The Demand for and Impact of Learning HIV Status: Evidence from a Field Experiment

Rebecca Thornton*
Harvard University

January 10, 2006

(Job Market Paper)

Abstract: It is commonly believed that HIV testing is essential for disease prevention. Indeed, spending on counseling and testing accounts for over half of the total expenditures on HIV prevention in some African countries. Despite this, there is evidence that even when testing is available most people do not take advantage of it, and there is little causal evidence on the behavioral response to knowing one's status. For this paper, I designed and implemented a randomized experiment to evaluate the demand for learning HIV results and to estimate subsequent behavior change. In the experiment, over 2,700 individuals in rural Malawi were randomly assigned monetary incentives to learn their HIV results after testing. Two months later, they were re-interviewed and given the opportunity to purchase condoms. I find that while less than half of the participants attended clinics to learn their HIV status without any incentive, even a very small incentive (about one-tenth of a day's wage) increased the share learning their results by 50%. Using the exogenously assigned incentives and distance from results centers as instruments for HIV knowledge, I find that HIV positive subjects with a sexual partner who learn their status are significantly more likely to purchase condoms; however, the average number of condoms purchased is low. Using the estimates of the effect of learning HIV status on condom purchases, I calibrate an epidemiological model of infection that suggests that HIV testing is not as cost-effective as other prevention strategies.

---

* e-mail: rlthornt@fas.harvard.edu; address: NBER, 1050 Massachusetts Ave, Cambridge, MA 02138; phone: (617)-588-0349; web address: www.people.fas.harvard.edu/~rlthornt/

I am grateful to Randy Akee, David Cutler, Eliza Hammel, Richard Holden, Erica Field, Ed Glaeser, Emir Kaminica, Larry Katz, Michael Kremer, David Laibson, Sendhil Mullainathan, Ben Olken, Emily Oster, Michelle Poulin, Mark Rosenzweig, Jesse Shapiro, and participants at the Harvard Development and Labor Lunches for helpful comments and discussion. I thank Jere Behrman, Hans-Peter Kohler, Susan Watkins and the MDICP team for support and data and Kondwani Chavula for excellent assistance with fieldwork.

The Malawi Diffusion and Ideational Change Project is supported by grants from the Rockefeller Foundation; NICHD (R01-HD4173, R01 HD372-276); NIA (AG1236-S3); the Center for AIDS Research and the Center on the Demography of Aging at the University of Pennsylvania. The project on Information, Incentives and HIV/AIDS Prevention is supported by the University Research Foundation, University of Pennsylvania. Follow-up data is supported by the Warburg Foundation of the Economics of Poverty. I acknowledge financial support from the Harvard Population Center, Harvard Graduate Council, the Harvard Center for International Development, National Bureau of Economic Research Fellowship on Aging, and the University of Pennsylvania.

1 Introduction

Over the past two decades, the HIV/AIDS epidemic has afflicted Africa, with over 2.3 million AIDS-related deaths and 25 million adults and children infected with HIV in 2001 (UNAIDS 2001). One suggested intervention to alleviate the spread of the disease is HIV testing, and some have gone so far as to declare that voluntary counseling and testing (VCT) is the "missing weapon" in the battle against AIDS (Holbrooke and Fuhrman 2004)[1]. Under the assumption that HIV testing is an effective prevention strategy, many international organizations and governments have called for increased investments into counseling and testing, requiring large amounts of monetary and human resources. For example, in South Africa, government expenditures on counseling and testing went from $2.4 million in 2000 to $17.3 million in 2004 and in Mozambique, 55 percent of the total expenditure on HIV/AIDS programs was spent on testing programs in 2000 (Martin 2003)[2]. Some governments have even suggested implementing universal testing programs for all citizens, sending nurses house-to-house (Donnelly 2005).

Underlying the emphasis on HIV testing for prevention – and the large expenditures on testing – are two rarely challenged assumptions: first, that despite important potential benefits, it is difficult for people to learn their HIV status. This may be due to possible psychological and social barriers. Second, after learning HIV results, there are positive effects on sexual behavior that prevent the spread of the disease: specifically, that those diagnosed negative will protect themselves from infection and those diagnosed positive will take precautions to protect others.

In this paper, I evaluate a field experiment in rural Malawi designed and implemented to address these claims. I find that any barriers to learning HIV results can be easily overcome by offering small cash incentives. However, while there are significant behavioral effects after learning HIV positive results, the overall magnitude of the effects are small. Given these analyses, I find that relative to other available prevention strategies, HIV testing is not the most cost-effective way to avert the most infections

[1] Several organizations such as the Global Business Coalition, WHO, UNAIDS, Viacom, the Henry J. Kaiser Family Foundation, and the Bill and Melinda Gates Foundation have begun investing in large-scale international media campaigns to increase testing (Global Business Coalition 2005; knowhivaids.org 2005).
[2] Although testing is currently conducted among voluntary clients, mandatory testing has been widely discussed as a prevention strategy (Reporter 2004, Lafraniere 2004, Nyathi 2003).

with a given budget.

Previous studies have attempted to measure the demand for learning HIV status, as well as the subsequent behavioral effects after learning HIV results. Most studies rely on self-reported behavior by asking individuals if they want to know their HIV status (e.g., Day et al. 2003, deGraft-Johnson et al. 2004, Laver 2001 and Yoder 2004) or by asking about sexual behavior (e.g., Coates et al. 2000; Kamega et al. 1991, Temmerman et al. 1990, and Weinhardt et al. 1999). Self-selection is also a serious limitation to evaluating the effects of learning HIV results. Most, if not all, studies use a sample of individuals who self-select into knowing their status. One exception to this is a study that randomly phased-in individuals into being tested (Coates et al. 2000). Their findings, that learning HIV results substantially reduced *reported* risky sexual behavior, has since been widely cited within the public health literature and used in subsequent simulations to conclude that counseling and testing is an effective strategy for preventing new infections (Sweat et al. 2000). However, even the study by Coates and his colleagues was conducted among self-selected individuals who choose to have an HIV test at urban health clinics and relied on reported sexual behavior as the measure of behavioral change. Another study by Boozer and Philipson (2000) found large effects of learning HIV status among gay men in San Francisco, accounting for their prior beliefs of infection; this study also suffers from the above limitations.

The design of this experiment avoids the usual complications of selection and reporting bias because it measured actual attendance and condom purchases, randomized the location of centers where HIV results were available, and randomized individual incentives to learn HIV status. This is important because factors impacting the decision to learn HIV results are generally correlated with behavioral outcomes, leading to biased estimates of the impact of learning HIV results on sexual behavior.

Respondents in rural Malawi were offered a free HIV test and were given randomly assigned vouchers between zero and three dollars, redeemable upon learning their results. The demand for HIV information without incentives was moderate: 39 percent of those given no monetary incentive attended centers to learn their results. However, learning HIV results was highly elastic to these incentives and those receiving positive-valued incentives were, on average, twice as likely to learn their HIV status as individuals receiving no incentive. Although the average incentive was worth about a day's wage, even the smallest amount, one-tenth of a day's wage, resulted in large attendance gains. The location of each results center was also randomly placed to evaluate the impact of distance on attendance: living over one kilometer from the VCT center reduced attendance by seven percent. There is also evidence of strategic complementarities of neighbors' and spouses' attendance with respondents' own attendance.

Several months later, follow-up interviews were conducted and respondents were given the opportunity to purchase condoms. Using the random allocation of incentives and distance as exogenous instruments for learning HIV status, I find that receiving an HIV positive diagnosis significantly increased the likelihood of purchasing condoms among those with a sexual partner. Learning HIV status had no impact among those that were HIV negative or those who were not sexually active, and there are no additional effects when both members of a married couple learn their HIV status.

Given the estimated effects of learning HIV results on the demand for condoms and epidemiological parameters determining disease transmission, I estimate the cost-effectiveness of HIV testing in terms of expected new infections averted and associated costs. Compared to cheaper prevention programs, I find that subsidized population-based testing is not a cost-effective strategy for averting infections. However, if governments or organizations do choose to adopt HIV testing as a prevention strategy, such as for targeting high-risk groups or pregnant women, offering small rewards to encourage people to learn their results may be effective.

The paper proceeds as follows: Section 2 discusses the study design. Section 3 presents the impact of incentives and distance on learning HIV status. Section 4 presents the effects of learning HIV status on sexual behavior. Section 5 evaluates cost-effectiveness. Section 6 concludes.

2 Project Design

2. 1 Background on Malawi and Description of the Data

The Malawi Diffusion and Ideational Change Project (MDICP) is conducted in Malawi, a land-locked country located in southern Africa (Figure 1). This collaborative project between the University of

Pennsylvania and the Malawi College of Medicine is a panel study of men and women randomly selected from 125 villages in the districts of Rumphi, Mchinji, and Balaka, located in the north, central, and southern regions respectively[3]. Approximately one in four households in each village were randomly selected to participate, and ever-married women and their husbands from these households were interviewed in 1998, 2001, and 2004. During data collection in 2004, an additional sample of adolescents (ages 15-24) residing in the original villages was added to the sample.

Between May-August of 2004, nurses from outside each area offered respondents free tests for HIV and three other sexually transmitted infections (STI's), gonorrhea, chlamydia, and trichomoniasis. Samples were taken through oral swabs to test for HIV and through urine (men) or self-administered vaginal swabs (women) to test for other STI's[4]. Across the three districts, 2769 respondents accepted a test for at least one sexually transmitted disease. Sample attrition and test refusals are discussed below.

The sample is 46 percent male with an average age of 34 (Table 1, Panel A). Sixty-eight percent of the respondents were married at the time of the interview and 80 percent had ever attended school, attending an average of five years. There are large differences in ethnicity and religion across the three districts: the Chewas in Mchinji and the Tumbukas in Rumphi are primarily Christian, and the Yaos in Balaka traditionally practice Islam. The majority of the respondents are subsistence farmers with an average annual crop yield in 2004 of 2,326.4 dollars per household (median value of 1,803.4 dollars); most crops are produced for home consumption[5].

The HIV prevalence rate was 6.7 percent (7.5 percent rates for females, 5.5 percent for males). Prevalence rates for other sexually transmitted diseases were even lower, with 3.5 percent infected with gonorrhea, 0.4 percent with chlamydia, and 2.1 percent with trichomoniasis (Table 1, Panel B)[6]. The level of HIV infections in the MDICP sample is considerably lower than national prevalence rates; this discrepancy may be due to the fact that national estimates are typically derived from urban testing centers

[3] For further sampling details see http://www.malawi.pop.upenn.edu/Level%203/Malawi/level3_malawi_sampling.htm
[4] Bignami-Van Assche et al. 2004 provides the full testing protocol.
[5] Annual production yield is calculated as the total value of shelled maize, tobacco, cotton, and soybeans.
[6] Trichomoniasis was only tested among women.

and antenatal clinics rather than rural representative populations[7]. Longitudinal sample attrition from death and migration (discussed below) may also bias the estimates downward as well as the disproportionate number of adolescents in the sample who have lower rates of infection (1.7 percent).

2.2 Experimental Design

The first part of the experimental design involved giving monetary incentives to encourage respondents to obtain their test results. After taking the test samples, nurses gave each respondent vouchers redeemable upon obtaining either HIV or STI results. Voucher amounts were randomized by letting each respondent draw a token indicating a monetary amount out of a bag. In Mchinji and Balaka each respondent received two vouchers, one for returning for HIV results, and one for returning for STI results. In Rumphi, respondents received only one voucher redeemable by returning for either HIV or STI results[8]. For the analysis, I examine the impact of the total value of the incentive (the sum of the HIV and STI incentives) on learning results. Vouchers ranged between one and three dollars and the average total voucher amount was 1.04 dollars, worth approximately a day's wage (Table 1, Panel C). Although most respondents work on their own agricultural plots, the reported average weekly income in 2001 was 9.5 dollars; the hourly wage of day-laborers from a different representative sample in Malawi was roughly 30 cents per hour in 2002 (IFPRI 2000-2002).

The distribution of vouchers was carefully monitored to ensure that each nurse followed the rules of randomization[9]. Each voucher included the amount, a respondent ID, and the nurse's signature; a

---

[7] Recent population-based studies in Kenya, Mali, and Zambia also find significantly lower HIV prevalence rates than UNAIDS estimates (Walker et al. 2004). The estimated HIV prevalence rate for Malawi was 14.2 in 2004 (UNAIDS 2004)

[8] In Mchinji and Balaka, there were no individuals who did not want both HIV and STI results.

[9] During a pilot, nurses gave out higher incentive amounts than the distribution would suggest probable, likely feeling sympathetic to poor villagers. They were then instructed that continuation of employment was contingent upon following the instructions of randomization. However, it appears that fewer "zero incentives" were given out (Appendix A). On average, the cumulative distribution of actual incentives given out is $0.24 higher than the theoretical distribution (a Kolmogorov-Smirnov Test rejects the null hypothesis of equality at the 0.01 percent level, not shown). For incentive amounts over one dollar, there is no significant difference between the actual and theoretical incentives. It is possible that a few respondents were allowed to "re-draw" the voucher amount if they had originally selected a zero; there is no systematic difference by observable respondent or nurse characteristics (not shown). This would be most problematic for estimates if nurses favored individuals who were more likely to practice safe sex or purchase condoms after learning their HIV status.

carbon copy was made in order to prevent forgeries. If a respondent drew a token indicating zero incentive, no voucher was given to the respondent; 20 percent received no incentive to return for either HIV or STI results[10].

Two to four months after collecting samples, test results became available and temporary counseling centers consisting of small portable tents were placed randomly throughout the districts, stratified by village[11]. Based on their geo-spatial (GPS) coordinates, respondents' households in villages were grouped into zones, and a location within each zone was randomly selected to place a tent[12]. The average distance to a center was 2.0 km and over 95 percent of those tested lived within five kilometers. Distance to the results center is calculated as a straight-line and does not account for roads or paths[13].

Respondents were personally informed of the time and location of their assigned center (open Monday through Saturday from eight in the morning until seven in the evening) and centers were operational for approximately one week. Respondents were allowed to attend any of the VCT centers but were only informed of the location and time of their assigned center (less than six percent of respondents went to a different center than the one to which they were assigned). On average, nurses spent 30 minutes with each respondent and were instructed to spend equal time with HIV positives and negatives[14]. Couples were not informed of their results together, and results were verbally told to each respondent. Respondent were not allowed to redeem their voucher unless they heard their results. Those who were

[10] Drawing a "zero" may have had a de-motivating effect on individuals, which in turn may have impacted attendance. Because all of the respondents participated in the "lottery" draw, it is impossible to estimate the potential effect of disappointment.

[11] Although rapid tests were available for HIV tests, test for the STI's required laboratory analysis. The large number of samples analyzed at the laboratory as well as shortages in chemical reagents also significantly increased the time until results were available (Anglewicz et al. 2004).

[12] GPS locations are accurate between 10-15 meters. In most cases tents were placed in the exact randomly selected location and paths were created for easy accessibility for all to attend.

[13] Calculating straight-line distance ignores natural boundaries such as roads or rivers and may underestimate the actual distance needed to travel. Respondents attending were asked how long it took them to travel to the center (multiplying total time by two for those traveling by bicycle). The average time to reach the center was 42 minutes. Approximately seven percent of households have missing household GPS coordinates and, in this case, the distance to an assigned VCT center is replaced with the average village distance to the results center. Note that the permanent health clinics nearest to each of the study sites are located over 50 kilometers away.

[14] Several studies have shown that education can have an impact on condom use and HIV prevention (see Stryker et al. 1995; Sikkema et al. 2005). In this study, however, it is impossible to distinguish a pure information effect (learning HIV status) from an educational effect (counseling).

HIV positive were referred to the nearest clinic for further counseling and those who were positive for other sexually transmitted diseases were given free treatment[15].

Approximately two months after results were available, all respondents who tested for HIV in two districts, Balaka and Rumphi, were re-interviewed in their homes. During the interview, respondents were asked about their sexual behavior in the past two months and their attitudes towards condoms. Respondents were then given 30 cents as appreciation for participation and were offered the opportunity to purchase condoms at half the subsidized retail price: five cents for a package of three condoms or two cents for a single condom[16]. Respondents were only allowed to purchase condoms from the 30 cents they had been given to prevent condom purchases from being correlated with having had received a monetary incentive at the results center. Only three individuals purchased the maximum number of condoms.

2.3 Sample Attrition and Test Refusals

The sample used for analysis throughout this paper consists of the 2,769 who accepted an HIV test. Although the original sample in 1998 was randomly drawn, sample attrition across waves of data collection affects the degree to which this sample is representative. The primary reason for attrition across all waves of data is temporary and permanent migration; in 2004, 18 percent of those interviewed in 2001 were away or had moved, which is comparable to the attrition rates of other longitudinal studies in Africa (Chapoto and Jayne 2005; Maluccio 2000)[17]. No village ever refused to participate in data collection and less than one percent of those approached in 2004 refused to be interviewed[18]. Only three percent of the attrition from 2001 to 2004 was due to reported death or hospitalization.

Sample attrition from the panel means there are disproportionately fewer mobile and sick individuals, potentially leading to a downward bias in HIV prevalence rate. Test refusals may also be a

[15] Free treatment for STI's may have also provided additional incentive to attend VCT centers, over and above the monetary incentive. However, STI prevalence rates were very low and only 15.2 percent of respondents reported believing there was any chance of their being infected with a non-HIV STI.
[16] The most common condom in Malawi are subsidized by Population Services International. A package of three condoms typically costs ten cents. Other more expensive varieties are available but not commonly used.
[17] Also, between 1998 and 2001, 19 percent of males and 16 percent of females were attritors, mainly due to migration (Van-Asche, Reniers and Weinreb, 2003).
[18] In general there is a good relationship between MDICP and its respondents, which may be in part due to small gifts for participation or to the employment of local high-school graduates as interviewers.

source of bias – 91 percent of those approached agreed to be tested for HIV[19]. However, this is a relatively high acceptance rate; this may be due to the fact that HIV was tested through saliva samples, rather than blood, or that respondents were not required to learn their results at the time of testing[20]. Not all spouses of respondents were offered a test: men who divorced or were widowers and spouses of the newly sampled adolescents were ineligible for testing.

Baseline characteristics are similar across groups receiving any incentive amount (including zero) and living within various distances to the results centers. Although there are certain statistically significant differences among these groups, the differences are small (e.g., those receiving an incentive were on average 1.3 years older and had 0.8 fewer years of education; not shown).

During the follow-up survey, 82 percent of all of those tested were interviewed. Having learned HIV results and HIV status are both separately associated with attrition from the follow-up sample. Being HIV positive increases the likelihood of attrition by 14.7 percentage points, which could be due, in part, by the fact that several of the HIV positive respondents passed away in the time between being tested until the follow-up survey and others reported being too sick to be interviewed. Those attending the VCT center were 17.9 percentage points less likely to attrit, which to a certain extent is mechanical – those who were available to attend the VCT center were also more likely to be available for the follow-up interview (if for example, they had not temporarily migrated). Importantly, all exogenously-assigned variables (receiving an incentive, the amount of the incentive, and the distance from the VCT center) have no significant effect on likelihood of attrition at the follow-up. Thus, while sample attrition and HIV test refusals may pose a potential threat to the external validity of the study, because there is no differential attrition associated with incentives or distance, the risk to the internal validity of the study is minimal (See Appendix B for attrition statistics).

---

[19] Of all married individuals, 65 percent of spouses were tested for HIV, 3.1 percent refused a test, and 32 percent were ineligible or away and were not approached by nurses.

[20] Observable characteristics of gender, age, religion, or education, are not significant predictors of accepting an HIV test. Response bias is also not likely to have affected estimated prevalence rates (Obare 2005). Test acceptance rates for other sexually transmitted diseases were similarly high.

3 Learning HIV Results

3.1 Theoretical Considerations

In an expected utility framework, the benefits for learning HIV status are positive to the extent that individuals use the information to update their behavior. Those diagnosed negative can practice safe sex to protect themselves from future infection; those diagnosed positive can seek treatment, and if altruistic, can prevent spreading the virus to children or sexual partners. Furthermore, all individuals are able to more accurately plan for the future.

However, while there may be strong motivations for testing and acquiring the results for treatable diseases or STI's, these incentives may be absent for HIV because there is no cure for this disease. Moreover, in low-income countries the access to anti-retroviral therapies is limited, further reducing the incentive to learn HIV results (Glick 2004; Stein 2005)[21]. The costs of testing and travel also prevent individuals from learning their HIV status (Forsythe et al. 2002; Laver 2001; Lebowitz and Taylor 2004; Fernandez et al. 2005). However, utilization rates are low even when even when testing services are free or low-cost. For example, in Malawi where HIV testing is free, only nine percent of individuals reported actually having been tested (Malawi DHS 2000)[22]. Moreover, even when individuals choose to be tested for HIV, many do not return for their results: only approximately 65 percent of individuals who test for HIV return to learn their results (Cartoux et al. 1998; Ekwueme et al. 2003).

It is therefore commonly suggested that psychological costs are important, perhaps crucial, barriers to testing and learning results. The psychological costs associated with learning HIV results can be either internal, such as having stress, worry, or fear, or external, such as experiencing social stigma (See for example HITS-2000 Investigators, 2004; Mugusi et al. 2002; Ginwalla 2002; Baggaley 1998; Hutchinson 2004; Ford 2004; Coulibaly 1998; Kalichman 2003; and Wolff 2005)[23].

---

[21] Also, even when there are anti-retroviral therapies available, most patients must wait until they have severe symptoms before receiving treatment.

[22] In Malawi, all HIV testing services are free. Individuals need only pay for transportation to clinics.

[23] There is also a growing body of literature within behavioral economics suggesting anxiety or fear may be important factors in decision-making, especially in seeking health information (See Camerer, Lowenstein and Rabin 2003, Frank 2004, Rabin 1998, and Wu 2003). Caplin and Leahy (2001) present a model of psychological expected

There are several mechanisms through which offering monetary incentives may affect learning HIV results. First, incentives may directly compensate for the costs of learning HIV results, including the monetary costs of time or travel, or internal psychological costs. Second, monetary incentives may reduce actual or anticipated stigma. For example, others could interpret attending a results center as a signal of self-perceived risk of infection or of prior sexual behavior. Monetary incentives may provide individuals with an excuse for going to the results center, thereby reducing or eliminating others' ability to make negative inferences by observing attendance.

There may also be strategic complementarities of others' attendance. These may be positive if, for example, neighbors or spouses provide additional emotional support, reducing internal psychological costs, or if there are economies of scale of travel costs. Alternatively, they may be negative if increased attendance by neighbors increase external psychological costs, reducing own attendance.

3.2 Impact of Incentives and Distance on Learning HIV Results

Across the three districts, 72 percent of all respondents attended the results centers and both incentives and distance had large effects on seeking HIV results (Table 1, Panel C)[24]. Figure 2 presents the percent attending a results center as a function of receiving any incentive (Panel A) and the total amount of the incentive (Panel B). These figures illustrate the large impact of receiving an incentive on attendance, as well as nonlinearities: positive-valued vouchers had similar effects on attendance regardless of their amount. Error bars are also presented showing precisely estimated effects.

Turning to the effects of distance, Figure 3, Panel A, presents the impact of living varying kilometers from the nearest results center on attendance, estimated by a non-parametric, locally weighted regression restricted to the closest 95 percent of the sample (Fan 1992). There was a strong negative effect

---

utility and in an expansion of this work, Caplin and Eliaz (2003) and Koszegi (2003) model anxiety to learn health status. See also Philipson and Posner (1995). It is also possible that individuals over-estimate the potential psychological effects of receiving an HIV diagnosis (Wilson and Gilbert 2003, Sieff et al. 1999).

[24] VCT attendance varied by district: 81 percent attended in Mchinji, 75 percent in Balaka, and 61 percent in Rumphi. These differences may be due to the fact that HIV results were available during different cycles of the agricultural season and higher opportunity costs of time during the planting season may have resulted in lower average attendance rates in Balaka and Rumphi. Without more detailed time use data, however, effects of the agricultural season cannot be distinguished from effects of characteristics that vary systematically by district.

of distance, especially among those living less than one kilometer from the center.

To measure the demand for learning HIV results in a regression framework I estimate

$$\text{(1)} \qquad \text{Got Results}_{\text{ij}} = \alpha + \beta_1\text{Any}_{\text{ij}} + \beta_2\text{Amount}_{\text{ij}} + \beta_3\text{Distance}_{\text{ij}} + \beta_4\text{Distance}_{\text{ij}}^{\ 2} + X'_{ij}\mu + \varepsilon_{ij}$$

Attendance at the VCT center is indicated by "Got Results" = 1 for person i in village j. "Any" indicates if the respondent received any non-zero voucher and "Amount" is the dollar amount of the incentive. Including both terms allows for non-linear effects of the incentive. "Distance" is the number of kilometers from the randomly placed VCT center assigned to person i. A vector of controls, X, includes covariates of gender, age, age-squared, HIV status, district dummies, as well as a control for a simulated average distance in each VCT zone. Because the locations of the centers were chosen randomly, as opposed to randomly assigning the distance needed to travel, I draw 1,000 simulated random locations in each VCT zone and calculate the average distance of each tested respondent from each of the 1,000 simulated locations. I average this distance for each respondent and take the mean across all respondents living in each zone. Not controlling for this simulated average term would ignore the fact that each VCT zone is bounded and that in expectation, more central households have shorter distances to travel from any randomized location (although including this term does not significantly change any of the estimates). I cluster standard errors by village, for 125 villages[25]. Although the dependent variable is binary, the linear specifications do not differ greatly from estimations from probits; I present the linear results.

Learning HIV results was highly elastic in response to incentives. The average attendance of those receiving any positive-valued voucher is twice that of those receiving no incentive, a difference of 39 percentage points (Table 2, Column 1). Moreover, the probability of attendance increased by an additional 8.9 percentage points with every one dollar of incentive (Column 2).

[25] The VCT centers were placed throughout the study site such that many villages were assigned the same center – this ranged from as few as three villages at one center (in Balaka) to as many as 14 villages attending one center (in Rumphi). Clustering standard errors by VCT center, rather than village reduces the standard errors on the impact of incentives, but increases the standard error of the effect of distance on attendance. However, the results are not significantly different when using larger clusters. Also, because there were only 16 VCT centers, there may be omitted variables correlated with the center location that may be systematic, despite the randomized design. However, observable characteristics are balanced among the different centers.

There was no significant impact of HIV status on attendance in two of the three districts. However, the small number of HIV positives in those districts makes statistical inference difficult. The third district, Balaka, had the highest HIV prevalence rate, and those infected with HIV were eleven percentage points less likely to attend the VCT center than those that were HIV negative (significant at the 0.02 percent level; regression not shown). Overall, males were no more likely to attend than women although attendance varied by gender among adolescents. Girls younger than 20 years were 9.1 percentage points less likely to attend the results center (significant at the 0.01 percent level; regression not shown). It may have been more costly for adolescent girls to travel to the clinics because attendance could be interpreted as a signal of sexually activity[26]. Opportunity costs may also be important. For example, ever attending school reduced the probability of attending the VCT by 5.0 percentage points (significant at the 0.01 percent level; regression not shown).

Distance and distance squared had significant impacts; also, those living more than one kilometer away from the center were 5.1 percentage points, or seven percent, less likely to learn HIV results (Table 2, Columns 3, 4)[27]. There were several VCT centers in Balaka placed over nine kilometers from sample households and were open several days without any attendance. Because of the lack of attendance, new random locations were chosen, sites re-located, and respondents informed of the new locations; these locations are excluded from the analysis. However, this experience shows that despite the offer of the monetary incentive, there is an upper bound to the distance that individuals are willing to travel to learn their results, suggesting that distance and transactional costs may be the strongest contributing factors to low rates of obtaining results.

Theoretically, those who are most uncertain of their HIV status would expect to gain the most from learning their results. In contrast, for those who are most certain of their status, there is little benefit, because they receive no new information in expectation. For example, 17 percent of respondents reported

[26] For example, although 57 percent of adolescents reported ever having sex, only 15 percent were married.
[27] A similar estimate can be made with the reported minutes needed to travel to the VCT center and imputing time for those not attending, with the nearest neighbors' time: each additional hour needed to travel reduces the likelihood of attendance by 4.1 percentage points (standard error 0.011; not shown).

having a previous test for HIV; these individuals were 3.9 percentage points less likely to attend the results centers (significant at the 0.10 percent level, standard error 0.024; regression not shown)[28]. On the other hand, there were no significant differences in attendance between individuals having different subjective probabilities of infection (not shown)[29]. One reason why attendance did not vary with prior beliefs is that respondents' priors may be unreliable. There is some evidence of this. Respondents had a tendency to over-estimate their own likelihood of HIV infection (Anglewicz and Kohler 2005; Bignami-Van Assche, Anglewicz and Chao 2005) and also over-estimated the total HIV prevalence in their communities – when asked "out of 10 people in your community, how many do you think are HIV positive?," respondents estimated that on average, 40 percent of the adults were infected (not shown)[30].

Receiving a voucher may have provided justification for some individuals to attend the center. For example, because of historical gender restrictions within Islam, women in Balaka may have more restriction on their travel, preventing them from easily attending the results centers[31]. In Balaka, men receiving no incentive were significantly more likely to attend the center than women receiving no incentive. However, men and women receiving an incentive are equally likely to attend, closing the gender gap of attendance in Balaka (Table 3, Column 1). In Mchinji and Rumphi there were no differential impacts of the incentive on attendance between men and women.

For those who have never had sex, there is little reason to attend the VCT center except to redeem the monetary voucher. Although not statistically significant, the impact of receiving an incentive is much more elastic among unmarried respondents who report never having had sex – those never having sex who received an incentive were 7.6 percentage points more likely to attend than those who ever had sex who received an incentive (regression not shown). Monetary incentives were especially important for

---

[28] Only 54 percent of those that were previously tested reported actually having learned their HIV results. There was no significant difference in attendance between those who previously knew and did now know their status.
[29] Individuals were asked "what is the chance that you are infected with HIV?" Possible answers were "no likelihood," "some likelihood," "high likelihood," and "don't know."
[30] When asked the prevalence rate out of 1,000 tested individuals, the estimated prevalence rate was 22 percent. Framing effects are likely to have affected the responses.
[31] In Balaka, 81 percent of the respondents are Muslim as opposed to less than one percent in Mchinji and Rumphi. Women in Balaka may also be less independent: in 2001, only ten percent of Balaka women reported going to the health center without their husband's permission as opposed to 22 percent in the other districts.

those living further from the VCT center: receiving an incentive increased attendance by 28.8 percentage points among those living over one kilometer away (versus 22.8 for those living less than one kilometer from the center; Table 3, Column 2). This can also be seen in Figure 3, Panel B that graphs the impact of distance on attendance among those receiving any incentive and those receiving no incentive[32]. HIV positives receiving an incentive were 6.2 percentage points less likely to attend than HIV negatives receiving an incentive (Table 3, Column 3). There was also an effect of being HIV positive and living further away from the VCT center: HIV positives living over one kilometer from the center were 18.7 percentage points less likely to attend than HIV negatives living over one kilometer, perhaps because they were more likely to be sick, making travel more costly (Table 3, Column 4).

Offering monetary rewards and reducing the distance needed to travel had large effects on the decision to learn HIV results. One woman's remark, overheard by an interviewer, is especially illustrative:

> "Those who were lucky were picking vouchers with some figures. They were courageous to go and check their tests results because they were also receiving money. I got a zero and did not even go and check the results because I knew that there was nothing for me there[33]."

These findings re-emphasize the importance of economic costs such as travel or opportunity costs in decision-making.

3.3 Impact of Neighbors' and Spouses' Attendance

Neighbors' and spouses' attendance could theoretically have either positive or negative effects on others' attendance. There is anecdotal evidence of variation across regions and communities in the acceptability of HIV testing throughout Africa and it is possible that strategic complementarities could create these multiple equilibria. Given existing literature on the different roles of social networks among men and women, it is also likely that the effects of neighbors' and spouses' attendance vary by gender (Moore

[32] Another way to think about the interaction between distance and money is to calculate how much must be paid to compensate for distance. By dividing the estimated coefficient of the effect of distance on attendance by the coefficient of the total incentive, one addition kilometer from the VCT center is roughly equivalent to between \$0.12/km and \$0.22/km (not including and including the indicator of receiving any incentive, respectively).
[33] Author's fieldnotes, December, 2004.

1990). To test this, I identify all non-spousal neighbors living within 0.2-kilometer radii bands of each respondent who tested for HIV, using household location coordinates. On average, each radius band contained between 8 and 12 neighbors who tested. I estimate:

$$(2)\ \text{Got Results}_{\text{ij}} = \alpha + \beta_1 \text{Any}_{\text{ij}} + \beta_2 \text{Amount}_{\text{ij}} + \beta_3 \text{Distance}_{\text{ij}} + \beta_4 \text{Distance}^2{}_{\text{ij}} + \sum_r \beta_r \cdot N_{rij} + X'_{ij}\mu + \varepsilon_{ij}$$

N is equal to the fraction of tested neighbors who attended the VCT center living within 0.2 kilometer, mutually exclusive radius bands ‘r’ from respondent ‘i's’ household, where r = {(0 – 0.2), (0.2 – 0.4), (0.4 – 0.6)}[34]. I instrument N by an indicator of neighbors being assigned any incentive and the average amount of incentive of neighbors’ living within each radius band[35].

Table 4 shows the OLS and IV estimates of the effect of neighbors’ attendance on respondents’ attendance at the results centers for men and women. There is a significant effect of neighbors living within 0.2 kilometers attending the center on women’s attendance, and no effect on men’s attendance (Table 4, Columns 2, 4). There are no significant effects of neighbors’ attendance living further than 0.2 kilometers away. The strong impact of neighbors on women’s attendance to the results centers may be due to additional support received from close neighbors – that are likely to be next of kin, reducing external or internal psychological costs of attending. There may also be economies of scale of traveling to the clinic that are especially important for women.

To measure the impact of spouses’ attendance, I restrict the sample to married couples where both spouses agreed to be tested. A husband’s attendance had a small effect on his wife’s attendance (Table 5, Column 2). However, a wife’s attendance had a large effect on husbands, increasing the likelihood of attendance by 35 percentage points. One possible explanation for the differential impacts of spouses’ incentives is that wives may be able to influence their husbands to attend the VCT center. In this setting, it is also likely that men feel the need to escort their wives to the center.

---

[34] Recall that in 1998 approximately one in four households were randomly sampled and it is reasonable to expect that the households that tested for HIV are evenly distributed with density roughly proportional to all the households within the sample villages.

[35] Miguel and Kremer (2004) measure externalities using similar methodology. The correlation between average neighbors’ and own distance to centers is 0.22, 0.18, 0.30 and between neighbors’ incentives and own incentives is 0.02, 0.03, and 0.04 for radius bands of 0.2, 0.4, and 0.6 respectively. Because neighbors’ distance may not satisfy the exclusion restrictions for the IV specification, I do not include average neighbors’ distance as an instrument.

In sum, both monetary incentives and distance had large impacts on learning HIV results. Many have suggested that psychological barriers, such as fear or stigma, play a large role in deterring individuals from testing. I find, however, that these costs, if they exist at all, can be easily overcome by offering small cash rewards. Incentives may compensate individuals directly for the costs of learning their results or serve as justification for attending a results center, indirectly reducing psychological costs. Moreover, the suggestive evidence of positive strategic complementarities of others' attendance implies even larger effects of offering incentives.

4 Impact of Learning HIV Status

4.1 Theoretical and Measurement Considerations

Meta-analyses examining the effects of HIV testing on sexual behavior have been inconclusive. For example, one review summed up the findings of 35 studies by noting "there is no question that HIV testing can and does motivate behavioral change in some individuals, but testing does not always lead to changes and does not have the same effect in all populations" (Higgins et al. 1991; Wolitski et al. 1997). Several studies indicate little to no behavioral differences among those learning they are HIV negative and there has been some evidence that discordant couples may reduce risky sexual practices after testing (Coates et al. 2000; Wolitski et al. 1997; Weinhardt 1999).

In all previous studies, selection bias of who chooses to learn their results poses a challenge for estimating the impact of knowing one's HIV status. In this study, however, the randomly assigned distances and incentives serve as exogenous instruments for knowledge of status. Nevertheless, there are several other estimation challenges. The follow-up survey was conducted two months after results were available – this time period may not have been long enough for the knowledge of HIV status to have affected sexual behavior. Future rounds of data collection and re-testing in June 2006 will provide another opportunity to measure longer-run behavior impacts of learning HIV results, as well as more detailed measures of sexual behavior.

Another challenge is that sexual behavior is difficult to measure and self-reports may be unreliable. Sexual behavior such as number of sexual partners is likely to be under-reported (Meyer et al. 2004), and contraceptive use has been found to be over-reported. One study of an urban clinic in Lusaka, Zambia, found that 50 percent of women reporting that they had always used condoms prior to a survey had semen detected on vaginal samples (Allen et al. 2003). Because of the potential bias of self-reports, the primary outcome variable used to measure the demand for safe sex in this analysis is the actual purchase of condoms from survey interviewers. Also, to mitigate potential reporting bias during follow-up interviews, every effort was made to conduct interviews privately with an interviewer of the same sex as the respondent, who had no part in the HIV testing or giving results.

It is important to keep in mind that condom purchases may not reflect the true demand for safe sex. If knowledge of HIV status increases abstinence, the demand for condoms could fall. The demand for condoms will also differ among those with a sexual partner and those without, and the impact of learning HIV status on condom purchases is likely to depend on this partnership status. For this reason, each empirical specification distinguishes between those with and without a sexual partner by including an interaction term or looking at each sample separately. Also, because partnership may be endogenous to learning HIV results (Reniers, 2005), I first present results for those who were sexually active at the time of the baseline survey (before testing) as well as among those that had sex at the time of the follow-up survey (after results were available). It is worth noting, however, that there were no significant changes in marriage or partnership attributable to learning HIV positive or negative status (not shown).

In a standard expected utility framework, individuals do not receive any additional utility from learning their HIV status – testing is useful only to the extent that it provides new information that can be used for updating behavior such as making optimal decisions related to sexual behavior, seeking health care, or planning for the future[36]. However, it is theoretically ambiguous how the knowledge of HIV status impacts sexual behavior, and in particular, the demand for condoms. For those diagnosed HIV positive, the direct benefits of using condoms fall because (if safe sex is costly) there is no longer any need of protection. However, HIV positive individuals who are sufficiently altruistic may exhibit a higher

[36] For a formal model of testing see Boozer and Philipson (2000).

demand for condoms after learning their status. On the other hand, if infected individuals behave selfishly, there may be a decrease in the demand for condoms (Mechoulan 2004). For HIV negatives, the benefit from using condoms increases after diagnosis; however, there is no longer any need to protect a sexual partner and so condom use may fall[37].

4.2 Summary Statistics of Sexual Activity and Condom Use

In the baseline 2004 survey, 77.4 percent reported having had sex during the previous year and 62 percent of the follow-up sample reported having had sex during the previous two months (Table 1, Panels B and D). Note that of those reporting having sex, 88 percent, were married. Since only four percent of respondents reported having more than one sexual partner during the two months after VCT and there is no statistical impact of learning HIV status on this reported number; the analysis of number of sexual partners is omitted from this analysis[38].

During the 2004 main survey, 21.0 percent of all respondents reported using a condom with a sexual partner during the year. At the time of the follow-up survey, respondents were asked if they had purchased condoms at any time between VCT and the follow-up interviews: only 8.4 percent of the sample reported purchasing condoms during this time. In terms of the subsidized condoms that were offered at the end of the follow-up interview, 24 percent purchased at least one; of those that purchased any, the average number purchased was 3.7 (Table 1, Panel D). Men were more likely to purchase than women and also bought more when purchasing. Married respondents were equally likely as unmarried respondents to purchase condoms, however, there is a difference by reported sexual activity: 28 percent of those who had sex purchased condoms as opposed to 17 percent of those who reported not having sex[39].

---

[37] Individuals may also update their priors, believing they have a lower likelihood of future infection, resulting in no change after learning HIV results.

[38] Number of partners is important in determining the overall prevalence rate (Kremer 1996); this will be included in future rounds of data collection.

[39] The results in this paper do not take into account the frequency of condom use. For example, of those that reported ever using a condom with their last partner in the baseline survey, 22. 2 percent reported using a condom only at the beginning of the relationship, 47. 8 percent reported using a condom sometimes, 16.3 percent reported using condoms almost every time, and only 5.5 reported using condoms all the time with their partner. This will be studied with future data collection.

4.3 Receiving HIV Positive Diagnoses

I first examine the effects of receiving an HIV positive diagnosis on condom purchases and sexual behavior among those that reported having sex at the time of the baseline survey (85 percent of the HIV positives). Panel A of Figure 4 presents the percent purchasing condoms among those that knew and did not know their HIV status. Approximately half who learned they were infected purchased condoms. This figure illustrates the following OLS regression:

$$\text{(3)} \qquad \mathrm{Y_{ij}} = \alpha + \beta \text{ Got Results}_{\mathrm{ij}} + X'_{ij}\mu + \varepsilon_{ij}$$

Y indicates sexual behavior at the time of the follow-up survey (including if the respondent purchased condoms, the total number of condoms purchased, or if the respondent reported having sex), and "Got Results" indicates knowledge of HIV status. The fact that individuals choose to learn their HIV status means that OLS estimates are likely to be biased, but estimating the effects of knowing HIV status with exogenous instruments provides unbiased estimates. In particular, I instrument "Got Results" with being offered any incentive, the amount of the incentive, and living over one kilometer from the assigned center. To account for differential effects among men and women I also include interactions with gender and predict getting results[40]:

$$\text{(4)} \qquad \begin{aligned} \text{Got Results}_{\mathrm{ij}} &= \alpha + \beta_1 \text{Any}_{\mathrm{ij}} + \beta_2 \text{Amount}_{\mathrm{ij}} + \beta_3 \text{Over 1 km}_{\mathrm{ij}} + \\ &\beta_4(\text{Any}_{\mathrm{ij}} \cdot \text{Male}_{\mathrm{ij}}) + \beta_5(\text{Amount}_{\mathrm{ij}} \cdot \text{Male}_{\mathrm{ij}}) + \beta_6(\text{Over 1 km}_{\mathrm{ij}} \cdot \text{Male}_{\mathrm{ij}}) + X_{ij}\mu + \varepsilon_{ij} \end{aligned}$$

Covariates, X, include age, age-squared, a dummy for male, simulated average distance to the VCT, and district dummies. Because the monetary incentives and distance were both exogenously assigned to each respondent, they are uncorrelated with the error term. In the analysis, although the measure of purchasing condoms is binary, estimates do not differ greatly from a probit model and I use a linear model to estimate (3). The first stage of learning a positive status is strong with an F-statistic equal to 21.62 (Appendix C). It is important to point out that the IV estimates in (3) are local average treatment effects (LATE), which estimates the effect of knowing HIV status among "compliers", or those for whom respond to incentives

[40] Not including gender interactions reduces the first stage F-statistic to 3.6 (from 21.6).

and distance. This estimate is not necessarily equal to the average treatment effect (ATE), which is what we would want to estimate to know the effects of telling an average person their HIV status. Also, while I account for heterogeneous treatment effects by gender, there may be other differential effects of the incentive and distance that are not included in the IV analysis (see Imbens and Angrist (1994) and Heckman, Smith, and Clements (1997)).

Among those with a sexual partner at the time of the baseline 2004 survey, learning HIV positive results significantly increased the likelihood of purchasing condoms – by 28 percentage points, a three-fold increase compared to those that did not learn their results (significant at 0.07 percent, Table 6, Panel A, Column 2). This result is robust to using the total number of condoms purchased as an outcome, where the average number of condoms purchased increased by 1.8 condoms (Table 6, Panel A, Column 4).

Recall that the condoms sold during the follow-up study were subsidized in price and in the cost of travel to obtain the condoms. However, there is also a positive coefficient on reported purchases of non-subsidized condoms during the two months between receiving an HIV positive diagnosis and the follow-up survey (Table 6, Panel A, Column 6). Although statistically insignificant, the 90 percent confidence interval lies between -0.04 and + 0.22. There is no significant effect of learning HIV status on reported sexual activity (Table 6, Panel A, Column 8).

Of the 71 HIV positives that had sex during 2004 (reported at the baseline survey), 50 also reported having sex in the two months after VCT. Among these individuals who learned their HIV results, there was a 59 percentage point increase in the likelihood of purchasing condoms (Table 6, Panel B, Column 2) and an increase of 2.5 condoms purchased (Table 6, Panel B, Column 4). The point estimate on reported purchases of non-subsidized condoms is positive but insignificant (Table 6, Panel B, Column 6). To some extent, the estimates in Panel B are endogenous because they condition on having had sex at the time of the follow-up survey, however, these estimates are relevant for addressing the rate of condom purchases among a currently sexually active population.

The OLS estimates of learning HIV status on condom purchases are slightly larger than the IV estimates, implying that omitted variables affecting the decision to learn HIV results are likely to be positively correlated with purchasing condoms (Panels A and B, Columns 1 and 2). This occurs, for example, if "safe" individuals are more likely to purchase condoms and learn their HIV status, implying that studies not taking into account selection bias will overestimate the true impact of learning HIV results on later behavior and attitudes.

It is possible that HIV positives only purchased condoms out of guilt, believing (incorrectly) that the interviewer knows their status. In this case they may have only purchased one condom as a token, keeping the remaining money. However, only two HIV positives purchased just one condom; omitting these individuals or coding their purchases as zero does not affect any of the results; although impossible to rule out, this suggests that guilt or shame may not have been a large factor in the observed increase in condom sales.

Thus, it appears that individuals learning their HIV positive status incur private costs to protect their sexual partners. It is important to note that the effects are greatest among those that are sexual active at the time they were offered condoms. There is no evidence that sexual intercourse increased or decreased after learning HIV status.

4.4 Receiving HIV Negative Diagnoses

In contrast to HIV positive individuals, there were no significant effects of receiving an HIV *negative* diagnosis on purchasing condoms, reported purchases of non-subsidized condoms, or reported sexual intercourse. Among those who had sex at baseline, Panel B of Figure 4 illustrates no discernable difference between those knowing and not knowing their HIV status in the likelihood of purchasing condoms. In a regression framework I estimate:

$$(5) \qquad Y_{ij} = \alpha + \beta_1 \text{Got Results}_{ij} + \beta_2 \text{Had Sex}_{ij} + \beta_3 (\text{Got Results}_{ij} \cdot \text{Had Sex}_{ij}) + X'_{ij}\mu + \varepsilon_{ij}$$

I continue to use the same set of instruments as in (4) above, except that I also include the interaction of having had sex (either at baseline or at the follow-up) with the set of instruments.

Row one of Table 7 shows that there were no significant effects of knowing results on any measure of sexual behavior among those who did not have sex at baseline (Table 7, Panel A, Columns 2, 4, 6, and 8). Moreover, the interaction term between knowing HIV status and having sex is not significant, indicating no significant impact among those that reported having sex at the baseline survey. However, the standard errors of the IV estimates are large, making it impossible to reject zero impact of learning HIV status. There were also no significant effects among those with a sexual partner at the time of the follow-up survey and unlike the HIV positives, where magnitudes of coefficients increased among the sample of those having sex at the follow-up, the coefficients among the HIV negatives of this sample tend towards zero (Table 7, Panel B).

Table 8 presents the impact of learning HIV status among both HIV positives and negatives, separately for those that had sex and those that did not. Among those that had sex (either at baseline, or at the follow-up), there was a large positive increase in condom purchases after receiving an HIV positive diagnosis (Table 8, Panels A and B, Columns 1 – 3). There are no significant effects among those learning an HIV negative status or those that did not have sex at the baseline or follow-up surveys (Panel A, Columns 5 – 8; Panel B, Columns 4 – 6). These estimates are used in calibrations in section 5.

It is useful to note that there were no differential effects of learning HIV status on sexual behavior among other observable respondent characteristics. For example, although men on average were more likely to purchase condoms than women, there were no differences in the impact of learning HIV status on purchases by gender. There were also no differences by age, or ever having gone to school. Learning HIV results (either positive or negative) did not significantly affect having discussions with friends or spouses about condoms or AIDS. Respondents were also asked about their attitudes towards condoms[41]. These attitudes were strong determinants of purchasing condoms, for example, those that "agreed" that condoms are acceptable to use with a spouse were twice more likely to purchase condoms than those that "disagree." However, there were no effects of receiving either a positive or negative diagnosis on attitudes towards condoms among either HIV positives or negatives.

[41] In particular, respondents were asked if they agree, disagree, or don't know whether it is acceptable to use condoms with a spouse or sexual partner to protect against HIV; if friends say to always use condoms; if condoms prevent pregnancy; if condoms fully protect from HIV; if condoms feel bad, break or slip; or if it is embarrassing to purchase or put on condoms.

In sum, HIV positive individuals who learned their status were more likely to purchase condoms. However, the increase in the demand for condoms was only among a small proportion of the sample – there were virtually no effects among HIV negatives or those that were not sexually active. Also, there were no significant differences in reported purchases of non-subsidized condoms or in reported sexual activity among all respondents. The large effects that are claimed to be the "single most influential driver for behavior change" (Global Business Coalition 2005), are not detected within these data. Neither the knowledge of HIV status nor the personal attention and education from the nurses to practice safe sex during the VCT counseling sessions appear to have had significantly large impacts on sexual behavior after two months.

4.5 Robustness and Discussion

An important consideration is whether respondents believed the diagnoses they received from the nurses. If it were the case that the diagnoses were not viewed as credible, there would be little reason to believe that hearing results should impact behavior. However, by comparing respondents' belief of their likelihood of infection before and after learning results there is evidence that individuals updated their priors, suggesting that respondents did, in fact, believe their results. Before being tested, 48 percent of HIV negative respondents thought there was no likelihood they were infected, 22 percent thought the likelihood was low, five percent thought there was a high likelihood, and 26 percent said they did not know (Table 9, Panel A, Column 1).

During the follow-up survey, respondents were again asked about their beliefs of being infected. Of those that heard their negative results, 88 percent believed that there was no likelihood of being infected, while only 50 percent of those that did not learn their negative results thought that there was no likelihood (Table 9, Columns 2 and 3). Column 4 presents the differences of average responses for each category, among those that did and did not learn their results. Receiving an HIV negative diagnosis significantly reduced the likelihood of believing there was a chance of being infected.

Among the HIV positives, before being tested, 31 percent believed that there was no likelihood of infection, 29 percent thought a low likelihood, 7 percent thought a high likelihood, and 33 percent said they did not know (Table 9, Panel B, Column 1). This indicates that HIV positives who learn their status also update their beliefs, although the differences are not statistically significant (Panel B, Column 4).

Another argument as to why there might be no measured effect of learning HIV status on sexual behavior is that only individuals who are surprised by their HIV results should respond to the information. Using data from unmarried respondents interviewed and tested in San Francisco during 1988 and 1989, Boozer and Philipson (2000) find asymmetric results: those who thought they were at risk and were diagnosed HIV negative increased sexual contact by 20 percent; those who thought they were not at risk but were diagnosed HIV positive, decreased sexual contact by 50 percent. Their findings give important theoretical insights as to who benefits from the information provided by HIV testing and potential asymmetric behavior among those with different prior beliefs. Using the same measure of likelihood of infection as Boozer and Philipson, I find no significant relationship between prior beliefs and sexual behavior, or interaction effects with priors and learning HIV status (not shown)[42].

Another important consideration is the fact that many respondents were testing with their spouse. Theoretically, if one spouse learns he or she is HIV negative and the other does not know, condom use may increase to protect the HIV negative individual from contracting the disease. However, if both spouses learn they are HIV negative, condom use is likely to decrease. On the other hand, it has been suggested that testing couples together may be more effective in promoting safe sexual behavior than testing individuals alone and some organizations have begun emphasizing couple counseling, even considering making it mandatory before marriage (Painter 2001, Nyathi 2003). However, when I estimate the effects of having one or both spouses learn their HIV status, I find no difference in the likelihood of purchasing condoms, either when both are HIV negative or when at least one is HIV positive (Appendix D). Contrary to conventional views within public policy, there appears to be no additional impact of both

[42] As mentioned above, it is possible that respondents have uncertain priors and measuring respondents' confidence of their priors is difficult. Also the respondents in the Boozer and Philipson study are quite different than those in this current study, and are likely to have different distributions of priors. Most importantly, this comparison is difficult to make without a measure of the *change* in condom purchases.

partners learning their status on demand for condoms[43].

The finding that few individuals change their sexual behavior in response to the knowledge of their HIV status is consistent with the view that many individuals place little value on this information or are indifferent to learning their status. It is not clear why the value of this information would be so low as to not promote behavior change, especially since HIV is such a deadly disease and the expected returns to practicing safe sex should be high. It is also important to note that these analyses are limited to examining the impact of learning HIV status on condom purchases – future data collection in summer 2006 will include testing respondents again for HIV, providing further data to measure potential long-term effects.

5 Cost-Effectiveness and Calibration

The results in this study indicate that small financial incentives increase the knowledge of HIV status after being tested and that those diagnosed HIV positive are more likely to purchase condoms if they have a spouse or sexual partner. The fact that HIV positives are willing to incur private costs to protect sexual partners from infection suggests that testing and offering incentives might be a promising strategy to prevent new HIV infections. However, testing is costly, and for such a program to succeed it may be necessary to subsidize individuals' private costs of testing and receiving results. Recall that this particular program tested individuals in their homes, set up results centers within rural villages, and sold condoms only after giving money to individuals to purchase them. While these subsidies and incentives increased participation rates, they also resulted in higher total project costs. Because individuals do not internalize the negative externalities of high HIV prevalence rates, it may be necessary for governments to bear these costs. Thus, it is important to establish whether this strategy might be cost-effective.

To calculate the cost-effectiveness, I compare the incremental costs of testing, offering monetary incentives, and selling subsidized condoms, to the expected benefits as measured by the number of HIV

[43] To estimate this I instrument whether a respondent knows her status, if her spouse knows his status, and an interaction term indicating whether both know their status. In addition to the instruments in (4) above, I also include an indicator whether the respondent's spouse received an incentive, the total value of the spouse's incentive, and interaction terms of respondent's and spouse's incentives. It is important to note that the impact of spouses' information relies on the degree to which spouses disclose their HIV results to each other. In this study, of those diagnosed HIV negative, 95 percent report telling their spouse of their status, while 82 percent of those diagnosed HIV positive report sharing the information with their spouse.

infections averted. The costs are obtained from the actual project costs in Malawi, excluding any research-related expenses; for sensitivity analyses I also use costs from other less-expensive testing programs. I calculate expected infections averted for each intervention by estimating a simple probability-based model of infection using sexual behavior and transmission parameters from the data and the epidemiological literature. Because of the limited data, these calculations rely on several simplifying assumptions – I thus allow each parameter to take on a range of values, and run a Monte Carlo simulation of 1,000 independent draws to estimate cost-effectiveness confidence intervals (See Appendix E for further details).

Given the ranges of costs per respondent and simulated infection rates for each program, the marginal cost, infections averted, and cost per infection averted, are presented in Table 10. This is calculated for 10,000 people over one year and separately calibrates estimates with and without accounting for effects of learning HIV status on purchasing non-subsidized condoms. The most optimistic estimate assumes that there were differential effects of learning HIV results on both purchasing subsidized condoms and on reported purchases of non-subsidized condoms (Table 10, Column 2). In this case, by introducing all programs, the largest estimated number of infections, 23, could be averted, with an average cost per infection averted of $26,904. Testing on its own averts an average of 8 infections at a cost of $87,108 per infection. Selling condoms averts the least infections, but because it is not as costly as testing, it is more cost-effective at $10,650 dollars per infection averted. Lastly, adding incentives to a program averts an estimated 11 additional infections on average, at a cost of $1,136 per infection. The standard errors for all cost-effectiveness estimates are relatively small which gives reasonably tight 95 percent confidence intervals[44].

[44] Disability adjusted life years (DALY's) are used to compare the benefits of health interventions and are calculated by estimating the expected number of years of disability and death are prevented through averting infections. One HIV infection is equivalent to approximately 21.7 DALY's. This uses the average age in the main sample (33.5 years), life tables and demographic data from the region; I also assume an average of 8 years from infection until the onset of AIDS and an additional year until death, disability weights for HIV and AIDS as 0.123 and 0.505 respectively, and a discount rate of 3% (U.S. Census Bureau 1999; Homedes 2000; Sweat et al. 2000). Thus the equivalent cost per DALY saved in Table 10, Column 2 is $52.4 for vouchers, $490.8 for condoms, and $4,014.2 for testing alone.

Cost-effectiveness estimates are driven by increases in purchases of subsidized and non-subsidized condoms by HIV positive individuals who learned their HIV status. However, recall that the IV estimates of the impact of knowing HIV status on reported purchases of non-subsidized condoms, although positive, were not statistically significant (Tables 5, 7). If all respondents, regardless of their knowledge of their HIV status, were equally as likely to purchase non-subsidized condoms, implementing all three programs would only avert a total of five infections. Testing alone would avert no additional infections, making the cost per infection infinite (Table 10, Column 3).

It is important to note that the estimates in Table 10 are based on the sample reporting having sex at the *follow-up survey* (Table 8, Panel B). If the sample having sex at the *baseline survey* had been used to estimate the effects of learning HIV status, the programs would be even less effective. In that case, the estimated average cost per infection averted by offering incentives is $1,697, selling condoms is $10,896, testing is $181,235, and implementing all programs is $44,883.

Other interventions may be much more effective in preventing the spread of HIV. For example, an evaluation of a randomized trial in Mwanza, Tanzania estimated that treating sexually transmitted diseases had an incremental costs of $217.62 per HIV infection averted (Gilson et al. 1997). In simulations of the effect of treating sexually transmitted diseases, Oster (2005b) estimates a cost of $78.24 per infection averted. Other types of interventions may also be effective strategies for HIV prevention – for example, improving blood supply safety ($172; Winsbury 1995), preventing mother to child transmission ($298 – $506; Marseille et al. 1999), or circumcision (Auvert 2005)[45].

Part of the reason for high rates of participation was due to the in-home tests and the close proximity of the results center, but this also increases costs. An alternative strategy might be to bring mobile VCT centers into villages and use rapid tests, which has been successful in other countries (Mutevedzi 2002; Morin and Sakutukwa, 2002). Another strategy to reduce program costs is to lower the

[45] These results differ from those found in Sweat et al. (2000), who estimated that HIV testing in Kenya and Tanzania resulted in a cost per infection averted of $249 and $346 respectively. The difference is likely to be due to selection and reporting bias in their study.

amount of the voucher – recall that the total voucher amount was irrelevant for overall VCT attendance[46]. Also, targeting high-risk populations such as sex workers is likely to be more cost-effective.

In sum, these results suggest that given reasonable ranges of epidemiological and behavioral parameters, and the estimates on condom use from this experiment, population-based testing is *not* as cost-effective as other strategies for averting HIV infections. Even in most favorable specification, the highest number of infections averted by testing within the 95 percent confidence interval is 11 infections, after a one-year program targeting 10,000 people (Table 10, column 2). It should also be noted that purchases of subsidized condoms were only made after respondents were given 30 cents – without this gift, condom sales would be even lower. Also, as parameters values indicate lower levels of risky sex, cost-effectiveness decreases. This means that if the HIV/AIDS epidemic declines, testing and counseling will become even less cost-effective.

6 Conclusion

This paper is the first to randomize the private benefits and costs that individuals face when deciding to learn their HIV status. Using this randomization, I estimate the elasticity of demand for learning HIV status and the impact of learning HIV results on subsequent sexual behavior. There are three main findings of this study. First, a monetary incentive of less than a tenth of a day's wage doubled the rate of tested individuals learning their HIV results; distance also had a strong negative impact on attendance to randomly placed results centers. Second, among HIV positive individuals with a sexual partner, those who learned their status exhibited a higher demand for condoms than those who did not know their status, supporting the view that individuals are willing to bear the costs of safe sex in order to protect sexual partners. Third, because the magnitudes of the effects of learning HIV status were relatively small, the overall benefit to testing was low in terms of total infections averted. It should be noted that estimates of infections averted are based only on condom purchases after learning HIV results – other behaviors and

[46] Lowering the voucher amount to $0.20 with the most optimistic calibration would reduce the cost per infection averted of offering incentives to $381. On the other hand, vouchers in the form of travel compensation may not be as effective. Recall that results centers located far from respondents' homes (e.g., over 10 km) had no attendance despite large incentives that were offered. Another strategy would be to reduce the total cost of testing to say $8 per respondent, in which case the cost per infection averted for testing would be $24,005.

actual HIV status will be included in the analysis as future data becomes available.

This study contradicts widely held views that large psychological or stigma-related costs are barriers to learning HIV results. One organization, writing about individuals traveling to HIV results centers in Zimbabwe stated: "the cost of stigma is quite high [to go to centers to be tested for HIV], more so than the bus fare to town" (Gunduza, 2002). The evidence from this experiment in Malawi provides contrary evidence. Such psychological barriers, if they exist, can easily and inexpensively be overcome. Cash incentives may directly compensate for psychological costs of learning HIV results or indirectly reduce external psychological costs, such as stigma, by providing individuals with an excuse to attend the results center. Rather than expensive campaigns to reduce perceptions of stigma, simply reducing the opportunity costs of attendance by offering subsidies or increasing the accessibility of clinics may be effective strategies for increasing uptake. It is important to keep in mind, however, that these results may differ in settings with higher prevalence rates or under different circumstances.

There may also be scope for offering subsidies or incentives for other positive health behavior, such as remaining HIV negative, adhering to antiretroviral therapy, getting circumcised, attending prenatal clinics (Dupas 2005), or being tested and treated for other sexually transmitted diseases (Oster 2005). This is the subject of future research.

Testing had no impact on HIV negatives, or those without a sexual partner, despite lengthy pre- and post-test counseling sessions with nurses, education about safe sex, and offers of subsidized condoms. Given the findings in this study, other prevention programs should be explored before investing into costly testing programs. However, if governments or organizations choose to invest in testing programs, or if a testing program is already in place, offering small monetary rewards may help to increase uptake and return rates. Because there are small positive behavioral effects on HIV positives learning their status, inexpensive boosts in participation may help to increase the effectiveness of such programs.

It is important to revisit and challenge previous assumptions about HIV testing and sexual behavior. With rigorous empirical evidence and a better understanding of behavior in Africa, policies may be more accurately and effectively designed to reduce the spread of HIV.

References

[1] Allen, Susan, Jareen Meinzen-Derr, Michele Kautzman, Isaac Zulu, Stanley Trask, Ulgen Fideli, Rosemary Musonda, Francis Kasolo, Feng Gao, Alan Haworth. "Sexual Behavior of HIV Discordant Couples after HIV Counseling and Testing." *AIDS*, 2003, 17 (5), pp. 733-740.

[2] Anglewicz, Philip, and Hans-Peter Kohler. "The Construction of Self-Assessed Likelihood HIV/AIDS Infection: An Examination from Rural Malawi." Paper read at the Meeting of the International Union for the Study of Population, Tours, France, July, 2005, pp. 18-23.

[3] Anglewicz, Philip, Simona Bignami-Van Assche, Li-Wei Chao, David Chilongozi, Irving Hoffman, Hans-Peter Kohler, Francis Martinson, Francis Onyango, Georges Reniers, and Kirsten Smith. "HIV/STI Testing in the 2004 Malawi Diffusion and Ideational Change Project: Lessons Learned." Social Networks Project Working Papers. Philadelphia: University of Pennsylvania, 2004.

[4] Auvert, Bertran, Dirk Taljaard, Emmanuel Lagarde, Joelle Sobngwi-Tambekou, Remi Sitta, and Adrian Puren. "Randomized, Controlled Intervention Trial of Male Circumcision for Reduction of HIV Infection Risk: The ANRS 1265 Trial", *PLOS Medicine*, Nov, 2005, 2 (11), e298.

[5] Baggaley, Rachel, Michael Kelly, Sonja Weinreich, Ignatius Kayawe, George Phiri, W. Mulongo, and M. Phiri. "HIV counseling and testing in Zambia: the Kara Counseling Experience." *South African AIDS News*, 1998, 6 (2), pp. 2-8.

[6] Bignami-Van Assche, Simona, Philip Anglewick, and Li-Wei Chao. "Validity of Self-Reports of HIV/AIDS and Sexually Transmitted Infections in Rural Malawi." Paper read at the Meeting of the International Union for the Study of Population, Tours, France, July, 2005, pp. 18-23.

[7] Bignami, Simona, Reniers Georges and Weinreb Alex. "An Assessment of the KDICP and MDICP Data Quality: Interviewer Effects, Question Reliability and Sample Attrition." *Demographic Research*, 2003, Special Collection 1, Article 2.

[8] Bignami-Van Assche, Simona, Kirsten Smith, Georges Reniers, Philip Anglewicz, Rebecca Thornton, Li-Wei Chao, Alex Weinreb, Susan Cotts Watkins, Irving Hoffman, and the MDICP Team. "Protocol for Biomarker Testing In the 2004 Malawi Diffusion and Ideational Change Project." *Social Networks Project Working Papers.* Philadelphia: University of Pennsylvania, 2004.

[9] Boozer, Michael, and Tomas Philipson. "The Impact of Public Testing for Human Immunodeficiency Virus." *The Journal of Human Resources*, 2000, 35 (3), pp. 419-446.

[10] Bouey, Paul, Tobi Saidel, and Thomas Rehle. "AVERT- A Tool for Estimating Intervention Effects On the Reduction of HIV Transmission." USAID and FHI Manual, 1998.

[11] Bracher, Michael, Gigi Santow, and Susan Cotts Watkins. "A Micro Simulation Study on the Effects of Marriage, Divorce and Remarriage on Lifetime Risks of HIV/AIDS in Rural Malawi." Paper Read at Population Association of America, Minneapolis MN, May 1-3, 2003.

[12] Briggs, Andrew and Paul Fenn. "Confidence Intervals or Surfaces? Uncertainty on the Cost Effectiveness Plane." *Health Economics*, 1998, 7(8), pp. 723–740

[13] Camerer, Colin F., George Loewenstein and Matthew Rabin. *Advances in Behavioral Economics*. Princeton: Princeton University Press, 2004.

[14] Caplin, Andrew, and Kfir Eliaz. "AIDS and Psychology: A Mechanism Design Approach." *RAND Journal of Economics*, 2003, 34(4), pp. 631-646.

[15] Caplin, Andrew and John Leahy. "Behavioral Policy." In *Essays in Psychology and Economics*, edited by Isabelle Brocas and Juan Carillo, Oxford University Press, 2003.

[16] Caplin Andrew and John Leahy. "Psychological Expected Utility Theory and Anticipated Feelings." *Quarterly Journal of Economics*, 2001, 116 (1), pp 55-80.

[17] Caplin Andrew and John Leahy. "The Supply of Information by a Concerned Expert." *The Economic*

Journal, 2004, 114 (497), pp 487-505.

[18] Cartoux, Michel, Nicholas Meda, Philippe Van De Perre, Marie Louise Newell, Isabelle de Vincenzi, and Francois Dabis. “Acceptability of Voluntary HIV Testing by Pregnant Women in Developing Countries: An International Survey.” Ghent International Working Group on Mother-To-Child Transmission of HIV. *AIDS*, 1998, 12 (18), pp 2489-2493.

[19] Catania, Joseph, David R. Gibson, Dale D. Chitwood, and Thomas Coates. “Methodological Problems in AIDS Behavioral Research: Influences on Measurement Error and Participation Bias in Studies of Sexual Behavior.” *Psychological Bulletin,* 1990, 108 (3), pp. 339-362.

[20] Census, U. S. Life-Tables: Tanzania, Kenya: US Bureau of Census International Database 1999

[21] Chapoto, Anthony, and Thomas Jayne. “Socio-Economic Characteristics of Individuals Afflicted By AIDS Related Prime-Age Mortality in Zambia.” Paper Read at IFPRI/Renewal Conference on HIV/AIDS and Food and Nutrition Security, Durban South Africa, April, 2005, pp. 14-16.

[22] Coates, Thomas, Olga Grinstead, Steven Gregorich, David Heilbron, William Wolf, Kyung-Hee Choi, Julius Schachter, Peter Scheirer, Ariane Van Der Straten, Munkolekole Claudes Kamenga, Michael Sweat, Isabelle de Zoysa, Gina Dallabetta, Kevin O'Reilly, Eric Van Praag, David Miller, Monica Ruiz, Sam Kalibala, Ben Nkowane, Don Balmer, Francis Kihuho, Steve Moses, Francis Plummer, Gloria Sangiwa, Margaret Hogan, Japhet Killewo, Davis Mwakigile, and Colin Furlonge. “Efficacy of Voluntary HIV-1 Counseling and Testing In Individuals and Couples In Kenya, Tanzania, and Trinidad: A Randomized Trial.” *Lancet,* 2000, 356 (9224), pp. 103-112.

[23] Coulibaly, Djénéba, Philippe Msellati, Seri Dedy, Christiane Welffens-Ekra, and Francois Dabis. “Attitudes and Behavior of Pregnant Women towards HIV Screening in Abidjan (Ivory Coast) in 1995 and 1996.” *Sante*, 1998, 8 (3), pp. 234-8.

[24] Day, J. H., K. Miyamura, A. D. Grant, A. Leeuw, J. Munsamy, R. Baggaley, And G. J. Churchyard. “Attitudes to HIV Voluntary Counselling and Testing among Mineworkers in South Africa: Will Availability of Antiretroviral Therapy Encourage Testing?” *AIDS Care,* 2003, 15 (5), pp. 665-672.

[25] de Graft-Johnson, Joseph., Valerie Paz-Soldan, Antonio Kasote, and Amy Tsui. “HIV Voluntary Counseling and Testing Service Preferences in a Rural Malawi Population.” Mimeo, 2004.

[26] Malawi Demographic and Health Survey, National Statistics Office (Malawi) and ORC MACRO, 2000.

[27] Donnelly, John. “Dire Situation, Drastic Measures: AIDS testing urged for all in ravaged nation.” *Boston Globe*, October 23.

[28] Dupas, Pascaline. “The Impact of Conditional In-Kind Subsidies on Preventive Health Behaviors: Evidence from Western Kenya.” Mimeo, New York University, 2005.

[29] Ekwueme, Donatus, Steven Pinkerton, David Holtgrave, and Bernard Branson. “Cost Comparison of Three HIV Counseling and Testing Technologies.” *American Journal of Preventative Medicine*, 2003, 25 (2), pp. 112-21.

[30] Family Health International. “When Partners Talk, Behavior May Change.” *Network*, 2002, 21 (4).

[31] Fan, Jianqing. “Design-Adaptive Nonparametric Regression.” *Journal of the American Statistical Association*, 1992, 87(420), pp. 998-1004.

[32] Fernandez, M. Isabel, Jose Collazo, Stephen G. Bowen, Leah M. Varga, Nilda Hernandez, and Tatiana Perrino. “Predictors of HIV Testing and Intention to Test among Hispanic Farmworkers in South Florida.” *Journal of Rural Health*, 2005, 21 (1), pp. 56-64.

[33] Ford, Kathleen, Dewa Nyoman Wirawan, Gusti Made Sumantera, Anak Agung Sawitri, and Mandy Stahre. “Voluntary HIV testing, disclosure, and stigma among injection drug users in Bali, Indonesia.” *AIDS Education and Prevention*, 2004, 16 (6), pp. 487-98.

[34] Forsythe, Steven, Gilly Arthur, Gilbert Ngatia, Roselyn Mutemi, Joseph Odhiambo, and Charles Gilks. “Using Economics To Reveal Why Kenyans Do (Or Do Not) Get Tested for HIV.”

International Conference on AIDS 14, Abstract No. E11511, 2002.

[35] Frank, Richard G. “Behavioral Economics and Health Economics.” Paper Prepared for Yrjö Jahnsson Foundation 50th Anniversary Conference on Economic Institutions and Behavioral Economics, June 22-24, 2004.

[36] Gilson, Lucy, Rashid Mkanje, Heiner Grosskurth, Frank Mosha, John Picard, Awena Gavyole, James Todd, Philippe Mayaud, Roland Swai, Lieve Fransen, David Mabey, Anne Mills, Richard Hayes. “Cost-effectiveness of improved treatment services for sexually transmitted diseases in preventing HIV-1 infection in Mwanza Region, Tanzania.” *Lancet*, December 20-27, 1997, 350(9094), pp. 1805-1809.

[37] Ginwalla, S., A. Grant, J. Day, T. Dlova, S. MacIntyre, Rachel Baggaley, and G. Churchyard. “Use of UNAIDS Tools to Evaluate HIV Voluntary Counseling and Testing Services for Mineworkers in South Africa.” *AIDS Care*, 2002, 14 (5), pp.707-726.

[38] Glick, Peter. “Scaling Up HIV Voluntary Counseling and Testing In Africa: What Can Evaluation Studies Tell Us About Potential Prevention Impacts?” *Education Review*, 2005, 29 (4), pp.331-357.

[39] Global Business Coalition. “The Need to Know: Accelerating Access to Testing.” 2005.

[40] Gneezy, Uri and Aldo Rustichini. “Pay Enough or Don’t Pay At All.” *The Quarterly Journal of Economics*, August 2000, 115 (3), pp.791-810.

[41] Gray, Ronald, Maria Wawer, Ron Brookmeyer, Nelson Sewankambo, David Serwadda, Fred Wabwire-Mangen, Tom Lutalo, Xianbin Li, Thomas van Cott, Thomas Quinn, and the Rakai Project Team, “Probability of HIV-1 Transmission Per Coital Act in Monogamous, Heterosexual, HIV-1 Discordant Couples in Rakai, Uganda," *The Lancet*, 2001, 357(9263), pp. 1149-1153.

[42] Gunduza, Emedie. “The Hidden Cost of Voluntary Counseling and Testing for Women.” *Women and AIDS Support Network (WASN).* December 31, 2002.

[43] Heckman, J., J. Smith, and N. Clements. “Making the most out of programme evaluations and social experiments: accounting for heterogeneity in programme impacts,” *Review of Economic Studies*, 1997, 64(4), pp. 487–535.

[44] Higgins, Donna, C. Galavotti, Kevin O'Reilly, D. Schnell, M. Moore, D. Rugg, and R. Johnson. “Evidence for the Effects of HIV Antibody Counseling and Testing On Risk Behaviors.” *Journal of the American Medical Association*, 1991, 266(17), pp. 2419-2429.

[45] HITS-2000 Investigators. “Failure to Return for HIV Test Results Among Persons at High Risk for HIV Infection: Results from A Multi-State Interview Project.” *Journal of Acquired Immune Deficiency Syndrome*, 2004, 35 (5), pp. 511-518.

[46] Holbrooke, Richard, and Richard Furman. “A Global Battle's Missing Weapon.” *New York Times*, Op-ed, 2004, February 10.

[47] Homedes, Nuria. “The Disability-Adjusted Life Year (DALY) Definition, Measurement and Potential Use.” *Human Capital Development and Operations Policy Working Papers,* 2000.

[48] Hutchinson, A. Brian, Giselle Corbie-Smith, Stephen B. Thomas, S. Mohanan, and Carlos del Rio. “Understanding the patient's perspective on rapid and routine HIV testing in an inner-city urgent care center.” *AIDS Education and Prevention,* 2004, 16 (2), pp. 101-14.

[49] Imbens, G. W., and J. D. Angrist. “Identification and Estimation of Local Average Treatment Effects,” *Econometrica*, 1994, 62(2), 467–475.

[50] International Food Policy Research Institute (IFPRI), Complementary Panel Survey, Malawi 2002.

[51] Kalichman, S.C., and L.C. Simbayi. “HIV Testing Attitudes, AIDS Stigma, and Voluntary HIV Counseling and Testing in a Black Township in Cape Town, South Africa.” *Sexually Transmitted Infections,* 2003, 79 (6), pp. 442-447.

[52] Kamega M., R. Ryder and M. Jingu, et al. “Evidence of Marked Sexual Behavior Change Associated

with Low HIV-1 Seroconversion in 149 Married Couples with Discordant HIV-1 Serostatus: Experience at an HIV-Counseling Centre in Zaire. *AIDS*, 1991, 5, pp. 61-67.

[53] www.knowhivaids.org. "about us" 2005.

[54] Koszegi, Botond. "Health Anxiety and Patient Behavior." *Journal of Health Economics,* 2003, 22(6), pp. 1073-1084.

[55] Kremer, Michael. "Incorporating Behavioral Choice into Epidemiological Models of AIDS". *Quarterly Journal of Economics,* 1996, 111(2).

[56] Laver, Susan M. "Voluntary Testing and Counseling for HIV: Are Adults in Rural Communities Ready to Test? A Descriptive Survey." *Central African Journal of Medicine*, 2001, 47 (4), pp. 92-97.

[57] LaFraniere Sharon. "Mandatory Testing Bolsters Botswana In Combating AIDS." *New York Times*, 2004, June 14, A1.

[58] Leibowitz, Arleen, and Stephanie Taylor. "Distance and Time Costs of HIV-Testing." Working Paper Presented at the University of Chicago Demography Workshop, 2004.

[59] Loewenstein, George, "Anticipation and the Valuation of Delayed Consumption." *Economic Journal*, 1987, 97(387), pp. 666-684.

[60] Marum, Lawrence, James N. Muttunga, Francis M. Munene, and Boaz K. Cheluget. Kenya Demographic and Health Survey. Central Bureau of Statistics (Kenya), Ministry of Health (Kenya), and ORC Macro, 2004.

[61] Maluccio, John. "Attrition in the Kwazulu Natal Income Dynamics Study, 1993-1998." *International Food Policy Research Institute Food Consumption and Nutrition Division*, 2000, Discussion Paper 95.

[62] Marseille, Elliot, James G. Kahn, Francis Mmiro, Laura Guay, Philippa Musoke, Mary Glenn Fowler, J. Brooks Jackson. "Cost Effectiveness of Single-Dose Nevirapine Regimen for Mothers and Babies to Decrease Vertical HIV-1 Transmission in Sub-Saharan Africa." *Lancet*, 1999 Sep, 354(9181), pp. 803-809

[63] Martin, Gayle. "A Comparative Analysis of the Financing of HIV/AIDS Programmes in Botswana, Lesotho, Mozambique, South Africa, Swaziland, and Zimbabwe." Social Aspects of HIV/AIDS and Health Research Programme of the Human Sciences Research Council, 2003.

[64] Matovu, Joseph KB., Godfrey Kigozi, Fred Nalugoda, F. Wabwire-Mangen, and Ronald H. Gray. "The Rakai Project Counseling Programme Experience." *Tropical Medicine and International Health*, 2002, 7 (12), pp. 1064-1067.

[65] Mechoulan, Stephane. "HIV Testing: a Trojan Horse?" *Topics in Economic Analysis & Policy*, 2004, 4 (1), Article 18.

[66] Miguel, Edward and Michael Kremer. "Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities." *Econometrica*, 2004, 72(1), pp. 159-217.

[67] Moore, Gwen. "Structural Determinants of Men's and Women's Personal Networks." *American Sociological Review*, 1990, 55(5), pp. 987-989.

[68] Morin, Steve and Gertrude Khumalo-Sakutukwa. "The Feasibility of Community-Based VCT in Zimbabwe" Mimeo, 2004.

[69] Mugusi, F., R. Josiah, A. Moshi, S. Chale, M. Bakari, E. Aris, P. Magao, N. Pallangyo, E. Sandstrom, G. Biberfeld, F. Mhalu, and K. Pallangyo. "Dropouts in a Long-Term Follow-Up Study Involving Voluntary Counseling and HIV Testing: Experience from a Cohort of Police Officers in Dar Es Salaam, Tanzania." *Journal of Acquired Immune Deficiency Syndromes,* 2004, 30(1), pp. 119-23.

[70] Myer, L, C Morroni, and B G Link. "Impact of Measurement Error in the Study of Sexually Transmitted Infections." *Sexually Transmitted Infections*, 2004, 80(4), pp. 318-323.

[71] Nyathi, Kholwani "Churches Introduce Mandatory HIV Testing." *Zimbabwe Times*, December 17, 2003.

[72] Obare, Francis. "The Effect of Non-Response on Population-Based HIV Prevalence Estimates: the Case of Rural Malawi." *Social Networks Project Working Papers*, 2005, No. 7.

[73] Oster, Emily. "Sexually Transmitted Infections, Sexual Behavior and the HIV/AIDS Epidemic." *Quarterly Journal of Economics*, 2005, 120(2), pp.467-515.

[74] Oster, Emily. "Behavior Change: Why Not Africa?" Harvard University Mimeo, 2005.

[75] Paltiel, David, Milton C. Weinstein, April D. Kimmel, George R. Seage, III, Elena Losina, Hong Zhang, Kenneth A. Freedberg, and Rochelle P. Walensky. "Expanded Screening for HIV in the United States - an Analysis of Cost-Effectiveness." *New England Journal of Medicine*, 2005, 352 (6), pp. 586-595.

[76] Painter, Thomas M. "Voluntary Counseling and Testing for Couples: A High-Leverage Intervention for HIV/AIDS Prevention in Sub-Saharan Africa." *Social Science & Medicine*, 2001, 53 (11), pp. 1397-1411.

[77] Philipson, Thomas and Richard A. Posner. "A Theoretical and Empirical Investigation of the Effects of Public Health Subsidies," *Quarterly Journal of Economics*, 1995, 110(2), pp. 445-474.

[78] Pinkerton, Steven D. and Paul R. Abramson. "Effectiveness of Condoms in Preventing HIV Transmission." *Social Science and Medicine*, 1997, 44(9), pp. 1303-1312.

[79] Rabin, Matthew. "Psychology and Economics." *Journal of Economic Literature*, 1998, 36(1), pp. 11-46.

[80] Georges Reniers. "Marital Strategies for Managing Exposure to HIV in Rural Malawi." Paper Presented at the Annual Meeting of the Population Association of America. Philadelphia, March 31 - April 2, 2005.

[81] Times Reporter. "Mandatory AIDS Testing Coming." *Times of Zambia.* May 19, 2004.

[82] Routh, Janell, Gertrude Khumalo-Sakutukwa1, Katherine Fritz, Taurai Vaki, Tinofa Mutevedzi, and Stephen F. Morin. "Removing Barriers to Learning One's HIV Status: A Comparison of Testers and Non-Testers in Sub-Saharan Africa." Mimeo International Conference on AIDS, Barcelona, Spain, 2002, July 7-12; 14, (abstract no. LbPeD9042).

[83] Sanders, Gillian D., Ahmed M. Bayoumi, Vandana Sundaram, Pinar Bilir, Christopher P. Neukermans, Chara E. Rydzak, Lena R. Douglass, Laura C. Lazzeroni, Mark Holodniy, and Douglas K. Owens. "Cost-Effectiveness of Screening for HIV in the Era of Highly Active Antiretroviral Therapy." *New England Journal of Medicine*, 2005, 6 (352), pp. 570-585.

[84] Sieff, Elaine M. Dawes, Robyn M. Loewenstein, George. "Anticipated Versus Actual Reaction to HIV Test Results." *American Journal of Psychology*, 1999, 112(2), pp. 297-311

[85] Sikkema Kathleen J, Eileen S Anderson, Jeffrey Kelly, Richard A Winett, Cheryl Gore-Felton, Roger A Roffman, Timothy G Heckman, Kristi Graves, Raymond Hoffmann, Michael J Brondino. "Outcomes of a Randomized Controlled Community-Level HIV Prevention Intervention for Adolescents in Low-Income Housing Developments." *AIDS,* 2005, 19(14), pp. 1509-1516.

[86] Stein, Jo. "The Impact of Antiretroviral (ARV) Provision on HIV/AIDS Prevention." *AIDS Bulletin, Medical Research Council of South Africa*, 2005, 14(1).

[87] Stryker Jeff, Thomas J Coates, Pamela Decarlo, Katherine Haynes-Sanstad, Mike Shriver, Harvey J Makadon. "Prevention of HIV Infection. Looking Back, Looking Ahead." *Journal of American Medical Association*, 1995, 273(14), pp. 1143-1148.

[88] Sweat, Michael, Steven Gregorich, Gloria Sangiwa, Colin Furlonge, Don Balmer, Munkolekole Claudes Kamenga, Olga Grinstead, and Thomas Coates. "Cost-Effectiveness of Voluntary HIV-1 Counseling and Testing in Reducing Sexual Transmission of HIV-1 in Kenya and Tanzania." *Lancet*, 2000, 356 (9224), pp. 113-121.

[89] Temmerman M., S. Moses, D. Kiragu, S. Fusallah, I.A. Wamola, and P. Piot. "Impact of Single Session Post-Partum Counseling of HIV Infected Women on their Subsequent Reproductive Behavior. " *AIDS Care,* 1990, pp. 247-252.

[90] UNAIDS. "AIDS Epidemic Update December 2001." UNAIDS Joint United Nations Program on HIV/AIDS, 2001.

[91] UNAIDS. "Epidemiological Fact Sheet: Malawi." UNAIDS Joint United Nations Program on HIV/AIDS, 2004.

[92] Neff Walker, Nicholas C Grassly, Geoff P Garnett, Karen A Stanecki, Peter D Ghys. "Estimating the Global Burden of HIV/AIDS: What Do We Really Know About the HIV Pandemic?" *Lancet*, 2004, 363(9427), pp. 2180-2185.

[93] Weinhardt, Lance S., Michael P. Carey, Johnson T. Blair, Bickham Nicole L. "Effects of HIV Counseling and Testing on Sexual Risk Behavior: A Meta-Analytic Review of Published Research, 1985-1997." *American Public Health Association,* 1999, 89 (9), pp. 1397-1405.

[94] Wilson, Timothy and Dan Gilbert, "Affective Forecasting." In M. P. Zanna (Ed.), *Advances in Experimental Social Psychology*, San Diego, CA: Academic Press, 2003, 35, pp. 345-411.

[95] Wolitski, J. Richard, Robin J. Macgowan, Donna L. Higgins, and Cynthia M. Jorgensen. "The Effects of HIV Counseling and Testing on Risk-Related Practices and Help-Seeking Behavior." *AIDS Education and Prevention*, 1997, 9 (3), pp. 52-67.

[96] Wolff, Brent, Barbara Nyanzi, George Katongole, Deo Ssesanga, Anthony Ruberantwari, and Jimmy Whitworth. "Evaluation of a home-based voluntary counseling and testing intervention in rural Uganda." *Health Policy Plan*, 2005, 20 (2), pp. 109-16.

[97] Winsbury, Rex. "Safe Blood in Developing Countries: The Lessons from Uganda." Luxembourg, European Commission, as reported in *Confronting A1DS: Public Priorities in a Global Epidemic.*Washington, DC: Oxford University Press, 1997.

[98] Wu, Steven. "Sickness and Preventative Medical Behavior." *Journal of Health Economics*, 2003, July.

[99] Yoder, Stanley and Priscilla Matinga. "Voluntary Counseling and Testing (VCT) for HIV in Malawi: Public Perspectives and Recent VCT Experiences, " Mimeo, 2004, MACRO ORC.

Table 1: Summary Statistics

Panel A: Respondent Characteristics	Full Sample[1]		Follow-up Sample[2]	
Sample size	2769		1553	
	Mean	Standard Deviation	Mean	Standard Deviation
Male	0.46	(0.50)	0.46	(0.50)
Age	33.4	(13.7)	34.6	(14.3)
Years of education	5.0	(3.5)	5.2	(3.6)
Weekly income in 2001 (Dollars)	9.8	(3.5)	9.9	(3.6)
Value of crops produced in 2004 (Dollars)	2,326.4	(2,117.5)	2,430.1	(2,065.2)
Panel B: Health				
HIV positive[3]	0.067	(0.250)	0.054	(0.226)
Gonorrhea positive[4]	0.031	(0.174)	0.001	(0.026)
Chlamydia positive[5]	0.003	(0.055)	0.004	(0.064)
Trichomoniasis positive[6]	0.024	(0.153)	0.016	(0.125)
Reported having sex during 2004	0.77	(0.42)	0.79	(0.41)
Reported using condoms during 2004	0.21	(0.41)	0.21	(0.41)
Sexual acts in one month[7] (if >0)	5.6	(5.4)	5.4	(5.2)
Panel C: Incentives, Distance and Attendance at Results Centers				
Monetary incentive[8] (Dollars)	1.04	(0.89)	1.05	(0.91)
Distance to VCT center[9] (km)	2.0	(1.30)	2.2	(1.33)
Attended VCT center	0.72	(0.45)	0.73	(0.45)
Attended VCT center (if incentive=0)	0.39	(0.49)	0.37	(0.48)
Panel D: Follow-up Condom Sales				
Purchased condoms at the follow-up	--		0.24	(0.43)
Number of condoms purchased (if #>0)	--		3.7	(2.18)
Reported purchasing condoms	--		0.08	(0.28)
Reported having sex after VCT[10]			0.62	(0.49)

Notes:
1 Full sample includes respondents who accepted a test for HIV in 2004 and have age data.
2 Follow-up sample includes respondents in two districts who were re-interviewed in 2005.
3 HIV prevalence rates do not include 14 respondents with indeterminate diagnoses
4 Gonorrhea prevalence rates do not include 85 respondents with indeterminate diagnoses
5 Chlamydia prevalence rates do not include 85 respondents with indeterminate diagnoses
6 Trichomoniasis was only tested among women
7 Sexual acts per month were only asked during the nurses' survey in Balaka.
8 The monetary incentive is a sum of an incentive for learning HIV results and an incentive for learning other STI results
9 Distance from assigned testing centers to respondents' homes is a straight line spherical distance measured in kilometers.

Table 2: Impact of Monetary Incentives and Distance on Learning HIV Test Results

Dependent Variable: Attendance at HIV Results Centers

	All Respondents			
	(1)	(2)	(3)	(4)
Any	0.390***	0.271***	0.274***	0.273***
	(0.025)	(0.028)	(0.028)	(0.028)
Amount		0.089***	0.088***	0.088***
		(0.012)	(0.012)	(0.012)
HIV	-0.049	-0.049	-0.054*	-0.051
	(0.032)	(0.033)	(0.032)	(0.032)
Distance (km)			-0.066**	
			(0.026)	
Distance Squared			0.009**	
			(0.004)	
Over 1 km				-0.051*
				(0.024)
Male	0.003	0.001	0.001	0.001
	(0.017)	(0.017)	(0.017)	(0.017)
Age	0.005	0.005	0.005	0.005
	(0.003)	(0.003)	(0.003)	(0.003)
Simulated average distance			0.003	-0.006
			(0.014)	(0.011)
Constant	0.359***	0.375***	0.438***	0.415***
	(0.064)	(0.063)	(0.068)	(0.067)
Sample Size	2769	2769	2769	2769
R^2	0.15	0.17	0.17	0.17
Average Attendance	0.72	0.72	0.72	0.72

Notes:
Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence.

Sample includes individuals who tested for HIV and have age data. Columns represent OLS coefficients; robust standard errors clustered by village (for 125 villages) with district fixed effects are in parenthesis. “Any” is an indicator if the respondent received any positive valued monetary incentive and “Amount” is the total value of the incentive. “HIV” is an indicator of being HIV positive. Controls also include age squared. Simulated average distance is an average distance of respondents’ households to simulated randomized locations of HIV results centers. Distance is measured as a straight line spherical distance from a respondent’s home to randomly assigned VCT center from geospatial coordinates and is measured in kilometers.

Table 3: Interaction Effects with Incentives and Distance

Dependent Variable: Attendance at HIV Results Centers

	Balaka[1]	All Respondents		
	(1)	(2)	(3)	(4)
Any	0.462***	0.228***	0.280***	0.273***
	(0.062)	(0.054)	(0.028)	(0.025)
Amount	0.119***	0.088***	0.088***	0.089***
	(0.019)	(0.012)	(0.012)	(0.012)
Male	0.240***	0.002	0.001	0.001
	(0.093)	(0.017)	(0.017)	(0.016)
Male * Any	-0.271***			
	(0.087)			
HIV	-0.122**	-0.052	0.038	0.055
	(0.048)	(0.032)	(0.087)	(0.061)
Over 1 km	-0.028	-0.098*	-0.050*	-0.040
	(0.033)	(0.057)	(0.024)	(0.025)
Over 1 km*Any		0.060		
		(0.056)		
HIV * Any			-0.110	
			(0.093)	
Over 1 km* HIV				-0.147**
				(0.073)
Constant	0.081	0.452***	0.410***	0.405***
	(0.101)	(0.082)	(0.067)	(0.067)
Sample Size	1020	2769	2769	2769
R^2	0.16	0.17	0.17	0.17
Average Attendance	0.75	0.72	0.72	0.72

Notes:

Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence.

Sample includes individuals who tested for HIV and have age data. Columns represent OLS coefficients; robust standard errors clustered by village (for 125 villages) with district fixed effects are in parenthesis. “Any” is an indicator if the respondent received any positive valued monetary incentive and “Amount” is the total value of the incentive. “HIV” is an indicator of being HIV positive. Controls also include age, age squared and a simulated average distance variable (an average distance of respondents’ households to simulated randomized locations of HIV results centers). Distance is measured as a straight line spherical distance from a respondent’s home to randomly assigned VCT center from geospatial coordinates and is measured in kilometers. “Over 1 km” is an indicator if the respondent lived over one kilometer from the assigned HIV results center.

[1]Balaka is one region in the sample. There was no differential effect of incentives and gender in the other two districts.

Table 4: Impact of Neighbors' Attendance on Learning HIV Results

Dependent Variable: Attendance at the VCT Center

	Females		Males	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
% Neighbors attend (0 – 0.2 km)	0.099***	0.103**	0.104***	-0.022
	(0.038)	(0.047)	(0.035)	(0.061)
% Neighbors attend (0.2 -0.4 km)	0.031	0.030	0.003	0.060
	(0.040)	(0.057)	(0.040)	(0.057)
% Neighbors attend (0.4 – 0.6 km)	0.007	0.010	-0.018	0.006
	(0.038)	(0.066)	(0.045)	(0.066)
Any	0.292***	0.292***	0.245***	0.253***
	(0.039)	(0.038)	(0.041)	(0.041)
Amount	0.092***	0.092***	0.086***	0.083***
	(0.016)	(0.016)	(0.016)	(0.016)
Constant	0.288***	0.284**	0.474**	0.497**
	(0.102)	(0.106)	(0.092)	(0.088)
Sample Size	1484	1484	1269	1269
R^2	0.20	0.20	0.17	0.16
Average Attendance	0.71	0.71	0.72	0.72

Notes: Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. Sample includes individuals who tested for HIV and have age data. Proportion attendance represents the fraction of neighbors living within 0.2, 0.4, or 0.6 kilometer bands from the each respondent's household that attended the HIV results center. The first stage of the IV estimates include instruments for each kilometer band: if any neighbor received a positive-valued incentive, the average incentive offered, and the average distance from neighbors' households to the HIV results center. Robust standard errors clustered by village (for 125 villages) with district fixed effects are in parenthesis. "Any" is an indicator if the respondent received any positive valued monetary incentive and "Amount" is the total value of the incentive. Controls also include gender, HIV status, distance and distance-squared from the assigned HIV results center, age, age squared, number of neighbors within each kilometer band, and a simulated average distance variable (an average distance of respondents' households to simulated randomized locations of HIV results centers). Distance is measured as a straight line spherical distance from a respondent's home to randomly assigned VCT center from geospatial coordinates and is measured in kilometers.

Table 5: Impact of Spouses' Attendance on Learning HIV Results, Married Respondents

Dependent Variable: Attendance at the VCT Center

	Females		Males	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Spouse attended results center	0.256***	0.112	0.308***	0.345***
	(0.037)	(0.084)	(0.038)	(0.077)
Any	0.257***	0.260***	0.204***	0.200***
	(0.060)	(0.059)	(0.055)	(0.055)
Amount	0.081***	0.089***	0.102***	0.102***
	(0.018)	(0.016)	(0.020)	(0.020)
Constant	0.530***	0.546***	0.170	0.136
	(0.168)	(0.189)	(0.192)	(0.194)
Sample Size	677	677	679	679
R^2	0.26	0.24	0.25	0.25
Average Attendance	0.77	0.77	0.75	0.75

Notes: Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. Sample includes individuals who tested for HIV and have age data. Proportion attendance represents the fraction of neighbors living within 0.2, 0.4, or 0.6 kilometer bands from the each respondent's household that attended the HIV results center. The first stage of the IV estimates include: if the spouse received a positive-valued incentive and the amount of the spouses' incentive. Robust standard errors clustered by village (for 125 villages) with district fixed effects are in parenthesis. "Any" is an indicator if the respondent received any positive valued monetary incentive and "Amount" is the total value of the incentive. Controls also include gender, HIV status, distance and distance squared from the assigned HIV results center, age, age squared, number of neighbors within each kilometer band, and a simulated average distance variable (an average distance of respondents' households to simulated randomized locations of HIV results centers). Distance is measured as a straight line spherical distance from a respondent's home to randomly assigned VCT center from geospatial coordinates and is measured in kilometers.

Table 6: Reactions to Learning HIV Positive Results

Panel A: Sample who reported having sex at baseline

Dependent Variables:	Bought Condoms		Number of Condoms		Reported Buying Condoms		Reported Having Sex at Follow-up	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Knows Status	0.326**	0.279*	1.371***	1.849*	0.148***	0.088	0.032	-0.003
	(0.137)	(0.148)	(0.515)	(0.964)	(0.057)	(0.077)	(0.122)	(0.194)
Constant	-0.007	0.107	-0.538	-1.698	0.098	0.243	1.215	1.299
	(0.962)	(0.874)	(2.909)	(3.320)	(0.520)	(0.585)	(0.816)	(0.864)
Sample Size	71	71	71	71	71	71	71	71
R^2	0.15	0.15	0.13	0.12	0.12	0.11	0.12	0.11
Mean	0.37	0.37	1.24	1.24	0.11	0.11	0.70	0.70

Panel B: Sample who reported having sex at baseline and at follow-up

Dependent Variables:	Bought Condoms		Number of Condoms		Reported Buying Condoms	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Knows Status	0.456***	0.585***	1.577**	2.493**	0.233***	0.221
	(0.016)	(0.196)	(0.629)	(1.144)	(0.092)	(0.152)
Constant	0.345	0.098	0.873	-0.872	-0.043	-0.019
	(0.990)	(0.946)	(3.369)	(3.966)	(0.649)	(0.639)
Sample Size	50	50	50	50	50	50
R^2	0.20	0.18	0.15	0.10	0.14	0.14
Mean	0.38	0.38	1.26	1.26	0.16	0.16

Notes:
Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence.

Sample includes HIV positive respondents in Balaka and Rumphi that were tested for HIV and were re-interviewed in 2005. Robust standard errors clustered by village (for 125 villages) with district fixed effects are in parenthesis. Controls also include gender, age, age squared, and a simulated average distance variable (an average distance of respondents' households to simulated randomized locations of HIV results centers). Coefficients are either OLS or IV estimates where knowing HIV status is instrumented by having any positive-valued incentive, the total amount of the incentive, living over one kilometer of the HIV results center and all terms interacted with gender.

"Bought Condoms" is an indicator if any condoms were purchased from interviewers at the follow-up survey; "Number of Condoms" is the total number of condoms purchased from interviewers; "Reported Buying Condoms" having sex and reported buying condoms were asked at the follow-up interview in 2005 and refer to the previous two months since the VCT was available. "Reported Having Sex" is if the respondent reported any sexual intercourse at the time of the follow-up survey.

Table 7: Reactions to Learning HIV Negative Results

Panel A: Effects among those having sex and not having sex at Baseline

Dependent Variables:	Bought Condoms		Number of Condoms		Reported Buying Condoms		Reported Having Sex at Follow-up	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Knows Status	0.018	-0.097	0.227	-0.034	-0.030	-0.054	0.017	-0.016
	(0.050)	(0.104)	(0.191)	(0.431)	(0.027)	(0.054)	(0.060)	(0.123)
Had Sex at Baseline * Knows Status	-0.042	0.100	-0.443**	-0.149	0.021	0.092	-0.023	-0.057
	(0.052)	(0.121)	(0.217)	(0.542)	(0.032)	(0.073)	(0.068)	(0.133)
Had Sex at Baseline	0.117***	0.018	0.631***	0.425	0.050	0.000	0.378***	0.402***
	(0.037)	(0.084)	(0.176)	(0.380)	(0.030)	(0.052)	(0.061)	(0.100)
Constant	0.497***	0.586***	1.743***	1.947***	0.122***	0.142**	-0.328***	-0.306***
	(0.100)	(0.113)	(0.368)	(0.406)	(0.045)	(0.065)	(0.078)	(0.116)
Sample Size	1478	1478	1478	1478	1478	1478	1478	1478
R^2	0.16	0.16	0.09	0.09	0.06	0.06	0.22	0.22
Mean	0.25	0.25	0.91	0.91	0.09	0.09	0.73	0.73

Panel B: Effects among those having sex and not having sex at baseline and at the follow-up

Dependent Variables:	Bought Condoms		Number of Condoms		Reported Buying Condoms	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Knows Status	-0.017	-0.028	-0.029	-0.042	-0.008	-0.018
	(0.039)	(0.088)	(0.184)	(0.406)	(0.018)	(0.045)
Had Sex at Follow-up and Baseline * Knows Status	0.004	0.024	-0.156	-0.135	-0.011	0.059
	(0.057)	(0.116)	(0.264)	(0.551)	(0.029)	(0.073)
Had Sex at Follow-up and Baseline	0.098	0.084	0.479**	0.464	0.093***	0.042
	(0.039)	(0.077)	(0.204)	(0.391)	(0.026)	(0.056)
Constant	0.556***	0.564***	2.071***	2.080***	0.134***	0.140***
	(0.095)	(0.112)	(0.373)	(0.451)	(0.047)	(0.056)
Sample Size	1478	1478	1478	1478	1478	1478
R^2	0.17	0.17	0.09	0.09	0.07	0.07
Mean	0.27	0.27	0.99	0.99	0.11	0.11

Notes:
Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence.

Sample includes HIV negative respondents in Balaka and Rumphi that were tested for HIV and were re-interviewed in 2005. Robust standard errors clustered by village (for 125 villages) with district fixed effects are in parenthesis. Controls also include gender, age, age squared, and a simulated average distance variable (an average distance of respondents' households to simulated randomized locations of HIV results centers). Coefficients are either OLS or IV estimates where knowing HIV status is instrumented by having any positive-valued incentive, the total amount of the incentive, living over one kilometer of the HIV results center and all terms interacted with gender and having sex.
"Bought Condoms" is an indicator if any condoms were purchased from interviewers at the follow-up survey; "Number of Condoms" is the total number of condoms purchased from interviewers; "Reported Buying Condoms" having sex and reported buying condoms were asked at the follow-up interview in 2005 and refer to the previous two months since the VCT was available. "Reported Having Sex" is if the respondent reported any sexual intercourse at the time of the follow-up survey.

Table 8: Reactions to Learning HIV Results

Panel A: Sample who reported having sex and not having sex at baseline

Sample:	Had Sex at Baseline				Did Not Have Sex at Baseline			
Dependent Variables:	Bought Condoms	Number of Condoms	Reported Buying Condoms	Reported Having Sex at Follow-up	Bought Condoms	Number of Condoms	Reported Buying Condoms	Reported Having Sex at Follow-up
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Knows Status	-0.001	-0.198	0.036	-0.074	0.008	0.292	-0.024	0.005
	(0.054)	(0.249)	(0.046)	(0.065)	(0.109)	(0.467)	(0.058)	(0.130)
Knows Status * HIV	0.161	1.562**	0.049	0.072	-0.197	-0.825	0.067	0.117
	(0.151)	(0.797)	(0.089)	(0.227)	(0.380)	(1.276)	(0.060)	(0.493)
HIV	-0.033	-0.864	0.002	-0.225	0.221	0.776	-0.037	-0.224
	(0.119)	(0.487)	(0.066)	(0.194)	(0.256)	(0.834)	(0.046)	(0.281)
Male	0.118***	0.474***	0.119***	-0.001	0.224***	0.691***	0.073***	0.106***
	(0.022)	(0.103)	(0.019)	(0.026)	(0.041)	(0.181)	(0.022)	(0.043)
Male * HIV	0.041	0.122	-0.008	-0.273***	0.024	0.010	0.227	0.016
	(0.105)	(0.432)	(0.084)	(0.103)	(0.333)	(1.077)	(0.274)	(0.380)
Constant	0.572***	2.164***	0.145**	0.200**	0.144	0.781	0.032	-0.387**
	(0.090)	(0.443)	(0.069)	(0.102)	(0.149)	(0.716)	(0.064)	(0.164)
Sample Size	1228	1228	1228	1228	334	334	334	334
R^2	0.16	0.09	0.06	0.05	0.13	0.06	0.07	0.13
Mean	0.26	0.93	0.09	0.72	0.18	0.70	0.05	0.24

Panel B: Sample who reported having sex and not having sex at the baseline and at the follow-up

Sample:	Had Sex at Baseline and Follow-up			Did Not Have Sex at Baseline or at the Follow-up		
Dependent Variables:	Bought Condoms	Number of Condoms	Reported Buying Condoms	Bought Condoms	Number of Condoms	Reported Buying Condoms
	(1)	(2)	(3)	(4)	(5)	(6)
Knows Status	-0.035	-0.272	0.027	-0.050	-0.007	0.010
	(0.064)	(0.295)	(0.055)	(0.125)	(0.532)	(0.046)
Knows Status * HIV	0.354**	1.866***	0.214	-0.071	-0.248	0.025
	(0.171)	(0.735)	(0.160)	(0.572)	(1.833)	(0.058)
HIV	-0.188	-1.246**	-0.090	0.176	0.558	-0.006
	(0.166)	(0.604)	(0.106)	(0.338)	(1.081)	(0.041)
Male	0.116***	0.400***	0.139***	0.247***	0.816***	0.039**
	(0.024)	(0.104)	(0.025)	(0.050)	(0.234)	(0.020)
Male * HIV	0.019	0.207	-0.025	0.368	-1.312	0.426
	(0.133)	(0.520)	(0.099)	(0.312)	(1.009)	(0.379)
Constant	0.811***	3.020***	0.340***	0.004	0.330	-0.007
	(0.115)	(0.704)	(0.122)	(0.037)	(0.699)	(0.030)
Sample Size	890	890	890	253	253	253
R^2	0.19	0.10	0.08	0.12	0.07	0.10
Mean	0.28	1.01	0.12	0.16	0.63	0.02

Notes: Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. Sample includes respondents in Balaka and Rumphi that were tested for HIV and were re-interviewed in 2005. Robust standard errors clustered by village (for 125 villages) with district fixed effects in parenthesis. Controls include gender, age, age squared, and a simulated average distance variable (an average distance of respondents' households to simulated randomized locations of HIV results centers). Coefficients are IV estimates where knowing HIV status is instrumented by having any positive-valued incentive, total amount of the incentive, living over one km of the HIV results center and all terms interacted with gender and HIV. "Bought Condoms" is an indicator if any condoms were purchased at the follow-up survey; "Number of Condoms" is the total number of condoms purchased; reported having sex and reported buying condoms refer to the previous two months since the VCT was available.

Table 9: Average Belief of Likelihood of Infection Before and After VCT

Dependent Variable: "How likely is it that you are infected with HIV now?"

Panel A: HIV Negatives	Before HIV Testing	After HIV Testing: Knows Status	After HIV Testing: Does Not Know Status	Difference	
	(1)	(2)	(3)	(4)	
No Likelihood	0.479	0.877	0.498	0.379***	(0.023)
Low Likelihood	0.222	0.049	0.129	-0.080***	(0.015)
High Likelihood	0.051	0.000	0.000	0.001	(0.003)
Don't know	0.258	0.071	0.371	-0.301***	(0.020)

Panel B: HIV Positives	Before HIV Testing	After HIV Testing: Knows Status	After HIV Testing: Does Not Know Status	Difference	
	(1)	(2)	(3)	(4)	
No Likelihood	0.313	0.526	0.539	-0.012	(0.120)
Low Likelihood	0.289	0.070	0.154	-0.084	(0.070)
High Likelihood	0.072	0.228	0.115	0.113	(0.094)
Don't know	0.325	0.175	0.192	-0.017	(0.092)

Notes:

Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence.

Sample includes married respondents in Balaka and Rumphi that were tested for HIV, were re-interviewed in 2005, and whose spouse also agreed to an HIV test. Robust standard errors clustered by village (for 125 villages) with district fixed effects are in parenthesis. Controls also include gender, age, age squared, and a simulated average distance variable (an average distance of respondents' households to simulated randomized locations of HIV results centers). Coefficients in columns 1 – 3 represent the unconditional mean. Column 4 presents the OLS coefficient and standard errors.

Table 10: Marginal Cost of Interventions and Cost per Infection Averted

Includes IV estimates for differential effects of learning HIV status on purchasing:		Subsidized and Non-Subsidized Condoms		Subsidized Condoms Only	
	(1)	(2)		(3)	
	Marginal Cost	Infections averted	Cost per infection averted	Infections averted	Cost per infection averted
Incentives	7,535	11	1,136 (50)	1	7,399 (167)
95% CI	(7,191 – 7,879)	(2 – 20)	(1038 – 1234)	(0 – 1)	(7,070 – 7,727)
Condoms	64,203	3	10,650 (270)	4	10,532 (119)
95% CI	(63,645 – 64,761)	(2 – 5)	(10,119 – 11,181)	(2 – 6)	(10,299 – 10,765)
Test	401,289	8	87,108 (5,427)	0	∞
95% CI	(397,828 – 404,749)	(1 – 15)	(76,458 – 97,758)	--	--
Test, Condoms, and Incentives	469,098	23	26,904 (952)	5	66,716 (1,533)
95% CI	(465,586 – 472,611)	(7 – 40)	(25,036 – 28,771)	(3 – 7)	(63,707 – 69,725)

Notes:
Standard errors are in parentheses. Cost Effectiveness for 10,000 people for one year.
Marginal effectiveness of each program is calculated as the difference in costs, divided by the difference in infections averted for each program pair. Parameters include IV estimates accounting for effects on purchasing subsidized and non-subsidized condoms from Table 8, Panel B, Columns 1 and 3. Includes the estimates derived from using the sample of individuals that reported having sex at the follow-up survey.

Figure 1a: Malawi

Figure 1b: Location of Study Sites

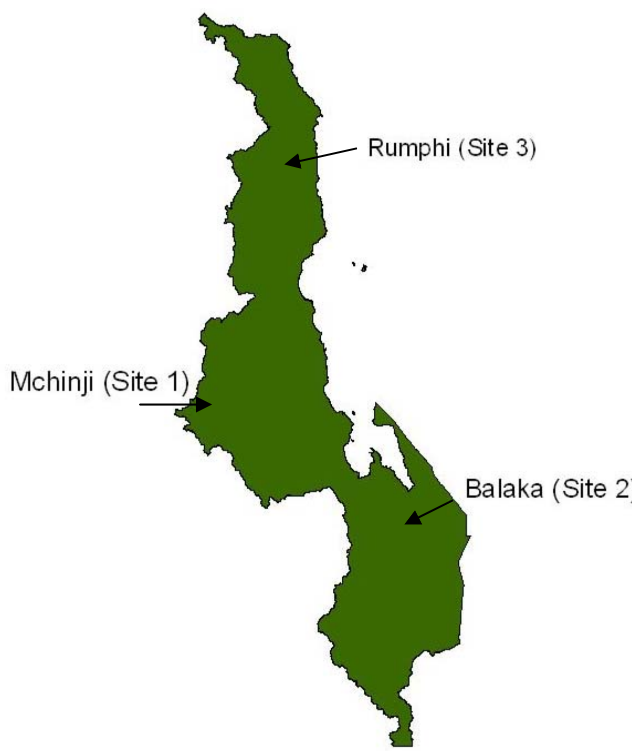
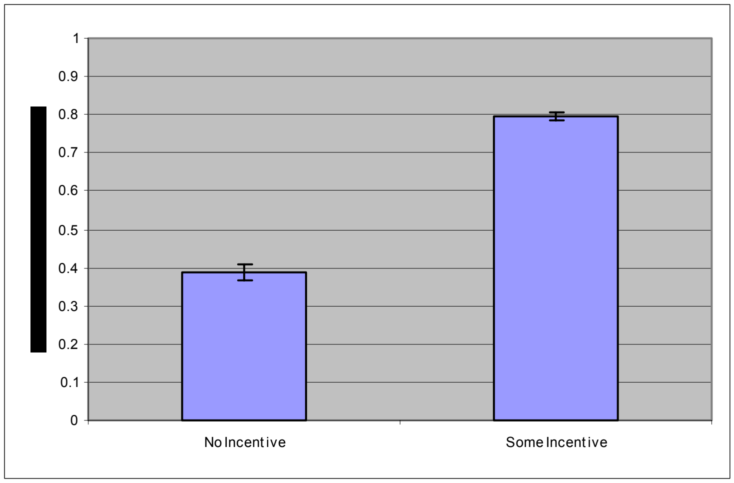
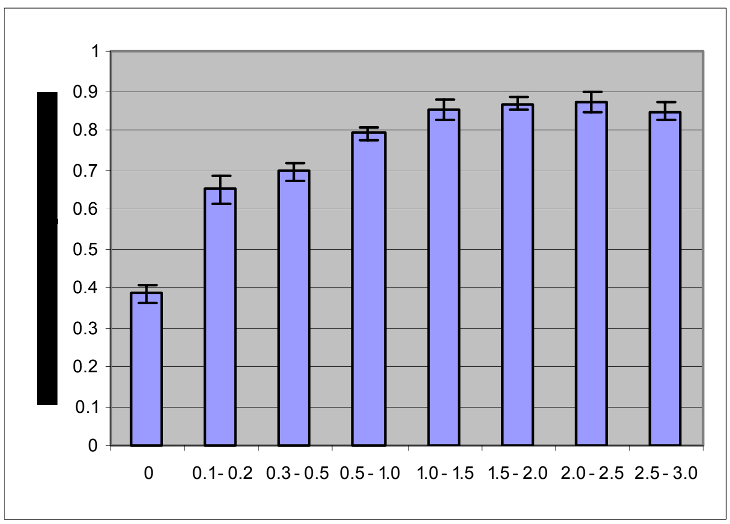


Figure 2: Percent Returning for HIV Results
by Amount of Monetary Incentive Offered (Dollars)



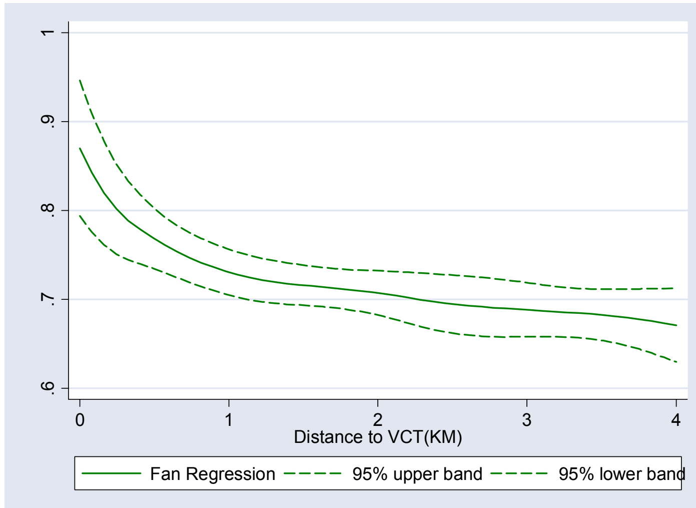
Panel A: Effects of Receiving Some Incentive



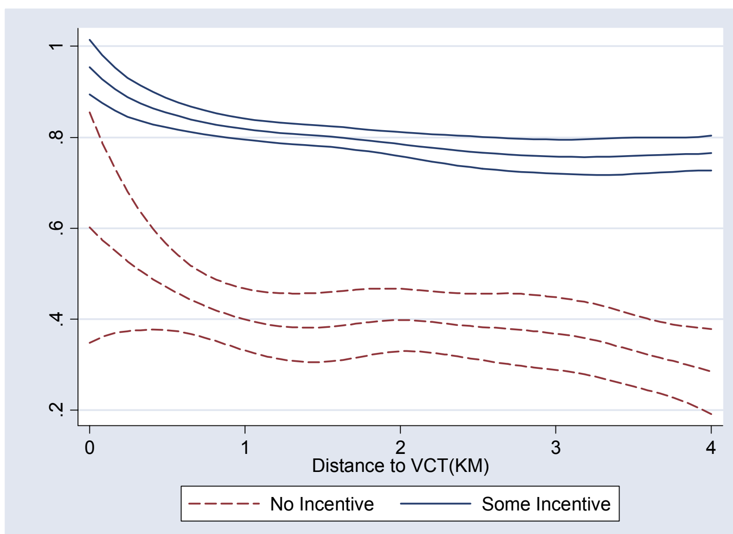
Panel B: Effects of the Total Amount of the Incentive

Notes: Sample includes 2,762 individuals who tested for HIV. 0.05 percent error bars are presented.

Figure 3: Impact of Distance to VCT on Probability of Returning for HIV Results
Non-parametric Fan Regression



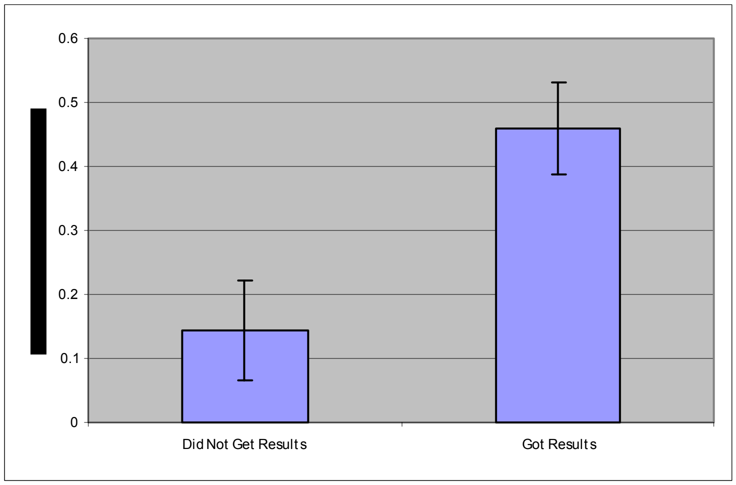
Panel A: Entire Sample



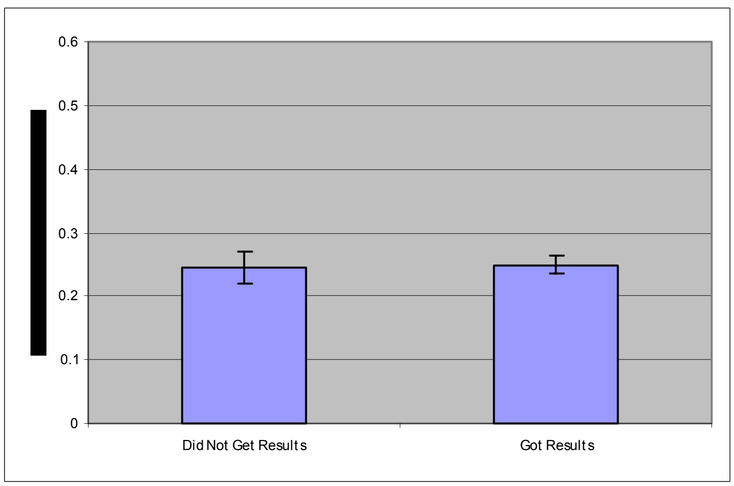
Panel B: Impact of Distance among those Receiving and Not Receiving an Incentive

<u>Notes:</u> Non-parametric Fan regression where distance is measured as a straight line spherical distance from a respondent's home to randomly assigned VCT center from geospatial coordinates and is measured in kilometers. Sample includes 2,762 individuals who tested for HIV. Lines indicate percent attending the results centers and upper and lower confidence intervals. Upper and lower confidence intervals are included in each figure.

Figure 4: Percent Purchasing Condoms
Among Respondents Who Reported Having Sex at Baseline Survey



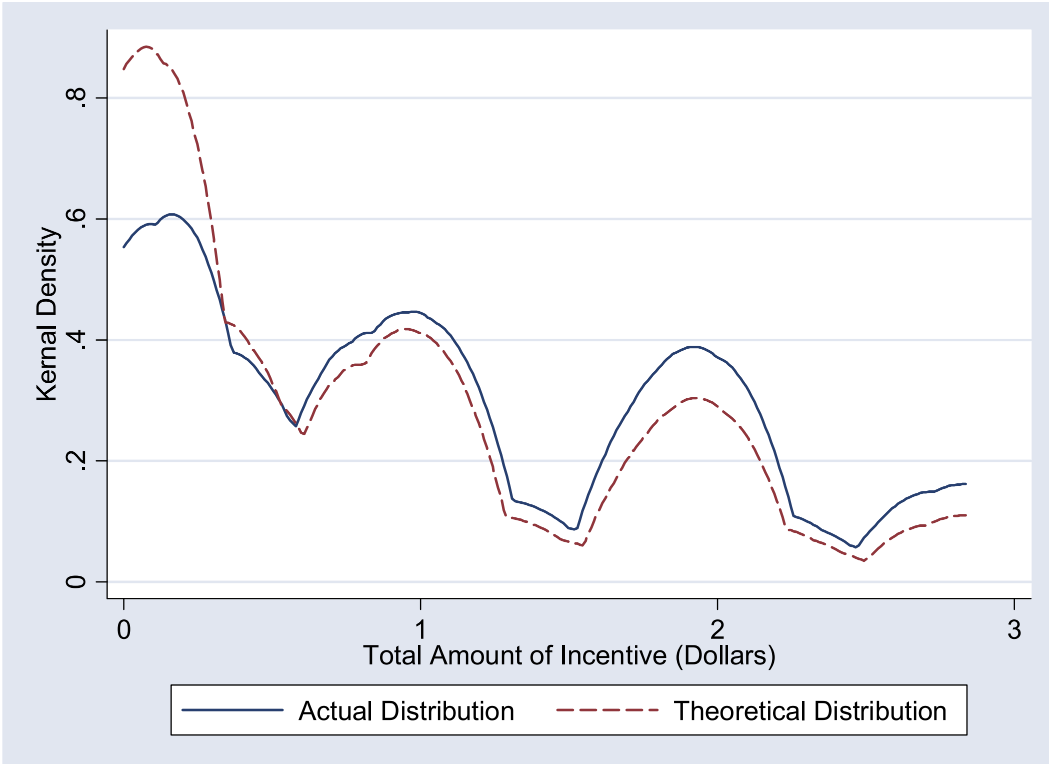
Panel A: HIV Positives



Panel B: HIV Negatives

Notes: Sample includes respondents in Balaka and Rumphi that were tested for HIV and were re-interviewed in 2005 who reported having sex during 2004.

Appendix A: Distribution of Monetary Incentives to Return for HIV Results (Dollars)



Notes: Sample includes 2,762 individuals who tested for HIV and have basic age data.

Appendix B: Sample Attrition

Panel A: Sample Size and Attrition	(1)
Adolescent sample interviewed in 2004[1]	973
Adult sample interviewed in 2004[2]	2231
Percent of baseline adult sample interviewed in 2001[3]	0.75
Percent of baseline adult sample interviewed in 2004	0.61
Panel B: Acceptance of HIV Test	
Percent Adolescents Tested for HIV	0.92
Percent Adults Tested for HIV	0.91
Panel C: Follow up Survey, 20054	
Adults and Adolescents Interviewed	0.82
Panel D: Determinants of Participation in the Follow up Survey	
Dependent Variable: Participation in Follow up Survey	OLS
	(1)
Received Any Incentive	-0.022
	(0.027)
Total Amount of Incentive	0.005
	(0.012)
Distance to VCT (km)	-0.004
	(0.018)
Sample Size	1915
R-square	0.01

Notes:
[1] Married and unmarried adolescents between 15 and 24 were randomly selected from sample villages in 2004 and added to the longitudinal sample. No information about these individuals is available before 2004.
[2] Includes married adult men and women that were initially sampled in 1998
[3] Attrition from the sample was due to a variety of reasons. In 2001 17.1 percent of the 1998 sample had migrated and 2.9 percent had died. 2.6 had died.
[4] Respondents that had been tested for either STIs or HIV in Balaka and Rumphi were re-interviewed in 2005.

Appendix C: First Stage Regression:
The Impact of Incentives and Distance on Knowing Results

	HIV Positives		HIV Negatives			
Sample	Had Sex at Baseline	Had Sex at Follow-up				
Dependent Variables	Knows Status	Knows Status	Knows Status * Had Sex at Baseline	Knows Status	Know Status * Had Sex at Follow-up	Knows Status
	(1)	(2)	(3)	(4)	(5)	(6)
Had Sex			0.349***	0.061	0.326***	-0.086
			(0.051)	(0.058)	(0.037)	(0.049)
Male	0.394*	0.432**	0.036	0.024	0.026	0.22
	(0.212)	(0.241)	(0.041)	(0.047)	(0.035)	(0.046)
Age	-0.093**	-0.035	0.006*	0.003	0.001	0.004
	(0.040)	(0.036)	(0.003)	(0.004)	(0.003)	(0.004)
Age2	0.001***	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Any	0.577***	0.599**	0.006	0.392***	0.002	0.239***
	(0.211)	(0.256)	(0.078)	(0.089)	(0.050)	(0.067)
Amount	0.156**	0.212**	-0.001	0.079**	0.002	0.092***
	(0.081)	(0.109)	(0.036)	(0.042)	(0.024)	(0.032)
Under 1 km	0.453***	0.297	0.005	0.073	0.002	0.116**
	(0.141)	(0.183)	(0.061)	(0.070)	(0.142)	(0.056)
Any * Male	-0.121	-0.154	-0.019	-0.022	-0.020	0.043
	(0.293)	(0.335)	(0.096)	(0.109)	(0.066)	(0.087)
Amount * Male	0.252*	0.229	0.000	-0.008	0.000	-0.019
	(0.147)	(0.159)	(0.053)	(0.061)	(0.035)	(0.047)
Under 1 km * Male	-0.709***	-0.493*	0.000	0.034	-0.002	-0.176
	(0.248)	(0.289)	(0.085)	(0.097)	(0.059)	(0.078)
Any * Had Sex			0.304***	-0.094	0.371***	0.125*
			(0.084)	(0.095)	(0.059)	(0.078)
Amount * Had Sex			0.105***	0.027	0.102***	0.012
			(0.041)	(0.047)	(0.030)	(0.040)
Under 1 * Had Sex			0.105	0.026	0.080	-0.038
			(0.070)	(0.080)	(0.053)	(0.070)
Any * Male * Had Sex			-0.007	0.008	-0.039	-0.098
			(0.099)	(0.123)	(0.072)	(0.095)
Amount * Male * Had Sex			0.009	0.015	0.022	0.041
			(0.060)	(0.069)	(0.045)	(0.059)
Under 1 * Male * Had Sex			-0.134	-0.094	-0.055	0.122
			(0.097)	(0.111)	(0.075)	(0.099)
Sample Size	71	53	1478	1478	1478	1478
R^2	0.29	0.39	0.51	0.22	0.65	0.22
F-test	21.62	12.31	324.29	20.14	540.92	18.84

Notes:
Sample includes respondents in Balaka and Rumphi that were tested for HIV and re-interviewed in 2005. Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. Robust standard errors clustered by village (for 125 villages) with district fixed effects are in parenthesis. Controls for gender, age, age squared and a simulated average distance are also included. Columns represent the first stage regressions from Table 7 and Table 8. "Had sex" refers to either having sex at the baseline or at the follow-up survey.

Appendix D: Effect of Learning HIV Results
Married Respondents whose Spouse also Tested

Dependent Variable: Purchasing Condoms

	Both HIV Negative		At Least One Spouse HIV Positive	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Knows HIV Status	0.079	0.085	0.443***	0.354
	(0.051)	(0.146)	(0.140)	(0.378)
Knows HIV Status * Spouse Knows HIV Status	-0.176***	-0.147	-0.055	0.398
	(0.068)	(0.228)	(0.139)	(0.703)
Spouse Knows HIV Status	0.093*	0.116	-0.034	-0.369
	(0.050)	(0.168)	(0.056)	(0.362)
Sample Size	699	699	73	73
R-square	0.18	0.18	0.21	0.15
Mean	0.21	0.21	0.30	0.30

Notes:
Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence.

Sample includes married respondents in Balaka and Rumphi that were tested for HIV, were re-interviewed in 2005, and whose spouse also agreed to an HIV test. Robust standard errors clustered by village (for 125 villages) with district fixed effects are in parenthesis. Controls also include gender, age, age squared, and a simulated average distance variable (an average distance of respondents' households to simulated randomized locations of HIV results centers). Coefficients are either OLS or IV estimates where knowing HIV status is instrumented by having any positive-valued incentive, the total amount of the incentive, living over one kilometer of the HIV results center and all terms interacted with gender. Spouse knowing HIV status is instrumented by if the spouse received a positive-valued incentive and the total amount of the incentive. The set of instruments also includes the interactions of spouses' and respondents' incentive. The dependent variable is an indicator if any condoms were purchased from interviewers at the follow-up survey.

Appendix E: Calculating Cost-Effectiveness

To calculate the cost-effectiveness, I first estimate the costs for each activity: HIV testing, counseling, and selling condoms[47].

Program Cost per Respondent (Dollars)

		Average	Percent	Low	High
Testing	Transportation	4.54	0.10	4.54	4.54
	Labor	13.57	0.31	11.75	16.15
	Training	1.66	0.04	1.66	1.66
	Laboratory costs	22.45	0.51	6.76*	22.45
	Supplies	1.84	0.04	1.06	2.87
	Subtotal	**44.06**		25.57	47.05
Counseling and Results	Transportation	0.68	0.13	0.68	0.68
	Labor	3.48	0.64	3.37	4.76
	Training	0.16	0.03	0.16	0.16
	Supplies	1.09	0.20	0.63	1.72
	Subtotal	**5.51**		4.53	6.64
Selling Condoms	Transportation	0.90	0.18	0.90	0.90
	Labor	3.33	0.68	3.23	3.48
	Supplies	0.64	0.13	0.18	1.06
	Subtotal	**4.87**		4.31	5.44
	Total Cost	**55.34**		34.41	59.14

The costs of each intervention include actual costs incurred during 2004 fieldwork in Malawi and do not include any research-related expenses; high and low estimates for any variable costs are also included in the analyses and all costs were converted to US$ (107 Malawi Kwacha per dollar). The HIV tests and related fees (e.g., equipment for sample collection, laboratory and processing fees) constituted the largest proportion of costs ($22.45 per respondent). The laboratory costs are somewhat higher than in other studies and for the purpose of comparability I assign the lowest estimated cost of testing to $6.76 per respondent, following Sweat et al. (2000).

Labor (e.g., salary, accommodations, and benefits), accounts for the second largest proportion of costs ($13.57 per respondent), followed by transportation costs that include vehicle rental and fuel to transport employees to the rural study sites, as well as the cost of transporting HIV samples to the laboratory ($4.54 per respondent). Training for collecting HIV samples was approximately one week and included the costs of paying salaries, instructors, and room rental fees ($1.66 per respondent); training for post-test counseling was a two-day seminar ($0.16 per respondent)[48]. Supplies included employee uniforms, freezers and cold packs for HIV samples, portable tents, and other necessary supplies and

[47] These costs are derived from one representative study site, Balaka, based on the actual number of days for each activity, employees hired, and respondents approached.
[48] Separate training was necessary in each of the district study sites because each area required nurses that spoke the local language unique to that district.

equipment ($1.84 per respondent).

The average total cost per respondent of testing was $44.06, of counseling was $5.51, and for selling condoms to respondents was $4.87. All effort was made to choose the least expensive options for each portion of the project and all parameters (except the costs of testing as explained above) are comparable to costs from other effectiveness studies. Calibrations also include the costs of vouchers ($0.96), approaching respondents who refused to be tested, ($20.80 per respondent), of subsidizing condoms ($0.05 per condom), and the 30 cents given to each respondent to purchase condoms.

Following Sweat et al. (2000) and Bouey, Saidel and Rehle (1998), I calculate the probability of HIV infection equal to:

$$1-\{P[1-R(1-F\cdot E)\cdot S]^{N}+(1-P)\}^{M}$$

P is the prevalence rate of sexual partners of the uninfected individual; R is the transmission rate per sexual act; F is the fraction of sexual acts with a condom; E is the efficacy of condoms; S is the likelihood of having sex; N is the average number of sexual acts in a month; and M is the average number of sexual partners. Each parameter is either derived from the data or from epidemiological values from other sources. Parameter values from other studies are assumed to be uniformly distributed between high and low published values. Other values are assumed to be distributed normally with means and standard deviations derived from the data.

Epidemiological Parameters

Panel A: Epidemiological Parameters	**Low**	**High**	**Distribution**	**Source**
Condom effectiveness (E)	0.90	0.95	Uniform	Pinkerton and Abramson (1997)
Transmission rates per sexual act (R)	0.0011	0.02	Uniform	Gray et al. (2001), Bouey, Saidel, and Rehle (1998)
Average number of partners (M)	1.07	1.32	Uniform	Follow-up survey, Sweat et al. (2000)
Panel B: Sexual Behavior Parameters	**Mean**	**Standard Deviation**	**Distribution**	**Source**
HIV prevalence (P)	0.067	(0.23)	Normal	Baseline 2004 Data
HIV prevalence (positive sexual partner) (P)	0.381	(0.488)	Normal	Baseline 2004 Data
HIV prevalence (negative sexual partner) (P)	0.047	(0.213)	Normal	Baseline 2004 Data
Average sexual acts (if #>0) (N)	82.9	(126.4)	Normal	Follow-up Rumphi Data
Likelihood of having sex (S)	78.6	(0.41)	Normal	Baseline 2004 Data
Likelihood of using condoms (F)	0.21	(0.41)	Normal	Baseline 2004 Data
Likelihood of purchasing non-subsidized condoms (F)	Varies by knowledge of HIV status		Normal	Predicted IV estimates from Table 8, Panel B, Column 3
Likelihood of purchasing subsidized condoms (F)	Varies by knowledge of HIV status		Normal	Predicted IV estimates from Table 8, Panel B, Column 1

Condom efficacy is assumed to range between 0.90 and 0.95 (Pinkerton and Abramson 1997) and a per-sex act transmission rate between 0.0011 and 0.02 (Gray et al. 2001; Bouey, Saidel, and Rehle

1998)[49]. The overall HIV prevalence rate was 0.067 in the MDICP sample, however, the HIV prevalence rate of spouses with an HIV positive partner was 0.381 (standard deviation 0.488) and the HIV prevalence rate of spouses with an HIV negative partner was 0.047 (standard deviation 0.213); these are the prevalence rates used in the calibrations[50]. During the follow-up survey in one district, respondents were asked how many sexual partners they had in the past two months. On average, respondents reported having 1.07 sexual partners; this value is used as the lowest estimate of sexual partners in a year, assuming number of sexual partners is constant over time. The highest average number of reported sexual partners in Sweat (2000) was 1.32 per year, and I use this estimate as the highest parameter value. Recall that the likelihood of having sex did not vary with knowledge of HIV status (Tables 5 – 7); I therefore assume a 78.6 percent likelihood of having sex in a year for all respondents, which is the value from the baseline 2004 survey (standard deviation 0.41). Before learning HIV results, at one study site, respondents were asked the number of times in the previous month they had engaged in sex – the average reported number of monthly sexual acts was 5.6, thus making an average annual rate of 82.9 acts[51].

The only parameter that varies by knowledge of HIV status is F, the fraction of sexual acts protected by condoms. Because of data limitations I am unable to measure if condom efficacy, average number of partners, or average number of sexual acts changed in response to learning HIV results. However, just as there was no significant impact of learning HIV status on reported likelihood of having sex, it is possible that there was little to no effect of testing on these measures (this will be further explored after the next round of data collection). I calculate F with measures of average baseline condom use, reported purchases of non-subsidized condoms, and purchases of subsidized condoms, separately for HIV positives and negatives who know and do not know their HIV status. I assume a baseline rate of condom use of 21.0 percent (standard deviation 0.41) for all respondents, which was the likelihood that a respondent used a condom with their last sexual partner in the baseline 2004 survey data. The average likelihood of purchasing subsidized condoms is assumed to be normally distributed with mean and standard deviation of the predicted values of the IV estimates in Table 8, Panel B, Column 1. This is calculated separately for HIV negatives and positives who know and do not know their status. The

---

[49] The low value of the transmission rate (0.0011) is derived from heterosexual couples in Uganda (Gray et al. 2001); the high value in Gray et al. (2001) was 0.0041 among those with genital ulcerations; however Bouey, Saidel, and Rehle (1998) estimate an average transmission rate of 0.002. I use this value in the calibrations. This also assumes that the per-sex transmission rate is constant over partners, and over time within the same partnership, which is likely to over-state the actual transmission rate, favoring testing effectiveness. Also see Bracher et al. (2004) for detailed discussion on breakage and slippage rates of condoms in Africa.

[50] Individuals having sexual relations outside of marriage or among high-risk groups would face different prevalence rates. Sweat et al. (2000) assumes that sexual partners of HIV positives have the same prevalence rate as the overall rate and that sexual partners of HIV negatives have a rate 10 percent lower than the overall rate, which are assumptions that are likely to understate actual infection rates, favoring cost-effectiveness.

[51] This assumes that HIV positives and HIV negatives have sex with the same frequency, although it is likely that HIV positives have fewer sexual acts (Gray et al. 2001).

average number of condoms purchased is 3.7 (standard deviation 2.2; Table 1, Panel D).

In no specification was the impact of HIV knowledge on reported purchases of condoms statistically significant, however, the coefficient had a positive point estimate. For this reason, I present estimates with and without using the estimates on the impact on *reported* condom purchases in the calibration, assuming one condom is purchased. I either assign all respondents an equal likelihood of purchasing non-subsidized condoms (0.08), or derive the average likelihood of purchasing non-subsidized condoms for HIV positives and negatives who know and do not know their status from the IV estimates in Table 8, Panel B, Column 3.

Given these parameters, I take 1,000 independent random draws from the distribution of each value to derive the expected probability of HIV positives infecting their partner(s) and of HIV negatives becoming infected, separately for those that do and do not purchase subsidized condoms, and those that do and do not know their HIV status. I assume that all purchased condoms are used during sexual intercourse, assume an HIV test acceptance rate of 0.92, and do not condition on distance from the VCT center to estimate the impact of receiving a voucher on learning HIV results. All results are calculated as 12 month effects per 10,000 people approached for an HIV test and re-approached once to purchase condoms.

Using the estimated probabilities of infection, I simulate the total number of new infections and total costs under six programs: (1) Testing, offering vouchers, and selling condoms, (2) testing without vouchers and selling condoms, (3) selling condoms, (4) testing with vouchers, (5) testing without vouchers, and (6) having no program. To measure the incremental cost-effectiveness ratio (ICER) for each additional program I divide the difference in costs by the difference in infections for each program (Briggs and Fenn 1998):

(7) $$\text{Incremental cost-effectiveness ratio (ICER)} = \frac{\overline{C}_1 - \overline{C}_2}{\bar{I}_1 - \bar{I}_2}$$

$\overline{C}_i$ is the average cost and $\bar{I}_i$ is the average number of infections of program i. The ICER is computed for each *incremental* program (e.g., vouchers, condoms, testing, and all three programs together) 1,000 times to compute an average ICER and bootstrapped standard errors.