04**
Ever tested for HIV	0.41	0.46	0.28	0.38	0.36	0.78
Partner may be HIV positive	0.37	0.45	0.23	0.30	0.38	0.41
Respondents	303	249	-	231	170	-

Note: To compute p-values, we regress each variable on attractiveness in Wave 1 and cluster standard errors by respondent. The "low-risk" group includes respondents who are HIV negative and also lack six additional risk factors, as explained in the text. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

	Treatment (1)	Control (2)	P-value (3)
Demographics			
Age	20.9	20.9	1.00
Attractiveness	3.49	3.57	0.10
Ngoni Tribe	0.39	0.37	0.56
Yao Tribe	0.31	0.27	0.11
Lomwe Tribe	0.15	0.16	0.67
Catholic	0.30	0.34	0.71
Protestant	0.46	0.47	0.69
Muslim	0.24	0.19	0.06*
HIV positive (endline)	0.13	0.07	0.01**
High HIV risk	0.60	0.56	0.26
Socioeconomic Status			
Enrolled in school	0.32	0.38	0.10
Employed full-time	0.19	0.17	0.48
Any savings	0.21	0.20	0.69
Household asset index	0.01	0.06	0.16
Preferences and Perceptions			
Thinks about future	3.33	3.44	0.03**
Subjective 5-year mort. risk	0.49	0.46	0.26
Subjective probability HIV positive	0.20	0.17	0.05*
Worried about HIV	1.93	2.40	0.38
Outcomes			
Married	0.50	0.49	0.92
Pregnant	0.12	0.14	0.34
Ever tested for HIV (parity= 0)	0.36	0.26	0.04**
Ever tested for HIV (parity> 0)	0.84	0.88	0.23
Partner may be HIV positive	0.47	0.45	0.44
Observations	453	451	-

Note: All means are weighted for age balance. To compute p-values, we regress each variable on treatment in Wave 1 and cluster standard errors by respondent. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

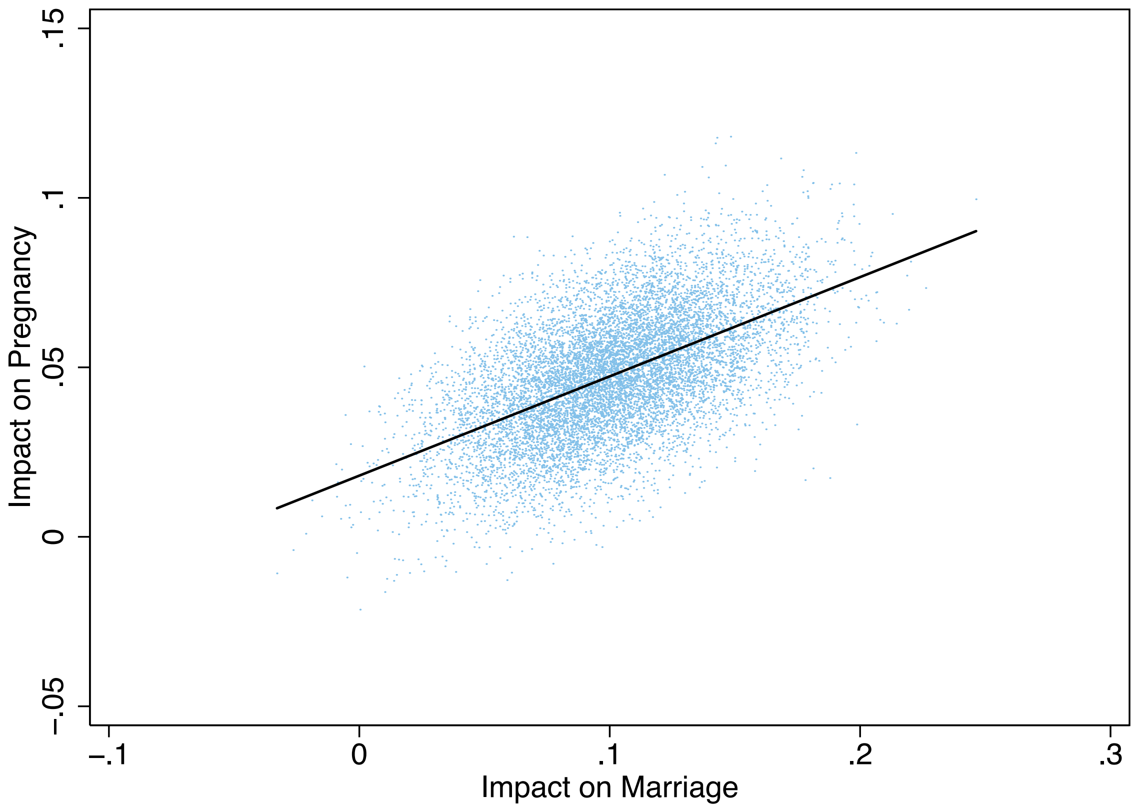

Figure A1: Effects on Marriage and Fertility for Baseline-Unmarried Low-Risk Respondents Across Bootstrap Replications

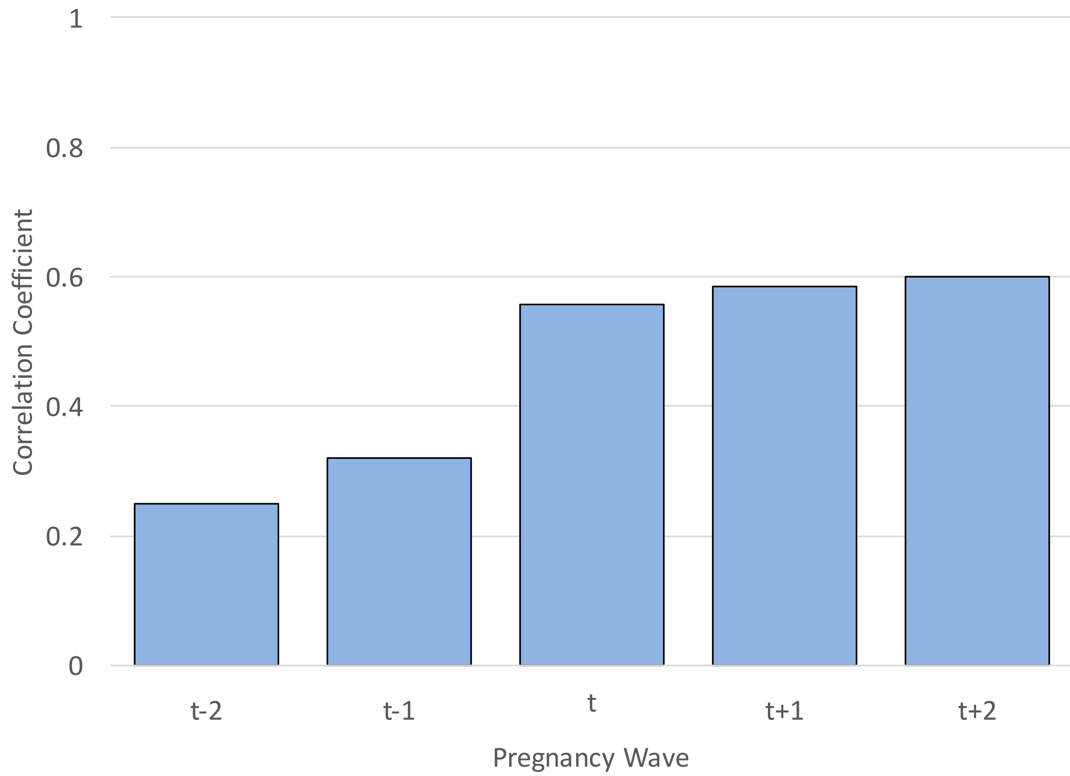

Figure A2: The Correlation Between the Treatment Effect on Marriage in Wave t and the Treatment Effect on Pregnancy in Waves $t-2$ to $t+2$

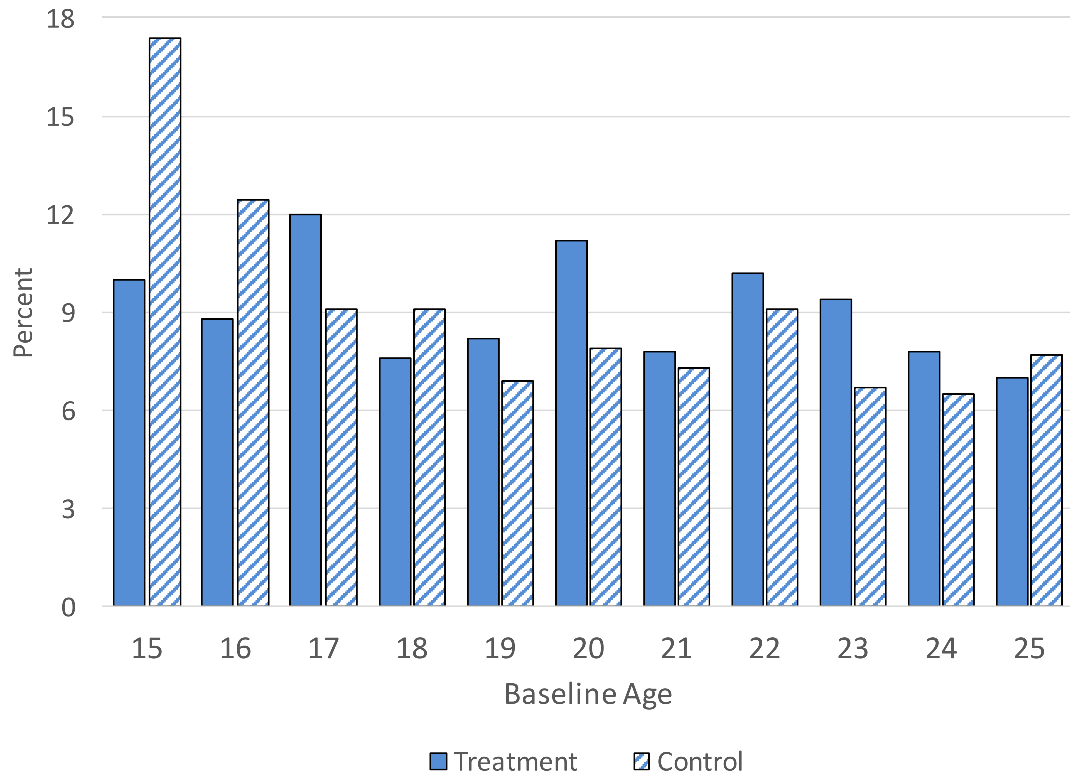

Figure A3: Age Distributions for the Treatment and Control Groups

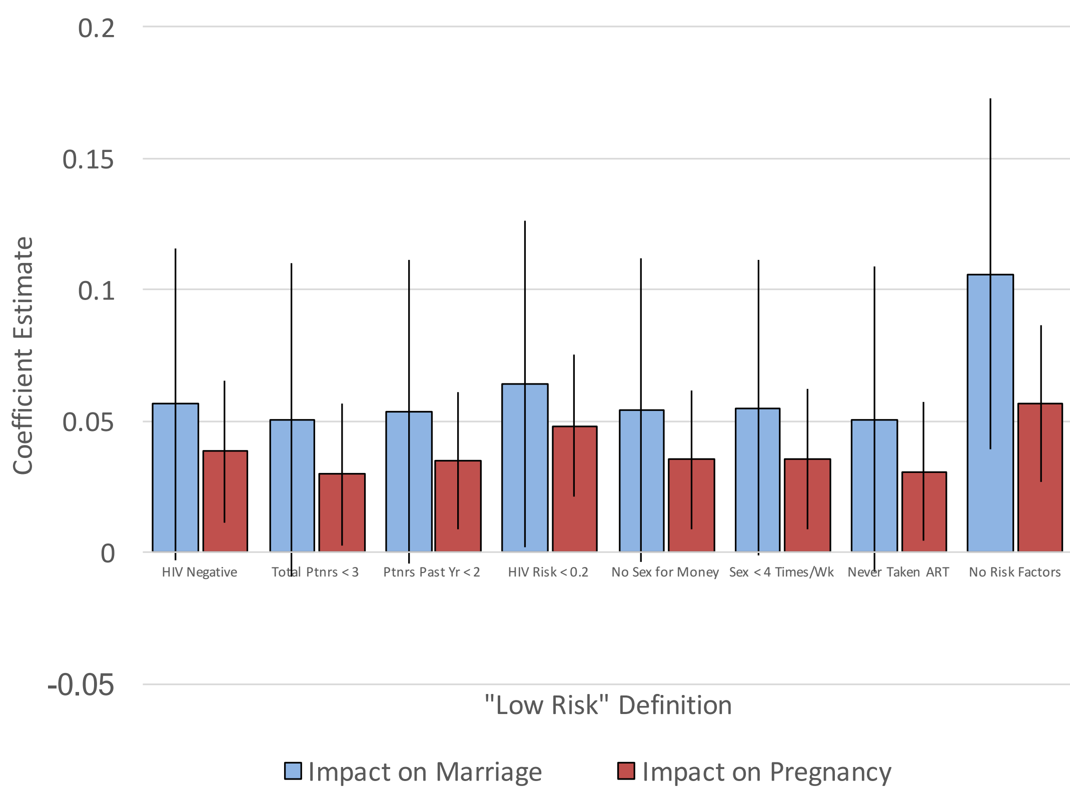

Figure A4: Estimated Treatment Effects on Marriage and Fertility for "Low-Risk" Respondents According to Seven Risk Factors

References

Adebowale, Stephen, Sunday Adedina, Latifat Ibisomi, and Martin Palamuleni, "Different effect of wealth quintile on modern contraceptive use and fertility: evidence from Malawian women," *BMC Women's Health*, March 2014, *14* (1).

Akerlof, George, "The Market for "Lemons": Quality Uncertainty and the Market Mechanism," *Quarterly Journal of Economics*, 1970, *89*, 488–500.

Angelucci, Manuela and Daniel Bennett, "Assortative Matching Under Asymmetric Information: Evidence from Malawi," *American Economic Review: Papers and Proceedings*, May 2017, *107* (5).

Attia, Suzanna, Matthias Egger, Monica Muller, Marcel Zwahlen, and Nicola Low, "Sexual transmission of HIV according to viral load and antiretroviral therapy: systematic review and meta-analysis," *AIDS*, 2009, *23* (11), 1397–1404.

Baird, Sarah, Erick Gong, Craig McIntosh, and Berk Ozler, "The Heterogeneous Effects of HIV Testing," *Journal of Health Economics*, September 2014, *37*, 98–112.

Becker, Gary, "A Theory of Marriage: Part 1," *Journal of Political Economy*, July 1973, *81* (4), 813–846.

——, *A Treatise on the Family*, Cambridge, MA: Harvard University Press, 1981.

Beegle, Kathleen, Michelle Poulin, and Gil Shapira, "HIV Testing, Behavior Change, and the Transition to Adulthood," *Economic Development and Cultural Change*, July 2015, *63* (4), 665–684.

Berendes, Sima and Rajiv Rimal, "Addressing the Slow Uptake of HIV Testing in Malawi: The Role of Stigma, Self-efficacy, and Knowledge in the Malawi BRIDGE Project," *Journal of the Association of Nurses in AIDS Care*, May/June 2011, *22* (3), 215–228.

Berge, Erling, Daimon Kambewa, Alister Munthali, and Henrick Wiig, "Lineage and land reforms in Malawi: Do matrilineal and patrilineal landholding systems represent a problem for land reforms in Malawi?," *Land Use Policy*, November 2014, *41*, 61–69.

Boileau, C, S Clark, S Bignami-Van Assche, M Poulin, G Reniers, S Watkins, HP Kohler, and S Heymann, "Sexual and marital trajectories and HIV infection among ever-married women in rural Malawi," *Sexually Transmitted Infections*, 2009, *85* (Suppl 1), i27–i33.

Bongaarts, John, "Late marriage and the HIV epidemic in sub-Saharan Africa," *Population Studies*, 2007, *61* (1), 73–83.

Chesney, Margaret and Ashley Smith, "Critical Delays in HIV Testing and Care: the Potential Role of Stigma," *American Behavioral Scientist*, April 1999, *42* (7), 1162–1174.

Chiappori, Pierre-Andre, Sonia Oreffice, and Climent Quintana-Domeque, "Fatter Attraction: Anthropometric and Socioeconomic Matching on the Marriage Market," *Journal of Political Economy*, August 2012, *120* (4), 659–695.

Chimbiri, Agnes, "The condom is an 'intruder' in marriage: Evidence from rural Malawi," *Social Science and Medicine*, March 2007, *64* (5), 1102–1115.

Delavande, Adeline and Hans-Peter Kohler, "The Impact of HIV Testing on Subjective Expectations and Risky Behaviors in Malawi," *Demography*, August 2012, *49* (3), 1011–36.

___ **, Xavier Gine, and David McKenzie**, "Measuring subjective expectations in developing countries: A critical review and new evidence," *Journal of Development Economics*, 2011, *94* (2), 151–163.

___ **, Zachary Wagner, and Neeraj Sood**, "The Impact of Repeat HIV Testing on Risky Sexual Behavior: Evidence from a Randomized Controlled Trial in Malawi," *Journal of AIDS and Clinical Research*, March 2016, *7* (3).

Doherty, Tanya, Hanani Tabana, Debra Jackson, Reshma Naik, Wanga Zemba, Carl Lombard, Sonja Swanevelder, Matthew Fox, Anna Torson, Anna Mia Ekstrom, and Mickey Chopra, "Effect of home based HIV counselling and testing intervention in rural South Africa: cluster randomised trial," *BMJ*, June 2013, *346.*

Edwards, Ryan and Jennifer Roff, "What mom and dad's match means for junior: Marital sorting and child outcomes," *Labour Economics*, June 2016, *40*, 43–56.

Fisman, Raymond, Sheena Iyengar, Emir Kamenica, and Itamar Simoson, "Gender Differences in Mate Selection: Evidence from a Speed Dating Experiment," *Quarterly Journal of Economics*, May 2006, *121* (2), 673–697.

Gale, D. and L.S. Shapley, "College Admissions and the Stability of Marriage," *American Mathematical Monthly*, August 1962, *69*, 9–16.

Gong, Erick, "HIV Testing and Risky Sexual Behavior," *Economic Journal*, February 2015, *125* (582), 32–60.

Gray, Ronald, Maria Wawer, Ron Brookmeyer, Nelson Sewankambo, David Serwadda, Fred Wabwire-Mangen, Tom Lutalo, Xianbin Li, Thomas vanCott, Thomas Quinn, and the Rakai Project Team, "Probability of HIV-1 transmission per coital act in monogamous, heterosexual, HIV-1-discordant couples in Rakai, Uganda," *The Lancet*, April 14 2001, *357*, 1149–1153.

Hainmueller, Jens, "Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies," *Political Analysis*, 2012, *20*, 25–45.

___ **and Yiqing Xu**, "ebalance: A Stata Package for Entropy Balancing," *Journal of Statistical Software*, 2013, *54* (7).

Hitsch, Gunter, Ali Hortacsu, and Dan Ariely, "Matching and Sorting in Online Dating," *American Economic Review*, 2010, *100* (1), 130–163.

Jensen, Robert and Rebecca Thornton, "Early female marriage in the developing world," *Gender and Development*, July 2003, *11* (2), 9–19.

Kaler, Amy, ""Many Divorces and Many Spinsters": Marriage as an Invented Tradition in Southern Malawi, 1946-1999," *Journal of Family History*, October 2001, *26* (4), 529–556.

——, "'When They See Money, They Think it's Life': Money, Modernity, and Morality in Two Sites in Rural Malawi," *Journal of Southern African Societies*, June 2006, *32* (2), 335–349.

Kalichman, SC and LC Simbayi, "HIV testing attitudes, AIDS stigma, and voluntary HIV testing and counselling in a black township in Cape Town, South Africa," *Sexually Transmitted Infections*, 2003, *79* (6), 442–447.

Kennedy, Caitlin, Virginia Fonner, Michael Sweat, F. Amolo Okero, Rachel Baggaley, and Kevin O'Reilly, "Provider-Initiated HIV Testing and Counseling in Low- and Middle-Income Countries: A Systematic Review," *AIDS and Behavior*, 2013, *17* (5), 1571–1590.

Low, Corrine, Cristian Pop-Eleches, Winnie Rono, Evan Plous, Angeli Kirk, Samson Ndege, Markus Goldstein, and Harsha Thirumurthy, "The effects of home-based HIV counseling and testing on HIV/AIDS stigma among individuals and community leaders in western Kenya: Evidence from a cluster-randomized trial," *AIDS Care*, June 2013, *25* (Supplement 1), 97–107.

Magruder, Jeremy, "Marital Shopping and Epidemic AIDS," *Demography*, November 2011, *48* (4), 1401–1428.

Maughan-Brown, Brendan and Laura Nyblade, "Different Dimensions of HIV-Related Stigma May Have Opposite Effects on HIV Testing: Evidence Among Young Men and Women in South Africa," *AIDS and Behavior*, May 2014, *18* (5), 958–965.

Meekers, Dominique, "The Process of Marriage in African Societies: A Multiple Indicator Approach," *Population and Development Review*, March 1992, *18* (1), 61–78.

Ngatia, Muthoni, "Social Interactions and Individual Reproductive Decisions," November 2011. Unpublished manuscript.

Oni, SA, CL Obi, A Okone, D Thabede, and A Jordaan, "The Economic Impact of HIV/AIDS on Rural Households in Limpopo Province," *South African Journal of Economics*, 2002, *70* (7), 551–562.

Oreficce, Sonia and Climent Quintana-Domeque, "Attractiveness, Anthropometry, or Both? Their Relationship and Role in Economic Research," October 2014. IZA Discussion Paper 8527.

Pinto, Andrew, Monique van Lettow, Beth Rachlis, Adrienne Chan, and Sumeet Sodhi, "Patient Costs Associated with Accessing HIV/AIDS Care in Malawi," *Journal of the International AIDS Society*, March 2013, *16*, 1–6.

Piwowar-Manning, Estelle, Nancy Tustin, Physiwell Sikateyo, Debbie Kamwendo, Chifundo Chipungu, Rashika Maharaj, Jabulani Mushanyu, Barbra Richardson, Sharon Hillier, and J. Brooks Jackson, "Validation of Rapid HIV Antibody Tests in Five African Countries," *Journal of the International Association of Physicians in AIDS Care*, 2010, *9* (3), 170–172.

Poulin, Michelle, "Sex, Money, and Premarital Relationships in Southern Malawi," *Social Science and Medicine*, December 2007, *65* (11), 2383–2393.

Rasul, Imran, "Household bargaining over fertility: Theory and evidence from Malaysia," *Journal of Development Economics*, June 2008, *86* (2), 215–241.

Reniers, Georges, "Marital Strategies for Regulating Exposure to HIV," *Demography*, May 2008, *45* (2), 417–438.

Sambisa, William, Sian Curtis, and Vinod Mishra, "AIDS stigma as an obstacle to uptake of HIV testing: evidence from a Zimbabwean national population-based survey," *AIDS Care*, February 2010, *22* (2), 170–186.

Schatz, Enid, "'Take your mat and go!': Rural Malawian women's strategies in the HIV/AIDS era," *Culture, Health, and Sexuality*, Sept/Oct 2005, *7* (5), 479–92.

Smith, Kirsten and Susan Watkins, "Perceptions of Risk and Strategies for Prevention: Responses to HIV/AIDS in Rural Malawi," *Social Science and Medicine*, February 2005, *60* (3), 649–660.

Tavory, Iddo and Ann Swindler, "Condom Semiotics: Meaning and Condom Use in Rural Malawi," *Americal Sociological Review*, April 2009, *74*, 171–189.

Thornton, Rebecca, "The Demand for, and Impact of, Learning HIV Status," *American Economic Review*, December 2008, *98* (5), 1829–1863.

UNAIDS, "The Gap Report," 2014.

Yeatman, Sara and Christie Sennott, "The Relationship Between Partners' Family-Size Preferences in Southern Malawi," *Studies in Family Planning*, September 2014, *45* (3), 361–377.

Young, Sean and Yuda Zhu, "Behavioral Evidence of HIV Testing Stigma," *AIDS and Behavior*, April 2012, *16* (3), 736–740.