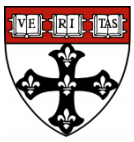

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# Using Interviewer Random Effects to Calculate Unbiased HIV Prevalence Estimates in the Presence of Non-Response: a Bayesian Approach

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## Abstract

Selection bias in HIV prevalence estimates occurs if refusal to test is correlated with HIV status. Interviewer identity is plausibly correlated with consenting to test, but not with HIV status, allowing a Heckman-type correction that produces consistent HIV prevalence estimates. We innovate on existing approaches by showing that an interviewer random effects Bayesian estimator produces prevalence estimates that are unbiased as well as consistent. An additional advantage of this new estimator is that it allows the construction of bootstrapped standard errors. It is also easily implemented in standard statistical software. The model is used to produce new estimates and confidence intervals for HIV prevalence among men in Zambia and Ghana.

Keywords: HIV, Heckman Selection Models, Missing Data, Bayesian Estimation

JEL Codes: C11; I19; J11

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## 1. Introduction

Estimates of HIV prevalence in developing countries from serologic testing in nationally representative household surveys have been considered a "gold standard" (Boerma et al., 2003). However, survey-based prevalence estimates have the drawback that that consent rates to test for HIV from respondents may be low. For example, in the 2003 Ghana Demographic and Health Survey (DHS) 17% of men declined to give blood for a HIV test, while in Zambia in 2007 the figure was 27%. If the rate of HIV infection is different in the group of individuals who refuse than in those who test, then ignoring this missing data may lead to biased estimates of population prevalence. Existing estimates typically impute data for these individuals under the assumption that nonresponses are "missing at random", conditional on the other covariates in the model. This produces population prevalence estimates that are very close to the estimates for the sample that consents to test (Mishra et al., 2008). Single imputation is the method recommended by the World Health Organisation for dealing with missing values. This approach uses the out of sample predicted value (i.e. the predicted value based on observable characteristics of those who have missing data on HIV status) from the independent variables in a model for observed HIV status. This method therefore requires the assumption that there are no unobserved variables that are correlated with both HIV status and consent to test, as do other methods which rely on the missing at random assumption, including propensity score re-weighting and multiple imputation. This assumption is unlikely to hold since a person's belief about his or her HIV status may be related to actual status and may influence the likelihood of consenting to a test. For example, individuals who already know, or suspect based on evaluation of past behavior, that they are HIV positive may be less likely to consent (Bärnighausen et al., 2012; Reniers and Eaton, 2009; Floyd et al., 2012). If this is the case, there will be a selection bias in population prevalence estimates based on an incorrect assumption of "missing at random".

An attractive alternative to simple imputation and other single equation methods is to use a selection model that explicitly accounts for the selection process, and procures population estimates that allow for this, such as those proposed by Heckman (1979) and Vella (1998). This approach allows consistent estimation of prevalence, even in the presence of unobservable influences on consent, such as beliefs about HIV status. By accounting for both consent and HIV

status equations (individuals first chose whether to test, and it is only conditional on this that we observe their HIV status) explicitly in the model, the key benefit of the Heckman method is that we do not require the missing at random assumption. Bärnighausen et al. (2011) adopted this approach and found a substantial bias from non-response. In the 2007 Zambia DHS survey, they estimated HIV prevalence among men who refused consent to be 53% using the sample selection model, but only 12% from an imputation model. This provides evidence that existing incidence estimates may be misleading, and that selection on unobservables may be an important source of bias in existing estimates. Hogan et al. (2012) used the same method to produce selection corrected HIV prevalence estimates for 12 African countries, while Clark and Houle (2012a) used the approach to produce corrected estimates for the Agincourt Health and Demographic Surveillance Site in South Africa.

Heckman type selection models require an exclusion restriction for identification, namely a variable which predicts consent but not HIV status. The innovation in Bärnighausen et al. (2011) is to use the identity of interviewers as a plausible variable that is correlated with consenting to test but not with HIV status. Higher quality interviewers may have a personality type (for example the ability to show empathy for the interviewee), or relevant experience, which increases their response rates. This is a testable assumption, and we show in tables 1 and 5 that interviewer identity is highly correlated with consent.

Interviewers are not randomly assigned to participants in DHS surveys, but are assigned to specific regions and tend to be matched to respondents by sex and language. However, as Bärnighausen et al. (2011) argue, sex and language (as well as rich set of other participant characteristics) are recorded in the DHS datasets and can thus be controlled for in the analysis. Once they have been controlled for, the interviewer assignment is plausibly random and uncorrelated with participants' HIV status. Clark and Houle (2012b) confirmed this theoretical claim by undertaking a simulation study of the Bärnighausen et al. (2011) approach in which they set the true prevalence rate in their simulated data and showed that the method provides reasonable estimates in large samples unless the assumption that conditional on observed participant characteristics interviewer identity is uncorrelated with HIV status is violated.

While the underlying model is reasonable, there are a number of potential limitations associated with the Bärnighausen et al. (2011) approach. Firstly, they use interviewer fixed effects as their key variables that affect consent but not status. These fixed effects, however, are not identified for interviewers who conducted only a very small number of interviews, or who have only successes or failures in consent, or who are the only interviewer to interview people of a particular language in a particular region, making their fixed effects collinear with these characteristics. In these cases, the interviewer fixed effects are pooled to a common value in the Bärnighausen et al. (2011) approach. The assumption of a common value for the interviewer effect on consent in these cases is difficult to justify. In addition, even when interviewer fixed effects are formally identified, this identification may be weak and lead to lack of convergence in model estimates; Clark and Houle (2012b) found that in a large percentage of their simulations the model failed to converge.

Secondly, the analytical standard errors for the HIV prevalence estimates reported by Bärnighausen et al. (2011) are too small since they do not account for regression parameter uncertainty in constructing the model of consent and HIV status. A bootstrap procedure which would allow correct inference is difficult to implement with the fixed effects approach, due to the fact that the number of interviewees per interviewer will vary with each bootstrap sample, and a different set of interviewer fixed effects becomes unidentified, requiring rewriting the model with different pooling of interviewer fixed effects for each bootstrap draw. Hogan et al. (2012) addressed this issue by using a parametric simulation approach; however, this approach requires a strong assumption on the correlations between unobserved error terms in the parameterization.

Finally, while the maximum likelihood estimator is consistent it can be biased in small samples, particularly where the estimated correlation coefficient between the error terms in the consent and prevalence equations, the key parameter in the model, is close to the boundary, either – 1 or +1, or when the likelihood function is very flat so that many values of the correlation parameter are almost equally likely.

We address these issues by proposing a random effects approach with Bayesian averaging. The random interviewer effect assumption means that the interviewer "quality" that affects consent to HIV testing, and likely to comprise some of the attributes described above, is taken to be a

random draw from a normal distribution instead of a fixed effect. This enables us to estimate the interviewer effects in cases in which it is difficult to identify and estimate fixed effects. As a consequence we avoid arbitrary pooling of interviewer effects and allow straightforward bootstrapping to obtain correct confidence intervals for our HIV prevalence estimates.

We address the issue of bias in the maximum likelihood estimator by using a Bayesian model averaging approach. We use a flat prior on the interval [-1, 1] for the correlation between the error terms in testing and HIV status. We then estimate a posterior distribution for the correlation based on the data and our selection model. We show that the expected value of the correlation parameter given the posterior distribution is a consistent and unbiased estimator.

We illustrate this methodology using data from the Demographic and Health Surveys, and implement our procedure for Zambian and Ghanaian men. We examine Zambian men so that the results of our random effects model can be compared with the fixed effects approach of Bärnighausen et al. (2011); we show the two approaches produce very similar results. We examine Ghanaian men because, as in Hogan et al. (2012), we find a correlation between being HIV positive and testing for this group to be close to perfect (a correlation of one), so that the maximum likelihood estimate for HIV prevalence among those who refuse to test is zero. We argue that our unbiased estimate of prevalence among those who refuse to test based on Bayesian averaging, which is positive, though small, is a more reasonable point estimate in this case. Our methodology is designed both to improve upon the existing fixed effects approach and to be easy to implement so that researchers can produce both prevalence estimates and valid confidence intervals given their survey data in a straightforward way.[5]

We have two main empirical results. The first result confirms the finding in Bärnighausen et al. (2011) that the point estimate for the HIV prevalence rate can change a great deal when we allow for response bias. The second result is that, as suggested by Hogan et al. (2012), the analytic standard errors reported by Bärnighausen et al. (2011) are too small. Our bootstrapped confidence intervals for the prevalence rate can be very large in the presence of response bias and uncertainty on the correlation between testing and HIV status. The correlation between

[5] We provide the STATA computer programs used in our analysis. These can easily be adapted to be used with other surveys.

agreeing to test and HIV status is fundamentally difficult to estimate, and when we have a large volume of non-response in a survey we are actually quite uncertain of the population prevalence rate. The confidence intervals for the standard imputation model, based on non-response generating data that is missing at random, are very small; this is because the model wrongly assumes that the correlation between testing and status is known with complete certainty to be zero.

The rest of this paper is structured as follows. Section 2 outlines the methodology and theory in more depth. Section 3 describes the data and Section 4 presents the results. Section 5 concludes.

## 2. The Model

Following Bärnighausen et al. (2011) we model consent to HIV testing for person *i* with interviewer *j* as the observed outcome arising from a latent variable that may interpreted as the propensity to consent to testing:

$$\begin{aligned} s_{ij}^{*} &= \beta_s x_{ij} + z_j + u_{ij} \\ s_{ij} &= 1 \; if \; s_{ij}^{*} > 0, s_{ij} = 0 \; otherwise \end{aligned} \tag{1}$$

where $s_{ij}$ is a dummy for agreeing to test, $x_{ij}$ are observed characteristics, $z_j$ are interviewer effects, $u_{ij}$ is random error, and $s_{ij}^{*}$ is an unobserved latent variable. The equation for HIV status $h_{ij}$ of individual i with interviewer *j* is:

$$\begin{aligned} h_{ij}^{*} &= \beta_h x_{ij} + \varepsilon_{ij} \\ h_{ij} &= 1 \; if \; h_{ij}^{*} > 0, h_{ij} = 0 \; otherwise \\ h_{ij} &\; observed \; only \; if \; s_{ij} = 1 \end{aligned} \tag{2}$$

where $h_{ij}^{*}$ is again a latent variable[6] and $\varepsilon_{ij}$ is an error term. We assume $(u_{ij}, \varepsilon_{ij})$ are bivariate normal, each with mean zero, variance 1, and correlation parameter $\rho = corr(u_{ij}, \varepsilon_{ij})$.

The key parameter in the model is $\rho$. If $\rho = 0$ there is no correlation between testing and HIV status. In this case, simple imputation of the HIV status of those who do not test based on their

[6] Which can be thought of as reflecting propensity to be infected.

observed characteristics, as in Mishra et al (2008), is possible. If $\rho > 0 \; or \; \rho < 0$ however, testing is correlated with HIV status and the predicted probability of being HIV positive of those who refuse to test will be different than the average prevalence rate among people with similar observable characteristics who do test. The key issue in the model is therefore to find robust estimates of $\rho$.

The inclusion of the interviewer effects, which are assumed to affect consent to testing, but not HIV status, is crucial to the model. Without variables in the selection equation that are excluded from the HIV status equation, the model is identified only by non-linearities, and does not provide robust estimates (Madden, 2008).

Even with a suitable exclusion restriction, there are a number of technical problems associated with estimating the model. The first concerns the estimates of the interviewer effects in $z_j$. Bärnighausen et al. (2011) estimate each $z_j$ as a parameter on an interviewer dummy, essentially an interviewer fixed effect. Estimation of these interviewer fixed effects may be difficult because for interviewers who have only successes or failures in obtaining consent, the parameter is not identified. For example, for interviewers who always obtain consent we know their interviewer effect is large and positive, but it can be arbitrarily large as any very large positive interviewer effect above some threshold will predict perfect success, and we cannot distinguish between different effect sizes. In addition, we control for the region and language of the interviewee. However, interviewers usually work in only a small number of regions, and an effort is made to match them with interviewees by language. This may create co-linearity between the interviewer effect and the region and language dummies in the equation, making it difficult or even impossible to estimate the interviewer effects (i.e. weak or no identification of the interviewer parameter). The approach in Bärnighausen et al. (2011) was to limit estimation of individual interviewer effects to those interviewers with more than 50 interviews, and not to use the individual effects of those interviewers who had conducted fewer than 50 interviews when including them caused the model to fail to converge (using the trace diagnostic – the estimated standard error on interviewers whose effects are only weakly identified are very large). Instead, interviewers without a personal estimated effect were grouped together as a baseline group with an assumed common interviewer effect.

One difficulty with this approach is that it is not possible to estimate the interviewer effect for the most informative interviewers – those who always achieve consent. A second difficulty is related to drawing bootstrap samples for uncertainty estimation. Bootstrapping is particularly appealing in this setting since we want to know the confidence intervals of the model's predictions as well as for its parameters. However, a bootstrap sample may not converge for the given set of interviewer effects being estimated and a particular bootstrap sample draw may not identify the effect of some interviewers that were previously identified. A final difficulty is that grouping all of the interviewers whose individual interviewer effects impede model convergence into a baseline group with the same assumed effect may not be a convincing strategy, because we may be pooling interviewers who always have consent with those who never achieve consent and assuming they have the same average effectiveness in obtaining consent. Even if the individual interviewer effect is not fully identified, it may be partially identified (e.g. see Tamer, 2010), so that based on the data we can limit its value to an interval, and the fixed effect approach with pooling for under-identified effects does not exploit this.

Our approach to this problem is to assume that the unobserved interviewer effects reflect some underlying parametric distribution which describes interviewer quality. Following Mundlak (1978) and Chamberlain (1980), we write the interviewer effect as:

$$z_j = \delta \bar{x}_j + v_j \, , \bar{x}_j = \sum_i^{n_j} x_{ij} / n_j \, , v_j : N(0, \sigma_z^2) \quad (3)$$

Where $\bar{x}_j$ represents the average characteristics of the $n_j$ people that interviewer $j$ interviews. Instead of thinking of the interviewer effects as "fixed effects" we think of them as correlated with the average characteristics of the people interviewed plus a random effect, a draw from a normal distribution with mean zero and variance $\sigma_z^2$. For example, interviewers who are assigned to particular regions and matched by language will have a disproportionate number of interviewees in these regions with that language, and this may affect their success rate in testing. Excluding these controls could potentially invalidate the exclusion restriction of the model. While the interviewer's effect may be correlated with the average characteristics of her interviewees, we assume that, controlling for these averages, it is not correlated with the

characteristics of a particular interviewee. The $\bar{x}_j$ are the average characteristics of the set of interviewees a particular interviewer is assigned to. As these depend only on the survey design, they should not enter the HIV status equation.

This assumption of interviewer random effects gives us the selection equation

$$\begin{aligned} s_{ij}^{*} &= \beta_s x_{ij} + \delta \bar{x}_j + v_j + u_{ij} \\ s_{ij} &= 1 \; if \; s_{ij}^{*} > 0, \; s_{ij} = 0 \; otherwise \end{aligned} \tag{4}$$

In principle we could estimate the system given by (2) and (4) by maximum likelihood. However, this is difficult as we have a selection equation which has a random effect, requiring numerical integration, inside a bivariate probit model.

There is, however, a simple consistent estimator. Dubin and Rivers (1989) show that the bivariate probit model with selection can be estimated by first finding a consistent estimate of the parameters of the selction model, ignoring the covariance of the error terms, and then estimating the parmeters of the full model by maximum likelihood, holding the selection equation paremeters at their first stage estimates. The procedure is as follows. We firstly estimate the interviewer effects from the selection equation only (stage one), then include these constructed parameters as our exclusion restriction in a Heckman model (stage two). This two-stage approach is consistent, though not fully efficient, and the second stage does not produce the correct analytic standard errors since the interviewer effects are estimated in the first stage but treated as exogenous variables in the second stage. Murphy and Topel (1985) discuss confidence intervals for models using data that are estimated from a first stage.

We can implement this approach by first estimating equation (4), the selection equation. Assuming random effects avoids the estimation problems of the fixed effects approach. The assumption that the interviewer effects are normally distributed random effects around $\delta \bar{x}_j$ means we have a smaller set of parameters to estimate than in the fixed effects model. A simple method of constructing consistent estimates of the selection equation for the first stage is to estimate a

probit model and compute the predicted random effect $\hat{v}_j$ as the average of the error term for each interviewer.[7]

We use these selection equation estimates to compute $\hat{z}_j = \hat{\delta}\overline{x}_j + \hat{v}_j$ the estimated interviewer effect, where $\hat{\delta}$ is the estimated coefficient, and $\hat{v}_j$ is the predicted random effect.[8] Having obtained the estimates $\hat{z}_j$, we can then estimate the full bivariate probit model in (1) and (2) using this estimated interviewer effect as the variable that affects selection but does not appear in the HIV status equation.

Since the first stage is a consistent estimator of the interviewer effect, the two-stage procedure will be consistent. We can address the problem of incorrect standard errors by bootstrapping over the whole two-stage procedure.[9] That is to say, for each bootstrap resample we re-estimate the first stage consent, and then run the bivariate probit model with the estimated interviewer effects from the first stage, and calculate the prevalence estimates based on the predicted HIV status of those who refuse to test. For both Zambia and Ghana we compare our estimates of the interviewer effects based on this random effects approach with those found by the fixed effects model.

While the random effects approach solves the convergence problems in Bärnighausen et al. (2011) and allows bootstrapping, a second problem remains. In small samples, or when failure to consent is rare, or the HIV rate is low, it is difficult to estimate the correlation between consent to test and HIV status, and the maximum likelihood estimates can be biased. In these cases the maximum likelihood estimate of ρ often converges to +1 or -1, on the boundary of the possible parameter space. This usually has the implication that in the predicted probabilities everyone who fails to test is either HIV positive with certainty ($\rho = -1$), or HIV negative with certainty ($\rho = +1$), which seems implausible. In these cases other estimators may have better small sample properties than the maximum likelihood estimator (Fosdick and Raftery 2012).

[7] Estimating this first stage as a random effects model is highly computationally intensive and produces almost identical results to the simple probit. The simple probit produces consistent estimates and this is all this is required for the second stage.

[8] Our results are robust to just using $\hat{v}_j$ alone as the interviewer effect rather than the full estimate $\hat{\delta}\overline{x}_j + \hat{v}_j$ .

[9] An alternative would be to calculate corrected standard errors as in Murphy and Topel (1985).

We wish to construct an estimate of $\rho$ that is consistent and also unbiased in small samples. The Bayesian approach has particular appeal within a likelihood framework. Take a data set $x*$ and a parameter vector $\theta*$. The likelihood of $\theta*$ is simply the probability of the data given these parameters:

$$L(\theta*) = P(x*|\theta*) \tag{5}$$

We are not generally concerned with the likelihood of the observed data given the parameters, we are more interested in the probability of the parameters given the observed data. However, by Bayes rule:

$$P(\theta*|x*) = \frac{P(x*|\theta*)P(\theta*)}{P(x*)} \tag{6}$$

Where $P(\theta*)$ is our prior on the parameters. If we have a flat prior so $P(\theta*)$ is the same for every $\theta*$ we have $P(\theta^*|x^*) \propto P(x^*|\theta^*) = L(\theta^*)$, and the maximum likelihood estimate of $\theta*$ is also the estimate that has the highest posterior probability given the data. While this estimate is the most likely parameter value given the data we can construct an alternative Bayesian estimator as

$$\widehat{\theta^*} = E(\theta^*|x^*) = \int \theta^* P(\theta^*|x^*) d\theta^* \tag{7}$$

If the prior is flat we have $P(\theta^*|x^*) \propto L(\theta^*)$, the posterior probability is proportional to the likelihood and we can write our estimator as

$$\widehat{\theta^*} = E(\theta^*|x^*) = \int \theta^* k L(\theta^*) d\theta^* \tag{8}$$

Where $k$ is a normalization factor so that the integral of the likelihood over the parameter space is one and $kL(\theta*)$ is a probability.

This approach essentially takes account of model uncertainty in small samples. The standard maximum likelihood approach chooses the most likely value of $\theta^*$ while our Bayesian approach gives us an average value of $\theta^*$ where we average over different models weighted by the probability of the model being correct. This Bayesian model averaging gives consistent estimates (the likelihood function asymptotically puts zero weight on incorrect parameters) and is clearly an unbiased estimator by construction. It has good small sample properties in Monte Carlo studies (Fosdick and Raftery 2012). This Bayesian model averaging approach may be particularly appropriate where we are interested in predictions, since these predictions take account of uncertainty of the model parameters (Hoeting et al., 1999). This approach is implemented by calculating the likelihood for each value of ρ, and then taking the weighted average of ρ, where the weights are the likelihood values (transformed so that these values integrate to 1).

In principle we could construct Bayesian model average estimates for all the parameters in the model. However, in practice the maximum likelihood estimates for most of the parameters in the model are well determined, and it is only the correlation parameter ρ that really poses problems. We therefore Bayesian model average only over ρ, using a concentrated likelihood for each ρ (see appendix for details). Once ρ is estimated by Bayesian model averaging we find the maximum likelihood values of the other parameters given this value of ρ. We report both the maximum likelihood estimates and our new Bayesian model average estimates together with bootstrapped standard errors and confidence intervals in each case.

As with any Bayesian estimator there is an issue with the choice of a prior distribution. We use a uniform prior for ρ on the interval [-1, 1]. Given our model, we can estimate the HIV status among those who do not consent to test by calculating a predicted probability of being HIV-positive given the model. We can bootstrap the estimated prevalence rate based on these predictions. This approach corrects both for the model uncertainty (each bootstrap replication estimates a new set of parameter values) as well as for sampling uncertainty since the bootstrap replicates the variation in who is sampled due to the survey design. The DHS surveys are carried out within fixed strata representing urban and rural areas of each region. With each stratum, a cluster of households is randomly selected from a set of possible primary sampling units defined by a preceding census. Our bootstrapping takes account of the stratification and cluster randomization of the survey design by drawing fixed number of clusters (the same as in the original data) from each stratum in each sample.

This approach gives us estimates for those who refuse to consent to test. For individuals who appeared on the household roster and were not contacted or interviewed we follow the standard approach to impute HIV status based on the set of reported characteristics of the person (reported by the household respondent). Bärnighausen et al. (2011) also used a selection model to predict the prevalence rate of this group. In principle rather than separate selection mdoels for contact and interview, and consenting to test, we should have a sequential model since requesting a test only occurs after people are interviewed (Clark and Houle 2012a). However both Bärnighausen et al. (2011) and Clark and Houle (2012a) found no evidence of a selection bias among those who were not interviewed, and we simply impute the HIV prevalence for this group based on the assumption they are missing at random and focus on the potential selection bias among those who were interviewed but refused to test.

## 3. Data

We use data from the nationally representative Demographic and Health Surveys of men aged 15-59. We present results from Zambia (2007) and Ghana (2003). The samples consist of 7,134 men in Zambia, and 5,334 men in Ghana. Table 9 in the appendix shows the numbers testing for HIV, the numbers who undertook a DHS interview but refused consent to test for HIV, and the number who did not undertake an individual interview and did not test. The majority of people in the latter group are those who could not be contacted and thus could not be offered interview participation and HIV testing. Further details of the sampling design and HIV testing procedure are provided in Bärnighausen et al. (2011). Descriptive statistics for the individual interview samples for Zambia and Ghana are shown in Tables 10 and 11 in the appendix.

Figure 3 in the appendix shows the distribution of the number of interviews by interviewer for men for Zambia. Approximately 12% (or 750 individuals) had an interviewer with less than 50 interviews in total. This highlights one of the advantages of the random effects approach we adopt in this paper, namely that we are able to estimate an interviewer effect for each of these individuals, even those whose interviewers conducted a single interview. As has been documented previously (Bärnighausen et al., 2011), there is substantial variation in the effectiveness of interviewers, as measured by the proportion of individuals they succeed in obtaining consent for a HIV test from. Figure 4 in the appendix shows the distribution of success rate by interviewer for Zambia. The mean success rate for interviewers is 0.8, with a standard deviation of 0.18. Figures 5 and 6 in the appendix show the distribution of the number of interviews by interviewer and the distribution of interviewer success rate for Ghana (where there is similar variation to Zambia).

## 4. Results

We begin by estimating the interviewer random effects as the average error term (per interviewer) from a probit model for HIV consent where we include a standard set of covariates along with the mean of these variables for each interviewer to capture the effects non-random allocation of interviewers to participants as shown in equation (4). Table 12 in the appendix presents marginal effects from the probit for HIV consent for Zambia. This table is a single regression for HIV consent where the first column shows the coefficients on the individual level X variables, while the corresponding coefficients on interviewer averages are shown in the second column. At the individual level the following variables affect consent to test: education, location, prior sexually transmitted disease (STDs), age at first intercourse, number of partners, willingness to care for a HIV positive relative, knowing an AIDS victim, and being a smoker. For example, an extra year of education increases the probability of consenting to a HIV test by roughly 0.5 percentage points. Apart from years of education, all interviewer averages are measured in percentages. Being in the poorest wealth category, belonging to the wealthiest quintile of households in the sample, the number of partners, smoking and alcohol use are statistically significant in affecting consent. For example, a 10 percentage point increase in those in the interviewer's group who drink alcohol reduces the probability of giving consent by 6.4 percentage points. The distribution of fitted predicted random effects, the average of the error terms for each interviewer, from this model are shown in Figure 7 in the appendix.

Table 1 presents marginal effects for the Heckman selection model consent and HIV equations for Zambia. The first two columns give results for the maximum likelihood fixed effects approach as used by Bärnighausen et al. (2011). The middle two colums give results for our

maximum liklihood random effects approach, and the final two columns give the results for Bayesian model averaging of the random effects model. Controls for age, region, ethnicity, and religion are included but not shown in the table.

The coefficients on the interviewer dummies for each interviewer for the consent equation in the Heckman approach in column 1 are also not shown because of space constraints.[10] In the consent equations for the random effects approach in columns 3 and 5, we report the coefficient on the interviewer effect estimated from the regression shown in Table 12. The first two models in Table 1 are estimated by maximum likelihood. To estimate the third model the Bayesian model average we estimate the likelihood on a grid of values of $\rho$ (we use values between -1 and +1 at intervals of 0.01). We than calculate the likelihood of the model and posterior probability of each value of $\rho$ and the then find the expected value of $\rho$ based on this probability.[11] The coefficients reported in table 1 for the Bayesian Model Average are calculated with this value of $\rho$ imposed. Coefficient estimates across the three models are very similar, and in any case our main parameter of interest when estimating correcting prevalence rates is the correlation between the selection and HIV equations.

In Table 2, we present the estimated correlation coefficient $\rho$ for Zambia from the different models. The negative values estimated indicate that those who refuse to test are more likely to be HIV-positive. The maximum likelihood random effects approaches yields a $\rho$ of around -0.50, which is somewhat lower than the 0-.75 obtained from the fixed effects estimator. The Bayesian Model Average Estimate is slightly smaller yet at -0.44.

[10] See figure 9 in the appendix.
[11] The posterior probability is calculated by applying a constant to the likelihood for each value of ρ such that these transformed likelihoods integrate to 1.

Figure 1 presents the Bayesian model averaging estimator posterior for $\rho$ where we use the random effects exclusion restriction and the concentrated likelihood approach described in section 2. Figure 1 also shows maximum likelihood estimate, the Bayesian model average estimate and the 95% bootstrap confidence interval for the Bayesian model average estimate. The 95% bootstrap confidence interval is calculated using the appropriate centiles from the empirical distribution of the bootstrap estimates. This approach is more appropriate than normal-based approximations when the distribution of the parameter of interest is skewed.

Table 3 presents estimates of HIV prevalence[12] among those who refused to consent to a HIV test, again comparing the fixed effect, random effect and Bayesian model average methods. As with the correlation coefficient, the estimates from the random effects model are lower than from the fixed effects model (32% v 52%). The corresponding estimate from the imputation model is 12%. The standard confidence intervals of these prevalence estimates do not take account of the uncertainty in the estimation of interviewer effects, and therefore overstate the precision of the point estimates for $\rho$ (Murphy and Topel, 1985). The bootstrap confidence interval for the random effects model is almost 10 times as wide as the analytic standard errors for the fixed effects model. Our random effects procedure allows us to implement a bootstrap where we re-estimate the random effects with each iteration to take account of this additional source of parameter uncertainty. We use 100 iterations to calculate these standard errors, and use the empirical distribution of the replications to calculate the confidence intervals in order to allow for asymmetry. As discussed above, it is not possible to bootstrap standard errors in the fixed effects model due to the difficulties associated with identifying interviewer effects for interviewers with a small number of interviews, which would change with each sample.

[12] All of our prevalence estimates are weighted and take account of survey design.

Table 4 presents the resulting population estimates. We also compare these results with the estimates from an imputation model, and show the prevalence among those who participated in HIV testing. The imputation model generates an estimate of 12%, which is substantially smaller than the 20% estimate from the fixed effects model. The random effects and Bayesian Model Average models give estimates that fall in between imputation and the fixed effects model (around 16%). Comparing the bootstrap and analytic standard errors again highlights how the precision of these estimates is greatly overstated with standard approaches which do not account for uncertainty in the estimation of $\rho$.

We next turn to estimating the models for men in Ghana, where the standard maximum likelihood estimator is difficult to implement because of the low HIV prevalence. In addition, the low sample size may induce bias in conventional maximum likelihood estimates which rely on asymptotic properties. Table 13 in the appendix presents marginal effects for the selection model used to calculate the interviewer effects and Table 5 gives the results of the three models for Ghana. As with Zambia, we compare results from the three approaches.[13]

Figure 2 demonstrates that for men in Ghana the concentrated likelihood function is monotonic in $\rho$, resulting in maximum likelihood estimates which are close to the boundary of the parameter space. We integrate over the posterior to obtain an unbiased estimate for $\rho$. We obtain a value of around 0.6, indicating that individuals with HIV are more likely to consent to testing in Ghana, at least for men.[14] Tables 6 shows our estimate of $\rho$ our confidence intervals using each model. The

[13] Coefficients on the interviewer fixed effects in the relevant consent equation for Ghana are presented in table 10 in the appendix.

[14] The fact that the random effects estimate lies outside the bootstrap confidence interval for the Bayesian model average estimate reflects the fact that the posterior distribution has a long left hand tail which is not accounted for by the standard maximum likelihood estimator, and that we use the empirical distribution of the bootstrap estimates to allow for asymmetry when calculating the confidence interval.

positive value of $\rho$ means that those who are HIV positive are more likely to test, and our population prevalence estimates are lower than those obtained from imputation methods. Tables 7 and 8 report our prevalence estimates for men who refuse to test and for the whole population. For the random effects model and the Bayesian model averaging approach we find a prevalence rate of approximately 1.4%, compared to 1.6% in the case of the imputation model.

## 5. Discussion and conclusions

This paper confirms that non-response can be an important source of bias in HIV prevalence estimates taken from household surveys. We innovate by proposing a random effects model which improves on previous fixed effects approaches by allowing us to estimate interviewer effects, even for interviewers with a limited number of interviews. In addition, we propose a Bayesian averaging procedure which facilitates the estimation of the correlation between HIV consent and HIV status, and allows for unbiased estimates in small samples. Using data from the 2007 Zambian Demographic and Health Survey, we find that men with HIV are less likely to consent to a HIV test. For Ghana, we find that conventional methods slightly overstate HIV prevalence.

Perhaps the most important result we find is that the corrected confidence intervals around the HIV prevalence point estimate can be very wide. These wide confidence intervals accurately reflect the fact that it is very difficult to correct statically for the bias that may occur when many people refuse to test. As long as consent rates are as low as they currently tend to be in many nationally representative HIV surveys in developing countries, uncertainty in HIV prevalence estimation will likely remain high. It is important not to understate this uncertainty and our

approach provides the first practical solution to account for both sampling and parameter uncertainty in the estimation of HIV prevalence confidence intervals.

A key assumption in the model is the exclusion restriction, which requires interviewer identity to be uncorrelated with HIV status. While it is plausible that interviewer allocation should only be affected by survey design, this is impossible to prove conclusively. As the resulting HIV prevalence prediction relates to a population for whom we never get to observe true HIV status, it is important to independently validate the model. We are therefore working to obtain objective data in the form of mortality records with which we can do so. This limitation, coupled with the wide confidence intervals we find, points towards an urgent need to improve HIV survey design and execution so as to increase the consent rates to reduce the uncertainty in HIV prevalence estimates that is induced by selection bias.

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## Tables

**Table 1 Marginal Effects for Heckman Selection Models (Men Zambia 2007)**

| | Maximum Likelihood Fixed Effects | | Maximum Likelihood Random Effects | | Bayesian Model Average Random Effects | |
|---|---|---|---|---|---|---|
| Variables | Consent Equation | HIV Equation | Consent Equation | HIV Equation | Consent Equation | HIV Equation |
| Interviewer Effect | | | | 0.2845*** | | 0.2857*** |
| | | | | (0.028) | | (0.028) |
| Years of Education | 0.0046** | 0.0012 | 0.0046** | 0.0019 | 0.0047** | 0.0020 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| **Wealth Category** | | | | | | |
| Poorest | -0.0172 | 0.0486** | -0.0199 | 0.0463** | -0.0198 | 0.0451** |
| | (0.020) | (0.020) | (0.020) | (0.018) | (0.020) | (0.018) |
| Poorer | -0.0262 | 0.0528*** | -0.0299 | 0.0479*** | -0.0298 | 0.0465*** |
| | (0.021) | (0.020) | (0.021) | (0.018) | (0.021) | (0.018) |
| Middle | 0.0028 | 0.0645*** | -0.0017 | 0.0630*** | -0.0017 | 0.0620*** |
| | (0.025) | (0.024) | (0.024) | (0.022) | (0.024) | (0.021) |
| Richer | 0.0055 | 0.0503* | 0.0072 | 0.0493* | 0.0071 | 0.0485* |
| | (0.031) | (0.028) | (0.030) | (0.025) | (0.030) | (0.025) |
| **Location** | | | | | | |
| Small City | 0.1529** | -0.0589 | 0.1626** | -0.0351 | 0.1622** | -0.0308 |
| | (0.067) | (0.045) | (0.064) | (0.035) | (0.064) | (0.032) |
| Town | 0.1605*** | -0.0764** | 0.1649*** | -0.0495* | 0.1647*** | -0.0449* |
| | (0.061) | (0.038) | (0.059) | (0.027) | (0.059) | (0.024) |
| Countryside | 0.1911*** | -0.1289*** | 0.1972*** | -0.0956*** | 0.1967*** | -0.0896*** |
| | (0.063) | (0.039) | (0.059) | (0.028) | (0.059) | (0.024) |
| **Marital Status** | | | | | | |
| Currently Married | 0.0105 | 0.0787*** | 0.0220 | 0.0791*** | 0.0221 | 0.0779*** |
| | (0.029) | (0.030) | (0.028) | (0.027) | (0.028) | (0.027) |
| Formerly Married | -0.0146 | 0.2125*** | -0.0093 | 0.1992*** | -0.0096 | 0.1950*** |
| | (0.028) | (0.026) | (0.028) | (0.025) | (0.028) | (0.023) |
| Had STD | 0.0407 | 0.1079*** | 0.0437* | 0.1073*** | 0.0437* | 0.1064*** |
| | (0.025) | (0.023) | (0.025) | (0.019) | (0.025) | (0.019) |
| **Age at First Intercourse** | | | | | | |
| 15 or Younger | 0.0502** | -0.0422 | 0.0485** | -0.0254 | 0.0487** | -0.0228 |
| | (0.022) | (0.035) | (0.021) | (0.031) | (0.021) | (0.030) |
| >15 | 0.0374* | -0.0475 | 0.0356* | -0.0334 | 0.0357* | -0.0312 |
| | (0.022) | (0.034) | (0.021) | (0.030) | (0.021) | (0.030) |
| Had High Risk Sex | 0.0531* | -0.0275 | 0.0546** | -0.0199 | 0.0544** | -0.0184 |
| | (0.027) | (0.025) | (0.027) | (0.022) | (0.027) | (0.021) |
| **Number of Partners** | | | | | | |
| 1 | -0.0433 | -0.0202 | -0.0520* | -0.0266 | -0.0521* | -0.0272 |
| | (0.027) | (0.029) | (0.027) | (0.026) | (0.027) | (0.025) |
| 2+ | -0.0259 | 0.0343 | -0.0371 | 0.0293 | -0.0371 | 0.0282 |
| | (0.042) | (0.039) | (0.042) | (0.034) | (0.042) | (0.033) |

| | | | | | | |
|---|---|---|---|---|---|---|
| Condom Last Intercourse | -0.0079 | 0.0629*** | -0.0017 | 0.0586*** | -0.0014 | 0.0575*** |
| | (0.016) | (0.015) | (0.015) | (0.014) | (0.015) | (0.013) |
| Would Care for HIV Relative | 0.0487** | 0.0170 | 0.0524*** | 0.0285 | 0.0525*** | 0.0298 |
| | (0.020) | (0.033) | (0.020) | (0.030) | (0.020) | (0.029) |
| Know Someone Who Died of AIDS | 0.0369*** | -0.0144 | 0.0370*** | -0.0081 | 0.0370*** | -0.0070 |
| | (0.010) | (0.014) | (0.010) | (0.011) | (0.010) | (0.011) |
| Previously HIV Tested | 0.0053 | 0.0340*** | 0.0050 | 0.0333*** | 0.0050 | 0.0328*** |
| | (0.013) | (0.013) | (0.013) | (0.011) | (0.013) | (0.011) |
| Smoker | 0.0371*** | -0.0262* | 0.0371*** | -0.0197 | 0.0370*** | -0.0183 |
| | (0.013) | (0.016) | (0.013) | (0.013) | (0.013) | (0.013) |
| Drinks Alcohol | 0.0097 | 0.0186 | 0.0106 | 0.0184* | 0.0105 | 0.0182* |
| | (0.012) | (0.012) | (0.012) | (0.011) | (0.012) | (0.011) |
| **Language** | | | | | | |
| English | 0.0276 | 0.0028 | 0.0350 | 0.0075 | 0.0351 | 0.0081 |
| | (0.028) | (0.033) | (0.029) | (0.029) | (0.029) | (0.029) |
| Bemba | 0.0953 | 0.0666 | 0.0916 | 0.0816* | 0.0919 | 0.0826** |
| | (0.061) | (0.049) | (0.058) | (0.042) | (0.058) | (0.041) |
| Lozi | 0.0980*** | -0.0076 | 0.0967*** | 0.0079 | 0.0965*** | 0.0099 |
| | (0.030) | (0.031) | (0.028) | (0.026) | (0.028) | (0.025) |
| Nyanja | 0.1017** | -0.0177 | 0.1133*** | 0.0035 | 0.1132*** | 0.0066 |
| | (0.046) | (0.043) | (0.036) | (0.035) | (0.036) | (0.034) |
| Tonga | 0.0376 | -0.0119 | 0.0453 | -0.0132 | 0.0452 | -0.0128 |
| | (0.048) | (0.064) | (0.051) | (0.063) | (0.051) | (0.062) |
| Observations | 6,416 | 6,416 | 6,416 | 6,416 | 6,416 | 6,416 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

**Note to table 1:** Controls for age, region, ethnicity, and religion are included in each regression equation but not shown. The model is a bivariate probit for HIV status and consent to a HIV test. Data are for men who completed an interview. The first model uses interviewer fixed effects as the exclusion restriction. The coefficients from the interviewer effects are not shown in the table. The second model uses the two step random effects procedure where HIV consent is regressed on the X variables outlined in table 2, along with the mean values for each interviewer. The average error term for each interviewer is added to the predicted value of the interviewer means, and used as the exclusion restriction in the HIV regression. The final model is the Bayesian Model Averaging procedure using the same exclusion restriction. Coefficients are obtained by restricting the value of RHO to its Bayesian estimate and running the bivariate probit. Source: DHS Zambia 2007 (men).

**Table 2 Correlation Coefficient Estimates for Men in Zambia**

| Correlation Coefficient for Refused Consent | Parameter Value | Analytic 95% CI | | Bootstrap 95% CI | |
|---|---|---|---|---|---|
| Fixed Effects Model | -0.7527 | -0.9397 | -0.2189 | | |
| Random Effects Model | -0.5018 | -0.7267 | -0.1777 | -0.6769 | 0.2282 |
| Bayesian Model Average Model | -0.4396 | | | -0.6378 | 0.4319 |

**Note to table 2**: The table shows the estimated correlation coefficient between consent and HIV status for the fixed effects, random effects and Bayesian Model Average models. Analytic standard errors are shown for the fixed effects and random effects models, with bootstrapped errors for random effects and Bayesian Model Average models. Also shown is the HIV rate using an imputation model for men who refused consent. Source: DHS Zambia 2007 (men).

**Table 3 HIV Rate among Men who Refused to Test in Zambia**

| HIV Rate for Refused Consent | Parameter Value | Analytic 95% CI | | Bootstrap 95% CI | |
|---|---|---|---|---|---|
| Fixed Effects Model | 0.5195 | 0.4987 | 0.5404 | | |
| Random Effects Model | 0.3203 | 0.3031 | 0.3376 | 0.0627 | 0.4268 |
| Bayesian Model Average Model | 0.2860 | 0.2656 | 0.2977 | 0.0317 | 0.3977 |
| Imputation Model | 0.1171 | 0.1078 | 0.1263 | | |

**Note to table 3**: The table shows the estimated HIV prevalence rate among individuals who refused consent for the fixed effects, random effects and Bayesian Model Average models. Analytic standard errors are shown for the fixed effects and random effects models, with bootstrapped errors for random effects and Bayesian Model Average models. Source: DHS Zambia 2007 (men).

**Table 4 HIV Rate among Men in Zambia**

| HIV Rate | Parameter Value | Analytic 95% CI | | Bootstrap 95% CI | |
|---|---|---|---|---|---|
| All Men - Fixed Effects Model | 0.2013 | 0.1896 | 0.2130 | | |
| All Men - Random Effects Model | 0.1627 | 0.1526 | 0.1728 | 0.1096 | 0.1842 |
| All Men - Bayesian Model Average Model | 0.1552 | 0.1454 | 0.1650 | 0.1024 | 0.1789 |
| | | | | | |
| Men with Non-Missing Data | 0.1213 | 0.1099 | 0.1327 | | |
| Men with No Contact - Imputation Model | 0.1525 | 0.1424 | 0.1626 | | |
| All Men - Imputation Model | 0.1233 | 0.1144 | 0.1322 | | |

**Note to table 4**: The table shows the estimated HIV prevalence rate among the population for the fixed effects, random effects and Bayesian Model Average models. Analytic standard errors are shown for the fixed effects and random effects models, with bootstrapped errors for random effects and Bayesian Model Average models. Results from an imputation model are also shown, along with estimates only using those without missing data. Source: DHS Zambia 2007 (men).

**Table 5 Marginal Effects for Heckman Selection Models (Ghana 2003)**

| | Maximum Likelihood Fixed Effects | | Maximum Likelihood Random Effects | | Bayesian Model Average Random Effects | |
|---|---|---|---|---|---|---|
| Variables | Consent Equation | HIV Equation | Consent Equation | HIV Equation | Consent Equation | HIV Equation |
| Interviewer Effect | | | | 0.2125*** | | 0.2120*** |
| | | | | (0.020) | | (0.020) |
| Years of Education | 0.0020 | 0.0004 | 0.0023* | 0.0004 | 0.0023* | 0.0004 |
| | (0.001) | (0.000) | (0.001) | (0.000) | (0.001) | (0.000) |
| **Wealth Category** | | | | | | |
| Poorest | -0.0187 | 0.0039 | -0.0184 | 0.0039 | -0.0184 | 0.0040 |
| | (0.020) | (0.005) | (0.019) | (0.005) | (0.019) | (0.005) |
| Poorer | -0.0563*** | 0.0078 | -0.0553*** | 0.0078 | -0.0554*** | 0.0079 |
| | (0.020) | (0.006) | (0.020) | (0.006) | (0.020) | (0.006) |
| Middle | -0.0747*** | 0.0017 | -0.0769*** | 0.0017 | -0.0769*** | 0.0018 |
| | (0.021) | (0.006) | (0.021) | (0.006) | (0.021) | (0.006) |
| Richer | -0.1139*** | 0.0021 | -0.1169*** | 0.0021 | -0.1171*** | 0.0023 |
| | (0.024) | (0.007) | (0.023) | (0.007) | (0.023) | (0.007) |
| **Marital Status** | | | | | | |
| Currently Married | 0.0241 | 0.0128 | 0.0235 | 0.0128 | 0.0237 | 0.0128 |
| | (0.022) | (0.008) | (0.022) | (0.008) | (0.022) | (0.008) |
| Formerly Married | -0.0382 | 0.0088 | -0.0420* | 0.0087 | -0.0418* | 0.0088 |
| | (0.024) | (0.008) | (0.024) | (0.008) | (0.024) | (0.008) |
| Had STD | 0.0461 | 0.0093 | 0.0457 | 0.0094 | 0.0456 | 0.0094 |
| | (0.029) | (0.007) | (0.028) | (0.007) | (0.028) | (0.007) |
| **Age at First Intercourse** | | | | | | |
| 15 or Younger | 0.0502** | -0.0422 | 0.0485** | -0.0254 | 0.0487** | -0.0228 |
| | (0.022) | (0.035) | (0.021) | (0.031) | (0.021) | (0.030) |
| >15 | 0.0374* | -0.0475 | 0.0356* | -0.0334 | 0.0357* | -0.0312 |
| | (0.022) | (0.034) | (0.021) | (0.030) | (0.021) | (0.030) |
| Had High Risk Sex | 0.0531* | -0.0275 | 0.0546** | -0.0199 | 0.0544** | -0.0184 |
| | (0.027) | (0.025) | (0.027) | (0.022) | (0.027) | (0.021) |
| **Number of Partners** | | | | | | |
| 1 | 0.0040 | -0.0100 | 0.0033 | -0.0100 | 0.0032 | -0.0101 |
| | (0.022) | (0.007) | (0.022) | (0.007) | (0.022) | (0.007) |
| 2+ | -0.0064 | -0.0178* | -0.0068 | -0.0180* | -0.0069 | -0.0180* |
| | (0.031) | (0.011) | (0.030) | (0.011) | (0.031) | (0.011) |
| Condom Last Intercourse | 0.0001 | 0.0083* | -0.0000 | 0.0082* | -0.0000 | 0.0083* |
| | (0.018) | (0.004) | (0.018) | (0.004) | (0.018) | (0.004) |
| Would Care for HIV Relative | 0.0221* | -0.0007 | 0.0230* | -0.0007 | 0.0229* | -0.0007 |
| | (0.013) | (0.004) | (0.012) | (0.004) | (0.012) | (0.004) |
| Know Someone Who Died of AIDS | 0.0163 | -0.0009 | 0.0162 | -0.0010 | 0.0161 | -0.0009 |
| | (0.011) | (0.003) | (0.011) | (0.003) | (0.011) | (0.003) |
| Previously HIV Tested | 0.0040 | -0.0054 | 0.0046 | -0.0054 | 0.0049 | -0.0056 |
| | (0.018) | (0.007) | (0.018) | (0.007) | (0.018) | (0.007) |
| Smoker | -0.0103 | 0.0057 | -0.0089 | 0.0057 | -0.0089 | 0.0058 |
| | (0.017) | (0.004) | (0.017) | (0.004) | (0.017) | (0.004) |

| | | | | | | |
|---|---|---|---|---|---|---|
| **Language** | | | | | | |
| Akan | 0.0474*** | 0.0042 | 0.0513*** | 0.0043 | 0.0511*** | 0.0044 |
| | (0.017) | (0.004) | (0.016) | (0.004) | (0.016) | (0.004) |
| Ga | 0.0766* | 0.0009 | 0.0864** | 0.0010 | 0.0858** | 0.0010 |
| | (0.045) | (0.013) | (0.044) | (0.013) | (0.044) | (0.013) |
| Ewe | 0.0338 | 0.0122 | 0.0272 | 0.0122 | 0.0271 | 0.0123 |
| | (0.045) | (0.009) | (0.045) | (0.009) | (0.045) | (0.009) |
| Nzema | 0.0537 | 0.0253* | 0.0484 | 0.0253* | 0.0489 | 0.0254* |
| | (0.060) | (0.015) | (0.052) | (0.015) | (0.052) | (0.015) |
| Dagbani | -0.0201 | -0.1142*** | 0.0187 | -0.1387*** | 0.0186 | -0.8298*** |
| | (0.063) | (0.018) | (0.038) | (0.021) | (0.038) | (0.084) |
| Other | -0.0391 | -0.0102 | -0.0327 | -0.0102 | -0.0328 | -0.0102 |
| | (0.027) | (0.009) | (0.025) | (0.009) | (0.025) | (0.009) |
| Observations | 4,955 | 4,955 | 4,955 | 4,955 | 4,955 | 4,955 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

**Note to table 5:** Controls for age, region, ethnicity, and religion are included in each regression equation but not shown. The model is a bivariate probit for HIV status and consent to a HIV test. Data are for men only. The first model uses interviewer fixed effects as the exclusion restriction. The second model uses the two step random effects procedure where HIV consent is regressed on the X variables outlined in table 3, along with the mean values for each interviewer. The average error term for each interviewer is added to the predicted value of the interviewer means, and used as the exclusion restriction in the HIV regression. The final model is the Bayesian Model Averaging procedure using the same exclusion restriction. Coefficients are obtained by restricting the value of RHO to its Bayesian estimate and running the bivariate probit. Source: DHS Ghana 2003 (men).

**Table 6 Correlation Coefficient Estimates for Men in Ghana**

| Correlation Coefficient for Refused Consent | Parameter Value | Analytic 95% CI | | Bootstrap 95% CI | |
|---|---|---|---|---|---|
| Fixed Effects Model | 0.9452 | -0.0514 | 0.9986 | | |
| Random Effects Model | 0.9332 | 0.0197 | 0.9978 | 0.5603 | 0.9999 |
| Bayesian Model Average Model | 0.5927 | | | 0.3856 | 0.7184 |

**Note to table 6**: The table shows the estimated correlation coefficient between consent and HIV status for the fixed effects, random effects and Bayesian Model Average models. Analytic standard errors are shown for the fixed effects and random effects models, with bootstrapped errors for random effects and Bayesian Model Average models. Source: DHS Ghana 2003 (men).

**Table 7 HIV Rate among Men who Refused to Test in Ghana**

| HIV Rate for Refused Consent | Parameter Value | Analytic 95% CI | | Bootstrap 95% CI | |
|---|---|---|---|---|---|
| Fixed Effects Model | 0.0000 | 0.0000 | 0.0000 | | |
| Random Effects Model | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0016 |
| Bayesian Model Average Model | 0.0007 | 0.0005 | 0.0009 | 0.0003 | 0.0035 |
| Imputation Model | 0.0182 | 0.0160 | 0.2048 | | |

**Note to table 7**: The table shows the estimated HIV prevalence rate among individuals who refused consent for the fixed effects, random effects and Bayesian Model Average models. Analytic standard errors are shown for the fixed effects and random effects models, with bootstrapped errors for random effects and Bayesian Model Average models. Source: DHS Ghana 2003 (men).

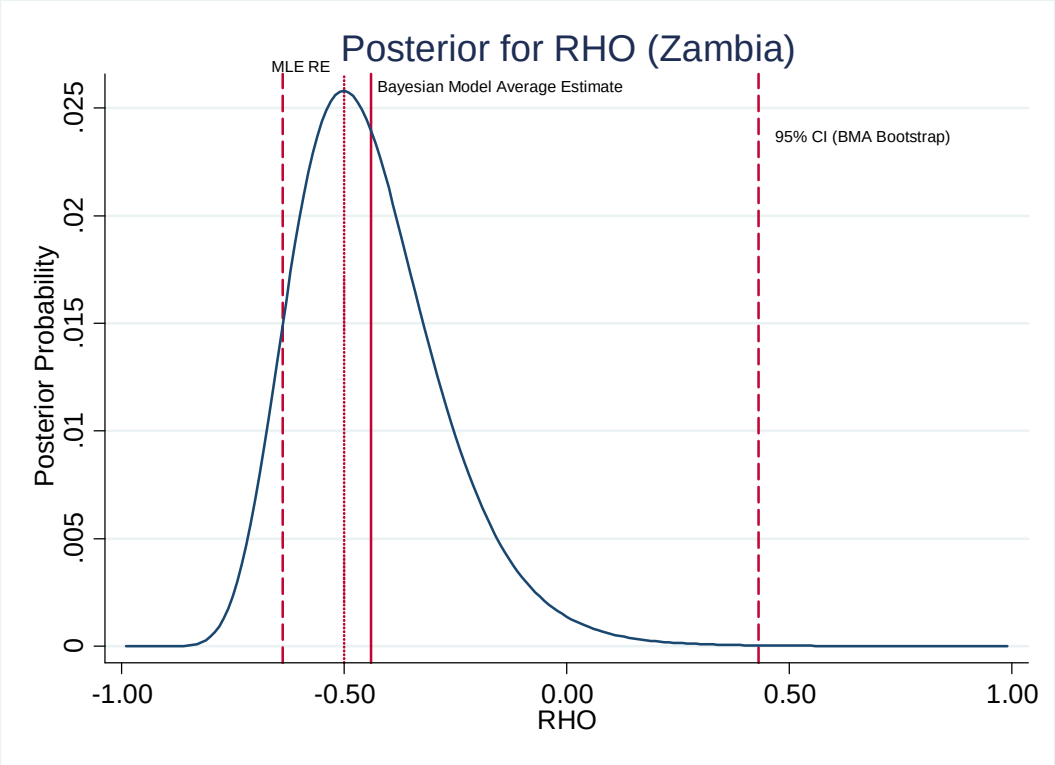
**Table 8 HIV Rate among Men in Ghana**

| HIV Rate | Parameter Value | Analytic 95% CI | | Bootstrap 95% CI | |
|---|---|---|---|---|---|
| All Men - Fixed Effects Model | 0.0138 | 0.0105 | 0.0171 | | |
| All Men - Random Effects Model | 0.0138 | 0.0105 | 0.0171 | 0.0116 | 0.0162 |
| All Men - Bayesian Model Average Model | 0.0139 | 0.0106 | 0.0172 | 0.0115 | 0.0163 |
| | | | | | |
| Men with Non-Missing Data | 0.0161 | 0.0120 | 0.0202 | | |
| Men with no contact - Imputation Model | 0.0157 | 0.0137 | 0.0177 | | |
| All Men - Imputation Model | 0.0164 | 0.0131 | 0.0197 | | |

**Note to table 8**: The table shows the estimated HIV prevalence rate among the population for the fixed effects, random effects and Bayesian Model Average models. Analytic standard errors are shown for the fixed effects and random effects models, with bootstrapped errors for random effects and Bayesian Model Average models. Results from an imputation model are also shown, along with estimates only using those without missing data. Source: DHS Ghana 2003 (men).

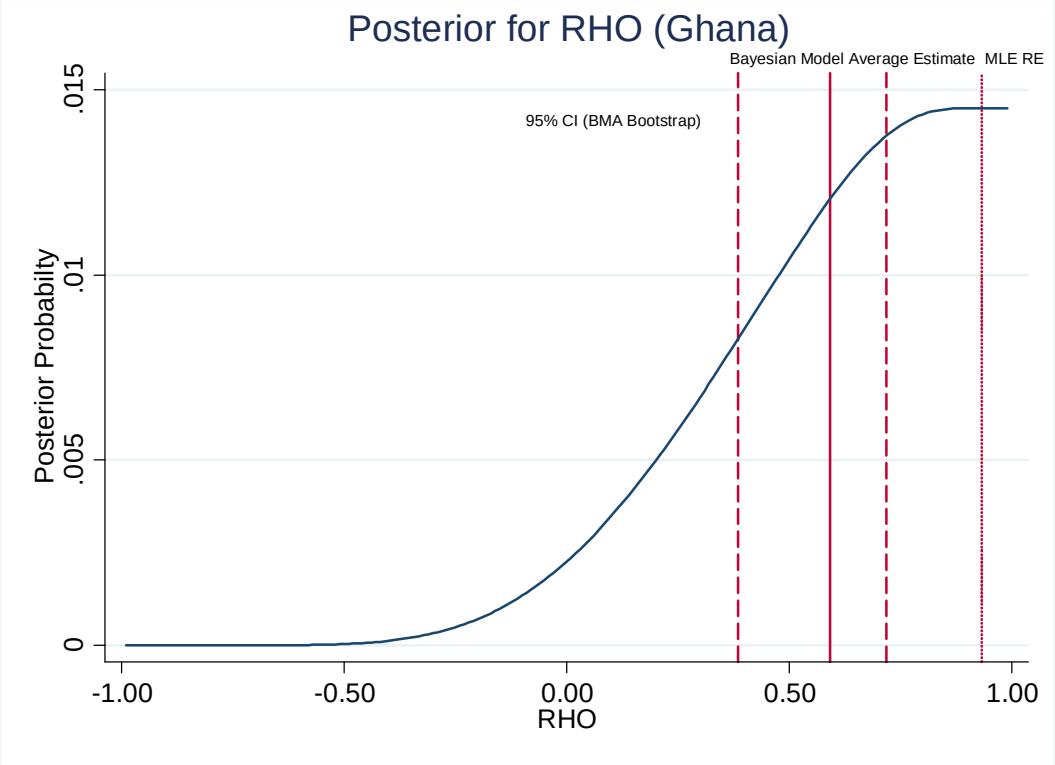
## Figures

**Figure 1 Bayesian Model Averaging Posterior for RHO Zambia 2007 (Men)**




**Note to Figure 1**: Graph shows the posterior for RHO calculated using Bayesian model averaging using interviewer random effects as the exclusion restriction. Bayesian Model Averaging is performed over selection models with interviewer effects as the exclusion restriction. Random effects are obtained as the average error from a probit for consent. Also shown are the 95% bootstrapped confidence interval for the Bayesian Model Average estimate, and the point estimate for the random effects maximum likelihood estimate for comparison. The bootstrap confidence interval is calculated using the empirical distribution of bootstrap estimates. Source: DHS Zambia 2007 (men).

**Figure 2 Bayesian Model Averaging Posterior for RHO Ghana 2003 (Men)**




**Note to Figure 2**: Graph shows the posterior for RHO calculated using Bayesian model averaging using interviewer random effects as the exclusion restriction. Bayesian Model Averaging is performed over selection models with interviewer effects as the exclusion restriction. Random effects are obtained as the average error from a probit for consent. Also shown are the 95% bootstrapped confidence interval for the Bayesian Model Average estimate, and the point estimate for the random effects maximum likelihood estimate for comparison. The bootstrap confidence interval is calculated using the empirical distribution of bootstrap estimates. Source: DHS Ghana 2003 (men).

## Appendix

**Table 9 Sample Size for HIV Estimation**

| | Zambia | Ghana |
|---|---|---|
| Observed HIV Status | 5,163 | 4,271 |
| Missing HIV Status (Consent Refused) | 1,318 | 743 |
| Missing HIV Status (No Contact) | 653 | 320 |
| Total | 7,134 | 5,334 |

**Table 10 Descriptive Statistics (Eligible Men) for Zambia DHS 2007 (Individual Sample)**

| | No. | % | | No. | % |
|---|---|---|---|---|---|
| **Age Category** | | | **Would Respondent Care for HIV Relative?** | | |
| 15-19 | 1,410 | 21.69 | No | 347 | 5.34 |
| 20-24 | 1,071 | 16.48 | Yes | 6,146 | 94.66 |
| 25-29 | 988 | 15.2 | *Total* | 6,493 | 100 |
| 30-34 | 933 | 14.35 | | | |
| | | | **Does Respondent Know Anyone Who Died of AIDS?** | | |
| 35-39 | 733 | 11.28 | | | |
| 40-44 | 471 | 7.25 | No | 2,866 | 44.19 |
| 45-49 | 397 | 6.11 | Yes | 3,619 | 55.81 |
| 50-54 | 294 | 4.52 | *Total* | 6,485 | 100 |
| 55-59 | 203 | 3.12 | | | |
| *Total* | 6,500 | 100 | **Ever Had HIV Test** | | |
| | | | No | 4,986 | 76.75 |
| **Years of Education** | | | Yes | 1,510 | 23.25 |
| 0 | 309 | 4.77 | *Total* | 6,496 | 100 |
| 1-7 | 3,118 | 48.14 | | | |
| 8+ | 3,050 | 47.09 | **Smoker** | | |
| *Total* | 6,477 | 100 | No | 4,951 | 76.25 |
| | | | Yes | 1,542 | 23.75 |
| **Wealth Quintile** | | | *Total* | 6,493 | 100 |
| Poorest | 1,145 | 17.62 | | | |
| 2nd poorest | 963 | 14.82 | **Alcohol Drinker** | | |
| 3rd poorest | 1,315 | 20.23 | No | 3,876 | 59.69 |
| 4th poorest | 1,600 | 24.62 | Yes | 2,618 | 40.31 |
| Wealthiest | 1,477 | 22.72 | *Total* | 6,494 | 100 |
| *Total* | 6,500 | 100 | | | |
| | | | **Ethnicity** | | |
| **Location** | | | Bemba | 1,117 | 17.18 |
| Capital, large city | 597 | 9.18 | Lunda (Luapula) | 152 | 2.34 |
| Small city | 469 | 7.22 | Lala | 166 | 2.55 |
| Town | 1,765 | 27.15 | Ushi | 149 | 2.29 |
| Countryside | 3,669 | 56.45 | Lamba | 121 | 1.86 |
| *Total* | 6,500 | 100 | Tonga | 766 | 11.78 |
| | | | Luvale | 160 | 2.46 |
| **Region** | | | Lunda (Northwestern) | 311 | 4.78 |
| Central | 600 | 9.23 | Mbunda | 124 | 1.91 |
| Copperbelt | 812 | 12.49 | Kaonde | 249 | 3.83 |
| Eastern | 857 | 13.18 | Lozi | 477 | 7.34 |
| Luapula | 560 | 8.62 | Chewa | 488 | 7.51 |
| Lusaka | 962 | 14.8 | Nsenga | 313 | 4.82 |

| | | |
|---|---|---|
| Northern | 715 | 11 |
| Northwestern | 630 | 9.69 |
| Southern | 774 | 11.91 |
| Western | 590 | 9.08 |
| *Total* | 6,500 | 100 |
| | | |
| **Marital Status** | | |
| Never married | 2,546 | 39.17 |
| Currently married | 3,630 | 55.85 |
| Formerly married | 324 | 4.98 |
| *Total* | 6,500 | 100 |
| | | |
| **Ever Had STD** | | |
| No | 6,165 | 95.09 |
| Yes | 318 | 4.91 |
| *Total* | 6,483 | 100 |
| | | |
| **Age at First Intercourse** | | |
| Never Had Sex | 908 | 13.98 |
| 15 or Younger | 2,016 | 31.04 |
| >15 | 3,571 | 54.98 |
| *Total* | 6,495 | 100 |
| | | |
| **Had High Risk Sex** | | |
| No | 4,707 | 72.62 |
| Yes | 1,775 | 27.38 |
| *Total* | 6,482 | 100 |
| | | |
| **Used Condom Last Intercourse** | | |
| No | 5,380 | 82.9 |
| Yes | 1,110 | 17.1 |
| Total | 6,490 | 100 |

| | | |
|---|---|---|
| Ngoni | 323 | 4.97 |
| Mambwe | 183 | 2.82 |
| Namwanga | 176 | 2.71 |
| Tumbuka | 295 | 4.54 |
| Other | 930 | 14.31 |
| *Total* | 6,500 | 100 |
| | | |
| **Language** | | |
| English | 465 | 7.15 |
| Bemba | 2,374 | 36.52 |
| Lozi | 611 | 9.4 |
| Nyanja | 1,955 | 30.08 |
| Tonga | 482 | 7.42 |
| Other | 613 | 9.43 |
| *Total* | 6,500 | 100 |
| | | |
| **Number of Partners** | | |
| None | 1,595 | 24.61 |
| 1 | 3,921 | 60.49 |
| 2+ | 966 | 14.9 |
| *Total* | 6,482 | 100 |
| | | |
| **Religion** | | |
| Catholic | 1,347 | 20.76 |
| Protestant | 4,932 | 75.99 |
| Muslim | 211 | 3.25 |
| *Total* | 6,490 | 100 |

**Table 11 Descriptive Statistics (Eligible Men) for Ghana DHS 2003 (Individual Sample)**

| | No. | % |
|---|---|---|
| **Age Category** | | |
| 15-19 | 1,096 | 21.85 |
| 20-24 | 697 | 13.9 |
| 25-29 | 715 | 14.26 |
| 30-34 | 632 | 12.6 |
| 35-39 | 526 | 10.49 |
| 40-44 | 407 | 8.12 |
| 45-49 | 435 | 8.67 |
| 50-54 | 302 | 6.02 |
| 55-59 | 205 | 4.09 |
| Total | 5,015 | 100 |
| **Years of Education** | | |
| 0 | 1,178 | 23.55 |
| 1-7 | 1,110 | 22.19 |
| 8+ | 2,714 | 54.26 |
| *Total* | 5,002 | 100 |
| **Wealth Quintile** | | |
| Poorest | 1,221 | 24.35 |
| Poorer | 953 | 19 |
| Middle | 883 | 17.61 |
| Richer | 906 | 18.07 |
| Richest | 1,052 | 20.98 |
| *Total* | 5,015 | 100 |
| **Region** | | |
| Western | 457 | 9.11 |
| Central | 300 | 5.98 |
| Greater Accra | 621 | 12.38 |
| Volta | 386 | 7.7 |
| Eastern | 453 | 9.03 |
| Ashanti | 785 | 15.65 |
| Brong Ahafo | 593 | 11.82 |
| Northern | 638 | 12.72 |
| Upper West | 387 | 7.72 |
| Upper East | 395 | 7.88 |
| *Total* | 5,015 | 100 |

| | No. | % |
|---|---|---|
| **Number of Partners** | | |
| None | 1,707 | 34.07 |
| 1 | 2,824 | 56.37 |
| 2+ | 479 | 9.56 |
| *Total* | 5,010 | 100 |
| **Used Condom Last Intercourse** | | |
| No | 4,451 | 88.86 |
| Yes | 558 | 11.14 |
| *Total* | 5,009 | 100 |
| **Respondent Would Care for HIV Relative** | | |
| No | 1,389 | 27.71 |
| Yes | 3,624 | 72.29 |
| *Total* | 5,013 | 100 |
| **Respondent Knows Anyone Who Died of AIDS** | | |
| No | 3,052 | 60.97 |
| Yes | 1,954 | 39.03 |
| *Total* | 5,006 | 100 |
| **Ever Had HIV Test** | | |
| No | 4,580 | 91.34 |
| Yes | 434 | 8.66 |
| *Total* | 5,014 | 100 |
| **Religion** | | |
| None | 358 | 7.14 |
| Roman Catholic | 794 | 15.84 |
| Anglican | 47 | 0.94 |
| Methodist | 301 | 6 |
| Presbyterian | 361 | 7.2 |
| Other Christian | 1,785 | 35.61 |
| Moslem | 1,050 | 20.95 |
| Traditional/Spiritualist | 317 | 6.32 |
| *Total* | 5,013 | 100 |

| | | | | | |
|---|---|---|---|---|---|
| **Marital Status** | | | **Smoker** | | |
| Never Married | 2,002 | 39.92 | No | 4,393 | 87.63 |
| Currently Married | 2,726 | 54.36 | Yes | 620 | 12.37 |
| Formerly Married | 287 | 5.72 | *Total* | 5,013 | 100 |
| *Total* | 5,015 | 100 | | | |
| | | | **Ethnicity** | | |
| **Ever Had STD** | | | Akan | 2,025 | 40.38 |
| No | 4,834 | 96.8 | Ga/Dangme | 338 | 6.74 |
| Yes | 160 | 3.2 | Ewe | 614 | 12.24 |
| *Total* | 4,994 | 100 | Guan | 191 | 3.81 |
| | | | Mole-Dagbani | 1,235 | 24.63 |
| **Age at First Intercourse** | | | Grussi | 157 | 3.13 |
| Never Had Sex | 1,171 | 23.38 | Gruma | 188 | 3.75 |
| 15 or Younger | 511 | 10.2 | Other | 267 | 5.32 |
| >15 | 3,327 | 66.42 | *Total* | 5,015 | 100 |
| *Total* | 5,009 | 100 | | | |
| | | | **Language** | | |
| **Had High Risk Sex** | | | English | 1,351 | 26.94 |
| No | 3,931 | 78.46 | Akan | 2,804 | 55.91 |
| Yes | 1,079 | 21.54 | Ga | 53 | 1.06 |
| *Total* | 5,010 | 100 | Ewe | 270 | 5.38 |
| | | | Nzema | 34 | 0.68 |
| | | | Dagbani | 203 | 4.05 |
| | | | Other | 300 | 5.98 |
| | | | *Total* | 5,015 | 100 |

**Table 12 Marginal Effects from Probit for HIV Test Consent for Zambian Men**

| | HIV Test Consent | |
|---|---|---|
| Variables | Individual Level Variables | Interviewer Level Averages |
| Years of Education | 0.00475** | -0.0001 |
| | (0.002) | (0.000) |
| **Wealth Category** | | |
| Poorest | -0.00731 | -0.0086*** |
| | (0.031) | (0.002) |
| Poorer | -0.02721 | -0.0046 |
| | (0.031) | (0.003) |
| Middle | -0.03732 | -0.0059 |
| | (0.027) | (0.004) |
| Richer | -0.00842 | -0.0109*** |
| | (0.019) | (0.004) |
| **Location** | | |
| Small city | 0.12471*** | 0.0039 |
| | (0.037) | (0.004) |
| Town | 0.14458*** | 0.0036 |
| | (0.044) | (0.004) |
| Countryside | 0.20348*** | 0.0008 |
| | (0.062) | (0.003) |
| **Marital Status** | | |
| Never Married | 0.01165 | 0.0036 |
| | (0.028) | (0.003) |
| Currently Married | 0.03447 | -0.0018 |
| | (0.031) | (0.004) |
| Had STD | 0.04105* | 0.0036 |
| | (0.022) | (0.003) |
| **Age at First Intercourse** | | |
| 15 or Younger | 0.04986** | 0.0043 |
| | (0.020) | (0.003) |
| >15 | 0.03778* | 0.0013 |
| | (0.022) | (0.003) |
| Had High Risk Sex | 0.05241** | -0.0038 |
| | (0.025) | (0.004) |
| **Number of Partners** | | |
| None | 0.05095** | -0.0078** |
| | (0.025) | (0.004) |
| 2+ | 0.01516 | 0.0021 |
| | (0.021) | (0.003) |
| Used Condom Last Intercourse | -0.00033 | -0.0005 |
| | (0.016) | (0.002) |
| Would Care for HIV Relative | 0.05757** | 0.0030 |
| | (0.024) | (0.003) |

| | | |
|---|---|---|
| Knows Someone Who Died of AIDS | 0.03795*** | -0.0011 |
| | (0.011) | (0.001) |
| Previously HIV Tested | 0.00504 | -0.0038 |
| | (0.013) | (0.002) |
| Smoker | 0.03644*** | 0.0041** |
| | (0.012) | (0.002) |
| Drinks Alcohol | 0.01008 | -0.0064*** |
| | (0.012) | (0.002) |
| **Language** | | |
| English | -0.04601 | 0.0079 |
| | (0.062) | (0.008) |
| Bemba | -0.00834 | 0.0037 |
| | (0.061) | (0.009) |
| Lozi | 0.04688 | 0.0130** |
| | (0.056) | (0.007) |
| Nyanja | 0.05083 | 0.0036 |
| | (0.055) | (0.009) |
| Tonga | 0.06172 | 0.0060 |
| | (0.062) | (0.008) |
| Observations | 6,416 | |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Age, region, religion, and ethnicity are included in the regression but are not shown in the table

**Note to table 12**: Each column is for the same regression where HIV consent is regressed on the X variables shown in the table and the corresponding average (in percent) for each interviewer.

**Table 13 Marginal Effects from Probit for HIV Test Consent for Ghanaian Men**

| | HIV Test Consent | |
|---|---|---|
| Variables | Individual Level Variables | Interviewer Level Averages |
| Years of Education | 0.00224* | -0.0002 |
| | (0.001) | (0.000) |
| **Wealth Category** | | |
| Poorest | 0.09640*** | 0.0136** |
| | (0.016) | (0.007) |
| Poorer | 0.08063*** | 0.0200*** |
| | (0.014) | (0.001) |
| Middle | 0.05346*** | 0.0417*** |
| | (0.015) | (0.007) |
| Richer | 0.03687*** | 0.0376*** |
| | (0.014) | (0.006) |
| **Marital Status** | | |
| Currently Married | 0.02424 | -0.0035 |
| | (0.022) | (0.004) |
| Formerly Married | -0.04371 | 0.0084 |
| | (0.028) | (0.011) |
| Had STD | 0.03830* | 0.0529*** |
| | (0.021) | (0.009) |
| **Age at First Intercourse** | | |
| Never Had Sex | 0.00918 | -0.0578*** |
| | (0.021) | (0.009) |
| 15 or Younger | -0.02929 | -0.0569*** |
| | (0.018) | (0.010) |
| Had High Risk Sex | 0.00272 | 0.0092 |
| | (0.019) | (0.007) |
| **Number of Partners** | | |
| 1 | 0.00304 | -0.0331*** |
| | (0.022) | (0.005) |
| 2+ | -0.00752 | 0.0218** |
| | (0.031) | (0.010) |
| Condom Last Intercourse | 0.00045 | -0.1715*** |
| | (0.017) | (0.024) |
| Would Care for HIV Relative | 0.02213* | 0.0124*** |
| | (0.013) | (0.001) |
| Know Someone Died of AIDS | 0.01538 | -0.0424*** |
| | (0.010) | (0.006) |
| Previously HIV Tested | 0.00564 | -0.0097*** |
| | (0.017) | (0.003) |
| Smoker | -0.00928 | -0.0233*** |
| | (0.017) | (0.007) |

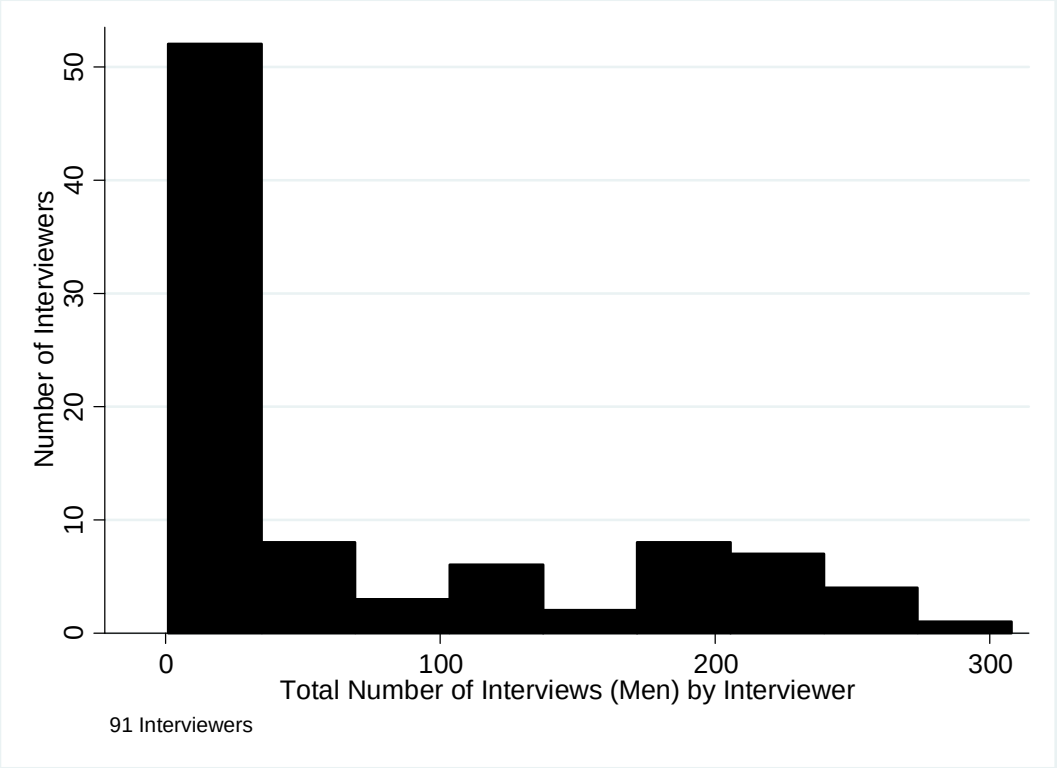
| | | |
|---|---|---|
| **Language** | | |
| Akan | 0.04955*** | -0.0147*** |
| | (0.017) | (0.001) |
| Ga | 0.06397** | 0.0282*** |
| | (0.026) | (0.005) |
| Ewe | 0.02522 | 0.0175*** |
| | (0.038) | (0.005) |
| Nzema | 0.04300 | 0.0112* |
| | (0.047) | (0.007) |
| Dagbani | 0.01586 | -0.0241*** |
| | (0.049) | (0.007) |
| Other | -0.03509 | |
| | (0.030) | |
| Observations | 4,955 | |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Age, region, religion, and ethnicity are included in the regression but not shown in the table

**Note to table 13**: Each column is for the same regression where HIV consent is regressed on the X variables shown in the table and the corresponding average (in percent) for each interviewer.

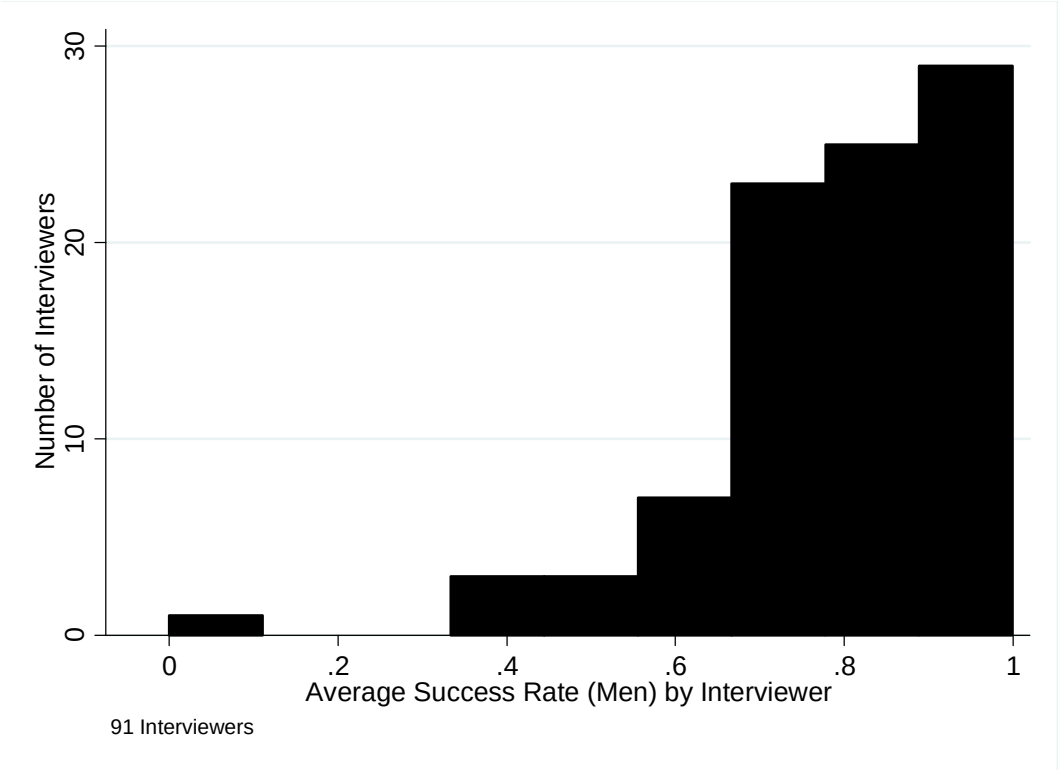
## Figures

**Figure 3 Number of Interviews by Interviewer Zambia 2007 (Men)**



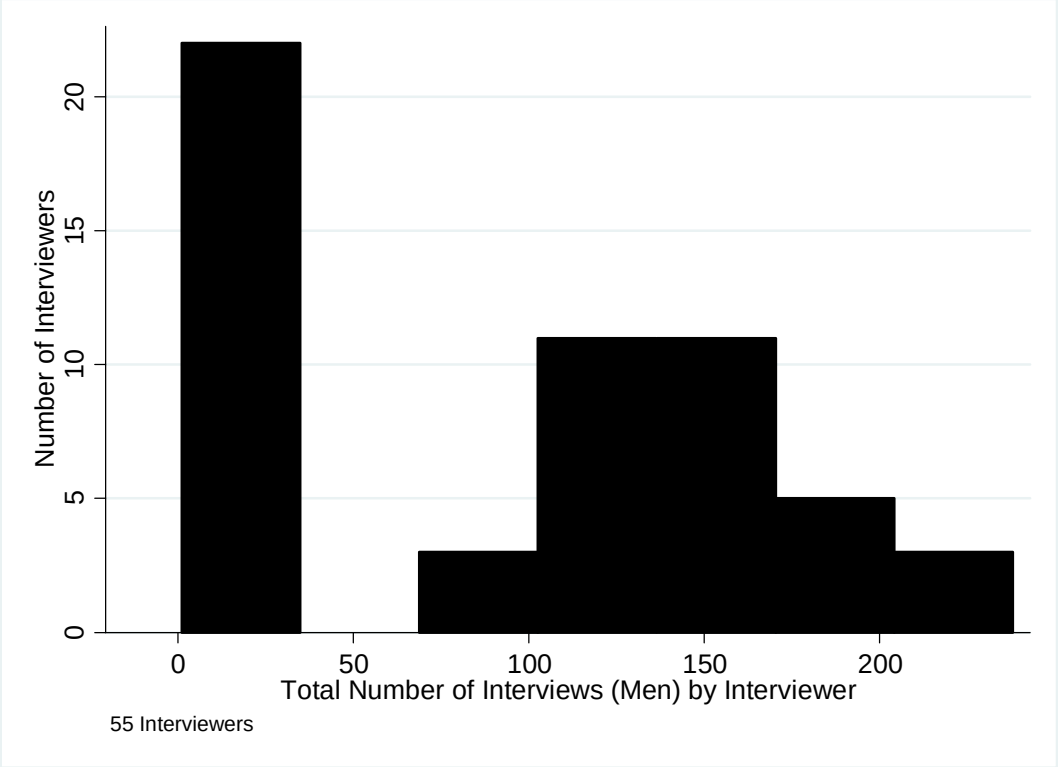

**Note to Figure 3**: Graph is at the interviewer level (one observation per interviewer).

**Figure 4 Success Rate of Interviewers Zambia 2007 (Men)**



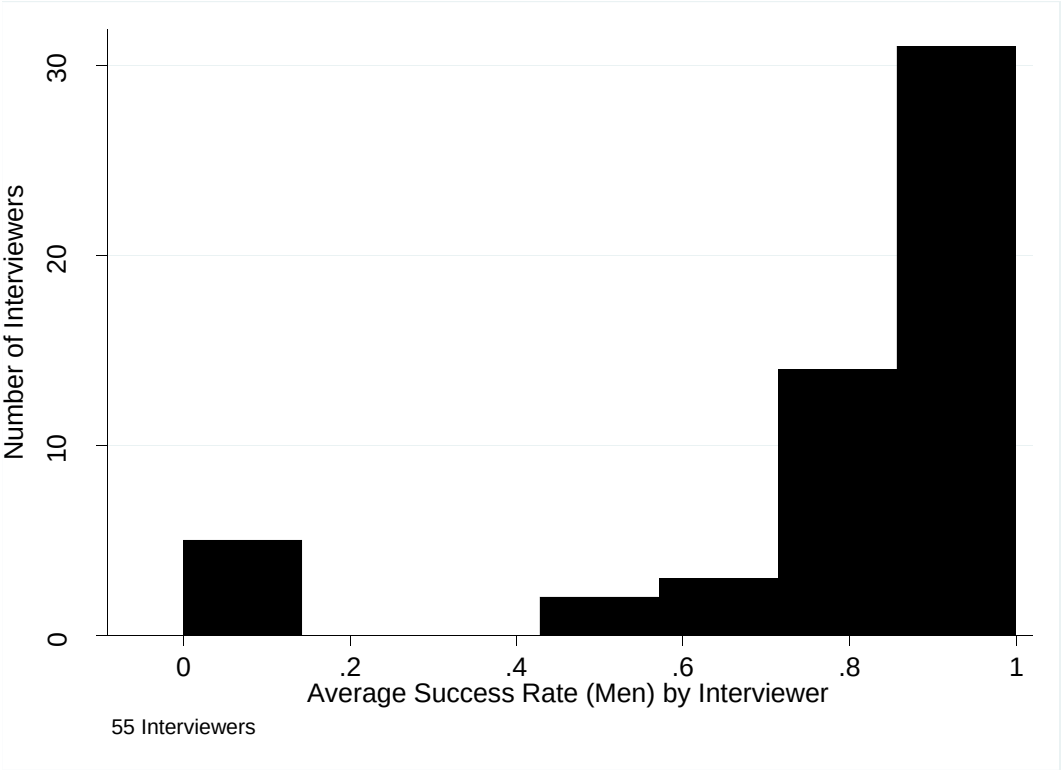

**Note to Figure 4**: Graph is at the interviewer level (one observation per interviewer).

**Figure 5 Number of Interviews by Interviewer Ghana 2003 (Men)**



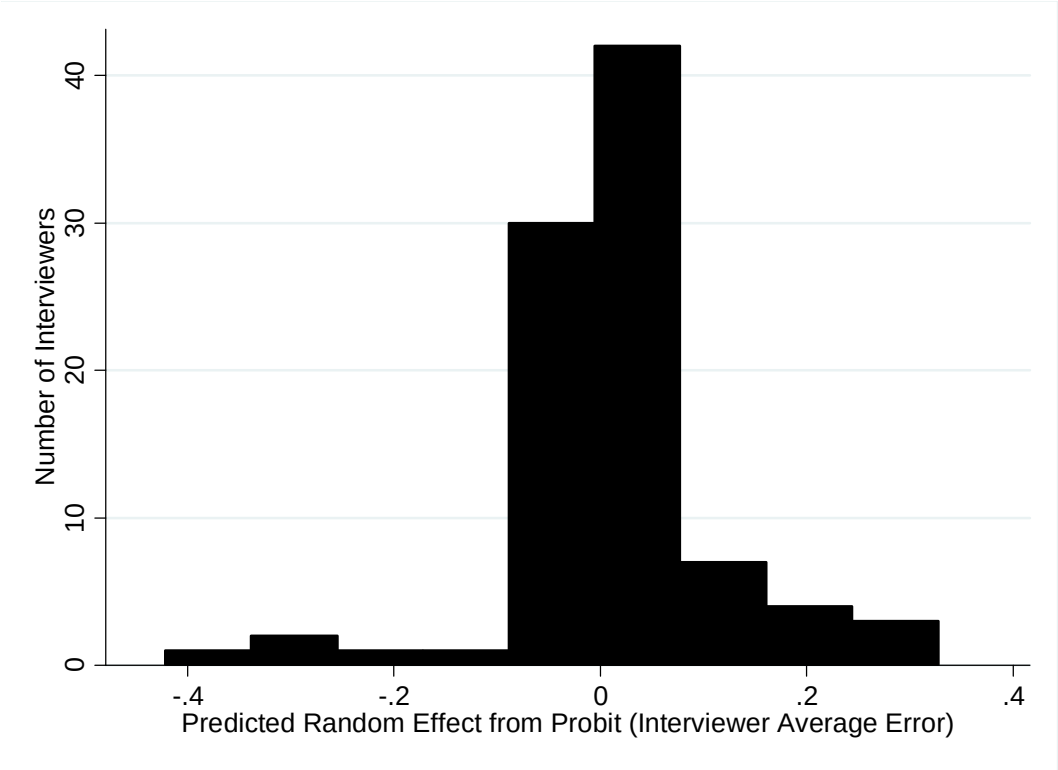

**Note to Figure 5**: Graph is at the interviewer level (one observation per interviewer).

**Figure 6 Success Rate of Interviewers Ghana 2003 (Men)**



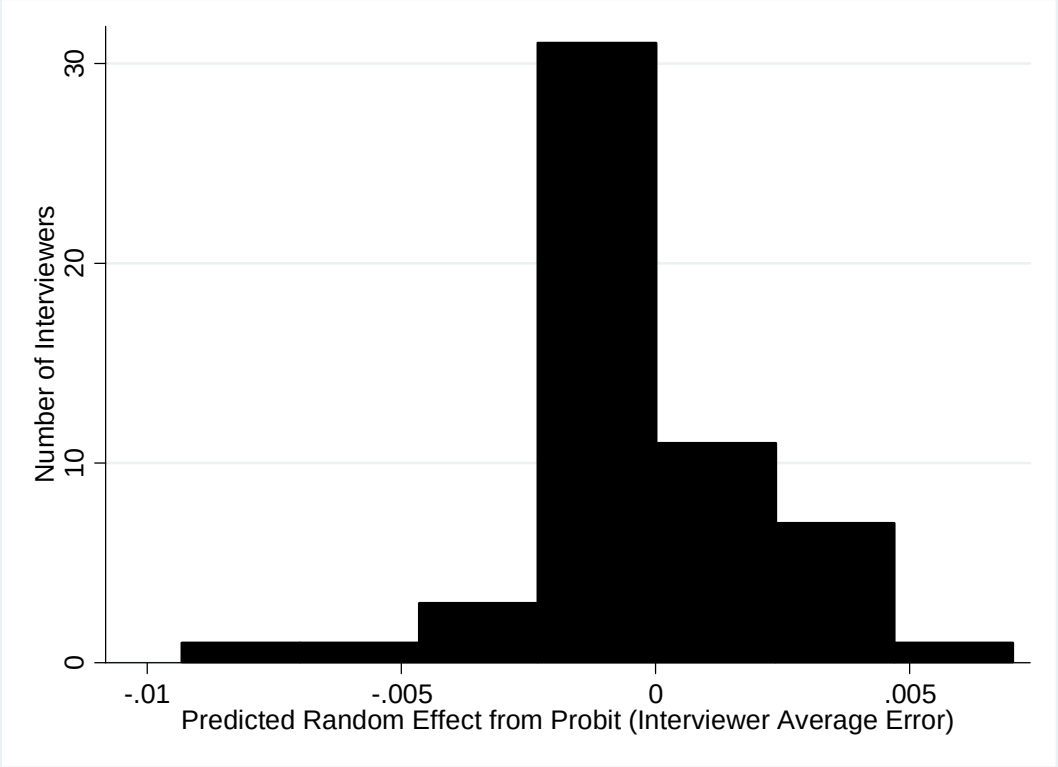

**Note to Figure 6**: Graph is at the interviewer level (one observation per interviewer).

**Figure 7 Random Effects from Probit for Consent Zambia 2007 (Men)**




**Note to Figure 7**: Random effects are calculated as the average error term for each interviewer from a probit regression for HIV consent, including the interviewer average of each variable in table 4. Graph is at the interviewer level (one observation per interviewer).

**Figure 8 Random Effects from Probit for Consent Ghana 2003 (Men)**




**Note to Figure 8**: Random effects are calculated as the average error term for each interviewer from a probit regression for HIV consent, including the interviewer average of each variable in table 5. Graph is at the interviewer level (one observation per interviewer).

**Figure 9 Interviewer Fixed Effect Coefficients Density Plot for Consent to HIV Test in Zambia (Table 1)**

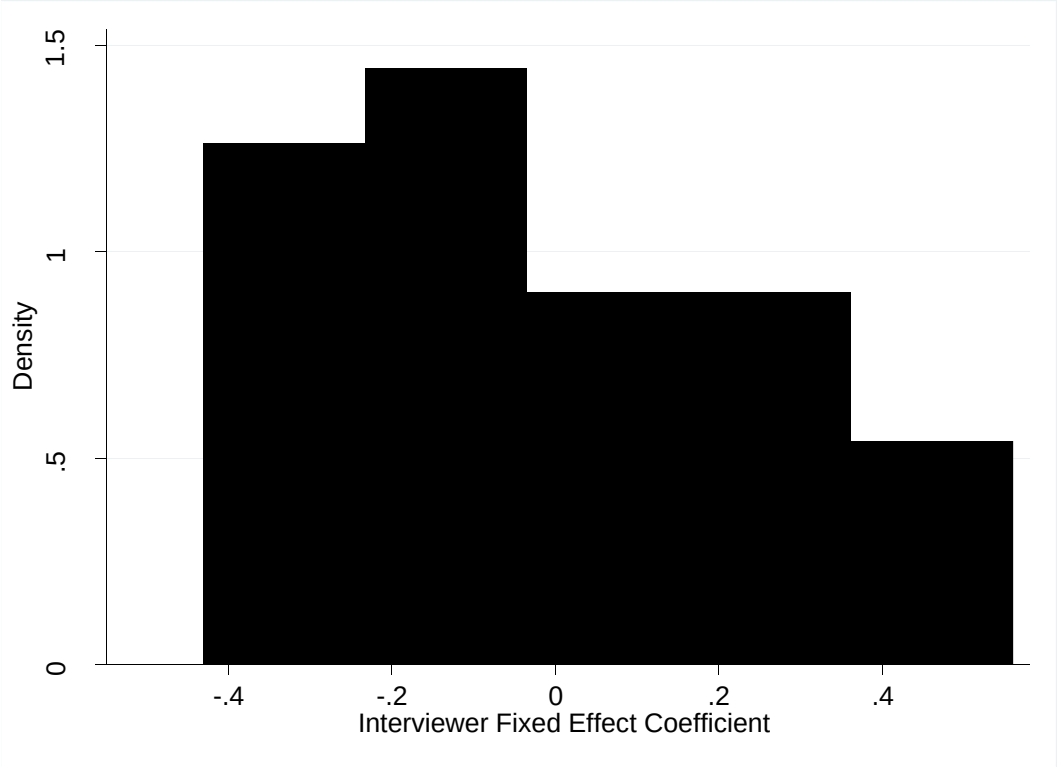
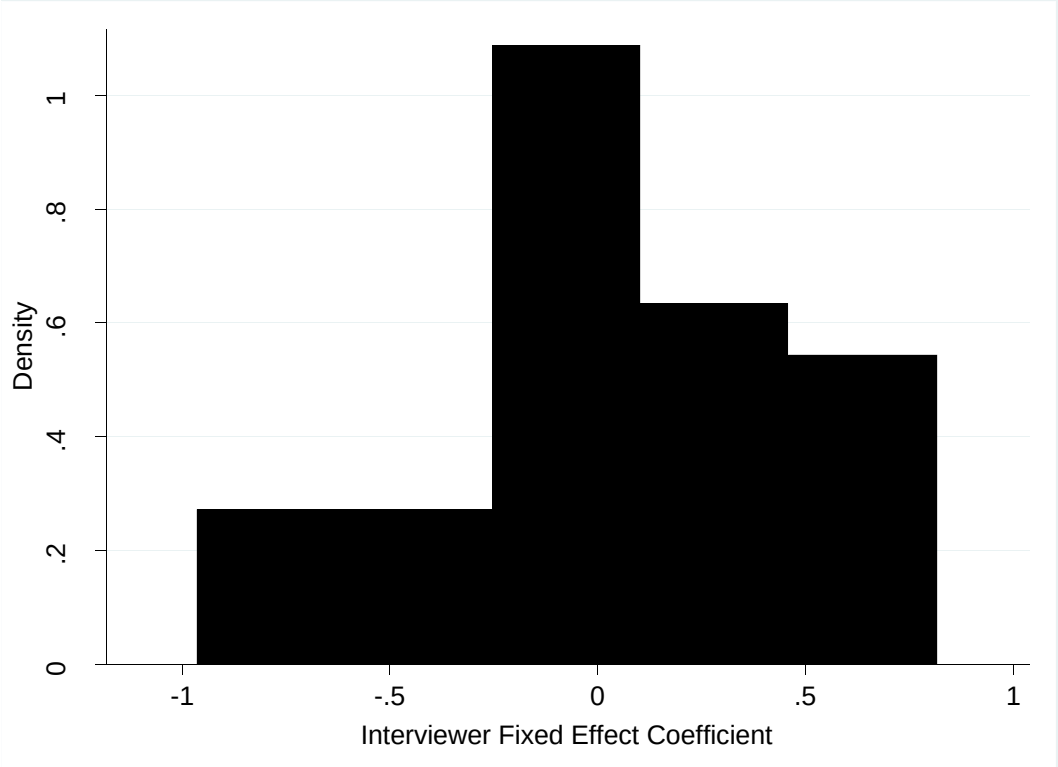


**Figure 10 Interviewer Fixed Effect Coefficients Density Plot for Consent to HIV Test in Ghana (Table 5)**



**The concentrated likelihood function**

The likelihood of the parameters $(\beta_s, \beta_h, \rho)$ given the full data set $(y, x, z)$ is

$$L(\beta_s, \beta_h, \rho) = P(y, x, z|\beta_s, \beta_h, \rho) \qquad (9)$$

For a given ρ we can concentrate the likelihood function by setting the other parameters at their maximum likelihood values given ρ:

$$L_c(\rho) = P\left(y, x, z\middle|\widehat{\beta_s}(\rho), \widehat{\beta_h}(\rho), \rho\right) \approx P(y, x, z|\rho) \qquad (10)$$

In large samples the approximation to $P(y, x, z|\rho)$ will become exact as the maximum likelihood estimates of the other parameters are consistent. Using the concentrated maximum likelihood the problem is reduced to a one parameter model and we can carry out our Bayesian averaging approach with a prior over ρ alone.