**STUDY PROTOCOL AND PRE-ANALYSIS PLAN**

# Title: Life Plans of Young Adults in Rural South Africa: an Intervention Study

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**Description of this document**
This document contains: **(A) Study Protocol**, as approved by the Boston University Medical Campus IRB and University of KwaZulu-Natal Biomedical Research Ethics Committee, with additional information on study procedures including: justification of the attention placebo control; sampling protocol; and randomization protocol; and **(B) Pre-Analysis Plan** for the primary evaluation of the intervention**.**

## A. STUDY PROTOCOL

### A1. Summary of project

Can information on population life expectancy gains increase survival expectations and human capital investment? In rural South Africa, adult life expectancy increased by 18 years from 2003 to 2015, with the scale-up of HIV treatment. We set out to conduct a randomized evaluation of a video intervention conveying information on these population life expectancy gains and assessed impacts on beliefs and on behaviors related to HIV testing, risk compensation, and human capital investment. The study is to be implemented in a population-representative sample (target n=450) of young adults ages 18-25 years residing in rural KwaZulu-Natal, South Africa.

The aims of the study are:

- To describe beliefs about HIV, ART, and life expectancy in the age of mass HIV treatment access
- To determine the impact of information on survival gains due to ART on beliefs about life expectancy
- To establish whether information on survival gains due to ART affect future-oriented health and education behaviors

### A2. Rationale

Economic theory predicts that increases in life expectancy should lead to more forward-looking behavior [1]. Individuals who face a higher probability of living to future periods are expected to invest more resources in education and job training today, in order to enjoy greater total consumption over their lifetime. Similarly, with greater longevity, individuals are less likely to take risks – e.g. drunk driving, smoking, or sex work – that jeopardize their enjoyment of future utility. In spite of its theoretical appeal, the evidence on a causal link between subjective life expectancy and economic behavior is limited [2–4], particularly related to young adults and HIV. Perhaps the strongest evidence comes from a study in rural Malawi which found that scale-up of antiretroviral therapy led to increased survival expectations, greater labor supply, and greater human capital investment and savings [5,6]. However, without randomization of ART scale-up it is possible that other factors could have confounded this relationship.

Antiretroviral treatment (ART) for HIV has dramatically reduced mortality due to HIV/AIDS, turning what was once a death sentence into a manageable chronic condition. In rural KwaZulu-Natal, roll out of ART in the public sector has led to large gains in adult life expectancy, with gains of 18 years since 2003 (update of Bor, et al. 2013 [7]). These changes may have widespread impacts on community norms, perceptions, and behaviors. Indeed, nearly 40% of community members in the area

live in a household or compound with someone who has sought care in the government ART program [8]. These longevity improvements likely influence how young adults think about their future. Little is known, however, about young adults' health perceptions in the age of ART and how perceptions affect health behaviors and feedback to the HIV epidemic itself. This increase in life expectancy may raise incentives for young adults to engage in preventive health behaviors – e.g., testing and linkage to HIV care – and future oriented behaviors like schooling and savings. However, limited information about these survival changes and bandwidth constraints to acquire this information [9] may minimize the impact these changes population changes in survival have on behavior. Young adults that are fatalistic about their future or believe they have low chances of survival may not perceive benefits to preventive health behaviors.

Formative qualitative research conducted in advance of this intervention study (and data from elsewhere [10]) revealed that many young adults have survival expectations substantially below the life expectancy implied by prevailing mortality rates in the population. Our qualitative data also showed that young adults who were pessimistic about their survival chances were often fatalistic about investing in their future. These beliefs may be self-fulfilling, such that fatalism about the future leads to riskier health behaviors and diminished opportunities later in life, generating a behavioral poverty trap [11]. Low survival expectations and associated fatalism may also reduce a young adult's likelihood of HIV testing and early ART initiation. This may help explain that, even though treatment is free, HIV mortality remains high and many deaths occur without the HIV-infected person seeking care [12].

We undertook a randomized evaluation to determine whether a video providing information on the gains in population life expectancy due to ART scale-up can shift survival expectations, and whether such an intervention can increase future-oriented behaviors, such as HIV testing, human capital investment, and savings. To provide this information, we developed a video intervention in collaboration with a local HIV support group and the community engagement unit of the Africa Health Research Institute, a research organization which operates a large demographic surveillance site in the study area.

Informational interventions have been used to affect perceived returns to education in the Dominican Republic [13] and Madagascar [14], and to change mental models of future opportunities [15,16] to increase education and savings. In addition, greater HIV prevalence and mortality risk reduced schooling in sub-Saharan Africa [3], while Malawi's rapid scale-up of HIV treatment was found to raise labor supply among the HIV negative attributable to changes in subjective mortality risk and mental health improvements [5] as well as savings and educational expenditure [6]. Although existing evidence is consistent with information on rising life expectancies affecting future-oriented behavior, to date no experimental study has sought to shift survival expectations in the context of rapidly rising life expectancy. If a video intervention is effective, then it would be easily scalable to other populations highly

affected by HIV where treatment is recently available. There is strong rationale to assess whether a video intervention can shift perceptions and future-oriented behaviors.

The study also sought to describe beliefs about the treatment and prevention benefits of ART; to describe subjective life expectancy in the era of mass HIV treatment; and to describe the validity of survey instruments to capture subjective probabilities in this setting, building on work in other sub-Saharan African settings [10].

## A3. Study context

The study was conducted in rural KwaZulu-Natal, South Africa, in partnership with the Africa Health Research Institute (AHRI). AHRI has maintained a health and demographic surveillance system since 2000 covering a geographically defined population of about 100,000 people residing in a 438 $km^2$ area [17]. The surveillance involves annual visits to all households and includes a complete population census and additional health, economic, behavioral, and HIV biomarker surveys. The database is also linked with the local public sector HIV treatment program. The surveillance area includes both rural and peri-urban areas, and is located in one of the poorest districts in South Africa.

HIV prevalence in this population is very high, with one third of adults HIV-infected. The availability of ART in the public sector, beginning in 2004, led to a major shift in the health risk environment. Based on data from AHRI's complete population surveillance of births, deaths, and migrations, we previously computed an increase in population adult life expectancy of 11.3 years between 2003 and 2011 [7]; by 2015, the increase in adult life expectancy had reached 18 years. This study was designed in part to evaluate a video-based dissemination strategy to share these findings with the population whose data gave rise to our prior research.

In formative qualitative research preceding this randomized trial, we conducted interviews and focus groups with young adults ages 16-29 years. Many respondents reported low survival chances, e.g.

> "I can't believe that I can reach 40, people are dying, especially the youth…. We are dying fast now, we are dying like flies…. We are living in difficult times."
>
> - *Female, 24 years old*

> "I was answering the question about, do I think I will live up to 60, I thought 60: I would be dead by that time"
>
> - *Male, 20 years old*

There are many reasons why young adults might not have updated their beliefs about future survival in the age of mass HIV treatment. First, they may base their

beliefs on the experiences of the previous generation – their parents, aunts, uncles, many of whose lives were cut short by HIV. Second, deaths among young people may be more salient than deaths among older people and have an outsized influence on beliefs. Third, people may think about the ages of people who have recently died in the community. This heuristic performs very poorly in a population where the age distribution is heavily skewed. Due to the much larger number of young people, the modal age at death among people who died in 2014 was 33 years, similar to what it was in 2003, even as the overall number of deaths has declined dramatically.

We therefore set out to provide information on the changes in adult life expectancy that have occurred in this population with HIV treatment scale-up. To the extent that such information leads individuals to revise upwards their beliefs about survival and to more future-oriented investment behaviors, our study will shed light on a potentially-important but little-utilized approach in public health information campaigns: providing good news on complementary health risks. Whereas most public health information campaigns seek to amplify perceived risks as an approach to motivate behavior change (e.g. smoking kills), if people over-estimate the risks in one domain (e.g. I've smoked, so I will die young), this has potential to result in sub-optimal behavior across other domains (e.g. if I'm going to die young anyway, I may as well drink too), leading to clustering of risk behaviors. Health risk-taking in one domain reduces returns to healthy behaviors in other domains, leading to a negative health behavior trap due to complementarity of risks [18].

In addition, our video intervention provides information on changes in survival as told through the personal experiences of HIV-positive individuals from the community who have live successfully for many years on ART. This design is intended to both to enhance the information's credibility and beliefs that the information applies to the viewer [19].

The randomized evaluation of the video intervention is nested in the AHRI surveillance. The surveillance population provides the sampling frame of eligible young adults to participate in the study. This will enable us to ensure balance between study arms on some key baseline covariates observed in the surveillance. Additionally, the video intervention will be timed so that it occurs 6-8 weeks prior to the annual AHRI surveillance visit, which includes an offer of home-based HIV counseling and testing, with referral to the local public sector HIV treatment program. Coordination with AHRI will enable us to assess uptake of HIV testing via home-based HIV testing offered in the surveillance, as well as longer run follow-up of education and labor supply outcomes.

## A4. Hypotheses

*We hypothesize that a video informing young adults about large recent life expectancy gains due to HIV treatment scale-up will:*

- ***Increase human capital investment,*** *by increasing their own survival expectations, hope for the future, and lead to increases in positive health and investment behaviors*
- ***Increase HIV testing and care-seeking,*** *by improving attitudes towards and the perceived benefits of ART*

Subjective life expectancy. We will test the null hypothesis that the video intervention described will have no effect on subjective life expectancy, as measured through ascertainment of quantitative beliefs about probabilities of living to different ages.

Pooled measure of forward-looking behaviors. We will test the null hypothesis that the video will have no effect on an index of measured forward-looking investment behaviors, including: uptake of HIV testing, participation in a job-search skills training workshop, and use of a savings device. For the pooled behavioral outcome, these behaviors will be aggregated using a simple count of the following: individual tested for HIV using either the voucher or surveillance; individual attended a job search skills workshop; individual allocated at least 50 Rand to the labeled savings tin at baseline.

*Because uptake of HIV testing could increase as a result of either the human capital channel or the perceived benefits of ART channel, we will additionally disaggregate HIV testing from the other outcomes and test the following null hypotheses separately:*

HIV testing. We will test the null hypothesis that the video intervention will have no effect on uptake of HIV testing as measured via either use of the HIV testing voucher or participation in home-based HIV testing in the AHRI surveillance visit occurring about six weeks after baseline.

Job search skills workshop. We will test the null hypothesis that the video intervention will have no effect on uptake of a job search skills workshop occurring 1-3 weeks after the baseline interview that all participants will be invited to.

Savings. We will test the null hypothesis that the video intervention will have no effect on the amount (out of 100 Rand) allocated to the savings tin offered at the end of the baseline visit.

Additionally, to better understand changes in beliefs and attitudes, we will test the null hypothesis that the video intervention will:

- Have no effect on attitudes towards HIV treatment and its efficacy
- Have no effect on perceptions of survival in the community

- Have no effect on measures of future orientation, locus of control, life satisfaction, and mental health
- Have no effect on beliefs about future education and employment
- Have no effect on attitudes towards the importance of using condoms

Additionally, to better understand changes in self-reported behaviors, we will test the null hypothesis that the video intervention will:

- Have no effect on time use allocated to education or labor supply activities, including job search
- Have no effect on self-reported HIV testing
- Have no effect on self-reported smoking, drinking, seatbelt use
- Have no effect on sexual activity and condom use

Additionally, it is possible that providing information on the benefits of HIV treatment would reduce demand for HIV prevention, a response known as risk compensation. It is also possible that increased optimism about the future would lead to greater demand for HIV prevention. Therefore, we will test the null hypothesis that the video intervention described will:

- Have no effect on demand for HIV prevention, as measured by number of condoms purchased when offered at the end of the baseline survey

*Subgroup analyses*

Theory predicts that the impact of the video on subjective life expectancy (and human capital investment behaviors) should be greater the lower the person's baseline survival expectations, as the informational intervention is stronger relative to the person's baseline beliefs. Additionally, theory predicts that the impact of the video on subjective life expectancy (and human capital investment behaviors as well as HIV testing) should be greater the higher a person's baseline perceived lifetime risk of HIV acquisition, as information on the life-prolonging benefits of ART will be irrelevant to someone who believes that they are unlikely ever to contract HIV. Therefore, we will test the null hypotheses that subjective life expectancy and perceived lifetime HIV risk are not linear effect modifiers of the primary outcomes: change in subjective life expectancy, HIV testing (with the voucher or home-based testing), participation in the training workshop, savings, and the pooled behavioral outcome described above.

The benefits of HIV testing vary with prior knowledge of status. Individuals who are certain that they are HIV-infected and individuals who are certain that they are HIV-uninfected have little incentive to test, suggesting an inverse-U relationship. Therefore, we will assess for effect modification of the impact of the video on HIV testing by current beliefs about HIV infection, comparing effects among respondents with greater uncertainty about HIV status vs. those with greater stated certainty.

Finally, we will assess for linear effect modification by age (the information may be more impactful on younger participants who have yet to establish behavioral patterns) and sex (a common effect modifier in HIV interventions).

## A5. Overview of Study Design

We will conduct a pilot intervention study to learn whether a short video providing young adults with information on recent longevity gains affects survival expectations, hope for the future, and ultimately health and educational behaviors. We intend to recruit approximately 450 young adults ages 18 to 25 for this research project. The study involves the following components:

- Conduct a survey to measure survival expectations and other perceptions of HIV and ART that affect health decision-making as well as health and education (future-oriented) actions,
- Randomize half of the study participants (50%) to view a 9-minute informational video explaining that life expectancy in their area has substantially increased since 2003 and that this gain can primarily be attributed to reduced HIV-related mortality because of increased access to treatment. The other half of study participants will be randomized 1:1 to an attention placebo control video (25%) and a pure control arm (25%).
- Conduct immediate post-video survey and assess uptake of job skills workshop, HIV testing voucher, condom offer, and a savings device
- Assess uptake of HIV testing during the home-based AHRI surveillance visit, which occurs about 6 weeks after the baseline survey
- Conduct follow-up survey at 8 weeks to measure changes produced by the video in survival expectations and health behaviors, and
- Assess longer run effects on linkage to care as well as other schooling and employment behaviors observed in the surveillance

The primary endpoint is an index of all human capital investment behaviors including use of the HIV testing voucher, uptake of HIV testing in the surveillance visit, participation in a job-skills workshop, and use of the savings device.

## A6. Study Population and Inclusion Criteria

The proposed study will investigate survival expectations and health behaviors among young adults residing in the AHRI surveillance who are ages 18 to 25.

*Inclusion criteria*

- Member of a household that participates in the AHRI population surveillance (i.e. not a visitor to the area)
- Resided in the AHRI surveillance area in the previous year (and was therefore included in the population listing that formed the sampling frame) and was still residing in that location when the first contact attempt was made
- Resides in one of the specific AHRI week-blocks that we sampled from for the study (further details below)
- Ages 18-25 years

*Exclusion criteria*

- <18 years at time of baseline interview
- >25 years at time of baseline interview
- Not able to provide consent
- Did not consent to participate

## A7. Sampling Strategy and Sample Size Determination

Participants were sampled at random from a population listing from AHRI/Africa Centre's surveillance. AHRI's surveillance is organized by geographically defined week blocks. Specific week blocks were chosen for this study based on the timing of AHRI's surveillance. We included respondents residing in week blocks where AHRI was scheduled to conduct its annual surveillance and HIV testing offer about 6-8 weeks after the baseline survey.

Details on the Sampling Strategy are contained in Appendix A.

We chose a target sample size of 450 individuals. The final actual sample size will be determined based on the number of successful interviews achieved within a pre-specified period, from a pre-specified roster of eligible participants. The number of eligible participants included in the study roster is based our target of 450 baseline interviews conducted. Based on prior studies of this age group at AHRI, we anticipate that we will be able to find and recruit 50- 60% of the eligible population. We anticipate that our study team is capable of conducting 60-75 interviews per week. We therefore sampled 120 individuals in each of seven week-blocks for a total of 840 individuals eligible for recruitment. The final sample size was the number of respondents successfully interviewed from the pre-specified study rosters.

With a target sample of 450 individuals, this study is powered at 80% to detect a 0.26 standard deviation difference in outcomes between the treatment and pooled control groups.

Example 1) We are powered to detect an increase in subjective (perceived) life expectancy from 50 to 54 years, assuming a standard deviation of 15 years. The baseline numbers – 50, sd=15 – are based on survey pre-tests conducted as part of a prior study: "Life plans of young adults in rural KZN: a qualitative study". A four-year increase would be about a quarter of the actual change in life expectancy that occurred between 2003 and 2014.

Example 2) We are powered to detect an increase in the proportion using the HIV testing voucher (or taking up the job search skills workshop) from 10% to 20%. As this is a pilot study, we are only powered to detect intervention effects if they are large.

## A8. Interventions and Randomization

### A8.1. Study Arms

**Active treatment.** We developed a 9-minute video entitled "Iksasa E'lihle" (A Beautiful Future) that provides information on the changes in HIV-related mortality and adult life expectancy in the Demographic Surveillance Area between 2003 and 2015 (an increase of 18 years). The video weaves public health information on life expectancy gains with narratives from members of a local HIV support group who have lived long and fulfilling lives on HIV treatment. Some of these individuals were among the first to receive ART in the area and have been on treatment for over a decade. We incorporated the stories of individuals on HIV treatment for several reasons including: to enhance the credibility of the information on rising life expectancy; to increase the personal relevance and salience of the information through real world examples; to convey information with no numeracy requirement; to facilitate comprehension using emotional signposting based on the narratives; to illustrate by example how specific individuals have been motivated by their longer life expectancy on ART to invest in themselves, their families, and in a small business; and to increase viewer engagement with the video content. The video was developed in partnership with AHRI's Community Engagement Unit and was filmed by Jive Media Africa (www.jivemedia.co.za), a South African media company with expertise in scientific communication through film. The goals of the video are (1) to increase survival expectations, hope, locus of control, and future orientation; and (2) to improve perceived benefits of HIV treatment and attitudes towards living on HIV treatment.

**Attention placebo control.** The attention placebo control arm will receive a 9-minute video clip of a video on another topic. Attention placebo controls should mimic the theoretically inactive components of the active treatment under study, while having none of the theoretically active components of that intervention (Freedland, http://europepmc.org/articles/pmc3091006). The attention placebo control video was matched to the active treatment video in length, pace, tone, narrative structure, visual imagery (daily life in a local community), and language (Zulu with English subtitles), but it was lacking specifically in the active intervention content (i.e. information about longevity gains with ART). The video selected was about the challenges of raising children in households where the household head has a physical disability. It was filmed in a nearby community that was not in the surveillance area. The video was found online and we received permission to use this video from its creators. The video was edited to be 9 minutes and to contain no discussion of HIV treatment and longevity. We note that in clinical behavioral intervention studies, attention placebo controls should induce the same expectation of therapeutic benefit as the treatment. Our study was not conducted in a clinical setting, and neither the active treatment nor the attention placebo control was presented as a therapeutic intervention.

**Pure control.** The pure control arm will view no video and will simply continue with the baseline survey.

## A8.2. Randomization procedures

The study is a population-based randomized controlled trial. To reduce potential for contamination, treatment assignment will be determined at the level of the household, such that if there are multiple eligible participants per household, they will receive the same treatment. Households will be randomized to study arm *ex ante*, i.e. before going into the field*. Ex ante* randomization has two benefits. First, as a quality control, *ex ante* randomization limits the opportunity for violations of treatment assignment. Second, because the study is nested in the AHRI population surveillance, *ex ante* randomization enables us to achieve balance across study arms with respect to several baseline characteristics of interest among eligible participants. We will use a constrained randomization approach to achieve approximate balance on: sex, age, whether the person was in school, orphans status, HIV positive status, HIV negative status (unknown is the third category), and household asset quintile. These factors were selected because of their potential influence on survival expectations. (For further details on randomization procedures, please see Appendix C.)

We note that sampling and randomization will be conducted using AHRI's public use (de-identified) data. We will provide a list of public use identifiers and treatment assignments to AHRI's data management staff, and they will create an operational database to be used during data collection. Information on the characteristics above (e.g. HIV status) will never be linked with personally-identifiable information.

Participants will be randomized using a ratio of 50% active treatment, 25% attention placebo control, 25% pure control.

| | Active Treatment Video | Attention Placebo Video |
|---|---|---|
| Arm 1, Active Treatment | X | |
| Arm 2, Attention Placebo Control | | X |
| Arm 3, Pure Control | | |

## A8.3. Blinding

*Participants and interviewers*
During recruitment, consent, and the first half of the baseline survey, participants and interviewers will be blinded as to the participant's treatment assignment. Randomized treatment assignment will be revealed at the moment when participants received the intervention. However, we will not indicate to the participants whether the video they see is the treatment or attention placebo video.

*Researchers*

The researchers will be blinded to the randomization status of participants throughout the data collection, extraction, and cleaning/quality control process, up until the date when randomized assignments are unmasked. To maintain blinding of treatment assignments vis-à-vis the research team during the study, we will use the following procedures:

- Treatment assignment will be stored as a hidden field in the REDCap database. I.e. during the baseline survey it will generate a pop-up message telling the interviewer to show the participant the active treatment video, active control video, or no video, however this field will not be visible to anyone on the research team.
- The treatment assignment field will not be exported during routine quality control procedures.
- Treatment assignment will be extracted along with study ID into its own table and will not be merged with any of the other data until randomization is unblinded.
- Randomization will be unblinded only after the final Pre-Analysis Plan is posted to www.clinicaltrials.gov. In developing our pre-analysis plan, we conducted some descriptive analysis of the blinded baseline pre-intervention survey data. Additionally, we monitored overall levels of uptake of the HIV testing voucher and job search training workshop in order to determine potential statistical power with these outcomes. For guidance on using baseline data to inform Pre-Analysis Plans, see Olken (2015).
- We will submit an update to our clinicaltrials.gov study profile after treatment assignment is unblinded, noting the date when we have done so.

As this is a short (4-month) pilot study and the intervention is minimal risk, we do not have a DSMB and do not have stopping rules for the trial based on preliminary analysis of efficacy.

## A9. Baseline interview and intervention

The baseline survey visit is designed to take about 1.5 hours, and consists of the following sub-components:

### A9.0. Recruitment and Informed Consent

Potential respondents will be recruited via visits to their homesteads. Permission will be obtained from the head of household to enter and to speak to the sampled participant. The study will be described and participants will provide written informed consent to participate in the study, including the baseline visit, a follow-up visit, and permission to link with demographic surveillance data. See Appendix B for the approved consent form.

### A9.1. Survey module

First, we will conduct a survey questionnaire with young adults ages 18 to 25 to learn about their survival expectations, perceptions of HIV risks and treatment, future-oriented behaviors such as smoking, alcohol use, HIV risk behaviors, savings, locus of control, mental health, time and risk preference, and life satisfaction.

### A9.2. Video intervention

All study participants will have been randomly assigned *ex ante* to one of the three study arms: active treatment, attention placebo control, and pure control. Both interview participants and interviewers will be blinded to treatment assignment during the baseline survey and treatment arm will only be revealed at the time when the video will be shown. During the baseline survey, randomization status will be revealed, and the participant will be shown the video corresponding to their treatment arm, or will not be shown a video if randomized to pure control.

### A9.3. Re-survey module

Immediately after the video intervention, all study participants will be asked a brief subset of the survey module questions to re-measure survival expectations, locus of control, and life satisfaction. This step will aim to assess the immediate impact of the video intervention on beliefs.

### A9.4. Rapid assessment behaviors

Following the baseline survey participants will be offered several behavioral prompts:

**HIV Testing Voucher.** Immediately after the re-survey module, we will offer participants a voucher for free HIV testing at a local pharmacy. We will provide respondents with information about the benefits of testing, the benefits of HIV treatment, and the availability of free testing and treatment in public sector facilities. We will then offer a voucher for free HIV testing in local private pharmacies. Participants will be provided R10 with the voucher, to subsidize transport. The cost of testing in these facilities is usually R100, but will be available to people for free during a time-limited period if they bring the voucher. At the end of the study, we will visit the pharmacies to collect the vouchers that have been redeemed and we will pay the pharmacies for the cost of the testing provided. We will record the voucher numbers redeemed, as an indicator of participation in HIV testing, but will not record any information about participants' test results. This information will be stated clearly on the voucher.

**Condom offer.** Immediately after the giving participants the HIV testing voucher, all participants will be offered the opportunity to purchase condoms. Each participant will be provided with R20 in the form of ten (10) R2 coins. They will be offered the chance to use that money to buy condoms of a locally popular brand for a

discounted price (up to 10 condoms at R2 each) and to keep any remaining money. We will record how many condoms the respondent purchases.

**Invitation to Job Search Skills Workshop.** The Job Search Skills Workshop will be delivered by a local consultant or organization with experience in human resources and career counseling, as well as the local employment context. The workshop will last 3 hours. The curriculum, developed by the researchers in consultation with a local HR expert, will cover the following: (30 min) introduction and review of process of job search, including identifying opportunities, building up relevant skills and qualifications, and submitting applications; (60 min) hands on tutorial on how to write an application cover letter; (15 min) break; (60 min) interview skills and practice; (15 min) presentation on local resources for career counseling and job fairs, with hand outs from these existing organizations. Other topics related to job search skills may be covered as well. The workshop will be offered either at AHRI or at a local community hall. The workshop will be held 1-3 weeks after the baseline interview and attendance of study participants will be recorded. The goal is to measure differences in uptake across treated and control arms, which will provide short-run evidence of the impact of the video on human capital investment behaviors. In order to plan for the number of attendees, we will require participants to reserve a space in the workshop. We will give them a cash payment and give them the option to keep the cash or use it to reserve a space. Participants will be randomized to receive R30 (and pay R20 to reserve) vs. R15 (and pay R5 to reserve); in both cases they will have R10 for transport. We are randomizing the incentive size in order to understand whether differences in price affect demand for the workshop, which will provide a point of comparison for the effect of the intervention video. We will be collecting data on attendance of the Job Search Skills Workshop at the individual level. Participants will reserve a spot in the workshop during the baseline survey. A workshop roster of registered attendees will be generated that contains: study ID, name, sex, date of birth, phone number, and household head name. Phone numbers will be used to remind all registered attendees 1-2 days before the workshop event. When participants arrive at the workshop, they will be asked to confirm their identity by correctly identifying one of the following: date of birth, phone number, or name household head. Attendance information will be entered into the study database and linked based on study ID. The physical attendance roster will be scanned and destroyed after data are entered. The scanned copy will be retained in a password-protected folder on the AHRI server, along with other identified operational data.

**Savings choice**. Lastly, all participants will be provided with a labeled savings tin. During the disbursement of participant compensation (R100), the participants will be given the opportunity to save a portion of the money by placing it in the sealed savings tin. We will record the amount of money the participant chooses to save and hold a brief discussion with the participant about saving for the financial aspects of their future goals. We provide this option for study participants so that we can immediately measure intentions for future-oriented behavior and to estimate the

video's short-term effect based on differences in the amount saved between the treatment and control groups.

### A10. Follow-up interview at 2 months

Study participants will be contacted 2 months after their initial interview and provided with a follow-up survey that measures survival expectations, locus of control, life satisfaction, mental health, savings, health behaviors, time use, and HIV-related attitudes and beliefs. Participants will receive R50 in compensation for participation in the follow-up survey.

### A11. Follow-up in the AHRI Surveillance

**Home-based HIV testing.** Data from this study will be linked at the individual level to the AHRI/Africa Centre population surveillance. The baseline survey is timed so that, about 6-8 weeks afterwards, the same household will be visited by the AHRI surveillance team for their routine annual surveillance visit. During this visit, all people ages 15 and older are offered home-based HIV testing and counseling, with referral to the public sector HIV program. Uptake of HIV testing in the AHRI surveillance visit at 6-8 weeks will be combined with use of the HIV testing voucher offered at baseline, to assess impacts on HIV testing uptake. Linkage with the surveillance will be conducted by AHRI data management staff.

**Other outcomes.** Participants will be followed up in future rounds of AHRI's routine population surveillance. In addition to HIV testing, the AHRI surveillance collects information on schooling, employment, and migration, among other outcomes, and is also linked to the public sector clinical record system. These linkages will be conducted by AHRI data management after data are de-identified.

### A12. Project Timeline

| | |
|---|---|
| April 2016 – December 2016 | formative qualitative research |
| December 2016 – April 2017 | creation of video intervention |
| May – July 2017 | REDCap database development, pre-testing |
| June 2017 | *Registered on www.clinicaltrials.gov* |
| August 2017 | Recruitment and training of field interviewers |
| September – October 2017 | Baseline survey and intervention |
| September – November 2017 | Job search workshops, HIV testing vouchers |
| November – December 2017 | Follow-up survey, home-based HIV testing |

| | |
|---|---|
| December 2017 | *Updated on www.clinicaltrials.gov* |
| January – August 2018 | Data extraction, quality checks, passive follow-up of HIV testing in the AHRI surveillance |
| September 17, 2018 | *Final Protocol and Pre-Analysis Plan posted to www.clinicaltrials.gov* |
| September 18, 2018 | Randomization status unblinded to researchers and posted to *www.clinicaltrials.gov* |
| March 2018 – January 2019 | Analysis and preparation of manuscripts |

## A13. Funding and Registration

This randomized trial was jointly funded by two grants from the National Institutes of Health: 1K01MH105320 (Bor) "Economic, health, and behavioral dimensions of HIV treatment scale-up" and 1R01HD084233 (Tanser/Barnighausen) "Causal pathways to population health impact of HIV antiretroviral treatment".

The trial is registered on www.clinicaltrials.gov as NCT03215901.

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## B. PRE-ANALYSIS PLAN

### B1. Outcomes Variables and Definitions

### B1.1 Primary Outcome Measures

*Behaviors*

1. Participation in HIV testing
Definition: Use of HIV testing voucher to be provided OR participation in HIV testing during surveillance household survey visit conducted at 1 to 2 months follow-up.
[Time Frame: 2 months]

2. Composite measure of future-oriented behaviors
Definition: Count variable (range 0:3) equal to the sum of the following indicators:

- Participation in HIV testing (either via the voucher or home-based testing)
- Attendance at a job search skills workshop: 1=attended
- Allocation of funds to savings tin during baseline interview: 1=participant allocated at least 50 Rand (out of 100 Rand) to the savings tin

*Beliefs and Attitudes*

3. Change in subjective life expectancy (at 2 months)
Definition: Subjective life expectancy is defined as the area under the subjective survival curve. Respondents will be asked about the chances of surviving to age 30, 40, ..., 80 years. We will linearly interpolate the responses beginning at their current age and ending at 80 years. We will then calculate the area under this curve, which we define as subjective life expectancy (number of years expected to live between current age and age 80). Our primary outcome for beliefs will be the change in subjective life expectancy between baseline (pre-intervention) and follow-up (at 2 months). We will also assess change in subjective life expectancy between baseline and immediate post-intervention assessment as a secondary outcome.
[Time Frame: 2 months]

4. Positive attitudes towards HIV and HIV treatment (at 2 months)
Definition: Immediately after the intervention and at 2 months, we ask participants whether they strongly disagree, disagree, agree, or strongly agree with a series of statements regarding attitudes towards HIV and HIV treatment. As a primary outcome, we will assess differences in a composite index of 12 of these measures, comparing attitudes between treatment arms at 2 months. We will assign points (1=strongly disagree, 2=disagree, 3=agree, 4=strongly agree) based on responses, we will sum over the following items, and we will construct a z-score based on the mean and standard deviation in the baseline data. “Reverse code” denotes that the variable will be scored (4=strongly disagree, 3=disagree, 2=agree, 1=strongly agree). We will also assess differences immediately after the intervention as a

secondary outcome. Note: the attitudes questions are asked after the intervention. There is no baseline.
[Time Frame: 2 months]

| ITEM | REVERSE CODE? |
|---|---|
| If I were HIV infected, I could have children and a family | |
| If I were HIV infected, it would be difficult to find a romantic partner | X |
| If I were HIV infected, I could still do all the activities I enjoy | |
| If I were HIV infected, it would make it harder to get and keep a job | X |
| If I were HIV infected, it would change my career plans | X |
| If I were HIV infected, I could expect to live to pension age (age 60) | |
| If I were HIV infected, I could expect to live long enough to see my children grow up and to meet my grandchildren | |
| I am scared to find out my HIV status | X |
| If I were HIV infected, it would be hard to start ARVs (HIV treatment) | X |
| If I were HIV infected, I would want to start ARVs (HIV treatment) as soon as possible, even if I didn't feel sick | |
| If I were HIV infected, I would prefer to wait as long as possible to start ARVs | X |
| If I were HIV infected, I would choose not to start ARVs. | X |

## B1.2 Secondary Outcome Measures

*Behaviors*

Attendance at job search skills workshop
Definition: Participants will be invited to a job search skills workshop; workshops were held in nearby locations 1-3 weeks after each survey week-block; attendance will be measured as a binary indicator
[Time Frame: 2 weeks]

Condoms purchased

Definition: Participants will be offered the opportunity to purchase discount condoms; the number purchased will be assessed; range 0 to 10 condoms
[Time Frame: Baseline survey]

Savings allocated
Definition: Participants will be offered the opportunity to allocate a portion of their participation incentive to a savings device (lock box); amount allocated will be assessed; range 0 to 100 Rand
[Time Frame: Baseline survey]

Savings at 2-month follow-up
Definition: At follow-up, participants are asked if they still have the savings tin and if they have added money to the tin since baseline. They are then asked to estimate the total savings inside today or on the date when they opened it. If they still have the tin on hand, we then open it, count their savings, and provide them with a new tin to continue saving. We will define 2-month savings in two ways: first, as the amount the participants self-report they have in the tin on the follow-up interview date or, if they no longer have the tin, on the last day they had it; and second, as the amount we validate by opening the tin, with zeroes imputed if the tin is not available or money used.
[Time Frame: 2-months]

Use of HIV testing voucher
Definition: Participants will be provided with a voucher for free HIV testing at a local service provider (we partnered with two local pharmacies that provided government certified HIV testing services); use of voucher will be assessed by obtaining voucher numbers from the service provider
[Time Frame: 1 month]

Uptake of HIV testing in the AHRI surveillance
Definition: Participants will be offered the opportunity to test for HIV via a household visit buy the AHRI surveillance team 4-6 weeks after the baseline survey. By linking with the AHRI surveillance, we will assess whether individuals tested in the AHRI home visit. All those who tested will be coded as 1; all others, including those who were not at home will be coded as 0.
[Time Frame: 1 month]

Use of clinical HIV services
Definition: For individuals who are HIV-infected, the intervention could increase linkage to care and retention on HIV treatment. We will assess use of clinical HIV services as a binary indicator for whether the individual sought care in the public sector HIV care and treatment program in the three months after baseline. Clinical data from the public sector HIV treatment program are routinely linked into the population surveillance platform by AHRI data management.
[Time Frame: 3 months]

*Self-reported behaviors*

Change in time allocated to education or labor supply
Definition: At both baseline (pre-intervention) and 2-months follow-up, we ask respondents to report hours spent on different activities in the last week. These questions were adapted from the Malawi Living Standards Measurement Survey, Module E (p17). We will assess change in the total hours spent on the following activities (aggregating time spent across these activities):
- a household business
- formal work for pay
- informal work for pay
- apprenticeship/internship
- looking for work
- schooling
- job training

(The activities that we measured but excluded from this list are: housework, caregiving, socializing with friends, doing nothing, and sleep)
[Time Frame: 2 months]

Change in self-reported savings
Definition: Difference between baseline and 2-month follow up in response to the following question: how much have you saved in bank accounts, at home, or elsewhere?
[Time Frame: 2 months]

Change in self-reported non-HIV health risk behaviors
Definition: We will assess changes between baseline and 2-month follow-up in self-reported health risk behaviors. We will conduct statistical testing on an additive index of the following: any smoking in the last week; any drinking in the last week; any heavy drinking (3+ drinks) in the last week; does not always use a seatbelt. Range of this index will be 0 to 4.
[Time Frame: 2 months]

Change in self-reported unprotected sexual activity
Definition: We will assess changes between baseline and 2-month follow-up in self-reported unprotected sex. Specifically, we will assess number of times a person reported having condomless sex in the last 30 days.
[Time Frame: 2 months]

Change in self-reported current HIV care-seeking
Definition: We will assess change between baseline and 2-month follow-up in a combined self-report measure of whether the respondent either tested for HIV within the last 3 months and whether the respondent is currently taking ARVs.
[Time Frame: 2 months]

Employment, education, and care-seeking outcomes in the AHRI surveillance
Definition: We will assess longer run education, employment, migration, and care-seeking outcomes observed through household proxy report and clinic linkage in the AHRI population-based surveillance platform. Specifically, we will assess whether the respondent is reported to be in school, working, looking for work, or none of the above; whether the respondent is still residing in the surveillance area; and whether the respondent has linked to clinical HIV services.
[Time Frame: 1 year, 2 years]

*Attitudes and beliefs*

Change in beliefs about life expectancy in the community
Definition: at baseline and at 2-month follow-up, we ask people to consider 20 people their age and gender in the community and to estimate how many they think will be alive at age 30, 40, … 80. We will construct a measure of perceived community life expectancy by linearly interpolating the responses to these questions (starting at the respondent's age) and computing the area under the curve. This value is the average number of years they expect a demographically similar person in the community to live.
[Time Frame: 2 months]

Change in beliefs about lifetime risk of HIV infection
Definition: change between baseline and 2-month follow-up in the subjective probability (out of 20) of becoming HIV-infected at some point in their life. Although we do not expect that the video will change underlying beliefs about HIV risk; it could change people's willingness to consider these risks, which would be important to know in interpreting our results.
[Time Frame: 2 months]

Change in beliefs about work and schooling
Definition: At baseline and 2-month follow-up we asked respondents about the chances (out of 20) that they would complete different levels of schooling and about the chances they would be employed in the future. We will assess changes in these beliefs.
[Time Frame: 2-months]

Change in future orientation
Definition: change between baseline and 2-month follow-up in an index of statements (strongly disagree, disagree, agree, strongly agree) measuring future orientation.

| ITEM | REVERSE CODE? |
|---|---|
| Sometimes I act spontaneously instead of thinking too much about the consequences of my actions. | X |

| | |
|---|---|
| If I get money, I tend to spend it too quickly. | X |
| I am a patient person | |
| I tend to avoid thinking about the future | X |
| I enjoy thinking about the future | |
| I am excited for what the future will bring | |
| I worry about what the future will bring | X |
| Things in life never seem to get better | X |
| My future is bright | |
| I am usually able to anticipate what the future will bring | |
| I tend to be good at predicting what will happen in the future. | |
| I find it difficult to anticipate what the future will bring | X |
| It is bad luck to try to predict what the future will bring | X |
| Thinking about what the future will bring is important to help me plan for and achieve my goals | |

The above questions include different facets of future orientation including patience, optimism, ability to form accurate future expectations, attitudes towards forming future expectations. To reduce the likelihood of false positives, we create an index of all questions related to future orientation by aggregating scores (reverse coding such that higher score for all questions reflect greater degree of future-orientation). If we observe statistical significance at the index level, we then will investigate treatment effects by individual components.

Change in locus of control

Definition: change from baseline to 2-month follow-up in an index of questions on locus of control. Research in economics that investigates the effect of perceptions on human capital formation (Bernard et al. 2014, Heckman and Kautz 2012, Heckman et al. 2006) commonly uses the Internality, Powerful Others, and Chance (IPC) scale to explore three separate components of locus of control (Levenson 1981, [20]). We use a subset of Levenson's scale, two questions per component, for each component of the IPC scale. We score respondents with a 1 if the strongly disagree and a 4 if the strongly agree. To create a composite score for each component of the scale, we then combine the total per question. A score of 8 on the internality scale implies that an individual feels the highest level of control over their lives. Based on our formative qualitative research, we also added a question on the role of external forces, e.g. God, witchcraft, etc. To reduce the likelihood of false positives, we create an index of all questions related to locus of control by aggregating scores (reverse coding such that higher score for all questions reflect greater perceptions of control). We will construct a z-score by subtracting the baseline mean and dividing by the baseline standard deviation. If we observe statistical significance at the index level, we then will investigate treatment effects by sub-scale and individual components.

| ITEM | SUB-SCALE | REVERSE |
|---|---|---|
| I can mostly determine what will happen in my life | Internality | |
| My life is determined by my own actions | Internality | |
| I feel like what happens in my life is mostly determined by powerful people, including parents, friends, and politicians. | Powerful others | X |
| People like myself have very little chance of protecting our personal interests when they conflict with those of more powerful people | Powerful others | X |
| I feel like what happens in my life is mostly determined by external forces, such as god, the ancestors, or witchcraft. | External forces | X |
| To a great extent my life is controlled by accidental/chance happenings, luck, or fate | Chance | X |
| It is not always wise for me to plan too far ahead because many things turn out to be a matter of good or bad fortune | Chance | X |

Change in health-related locus of control

Definition: We adapted the locus of control scale to focus on the domain of health. We will assess change from baseline to 2-month follow-up in a 4-item index of questions on health-related locus of control. We score respondents with a 1 if the strongly disagree and a 4 if the strongly agree. We will construct a z-score by subtracting the mean and dividing by standard deviation in the baseline survey. If we observe statistical significance at the index level, we then will investigate treatment effects by individual components.

| ITEM | REVERSE |
|---|---|
| My health is mostly the result of decisions I make | |
| My health is mostly the result of decisions other people make. | X |
| My health is mostly the result of chance happenings and luck. | X |
| My health is mostly the result of forces beyond my control. | X |

Change in life satisfaction

Definition: change from baseline to 2-month follow-up in reported life satisfaction today (0-10 scale) and in five years (0-10 scale). Subjective wellbeing (SWB) can be separated into three distinct concepts: evaluative, hedonic, and eudaimonic (Graham and Nikolov 2014; Stone and Mackie 2014). Evaluative SWB or life satisfaction is reflective of an individual's perceptions of their life as a whole,

instead of reflecting current emotional states. Hedonic SWB reflects an individual's affective state and current emotional experiences in daily life. Eudaimonic refers to wellbeing related to an evaluation of overall life purpose. Because various authors find that objective measures of wellbeing such as income and education are correlated most with evaluative well-being (Kahneman and Deaton 2010), we use this definition of SWB. To measure evaluative SWB, we use the best possible life (BPL) Cantril's Ladder question, which asks respondents to compare their life to the best possible life they can imagine on a scale from 0 to 10 (Cantril 1965) with 10 corresponding to the best possible life and 0 corresponding to the worst possible life. Because Cantril's Ladder asks respondents to compare themselves to a notional best life, answers also correlate to open-ended happiness questions and other life satisfaction questions (Graham et al. 2010). We also ask respondents to answer Cantril's ladder in 5 years, which can be interpreted as a measure of future optimism.
[Time frame: 2 months]

Change in mental health and well-being
Definition: mental health will be assessed using 7 mental health questions from the SF-12 Mental Component Survey with the greatest discrimination with respect to mental health. We will aggregate these questions using published weights [21,22], also shown below:
https://www.researchgate.net/publication/291994160_How_to_score_SF-12_items

(GH1) In general would you say your health is?
- Poor = -1.71175
- Fair = -0.16891
- Good = 0.03482
- Very Good = -0.06064
- Excellent = 0

During the past 4 weeks, have you had any of the following problems with your work or other regular daily activities as a result of any emotional problems (such as feeling anxious or depressed)?
(RE2) Accomplished less than you would like
- Yes = -6.82672
- No = 0

(RE3) Didn't do work or other activities as carefully as usual
- Yes = -5.69921
- No = 0

How often in the past 4 weeks:
(MH3) have you felt calm and peaceful?
- None of the time = -10.19085
- Some of the time = -6.31121
- A good bit of the time = -4.09842
- Most of the time = -1.94949
- All of the time = 0

(VT2) Did you have a lot of energy?

None of the time = -6.02409
Some of the time = -3.29805
A good bit of the time = -1.65178
Most of the time = -0.92057
All of the time = 0

(MH4) Have you felt downhearted and blue (reverse code)?
None of the time = 0
Some of the time = -4.59055
A good bit of the time = -8.09914
Most of the time = -10.77911
All of the time = -16.15395

(SF2) During the past 4 weeks, how much of the time has your physical health or emotional problems interfered with your social activities (like visiting friends, relatives, etc.)?
None of the time = 0
Some of the time = -3.13896
A good bit of the time = -5.63286
Most of the time = -8.26066
All of the time = -6.29724

After aggregating with these weights, we will construct a z-score based on the mean and standard deviation in the baseline data.

[Time frame: 2 months]

**B2. Comparisons to be made across treatment arms**

We have three treatment arms:
1. Intervention video (50%)
2. Attention placebo control (active control) (25%)
3. No video (pure control) (25%)

Our primary analyses will assess: *intervention video (arm 1, 50%) versus no intervention video (arms 2 and 3, 50%).* This is the highest powered test we have. It is justified under the *a priori* assumption that the attention placebo control video has no effect on outcomes. Indeed, this is the same assumption that is made (implicitly) any time a placebo control is used, with the assumption justified by the design of the placebo as not having any theoretically active components. We note that this primary analysis rests on an a priori assumption. We are not powered to test the null hypothesis that Arms 2 and 3 are the same. And because of limited power, failure to reject the null hypothesis that arms 2 and 3 are the same would not be strong evidence that the null hypothesis is true. Hence, approach 1 is identified under an a priori assumption of no difference between arms 2 and 3.

In secondary analyses, we will loosen the a priori assumption and now compare:
*A. intervention video (arm 1, 50%) versus attention placebo control (arm 2, 25%)*
This comparison assesses the impact of information on survival gains due to ART on outcomes, holding constant the nature of the video intervention (attention control). Therefore, it comes closest to capturing the theoretical construct of interest.

*B. intervention video (arm 1, 50%) versus pure control no video (arm 3, 25%)*
This comparison assesses the impact of the informational video vis-à-vis standard of care, which is no video. This comes closest to capturing the effect of policy interest. Additionally, this comparison insures us against the possibility that our a priori assumption is incorrect (i.e the placebo video does affect outcomes), giving us an estimate of effect that does not include the effect of the placebo.

## B3. Assessment of Balance

We conducted constrained randomization to ensure balance among individuals who were sampled and eligible to participate in the study. However, balance on these factors – as well as other factors not considered in the randomization – is unknown among the participants who are actually recruited into the study.

We will assess balance on the following characteristics that were included in the randomization: sex, age, whether the person was in school, orphan status, HIV positive status, HIV negative status (unknown is the third category), and household asset quintile.

We will additionally assess balance on all primary and secondary outcomes described above that are measured at baseline.

Through linkage to the AHRI surveillance, we will additionally assess balance on: whether the person has a child; household size; distance to the nearest clinic, whether anyone in the household died from HIV in the prior 10 years; whether anyone in the household is receiving public sector ART at time of baseline.

For each baseline covariate, we will report a t-test for the null hypothesis of equal means in Arm 1 vs. Arms 2+3 (pooled).

## B4. Statistical analysis of treatment effects

All statistical analyses will involve comparisons across the treatment arms described above. For each outcome, we will generate the following data:

*Example Results Table (fake data)*

| | Sample Means | | | | Treatment Effect Estimate (95% CI) | | |
|---|---|---|---|---|---|---|---|
| | Arm 1, n=XXX | Arms 2+3, n=XXX | Arm 2, n=xxx | Arm 3, n=xxx | Crude difference | Adjusted for design variables[a] | Adjusted for design, strong predictors, and baseline imbalance[b] |
| **Outcome 1** | 1.2 | 1.5 | | | -0.3 (-0.5 to 0.2) | -0.3 (-0.5 to 0.2) | -0.3 (-0.5 to 0.2) |
| | 1.2 | | 1.7 | | -0.5 (-0.8 to 0.0)* | -0.5 (-0.7 to 0.0)* | -0.5 (-0.7 to 0.0)* |
| | 1.2 | | | 1.4 | -0.3 (-0.5 to 0.3) | -0.3 (-0.5 to 0.2) | -0.3 (-0.5 to 0.2) |
| | | | | | | | |
| **Outcome 2** | | | | | | | |
| | | | | | | | |
| | | | | | | | |
| **…** | | | | | | | |
| **Outcome k** | | | | | | | |
| | | | | | | | |

[a]Design variables are those variables on which balance was explicitly assessed during randomization. [b]Strong predictors of the outcome are those factors in the baseline balance table that are djusted differences include regression adjustment for stratifying variables, variables used in the constrained randomization, and variables that were not balanced at baseline despite randomization (p-value <0.1 in F-test).

Sample means will be reported for Arm 1, the combined control (Arms 2+3), and for Arm 2 and 3 separately. When assessing changes from baseline to follow-up, we will simply define the outcomes as within-subject "differences" to facilitate comparisons with other outcomes observed only after the intervention.

Crude comparisons of outcomes between treated and comparison arms will be assessed in linear regression models, regressing the outcome on an indicator for treatment arm and indicators for week-block, a stratification variable. We will adjust our standard errors for clustering at the household level, given that error terms without households may be correlated.

Adjusted effect estimates. We will estimate adjusted effect estimates in multivariate linear regression models. Adjustment for covariates in an RCT has three aims[23]:

a) (design variables) to obtain appropriate standard errors when conducting stratified or constrained randomization;
b) (strong predictors of the outcome) to reduce residual variance in the outcome, leading to smaller standard errors and narrower confidence intervals; and
c) (factors with strong baseline imbalance) to eliminate the impact of any imbalances on observed baseline factors, despite randomization

(a) In our adjusted analyses, we will include the following design variables:

- Stratification variables: week block
- Constrained randomization variables: sex, age, whether the person was in school, orphan status, HIV positive status, HIV negative status (unknown is the third category), and household asset quintile

(b) We will include the following variables believed *a priori* to be strong predictors of HIV testing and human capital investment:

- Distance to nearest clinic
- Baseline subjective life expectancy
- Baseline beliefs about lifetime HIV infection risk

(c) Finally, to eliminate imbalances on observables, we will adjust for variables with $p<0.1$ for the t-test of differences between Arm 1 and Arm 2+3.

### B5. Subgroup analyses

The informational intervention is expected to have the greatest impact for individuals with low baseline survival beliefs and for individuals with high baseline beliefs about lifetime chances of contracting HIV.

**Perceived likelihood of lifetime HIV infection.** The impact of the informational video is likely to vary by baseline perceived risk of lifetime HIV infection. We will assess for linear effect modification of our primary outcomes by perceived lifetime infection risk.

**Baseline survival expectations.** The impact of the informational video is likely to vary by baseline survival expectations, with the greatest potential shift in beliefs for individuals with previously low survival expectations. We will assess for linear effect modification of our primary outcomes by baseline subjective life expectancy.

Finally, we will assess effect modification by age (continuous, 18-25) and sex of the respondent (M/F).

### B6. Robustness checks

We will subject our primary results (Arm 1 vs. Arms 2+3 for primary outcomes) to robustness checks.

**Logistic regression model.** We will also estimate our primary models using multivariate logistic regression to assess sensitivity to specification. In lieu of odds ratios, we will report marginal effects associated with the treatment arm, to enable comparison with the estimate from the OLS models. Again, standard errors will be clustered at the household level.

**Restrict to HIV-uninfected.** HIV testing uptake – one of our primary outcomes – is less relevant for individuals who already know their current HIV status. In a robustness check, we will assess HIV uptake after excluding: individuals who reported that they had ever tested for HIV and were certain that they were HIV positive at baseline (20 out of 20 beans); and individuals who reported testing for

HIV in the prior 3 months and reported that they were sure that they were HIV negative (0 out of 20 beans).

**Remove participants with inconsistent answers and extreme outliers.** A small number of participants reported inconsistent answers to the survival expectations questions at baseline. We will assess robustness of results after removing: participants who reported non-zero chances of survival at ages greater than the "oldest possible age" they could imagine living to; participants who reported that it was unlikely they would live to 60 years but allocated >10 beans; participants who reported that it was likely they would live to 60 years but allocated <10 beans. Demographic differences between persons with consistent and inconsistent answers will be reported.

## B7. Adjusting Inference for Multiple Hypothesis Testing

No adjustments will be made for our primary outcomes.

For secondary outcomes, the primary manner in which we adjust for multiple hypotheses being tested is by grouping outcomes into indices such that their joint significance will be tested. If we do not find that there is overall significance for aggregated measures of behaviors such as local of control, we will not move test statistical significance of individual index components. In addition, for families of outcomes where we are interested in testing the treatment effect both for an index overall as well as individual components, when the number of tests is large (greater than 10), we adjust our standard errors using the Benjamini-Hochberg procedure[24] for controlling the false discovery rate to less than 0.05.

## B7. Handling attrition

Attrition will be quantified across treatment arms. We will adjust for attrition using inverse probability weighting if we lose more than 20% of the sample between baseline and follow-up.

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## LIST OF APPENDICES

# Appendix A. Sampling Strategy

## Sampling Protocol

The sampling frame was all young adults who resided in bounded structures located in PIPSA week blocks 33-41 and were part of the AHRI PIPSA eligibility list. Young adults were defined as individuals who were 18 years or older on August 15, 2017 and were 25 years or younger on October 1, 2017. Week blocks 34-41 were selected because the AHRI surveillance was scheduled to visit these weekblocks for the annual surveillance visit and HIV testing 4 weeks after we would be there. With the AHRI surveillance 2 weeks behind schedule, we are expecting our respondents to be visited by the surveillance 4-6 weeks after the baseline survey visit. The AHRI eligibility list includes those known to reside in the surveillance and includes all those who resided in the surveillance at last visit in 2016 as well as some known to have moved in.

We randomly sampled Bounded Structures within each week block and included all individuals in each Bounded Structure. We proceeded until we had 120 individuals per Week Block, or 20 per field worker per week. We anticipate that each field worker will be able to complete 10-15 interviews per week.

The target sample size for interviews completed was 450. We assumed a 62.5% response rate, which is similar to prior studies of young adults at AHRI. Based on this response rate, we estimated that we needed to sample 720 individuals to complete 450 interviews. With a 6-field worker team and 2.5 interviews conducted per field worker per day, we project that the baseline survey will be completed in six weeks.

We sampled 8 weeks of data to ensure that if unforeseen circumstances (e.g., difficulties locating respondents, fieldworker absences, etc.) dictate lower productivity, then we will have respondents already sampled to finish fieldwork.

Our "stopping rule" for study enrollment is defined as: "Data collection will proceed for six weeks. If at the end of six weeks, we have not yet recruited 450 participants, then data collection will continue for a seventh week. If at the end of seven weeks, we have not yet recruited 450 participants, then data collection will continue for an eighth week. If after eight weeks, we still have not attained 450 participants, we will cease enrollment."

We have sampled 20 potential respondents per interviewer. Each interviewer is responsible for making three contacts with these respondents during the week. The total number (currently 20) of respondents allocated to each interviewer may change during the pilot stage, depending on what are acceptable workloads.

The following table describes the sampling strategy:

| Week Block | Sample Size | Expected Interviews completed | Under the lower productivity scenario |
|---|---|---|---|
| 34 | 120 | 75 | 58 |
| 35 | 120 | 75 | 58 |
| 36 | 120 | 75 | 58 |
| 37 | 120 | 75 | 58 |
| 38 | 120 | 75 | 58 |
| 39 | 120 | 75 | 58 |
| 40 | 120 | 0 | 58 |
| 41 | 120 | 0 | 50 |
| **TOTAL** | **960** | **450** | **450** |

# Appendix B. Consent Form

**Information Sheet and Consent to Participate in Research**

Re: Life expectations of young adults in rural KwaZulu-Natal: An Intervention Study

Dear Sir/Madam:

My name is _____, from the African Health Research Institute (AHRI), also known as the Africa Centre for Population Health, in Somkhele.

You are being invited to consider participating in a research study. Research is a systematic investigation designed to develop generalizable knowledge. The purpose of this study is to understand how young adults in this community think about and plan for the future, and how they perceive different health risks in the community. The study is expected to enroll 450 participants, ages 18 – 25 years, from households chosen at random in this area.

Participation in this study involves two sessions:

1) Interview 1 will last about two hours and will be conducted at your home. We will ask you questions about your expectations for the future and perceptions of different risks. After completing this questionnaire, you will view a 10-minute video and afterward we will ask you to answer some additional questions. You will be compensated R100 in acknowledgement of your time answering our questions and in appreciation of your participation. At the end of today's session, will provide you with some information about an opportunity to participate in a job search skills workshop, a voucher for free HIV testing, an opportunity to purchase subsidized condoms, and a savings opportunity. You do not have to participate in any of these activities to be part of the study.
2) Interview 2 will take place 2-3 months from now, will last about 30 minutes, and will be conducted by phone or at your home. We will schedule the interview using the contact number you provide us today. You will be compensated R50 for this second session.

In order to better understand life plans in this area, your survey responses will be linked and analyzed alongside survey data collected by AHRI during other visits to your household. All data will be de-identified (made anonymous) prior to being analyzed by the researchers.

Participation in this study carries a small risk of psychological or emotional discomfort. Some of the questions in the interviews may be of a personal nature. For example, we will ask you about what you expect the future to bring. We will also ask you to consider the likelihood of different life events, such as getting a

job, marriage, contracting HIV, or surviving to a certain age. If you find the material that we discuss uncomfortable, you are free to skip any questions that you do not wish to answer or to stop the interview at any time. The interview will take place in a setting that ensures your privacy. This research poses no risk of physical injury.

The study provides no direct benefits to participants, however some people find it enjoyable to answer questions about their future plans. The scientific benefits of this study will be a deeper understanding of how young adults in rural KwaZulu-Natal think about the future. There are no costs to you for participating in this research study.

In the event of any problems or concerns/questions you may contact Jacob Bor, the researcher, or Ncengani Mthethwa, AHRI/Africa Centre's Community Engagement Coordinator, at (0)35 5507500. You may also contact the UKZN Biomedical Research Ethics Committee, at (0)31 2604769 (full contact details below).

Participation in this research is voluntary. Your alternative is not to participate. If you decide to participate, you may later withdraw from the study at any point, without any penalty. You will still receive payment for the session that you attend.

Who will see the information that is collected? All the information collected is kept private and confidential. Your responses to this study will be assigned a secret number. All the data will be kept on a secure computer using only this number and not your name. Your identifiable information (such as your name, the place where you live, or your mobile phone number) will be kept in a separate secure location. In this way the data are locked so that scientists cannot link the information they are analyzing to named individuals. Scientists at AHRI and other institutions can be given permission to analyze the findings from this study and may also write about the findings in scientific journals to share the information that we learn with scientists, doctors and others in South Africa and the world. Scientific writing is never about named individuals. We take all possible steps to reduce the risk of people being identified.

Do you have any questions for me about this study?

**CONSENT**

I _____ have been informed about the study entitled Life plans of young adults in rural KwaZulu-Natal: an intervention study.

I understand the purpose and procedures of the study.

I have been given an opportunity to ask questions about the study and have had answers to my satisfaction.

I understand that my participation in this study is entirely voluntary and that I may withdraw at any time.

I understand that by consenting to participate in this study I do not waive any of my legal rights.

If I have any further questions/concerns or queries related to the study I understand that I may contact the researcher, Jacob Bor, at AHRI/Africa Centre (0)35 5507500, or at Boston University School of Public Health, USA, +1 617 429 6910. I may also contact the Community Engagement Coordinator, Ncengani Mthethwa, at the Africa Centre, (0)35 5507500.

**African Health Research Institute (also known as Africa Centre for Population Health)**
R618 en route to Hlabisa, Somkhele, KwaZulu-Natal
P.O. Box 198, Mtubatuba, 3935, KwaZulu-Natal
(0)35 5507500

If I have any questions or concerns about my rights as a study participant, or if I am concerned about an aspect of the study or the researchers then I may contact:

**BIOMEDICAL RESEARCH ETHICS ADMINISTRATION**
Research Office, Westville Campus
Govan Mbeki Building
University of KwaZulu-Natal
Private Bag X 54001, Durban, 4000
KwaZulu-Natal, SOUTH AFRICA
Tel: 27 31 2602486 - Fax: 27 31 2604609
Email: BREC@ukzn.ac.za

I have been given a copy of this form to keep.

_____ _____

**Signature of Participant** **Date**

_____ _____

**Signature of Witness (if necessary)** **Date**

## Appendix C. Randomization Procedures

**Variables Randomized.** Study participants are randomized with respect to:

1) Intervention video vs. no intervention video
2) Pure control vs. attention placebo control (among those randomized to no intervention video)
3) 20 Rand vs. 5 Rand cost to reserve spot at job search skills workshop
4) Male-vs.-female-oriented HIV transmission questions

### 1. Intervention video vs. no intervention video

Level of randomization. Individuals are randomized to video intervention vs. control. Randomization is at the bounded structure level, so that all respondents within a bounded structure get the same treatment.

Stratification. Randomization is stratified by weekblock to ensure approximately equal numbers of treated and controls within weekblocks.

Constrained (restricted) randomization. The AHRI data contain robust information on baseline covariates. However, because randomization is at the level of the bounded structure, it was not possible to stratify by individual characteristics. Instead, we use a constrained (aka restricted[25]) randomization approach. For the primary analysis – comparison of intervention video vs. no intervention video, we constrained randomization to force balance on the following characteristics: sex, age, whether the person was in school, orphan status, HIV positive status, HIV negative status (unknown is the third category), and household asset quintile. We randomized the intervention treatment assignment (intervention video vs. no intervention video) 3078 times using different randomization seeds. Each time, we assessed balance on key characteristics through a t-test (i.e regression of treatment assignment on the covariate). All randomization seeds that lead to a maximum t-test value greater that 1.68 ($p<0.1$) were excluded. This excluded 62% and retained 38% of the seeds (1060 seeds). We then randomly selected two seeds from these 1060 acceptable seeds. One was used to randomize the video, the other was used to randomized the incentive for the job search skills workshop. We assessed correlation of these two interventions and re-randomized if the Chi2 p-value for the cross-tab was less than 0.5 (see tables below).

### 2. Pure control vs. attention placebo control (among those randomized to no intervention video)

Within the control group – i.e. individuals assigned to "no intervention video" – we used simple randomization to assign respondents to "pure control" (no video) and to "attention placebo control" (video on another topic).

### 3. 20 Rand vs. 5 Rand to reserve spot at job search skills workshop

Participants were given 30 or 15 Rand and were then asked to pay 20 or 5 Rand respectively to reserve a spot in the job search skills workshop. Randomization was conducted as described above in concert with the intervention video comparison.

### 4. Male-vs.-female-oriented HIV transmission questions.

In the survey, respondents were asked to report on the chances that an HIV-negative woman (man) would become HIV-infected after having sex with an HIV-positive man (woman). We used simple randomization to assign respondents to the male-focused or female-focused HIV transmission questions.

## Data management procedures

These random treatment assignments – video assignment, job search workshop incentive assignment , together with their AHRI internal ID numbers were submitted to the AHRI data management core, which linked them with the operational data.

## Blinding

The investigators are blinded to randomization assignment throughout data collection, cleaning, and development of the analysis plan. Randomization status with respect to the intervention and placebo videos will not be revealed until after all data on the primary outcome (including uptake of testing in the 2017 AHRI surveillance) have been collected, the data have been cleaned, and final pre-analysis plan is posted.[26]

**Tables. Intervention video stratified by weekblock and approximately balanced vs. Job Skills Workshop incentive**

```
. tab week TreatmentArm
```

| week | TreatmentArm C | T | Total |
|---|---|---|---|
| 34 | 60 | 60 | 120 |
| 35 | 60 | 60 | 120 |
| 36 | 60 | 60 | 120 |
| 37 | 61 | 59 | 120 |
| 38 | 60 | 60 | 120 |
| 39 | 60 | 60 | 120 |
| 40 | 59 | 61 | 120 |
| 41 | 62 | 58 | 120 |
| Total | 482 | 478 | 960 |

```
. tab TreatmentArm JobWorkshopArm, chi2
```

| TreatmentArm | JobWorkshopArm C | T | Total |
|---|---|---|---|
| C | 245 | 237 | 482 |
| T | 243 | 235 | 478 |
| Total | 488 | 472 | 960 |

Pearson chi2(1) = 0.0000 Pr = 0.998