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

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Abstract

This article assesses how married individuals' knowledge of HIV status gained through HIV testing and counseling (HTC) affects divorce, the number of sexual partners, and the use of condoms within marriage. This study improves upon previous studies on this topic because the randomized incentives affecting the propensity to be tested for HIV permit control for selective testing. Instrumental variable probit and linear models are estimated, using a randomized experiment administered as part of the Malawi Longitudinal Study of Families and Health (MLSFH). 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 reported sexual partners among HIV-positive respondents; and (3) increases reported condom use with spouses for both HIV-negative and HIV-positive respondents. These results imply that individuals actively respond to information about their HIV status that they learn during HTC, invoking protective behavior against future risk of HIV/AIDS for them-selves and their actual and potential sexual partners. Some limitations of this study are a small sample size for those who are HIV-positive and dependence on self-reported sexual behaviors.

Keywords

Divorce; Sexual behavior; Malawi; HIV testing and counseling (HTC); Information and health

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Introduction

In most of sub-Saharan Africa (SSA), HIV testing and counseling (HTC) is promoted and implemented as an important part of HIV prevention (World Health Organization 2010). Reduced risky sexual behavior after learning HIV status is presumed, but observed results are mixed, leading to doubt about the efficacy of HTC in the prevention of HIV. Study findings vary dramatically, with some finding more condom use or fewer sexual partners after HTC and others finding no behavioral change or even increased risky behaviors (Denison et al. 2008; De Paula et al. 2014; Fonner et al. 2012; Matovu et al. 2005; Sherr et al. 2007; Stoneburner and Low-Beer 2004; Thornton 2008). Relationship-based prevention tactics, such as divorce or selecting partners based on HIV status (serosorting), is also an area of exploration in conjunction with learning HIV status through HTC (Gregson et al. 1998; Grinstead et al. 2001; Porter et al. 2004; Reniers and Helleringer 2011). Several studies find that both women and men are more likely to divorce a spouse who is known or suspected to be HIV-positive (Gregson et al. 1998; Grinstead et al. 2001; Porter et al. 2004; Reniers 2008; Smith and Watkins 2005).

A prominent difficulty in assessing the validity of these outcomes is that those seeking to know their HIV status through HTC are self-selected (Conroy 2014; Kranzer et al. 2008; Matovu et al. 2005). That is, those tested represent a select portion of the population who may be driven both to seek testing and to take preventative measures. For this reason, assuming that testing *causes* preventative behavior from associations between testing and behavioral change is problematic. The aim of the current study is to clarify the causal effect of HTC on post-testing behaviors by using an experimental design that randomizes additional motivation to learn HIV test results. We examine how knowledge of HIV status is used by married individuals in ways that may protect against HIV/AIDS risk through (1) divorce, (2) reducing the number of sexual partners, and (3) condom use with spouses. We are able to measure the causal impact of HTC on subsequent behavior, not just associations, by using data from the Malawi Longitudinal Study of Families and Health (MLSFH) (Kohler et al. 2014), exploiting in particular the random variation in HTC uptake in the 2004 MLSFH that resulted from an experimental design. Specifically, the HIV testing technology at the time of the 2004 MLSFH required lab analyses of the saliva specimen obtained from respondents. A four- to six-week delay occurred between the collection of the saliva specimen and the availability of the HIV test results. To create exogenous variation in the rate at which MLSFH respondents learned their HIV status, respondents during the 2004 MLSFH round were randomly incentivized through both varying monetary rewards and travel distances for visiting a MLSFH HTC site where they could obtain their HIV test results. Because of their randomization during the 2004 MLSFH HTC, the randomized incentives and distances to HTC sites provide plausible instruments to control for selection in two-stage estimates of effects of learning HIV status on later behaviors. By exploring the extent to which selection bias into testing plays a role in later behaviors, we hope to shed light on some of the contradictory findings from previous studies and to alleviate concerns for the potential unintended negative consequences of HTC.

Literature Review

In high-HIV-prevalence SSA, the risk of HIV transmission from one's spouse is substantial. Several studies have indicated that a moderate to large proportion of new heterosexually acquired HIV infections occur within marriage or stable relationships (Bellan et al. 2013; Chemaitelly et al. 2014; Dunkle et al. 2008; Matovu 2010; UNAIDS 2010), and serodiscordant couples are more common than seroconcordant HIV-positive couples (De Walque 2007). Serodiscordant couples can arise from extramarital partnerships but also from premarital partners and prior marriages. Individuals who are divorced are more likely to be HIV-positive than individuals who are currently married or never married (Boileau et al. 2009; Kohler et al. 2014; Macro International, Inc. 2008; Tenkorang 2014). In rural Malawi, the majority of individuals are either currently married or have already been married at least once by their mid-20s, though there is also substantial "marital churning," or divorce and remarriage (National Statistical Office and ICF Macro 2011b), highlighting the importance of divorce as a source of HIV risk in this context.

The positive association between divorce and HIV-positive status is attributed to several causes. Divorce could cause increased HIV risk because divorcees are more likely to have a higher number of sexual partners throughout life, leading to higher chances of becoming HIV-positive. However, it is also possible that engaging in risky behavior, such as cheating on a spouse, leads to higher chances of both divorce and becoming HIV-positive. Another possibility is that knowledge of HIV status, in and of itself, is the impetus for divorce (Gregson et al. 1998; Grinstead et al. 2001; Porter et al. 2004; Tenkorang 2014). Many individuals recognize their spouse as a significant potential source of HIV risk, and divorcing a spouse who is known or suspected of being HIV-positive or of having extramarital partners is another prevention tactic individuals use (Gregson et al. 1998; Reniers 2008; Schatz 2005; Smith and Watkins 2005; Tenkorang 2014). Specifically in Malawi, both men and women increasingly use divorce as a risk-reduction strategy when they believe their marriage puts them at high risk of HIV infection (Reniers et al. 2009). Those who learn they are HIV-negative and know or suspect their partner of being or becoming HIV-positive may prefer getting divorced in order to protect themselves from HIV risk. Those who learn they are HIV-negative may also see this as an increase in their "value" within marriage and may use this as an impetus to change partners. However, this explanation may oversimplify interpersonal relationship dynamics. We believe that previous findings suggesting that HIV testing may lead to divorce are most probably biased by the fact that those who choose to divorce may also choose to be tested, possibly for other reasons, such as marital discord or a cheating spouse. In order to accurately assess the direct impact of HIV testing on divorce decisions, one must account for the effect of selection into testing. Although we cannot measure couple-level dynamics explicitly in this study, the ability to assess which choices are made while adjusting for selection into the sample of those seeking testing is an important contribution to the literature. After accounting for selection, we believe married individuals will be more likely to seek preventative tactics other than divorce within their existing relationships. If we find no effect of learning HIV status on divorce for HIV-positive individuals, this provides evidence that testing may not cause divorce but, rather, that choosing to be tested is related to divorce.

Once HIV status is learned, other viable prevention tactics are, of course, available to individuals or couples who choose to stay together. For example, HIV-positive individuals are found to decrease their reported number of sexual partners and increase condom use after learning HIV status (Anglewicz and Clark 2013; Arthur et al. 2007; Cremin et al. 2010; De Paula et al. 2014; Fonner et al. 2012; Sherr et al. 2007; Thornton 2008). The motivation for such behavioral change is assumed to be an altruistic desire to protect sexual partners from HIV risk or an avoidance of social sanctions (De Paula et al. 2014; Thornton 2008). HIV-negative individuals also increase condom use and engage in other preventative behaviors to protect against their spouses' suspected HIV status, known HIV status, or suspected behavior (Arthur et al. 2007; Denison et al. 2008; Mola et al. 2006; Stoneburner and Low-Beer 2004). This is particularly true for HIV-negative women (Cremin et al. 2010; Gregson et al. 1998; Kabiru et al. 2010) or for an HIV-negative test result that is surprising (Gong 2014). Individuals in Malawi are overly pessimistic, assuming HIV-positive status prior to testing (Delavande and Kohler 2012) and often finding HIV-negative test results to be surprising (Gong 2014). Increased protective behavior after learning HIV-negative status may be very likely in this context as individuals seek to protect a newly learned and possibly unexpected HIV-negative status.

Importantly, several studies find no association of HTC with risky behavior or even increased risky behavior as an unintended consequence of HTC, especially among HIV-negative individuals (De Paula et al. 2014; Fonner et al. 2012; Kabiru et al. 2010; Matambo et al. 2006; Matovu et al. 2005; Sherr et al. 2007). For example, studies have found higher numbers of sexual partners among married HIV-negative men (De Paula et al. 2014), increases in unprotected sex and risky partners among all females who are tested, and increases in the number of concurrent partners among all males who are tested (Kabiru et al. 2010). Although these studies have found evidence of increased risky sexual behavior or unintended negative consequences of HTC, other studies have found that this may be because those seeking HIV testing may have alternative motives for testing (Kranzer et al. 2008; Matovu et al. 2005), such as checking HIV status prior to becoming pregnant or prior to switching partners. This makes accounting for selection into HIV testing important when assessing post-testing behavior. In the current study, if we find no effect of HTC on behavior or find reduced risky sexual behavior after accounting for selection into testing, we will have compelling evidence that HIV testing does not inadvertently *cause* increased risky behavior among those found to be HIV-negative but, rather, that the choice to be tested creates bias in results in studies that do not control for the testing choice.

Many of these contradictory results of the effect of HTC on divorce, condom use, and sexual partnership are from studies limited to selective samples from clinic-based HTC sites, comprising only individuals who seek out HIV testing (Arthur et al. 2007; Cremin et al. 2010; Grinstead et al. 2001; Matovu et al. 2005; Mola et al. 2006). A few studies were able to utilize samples that are part of a randomized survey (De Paula et al. 2014; Matovu et al. 2005; Thornton 2008). However, even in randomized samples, those who chose to accept HIV testing are still probably selective and 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 within a randomized sample in order to improve the measurement of behavioral change after HTC (De Paula et al. 2014; Thornton 2008, 2012), as we do here.

In this article, we examine how learning HIV status affects divorce and other HIV prevention behaviors among married individuals in models that control for selection bias into choosing to be tested for HIV/AIDS. In general, we expect to find that HTC does not *cause* certain unintended negative consequences, such as divorce or increased risky behavior; rather, other factors associated with choosing to be tested are more likely sources of the unintended negative consequences of HTC found in some previous studies. For both HIV-negative and HIV-positive individuals, we expect to find no effect of learning HIV status on divorce. Furthermore, we expect that both HIV-positive and HIV-negative individuals will engage in more-careful sexual behaviors, such as fewer extramarital partners and more condom use, in order to protect either their partners or their newly known HIV-negative status.

Data and Methods

The MLSFH is a longitudinal study in Malawi repeated for six waves between 1998 and 2010. The study design, sample selection, survey content, follow-up rates and attrition are described in the *MLSFH Cohort Profile* (Kohler et al. 2014). 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 owing to migration.[1] However, in 2004, the baseline characteristics of respondents were still comparable to other surveys conducted in Malawi (Anglewicz et al. 2009). We use a subsample of data from the 2004, 2006, and 2008 waves, 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. In this study, the 2006, 2007, and 2008 waves of data are combined to form follow-up data and assess behavioral changes after learning HIV status in 2004. The majority of respondents in the follow-up sample are taken from the 2006 wave (91 % of the final sample). However, if respondents were not found for reinterview in 2006, the 2007 data are used when possible. If the respondents were not reinterviewed in 2006 or 2007, the 2008 data are used when possible. We use only data from the first MLSFH survey in which respondents participated after 2004, whether from 2006, 2007, or 2008. Because respondents were retested for HIV in 2006, 2007, and 2008, using only the first survey after 2004 ensures that respondents did not receive additional information from the MLSFH about their HIV status after 2004.[2] Furthermore, when comparing changes in outcomes for divorce, we count only divorces recorded between 2004 and 2006, ensuring the same exposure period for reaction to HIV test results for all respondents.[3] The final sample is restricted to individuals in 2004 who

[1]Seventy-four percent of attrition between 1998 and 2001 was attributable to migration (total attrition from 1998–2001 was 23 %). Similarly, 54 % of the attrition between 2001 and 2004 was attributable to migration (total attrition from 2001 to 2004 was 26 %).
[2]Eighty-five percent of respondents were not tested from an outside source between 2004 and follow-up, but results are robust to the exclusion of individuals who did report an additional test after 2004.
[3]Seven divorces were dropped because they occurred after 2006.

were married, agreed to the 2004 MLSFH-provided HIV test,[4] provided basic demographic data, and were reinterviewed in 2006, 2007, or 2008.

In 2004, rapid testing was not available in the study area. 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 3 U.S. dollars in 2004). The location for pick-up of HIV test results was also randomly assigned in each community, resulting in random distances from respondents' homes. Respondents' average distance from home to HTC pick-up location is 2 km (standard deviation = 1.26); the maximum distance is 5.2 km; and over 90 % of respondents live less than 4 km away. The distribution of monetary incentives is nonnormal, with discontinuities near 0 and incentives clustered around 50, 100, 200, 250, and 300 kwacha. For this reason, incentives are categorized as no incentive, 10–50 kwacha, 60–100 kwacha, 110–200 kwacha, or 200–300 kwacha, although results are robust to using incentives in continuous and other forms. To adjust for the possibility of nonlinearity in the relationship between learning HIV status and distance to HTC, we add a term for squared distance to HTC site to the models.

Marital status change from 2004 to follow-up, identified using marital history information, is categorized as still married or divorced. "Married" is defined as individuals who were either married or living with a partner, which is a definition that is both culturally relevant and accurate in terms of the behavior we wish to measure.[5] Marital status changes to widowhood are excluded from the analysis. Widowhood constitutes less than 1 % of the sample but approximately 21 % of marital dissolutions during 2004–2006. The small number of widowed respondents partially alleviates concerns of unequal attrition of HIV-positive respondents resulting from death; however, the proportion of widowhood among those who experienced marital dissolution after 2004 is fairly large and is acknowledged as a limitation. "Divorced" category includes individuals who divorced between 2004 and 2006, regardless of whether they remarried before follow-up.[6] 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. Results are robust to other specifications of this variable, including various dichotomous variables representing differences between meaningful cut points in the number of partners. Condom use with husbands, wives, and live-in partners in 2004, 2006, and 2007 is used to determine change in condom-use patterns after learning HIV status. Questions about condom use within marriage were not asked in 2008. Response categories move loosely

[4]Less 10 respondents with indeterminate results.

[5]The marriage question is as follows: "I am interested both in marriages that involved your family and ankhoswe, and marriages where you and your husband just started living together without involving ankhoswe." Therefore, the sample of "married" individuals includes both married and cohabiting couples.

[6]Of those who divorced after 2004, approximately 80 % remarried by follow-up.

from low to high frequency as follows: never, sometimes, almost every time, and every time. If a respondent indicates a higher frequency category in 2006/2007 than in 2004, 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.[7] Several predetermined control variables that are not affected by the 2004 test results are included to increase estimate precision: age in 2004,[8] region of residence, and level of schooling in 2004.[9]

To investigate the causal effects of learning one's HIV status in 2004 on subsequent divorce and sexual behaviors, we estimate nonlinear two-stage probit and two-stage least squares models.[10] These nonlinear two-stage models are more appropriate in analyses that include a binary endogenous variable, as is the case here with learning HIV status. The nonlinear two-stage estimators, as outlined by Wooldridge (2002:623) and Angrist and Pischke (2008:191), differ from the standard instrumental variable (IV) estimation by having an additional step prior to the first stage of the standard two-stage probit or least squares estimation.

Specifically, to account for our dichotomous endogenous variable, we follow Wooldridge (2002:623) and Angrist and Pischke (2009:191) and first estimate a probit model for knowing HIV status, $Know_i$, using exogenous or predetermined variables as predictors: monetary incentives; distance to the HTC site; distance squared; and selected individual characteristics, $\mathbf{X}_i$. This pre-first-stage probit model for our binary endogenous variable is specified as follows:

$$\begin{aligned}\Pr(Know_i{=}1){=}\Pr\big(\alpha_0 &\\ +\alpha_1 Incentive_i &\\ +\alpha_2 Dist_i+\alpha_3 Dist_i^2+\mathbf{X}_i\boldsymbol{\alpha}'_x+\varepsilon_i>0{=}\Phi\big(\alpha_0 &\\ +\alpha_1 Incentive_i &\\ +\alpha_2 Dist_i+\alpha_3 Dist_i^2+\mathbf{X}_i\boldsymbol{\alpha}'_x\big)\text{, given}\,\varepsilon_i\sim N(0,1), &\end{aligned} \tag{1}$$

where $Know_i$ is learning HIV status (binary variable for picking up HTC test results as part of the 2004 MLSFH HTC) for respondent i; $Incentive_i$ is the amount of incentive offered as part of the 2004 MLSFH HTC; $Dist_i$ is the distance to HTC site; $\mathbf{X}_i$ is a vector of exogenous or predetermined covariates (age, education level, and region); $\boldsymbol{\alpha}'_x$ is the vector of coefficients for each X covariate; $\Phi(\cdot)$ is the cumulative normal distribution function; and ε_i is the error, which is assumed normally distributed with a mean of 0 and a standard deviation of 1. Next, we calculate the predicted probabilities of learning HIV status, $Know_i$, from the probit models.

[7]The level of condom use was assessed in continuous form and several dichotomous forms: decreased condom use, increased condom use, change in condom use from "never" to "any," and have ever used a condom.
[8]Age in 2004 was estimated by the interviewer for 33 respondents in 2004 and 7 in 2008 (2.2 % of the total sample).
[9]Age and schooling level are taken from the 2004 data. Missing values in 2004 are imputed from 2006, 2007, and 2008 data (0.4 % of the age variable and 13.5 % of the schooling variable).
[10]Least squares linear regression models are estimated for the number of sexual partners in follow-up year.

Finally, we use these predicted probabilities, $\hat{P}(Know_i)$, and $\mathbf{X}_i$ as instruments in two-stage models predicting the causal effect of learning HIV status on the outcomes: (1) divorce, (2) the number of sexual partners during follow-up year, and (3) condom use within marriage.[11]

Our first-stage specification for each of these outcomes is then given by

$$Know_i=\alpha_0+\alpha_1\hat{P}(Know_i)+\mathbf{X}_i\boldsymbol{\alpha}_x^{\prime}+\nu_i, \quad (2)$$

where the predicted values obtained from the pre-first-stage Eq. (1), $\hat{P}(Know_i)$, are used to predict the probability of knowing HIV status. The second-stage probit model for the (binary) outcomes themselves is then specified as:

$$\Pr\left(Y_i=1\middle|\mathbf{X}_i, K\hat{n}ow_i\right)=\Pr\left(\beta_0+\beta_1 K\hat{n}ow_i+\mathbf{X}_i\beta_x^{\prime}+\varepsilon_i>0\right)=\Phi\left(\beta_0+\beta_1 K\hat{n}ow_i+\mathbf{X}_i\beta_x^{\prime}\right), \text{given} \varepsilon_i\sim N(0,1), \quad (3)$$

where Y_i is the final outcome variable; $K\hat{n}ow_i$ is the predicted value of learning HIV status, as estimated in the first-stage model; $\mathbf{X}_i$ is a vector of covariates; $\boldsymbol{\beta}_x^{\prime}$ is the vector of coefficients for each X covariate; and ε_i is the error, which is assumed to be normally distributed with a mean of 0 and a standard deviation of 1. The equation for the second-stage linear model for the outcome for number of sexual partners follows the same general form as the second-stage probit equation given as the preceding example.

There are several advantages to the nonlinear two-stage estimator, which enables the inclusion of the pre-first-stage probit estimation in Eq. (1). Most importantly, this has the advantage of being able to account for the binary nature of the variables at both the first and second stages of the analysis. By doing so, we generate a better approximation of $Know_i$ than a linear model would yield. Therefore, the IV estimates are more efficient and precise. Other advantages include asymptotically valid test statistics and correct standard errors in the two-stage portion of the analysis (Angrist and Pischke 2009; Wooldridge 2002).

Regular probit and linear regression models, which do not control for the endogeneity of knowing one's HIV status, are also included for comparison, with the expectation that coefficients will change in the IV probit and IV regression models owing to reductions in bias. The analysis is divided by HIV status because it is expected that the propensity to divorce, to have more sexual partners, or to 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.

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;

[11]The two stages are estimated jointly to ensure accurate standard errors using the *ivreg* and *ivprobit* commands in STATA12.

this is consistent with Demographic and Health Survey (DHS) data, in which 15 % of the population in Malawi reported ever having had an HIV test as of 2004 (National Statistical Office and ORC Macro 2005). HIV testing outside of the MLSFH was not only rare in Malawi before 2004, but the rate of increase in testing between 2004 and 2006 was also modest. Substantial gains in testing were made by 2010; even then, however, only around 53 % of adults reported ever having had an HIV test (National Statistical Office and IRC Macro 2011a). In the 2006 MLSFH, 80 % of those previously tested for HIV had been tested only once, and 98 % of these tests were with the 2004 MLSFH. Of those tested in 2004, only 9.9 % received another test after 2004 but before 2006. The 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.

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 pattern reflects misreporting is unclear, but previous research on reports of sexual behavior in Malawi suggests that misreporting may be fairly high (Helleringer et al. 2011; Mensch et al. 2008). In some instances, reports of sexual behavior, although lower than expected, are more internally consistent in face-to-face interviews than in alternative interviews using techniques designed to reduce reporting bias, such as audio computer-assisted self-interviewing (Mensch et al. 2008). Although 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.

Some concern may exist about the representativeness of the data because of attrition after 2004. Migration attrition after the 2004 wave of data is 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, which was specifically designed to find respondents not interviewed in 2006 owing to migration, are included with the follow-up data to reduce migration attrition bias. We also perform additional analyses of attrition 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 although observed characteristics differ between those who attrit and those who do not, these differences are not large enough to significantly bias parameter estimates in regression outcomes. See complete details of the attrition analysis in Online Resource 1 as well as the MLSFH Cohort Profile for related analyses of attrition (Kohler et al. 2014).

Results

Descriptive statistics, separated by HIV status and sex, are presented in Table 1.[12] Panel A describes the outcome variables, and panel B gives descriptive information for other variables. Panel A shows that 19 % of the 2004 HIV-positive respondents divorced after

[12]Descriptive statistics for HIV-positive respondents separated by sex are in Table S24 in Online Resource 1.

2004, compared with only 4 % or 5 % of HIV-negative respondents. Almost 16 % of HIV-positive respondents reported no sexual partner in the year of follow-up. In contrast, 5 % of HIV-negative women reported no sexual partner, and only approximately 1 % of HIV-negative men reported no sexual partner at follow-up. About 20 % of HIV-negative men and no HIV-negative women reported more than one sexual partner at follow-up, reflecting gender biases in reports of sexual behavior. However, this pattern is consistent with 2004 DHS data, in which 1.1 % of women and 11.8 % of men reported more than one sexual partner (National Statistical Office and ORC Macro 2005). Twenty-eight percent of HIV-positive respondents and approximately 16 % of HIV-negative respondents reported increased condom use with their spouse after 2004. Panel B shows fairly equivalent distributions in incentive amounts and distances to HTC sites between HIV-negative men, HIV-negative women, and HIV-positive respondents. HIV-positive respondents are 7–9 percentage points less likely to have picked up test results compared with HIV-negative men and women in the sample.

Table 2 shows results from the first-stage probit models, which estimate the propensity to pick up HTC test results based on the incentives and distance to the HTC site. These estimates show that the randomized monetary incentives offered to respondents for picking up HIV test results are important and significant predictors of whether respondents indeed picked up their HIV status during the 2004 MLSFH. Higher incentive amounts result in a greater likelihood of picking up test results. The results for each outcome are very similar to one another and differ primarily with respect to variations in sample size due to the number of respondents who reported each outcome. 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 HTC site is a less effective instrument for predicting pick up of HIV test results, but shorter distances to HTC sites still increase the propensity to pick up results in some instances—in particular, for the number of sexual partners. The *F* statistic values in the first-stage models provide further evidence of instrument strength. (In Online Resource 1, Table S1, the estimated coefficients for the predicted probabilities are also very close to 1, as expected.) The conventional rule of thumb is that *F* statistic values above 10 indicate sufficient predictive power of the instruments (Wooldridge 2009). The *F* statistics in the HIV-negative models range from 61.0 to 33.9, indicating good prediction of learning HIV status and lending confidence to the ability of the instruments to randomize the sample of HIV-negative individuals choosing to learn HIV status. The models for HIV-positive respondents yield *F* statistics of 12.8, 12.4, and 7.0. Potential limitations resulting from the weaker *F* test (for condom use) are discussed in the next section.

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 OLS and second-stage IV regression coefficients for number of sexual partners in the year of follow-up.[13] Table 3 reports results for HIV-negative women; Table 4, for HIV-negative men; and Table 5, for HIV-positive respondents.

[13]Marginal effects are evaluated at the mean values of the RHS variables.

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Models 1 and 2 of Table 3 report 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 among HIV-negative women by 2.8 % in the subsequent two years (p = .10). Results for the second-stage IV model in column 2 do not retain significance, and the point estimate decreases from .28 to .17 (a 39 % decline). This highlights the selection bias present in the noninstrumented models and the reduction in that bias through the instrumentation of learning HIV test results. The IV estimates in Model 4 suggest that learning HIV status increases the number of additional sexual partners for HIV-negative women by 0.075, an effect that is substantially higher (by 36 %) than that indicated by the OLS analyses, which are subject to endogeneity concerns. Recalling that the vast majority of women reported either no partners or one partner, this higher chance of having a partner among HIV-negative women who learn their status may reflect a lack of marginalization among known HIV-negative women. Learning HIV status does not significantly affect condom use in the regular probit model, but in the instrumented model, we find a statistically significant and substantively important 16.2 percentage point increase in reported condom use with spouses among HIV-negative women who learn their status.

Table 4 presents the second-stage results (marginal effects) for the effect of knowing HIV status on subsequent behaviors among HIV-negative men. Results for divorce and number of sexual partners in the year of follow-up are not significant in either the regular probit or the IV probit model. Probit results in Model 6 show an increase in men's condom use with their spouse after learning HIV-negative status, but this estimated effect is substantially diminished in IV probit results and does not retain statistical significance. Results from the regular probit model reflect the outcome for the select group of men who may seek out testing. Overall, however, learning HIV-negative status is not the cause of any observed behavioral change among men.

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, Table 5, Model 1 shows that learning HIV-positive status for the first time decreases the risk of divorce in the subsequent two years by almost 14 percentage points (p = .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. Results for both number of sexual partners and condom use with one's spouse show the opposite pattern; this seems to indicate that learning HIV status has no effect on number of partners or condom use in the regular probit and OLS models but has a significant effect in the IV models. Learning HIV status decreases the number of reported sexual partners at follow-up for HIV-positive respondents by almost 40 percentage points and increases reported condom use with one's spouse after 2004 by 40 percentage points as well.

The results for HIV-positive respondents should be interpreted with caution given the small sample size and low F statistics from first-stage estimates of learning HIV status on condom use. It would have also been preferable to separate the analysis for men and women who are HIV-positive. The small HIV-positive sample necessitates that our results be interpreted with some caution because the standard errors of the coefficients are large. The divorce outcome (n = 106) has a power of only 0.59 at alpha = .10 in detecting differences in divorce by knowledge of HIV status. The required sample size to attain a minimum desired power of .80 at alpha = 0.10 is n = 156. 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 study was to improve upon previous research measuring the effect of married individuals learning HIV status on later behaviors, including divorce, number of sexual partners, and condom use with spouse. We used instrumental variables to control for selection bias that has often probably been present in similar studies and was present in this study prior to control. We find that knowledge of HIV-negative status does not lead to different chances of divorce. Our findings suggest that this is also true for HIV-positive individuals (although this is based on a relatively small sample size). Our results show that learning one's HIV status leads to reduced risky behavior, including more condom use with spouse and fewer sexual partners, among HIV-negative women and HIV-positive individuals. Importantly, these effects are often only present after selection bias into HIV testing is accounted for. These results imply that HIV testing does not cause divorce or increased risky behavior but instead suggest that there may be reductions in risky sexual behavior. Previous studies finding unintended negative consequences of HTC are most likely driven by sample selection: individuals who seek testing also differ on other unmeasured characteristics that may affect relationship dynamics and sexual behavior, as well as bias conclusions about the effect of testing on later behaviors.

Women who actually seek testing are less likely to divorce after learning of their HIV-negative status. This self-selected group of women may display a difference in the desire to know their HIV status because of strong perceived HIV risk or marital discord that existed prior to testing, which could be the actual drivers affecting the 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. HIV-negative women who learn their status are also slightly more likely to have an additional sexual partner; most often the "additional" partner represents one partner as opposed to no partners.[14] Assurance of HIV-negative status may be a signal to women about the faithfulness of their partner, making them more willing to stay with that partner. 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 that HIV-negative women may be more highly valued. HIV-negative women who learn their status are also more likely to report increased condom use with their spouse or live-in partner than are HIV-negative women who do not learn their status. We observe this effect only after accounting for selection into HIV testing, implying that women who would have learned their HIV status regardless of additional incentives are not more likely to increase condom use. This could be true for many reasons. Perhaps when women are interested in learning HIV status, they are interested in ensuring their negative status

[14]Results are the same with a dichotomous variable for zero versus one partner.

before becoming pregnant, in which case condom use would not be expected to increase. Increased condom use after taking into account selection into HIV testing may be a result of being surprised about an HIV-negative test result or being motivated by a desire to protect their newly discovered HIV-negative status (Anglewicz and Kohler 2009; Gong 2014).

For HIV-positive respondents, knowing HIV status has no effect on the propensity to divorce in the IV models; however, before we control for selection into choosing to learn HIV status, they are 14 % less likely to divorce. This implies that only those who seek to know their HIV test results are actively using that information to make decisions about 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 HIV-positive, then both spouses must be HIV-positive. Learning HIV-positive status also results in fewer reported sexual partners and a substantial increase in reported condom use with spouses and live-in partners in IV models, suggesting that testing may cause reduced risky behavior among those who are HIV-positive. Overall, these results suggest that HIV testing is not the cause of divorce among married individuals who test HIV-positive, and these individuals are also more likely to report a reduction in risky behaviors, such as multiple sexual partnerships or unprotected sex with their spouse, as supported by similar studies (De Paula et al. 2014; Thornton 2008).

It is possible that the results from this study were underpowered for HIV-positive individuals. Thus, these results should be interpreted as only suggestive. Future research on the effect of learning HIV-positive status utilizing a larger sample would be beneficial, although no suitable data to do so currently exist. The inability to separate the analysis for men and women among the HIV-positive sample is a limitation as well, making it unclear whether results for divorce would differ by sex. In descriptive statistics alone, HIV-positive women divorce more frequently (see Online Resource 1, Table S10). In additional analyses of models separated by sex for HIV-positive respondents, results indicate that HIV-positive men may be significantly less likely to divorce after learning HIV-positive status, whereas learning HIV-positive status has no effect on chances of divorce among HIV-positive women. However, these analyses are somewhat underpowered, with sample sizes of 42 for men and 64 for women.

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: very large sample sizes are needed to estimate new incidence because HIV annual incidence rates are very low, usually between 1 % and 3 %. We did estimate whether learning HIV status in 2004 reduced the chances of becoming HIV-positive in the years to follow, although the number of new HIV-positive cases is small (see Online Resource 1, pp. 6–8 and Tables S5–S7). The results are significant for the noninstrumented models but not the IV estimates, suggesting that HIV testing may not cause lower HIV incidence but that the availability of HIV testing causes lower incidence among those who access testing.

Because this study focuses on married respondents, it is also of interest to know whether the same effects would remain within couple-based HTC programs. The impact of couple-based HTC programs on HIV prevention has recently gained credibility and emphasis in the literature, with studies often finding associations between couple-based HTC and increased prevention efforts (Burton et al. 2010; Desgrees-du-Lou and Orne-Gliemann 2008).[15] We created a proxy for couple-based HTC by matching respondents to their spouses if their spouses also participated in the program (see Online Resource 1, pp. 9–10, Tables S8–S10). The sample size for couples who were both tested is much smaller than the individual-level sample size, and the number of endogenous variables needing instrumentation increases, limiting our ability to obtain converging results for binary outcomes in IV models. We treated the binary outcomes as continuous in order to obtain estimates, but none were significant for learning HIV status, spouse learning HIV status, or both spouses learning HIV status. It is unclear whether larger positive effects exist for couples in the sample or whether the analysis lacks statistical power to detect a potential effect.

An interesting change that occurred after the data used in this study had been gathered is 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 found that the availability of ART has 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 marital and sexual dynamics would also be affected if the availability of ART results in a more optimistic view of the future. Future analysis should explore whether divorce and sexual behaviors are affected by the availability of ART.

Reliance on self-reported behavior of sensitive topics is a potential 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 nearly as much bias in the current analysis. Although divorce may not be reported perfectly, it is subject to much less misreporting than sexual behavior. A cross-check between several different variables in the repeated longitudinal marriage history available in the MLSFH gives us even more confidence in the accuracy of reported divorce.

Although it is possible that the magnitude of the effect of learning HIV status on the outcomes of interest might increase in the IV models relative to the standard estimates if random measurement error is biasing the standard estimates toward zero, it is more likely 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 effect on the IV estimates. 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 toward zero. Conversely, the IV estimates more accurately represent a broader range of individuals,

[15]Eriksson and Sovero (2013) examined the effect of HIV testing on divorce among HIV-negative couples. We have been unable to replicate these findings, despite our best attempts to duplicate their approach.

cautious and not cautious; thus, 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 are 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 time before the next MLSFH follow-up survey, although increases in the availability of HIV testing in Malawi were modest between 2004 and 2006. The potential dilution of the presumed effect of picking up HTC test results in 2004 may also be higher for HIV-positive respondents as compared with HIV-negative respondents. It could also be that HIV-positive respondents were less likely to pick up their results in 2004 if they already knew their status prior to the 2004 survey.[16] Any of these scenarios would lead to systematic error in the endogenous variable such that the results presented here would be biased downward, meaning that our conclusions would be strengthened with the removal of this potential bias.

Although IV models reduce bias, they may not completely remove all bias stemming from omitted variables and selection (Deaton 2010; Easterly 2009; Heckman and Urzua 2009; Imbens 2010). Persons at the margins of certain behaviors or characteristics may be more likely to engage in behaviors that deviate from the norm in ways that reduce 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. However, the attempt to randomize individuals who pick up HIV tests will most likely result in *less* biased estimates. Advocates of randomized experiments focus on the importance of the improvement in accuracy and the viability of results drawn from statistical methods that fully utilize the advantages inherent in such designs (Imbens 2010). The MLSFH experimental design still makes significant progress in reducing bias and endogeneity issues.

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. Our research in Malawi utilizes a randomized design, resulting in exogenous variation in HIV-status knowledge. The results suggest that learning one's HIV status does *not* significantly affect the propensity to divorce in Malawi. Our study convincingly documents these effects (or non-effects) of knowledge of one's HIV status on divorce for HIV-negative individuals. Our conclusions are somewhat more suggestive for HIV-positive individuals owing to a small sample size. Combined with our findings of changes in the number of reported sexual partners and reported condom use with one's spouse after learning HIV status, the results give a clearer picture of how relationships and HIV prevention are jointly navigated in the high-HIV context of Malawi. Our study reinforces earlier findings that HTC is an important component of HIV reduction. Findings support the assumptions that HIV-negative individuals will protect themselves against future risk of infection and that HIV-

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

positive individuals will be motivated to take precautions to protect others. At the very least, our study provides strong evidence that HTC does not cause *increased* risky sexual behavior. Most importantly, providing HIV testing and counseling remains an important element of HIV prevention by providing individuals the opportunity to gain knowledge and control of their own health through mechanisms such as changing sexual behaviors and/or condom use.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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Table 1

Descriptive statistics

	HIV-Negative Women		HIV-Negative Men		HIV-Positive	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
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	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						

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	HIV-Negative Women		HIV-Negative Men		HIV-Positive	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
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

Nonlinear two-stage estimator: First-stage probit estimates of 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	0.876** (0.143)	1.038** (0.164)	1.132** (0.398)	0.876** (0.137)	1.014** (0.160)	0.783* (0.364)	0.904** (0.155)	1.049** (0.188)	1.769** (0.484)
60–100 kwacha	1.376** (0.133)	1.231** (0.161)	2.540** (0.499)	1.389** (0.128)	1.218** (0.163)	2.416** (0.459)	1.334** (0.143)	1.364** (0.189)	
110–200 kwacha	1.629** (0.136)	1.573** (0.163)	2.572** (0.543)	1.623** (0.137)	1.591** (0.180)	2.527** (0.447)	1.623** (0.156)	1.633** (0.200)	2.287** (0.516)
210–300 kwacha	1.925** (0.193)	1.936** (0.213)	2.035** (0.483)	1.989** (0.202)	1.894** (0.211)	1.907** (0.474)	1.704** (0.213)	1.937** (0.253)	2.333** (0.711)
Distance to HTC (km)	–0.312† (0.175)	–0.013 (0.168)	–0.733 (0.596)	–0.325† (0.182)	–0.060 (0.161)	–1.118* (0.520)	–0.304 (0.201)	0.087 (0.194)	–0.609 (0.858)
Distance to HTC, squared	0.043 (0.033)	–0.002 (0.029)	0.172 (0.111)	0.045 (0.034)	0.006 (0.028)	0.228* (0.098)	0.037 (0.038)	–0.015 (0.033)	0.131 (0.165)
Observations	1,008	775	106	1,004	750	113	758	595	106
Psuedo-R^2	.219	.213	.376	.222	.213	.343	.204	.225	.288
Wald Chi-Square	276.6	159.4	45.3	290.4	155.0	57.0	199.0	146.9	29.7
p Value	.000	.000	.000	.000	.000	.000	.000	.000	.001

Notes: Robust standard errors clustered by village are shown in parentheses. OLS coefficients are reported for outcomes. Covariates for all models include age, region of residence, education, and a dummy variable for gender in the HIV-positive models. For the condom use outcome, too few cases in the 60–100 incentive category required combining this category with the 10–50 category.

† $p < .10$;

* $p < .05$;

** $p < .01$

Table 3

Nonlinear two-stage estimator: Second-stage estimates for HIV-negative women

	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 Regression	(5) Probit	(6) IV Probit
Learned HIV Status	−0.028†	−0.017	0.055*	0.075*	0.033	0.162*
	(0.015)	(0.030)	(0.023)	(0.037)	(0.028)	(0.071)
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	−0.022	−0.004	−0.002	0.132**	0.128**
	(0.014)	(0.018)	(0.020)	(0.019)	(0.042)	(0.038)
Balaka, south	0.040**	0.039**	0.004	0.004	0.055	0.048
	(0.014)	(0.013)	(0.024)	(0.023)	(0.034)	(0.033)
Education (relative to no education)						
Primary	0.011	0.014	0.026	0.028	0.041	0.054†
	(0.013)	(0.015)	(0.024)	(0.024)	(0.030)	(0.032)
Secondary	0.047	0.040	0.046	0.050	0.112	0.124*
	(0.046)	(0.029)	(0.036)	(0.036)	(0.078)	(0.062)
Observations	1,008	1,008	1,004	1,004	758	758

Notes: Robust standard errors clustered by village are shown in parentheses. Marginal effects are reported for all binary outcomes; OLS coefficients are reported for the continuous outcome (Model 4). For Model 4, the value of Wooldridge's robust score test of endogeneity is 0.505, with a *p* value of .4787.

† $p < .10$;

* $p < .05$;

** $p < .01$

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Table 4

Nonlinear two-stage estimator: Second-stage estimates for HIV-negative men

	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 Regression**	**(5) Probit**	**(6) IV Probit**
Learned HIV Status	0.015 (0.013)	0.025 (0.034)	−0.063 (0.052)	−0.030 (0.105)	0.066* (0.033)	0.033 (0.074)
Age in 2004	−0.001* (0.001)	−0.002* (0.001)	−0.002 (0.002)	−0.002 (0.002)	−0.006** (0.001)	−0.006** (0.001)
Region (relative to Mchinji, central)						
Rumphi, north	−0.003 (0.018)	−0.002 (0.019)	0.188* (0.072)	0.192** (0.072)	0.103* (0.044)	0.093* (0.039)
Balaka, south	0.004 (0.016)	0.004 (0.017)	0.130† (0.067)	0.130† (0.067)	0.038 (0.036)	0.037 (0.034)
Education (relative to no education)						
Primary	0.021 (0.016)	0.025 (0.021)	−0.059 (0.092)	−0.057 (0.091)	−0.051 (0.050)	−0.052 (0.046)
Secondary	0.020 (0.029)	0.020 (0.024)	−0.116 (0.124)	−0.111 (0.125)	−0.027 (0.062)	−0.036 (0.070)
Observations	775	775	750	750	595	595

Notes: Robust standard errors clustered by village are shown in parentheses. Marginal effects are reported for all binary outcomes; OLS coefficients are reported for the continuous outcome (Model 4). For Model 4, the value of Wooldridge's robust score test of endogeneity is 0.159, with a *p* value of .6910.

† $p < .10$;

* $p < .05$;

** $p < .01$

Table 5

Nonlinear two-stage estimator: Second-stage estimates for HIV-positive men and women combined

	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 Regression**	**(5) Probit**	**(6) IV Probit**
Learned HIV Status	−0.136† (0.074)	−0.085 (0.109)	−0.084 (0.124)	−0.399* (0.184)	0.149 (0.102)	0.402** (0.117)
Male	−0.078 (0.073)	−0.078 (0.078)	0.513** (0.156)	0.531** (0.148)	0.256* (0.102)	0.233* (0.094)
Age in 2004	−0.004 (0.004)	−0.005 (0.005)	−0.009† (0.005)	−0.008 (0.005)	−0.011† (0.006)	−0.011* (0.005)
Region (relative to Mchinji, central)						
Rumphi, north	−0.002 (0.115)	0.010 (0.119)	0.276* (0.133)	0.192 (0.141)	0.183 (0.114)	0.243* (0.101)
Balaka, south	0.002 (0.077)	0.004 (0.078)	0.215† (0.124)	0.201 (0.134)	−0.090 (0.119)	−0.062 (0.107)
Education (relative to no education)						
Primary	0.024 (0.092)	0.029 (0.092)	0.000 (0.091)	−0.012 (0.089)	−0.107 (0.173)	−0.108 (0.175)
Secondary	0.048 (0.204)	0.052 (0.183)	−0.083 (0.298)	−0.123 (0.299)	−0.365 (0.248)	−0.326 (0.229)
Observations	106	106	113	113	74	74

Notes: Robust standard errors clustered by village are shown in parentheses. Marginal effects are reported for all binary outcomes; OLS coefficients are reported for the continuous outcome (Model 4). For Model 4, the value of Wooldridge's robust score of endogeneity is 4.38, with a *p* value of .0412.

† $p < .10$;

* $p < .05$;

** $p < .01$