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

Manuela Angelucci*
Daniel Bennett†
Jenny Trinitapoli‡

September 27, 2016

Abstract

Asymmetric information in the marriage market may delay marriage and reduce marital surplus through adverse selection. A signaling and screening technology may increase the marriage rate and the marital surplus of high-quality participants. HIV status is an important hidden partner attribute in HIV-endemic settings. We test this model through a randomized evaluation of an intensive "opt-out" HIV testing intervention that offered free tests to young women and their partners every four months for 2.3 years. This intervention increases the in-sample marriage rate by 27 percent and the probability of pregnancy by 26 percent for baseline-unmarried women. Impacts are largest for beautiful women, suggesting that these observable and hidden quality attributes are complements. To relate our findings to the literature, we show that an alternative single-test intervention does not have these effects.

JEL: J12, J13, I15, I18

We received helpful feedback from Dan Black, 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.
†Harris School of Public Policy Studies, University of Chicago, dmbennett@uchicago.edu
‡Department of Sociology, University of Chicago, jennytrini@uchicago.edu.

1 Introduction

In the marriage market, some important partner attributes such as finances, character, and health are difficult to observe during courtship. With few exceptions (e.g. Bergstrom and Bagnoli 1993, Weiss and Willis 1997), researchers have not yet considered the potential ramifications of asymmetric information in this setting.

This paper examines the impact of asymmetric information in the marriage market when marital surplus grows with partner's quality, and when one partner's trait, e.g., attractiveness, is always observed, but another one, e.g., health, is observed only over time. Partners always match in attractiveness. However, initially market participants do not know the health of others. In this case, high-health participants face a trade-off: enjoy a lower marital surplus for longer by marrying early, but at the risk of matching with a low-health partner, or enjoy a higher surplus for less time by marrying later to a high-health partner. Making health observable early increases the marriage rate and the marital surplus of high-health participants, since they can all marry early to a high-health partner. Low-health participants, conversely, always marry early, as they have no gains from waiting, and some marry high-health participants. Making health observable early reduces their marital surplus, as only low-health partners marry them once quality is observable, without changing the timing of marriage.

We test the implications of this model in the context of rural Malawi, where HIV prevalence is around 10 percent and HIV status is an important partner attribute. While HIV greatly diminishes a partner's contribution to household productivity, health, and financial security, the HIV status of partners is difficult to observe. Indicators that someone could be positive – visible AIDS symptoms, consumption of AIDS medicines, or non-monogamous behavior – may be evident only over time and with repeated interaction. Many HIV-negative singles fear that the spouse they choose could turn out to have HIV. Mukiza-Gapere and Ntozi (1995) argue that in Uganda, the fear of HIV has "poured cold water on the institution of marriage."

HIV testing has the potential to resolve HIV status uncertainty among partners, under the right circumstances. By testing frequently and sharing their results, HIV-negative people can signal their status. They can also screen potential partners according to the willingness to be tested. However, when testing is costly and others test infrequently, declining to be tested does not send a negative signal. This is the case in the status quo, an "opt-in" model in which a test seeker must take action by traveling for several hours and queueing in public at the testing facility (Pinto et al. 2013). Observers may infer (and gossip to others) that anyone willing to undergo this process must be worried about HIV (Chesney and Smith 1999, Ngatia 2011, Young and Zhu 2012). Conversely, to facilitate information provision among partners, HIV testing must be so inexpensive and convenient that seeking a test is free of negative connotations.

An intensive HIV testing intervention embedded in the Tsogolo La Thanzi (TLT) Panel Study allows us to examine how switching to a high-frequency, "opt-out" testing model alleviates asymmetric information in the marriage market, affecting marriage timing and surplus. The TLT Panel Study follows a representative sample of women aged $15-25$ and their partners over eight waves (spaced four months apart) from 2009 to 2012. Randomly selected intervention participants were offered an HIV test after every survey wave, while control participants were tested only once, at the end of the study. Surveyors made testing as convenient as possible: tests were conducted for free, on-site, immediately after the TLT interviews. In contrast to the status quo, participants needed to opt out of testing, rather than actively seek it. Partners of intervention women also received the intervention, enabling signaling and screening for both partners. In this setting, someone who refuses to be tested sends a signal that he or she may be HIV positive. Not testing repeatedly repeatedly, as this intervention allows, may strengthen the signal.

Our main outcomes are marital status and fertility, as well as perceptions of partner's HIV status. Under conditions that we discuss further below, fertility is a proxy for marital surplus. We find that our testing intervention increased the marriage probability by 3.5

percent and the pregnancy probability by 16 percent. Moreover, HIV-negative women who are unmarried at baseline respond the most to the intervention, as our model predicts. The probability of marriage increases by 33 percent and the probability of pregnancy increases by 40 percent for this group.

To assess the model further, we examine heterogeneous treatment effects by respondent attractiveness. Physical appearance, which we observe in our data, is an important observable marriage market attribute for women in this setting, while earnings potential, proxied by education, is a key attribute for men. Under positive assortative matching, the most beautiful women marry the men with the highest education. In this case, our model predicts that attractive women may be more likely to delay marriage. This may occur if attractiveness and health are complementary determinants of marital surplus, in which case attractive women have the most to lose by selecting an HIV-positive spouse, or if HIV prevalence is higher among attractive people, as is the case in our data. Consistent with the model's prediction, we find that the intervention has a 93 percent impact on the probability of marriage and a 58 percent impact on the probability of pregnancy for this group.

We also show that multiple features of the data are consistent with this theoretical framework. Although it has a minimal effect on own HIV status perceptions, the intervention improves the precision of beliefs about partner's HIV status. This finding supports the assumption that HIV status information is asymmetric. Secondly, baseline patterns in the data are consistent with assortative matching in terms of attractiveness and adverse selection on health. Attractive women marry more highly educated men and have more children, while HIV-positive women are more likely to be married than HIV-negative women at baseline. The additional correspondence between the model and our data increase our confidence that the model reflects the data.

Finally, we contrast our findings with existing HIV testing evaluations. Most HIV testing evaluations assess the impact of a single HIV test. Effects are small and appear to depend on the test outcome and whether the individual was surprised by the news.[1] Our study includes a third experimental arm that received a single HIV test midway through the study period. This alternative intervention has no effects on marriage or fertility, suggesting that the prolonged availability of HIV testing is an important feature of the intervention.

This paper makes the following primary contributions. We provide one of the first empirical examinations of asymmetric information and adverse selection in the marriage market when one trait is observed and one is not observed initially. Our conclusions are applicable elsewhere because unobservable partner quality (in terms of earnings potential, personality traits, or other household inputs) is an intrinsic feature of marriage markets in many settings. Secondly, we contribute to the literature on HIV testing by showing a novel impact of HIV testing. In 2007, the World Health Organization (WHO) issued guidelines recommending that countries and organizations adopt provider-initiated testing and counseling, our "opt-out" model, to increase HIV testing rates. Since then, HIV testing uptake has increased considerably (**?**). Therefore, it is important to consider how this new approach to testing affects marriage timing and surplus through its possible role as signals to others. Finally, we provide an alternative explanation for the positive correlation between HIV and age of marriage that has been observed in Sub-Saharan Africa both cross-sectionally and over time. Existing research conjectures that delays in marriage lead to higher HIV prevalence because transmission occurs during courtship (Bongaarts 2007, Magruder 2011). A complementary hypothesis, consistent with our theory and evidence, is that HIV prevalence leads to delays in marriage in response to adverse selection.

[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) shows 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 effect of a one-off testing intervention on school attendance, marriage, and fertility.

2 Theory

2.1 Setup

Consider a transferable utility setting with a continuum of men and women who live for two periods, $t \in \{1, 2\}$. In each period, people decide whether to participate in the marriage market and whom to marry. People receive surplus S, defined as the difference between the sum of the spouses' utility functions as married and singles, for one period of marriage and face discount factor $\delta > 0$, which can be greater than one because Period 2 lasts longer than Period 1. The discount factor is private information, varies across subjects, and is distributed uniformly between 0 and b: $\delta \sim U[0, b]$. Therefore, a person who marries in Period 1 has lifetime surplus $(1 + \delta)S$. People do not die and divorce is too costly, so that people who marry in Period 1 remain with their partners in Period 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 agents, defined by their attractiveness and health, with population shares p_{hh}, p_{lh}, p_{hl}, and p_{ll}, which sum up to one and are common knowledge. Attractiveness is observed by everyone in both periods. This dimension represents any other salient partner attribute, such as physical beauty for women and wealth for men. While people always know their own health, the health of others becomes observable only in Period 2. If a woman with attractiveness a and health b and a man with attractiveness c and health d marry, they generate a surplus $S^{ab}_{cd} > 0$ (where the superscript references the woman and the subscript references the man).

Attractive and healthy spouses generate more surplus than, respectively, unattractive and unhealthy ones. From the woman's perspective, a woman of attractiveness a and health b would enjoy the highest surplus by marrying a man with two high traits, the second highest surplus by marrying a man with one high trait, and the lowest surplus by marrying a man with no high traits, $S^{ab}_{hh} > S^{ab}_{hl} = S^{ab}_{lh} > S^{ab}_{ll} > 0$. Finally, in the population, attractiveness, health, and the discount factor are independent of gender, and attractiveness and health are independent of the discount factor.

2.2 Status Quo Equilibrium

Other people's health is unobservable in Period 1. Therefore, people must decide whether to marry in Period 1 or postpone marriage until Period 2 according to their beliefs of the distribution of healthy and unhealthy people in the pool at this point. The proportion of healthy people in Period 1 is a function of the beliefs and strategies of all market participants. We rely on the Perfect Bayesian Equilbrium solution concept, by which people correctly perceive the shares of each type who seek to marry in each period. The remainder of this section considers women's choices, to be consistent with our empirical exercise.

Consider an exogenous matching process that ranks people based on the number of observed high traits and then matches men and women of equal ranks. This setup generates a unique equilibrium in the marriage market in each Period, in which people marry assortatively in attractiveness in Period 1 and in the number of high traits in Period 2, all unhealthy women marry in Period 1 and some healthy women marry in Period 2, inducing adverse selection. We discuss the details in the following five Propositions.

Proposition 1. *For all people who choose to marry in Period 2, there exists a unique equilibrium in Period 2 in which people marry assortatively in the number of high traits.*

Proof. Since attractiveness, health, and the discount factor have the same the distribution for men and women and are independent of each other, there are equal numbers of marriageable men and women with the same attractiveness and health in each period. Therefore, attractive and healthy people marry each other and enjoy a surplus of S_{hh}^{hh}. Similarly, women with one high trait also marry men with one high trait and enjoy a surplus of S_{hl}^{lh}.[2] Therefore, people without high traits must also marry each other and enjoy a surplus of S_{ll}^{ll}. This equilibrium is stable, that is, no married person are better off unmarried, and no two people

[2]Note that all the following surpluses are identical by assumption: $S_{hl}^{lh} = S_{lh}^{lh} = S_{hl}^{hl} = S_{lh}^{hl}$.

prefer being married together than to their current spouse. This occurs because, while any person would prefer to marry a partner with more high traits than one's own, nobody wants to marry a partner with fewer high traits than one's own.

Proposition 2. *For all people who choose to marry in Period 1, there exists a unique equilibrium in Period 1 in which people marry assortatively in attractiveness.*

Proof. For women who choose to marry in Period 1, attractive women maximize surplus by marrying attractive men because the surplus from marrying an attractive partner is always (weakly) greater than the surplus from marrying an unattractive partner, since the former has (weakly) more high traits in expectation. Given that attractive people marry each other, unattractive people must marry each other too.[3]

Under sufficiently low divorce costs, this equilibrium would be unstable, since all people married to partners with fewer high traits than their own would prefer being married together to their current situation. We discuss this in section A.

Proposition 3. *Unhealthy women always marry in Period 1.*

Proof. Unhealthy women of attractiveness a, al types, marry early if the surplus from doing so is greater than the surplus from marrying late, that is, if the following inequality holds:

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

where p_{al} and p_{ah} are the population proportions of low and high health types with attractiveness a. Since $S^{al}_{ah} > S^{al}_{al}$, this expression is always true. Unhealthy women who marry in Period 1 receive additional surplus in two ways. First, they are married for longer, so they generate marital surplus for more time. Second, by marrying early, they may find a healthy spouse, who is unaware that they are unhealthy, and, obtain a higher marital surplus than if

[3]Marriage may not be assortative in attractiveness if health and attractiveness are correlated. For example, if they are negatively correlated, attractive people may prefer to marry unattractive people if their expected health is sufficiently higher. In this scenario, unlike in our model, HIV testing would cause people to match assortatively in attractiveness.

they married an unhealthy partner. This pattern does not depend on attractiveness. That is, both attractive and unattractive unhealthy women marry in Period 1.

Proposition 4. *Healthy women with sufficiently low discount factors marry in Period 1.*

Proof. The population proportion of healthy women of attractiveness a who marry in Period 1, μ_{ah}, can vary between 0 and the entire population proportion p_{ah}, $\mu_{ah} \in [0; p_{ah}]$. The parameter μ_{ah} depends on the marriage surpluses and the discount factor δ. We derive $\bar{\delta}_a$ considering the choice of healthy women of attractiveness a, who marry early if the expected surplus of marrying in Period 1 exceeds the surplus of marrying in Period 2, that is:

$$(1+\delta)\frac{p_{al}S_{al}^{ah} + \mu_{ah}S_{ah}^{ah}}{(p_{al} + \mu_{ah})} > \delta S_{ah}^{ah}. \tag{2}$$

Solving for δ yields an expression for $\bar{\delta}_a$, the threshold value that distinguishes healthy women who marry early and late. Healthy women of attractiveness a marry in Period 1 if they are sufficiently impatient that $\delta < \bar{\delta}_a$, where:

$$\bar{\delta}_a = \frac{\mu_{ah}S_{ah}^{ah} + p_{al}S_{al}^{ah}}{p_{al}(S_{ah}^{ah} - S_{al}^{ah})} > 0 \tag{3}$$

Equation (3) is always positive, because both its numerator and its denominator are positive, and depends on μ_{ah}, the share of other healthy people who marry early.

Since δ is distributed uniformly between 0 and b, we know that $\mu_{ah} = F(\bar{\delta}) = \bar{\delta}_a/b$ or that $\mu_{ah}b = \bar{\delta}_a$. Setting this expression equal to Equation (3) and solving for μ_{ah} gives:[4]

$$\mu_{ah}^* = \frac{1}{b(r_{ah} - 1) - \frac{r_{ah}}{p_{al}}}, \tag{4}$$

where r_{ah} is the ratio of marital surplus for a type ah woman who marries a healthy man over an unhealthy man, $r_{ah} = \frac{S_{ah}^{ah}}{S_{al}^{ah}}$, which is greater than 1 by assumption.

[4]You can substitute μ_{ah}^* into Equation (3) to obtain an expression for $\bar{\delta}_a^*$ in terms of b, S_{ah}^{ah} and S_{al}^{ah}: $\bar{\delta}^* = \frac{S_{ah}^{ah}S_{al}^{ah}}{bp_{al}(S_{ah}^{ah} - S_{al}^{ah})^2 - S_{ah}^{ah}(S_{ah}^{ah} - S_{al}^{ah})} + \frac{S_{al}^{ah}}{S_{ah}^{ah} - S_{al}^{ah}}$.

The variable μ^*_{ah} is positive only for values of b considerably greater than one, which occurs if Period 2 lasts many years. This is the case because the term $r_{ah} - 1$ in the denominator is positive and the last term in the denominator, $\frac{r_{ah}}{p_{al}}$, is greater than one. That is, $b > \frac{r_{ah}}{p_{al}(r_{ah}-1)}$.[5]

This equation shows that $\frac{\partial \mu^*_{ah}}{\partial b} < 0$, $\frac{\partial \mu^*_{ah}}{\partial p_{al}} < 0$, and $\frac{\partial \mu^*_{ah}}{\partial r_{ah}} < 0$, that is, the optimal fraction of type ah women who prefers an early marriage decreases with patience, which corresponds to a higher b, with the proportion of unhealthy types, p_{al}, and with the marital surplus ratio, r_{ah}.

Given that people have correct beliefs, there are no incentives to deviate from this equilibrium. If a sufficiently patient woman, that is, a woman whose $\delta > \bar{\delta}_a$, entered the marriage market in Period 1, her expected surplus would be lower than the surplus from waiting to marry in Period 2.

Proposition 5. *Under some conditions, a higher proportion of attractive and healthy women, types hh, marry in Period 2, than unattractive and healthy women, types lh. That is,* $\mu_{hh} < \mu_{lh}$.

Proof. From Equation 4, it follows that $\mu_{hh} < \mu_{lh}$ if:

$$\frac{S^{hh}_{hh}}{S^{hh}_{hl}}(b - \frac{1}{p_{hl}}) > \frac{S^{lh}_{lh}}{S^{lh}_{ll}}(b - \frac{1}{p_{ll}}) \tag{5}$$

This inequality may hold for either of two reasons. First, it may hold when the population proportions of unhealthy men is higher among the attractive types, i.e., $p_{hl} > p_{ll}$, and the gain that healthy women experience by marrying a healthy man rather than an unhealthy man is independent of women's attractiveness, i.e., $\frac{S^{hh}_{hh}}{S^{hh}_{hl}} = \frac{S^{lh}_{lh}}{S^{lh}_{ll}}$. In this case there are more "lemons" among attractive types, i.e., $p_{hl} > p_{ll}$, and the relative gain that healthy women experience by marrying a healthy man rather than an unhealthy man does not vary

[5]The variable μ^*_{ah} is also less than or equal to p_{ah}. This occurs for values of b such that $b \geq \frac{p_{al}+p_{ah}r_{ah}}{p_{al}p_{ah}(r_{ah}-1)}$.

with attractiveness, i.e., $\frac{S^{hh}_{hh}}{S^{hh}_{hl}} = \frac{S^{lh}_{lh}}{S^{lh}_{ll}}$.[6] Second, it may hold when $p_{hl} = p_{ll}$ and $\frac{S^{hh}_{hh}}{S^{hh}_{hl}} > \frac{S^{lh}_{lh}}{S^{lh}_{ll}}$. In this case, the proportion of "lemons" does not vary by attractiveness, but the relative gain that healthy women experience by marrying a healthy man rather than an unhealthy man is larger for attractive women than for unattractive women. This happens if, for example, the current low health of the husband causes a decrease in his future productivity. In that case, the future income generated by the husband would be much lower than the current one.[7]

In our data, $p_{hl} = 0.056$ and $p_{ll} = 0.042$, that is, $p_{hl} > p_{ll}$. Therefore, a higher proportion of attractive women prefers to wait to marry in Period 2 when $\frac{S^{hh}_{hh}}{S^{hh}_{hl}} = \frac{S^{lh}_{lh}}{S^{lh}_{ll}}$.[8] Therefore, we expect that healthy and attractive women in our data are more likely to postpone marriage than healthy and unattractive women.

This equilibrium has the following implications for Period 1, which we can check in our data to validate our theoretical setup:

1. By assumption, marital surplus is bigger for attractive than for unattractive women and for healthy than unhealthy women.

2. According to Proposition 2, the likelihood of being married to an attractive man is higher for attractive women than for unattractive women. Conversely, women's health and husbands' education should not be correlated because HIV status is hard to observe.

3. According to Propositions 3 and 4, the likelihood of being married is higher for unhealthy women than healthy women.

4. According to Proposition 5, the likelihood of being married is higher for unattractive women than attractive women.

[6] This ratio is constant for surplus functions such as $S^{ab}_{cd} = abcd$ or $S^{ab}_{cd} = (a + c)(b + d)$, $\forall l, h > 1$.

[7] This case can be can be illustrated by the following surplus, $S^{ab}_{cd} = (ac)^{bd}$, $\forall l, h > 1$.

[8] Moreover, the values of p_{hl} and p_{ll} in our data are such that the inequality in equation (5) holds also in some cases in which $\frac{S^{hh}_{hh}}{S^{hh}_{hl}} > \frac{S^{lh}_{lh}}{S^{lh}_{ll}}$.

2.3 Making Health Observable in Period 1

In this Section, we describe how making health observable in Period 1 changes the timing and the surplus of marriages for some women. To tie the theory to our data, which cover a short time span and an age interval in which people are choosing a lifetime partner, we consider the effects on marital state and surplus in Period 1.

To begin with, we consider the case in which one's health is private information in Period 1. In this setting, unhealthy women, al types, marry in Period 1 with probability 1. Their Period 1 expected surplus, $E[S^{al}]_1$ is:

$$E[S^{al}]_1 = \frac{(\mu^*_{ah} S^{al}_{ah} + p_{al} S^{al}_{al})}{p_{al} + \mu^*_{ah}}. \tag{6}$$

Healthy women, ah types, marry in Period 1 with probability $\frac{\mu^*_{ah}}{p_{ah}}$ and in Period 2 with probability $\frac{p_{ah}-\mu^*_{ah}}{p_{ah}}$. Their Period 1 expected surplus, $E[S^{ah}]_1$, is:[9]

$$E[S^{ah}]_1 = (\frac{\mu^*_{ah}}{p_{ah}}) \frac{(\mu^*_{ah} S^{ah}_{ah} + p_{al} S^{ah}_{al})}{(p_{al} + \mu^*_{ah})}. \tag{7}$$

Suppose that health becomes observable in Period 1. In this case, all women marry a partner with the same number of high traits in Period 1 and nobody postpones marriage to Period 2.

Consider some treatment, T, that makes health observable in Period 1 ($T = 1$) or not ($T = 0$), and some outcome of interest, Y (marital status and surplus, in our case). We can measure the average causal effect of making health observable in Period 1 by comparing the average outcomes when health is and is not observable.

According to our model, the treatment has no effect on marital status for unhealthy women, because they all marry in Period 1 regardless of whether health is observable. That

[9] Lifetime marriage surpluses for unhealthy and healthy women are $E[S^{al}] = \frac{(1+\delta)(\mu^*_{ah} S^{al}_{ah} + p_{al} S^{al}_{al})}{p_{al} + \mu^*_{ah}}$ and $E[S^{ah}] = (\frac{\mu^*_{ah}}{p_{ah}}) \frac{(1+\delta)(\mu^*_{ah} S^{ah}_{ah} + p_{al} S^{ah}_{al})}{(p_{al} + \mu^*_{ah})} + (\frac{p_{ah} - \mu^*_{ah}}{p_{ah}}) \delta S^{ah}_{ah}$.

is,

$$E[M_1^{al}|T=1] - E[M_1^{al}|T=0] = 0. \tag{8}$$

Conversely, the treatment increases the likelihood that healthy women marry in period one ($M_1^{ah} = 1$) from $\frac{\mu^*_{ah}}{p_{ah}}$ to 1. That is,

$$E[M_1^{ah}|T=1] - E[M_1^{ah}|T=0] = 1 - \frac{\mu^*_{ah}}{p_{ah}} \geq 0. \tag{9}$$

This occurs because the women who would have been better off by waiting to find out the health of potential partners can do so in Period 1, and, therefore, marry earlier.[10]

Recall that, in our data, the share of the population which is either attractive and healthy or unattractive and healthy are approximately the same, $p_{hh} = 0.447 \approx p_{lh} = 0.454$. Therefore, the treatment effect is bigger for attractive healthy women than for unattractive healthy women, as long as $\mu^*_{hh} < \mu^*_{lh}$, consistent with our data.

The treatment reduces average Period 1 marital surplus for unhealthy women from $\frac{(\mu^*_{ah} S^{al}_{ah} + p_{al} S^{al}_{al})}{p_{al} + \mu^*_{ah}}$ to S^{al}_{al}. That is,

$$E[S_1^{al}|T=1] - E[S_1^{al}|T=0] = -\frac{\mu^*_{ah}(S^{al}_{ah} - S^{al}_{al})}{(p_{al} + \mu^*_{ah})} < 0. \tag{10}$$

This negative treatment effect occurs because the treatment reveals these women's low health status, and therefore these women can marry unhealthy partners only. Note that the overall average treatment effect on Period 1 surplus is likely positive because there are many more healthy than unhealthy types in the population.

Conversely, the treatment increases the average Period 1 marital surplus (S) for healthy

[10] The treatment has no effect on lifetime marriage likelihood because all women marry by Period 2.

women from $(\frac{\mu^*_{ah}}{p_{ah}})\frac{(\mu^*_{ah}S^{ah}_{ah}+p_{al}S^{ah}_{al})}{(p_{al}+\mu^*_{ah})}$ to S^{ah}_{ah}. That is,

$$E[S_1^{ah}|T=1]-E[S_1^{ah}|T=0]=\frac{\mu^*_{ah}S^{ah}_{ah}(p_{ah}-\mu^*_{ah})+p_{al}(p_{ah}S^{ah}_{ah}-\mu^*_{ah}S^{ah}_{al})}{p_{ah}(p_{al}+\mu^*_{ah})}>0. \tag{11}$$

This is the combination of two positive effects. First, healthy women now marry earlier in expectation. Specifically, the fraction $1-\frac{\mu^*_{ah}}{p_{ah}}$ of women now marries in Period 1, while, in the absence of the treatment, would have married in Period 2. For these women, marital surplus increases from zero to S^{ah}_{ah} in Period 1. That is, these women marry the same type of man regardless of the treatment, but do so earlier. Second, the remaining women now all match with healthy men. For women who would have married an unhealthy man in Period 1, the treatment increases their surplus from S^{ah}_{al} to S^{ah}_{ah}. That is, these women marry at the same time regardless of the treatment, but they marry higher health partners.[11]

We can test the model by observing in our data whether making health observable changes marriage propensity and marital surplus for unhealthy and healthy women in the way described above. Since our model studies the process of getting married, we test these hypotheses by looking at the behavior of unmarried women. However, we observe both unmarried and married women in our data. Therefore, it is useful to understand whether making health observable in Period 1 also changes the marital status and surplus for women already married.

When we rule out divorce, the model predicts no treatment effects on marriage, since women are already married. The treatment also has no average effect on surplus for women married at baseline, regardless of whether surplus depends on partner's health (i.e., whether one's partner is healthy) or on knowledge of a partner's health (i.e., whether spouses think their partners are healthy).

If surplus depends on health, then providing knowledge on partner's health has no effect on surplus. Conversely, if surplus varies with knowledge of partner's health, making health

[11] The treatment's effect on lifetime surplus is negative for unhealthy women and and positive for healthy ones.

observable increases the surplus of some couples and decrease the surplus of others without changing the average surplus of a type. For example, consider married lh women in Period 1 when health is unobserved. Each woman has an expected surplus of $\frac{p_{lh}S^{lh}_{lh}+p_{ll}S^{lh}_{ll}}{p_{lh}+p_{ll}}$. When health becomes observed, women married to lh and ll men find out that their surplus is S^{lh}_{lh} and S^{lh}_{ll}. However, the expected surplus is still $\frac{p_{lh}S^{lh}_{lh}+p_{ll}S^{lh}_{ll}}{p_{lh}+p_{ll}}$.

Lastly, our model makes specific assumptions about risk preferences, the relative values of traits, divorce costs, and the correlation between the traits. With minor exception, making health a larger determinant of surplus than attractiveness, allowing for divorce, letting attractive people be more patient, and letting trait frequencies vary by gender generally does not change our equilibrium and predictions. Appendix A considers these cases in more details.

3 Context and Data

3.1 HIV Testing

The HIV/AIDS epidemic is a critical public health issue in Malawi and elsewhere in Sub-Saharan Africa. According to the 2010 DHS, HIV prevalence is 10.6 percent among Malawians aged $15-49$. International initiatives led by the Global Fund to Fight AIDS, TB, and Malaria have made free HIV testing and antiretroviral therapy available through public health clinics throughout Malawi in the past decade. During this time, HIV testing technology has also evolved so that "rapid" HIV tests, which yield results in around 30 minutes, are now widely available. These tests are 98 percent accurate and require a confirmatory test to reach a definitive diagnosis. The Ministry of Health requires that everyone receiving an HIV test also receives standardized pre-test and post-test counseling. The pre-test counseling session provides information about the causes and consequences of HIV. Post-test counseling for HIV-negative people focuses on HIV prevention while post-test counseling for HIV-positive people also provides information about how to seek treatment.

HIV testing through public health clinics is inconvenient for many people. A test seeker must travel (typically by foot or bicycle) for several kilometers over unimproved roads to reach the facility. Once there, he or she may wait for several hours to be seen. Pinto et al. (2013) find that patients spend an average of 7.1 hours per visit seeking HIV-related care in the adjacent Zomba District of Malawi. Test seekers also face the risk that testing may not be available at the particular time that they visit the facility.

Stigma is another intangible but important cost of HIV testing. Although tests are typically conducted in private, test seekers often have to wait in public areas. Seeking an HIV test may lead to rumors that a person is HIV positive or unfaithful (Sambisa et al. 2010, Berendes and Rimal 2011, Maughan-Brown and Nyblade 2014). Ngatia (2011) develops a model with the reasonable premise that stigma is negatively related to the testing rate, so that testing exhibits strategic complementarities. In Appendix Section B, we show that this framework leads to multiple equilibria, and that small changes in the time cost of testing may heavily influence testing adoption. Strategic complementarities due to stigma may explain why people do not spontaneously adopt testing as a signaling device in the status quo.

An economic literature evaluates the impact of HIV testing on risky sexual behavior and other outcomes. Thornton (2008) finds no significant effect of HIV testing on HIV-negative people. Baird et al. (2014) and Gong (2015) find that testing has effects only when test takers are surprised by the results. Beegle et al. (2015) show no effects of testing on marriage or human capital investment. In general, these papers focus on the way that HIV status information may affect individual optimization. No research (to our knowledge) has considered the possibility that HIV test results may serve as a signal to others. The lack of an impact on HIV-negative people suggests that HIV testing does not lead people to update their beliefs in most cases. Most people correctly believe that they are HIV negative but they have much less information about the HIV status of their partners.

3.2 Setting

The TLT Panel Study took place in the Balaka District of Southern Malawi. Most households in this area practice matrilineal kinship and matrilocal marriage (Reniers 2008, Berge et al. 2014). The partners, rather than their families, decide whether to marry and determine marriage timing (Kaler 2001, Kaler 2006). As in other matrilineal settings, significant marriage payments are uncommon.[12] Polygamy exists but does not occur frequently.

The gender age gap in Malawi is around five years: women typically marry at age 20 and men typically marry at age 25. This pattern is consistent with Bergstrom and Bagnoli (1993) hypothesis that asymmetric information about male earnings capacity leads men to marry later. Despite this age gap, the greater female susceptibility means HIV prevalence is similar for men and women in the marriage market. Although Malawi has one of the worst HIV/AIDS epidemics in the world, the prevalence of 10.6 percent means that most people who are tested for HIV receive a negative diagnosis.

HIV prevalence and marriage timing are negatively correlated in Sub-Saharan Africa. The rise of HIV/AIDS coincides with a delay in marriage in many parts of Sub-Saharan Africa (Harwood-Lejeune 2001, Marston et al. 2009, Hosegood et al. 2009). Bongaarts (2007) also documents a cross-sectional correlation between HIV prevalence and marriage timing. HIV prevalence is over 20 percent and the median age at first marriage is over six years later in Southern Africa compared to the rest of SSA. Magruder (2011) conjectures that the extension of courtship contributes to the spread of HIV. Adverse selection is a complementary explanation for this correlation.

Demographic research documents the correlation between HIV risk and the timing and match quality of marriage (Schatz 2005, Reniers 2008). Mukiza-Gapere and Ntozi (1995) and Watkins (2004) indicate that people believe that the primary threat of HIV/AIDS comes from the spouse. Mukiza-Gapere and Ntozi (1995) argue that in Uganda, “the fear

[12]Meekers (1992) argues that marriage payments and other formalities are less important in matrilineal societies because marriage does not transfer ownership of the wife’s assets or the couple’s children to the husband.

of HIV/AIDS has 'poured cold water' on the institution of marriage", leading to marriage delays. According to Watkins (2004), women are concerned their husbands will "bring AIDS into the house", which will lead to suffering and make it difficult to care for children. People (particularly women) are conscious of this risk and encourage each other to make marriage decisions strategically to avoid HIV.

3.3 Intervention and Data

This study uses data from the Tsogolo La Thansi (TLT) Panel Study, which was conducted in Balaka, Malawi from 2009 to 2011. The panel contains eight waves, each spaced four months apart for 2.3 years and collected between June 2009 and October 2011. Each wave collects data on a broad range of socio-economic variables, including HIV beliefs and status, marital status, and fertility. Our main sample consists of 1000 young women aged 15-25 from the area, selected to be representative of this age group within the community. Each survey participant receive a $3 compensation in each wave.

Five hundred women were randomly assigned to a "treatment" arm, which offered HIV testing after every survey wave, for a total of eight tests, while the "control" arm offered HIV tests after Wave 8 only.[13] Surveyors provided rapid HIV tests, which yield results in around 30 minutes.[14] To ensure confidentiality, participants did not receive written documentation of their HIV status.

Women were encouraged to invite their partners to enroll in the study, in which case the partners of treatment group women would receive high-frequency HIV testing as well. The unwillingness to get tested, and, in the partners' case, also to participate in the study, thus foregoing monetary compensation, may act as potential screening devices for both the respondents and their partners, indirectly revealing that they are HIV-positive.

[13] Since the tests were offered after participants completed the survey, being tested in wave t can affect the outcomes of waves $t + 1$ and subsequent ones, but not wave t outcomes. Since the first tests were given at the end of Wave 1, we consider Wave 1 the baseline and Waves $2 - 8$ to be the follow-up.

[14] Surveyors had been trained to provide both pre-test and post-test counseling to respondents and, if applicable, referral for follow-up care. The content of the counseling was contingent on the test result.

Figure 1 shows that the treatment more than doubles the likelihood that women and their partners were tested in the previous 4 months, increasing it from 30 to 70 percent for women and from 25 to 48 percent for their partners. This is consistent with the treatment reducing the cost of HIV testing.

Our empirical analysis focuses on marriage and fertility, which are our main outcomes of interest. Attractiveness, HIV status, and beliefs about own and others' HIV status are other important variables in our study.

Marital status is measured in each wave and is linked to a specific partner. Around 44 percent of women are married at baseline. This figure rises to 63 percent by the end of the study. Divorce is possible in this setting, and could be an implication of the HIV testing intervention. Around 7 percent of respondents divorce over the study period. This rate is balanced across treatment and control groups (as we discuss further below), so that in practice we do not find treatment effects on divorce.

We propose to consider number of children as a proxy for surplus. This is the case if the costs of bearing and rearing a child decrease with surplus (e.g., frequency of sexual act and ease to collaborate with spouse in raising child are higher the higher the surplus. In every wave, respondents were asked to take a pregnancy test. Compliance with this request was over 95 percent and most non-compliers were women who were obviously pregnant. As we show below, fertility patterns by HIV status and attractiveness are consistent with the surplus interpretation of pregnancy.

We use physical beauty for women and education for men as our proxies for attractiveness in the marriage market. These choices are based on evidence that, while both male and female marriage market participants have strong preferences for beautiful and wealthy partners (Buss 1989, Buss and Schmitt 1993, Eastwick and Finkel 2008, Fisman et al. 2006, Hitsch et al 2010, Kurzban and Weeden 2005, Regan et al. 2000), men have a comparatively stronger preference for beauty and women do so for wealth (Hitsch et al 2010).

Surveyors assess physical attractiveness on a five-point Likert scale with the following categories: 1 (much more attractive than average), 2 (more attractive than average), 3 (average), 4 (less attractive than average), and 5 (much less attractive than average). Since only one respondent is classified in the "much less attractive" category, we combine this category with Group 4. Figure 2 shows the baseline frequency distribution of this variable. In the analysis below, we distinguish between people in Groups 1 and 2 (who are defined as attractive) and Groups 3, 4, and 5, who are defined as unattractive. Since all surveyors were women, this variable captures female assessments of the attractiveness of the women in our sample, rather than male assessments, which are arguably more relevant for the marriage market. This issue introduces measurement error, which should make it more difficult to find results.

The analog to physical attractiveness for men is wealth, or earnings potential, neither of which we observe in our data. In their stead, we use completed education as a proxy. Survey respondents indicate the level of completed schooling for each partner on a four-point scale, in which 0 indicates less than primary completion, 1 indicates primary completion, 2 indicates secondary completion, and 3 indicates higher education. At baseline, around 1 percent of partners did not complete primary, 40 percent completed primary, 52 percent completed secondary, and 7 percent completed higher education.

The effects of offering opt-out, high-frequency testing on marriage and fertility differ by HIV status. However, there are several issues with studying heterogeneous effects by HIV status. First, since HIV prevalence is six percent, the sample of HIV-positive people is small, which limits the power to estimate effects in this group. Second, the provision of HIV testing could have induced people to alter their risk exposure in a way that would influence HIV status. In practice, this phenomenon is not likely to be statistically important because the HIV sero-conversion rate is sufficiently low. In the treatment group, for whom we have multiple HIV tests over the sample period, only 11 out of 507 people change status over 2 years and four months, that is, the prevalence of HIV increases by about 2.2 percentage points in this time interval. 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 suggesting an increase in HIV prevalence by about one percentage point per year, which is approximately what we find in our treatment group. Furthermore, existing research suggests that HIV testing has small effects on risky sexual behavior. Lastly, data on HIV status are more complete for the treatment group than the control group, since the control group was offered tests only once, and, therefore, there is a higher rate of coverage for the treatment group than the control group. For the treatment group, we observe the HIV status at least once for 97 percent of participants. For the control group we observe the status for 75 percent of participants. Empirically, we can either discard data for women whose status is unknown, or consider them either HIV positive or negative. Statistically, it is likely that most of the non-testing people in the control group are HIV negative. Among testers, the prevalence is 10.5 percent in the treatment group and 4.9 percent in the control group. To match the prevalence in the treatment group, prevalence among non-testers in the control group would need to be around 20 percent, consistent with the idea that HIV-positive women prefer to not get tested. Therefore classifying non-tester as HIV negative for the purpose of the analysis reduces measurement error compared to the alternative of classifying them as HIV-positive. We return to this point in section 4.1, when we discuss our estimation issues.

To examine the role of HIV status information, we estimate effects on perceptions of own and partner HIV status. The survey elicits the subjective probability that the respondent is currently HIV positive. To assess this and other subjective probability, surveyors used the "bean" methodology and took extra care to ensure that respondents understood the concept of probability and that they provided internally-valid responses. This variable ranges from 0 to 100 percent. 54 percent of people indicate zero probability that they are infected and 93 percent of people believe their infection probability is 50 percent or lower.

The survey also elicits the likelihood that the respondent's partner (not necessarily a spouse) has HIV on a five-point Likert scale. This variable is available only for respondents who have partners at the time of the interview, but the availability does not depend on whether the partner chooses to participate in the study. To simplify the interpretation of this variable, we create binary versions that equal 1 if the respondent believes her partner "may have HIV." Estimates using the full Likert scale are available from the authors.

A final data-related issue is that, while all respondents range from 15 to 25 at baseline, control respondents are an average of 0.6 years younger than treatment respondents. This imbalance is apparently due to chance, since other orthogonal characteristics are balanced. Since marriage and fertility increase with age in this range, the age imbalance could spuriously indicate higher marriage and pregnancy propensities for the treatment group. We address this issue by employing entropy weights throughout the analysis. 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).

Table 1 provides summary statistics for the treatment and control arms conditional on age re-weighting.[15] Demographic and socioeconomic variables appear balanced, including tribe, religion, HIV prevalence, school enrollment, and employment. The household asset index is a standardized sum of indicators that the household has a durable roof, a durable floor, electricity, a television, a telephone, and an improved toilet. The table also shows that treatment and control respondents have similar levels of future orientation and subjective mortality risks. Balance for these parameters is important because the internal discount rate, δ, determines marriage timing in our model. Finally, the table shows that marriage and pregnancy rates do not differ significantly across treatment arms at baseline.

Since we will estimate treatment effects by HIV status and baseline attractiveness for baseline-unmarried respondents, columns $1-3$ of Table 5 distinguish between HIV-positive and HIV-negative respondents. We do not oversample HIV-positive respondents, and the unmarried subsample includes only 37 HIV-positive women. As anticipated, the HIV-positive

[15] Unweighted versions of all results are available from the authors. The unweighted version of Table 1 shows that marriage and fertility are significantly higher in the treatment group.

and HIV-negative groups differ in several important ways. HIV-positive women are older and have lower socioeconomic status. They also perceive significantly higher mortality risk. Attractiveness does not differ significantly by HIV status, which is consistent with the theoretical assumption that health and attractiveness are independent. Columns $4-6$ of Table 5 further limit the sample to HIV-negative baseline-unmarried respondents and cut by attractiveness. Attractiveness is not correlated with demographic characteristics. However, attractive women have higher socioeconomic status in several dimensions. Attractiveness may be correlated with SES if wealthy respondents spend more on clothing and grooming. Attractive women also "think about the future" to a greater degree than unattractive women. These correlations may confound our estimates if attractiveness interactions spuriously capture interactions with SES or time preferences. We discuss these issues further below.

3.4 TLT's Intensive HIV Testing Makes HIV Status Observable

The TLT intervention has three components that let testing be used as a signaling and screening device, thus making the HIV status of others more easily observed. (i) As we show in Appendix B, offering on-site, routine testing reduces the cost of getting tested, and, therefore, may help select an equilibrium in which being tested is the default behavior in the marriage market. (ii) Offering the test to both men and women reduces the asymmetric information for both parties in the marriage market: if only women (or men) get tested, they can signal their status but cannot learn the status of their potential partners. (iii) Offering the test every four months reinforces the negative signal of opting out of testing: while it is easy to find plausible excuses for skipping one round of testing, repeated refusals to be tested are less easy to justify.

People in principle could lie about having been tested or about their HIV status: positive people may report having tested negative. However, lying is costly, especially within an intimate relationship. Moreover, testing, although confidential, may be indirectly observable:

injections leave marks, subject spend more time at the clinic to wait for the test results, and importantly, testing stops being offered after a subject tests positive.

3.5 Validity of Model and Data Assumptions

Our model assumes positive assortative matching and, therefore, that marital surplus is ranked in specific ways. In addition, health is assumed to be private information before our intervention starts. These assumptions imply that marriage rates differ for healthy and unhealthy women. Lastly, our empirical analysis assumes that fertility is a proxy for marital surplus and physical attractiveness and earnings potential are important traits along which the assortative matching occurs. This section shows that these assumptions are consistent with the data patterns we observe at baseline.

Our model assumes that marital surplus is bigger for attractive than for unattractive women and for healthy than unhealthy women. Consistent with this assumption, Figure 3 shows that the number of children by year of marriage, our proxy of marital surplus, grows with wives' beauty and health, with the exception of wives with below average beauty, who are, however, about 3 percent of the sample only, and whose data, therefore, are measured with less accuracy.[16]

Moreover, attractive men and women are assumed to be more likely to marry each other. If that is the case, wife's beauty and husband's education should be positively correlated. Conversely, there is no such correlation between health and attractiveness of wives and husbands because HIV status is private information at baseline. Consistent with this assumption, Figure 4 shows that attractive women marry more educated men, while healthy women do not.[17]

Lastly, the likelihood of being married is higher for unhealthy women than healthy women and for unattractive women than attractive women, because of adverse selection.

[16] If we group women into below average and at least average, fertility grows with beauty.

[17] We observe health status, proxied by HIV, only for 58% of males in our sample, and, therefore, we cannot correlate spouses' health, which, in our model, is uncorrelated in Period 1.

Consistent with this assumption, Figure 5 shows that both beautiful and HIV-negative women are less likely to be married than less beautiful and HIV-positive women.

4 The Impact of Intensive HIV Testing

This section estimates the impacts of the HIV testing intervention. To support the adverse selection interpretation, we first show that the intervention provides information about partner HIV status but (for the most part) does not update beliefs about own HIV status. Next we show effects on marriage and fertility both overall, and by baseline marital status, HIV status, and attractiveness.

4.1 Identification and Estimation

Our primary specification pools the follow-up waves (Waves $2-8$) to estimate a combined average treatment effect. We estimate the parameters of the following equation:

$$Y_{it} = \beta_0 + \beta_1 T_i + \beta_2 Y_i^b + X_i^{b\prime}\beta_3 + \delta_t + \varepsilon_{it} \tag{12}$$

In this equation, Y is either beliefs about own and partner's HIV status, or marriage and fertility, our mai outcomes. The variable T is an indicator for the assignment to the treatment group, Y^b is the dependent variable at baseline, and X^b is a vector of other baseline characteristics, which we include to increase the precision of the estimates. δ_t is a set of wave indicators.

Being offered an HIV test in every wave is the "treatment" for this analysis. Therefore, assignment to and receipt of treatment are equivalent. In this setting, the parameter β_1 identifies the average treatment effect on the treated (ATT) under the assumptions that (i) a subject's potential outcomes are unaffected by another subject's treatment assignment and (ii) the treatment is independent of potential outcomes conditional on age. General equilibrium effects on the marriage market are a potential threat to the first assumption.

However, the treatment group represents at most only around 10 percent of local marriage market participants, minimizing this concern. We estimate Equation (12) by OLS and cluster the standard errors by subject.

4.2 The Impact on Information About Partner Quality

The model describes the effects of making health observable. To do that, our treatment, offering high-frequency free HIV testing, must increase the frequency of HIV testing and change the market's perception of the health status of treated people. We present several pieces of evidence consistent with these predictions.

Table 5 shows the estimated treatment effects on women's beliefs about own and partner's HIV status. While, ideally, we would like to observe how the treatment changes the market's beliefs regarding people's health, we do not observe these data, so we consider a woman's beliefs about her partner's HIV status as being positively correlated with the market's beliefs. We divided the sample between HIV negative and HIV positive respondents because the treatment is expected to have opposite effects on these group's beliefs. Column 1 of table 5 shows that baseline beliefs are positively correlated with HIV status, as the baseline belief of being HIV positive is 43 percentage points lower for HIV negative women than for HIV positive ones. Out of all the women who tested HIV positive in our data, about half are certain to be HIV positive and about 80 percent think they are at least 50 percent likely to be HIV positive.

In addition, being offered high-frequency HIV testing does not change HIV-negative treated women's belief of their HIV status, despite the fact that almost all women are tested at least once during the intervention: the estimate ATT is 0.004 and not statistically significant. This suggests that most women do not get tested to find out their HIV status, as, if it were the case, we would find them to update their belief after being tested. Conversely, the estimated treatment effect is 0.17 and statistically significant for HIV-positive women. By looking at the distribution of beliefs in the treatment and control group we notice that the increase in the belief of being HIV positive after testing positive is driven by 15 women only, who think they are at least 50 percent likely to be HIV positive after testing positive and who, therefore, must have been unaware of being infected with HIV. That is, most treated women choose to be tested at least once despite having mostly correct beliefs of being HIV negative. Column 2 of table 5 uses only the sample of women with partners and finds similar results to the whole sample.

Lastly, column 3 of table 5 shows that the treatment statistically decreases HIV-negative women's beliefs that their partners may have HIV by 4.8 percentage points, or about 14 percent. Since this change does not occur by altering beliefs about one's status, it must work by inducing partners to get tested and by testing negative.[18] In sum, the evidence from table 5 is consistent with HIV testing being used to *signal* one's HIV status.

Figure 6 provides additional evidence consistent with this conjecture by showing that women's perceived partner HIV status correlates with their partner's testing and outcomes: 83 percent of women whose partners test positive believe the partners may have HIV, compared to 49 percent for women whose partners do not test, and 25 percent for women whose partners test negative at least once. We cannot tell to what extent these patterns are caused by selection (e.g., partners about whose health women are uncertain are less likely to accept to get tested) and to what extent by belief updating (e.g., if a man tests negative, his significant other becomes more confident that the man is negative). Nevertheless, both selection and updating are consistent with testing as a signaling device. Given that people are likely to have better than average information about their partner's type, we interpret changes in these beliefs as a lower bound of the effect of the treatment on the marriage market's beliefs regarding the health type of its participants.[19]

Taken together, the evidence presented in this section is consistent with the hypothesis

[18]Note that women have more pessimistic beliefs about their partner's HIV status – 34 and 72 percent of women with HIV-negative and HIV-positive partners think their partner may be HIV positive. However, beliefs about own and partner's HIV status are measured in different ways, therefore differences in measurement may explain the differences in beliefs.

[19]We do not observe partner's belief about women's HIV status, so we cannot do a similar exercise for women's testing and partner's beliefs.

that our treatment lowers the cost of HIV testing and that testing has a signaling role in the marriage market.

4.3 The Impact on Marriage and Fertility

Table 5 shows the estimates of the ATT effects on marriage and pregnancy for the entire sample (Panel A) and by baseline marital status (Panel B). Panel A shows that the treatment increases the likelihoods of being married and pregnant for the whole group by 0.035 and 0.021 (s.e.'s 0.017 and 0.010). Compared to the control group means of 0.55 and 0.13, these are 6 and 12 percent increases. Panel B shows that these changes are driven by women who are unmarried at baseline, consistent with our model: the treatment increases the likelihoods of being married and pregnant for these women by 0.056 and 0.031 (s.e.'s 0.028 and 0.013). Compared to the control group means of 0.21 and 0.12, these are 26.7 and 26 percent increases. Conversely, the treatment effects for baseline married women are smaller and not statistically significant, although they do not statistically differ from the estimated effects for unmarried women.

Table 5 presents our main results by considering women unmarried at baseline only, since the model predicted changes in marriage and fertility for this group only. Panel A, which cuts by HIV status, shows statistically significant increases in the likelihoods of being currently married and currently pregnant for HIV negative women, with effect sizes of X and Y TBC and statistically insignificant decreases in these outcomes for HIV positive women. Both sets of estimates are consistent with the model's predictions that the treatment should induce healthy women to marry earlier and have a higher marital surplus, while it should not change marriage rates and surplus for unhealthy women. The two sets of estimates do not statistically differ by women's HIV status. However, since we have only 73 HIV positive women in our sample, it is not surprising that their ATT effects are imprecisely estimated.

Panel B considers HIV negative unmarried women only, and estimates the treatment effects by attractiveness. As predicted by the model, the ATT effect on marriage is statistically larger for attractive than unattractive women, while the ATT effects on fertility are positive for both groups and not statistically different from each other.

Since health and attractiveness are correlated with various determinants of marriage and fertility, Columns 2 and 4 of Table 5 estimate a version of Equation (12) that further interacts the treatment dummy by three standardized indices of demographic, socio-economic, and time preference variables, as in Katz et al. (2001). Comparing across columns shows that the estimated ATT are stable regardless of which specification we use and that, therefore, the positive ATT effects are not caused by attractive and healthy women having different demographic and socio-economic characteristics or being more forward-looking. The estimates from these columns show increases in the marriage and fertility likelihoods of of 14 and 5.6 percentage points, 127 and 70 percent increases compared to the control group means of 0.11 and 0.08.

Figure 7 shows this pattern graphically by plotting the marriage and pregnancy rates for baseline-unmarried women in the treatment and control groups. Both outcomes, and especially marriage, trend upward in the control group, as expected. Moreover, the treatment causes steeper trends, with this pattern, again, stronger for marriage. The differences between the treatment and control groups grow gradually in the first three waves, consistent with the idea that it takes some time for testing to be used a signaling device, as the signal is reinforced with repeated testing (or lack of it). The differences in outcomes between the treatment and control groups then peak in the central waves.

Figures 9 and 8 shows these pattern further cutting by attractiveness. The outcomes trend upward in the control group for both sets of women and especially for marriage. In the control group, marriage and fertility are delayed for attractive women compared to unattractive women, consistent with our model. However, in the treatment group, this delay disappears and both types marry and become pregnant at similar rates. An additional implication of our model is that the HIV testing intervention should not lead to changes in matching on attractiveness, since this trait can always be observed. Consistent with this feature of the model, figure 10 shows that among married women at Wave 8, treatment and control participants have husbands with nearly the same levels of education.

4.4 Robustness Checks and Alternative Explanations

Our results are robust to a number of checks and to alternative hypotheses.

First, columns 2 and 4 of table 5 show that the effects are not driven by the different socio-economic characteristics and time preferences of the different subsets of women, as already discussed.

Second, we checked that our results are robust to excluding from the analysis all subjects who drop out of the study at any point. Table A**??** provides estimates of the ATT effects for both the entire sample (Panel A) and women unmarried at baseline, cut by HIV status (Panel B) and attractiveness (Panel C). Columns 1 and 3 show the estimates including all available observations, while columns 2 and 4 estimate the ATT effects separately for women who never drop out and women who miss at least one wave. In this second case, we also show the estimate of the difference between the ATT effects for women who never drop out and women who do at least once. The table shows that excluding or including dropouts does not change the estimated ATT effects.

Third, when we replace "marriage" by "committed relationship" in our empirical analysis, we find the same pattern of results: the treatment increases marriage and fertility for HIV-negative women who were single at baseline, more so if they are of average beauty or higher.

Lastly, our findings that the treatment increases marriage and fertility for HIV negative women could be explained by the following four alternative or additional explanations. (i) The treatment makes thinking about family formation and planning more salient and, therefore, increases marriage and fertility rates of HIV negative women. (ii) Testing negative to HIV makes women more inclined to have unprotected sex and, therefore, to be more likely to become pregnant. (iii) Some women prefer to marry later and, therefore, postpone becoming sexually active and entering the marriage market, which would induce a negative correlation between health and marriage likelihood and fertility. (iv) The increase in fertility is driven by changes in bargaining power. While increasing marital surplus, the treatment reduces the bargaining power of HIV negative women because they can now marry healthier partners: to the extent that health is a determinant of bargaining power, if the health of the wife is unchanged but the health of the husband increases, the relative health and power of the woman decreases. Therefore, if women desire fewer children than their husbands, a reduction in female bargaining power would increase fertility.

These alternative hypotheses cannot explain our findings. (i) It is possible that the treatment makes family formation and planning salient, so this could reinforce some of our results. However, this salience could not explain why the increase in marriage rates is larger for attractive women, as the salience of marriage increases also for unattractive ones. (ii) Testing negative for HIV does not change subjective beliefs on own HIV status, therefore, it should not directly affect sexual behavior. Other studies (e.g., (Thornton 2008, **?**, Gong 2015)) also show no or limited effect of HIV tests on sexual behavior. (iii) If marriage rates vary because of different preferences, which also cause unmarried women to be less likely to be HIV positive, then HIV testing should have no effect on marriage, while we find that it does. Moreover, the different preferences hypothesis cannot explain our finding that the treatment has differential effects on marriage likelihood by beauty. (iv) In our sample, women married at baseline desire as many children as their husbands. However, their husbands desire children sooner. If the treatment causes a drop in healthy women's bargaining power, then it could explain their higher fertility (as husbands' preferences have more weight in the couple's fertility decisions). However, it is not obvious that the treatment changes healthy women's bargaining power in the marriage *in Period 1*: when HIV testing makes health more easily observable in Period 1, the spouses have the same HIV status, while some spouses have different health when the HIV status of others is unobserved. However, when health is unobserved, marital happiness depends on perceived health, in which case husbands' and wives' health are the expected health in the population who marries in Period $1 - \mu_{ah}h + p_{al}l(p_{al} + \mu_{ah})$ for a couple with attractiveness a. Also in this case, therefore, the expected health of husbands and wives is identical.[20] In this case, the treatment does not affect the bargaining power of healthy women, and, therefore, the observed increase in fertility is not caused by changes in intra-household bargaining. Our data support this notion since we do not find any changes in the likelihood that HIV-negative women receive gifts from their husbands or have more decision-making autonomy in the marriage (results available upon request).

4.5 The Effect of a One-Shot HIV Testing Intervention

Few studies have examined the impact of HIV testing on marriage. One recent example comes from Beegle et al. (2015), who find no effect of a one-off testing intervention on marriage and fertility in Malawi. Their study, like the others in this literature that find no effects on risky sexual behavior, typically evaluate the effect of single HIV testing interventions. We argue that, in order for not being tested to send a negative signal about one's health, HIV testing must be offered (and turned out) multiple times: refusing to be tested multiple times sends a stronger signal than not being tested only once.

The TLT design allows us to examine the impact of a single test through an alternative randomly selected representative group of 500 women who were offered HIV tests twice, in Waves 4 and 8. Recall that, since the tests are offered *after* collecting survey data, the testing done in wave 4 can have effect on outcomes only from Wave 5 on, and, similarly, that the wave 8 testing cannot affect the observed outcomes by design. Therefore, by comparing this second treatment arm with our control group, which gets tested only in wave 8, we can estimate the effect of being offered testing in wave 4 only. To do that, we compare marriage and fertility for this alternative treatment arm and the control group including only Waves 4 to 8. In this approach, Wave 4 is the baseline and Waves $5 - 8$ are the follow-up. This

[20] In Period 2, once spousal health is revealed to each partner, then marital happiness will depend on actual health, which will differ for some couples.

approach is not entirely analogous to our earlier experiment because there are fewer follow-up waves.

Table AD, in the Appendix, provides summary statistics for this second treatment arm and the control arm at Wave 4, while Table 5 reproduces the estimates for this alternative treatment in Panel A (analogous to the estimates from Table 5), cutting by HIV status in Panel B and by attractiveness conditional for the HIV-negative sample in Panel C (analogous to the estimates from Table 5). All panels show no significant effects of the one-shot testing intervention on marriage and fertility, unlike our previous findings. Moreover, we report in brackets the p-value of the test of equality of effects from the single-test intervention and waves 1 to 5 of the multiple-test intervention (so that we are comparing being offered one versus 4 HIV tests within 16 months). We fail to reject this hypothesis in all cases. These results suggest that the frequency of testing in our main intervention is a key reason why HIV testing increases marriage and fertility for HIV-negative women.

5 Conclusion

Adverse selection is an important phenomenon in various product and labor markets that feature asymmetric information. Little or no research to date has examined the implications of this model in the marriage market. We combine a simple assortative matching model with the randomized evaluation of a novel intervention to reduce asymmetric information related to HIV risk among partners.

Our model predicts that asymmetric information about a partner's health will lead to adverse selection in the marriage market: some healthy types prefer to postpone marriage until they know their prospective partner's health, with this selection more pronounced for attractive types, under some conditions. Removing the asymmetric information increases marriage rates and surplus for healthy types. Under some conditions, the increase in marriage is more pronounced for attractive types.

We find that the model predictions match our empirical findings using data from the TLT experiment, an intervention that offered free, on-site HIV testing to aged 15 to 25 women and their partners every four months for 2.3 years: this intervention increased the probability of marriage and pregnancy by 3.5 percentage points and 2.1 percentage points, respectively, for these women. Estimates are stronger for HIV-negative and baseline-unmarried women, and, among this group, for women who are physically attractive. The effect sizes are very large for this group, averaging 127 percent for marriage and 70 percent for fertility.

Researchers have found small and contingent effects of HIV testing on risky sexual behavior, which has limited the potential role of HIV testing in the eyes of some policymakers. A key distinction between this evaluation and others is the frequency with which tests were offered. Participants and their partners received up to eight HIV tests at regular intervals for 2.5 years in this intervention, altering perceptions of the cost of testing and enabling people to signal and screen. These benefits of testing make less sense for interventions that offer a single test. To support this interpretation, we show that an alternative one-shot testing intervention in the same population had no significant effects on marriage and fertility, even among the subgroups who respond the most to repeated testing. Despite the lack of a response for risky sexual behavior (we also find insignificant effects on these outcomes), an impact on marriage is relevant for the epidemiology of HIV because discordant spouses are a major source of new HIV infections.

With the active involvement of the WHO and UNAIDS, there is a movement to offer routine on-site testing, switching from an "opt in" model in which subjects have to take action to get tested to an "opt out" model in which inaction leads to regular testing (Thornton 2008). Our theory and evidence predict that the switch to an opt out model will anticipate the timing of marriage and childbearing.

More broadly, our model predicts that changes in the degree of asymmetric information, as, for example, those caused by higher labor mobility or the diffusion of concealable contraception, will impact marriage and fertility in more ways than generally expected.

Table 1: Baseline Characteristics by Treatment Status

	Full Sample		
	Treatment	Control	P-value
	(1)	(2)	(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
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	0.63	0.62	0.83
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: Baseline Characteristics by HIV Status and Attractiveness

	Unmarried Sample			Unmarried / HIV Negative Sample		
	HIV−	HIV+	P-value	Attractive	Unattractive	P-value
	(1)	(2)	(3)	(4)	(5)	(6)
Demographics						
Age	18.4	21.2	0.00***	18.3	18.4	0.75
Ngoni Tribe	0.35	0.40	0.56	0.36	0.35	0.98
Yao Tribe	0.25	0.21	0.60	0.23	0.27	0.36
Lomwe Tribe	0.18	0.14	0.47	0.19	0.19	0.98
Catholic	0.38	0.56	0.03**	0.36	0.40	0.31
Protestant	0.48	0.27	0.01***	0.50	0.43	0.17
Muslim	0.16	0.17	0.84	0.15	0.17	0.59
Attractiveness	3.61	3.38	0.15	4.14	2.96	0.00***
Socioeconomic Status						
Enrolled in school	0.68	0.23	0.00***	0.76	0.59	0.00***
Employed full-time	0.07	0.36	0.00***	0.04	0.10	0.02**
Any savings	0.12	0.22	0.17	0.10	0.14	0.26
Household asset index	0.30	-0.14	0.01**	0.58	-0.05	0.00***
Preferences and Perceptions						
Thinks about future	3.32	3.04	0.15	3.54	3.04	0.00***
Subjective 5-year mort. risk	0.31	0.43	0.04**	0.33	0.30	0.24
Subjective probability HIV+	0.08	0.34	0.00***	0.08	0.08	0.65
Worried about HIV	0.73	1.18	0.03**	0.70	0.73	0.78
Outcomes						
Pregnant	0.05	0.07	0.55	0.05	0.04	0.68
Ever tested for HIV	0.44	0.58	0.09*	0.41	0.46	0.28
Partner may be HIV positive	0.40	0.75	0.00***	0.37	0.45	0.23
Respondents	552	37	-	303	249	-

Note: To compute p-values, we regress each variable on HIV status or attractiveness in Wave 1 and cluster standard errors by respondent. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: 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)
Treatment · HIV positive	0.17***	0.17**	0.054
	(0.064)	(0.072)	(0.062)
Equality of coefficients (p-value)	0.01	0.03	0.12
Control mean (HIV negative)	0.14	0.15	0.34
Control mean (HIV positive)	0.57	0.60	0.72
Observations	6048	4456	4456

Note: Clustered standard errors appear in parentheses. 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 4: The Impact of HIV Testing on Marriage and Fertility

	Currently Married	Currently Pregnant
	(1)	(2)
A: Main Effects		
Treatment	0.035**	0.021**
	(0.017)	(0.010)
Control mean	0.55	0.13
B: Estimates by Baseline Marital Status		
Treatment · Unmarried	0.056**	0.031**
	(0.028)	(0.013)
Treatment · Married	0.010	0.016
	(0.018)	(0.015)
Control mean (unmarried)	0.21	0.12
Control mean (married)	0.93	0.17
Equality of coeffs. (p-value)	0.17	0.47
Observations	6048	6048

Note: Clustered standard errors appear in parentheses. All regressions include Waves $2-8$ and reweight to balance by age. Estimates control for the baseline dependent variable and wave indicators. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Baseline-Unmarried Estimates by HIV Status and Attractiveness

	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)
Treatment · HIV Positive	-0.15	-0.053	-0.057	-0.071
	(0.13)	(0.13)	(0.061)	(0.057)
Significance of controls (p-value)	-	0.00	-	0.00
Equality of coefficients (p-value)	0.13	0.45	0.39	0.52
Observations	3437	3437	3437	3437
B: HIV-Negative Estimates by Attractiveness				
Treatment · Attractive	0.10***	0.14***	0.046***	0.056***
	(0.038)	(0.037)	(0.018)	(0.018)
Treatment · Not Attractive	-0.0094	-0.050	0.021	0.013
	(0.048)	(0.046)	(0.022)	(0.020)
Significance of controls (p-value)	-	0.00	-	0.00
Equality of coefficients (p-value)	0.06	0.00	0.35	0.12
Observations	3200	3200	3200	3200
Control for:				
Treatment · Demographics	No	Yes	No	Yes
Treatment · SES	No	Yes	No	Yes
Treatment · Time preferences	No	Yes	No	Yes

Note: Clustered standard errors appear in parentheses. All regressions include Waves $2-8$ and reweight to balance by age. Estimates control for the baseline dependent variable and wave indicators. Demographic controls include tribe, religion, and age. SES controls include employment, durable roof, durable floor, electricity, telephone ownership, and television ownership. Time preference controls include future orientation and subjective mortality risk within 1, 5, and 10 years. Controls 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)	(3)	(4)
A: Main Effects				
Treatment	-0.025	-	-0.004	-
	(0.018)	-	(0.014)	-
	[0.03]	-	[0.16]	-
Control mean	0.59	-	0.14	-
Observations	3308	-	3308	-
B: Baseline-Unmarried Estimates by HIV Status				
Treatment · HIV Negative	-0.030	-0.038	-0.008	-0.008
	(0.031)	(0.034)	(0.019)	(0.021)
	[0.07]	[0.07]	[0.04]	[0.06]
Treatment · HIV Positive	-0.030	-0.062	0.031	0.025
	(0.065)	(0.066)	(0.049)	(0.045)
	[0.57]	[0.09]	[0.55]	[0.69]
Equality of coefficients (p-value)	0.94	0.99	0.39	0.38
Observations	1649	1649	1649	1649
C: HIV-Negative Baseline-Unmarried Estimates by Attractiveness				
Treatment · Attractive	-0.034	-0.024	-0.030	-0.033
	(0.035)	(0.044)	(0.025)	(0.030)
	[0.00]	[0.00]	[0.00]	[0.00]
Treatment · Not Attractive	-0.0025	-0.0082	0.018	0.0064
	(0.050)	(0.056)	(0.030)	(0.034)
	[0.69]	[0.45]	[0.45]	[0.54]
Equality of coefficients (p-value)	0.54	0.66	0.32	0.45
Observations	1511	1511	1511	1511
Control for:				
Treatment · Demographics	-	Yes	-	Yes

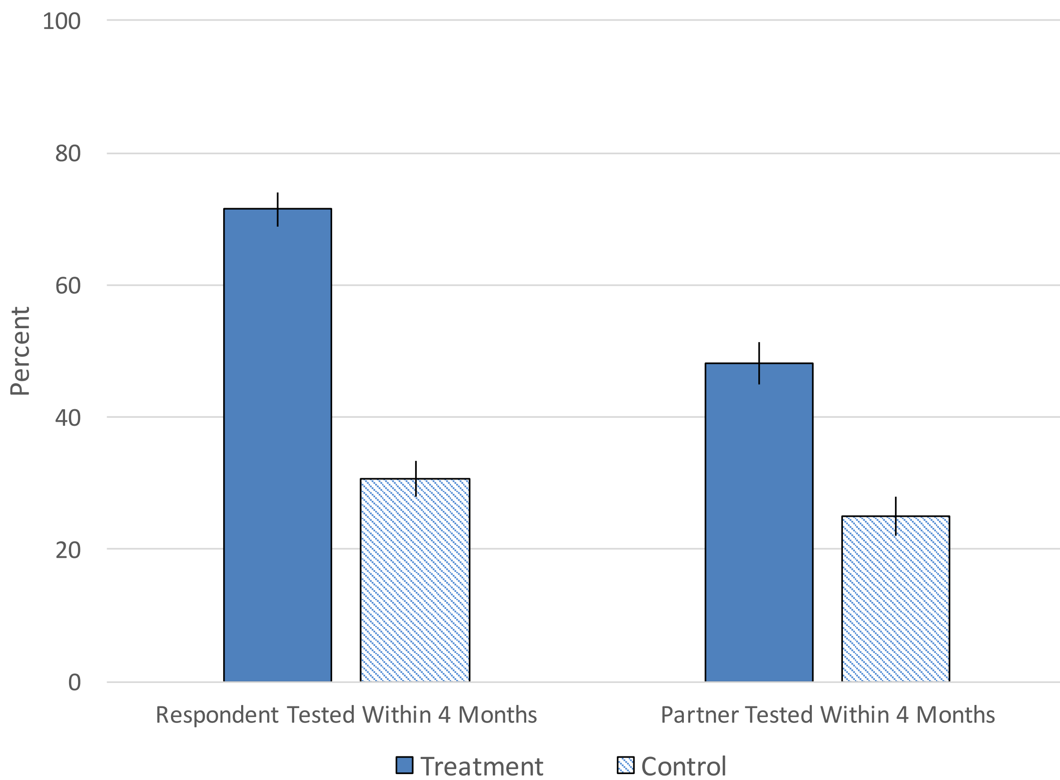


Figure 1: Probability of Testing within Four Months by Treatment Arm

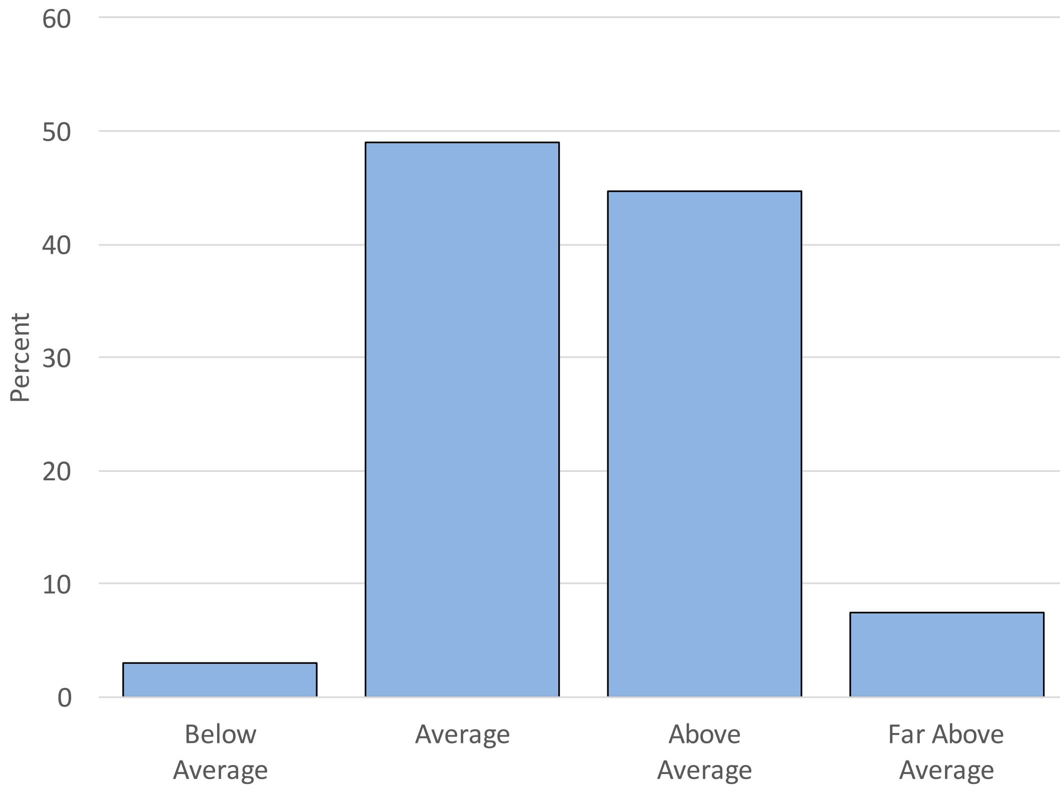
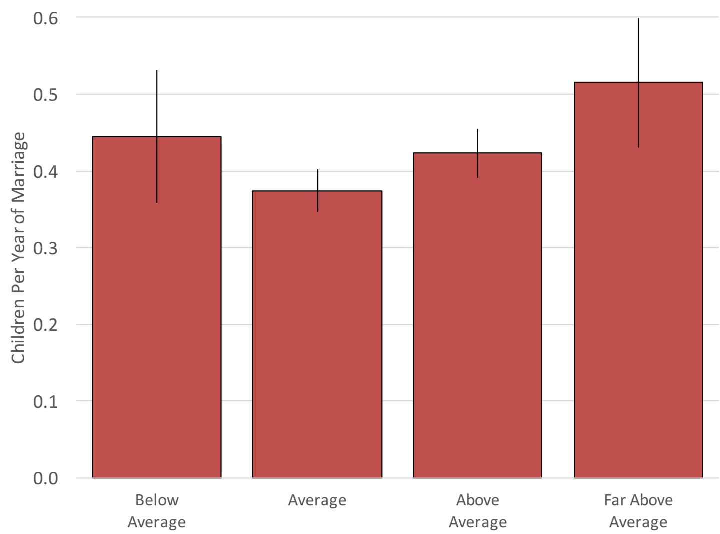
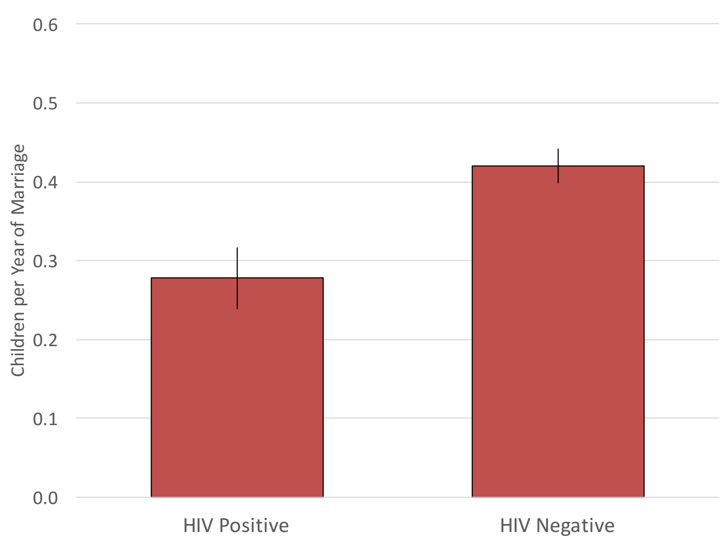


Figure 2: The Distribution of Baseline Attractiveness

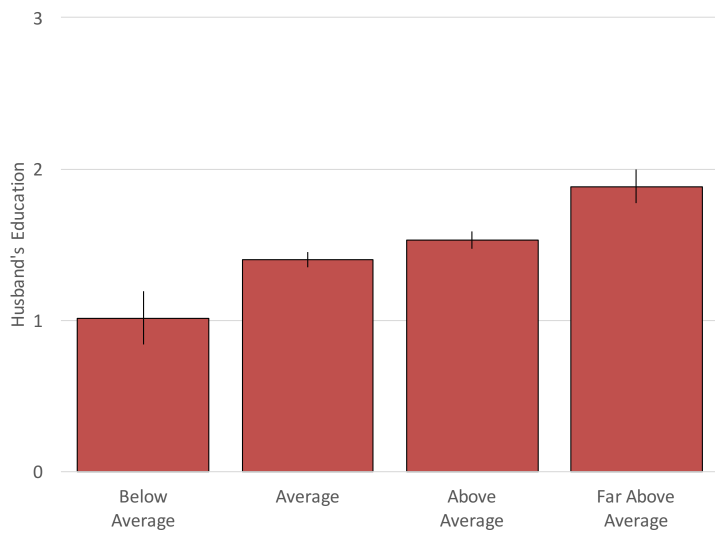


(a) By Beauty

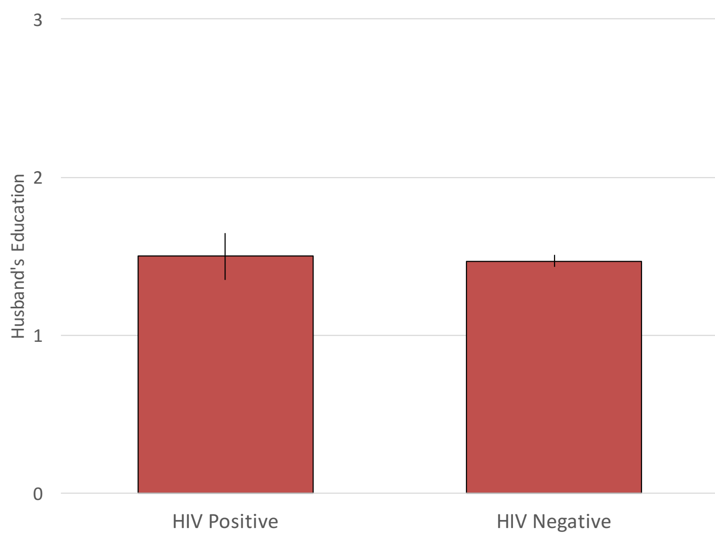


(b) By HIV Status

Figure 3: Children per year of Marriage at Baseline

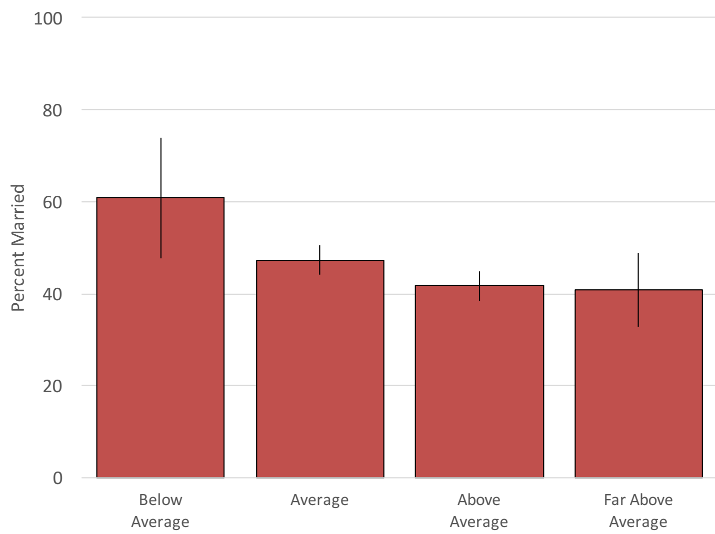


(a) Husband's Education by Wife's Beauty

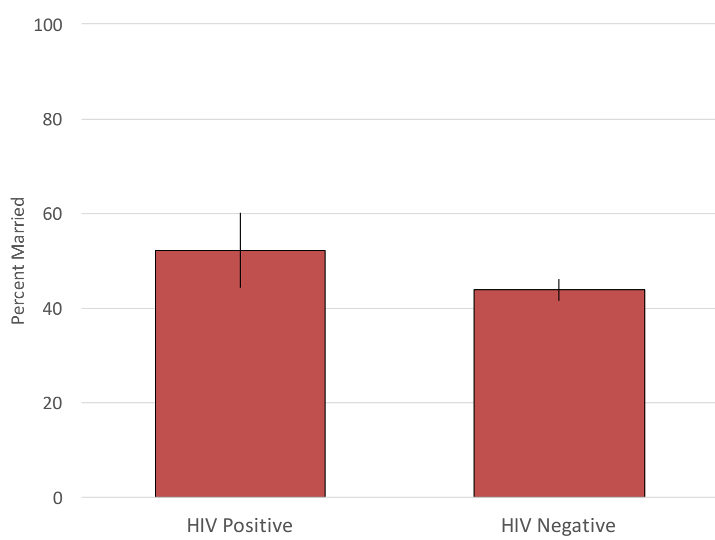


(b) Husband's Education by Wife's HIV Status

Figure 4: Evidence of Assortative Matching at Baseline



(a) Marriage Probability by Beauty



(b) Marriage Rate by Wife's HIV Status

Figure 5: Baseline Marriage Probabilities

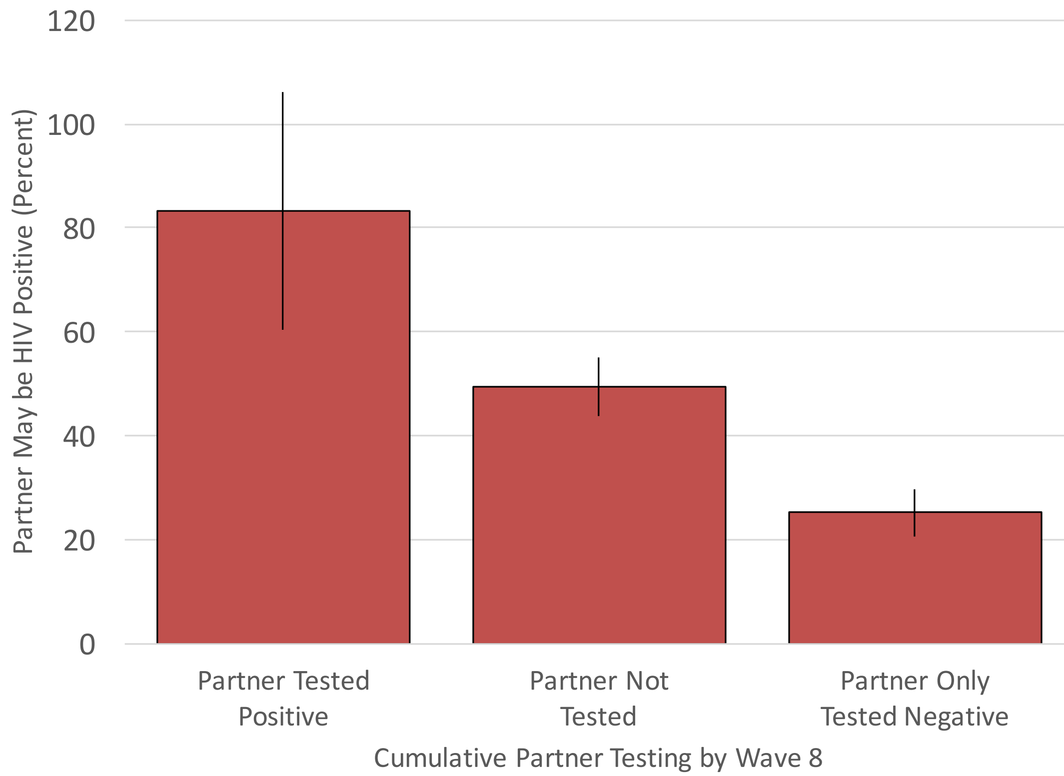


Figure 6: Perceptions of Partner HIV Status at Wave 8

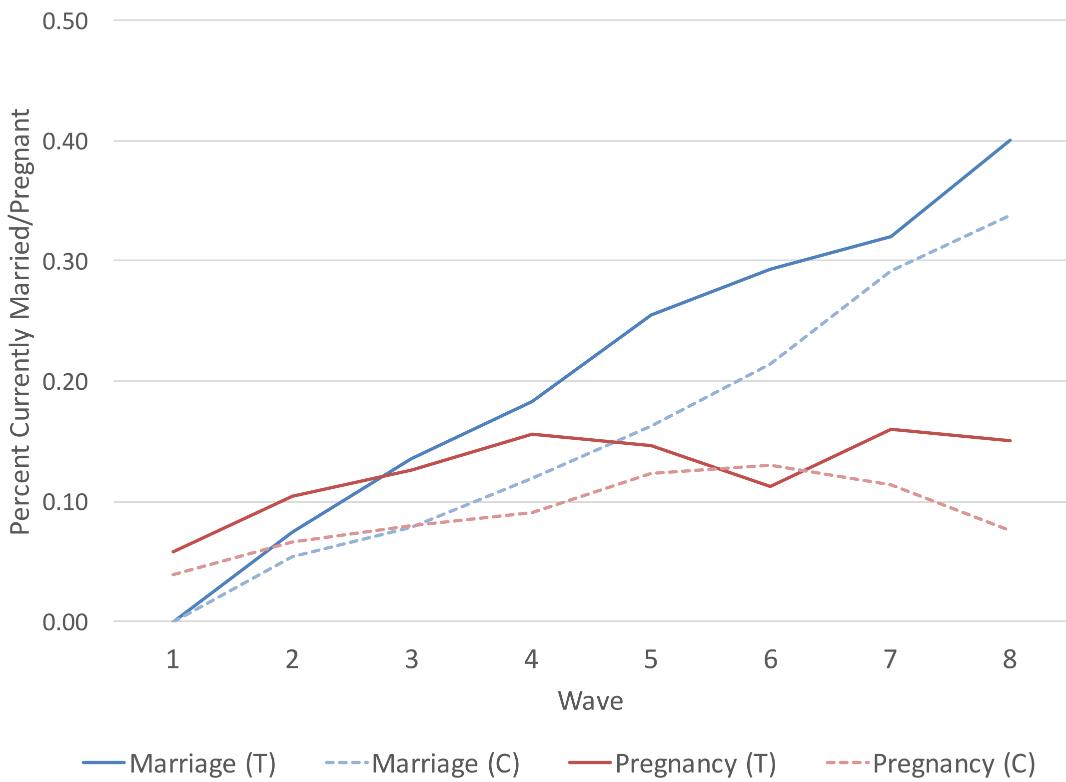


Figure 7: Marriage and Pregnancy for HIV-Negative Baseline Singles

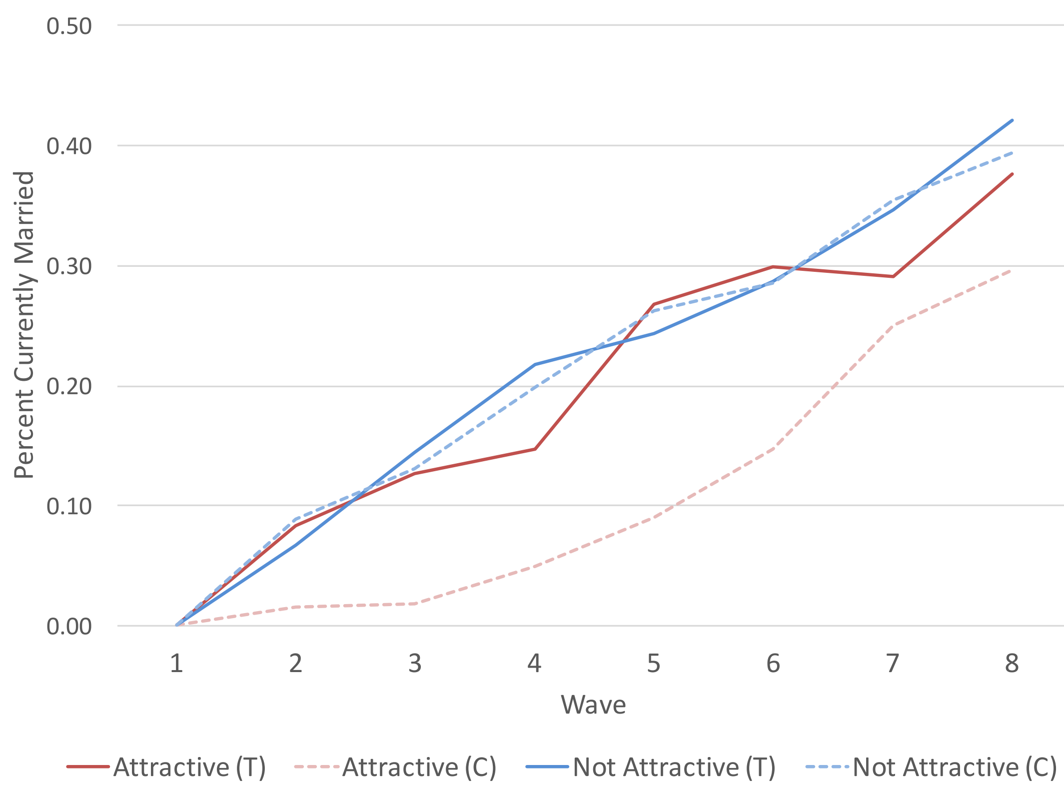


Figure 8: Marriage by Attractiveness for HIV-Negative Baseline Singles

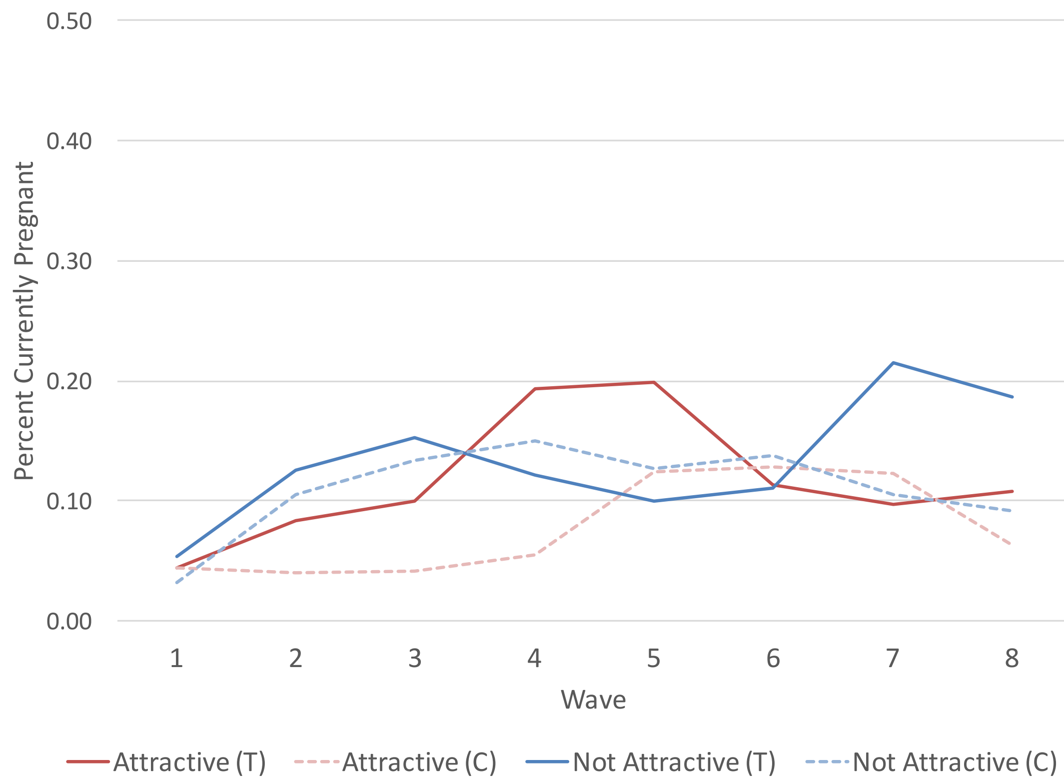


Figure 9: Pregnancy by Attractiveness for HIV-Negative Baseline Singles

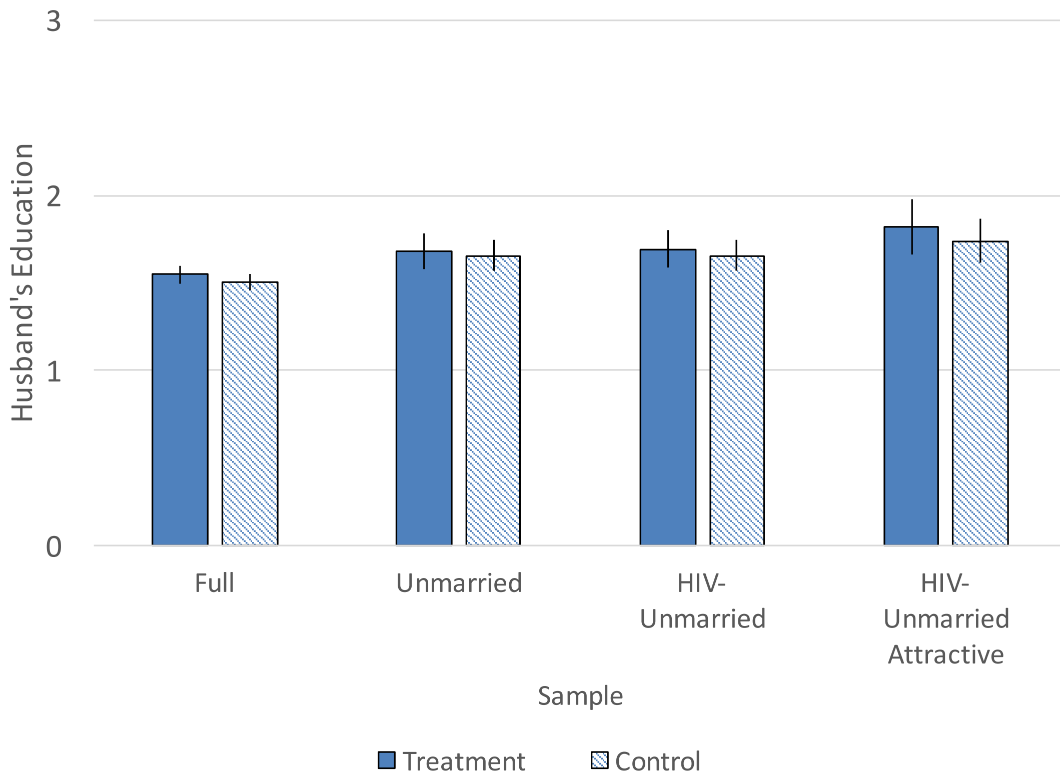


Figure 10: Husband's Education for Married Women at Wave 8

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

For the following relationship to hold, $S^{ab}_{hh} > S^{ab}_{hl} = S^{ab}_{lh} > S^{ab}_{ll} > 0$, each trait must have a similar effect on marriage surplus. However, if health were a larger determinant of surplus than attractiveness, marrying an unattractive but healthy person, a type lh, would yield a higher surplus than marrying an attractive but unhealthy person, a type hl and, therefore, 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 change Proposition 1: in this case, in Period 2, people would marry a type with their exact traits, rather than a type with the same number of high traits. Conversely, all the other propositions do not change. In particular, there would still be assortative matching in attractiveness in Period 1. To see why this is the case, consider the case in which all healthy people marry in Period 2. In that case, there is assortative matching in attractiveness in Period 1 because attractive women, who are all unhealthy, maximize marriage surplus by marrying attractive men, who are also all unhealthy, rather than marrying unattractive and unhealthy men, which is the other alternative option. Suppose now that some healthy women prefer to marry in Period 1. There is assortative matching in attractiveness also in this case. If the average surplus from marrying an attractive partner is higher than marrying an unattractive partner, then all want to marry attractive men, but attractive men prefer to marry attractive women. On the other hand, if the average surplus from marrying an attractive partner is lower than marrying an unattractive partner, then all want to marry unattractive men, but unattractive men prefer to marry unattractive women. Propositions 3 to 5 are also unchanged qualitatively, as they do not depend on the different ranking of surpluses. However, the incentives for delaying marriage are now higher for healthy women, as the cost of marrying an unhealthy partner in Period 1 is greater than in the previous version of the model. Therefore, making marrying unhealthy men less appealing exacerbates the adverse selection in the marriage market in Period 1.

In this setting, 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. This version of the model also implies that making health observable in Period 1 has no effect on the marital status and surplus of already married women, since divorce is ruled out and the average surplus does not change.

A.2 Divorce Costs

We modeled divorce costs as being sufficiently high to prevent married people from divorcing. If we relax this assumption and, for simplicity, make divorce costless, Propositions 1 to 3 do not change, while 4 and 5 do. The main difference is that everybody marries a person with the same attractiveness in Period 1 because doing that gives a strictly positive surplus. In Period 2, people who are mismatched 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.

A.3 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, our Propositions do not change. In fact, 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. All the other predictions are unchanged.

A.4 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 spirit of the propositions 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.5 Own Health is Unobservable

We also consider a scenario in which people do not know their own health in Period 1. In that case, Propositions 1 and 2 do not change: marriage is still assortative in the number of high traits in Period 2 and in attractiveness in Period 1. Propositions 3 and 4 change because all people have an expected health equal to the population average and will all behave identically, conditional on attractiveness. Therefore, sufficiently patient healthy and unhealthy women marry in Period 2, while impatient healthy and unhealthy women marry in Period 1. Proposition 5 does not change as, under some conditions, attractive women are 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.

A.6 Endogenous Health

Lastly, we consider the implication of health being endogenous. If being unhealthy had zero cost, then every woman, attractive and unattractive, would choose to be healthy, because that gives the highest marital surplus (and the highest utility, although we do not model it). From this fact, we can deduce that being healthy must be costly for some women. In our empirical setting, in which our unobserved dimension of health is HIV risk, there may be variation in access to condoms or, more generally, in the cost of safe sex.

Endogenizing health by creating variation in the cost of being healthy does not change the mechanics and predictions of the model, regardless of whether this cost is (i) orthogonal to traits and time preferences, (ii) correlated with attractiveness, or (iii) correlated with time preferences.

In the first case, high health cost women are equally represented in all groups and none of our Propositions are affected. In the second case, the correlation between health and attractiveness changes the population proportions of the four types of women, p_{hh}, p_{lh}, p_{hl}, and p_{ll}, which are common knowledge. While none of our Propositions are affected, Proposition 5 implies that, the more negative the correlation between health and attractiveness, the higher the incentives to postpone marriage for attractive and healthy women, because there are more "lemons" among attractive types, i.e., $p_{hl} > p_{ll}$.

In the third case, e.g., if patient women are more likely to be healthy, none of the Propositions are affected. By Proposition 4, the higher the patience of healthy women, the higher the fraction who will postpone marriage.[21]

B Multiple Equilibria and the Demand for HIV Testing

This section describes the demand for HIV testing as a coordination game. Section 2 argues that HIV testing has substantial marriage market benefits for healthy people. It may therefore seem paradoxical that only 38 percent 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, simply seeking a test may connote promiscuity and HIV risk to observers in the community. This cost is lower if seeking an

[21] If attractive women are more patient and patient women are more likely to be healthy, we are back in the second case, but, this time, the correlation between health and attractiveness is positive and, therefore, attractive and healthy women have fewer incentives to postpone marriage than unattractive and healthy women.

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 on-site, opt-out HIV testing as part of the TLT Panel Study. 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 (i.e. that β may be large) 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 Age-Unweighted Estimates

This section provides additional detail regarding the age imbalance in the data. Figure A1 shows the unweighted age distributions of the treatment and control arms. Treatment respondents are an average of 0.6 years older than control respondents. The overall age imbalance arises because there are around 57 "extra" control respondents who are 15 or 16 years old. However the figure indicates that 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 AD reproduces Table 1 from the paper but does not reweight to balance by age. Without reweighting, several additional variables are unbalanced across treatment arms. Treatment respondents are more likely to be pregnant and less likely to be enrolled in school. They are also slightly more likely to have HIV. These patterns are the expected result of an age imbalance since pregnancy and school enrollment are highly age dependent. A comparison with Table 1 shows that none of these variables are significantly imbalanced after reweighting by age. Unweighted mean comparisons by HIV status and attractiveness, which closely resemble the results in Table 5, are available from the authors.

In contrast several other variables do not differ significantly, including the marriage rate, attractiveness, employment, and household assets. Surveyors cannot identify any issue with the sampling methodology that generated this issue. The balance across age-orthogonal characteristics in this table, including religion, tribe, and household assets, suggests that the age imbalance is due to chance rather than through a flaw in the randomization procedure.

Next we examine the impact of age weighting on our estimates. Table AD reproduces our main results without reweighting by age. In general, estimates become slightly larger and more statistically significant without age weights. Panel A shows the main effects on marriage and pregnancy in the full sample. Comparing Tables 5 and AD, the impact on marriage increases from 0.035 to 0.045 and the impact on pregnancy increases from 0.021 to 0.025. The interactions with HIV status and attractiveness in Panels B and C closely resemble earlier estimates. In addition, estimates that control for age rather than reweight (available from the authors) closely resemble the estimates in the paper. These findings indicate that the age imbalance and the weighting procedure are unlikely to significantly influence our results.

D Estimates for Marriage-Pregnancy Interactions

This section provides estimates for the interaction between marriage and pregnancy. We create two additional variables: currently married · currently pregnant and currently unmarried · currently pregnant. A comparison of the impacts on these outcomes provides suggestive evidence about whether fertility impacts primarily occur within or outside of marriage. If the impact on fertility is uncorrelated with marital status, then the estimates for the two outcomes should be similar. However if fertility impacts are correlated with marriage impacts, then estimates for marriage · pregnancy should be larger.

Estimates for these outcomes appear in Table AD. The table provides the key estimates in the paper. Panel A shows main effects in the full sample. A comparison of Columns 1 and 2 shows that the impact on marriage · pregnancy is substantially larger than the impact on non-marriage · pregnancy. The Column 1 point estimate is 400 times larger than the Column 2 point estimate. Even after normalizing by the dependent variable means, the partial elasticity in Column 1 is 50 percent larger. Consistent with the specification in the paper, Panel B limits the sample to baseline-unmarried respondents and shows HIV status interactions while Panel C limits the sample further to baseline-unmarried HIV-negative respondents and shows attractiveness interactions. In Column 1, the pattern for marriage · pregnancy closely resembles the pattern in the paper with larger estimates for HIV-negative and attractive respondents. However in Column 2

Table A1: Wave-4 Characteristics by Treatment Status for One-Shot Testing

	Full Sample		
	Treatment	Control	P-value
	(1)	(2)	(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**
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	0.65	0.60	0.13
Partner may be HIV positive	0.47	0.45	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 A2: Estimates by Attrition Status

	Currently Married		Currently Pregnant	
	(1)	(2)	(3)	(4)
A: Main Effects				
Treatment	0.033*	0.035*	0.023**	0.021*
	(0.019)	(0.019)	(0.011)	(0.011)
Treatment · # Missing Waves	-	-0.0038	-	-0.002
		(0.012)		(0.008)
B: Baseline Unmarried Estimates by HIV Status				
Treatment · HIV Negative	0.057	0.064*	0.043***	0.041***
	(0.035)	(0.034)	(0.016)	(0.015)
Treatment · HIV Negative · # Missing Waves	-	-0.016	-	-0.0078
		(0.018)		(0.0097)
Treatment · HIV Positive	-0.072	-0.12	-0.026	-0.044
	(0.14)	(0.14)	(0.065)	(0.063)
Treatment · HIV Positive · # Missing Waves	-	-0.068*	-	-0.026
		(0.040)		(0.016)
C: HIV-Negative Baseline-Unmarried Estimates by Attractiveness				
Treatment · Attractive	0.091**	0.11**	0.041*	0.042**
	(0.044)	(0.042)	(0.021)	(0.020)
Treatment · Attractive · # Missing Waves	-	-0.0027	-	0.0066
		(0.020)		(0.011)
Treatment · Not Attractive	0.0084	0.0049	0.035	0.031
	(0.055)	(0.052)	(0.024)	(0.023)
Treatment · Not Attractive · # Missing Waves	-	-0.041	-	-0.031*
		(0.031)		(0.017)
Sample	Non-Attriters	Full	Non-Attriters	Full

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

Table A3: 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**
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	0.63	0.56	0.02**
Partner may be HIV positive	0.48	0.44	0.45
Observations	500	507	-

Note: means are not weighted by age (in contrast to Table 1). 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: Age-Unweighted Estimates

	Currently Married		Currently Pregnant	
	(1)	(2)	(3)	(4)
A: Main Effects				
Treatment	0.045***	0.028	0.027***	0.022*
	(0.017)	(0.019)	(0.0099)	(0.012)
Control mean	0.49	0.65	0.13	0.14
Observations	6048	4563	6048	4563
B: Baseline-Unmarried Estimates by HIV Status				
Treatment · HIV Negative	0.077***	0.062	0.039***	0.039**
	(0.028)	(0.039)	(0.013)	(0.018)
Treatment · HIV Positive	-0.15	-0.18	-0.033	-0.055
	(0.13)	(0.14)	(0.059)	(0.067)
Control mean (HIV negative)	0.15	0.21	0.10	0.11
Control mean (HIV positive)	0.41	0.43	0.13	0.16
Equality of coefficients (p-value)	0.08	0.09	0.23	0.18
Observations	3437	1996	3437	1996
C: HIV-Negative Baseline-Unmarried Estimates by Attractiveness				
Treatment · Attractive	0.12***	0.11**	0.046***	0.051**
	(0.036)	(0.049)	(0.018)	(0.024)
Treatment · Not Attractive	0.016	-0.024	0.026	0.014
	(0.044)	(0.065)	(0.020)	(0.030)
Control mean (attractive)	0.10	0.15	0.08	0.09
Control mean (not attractive)	0.22	0.31	0.12	0.14
Equality of coefficients (p-value)	0.07	0.11	0.44	0.32
Observations	3200	1794	3200	1794
Sample	Full	Age $\geq$ 17	Full	Age $\geq$ 17

Note: Clustered standard errors appear in parentheses. 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 balanced. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Estimates for the Interaction Between Marriage and Pregnancy

	Married & Pregnant	Unmarried & Pregnant
	(1)	(2)
A: Main Effects		
Treatment	0.020**	0.004
	(0.0089)	(0.0052)
Control mean	0.10	0.03
Observations	6048	6048
B: Baseline-Unmarried Estimates by HIV Status		
Treatment · HIV negative	0.029***	0.009
	(0.010)	(0.010)
Treatment · HIV positive	-0.063	0.006
	(0.058)	(0.042)
Control mean (HIV negative)	0.06	0.06
Control mean (HIV positive)	0.07	0.05
Observations	3437	3437
C: HIV-Negative Baseline-Unmarried Estimates by Attractiveness		
Treatment · Attractive	0.038***	0.008
	(0.014)	(0.012)
Treatment · Not attractive	0.016	0.004
	(0.016)	(0.016)
Control mean (attractive)	0.05	0.05
Control mean (unattractive)	0.06	0.07
Observations	3200	3200

Note: clustered standard errors appear in parentheses. All regressions include Waves $2-8$ and reweight to balance by age. Estimates control for the baseline dependent variable and wave indicators. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

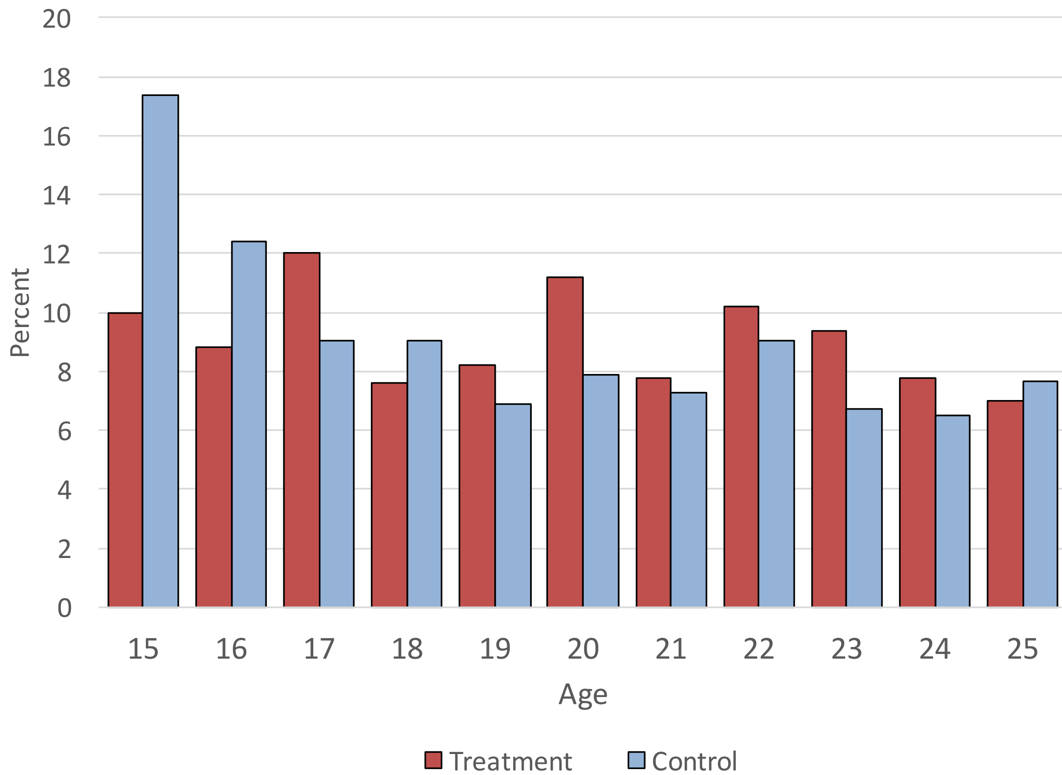


Figure A1: Age Distributions for the Treatment and Control Groups

References

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

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.

Bergstrom, Theodore and Mark Bagnoli, "Courtship as a Waiting Game," *Journal of Political Economy*, February 1993, *101* (1), 185–202.

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.

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

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).

Harwood-Lejeune, Audrey, "Rising Age at Marriage and Fertility in Southern and Eastern Africa," *European Journal of Population*, September 2001, *17* (3), 261–280.

Hosegood, Victoria, Nuala McGrath, and Tom Moultrie, "Dispensing with marriage: Marital and partnership trends in rural KwaZulu-Natal, South Africa 2000-2006," *Demographic Research*, 2009, *20*, 279–312.

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.

Katz, Lawrence, Jeffrey Kling, and Jeffrey Liebman, "Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment," *Quarterly Journal of Economics*, May 2001, *116* (2), 607–654.

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

Marston, M, E Slaymaker, S Floyd, N McGrath, I Kasamba, T Lutalo, M Nyirenda, A Ndyanabo, Z Mupambireyi, and B Zaba, "Trends in marriage and time spent single in sub-Saharan Africa: a comparative analysis of six population-based cohort studies and nine Demographic and Health Surveys," *Sexually Transmitted Infections*, 2009, *85*, i64–i71.

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.

Mukiza-Gapere, Jackson and James Ntozi, "Impact of AIDS on Marriage Patterns, Customs, and Practices in Uganda," *Health Transition Review*, 1995, *5S*, 201–208.

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

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.

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.

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

Watkins, Susan, "Navigating the AIDS Epidemic in Rural Malawi," *Population and Development Review*, December 2004, *30* (4), 673–705.

Weiss, Yoram and Robert Willis, "Match Quality, New Information, and Marital Dissolution," *Journal of Labor Economics*, 1997, *15* (1), S293–S329.

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