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The Impact of Married Individuals Learning HIV Status in Malawi: Divorce, Number of Sexual Partners, Condom Use with Spouses

Theresa M. Fedor
University of Pennsylvania, fedor@sas.upenn.edu

Hans-Peter Kohler
University of Pennsylvania, Dept. of Soc, HPKOHLER@POP.UPENN.EDU

Jere R. Behrman
University of Pennsylvania, jbehrman@econ.upenn.edu

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Keywords

Divorce, HIV testing, HIV testing and counseling, HTC, HIV/AIDS, Malawi, Sexual behavior

Disciplines

Demography, Population, and Ecology | Social and Behavioral Sciences | Sociology

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Theresa M. Fedor • Hans-Peter Kohler • Jere R. Behrman

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T.M. Fedor (corresponding author)
University of Pennsylvania, 3718 Locust Walk (266 McNeil Bldg), Philadelphia, PA 19104-6298, USA
Phone: 404-457-3496
Fax: 215-898-2124
e-mail: fedor@sas.upenn.edu

H.-P. Kohler
University of Pennsylvania, 3718 Locust Walk (272 McNeil Bldg), Philadelphia, PA 19104-6298, USA
e-mail: hpkohler@pop.upenn.edu

J.R. Behrman
University of Pennsylvania, 3718 Locust Walk (229 McNeil Bldg), Philadelphia, PA 19104-6298, USA
e-mail: jbehrman@econ.upenn.edu

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Keywords HIV testing · Divorce · Sexual behavior · Malawi · HIV testing and counseling (HTC)

Introduction

In most of sub-Saharan Africa (SSA) the risk of HIV/AIDS remains very high. In partial response, there has been increased promotion of and access to HIV testing and counseling (HTC) (World Health Organization 2010). Among potential benefits, HTC is assumed to cause behavioral changes that may prevent further spread of the disease. Those who learn they are HIV-positive may take precautions to protect others from infection and those who learn they are HIV-negative may take measures to maintain their HIV-negative status (Thornton 2008). The validity of these assumptions is very difficult to assess because those seeking to know their HIV status through HTC are self-selected (Kranzer et al. 2008). That is, they probably represent a select portion of the population who are driven both to seek testing and to take preventative measures. For this reason, assuming that testing *causes* prevention behavior from associations between testing and behavior change is problematic. This self-selection possibly is the reason for inconsistency among studies that analyze these effects (De Paula et al. 2013; Denison et al. 2008; Fonner et al. 2012; Gregson et al. 1998; Grinstead et al. 2001; Matovu et al. 2005; Porter et al. 2004; Stoneburner and Low-Beer 2004; Thornton 2008).

The current study uses an experimental design with randomized incentives to control for the selection bias into HIV testing in order to clarify the causal effect of HTC on post-testing behaviors. We examine how knowledge of HIV status is used by initially married individuals in ways that may protect against HIV/AIDS risk through (1) divorce, (2) reducing the number of sexual partners and (3) using condoms with spouses. We are able to measure impacts of HTC on subsequent behavior, not just associations, by using data from the Malawi Longitudinal Study of Families and Health (MLSFH), where as part of the 2004 MLSFH round, MLSFH respondents were randomly incentivized through both varying monetary rewards and travel distances for picking up HIV test results at HTC sites. These randomized incentives provide plausible instruments to control for selection in two-stage estimates of effects of learning HIV status on later behaviors.

The most significant contribution of this study is to address the causal relationship between divorce and HIV status by explicitly controlling for self-selection into HIV testing. We are able to contribute to the literature in this way by using a randomized experiment and instrumental variables to estimate the effect of testing on divorce and sexual behavior. The results indicate that knowledge of HIV status (1) does *not* affect chances of divorce for either HIV-negative or HIV-positive respondents; (2) reduces the number of sexual partners among HIV-positive respondents, and (3) increases condom use with spouses for both HIV-negative and HIV-positive respondents. These results imply that individuals actively respond to learning HIV status through HTC, invoking protective behavior against future risk of HIV/AIDS for themselves and their actual and potential sexual partners.

Literature Review

In many places in SSA, including rural Malawi, marriage is nearly universal though there is substantial "marital churning" or divorce and remarriage (Measure DHS 2005). Individuals who are divorced are more likely to be HIV-positive than individuals who are currently married or never married (Boileau et al. 2009; Measure DHS 2008). It is conceivable that the risk of HIV is higher among divorcees because individuals who divorce are more likely to have had a higher number of sexual partners throughout their lives, leading to higher chances of becoming HIV-positive. This implies a path in which divorce increases HIV risk. It is also possible that engaging in risky behavior, such as cheating on a spouse through having other sexual partners, leads to higher chances of both divorce and becoming HIV-positive. This implies that risky behavior leads to both HIV risk and divorce. Another possibility is that knowledge of HIV status, in and of itself, leads to divorce. Several studies have provided evidence supporting this hypothesis, especially for HIV-positive women who may be marginalized due to HIV-positive status (Gregson et al. 1998; Grinstead et al. 2001; Porter et al. 2004).

The idea that risky sexual behavior leads to both increased HIV risk and divorce rates is supported by a growing body of literature. Individuals in high-risk populations may be turning to divorce as a preventative strategy, particularly for women who may use divorce as a means of reducing personal risk of HIV infection when they suspect their husbands of having extramarital sexual relations (Gregson et al. 1998; Reniers 2008; Schatz 2005; Smith and Watkins 2005). One study finds that both men and women in Malawi increasingly use divorce as a risk-reduction strategy when they believe that they are in a marriage that puts them at high risk of HIV infection (Reniers et al. 2009). Smith and Watkins (2005) find that women use divorce to reduce their risk of HIV infection, while men adopt other preventative behavioral changes, such as fewer partners, to mediate HIV risk (Smith and Watkins 2005). A similar study finds that women are less and less likely to tolerate their husbands' extramarital partners due to the risk of HIV, and use divorce as a means of reducing their risk when other strategies to change their husbands' behaviors are unsuccessful (Schatz 2005). Furthermore, the cultural acceptance of women initiating a divorce may be increasing, specifically in cases of spousal infidelities because of the risk their cheating spouses create of bringing HIV into the household (Schatz 2005).

Although changing social norms may now give women more freedom and choice to divorce based on concerns of HIV *risk*, it does not necessarily follow that women will divorce a spouse who is *known* to be HIV-positive. Individuals may be more likely to decide to stay with an HIV-positive spouse, either to care for the spouse when he or she becomes ill, or under the assumption that both spouses must necessarily share the same HIV status. Some religious leaders recommend divorcing a spouse suspected of infidelity, while remaining with a spouse known to be HIV-positive so that the spouse can be cared for when he or she becomes sick with AIDS (Trinitapoli 2012). Divorcing an unfaithful spouse is described as a "window of opportunity" for preventing personal risk (Trinitapoli 2012), and points to the difference in behavioral responses that may occur in situations in which risk is suspected, as compared to situations in which risk and HIV status are known. In contrast to these findings, other studies find that HIV-positive individuals, especially women, suffer stigma and are more likely to get divorced after learning HIV-positive status, although many of these studies potentially suffer from selection bias into HIV testing (Gregson et al. 1998; Grinstead et al. 2001; Porter et al. 2004). One of the benefits of the current study is the ability to examine whether divorce risk increases or decreases once HIV status is known, disentangling one aspect of the complex association between HIV status and divorce.

If individuals remain married after knowledge of HIV-positive status, the degree to which they are likely to reduce risky behaviors is highly consequential because it is estimated that in some parts of SSA up to 60% of new infections are acquired within marriage or cohabitation (Matovu 2010; UNAIDS 2010a). Once HIV status is learned, HIV-positive individuals may be more likely to increase efforts to protect their spouse through condom use within marriage. HIV-negative respondents who learn their status may make active efforts to protect against their spouses' HIV status or suspected behavior through condom use within marriage as well. Respondents who learn that they are HIV-positive, whether remaining married or not, out of altruism may reduce the number of sexual partners. Respondents who learn that they are HIV-negative also may reduce their number of sexual partners as a risk-reducing strategy for their own possible infection and for their partners' possible infection, including their spouses if they remain married.

The results from existing studies examining the effect of HTC on prevention behavior vary widely. A recent article reviewing the literature on changes in risk-reducing behavior after HTC finds an overall decrease in the reported number of sexual partners and increases in condom use among HIV-positive individuals after HTC, but with significant heterogeneity among studies (Fonner et al. 2012). Some studies find no association of HTC with risky behavior or increased risky behavior as an unintended consequence of HTC, especially among HIV-negative individuals (Corbett et al. 2007; Kabiru et al. 2010; Matambo et al. 2006; Matovu et al. 2005; Sherr et al. 2007). However, many other studies find that HTC is associated with reduced risky sexual behavior (Arthur et al. 2007; Cremin et al. 2010; De Paula et al. 2013; Denison et al. 2008; Fonner et al. 2012; Gregson et al. 1998; Grinstead et al. 2001; Mola et al. 2006; Porter et al. 2004; Stoneburner and Low-Beer 2004; Thornton 2008). Regardless of the findings, many of these studies were limited to selective samples from clinic-based HTC centers, comprised only of individuals who sought out HIV testing (Arthur et al. 2007; Cremin et al. 2010; Grinstead et al. 2001; Matovu et al. 2005; Mola et al. 2006). Others were part of a HTC program offered by an employer (Corbett et al. 2007; Matambo et al. 2006), while a few others were able to utilize samples that were part of a randomized survey (Bakari et al. 2000; De Paula et al. 2013; Matovu et al. 2005; Thornton 2008). However, even in randomized samples, those who chose to accept HIV testing are still probably selective and often differ on important characteristics such as HIV status, gender and marital status (Bakari et al. 2000; Matovu et al. 2005; Thornton 2008). Only a few studies have been able to use more advanced methodologies or experimental data in order to improve upon the measurement of behavioral change after HTC (De Paula et al. 2013; Thornton 2008; Thornton 2012).

Data and Methods

The MLSFH is a longitudinal study in Malawi that began in 1998 and was repeated for five subsequent waves between 2001 and 2010. In 1998, the MLSFH randomly selected households from which to interview ever-married women and their husbands in three districts of rural Malawi: Rumphi in the north, Mchinji in the central region and Balaka in the south. A fair amount of attrition has occurred since 1998, mostly due to migration.[1] However, in 2004 the baseline characteristics of the respondents were still comparable to other surveys conducted in Malawi (Anglewicz et al. 2009). Some concern may exist about the representativeness of the data due to attrition after 2004. Migration attrition after the 2004 wave of data was a particular concern for this study because individuals who choose to migrate are more likely to be HIV-positive and are also more likely to move because of divorce (Anglewicz 2012). Data from a 2007 follow up survey, specifically designed to find respondents who were not interviewed in 2006 due to migration, was included with the follow-up data to reduce migration attrition bias. Additional analyses of attrition were also performed to assess the representativeness of the data based on tests from several previous studies (Alderman et al. 2001; Anglewicz et al. 2009; Becketti et al. 1988; Fitzgerald et al. 1998). Results indicate that even though there are differences in observed characteristics between those who attrite and those who do not, these differences are not large enough to significantly bias parameter estimates in regression outcomes. See the supplemental material for complete details of the attrition analysis.

[1] 74% of the attrition between 1998 and 2001 was due to migration (total attrition was 23% of the 1998 sample). Similarly, 54% of the attrition between 2001 and 2004 was due to migration (total attrition was 26% of the 2001 sample).

We use a subsample of data from the 2004, 2006 and 2008 waves of data, as well as a 2007 migrant survey. The 2004 survey includes the experimental design that randomized monetary incentives and distance to HTC sites, encouraging respondents to return for their HIV test results. The 2006, 2007 and 2008 waves of data are combined to form the follow-up data and to assess behavioral changes after learning HIV status in 2004. The majority of respondents in the follow-up sample were taken from the 2006 wave of data (91% of the final sample), but if respondents were not found for re-interview in 2006, the 2007 data was used when possible. If the respondents were not re-interviewed in 2006 or 2007, the 2008 data was used when possible. We only used data from the first MLSFH survey in which respondents participated after 2004, whether that be 2006, 2007 or 2008. Respondents were retested for HIV in 2006, 2007 and 2008 so only using the first survey after 2004 ensures that respondents did not receive additional information from the MLSFH about their HIV status after 2004. Furthermore, when comparing the changes in outcomes for divorce, only divorces recorded between 2004 and 2006 were counted in order to ensure the same window of time for reaction to HIV test results for all respondents, regardless of which follow-up year was used.[2] The final sample is restricted to individuals in 2004 who were married, agreed to the MLSFH-provided HIV test in 2004,[3] provided basic demographic data and were re-interviewed by the MLSFH in either 2006, 2007 or 2008.

Few individuals in the MLSFH knew their HIV status prior to the 2004 MLSFH testing, and few obtained outside testing between 2004 and 2006. In 2004 respondents were offered HTC and approximately 90% accepted. For the majority (85%) it was their first HIV test, which is consistent with Demographic and Health Survey (DHS) data in which 15% of the population in

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[2] Seven divorces were dropped because they occurred in 2007 or 2008.
[3] Minus approximately 10 respondents whose results were indeterminate.

Malawi reported ever having had an HIV test as of 2004 (Measure DHS 2004). HIV testing outside of the MLSFH was not only rare in Malawi before 2004, but increases in testing between 2004 and 2006 were also modest. Substantial gains in testing were made by 2010, although even then only around 53% of the adult population reported ever having had an HIV test (Measure DHS 2010). In the 2006 MLSFH, 80% of those who had previously been tested for HIV had only been tested once and 98% of these tests were done in 2004 with the MLSFH. The final results of the current analysis are robust to exclusion of respondents who reported being tested for HIV again after the 2004 MLSFH testing but before the follow-up assessment of behavioral changes of interest for this study.

In 2004 rapid testing was not available and/or approved in the study area. Therefore only lab-based HIV testing was used, requiring those tested to return several weeks later to learn their results. The MLSFH took advantage of this situation by randomly assigning a monetary incentive for picking up test results a few weeks later. The monetary incentives ranged from no incentive to 300 Malawian kwacha, which was equivalent to approximately two days' average wages for rural Malawians in 2004 (approximately three U.S. dollars in 2004). The location for pick-up of HIV test results was also randomly assigned in each community, thus resulting in random distances from respondents' homes. The average distance to HTC pick-up location from respondents' home was two kilometers (standard deviation 1.26), the maximum distance was 5.2 kilometers and over 90% of respondents lived under four kilometers away. The distribution of the monetary incentives is non-normal, with discontinuities near zero and incentives clustered around 50, 100, 200, 250 and 300 Kwacha. For this reason, the incentives are categorized as no incentive, 10-50 Kwacha, 60-100 Kwacha, 110-200 Kwacha, or 200-300 Kwacha, although results are robust to the incentives being in continuous and other forms. A term for distance to

HTC center squared is added to the models to adjust for the possibility of non-linearity in the relationship between learning HIV status and distance to HTC.

Marital status change from 2004 to follow up was identified using marital history information and is categorized as still married or divorced. Marital status changes due to widowhood are excluded from the analysis. Widowhood constitutes less than 1% of the sample but approximately 21% of marital dissolutions during 2004-06. The small number of widowed respondents partially alleviates concerns of unequal attrition of HIV-positive respondents due to death; however the proportion of widowhood among those who experience marital dissolution after 2004 is large and is acknowledged as a limitation. The divorced category includes individuals who divorced between 2004 and 2006 whether or not they remarried before follow up.[4] Polygamous men are also included in the analysis. The divorced category for polygamous men includes all men who divorced *any* of their wives between 2004 and 2006.

The MLSFH asked all respondents the number of sexual partners they had in the last 12 months, which is used as an outcome variable in continuous form in the current analysis. Other specifications of the number of partners in the last 12 months were also explored, including a dichotomous variable for zero or one partner as compared to 2 or more partners, and zero versus one or more sexual partners. Results were similar to those using the continuous form of this variable.[5] We recognize that measurement error in the number of sexual partners reported is probable. This pattern of reporting often follows gender-specific norms, with women reporting fewer sexual partners than men in face-to-face interviews. The degree to which this is due to misreporting is unclear but previous research on reports of sexual behavior in Malawi suggests

[4] Of those who divorced after 2004, approximately 80% remarried by follow-up.
[5] A variable indicating an increase in the total number of sexual partners ever was also explored, as well as dichotomous specifications of this measure. They were found to be inferior and inconsistent as compared to the variables for the number of sexual partners in the last 12 months, most likely due to misreporting.

that misreporting may be fairly high (Helleringer et al. 2011; Mensch et al. 2008). In some instances, reports of sexual behavior, while lower than expected, are more internally consistent in face-to-face interviews than while using alternative interviewing techniques designed to reduce reporting bias, such as audio computer-assisted self-interviewing (Mensch et al. 2008). Therefore, while sexual behavior is thought to be underreported by women and overreported by men, there is reason to believe that there is a benefit to using face-to-face reports of sexual behavior in terms of consistency between questions and between survey waves.

Condom use with husbands, wives and live-in partners in 2004, 2006 and 2007 is used to determine change in this variable after learning HIV status. Questions about condom use within marriage were not asked in 2008. Constructed response categories move loosely from low to high frequency and are as follows: never, sometimes, almost every time, and every time. If a respondent indicates a higher frequency category in 2006 or 2007 as compared to 2004 then condom use is coded as increasing. Several other specifications of condom use and change in condom use were also estimated, all of which resulted in the same substantive conclusions.[6]

Several pre-determined control variables that were not affected by the 2004 test results are included to increase precision of the estimates: age in 2004[7], region of residence and level of schooling in 2004.[8] Region of residence is either Rumphi in the north, Mchinji in central Malawi or Balaka in the south. Schooling level is grouped as no schooling, at least some primary school, or at least some secondary school.[9]

[6] The level of condom use was assessed as a continuous outcome, plus several dichotomous outcomes were created indicating less condom use at follow-up, increase in condom use as compared to "consistent" condom use at follow-up, a change in condom use from "never" to "any," and have ever used a condom as measured at follow-up.
[7] Age in 2004 is estimated by the interviewer for 33 respondents in 2004 and 7 in 2008 (2.2% of the total sample).
[8] Age and schooling level are taken from the 2004 data but missing values in 2004 are imputed from the 2006, 2007 and 2008 data (0.4% of the age variable and 13.5% of the schooling variable).
[9] The last category also includes schooling past secondary school (n=2).

In order to investigate the causal effects of learning one's HIV status in 2004 on subsequent divorce and sexual behaviors, two-stage probit and least squares models are estimated. Two-stage least squares linear regression models are only estimated for the continuous outcome, the number of sexual partners in the year of follow-up. In the first stage, the monetary incentives and distance to HTC center are used as instruments to predict the likelihood of picking up HTC results and learning one's HIV status. In the second stage of the model, the causal effect of learning one's HIV status on: (1) divorce, (2) the number of sexual partners during the follow-up year and (3) condom use within marriage are estimated using the first-stage predicted values of learning one's HIV status. The first-stage specification is as follows:

$$Know_i = \alpha_0 + \alpha_1 IncentiveAmount_i + \alpha_2 Distance_i + \alpha_3 Distance_i^2 + \boldsymbol{X_i \alpha'_x} + \nu_i \qquad (1)$$

where $Know_i$ is the value of learning HIV status (binary variable for picking up HTC test results as part of the 2004 MLSFH HTC) for respondent i, $IncentiveAmount_i$ is the amount of the incentive offered as part of the 2004 MLSFH HTC, $Distance_i$ is the distance to HTC center and $\boldsymbol{X_i}$ is a vector of exogenous or predetermined covariates where $\boldsymbol{\alpha'_x}$ is the vector of coefficients for each X covariate (age, education level and region). The second-stage probit model is as follows:

$$\Pr\,(Y_i = 1 \mid \boldsymbol{X_i}, \widehat{Know}_i) = \Phi(\beta_0 + \beta_1 \widehat{Know}_i + \boldsymbol{X_i \beta'_x}),\ given\ \varepsilon_i \sim N(0,1) \qquad (2)$$

where Y_i is the final outcome variable, $\widehat{Know}_i$ is the predicted value of learning HIV status as estimated in the first-stage model and $\boldsymbol{X_i}$ is a vector of covariates where $\boldsymbol{\beta'_x}$ is the vector of coefficients for each X covariate. The use of a binary endogenous regressor in models with limited dependent variables has been validated with strong evidence of similar average effects regardless of the nonlinearity that arises due to the binary nature of the endogenous variable in this model (Angrist 2001).

Regular probit and linear regression models are also included in the analysis for comparison, with the expectation that coefficients will change in the IV probit and IV regression models due to reductions in bias. The analysis is divided by HIV status because it is expected that the propensity to divorce, have more sexual partners or use condoms after learning HIV status may be very different for HIV-negative versus HIV-positive respondents. The analysis is further divided by sex for HIV-negative respondents because men and women are subject to different constraints when making decisions about divorce and sexual behavior. The analysis is not subdivided by sex for HIV-positive respondents because of the small sample size for those who are HIV-positive.

Results

Descriptive statistics for the sample, separated by HIV status and sex, are presented in table 1. Panel A describes the three outcome variables and panel B gives descriptive information about the other variables in the models. In panel A we see that 19% of HIV-positive respondents divorced after 2004 as compared to only 4 or 5% of HIV-negative respondents. Almost 16% of HIV-positive respondents report no sexual partner in the year of follow-up. In contrast, 5% of HIV-negative women report no sexual partner and only approximately 1% of HIV-negative men report no sexual partner at follow-up. Almost no women report more than one sexual partner at follow-up, reflecting gender biases in reports of sexual behavior. However, these numbers are quite consistent with DHS data from 2004 on reported number of sexual partners and the mean number of sexual partners (Measure DHS 2005). 28% of HIV-positive respondents report increased condom use with their spouse after 2004 while 15 or 16% of HIV-negative respondents report increases in condom use. Panel B shows fairly equivalent distributions in incentive amounts and distances to HTC centers between men, women and HIV-positive respondent. Also, HIV-positive respondents were usually 7 or 9 percentage points less likely to pick up test results as compared to HIV-negative men and women in the sample.

The first-stage results of the two-stage models are shown in table 2. These show that the randomized monetary incentives offered to respondents for learning their HIV test results are important and significant predictors of whether a respondent indeed learned his/her HIV status during the 2004 MLSFH. The results for each outcome are very similar, and the regression results differ primarily with respect to variations in sample size, which is the result of differences in responses for each outcome variable. The monetary incentive affects the propensity to pick up HIV test results, with higher incentive amounts resulting in greater likelihood of picking up results among all outcomes. The statistical significance of the coefficients in table 2 for the incentive amounts shows that this is a relevant predictor of the endogenous variable, learning HIV status. Distance to the assigned HTC center is a less effective instrument for predicting pick up of HIV test results, but shorter distances to HTC sites still increase propensity to pick up results in some instances, in particular for the number of sexual partners outcome. The F-statistic values in the first-stage models provide further evidence of instrument strength. The conventional "rule of thumb" is that values above ten indicate sufficient predictive power of the instruments (Wooldridge 2009). The F-statistics in the HIV-negative models range from 37.06 to 21.56, indicating good prediction of learning HIV status and lending confidence to the ability of the instruments to randomize the sample of individuals choosing to learn their HIV status among HIV-negative respondents. The models for HIV-positive respondents yield less confident results, with F-statistics between 8.86 and 5.69. The potential limitations resulting from these relatively weak F tests will be discussed below.

Tables 3, 4 and 5 report the marginal effects for probit and second-stage IV probit models predicting the effect of learning HIV status on divorce and condom use, as well as the OLS and second-stage IV regression coefficients for the continuous outcome, number of sexual partners in the year of follow-up. Table 3 reports results for HIV-negative women, table 4 for HIV-negative men and table 5 for HIV-positive respondents.

Models 1 and 2 of table 3 report the second-stage probit and IV probit results for the effect of knowing HIV status on divorce among HIV-negative women. Model 1 indicates that learning HIV status for the first time decreases risk of divorce by 2.8% in the following two years among HIV-negative women (significant at the 10% level). However, results for the second-stage IV model in column 2 do not retain significance, and the point estimate decreases from .28 to .19 (a decline of 32%). This highlights the selection bias present in the non-instrumented models and the reduction in that bias through the instrumentation of learning HIV test results. The IV estimates in Model 4 suggests that learning HIV status increases the number of additional sexual partners for HIV-negative women by 0.085, an effect that is substantially higher (by 54%) than that indicated by the OLS analyses that are subject to endogeneity concerns about learning one's HIV status. Recalling that the vast majority of women reported either zero or one partner, this increased chance of having a partner among HIV-negative women who learn their status may reflect a lack of marginalization among these women because of their known HIV-negative status. Learning HIV status does not significantly affect condom use in the regular probit model but in the instrumental variable model we find a statistically significant and substantively important 15.5% increase in condom use with spouses among HIV-negative women who learn their status.

Table 4 presents the second-stage results for the effect of knowing HIV status on subsequent behaviors among HIV-negative men. Results are not significant in either the regular probit models or the IV probit models for the divorce outcome and the outcome for the number of sexual partners in the year of follow-up. Probit results in model 6 indicate that men may appear to increase condom use with their spouse after learning of HIV-negative status, but this estimated effect is substantially diminished in the IV probit model and does not retain statistical significance. This leads us to the conclusion that the results from the regular probit model were an artifact of the bias in those who return to pick up their HIV test results and that overall, men are not affected by learning HIV-negative status.

Table 5 presents the second-stage results for the effect of learning HIV status among HIV-positive respondents. Before accounting for selection into choosing to learn HIV status, the probit model for divorce among HIV-positive respondents (table 5, model 1) indicates that learning HIV status for the first time decreases the risk of divorce in the two years following by almost 14% (significant at 10%). Similar to the results for HIV-negative women, results from the second-stage IV model (model 2) do not retain statistical significance, suggesting no effect of learning HIV status on divorce among HIV-positive men and women in general. Results for both the number of sexual partners and condom use with one's spouse show the opposite pattern, seeming to indicate that learning HIV status has no effect on influencing partner number or condom use in the regular probit and OLS models, but showing marginal significance in the IV models. Learning HIV status decreases the number of sexual partners at follow-up for HIV-positive respondents by 35% and increases condom use with one's spouse after 2004 by approximately 39%.

The results for HIV-positive respondents should be interpreted with caution given the small sample size for HIV-positive individuals and the low F-statistics from the first-stage estimation of learning HIV status. It also would have been preferable to separate the analysis for men and women who are HIV-positive. The small sample of HIV-positive respondents, even when pooled for both men and women, still calls into question the validity of the results among those who are HIV-positive. The simplest power calculation for the divorce outcome (n=106) gives a power of only 0.59 at alpha = 0.10 in detecting differences in divorce by knowledge of HIV status. The required sample size to attain a minimum desired power of 0.80, even at alpha = 0.10, is n = 156 at minimum. Since the IV probit models are underpowered, it is difficult to know with certainty whether the coefficients for learning HIV status accurately represent real differences in the models.

Discussion and Limitations

The main motivation of this paper was to improve upon previous research measuring the effect of married individuals learning HIV status on later behaviors, specifically divorce, number of sexual partners and condom use with spouse. We used instrumental variables to control for selection bias that often probably is present in similar studies. We find that knowledge of HIV status, whether positive or negative, does not lead to increased chances of divorce. Other characteristics of individuals are more likely driving the association between divorce and HIV-positive status. We also observe reduced risky behavior among HIV-negative women and HIV-positive respondents. HIV-negative women who learned their HIV status also report increased condom use, but only after controlling for selection bias for those who choose to be tested for HIV. HIV-positive respondents who learned their status reported fewer sexual partners during the year of follow-up and increased condom use with spouses, but this effect is present only after accounting for selection bias into HIV testing. Taken together, these results imply an active response to knowledge of HIV status that evokes behavioral responses that are self-protective as well as altruistically-driven in the attempt to reduce risk for others.

After correcting for selection into HIV testing, we find that HIV-negative women who know their status are not any less likely to divorce as compared to HIV-negative women who do not know their status. However, women who actually seek out testing are less likely to divorce after learning of HIV-negative status. The mirror of this is also true, that men whose HIV-negative wives learn their HIV status are also less likely to get divorced, although the point estimate for this result is not statistically significant due to sample size restrictions when linking the sample of spouses who both received HIV testing (results not shown). This self-selected group of women may display a difference in the desire to know their HIV status due to strong perceived HIV risk or marital discord that existed prior to testing. In these cases, seeking to learn HIV status may be a reaction to other events, such as turmoil within the marriage or risk perception, which are the actual drivers affecting chances of divorce. Although women who seek out testing might interpret their HIV-negative status as proof of their spouses' faithfulness or as a reason not to divorce, learning HIV status does not independently influence marital stability for HIV-negative women.

As compared to HIV-negative women who do not learn their status, HIV-negative women who learn their status are slightly more likely to have an additional sexual partner; most often the "additional" partner represents one partner as opposed to zero partners during the year of follow-up.[10] This result exists both before and after accounting for selection into HIV testing, although the magnitude of the effect is greater in the instrumented models. Assurance of HIV-negative status may be a signal to women about the faithfulness of their partner, making them more willing to stay with a partner. Alternatively, women who know they are HIV-negative may feel more able to safely pursue their relationship and fertility intentions after knowing that they are not putting others at risk. Also, if HIV status is communicated to one's partner, men may see their partners' HIV-negative status as an indication of their faithfulness and value as a partner, giving women greater status within their sexual relationships. If HIV-positive women are seen as less desirable sexual partners, as suggested by Gregson et al. (1998) and Porter et al. (2004), then it is also logical to find that women who are known to be HIV-negative may be more highly valued.

HIV-negative women who learn their status are also more likely to increase condom use with their spouse or live-in partner during the year of follow-up as compared to HIV-negative women who did not learn their status. This effect is only observed after accounting for selection into HIV testing, meaning that women who are not seeking out testing are more likely to increase condom use with their spouse or partner. Conversely, this implies that women who would have found out their HIV status regardless of additional incentives were not more likely to increase condom use. This could be true for many reasons. Perhaps when women are interested in learning their HIV status they are interested in ensuring their negative status before becoming pregnant, in which case condom use would not be expected to increase. After taking into account selection into HIV testing, we find increased condom use among HIV-negative women. Increased condom use may be a result of being surprised about an HIV-negative test result or

[10] Results are the same when the number of sexual partners is represented as a dichotomous variable for zero versus one partner.

simply motivated by a desire to protect their newly known HIV-negative status. This is a very plausible explanation considering that many people in Malawi are quite pessimistic about their HIV status, often vastly overestimating their chances of being HIV-positive (Anglewicz and Kohler 2009).

Results are very different for HIV-negative men. Learning HIV-negative status appears to have no effect on divorce, condom use, or the number of sexual partners in the year of follow-up. HIV-negative men who learned their status were more likely to increase condom use with their spouses and live-in partners before adjustments were made for the selectivity of those who chose to know their HIV status. The loss of significance for this result in the instrumental variable model leads to the conclusion that knowing HIV-negative status has no direct impact on condom use within marriage for men in general, but the significant results for the non instrumented model indicates that the select group of men who are more willing to get tested for HIV are also more likely to actively take preventative measures such as condom use.

For HIV-positive respondents, knowing HIV status has no effect on the propensity to divorce in the instrumented models. Before controlling for selection into choosing to learn HIV status, those who are HIV-positive are 14% less likely to divorce. It is possible that those who seek out knowledge of HIV status, regardless of incentive amount received, could be following the recommendations of religious leaders to get tested, and to stay with a spouse who is already HIV-positive in fulfillment of obligations to care for the sick (Trinitapoli 2012). They may also believe that if one spouse is positive then both spouses must be HIV-positive. After controlling for selection into HIV testing, the lack of a significant difference between HIV-positive individuals who learn or do not learn their HIV status indicates that only those who seek to know the results of their HIV test are actively using that information to make decisions about divorce.

However, learning HIV-positive status does result in fewer sexual partners during the follow-up year and a substantial increase in condom use with spouses and live-in partners, regardless of whether the individual was self motivated to learn their test results. The collective interpretation of these results is that married individuals who find out they are HIV-positive are not more or less likely to divorce, but they are more likely to reduce risky behavior, such as multiple sexual partnerships or unprotected sex with their spouse. These results are consistent with altruistic responses to learning HIV status that are assumed in many arguments to provide more access and promotion of HTC in areas of need. The reduction in numbers of sexual partners and increases in condom use among married, HIV-positive respondents is also supported by similar studies that find increases in preventative efforts among those who learn they are HIV-positive (De Paula et al. 2013; Thornton 2008).

These self-reported reductions in risky sexual behavior are encouraging, but a decrease in new HIV incidence would be even more encouraging. This is difficult to explore because very large sample sizes are needed to estimate new incidence due to the fact that HIV annual incidence rates are usually between 1% and 3%. We attempted to estimate whether learning HIV status in 2004 reduced the chances of becoming HIV-positive in the years to follow, even though results are underpowered. The results were not significant but the coefficients were in the right direction for women, suggesting that there could very well be reductions in HIV incidence among women. Details of this analysis can be found in the supplemental material.

Because this study focuses on married respondents, it may be of interest to know whether the same effects would remain or be even larger within couple-based HTC programs. The impact of couple-based HTC programs on HIV prevention efforts has gained credibility and emphasis in the literature recently, with studies often finding an association between couple HTC and increased HIV prevention efforts (Burton et al. 2010; Desgrees-du-Lou and Orne-Gliemann 2008). In order to add to this literature on couple HTC, as well as to make comparisons with the individual level results in this study, we created a proxy for couple HTC by matching the respondents to their spouses who also participated in the program. Due to sample size limitations we were unable to produce stable results so it is unclear whether larger positive effects exist for couples in the sample, or whether the analysis lacked the statistical power to detect a potential doubling of a positive effect within couple-based testing as compared to individual testing.[11]

An interesting change that occurred after the data used in this study had been gathered was the substantial increased availability of free Antiretroviral Treatment (ART) in Malawi. ART became available in the study areas between 2006 and 2008 through Malawi's Ministry of Health and the Global Fund to Fight AIDS, Tuberculosis and Malaria (Baranov et al. 2012). Previous research has found that the availability of ART has had a positive impact on mental health and economic productivity for both HIV-positive and HIV-negative individuals (Baranov et al. 2012). It is possible that the marital and sexual dynamics explored in this paper would also be affected if the availability of ART results in a more optimistic view of future survival and longevity for individuals. It would be of interest in future analysis to explore whether divorce and sexual behaviors are affected by the availability of ART in the study area.

There are several potential limitations to this study. It is probable that the results from this study were underpowered for HIV-positive individuals. Future research utilizing a larger sample of HIV-positive individuals would be beneficial for assessing the effect of learning HIV-positive status on divorce and sexual behavior. The inability to separate the analysis for men and

[11] We were unable to produce estimates for most outcomes because of sample size limitations, but for the one outcome that we were able to produce estimates, the number of sexual partners at follow-up, we did not find any significantly larger effects as compared to separate analysis of the effect of individual HTC (results not shown).

women among the HIV-positive sample is a limitation as well. It is unclear whether the results for divorce, for example, would have been different given the ability to separate the analysis by sex. In additional analyses we did run models separated by sex for HIV-positive respondents and the results seemed to indicate that HIV-positive men may be significantly less likely to get divorced after learning HIV-positive status, while there is no effect of learning HIV-positive status on chances of divorce among HIV-positive women. However, these results were greatly underpowered, with sample sizes for men and women of 42 and 64, respectively. Reliance on self-reported behavior on sensitive topics is also a limitation of this study. It is well-known that self-reported sexual behavior is often subject to inaccuracies (Helleringer et al. 2011; McCallum and Peterson 2012). Fortunately, the conclusions for divorce are not subject to this bias in the current analysis.

The magnitude of the effect of learning HIV status on the outcomes of interest might increase in the instrumental variable models, relative to the standard estimates, for two reasons. It is possible that either random measurement error is biasing the standard estimates towards zero or that some unobserved variable is working in the opposite direction of the effect of learning HIV status, therefore biasing the standard estimates toward zero but not having an affect on the instrumented estimates. The second reason is highly plausible in this instance. For example, if individuals who are more likely to learn HIV status are also more cautious individuals in general, making them less likely to engage in risky sexual behavior, then the standard estimates for *change* in behavior would be biased towards zero. Conversely, the instrumented estimates more accurately represent a broader range of individuals, cautious and not cautious, so learning HIV status might have a larger impact on changes in behavior if learned HIV status is truly a catalyst for reassessing behavioral practices.

It is also possible that there were other sources of learning HIV status. Respondents who did not pick up their HTC results from the 2004 MLSFH study could have learned their HIV status at another point in time before the next MLSFH follow-up survey, although increases in the availability of HIV testing in Malawi were still modest between 2004 and 2006. Furthermore, the potential dilution of the presumed effect of picking-up HTC test results in 2004 may be different for HIV-positive respondents as compared to HIV-negative respondents. HIV-positive respondents potentially have a different probability of seeking to learn HIV status outside of the HTC conducted as part of the MLSFH. It is also possible that HIV-positive respondents were less likely to pick up their results in 2004 if they already knew their status from a test prior to the 2004 survey.[12] If any of these scenarios are true, this would lead to systematic error in the endogenous variable such that the results presented here would be biased downward. Therefore, the conclusions made in this analysis would only be strengthened with the removal of this potential bias.

Another potential limitation is that learning HIV status may not actually change beliefs of HIV status in the black and white manner expected. Evidence has been found that even after learning HIV-positive status in 2004, a fair number of MLSFH respondents stated in 2006 that they had a low chance of being HIV-positive (Delavande and Kohler 2012). It is unclear whether this response accurately reflects true beliefs, or whether it reflects social desirability bias in reporting of STI's. If individuals learned their HIV status but did not truly believe the results, or if beliefs in results changed over time when illness and onset of AIDS did not occur, this would bias the results found here and could be responsible for the lack of significance in the results for divorce, especially among HIV-positive respondents.

[12] The 2004 HIV testing within the MLSFH was the first test for 85% of respondents.

Another important point to recognize is possible limitations in the study design of this project. Although instrumental variable models reduce bias, they may not completely remove all bias stemming from omitted variables and selection. The results of randomized experiments that incentivize certain behaviors such as the one used in this analysis have been contested (Deaton 2010; Easterly 2009; Heckman and Urzua 2009; Imbens 2010). It is possible that those at the margins of certain behaviors or characteristics may be more likely to engage in behavior that deviates from the norm in ways that specifically reduces the effectiveness of incentives designed to encourage pick-up of HTC results. This could lead to endogeneity of the instruments if respondents choose to pick-up results or not pick-up results in a way that is still correlated with the heterogeneity in propensity to pick-up HIV test results. Even though this may be true to a certain degree, it is also certainly true that the attempt to randomize the individuals who pick-up HIV tests will result in *less* biased estimates, even if they are not completely unbiased. Advocates of randomized experiments focus on the importance of the improvement in the ability to accurately measure the effect of such programs, and in the viability of results drawn from statistical methods that fully utilize the advantages inherent in such designs (Imbens 2010). There is certainly value in study designs such as the MLSFH's randomized experiment for picking up HTC results, even if the study design does not necessarily eliminate all bias. The MLSFH experimental design still makes significant progress in reducing bias and endogeneity issues, resulting in improved measurement of the effect of HIV testing on behavioral change.

Conclusion

The decision to divorce (or not) based on knowledge of HIV status has potentially important consequences for HIV prevention and transmission in high HIV prevalence SSA contexts. However, our research in Malawi which utilizes a randomized design, resulting in exogenous variation in HIV-status knowledge, suggests that learning one's HIV status does *not* significantly affect the propensity to divorce in Malawi. Combined with our findings about the changes in the number of sexual partners and the use of condoms during sexual relationships after learning HIV status, this gives a clearer picture of how relationships and HIV prevention are jointly navigated in the high HIV context of Malawi. Our study therefore makes an important contribution to the literature since the push to provide more widespread and regular HIV testing and counseling in SSA is based on the assumption that HIV-negative individuals will protect themselves against future risk of infection, and HIV-positive individuals will be altruistically motivated to take precautions to protect others. Results from the current study support this rationale, and our study reinforces earlier findings that HTC is as an important component of HIV reduction efforts. Most importantly, providing HIV testing and counseling remains an important element of HIV prevention efforts because it provides individuals the opportunity to gain knowledge and control of their own health, including through mechanisms such as changing sexual behaviors and/or condom use.

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Table 1 Descriptive statistics

	HIV-negative women		HIV-negative men		HIV-positive	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Panel A: outcome variables						
Divorced between 2004 and follow-up	0.05	0.21	0.04	0.20	0.19	0.39
Observations	*1,008*		*775*		*106*	
Number of Sexual Partners in follow-up year						
Zero	0.050		0.013		0.159	
One	0.940		0.769		0.788	
Two	0.001		0.176		0.018	
Three	0.000		0.031		0.027	
Four or More	0.002		0.011		0.009	
Observations	*1,004*		*750*		*113*	
Increase in Condom Use with Spouse	0.15	0.36	0.16	0.37	0.28	0.45
Observations	*758*		*595*		*74*	
Panel B: other variables						
Returned for HIV Results/Know HIV Status	0.72	0.45	0.71	0.45	0.64	0.48
Incentive Amount Offered						
None	0.24	0.43	0.24	0.43	0.28	0.45
10-50 Kwacha	0.20	0.40	0.19	0.39	0.18	0.39
60-100 Kwacha	0.20	0.40	0.19	0.40	0.20	0.40
110-200 Kwacha	0.22	0.42	0.25	0.43	0.19	0.39
210-300 Kwacha	0.14	0.35	0.13	0.34	0.15	0.36
Distance to HTC center	1.96	1.23	2.05	1.28	1.86	1.35
Age	34.10	11.27	40.49	12.66	35.72	10.38
Region						
Mchinji (center)	0.30	0.46	0.30	0.46	0.27	0.45
Balaka (south)	0.36	0.48	0.35	0.48	0.46	0.50
Rumphi (north)	0.34	0.48	0.35	0.48	0.26	0.44
Education:						
No Education	0.27	0.44	0.17	0.38	0.23	0.42
Primary	0.67	0.47	0.68	0.47	0.70	0.46
Secondary or more	0.06	0.25	0.15	0.36	0.08	0.27
Observations	*1,008*		*775*		*106*	

Table 2 First-stage estimates for identifying variables for learning HIV status

	Divorced after 2004			Number of sexual partners in year of follow-up			Increase in condom use with spouse/partner after 2004		
	(1) HIV-negative Women	(2) HIV-negative Men	(3) All HIV-positive	(4) HIV-negative Women	(5) HIV-negative Men	(6) All HIV-positive	(7) HIV-negative Women	(8) HIV-negative Men	(9) All HIV-positive
Incentive Amount (relative to no incentive)									
10-50 Kwacha	0.333**	0.391**	0.324*	0.333**	0.384**	0.267*	0.340**	0.393**	0.372*
	(0.050)	(0.055)	(0.133)	(0.048)	(0.054)	(0.114)	(0.053)	(0.063)	(0.164)
60-100 Kwacha	0.478**	0.446**	0.684**	0.480**	0.442**	0.675**	0.452**	0.477**	0.720**
	(0.041)	(0.051)	(0.118)	(0.040)	(0.052)	(0.117)	(0.043)	(0.056)	(0.126)
110-200 Kwacha	0.531**	0.528**	0.706**	0.528**	0.531**	0.707**	0.510**	0.537**	0.617**
	(0.038)	(0.044)	(0.118)	(0.039)	(0.047)	(0.101)	(0.041)	(0.050)	(0.141)
210-300 Kwacha	0.576**	0.587**	0.591**	0.583**	0.580**	0.574**	0.518**	0.584**	0.645**
	(0.043)	(0.044)	(0.125)	(0.044)	(0.044)	(0.121)	(0.049)	(0.049)	(0.160)
Distance to HTC (km)	-0.073	-0.004	-0.164	-0.076*	-0.016	-0.251†	-0.070	0.024	-0.177
	(0.044)	(0.044)	(0.158)	(0.045)	(0.043)	(0.140)	(0.051)	(0.051)	(0.227)
Distance to HTC squared	0.010	-0.000	0.040	0.010	0.002	0.052*	0.008	-0.004	0.040
	(0.008)	(0.008)	(0.028)	(0.009)	(0.008)	(0.026)	(0.010)	(0.009)	(0.045)
Observations	1,008	775	106	1,004	750	113	758	595	74
R-squared	0.256	0.255	0.419	0.257	0.255	0.380	0.235	0.267	0.405
F-statistic	35.57	22.49	8.86	37.06	21.90	8.46	25.59	21.56	5.69

Notes: Robust standard errors clustered by village are in parentheses. OLS coefficients reported for outcomes. Covariates for all models include age, region of residence, education and a dummy for gender in the HIV-positive models.
** p < .01; * p < .05; † p< .10

Table 3 Second-stage estimates for HIV-negative women

Outcomes:	Divorced after 2004		Number of sexual partners in year of follow-up (continuous)		Increase in condom use with spouse/partner after 2004	
	(1)	(2)	(3)	(4)	(5)	(6)
	Probit	IV Probit	OLS	IV Reg	Probit	IV Probit
Learned HIV Status	-0.028†	-0.019	0.055*	0.085*	0.033	0.155*
	(0.015)	(0.031)	(0.023)	(0.039)	(0.028)	(0.072)
Age in 2004	-0.001	-0.001	-0.002†	-0.002†	-0.006**	-0.006**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Region (relative to Mchinji - central)						
Rumphi - north	-0.020	-0.023	-0.004	-0.001	0.132**	0.128**
	(0.014)	(0.018)	(0.020)	(0.019)	(0.042)	(0.038)
Balaka - south	0.040**	0.039**	0.004	0.005	0.055	0.049
	(0.014)	(0.013)	(0.024)	(0.023)	(0.034)	(0.033)
Education (relative to no education)						
Primary	0.011	0.013	0.026	0.029	0.041	0.053
	(0.013)	(0.015)	(0.024)	(0.024)	(0.030)	(0.032)
Secondary	0.047	0.040	0.046	0.053	0.112	0.123*
	(0.046)	(0.029)	(0.036)	(0.035)	(0.078)	(0.062)
Observations	1,008	1,008	1,004	1,004	758	758

Notes: Robust standard errors clustered by village are in parentheses. Marginal effects are reported for all binary outcomes, OLS coefficients reported for the continuous outcome (model 4, number of sexual partners in year of follow-up). For model 4 the value of Sargan's Chi-squared overidentification test is 8.28 with a p-value of 0.141 and the value of Wooldridge's robust score test of endogeneity is 0.96 with a p-value of 0.328.

** p < .01; * p < .05; † p< .10

Table 4 Second-stage estimates for HIV-negative men

Outcomes:	Divorced after 2004		Number of sexual partners in year of follow-up (continuous)		Increase in condom use with spouse/partner after 2004	
	(1)	(2)	(3)	(4)	(5)	(6)
	Probit	IV Probit	OLS	IV Reg	Probit	IV Probit
Learned HIV Status	0.015	0.031	-0.063	-0.040	0.066*	0.028
	(0.013)	(0.037)	(0.052)	(0.103)	(0.033)	(0.078)
Age in 2004	-0.001*	-0.002*	-0.002	-0.002	-0.006**	-0.006**
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
Region (relative to Mchinji - central)						
Rumphi - north	-0.003	-0.002	0.188*	0.191**	0.103*	0.093*
	(0.018)	(0.020)	(0.072)	(0.072)	(0.044)	(0.039)
Balaka - south	0.004	0.005	0.130†	0.130†	0.038	0.037
	(0.016)	(0.018)	(0.067)	(0.067)	(0.036)	(0.034)
Education (relative to no education)						
Primary	0.021	0.026	-0.059	-0.058	-0.051	-0.052
	(0.016)	(0.021)	(0.092)	(0.091)	(0.050)	(0.046)
Secondary	0.020	0.022	-0.116	-0.112	-0.027	-0.037
	(0.029)	(0.024)	(0.124)	(0.125)	(0.062)	(0.070)
Observations	775	775	750	750	595	595

Notes: Robust standard errors clustered by village are in parentheses. Marginal effects are reported for all binary outcomes, OLS coefficients reported for the continuous outcome (model 4, number of sexual partners in year of follow-up). For model 4 the value of Sargan's Chi-squared overidentification test is 9.78 with a p-value of 0.771 and the value of Wooldridge's robust score test of endogeneity is 0.08 with a p-value of 0.771.

** p < .01; * p < .05; † p< .10

Table 5 Second-stage estimates for HIV-positive for men and women combined

Outcome:	Divorced after 2004		Number of sexual partners in year of follow-up (continuous)		Increase in condom use with spouse/partner after 2004	
	(1) Probit	(2) IV Probit	(3) OLS	(4) IV Reg	(5) Probit	(6) IV Probit
Learned HIV Status	-0.136†	-0.065	-0.084	-0.350†	0.151	0.388†
	(0.074)	(0.157)	(0.124)	(0.181)	(0.098)	(0.222)
Male	-0.078	-0.078	0.513**	0.529**	0.286*	0.247**
	(0.073)	(0.079)	(0.156)	(0.147)	(0.128)	(0.090)
Age in 2004	-0.004	-0.005	-0.009†	-0.008	-0.012†	-0.011*
	(0.004)	(0.005)	(0.005)	(0.005)	(0.007)	(0.005)
Region (relative to Mchinji - central)						
Rumphi - north	-0.002	0.014	0.276*	0.205	0.213	0.246*
	(0.115)	(0.127)	(0.133)	(0.141)	(0.145)	(0.101)
Balaka - south	0.002	0.004	0.215†	0.203	-0.097	-0.061
	(0.077)	(0.079)	(0.124)	(0.131)	(0.128)	(0.100)
Education (relative to no education)						
Primary	0.024	0.030	0.000	-0.010	-0.124	-0.114
	(0.092)	(0.092)	(0.091)	(0.088)	(0.208)	(0.176)
Secondary	0.048	0.057	-0.083	-0.117	-0.251*	-0.343
	(0.204)	(0.186)	(0.298)	(0.296)	(0.100)	(0.242)
Observations	106	106	113	113	74	74

Notes: Robust standard errors clustered by village are in parentheses. Marginal effects reported for all binary outcomes, OLS coefficients reported for continuous outcome (model 4, number of sexual partners in year of follow-up). For model 4 the value of Sargan's Chi-squared overidentification test is 4.15 with a p-value of 0.528 and the value of Wooldridge's robust score of endogeneity is 3.27 with a p-value of 0.076.
** p < .01; * p < .05; † p< .10

Supplemental Material

Analysis of Attrition

Any longitudinal data suffers some sample attrition, which can bias analyses if those who exit the sample are systematically different from those who do not, based on either observed or unobserved characteristics (Alderman et al. 2001; Anglewicz et al. 2009; Becketti et al. 1988; Fitzgerald et al. 1998). Migration attrition after the 2004 wave of data was a particular concern for this study because individuals who choose to migrate are more likely to be HIV-positive and are also more likely to move because of divorce (Anglewicz 2012). Two things were done to alleviate concerns that results were biased due to uneven attrition on these observed characteristics or on other unobserved characteristics. First, data from a 2007 follow up survey, which specifically attempted to find respondents who were not interviewed in 2006 due to migration, was added to the follow-up sample. Among the 2004 respondents used in this study, approximately 18% were not found for re-interview in 2006 because of migration, comprising the majority of the approximately 30% attrition between 2004 and 2006 (Anglewicz 2012). The 2007 migration study sought to interview respondents who migrated internally within Malawi[13] and successfully interviewed 56% of these respondents (Anglewicz 2012). By including the respondents found in the 2007 migration data, we reduce the potential bias due to migrants differing from non-migrants post 2004, as well as underrepresentation of divorced individuals and HIV-positive individuals.[14]

The second means of alleviating concerns of attrition bias in our results is by performing additional analyses of attrition. These analyses are based on similar work from several previous

[13] Of the migrants not found in 2006, only 11% moved outside of Malawi.

[14] The distribution of individuals by follow-up survey supports this point. It is shown in crosstabs with marital status and HIV status in appendix table A. Appendix table A shows that individuals found in the 2007 survey, as well as the 2008 survey, are more likely to have divorced after 2004 and are more likely to be HIV-positive.

studies (Alderman et al. 2001; Anglewicz et al. 2009; Becketti et al. 1988; Fitzgerald et al. 1998). First, we make descriptive comparisons between those who were re-interviewed and those who were not re-interviewed. Second, we perform a series of logistic regressions predicting attrition based on 2004 characteristics. Last, we perform a series of OLS and logistic regressions predicting several outcomes of interest from the 2004 data, which are chosen based on their ability to reflect outcomes of interest in this study (sometimes referred to as a BGLW test). This last series of regressions includes a global interaction of attrition and shows the degree to which attrition biases coefficients in regressions measuring similar outcomes to the outcomes of interest used in the current study.

The descriptive comparisons made between those who leave the sample and those who were found for re-interview in 2006, 2007 or 2008 are presented in table S1. These descriptive comparisons are based on observed characteristics from the 2004 wave of data and, as expected, indicate some differences between those who left the sample and those who were found during one of the follow-up waves. Those who leave the sample are slightly older, more likely to be from Balaka and are more likely to have either no education or more education than average. A series of outcome proxies from the 2004 data are also reported in table S1. These proxies are conceptually similar to the outcomes from follow-up waves used in the analysis presented in the two-stage least squares estimation in the main body of the text. Those who are not re-interviewed after 2004 are less likely to have agreed to an HIV test and had fewer sexual partners in the last 12 months compared to those who were re-interviewed after 2004. Furthermore, among those who agreed to an HIV test, those who were not found for re-interview were far more likely to be HIV-positive, to have not returned for their HIV test results, to have been offered a lower incentive amount for returning and lived further from the HTC pick-up site.

Table S2 presents the results of several logistic regression models predicting attrition after 2004. The first four models represent the bivariate relationships between attrition and variables from the 2004 data that are conceptually similar to the outcome variables in the later analysis: ever divorced, number of sexual partners in the last 12 months and ever used a condom with spouse. Additionally, the degree to which agreeing to an HIV test predicts attrition is also assessed. Those who have ever been divorced, those who refused an HIV test and those who are HIV-positive are more likely to have left the sample. Again, this is as expected based on previous research (Anglewicz 2012). Furthermore, those who have had more sexual partners in the last 12 months are slightly less likely to have left the sample, although these results are only marginally significant, at a p-value just under the 0.10 level. In the fifth model, all four of the previous variables are included simultaneously. Attrition is associated with refusing an HIV test, the number of sexual partners in the last 12 months loses all statistical significance, ever having divorced retains only marginal significance and ever having used a condom with one's spouse gains marginal significance. A similar pattern is observed in the eighth model, which includes all four variables discussed above as well as control variables for sex, age, region and education. The only difference in the eighth model is that the variable for ever used a condom with a spouse gains more statistical significance, indicating that those who have used a condom with their spouse are more highly associated with attrition. Models six and seven comprise a sample of only those who have agreed to an HIV test and assess how the results of the HIV test are associated with attrition. Both with and without control variables, being HIV-positive is strongly associated with attrition. Those who attrite were also offered a slightly lower incentive amount, lived further from the HTC pick up site and were less likely to return to the HTC pick up site to learn their HIV status.

Up to this point, the attrition analysis seems to indicate that the most important variables in the current analysis are associated with attrition, including ever having been divorced, agreeing to an HIV test, HIV status itself and whether respondents returned to learn their HIV test results. However, as recommended by previous studies such as Becketti et al. (1988) or Alderman et al. (2001), a third set of attrition tests, sometimes referred to as BGLW tests after the initials of the four authors of the paper originating this concept (Becketti et al. 1988), can still help to determine whether these real differences in observable characteristics between those who attrite and those who do not, are large enough to actually bias the coefficients in regression outcomes. In table S3 we present a series of logistic and OLS regressions using the outcomes of particular interest noted above. Included in these models are interactions between attrition and every variable in the model so that the significance of the difference between the outcomes by attrition status can be thoroughly analyzed. The interactions between each of the predictor variables and attrition are at the top of the table, followed by the coefficient for attrition (the first-order effects of the predictor variables are not shown). The interaction coefficients indicate which variables differentially affect the outcomes by attrition status. Very few of these coefficients are statistically significant. Attrition in the southern region of Balaka is associated with significantly lower chances of agreeing to an HIV test and ever having been divorced, while attrition in Balaka is associated with a marginally significant increase in the association with having ever used a condom with one's spouse. Those who have more education also exhibit a marginally significant association between attrition and having ever been divorced.

The Chi-squared tests for the joint effect of attrition on the constant and the coefficients, listed at the bottom of table S3, indicate whether or not each model, overall, is biased by attrition. This is the most direct measure of how much attrition may be biasing results. The most useful of these joint tests is the test for the effect of attrition on the coefficients only, which indicates whether there are significant differences in the outcomes between those who exit the sample and those who do not, as evidenced by the slope of the coefficients. If these tests fail to indicate a significant difference between the attrition group and the non-attrition group, then we have evidence that the estimated coefficients of the outcome variables are not significantly biased by attrition. For all four outcomes tested (agreement to an HIV test, ever having used a condom with one's spouse, ever having divorced and the number of sexual partners in the last 12 months) the chi-squared and F tests for the joint effect of attrition on the coefficients fail to predict a statistically significant difference in estimates between the attrition group and the non-attrition group. Therefore, while those who were lost to follow-up differ along several observable characteristics from those who were found for re-interview, the resulting bias is too small to significantly affect the parameter estimates for the four outcomes. This indirect assessment of the effect of attrition bias makes us more confident that attrition after 2004 does not significantly affect the substantive conclusions found in the main body of the paper.

HIV Incidence after Learning HIV Status

The self-reported reductions in risky sexual behavior found in the main body of the paper are encouraging, but decreases in HIV incidence would be the best measure of the effectiveness of HTC and of learning HIV status. Since HIV incidence rates are usually between one and three percent, new HIV infections are difficult to explore due to the need for very large sample sizes in order to produce accurate estimates of incidence. We still attempted to estimate the effect of learning HIV status in 2004 on reducing the chances of becoming HIV-positive in the years to follow, even though results are underpowered. Respondents were included in this sample if (1) they were married in 2004, (2) they were tested for HIV in 2004 and were not HIV-positive at that time and (3) were found for re-interview in one of the follow-up waves of the survey. Among these married respondents, only 23 women and 10 men became HIV-positive by 2008. Including all tested respondents, whether married or not, yields 30 new incidences of HIV among women and 15 new cases among men.

Table S4 reports the first-stage estimates for learning HIV status. Table S5 reports the probit models and second-stage IV probit models for the effect of knowing HIV status on new HIV incidence after 2004. In addition to focusing on only the married population, we estimated HIV incidence among all respondents who tested negative in 2004 and who were tested again in either 2006, 2007 or 2008. Results for the married sample and the whole sample are very similar. The results of IV probit models are not statistically significant, but the coefficients are in the right direction for women, suggesting that there could very well be reductions in HIV incidence among women even though the effects are underpowered in this analysis. This is consistent with the trend in self reported reductions in risky sexual behavior. Furthermore, probit models for both men and women indicate that among those who self select into HIV testing, women are

2.1% and men are 1.8% less likely to become HIV-positive after learning their HIV status. Given that the estimated overall incidence rate of HIV in Malawi in 2004 was approximately 1.4% (UNAIDS 2008; UNAIDS 2010b), these results are very encouraging in that they suggest a very large reduction in HIV incidence among the select population who are actively seeking knowledge of HIV status and taking preventative measures.

Table S1 2004 Descriptive Statistics among those Married in 2004 by Attrition Status at Follow-up

	Re-Interviewed		Not Reinterviewed		Difference		
	Mean	Std Dev	Mean	Std Dev	Means	t-test	p-value
Male	0.42	0.49	0.39	0.48	0.03	1.49	0.14
Age	36.37	12.37	38.98	15.60	-2.61	-4.59	0.00
Region:							
Mchinji (center)	0.35	0.48	0.32	0.47	0.02	1.19	0.24
Balaka (south)	0.33	0.47	0.37	0.48	-0.04	-1.99	0.05
Rumphi (north)	0.32	0.47	0.31	0.46	0.02	0.81	0.42
Education:							
No Education	0.23	0.42	0.25	0.43	-0.02	-1.29	0.20
Primary	0.67	0.47	0.62	0.48	0.05	2.21	0.03
Secondary or more	0.10	0.30	0.13	0.33	-0.02	-1.62	0.11
Observations	2,637	79.93%	662	20.07%			
Outcomes of Interest:							
Agreed to HIV test	0.91	0.29	0.86	0.35	0.05	2.61	0.01
Ever used a condom with spouse	0.19	0.39	0.22	0.41	-0.03	-0.93	0.35
Ever divorced, as of 2004	0.40	0.49	0.45	0.50	-0.06	-1.64	0.10
Number of sexual partners in last 12 months	1.12	0.47	1.05	0.51	0.08	2.32	0.02
Among those who agreed to VCT:							
Distance to VCT center	1.99	1.26	2.32	1.41	-0.33	-3.52	0.00
Incentive Amount Offered	103.02	95.04	88.28	90.57	14.75	2.14	0.03
Returned for HIV Results/Know HIV Status	0.71	0.45	0.55	0.50	0.16	4.85	0.00
HIV Positive	0.06	0.23	0.22	0.42	-0.16	-8.83	0.00
Observations	1,889	90.04%	209	9.96%			

Table S2 Odds Ratios Predicting Attrition between 2004 and Follow-up among those married in 2004

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ever divorced	1.354*				1.401†	1.221	1.162	1.400†
	(0.181)				(0.244)	(0.235)	(0.242)	(0.264)
Ever used a condom with spouse		1.126			1.405†	1.197	1.285	1.530*
		(0.203)			(0.277)	(0.262)	(0.293)	(0.315)
Number of sexual partners in the last 12 months			0.785†		0.864	0.839	0.772	0.716
			(0.114)		(0.193)	(0.207)	(0.199)	(0.166)
Agreed to HIV test				0.606*	0.577*			0.584*
				(0.118)	(0.142)			(0.145)
HIV positive						4.679**	4.430**	
						(1.137)	(1.095)	
Incentive amount offered						0.998†	0.998*	
						(0.001)	(0.001)	
Distance to VCT center						1.267**	1.198**	
						(0.083)	(0.082)	
Returned for HIV test results						0.577**	0.565**	
						(0.116)	(0.117)	
Male							0.945	1.068
							(0.196)	(0.201)
Age							1.004	0.996
							(0.009)	(0.008)
Region (Mchinji)								
Balaka							1.514†	2.219**
							(0.369)	(0.471)
Rumphi							0.646	0.739
							(0.181)	(0.197)
Education (no education)								
Primary							1.228	1.338
							(0.296)	(0.289)
Secondary or more							0.828	1.085
							(0.398)	(0.463)
Observations	2,450	2,193	2,410	2,356	1,957	1,759	1,756	1,954
Model Wald Chi-Squared	5.07	0.43	2.80	6.05	10.06	67.68	81.19	41.82
Model Wald p-value	0.03	0.51	0.09	0.01	0.04	0.00	0.00	0.00
Pseudo R-squared	0.00	0.00	0.00	0.00	0.01	0.07	0.09	0.04

Notes: Robust standard errors clustered by village are in parentheses. ** p < .01; * p < .05; † p< .10

Table S3 OLS and Logit Models Predicting Key Outcome Variables by Attrition between 2004 and Follow-up among those married in 2004

	(1) Logit - Agreed to HIV test	(2) Logit - Ever used condom with spouse	(3) Logit - Ever divorced as of 2004	(4) OLS - Num. of partners in last 12 months
Interactions with Attrition:				
Male*Attrition	-0.271	0.285	0.527	-0.031
	(0.454)	(0.427)	(0.335)	(0.069)
Age*Attrition	0.002	0.022	-0.010	-0.000
	(0.017)	(0.016)	(0.013)	(0.003)
Region Interactions:				
Rumphi*Attrition	-0.914	0.961	-0.470	-0.036
	(0.682)	(0.602)	(0.472)	(0.095)
Balaka*Attrition	-1.330*	0.931†	-0.758*	0.034
	(0.614)	(0.510)	(0.366)	(0.077)
Education Interactions (no educ):				
Primary*Attrition	-0.424	-0.607	-0.730†	-0.003
	(0.552)	(0.534)	(0.408)	(0.086)
Secondary*Attrition	-1.048	-0.281	-1.338†	0.224
	(0.797)	(0.827)	(0.773)	(0.145)
Attrition (effect of attrition on constant)	0.945	-0.881	1.355*	-0.098
	(0.958)	(0.854)	(0.665)	(0.139)
Observations	2,347	2,168	2,424	2,384
Model Wald Chi-Squared	15.92	116.7	343.2	.
Model Wald p-value	0.24	0.00	0.00	.
Pseudo R-squared	0.01	0.06	0.11	.
Adjusted R-squared	.	.	.	0.07
Chi2 test for joint effects of attrition on (F test for OLS regression in model 4):				
Constant only	0.97	1.06	4.15*	0.5
	[0.324]	[0.302]	[0.042]	[0.478]
Coefficients only, not constant	7.33	10.39	8.14	0.63
	[0.291]	[0.109]	[0.228]	[0.709]
Constant and coefficients	15.26*	13.69†	9.29	1.67
	[0.033]	[0.057]	[0.233]	[0.111]

Notes : Standard errors in parentheses; numbers in brackets [ ] represent probability > chi2; first order effects not shown; ** p < .01; * p < .05; † p< .10

Table S4 First Stage (learning HIV status) for HIV Incidence after 2004

	Women	Men	Combined
Incentive Amount (relative to no incen			
10-50 Kwacha	0.330**	0.389**	0.353**
	(0.048)	(0.052)	(0.035)
60-100 Kwacha	0.469**	0.411**	0.444**
	(0.041)	(0.053)	(0.033)
110-200 Kwacha	0.528**	0.515**	0.521**
	(0.039)	(0.045)	(0.030)
210-300 Kwacha	0.566**	0.572**	0.567**
	(0.045)	(0.047)	(0.033)
Distance to VCT (km)	-0.078†	-0.036	-0.061†
	(0.045)	(0.044)	(0.037)
Distance to VCT squared	0.011	0.006	0.009
	(0.008)	(0.008)	(0.007)
Age	0.000	0.003*	0.002†
	(0.001)	(0.001)	(0.001)
Region (relative to Mchinji - central)			
Rumphi - north	-0.064†	-0.085†	-0.074*
	(0.037)	(0.046)	(0.034)
Balaka - south	-0.041	-0.044	-0.043
	(0.039)	(0.040)	(0.032)
Male			0.005
			(0.018)
Constant	0.491**	0.355**	0.433**
	(0.071)	(0.074)	(0.056)
Observations	987	698	1,685
R-squared	0.241	0.248	0.240
F-statistic	33.93	23.03	38.74

Notes : Robust standard errors clustered by village are in parentheses. ** p < .01; * p < .05; † p< .10

Table S5 Second Stage - HIV Incidence after 2004

	Probit - Women	IV - Women	Probit - Men	IV - Men	Probit - combined	IV - combined
Learned HIV Status	-0.021**	-0.013	-0.018†	0.032	-0.020**	0.004
	(0.009)	(0.024)	(0.009)	(0.054)	(0.006)	(0.019)
Age in 2004	-0.000	-0.000	-0.000	-0.001	-0.000†	-0.001†
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Region (relative to Mchinji - central)						
Rumphi - north	-0.014	-0.012	0.006	0.018	-0.005	-0.002
	(0.016)	(0.015)	(0.014)	(0.018)	(0.010)	(0.011)
Balaka - south	0.036*	0.035**	0.028†	0.041	0.032*	0.033*
	(0.015)	(0.014)	(0.014)	(0.026)	(0.011)	(0.012)
Male					-0.005	-0.006
					(0.006)	(0.006)
Observations	987	987	698	698	1,685	1,685

Notes : Robust standard errors clustered by village are in parentheses. ** p < .01; * p < .05; † p< .10