Surviving Bad News: Health Information Without Treatment Options

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Abstract

Providing personal health information allows individuals to take action to improve their health. If treatment is not available, however, being informed about having a life-threatening disease could lead to feelings of despair or fatalistic behaviors resulting in negative health outcomes. We document this possibility utilizing an experiment in Malawi that randomized incentives to learn HIV testing results in a context where anti-retroviral treatment (ART) was not yet available. Six years after the experiment, receiving an HIV+ diagnosis reduced survival rates by 23% points and this effect persists after 15 years. We show that HIV+ persons who learned they were HIV+ engaged in more risky health behaviors, have greater anxiety and a higher discount rate. We do not find any effects of receiving an HIV– diagnosis on survival.

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1 Introduction

Improving individuals' information about their health is often seen as a key strategy for facilitating healthy behaviors and the adoption of disease prevention strategies (Dupas, 2011; Sarma *et al.*, 2019). A large body of evidence has documented that screenings for underlying health risks can increase investments in preventive health care, help individuals obtain the right treatment, and reduce risky behaviors (Bankhead *et al.*, 2003; Deutekom *et al.*, 2011).[1] Most research studying the effects of health information on behavior and health outcomes has occurred in contexts where individuals can effectively respond by accessing treatment. In the context of rapidly expanding health screening options, a key question that has not been widely addressed in the existing literature is what are the effects of improved health information when individuals have limited ability to act on information? This question is particularly relevant in contexts where treatments are not yet available or where healthcare systems are weak such as in low-income settings.[2] Without treatment options, health information could potentially lead to worse mental health and fatalistic behaviors (e.g., Kerwin, 2020).[3]

This paper fills the gap in the literature by measuring the causal effects of HIV testing on subsequent mortality during a time in which treatment for HIV was limited. We use 15 years of longitudinal data after a 2004 field experiment conducted in Malawi that provided exogenous variation in information about individuals' knowledge of their HIV status. Survey data and HIV tests were collected from respondents of the Malawi Longitudinal Study of Families and Health (MLSFH, Kohler *et al.*, 2015). Participants were offered a randomized incentive to learn their HIV results at results centers positioned randomly across geographical locations. Very few individuals had obtained HIV testing before this experiment. The incentives and distance to results centers created variation in individuals' learning of their HIV status (Thornton, 2008). As a result, this experiment provides us with one of the few study contexts with exogenous variation in individuals' health information (their HIV status) conditional on their known–to the researchers–underlying health condition (HIV infection).

Important for the context of our study is that anti-retroviral treatment (ART), which reduces the morbidity and mortality consequences of HIV infection, became available in rural Malawi only in 2007 (Baranov and Kohler, 2018), three years after the experiment, with treatment gradually

[1] In the UK, randomized controlled trials have found that individuals who receive medical check-ups and screenings for cardiovascular risk factors experience a decrease in coronary risk scores, smoke less, lose weight, and improve their blood pressure/cholesterol (Wood *et al.*, 1994; Group, 1995). In other settings, screenings have been found to increase the likelihood of obtaining hypertension medication (Ciancio *et al.*, 2021; Kämpfen *et al.*, 2023), lead to better mental health (Baranov *et al.*, 2020), increase the likelihood of the correct use of malaria pills (Cohen *et al.*, 2015), and the use of HIV and tuberculosis treatment (McNerney, 2015).

[2] Many diseases such as pancreatic cancer, Huntington's disease or Parkinson's have limited treatment options even in the most technologically advanced countries. In developing countries, the healthcare system may not be prepared to treat serious but curable diseases.

[3] A drop in life expectancy induced by bad news can also lead to a decrease in health investment and healthy behaviors (Oster *et al.*, 2013a), an effect referred to as the Mickey Mantle Effect (Fang *et al.*, 2007). Technological innovations and cost reductions increasingly expand the access to and potentials of health screenings to improve individuals' information about their own health (Bhatt *et al.*, 2018; Moses *et al.*, 2022; Marinucci and Dhein, 2022), in part as a result of global health programs (Angell *et al.*, 2015; Marinucci and Dhein, 2022).

expanding thereafter. As of 2010, 27% of HIV+ individuals in Malawi were receiving ART, and this rate did not exceed 75% until 2017 (AIDS info, 2022). In 2004, and for a considerable period thereafter, informing individuals about a positive HIV status was equivalent to providing "bad news": while the information could result in behavioral changes that reduced the risk of transmission to others, HIV+ individuals did not yet have viable options to reduce the morbidity and mortality consequences associated with the progression of HIV to AIDS. Ultimately, receiving an HIV+ diagnosis implied virtual certainty of premature death (Lewin, 2013). Not surprisingly, despair, fatalistic behaviors, depression and other negative life course consequences have often been documented among HIV+ individuals prior to the widespread availability of ART (Van Dyk and Van Dyk, 2003; Valente, 2003; Ejrnæs *et al.*, 2023).

Also potentially important for this study are the implications of receiving an HIV– diagnosis. For the same reasons why receiving "bad news" could have detrimental health effects, receiving "good news" could result in beneficial health outcomes. However, in a context with relatively high HIV prevalence and overall mortality, receiving an HIV– diagnosis would be unlikely to contain persistent informative value about one's long-term health. Indeed, Thornton (2012) found in this setting that there was no impact of receiving an HIV– diagnosis on short-run subjective beliefs of one's HIV status, consistent with a model in which individuals face further risk after receiving "good news" about their health. Ultimately, the effect of receiving an HIV– result on long-term mortality is ambiguous.

Using the 2004 experiment and unique longitudinal vital statistics data, we measure the impact of learning HIV status on mortality two, four, six, and fifteen years after the experiment. Attrition in vital status information is low in our study (less than 8% after 6 years) and uncorrelated with incentives and distance to HIV results. We find that HIV+ individuals who learned their status in 2004 were less likely to survive than HIV+ individuals who did not learn their status, and the effect is large: four years after the HIV testing in 2004, individuals who were HIV+ and learned about their status were 18% points less likely to have survived compared to HIV+ individuals who did not learn their status. The effect on survival is 23% points two years later (in 2010) and remains at 23% in 2018-2019— 15 years after the initial HIV testing in 2004. The effects we measure are causal, obtained by two-stage least-squares regressions instrumenting "learning one's HIV status" with the randomized financial incentives offered to study participants if they obtained their HIV test results at a local MLSFH study site, and the distance to these clinics (also randomized). In contrast to the stark mortality effects we estimate on HIV+ individuals, discovering one's HIV– status had no discernible effect on survival.

We then investigate the potential mechanisms that could underlie our results. We use data from a follow-up survey conducted in early 2005, just a few months after the experiment, which provides information on respondents that would not have yet been significantly impacted by the differential mortality selection implied by the effects on survival. We find that respondents who learned they were HIV+ were more likely to drink alcohol in 2005, worried about their health, worried about having enough food for their family, and were more present-oriented as measured by

a large change in time preferences. Among those who learned they were HIV–, all of the estimated coefficients are very small and close to zero. Taken together, our analyses suggest heightened risky health behaviors and anxiety in HIV+ individuals who learned their status compared to those who remained unaware, and no impact on those who learn they are HIV–.

Overall, our paper provides novel insights into the relationship between health information and survival using a rare study context that provided exogenous variation in health information to study participants (HIV test results) conditional on their underlying health condition (HIV infection). We use 15 years of data on survival with low attrition to produce the first causal estimate that receiving an HIV+ diagnosis can lead to long-term increases in mortality, with despair, increased risk-taking, and fatalism being among the possible mechanisms. On the other hand, learning to be HIV– does not lead to any effect on survival or behaviors.

The finding that health information revealed by well-intended screening interventions can lead to reduced survival when individuals do not have viable treatment options for the indicated diseases has far-reaching implications. For example, our results can possibly explain why individuals sometimes actively choose not to learn their health status and avoid being tested, documented in the case of Huntington's disease (Oster *et al.*, 2013b) and more recently for Covid-19 (Thunström *et al.*, 2021). This is true for both those who learn they are positive and negative.

Several policy implications arise from our results as developing countries continue increasing the capacity and access to health screenings at a pace that much exceeds the expansion of their capacity to treat corresponding diseases (Pai and Temesgen, 2017; Jacobson *et al.*, 2013). Providing health diagnoses for diseases without accessible treatment should be done with caution. More generally, adequate protocols for communicating "bad news" revealed by health screening tests should be developed to limit the negative mental health implications of screening outcomes (Bor *et al.*, 1993; Kpanake *et al.*, 2016). In addition, the advantage for learning negative results may be small if individuals continue to face risk for a disease.

This paper makes contributions to various strands of the literature. Information interventions have been found to be a cost-effective way to improve health behaviors but are not always successful (Dupas, 2011). This paper shows that sometimes health information can even have unintended consequences, even affecting survival. Similarly, also in the context of HIV in Malawi, Kerwin (2020) finds that people tend to overestimate the HIV transmission risk and evidence for fatalism. Our findings also point to indirect evidence of fatalistic behaviors upon receiving an HIV+ diagnosis, as we show that individuals who learn to be HIV+ are more likely to engage in risky health behaviors only a few months after learning their HIV results. Other papers have investigated the effects of providing information on HIV status on health and economic behaviors with mixed results.[4] Yet,

[4]Gong (2015) finds that an (unexpected) HIV+ diagnosis leads individuals to increase risky sex as evidenced by a higher incidence of sexually transmitted infections (STIs), while receiving an HIV– diagnosis reduces risky sexual behavior. Leveraging the same exogenous variation in health information as our study, Delavande and Kohler (2012) finds that those who learned to be HIV+ have fewer sexual partners and are more likely to use condoms, albeit it is unclear whether this result is driven by altruistic behavior or protective behavior, while the effect of learning to be HIV– on these outcomes is ambiguous. Our results are also in line with Thornton (2008) who finds that learning to be HIV+ increases condom purchases but has no effect for those who learned to be HIV-. Thornton (2012) finds no

none have examined the impact on survival.

2 Experiment and Data

2.1 Sample and HIV testing in 2004

The Malawi Longitudinal Study of Family and Health (MLSFH) is an ongoing longitudinal study of individuals in three regions of rural Malawi, Rumphi in the North, Mchinji in the Center and Balaka in the South. The study population is broadly representative of the rural population in Malawi. The MLSFH Cohort Profile provides detailed information on sampling procedures, study design and study instruments (Kohler *et al.*, 2015). The first wave was collected in 1998 and consisted of a random sample of married men and women from 125 villages listed in the 1998 census. In 2004, a random sample of young individuals (age 14-24) was added to the sample from the same villages. Also in the 2004 survey, MLSFH respondents were offered for the first time, the opportunity to get tested for HIV.[5] The majority of respondents who were offered an HIV test accepted (91%). Of those who tested, 6.1 percent were HIV+ in 2004.[6]

Our main analytical sample consists of 2,823 respondents who were tested for HIV in 2004. Table 1 presents the basic summary statistics for the full sample of respondents as well as separately for those who were HIV+ (N=173) and HIV– (N=2,650). The average age of the sample is about 39 years, and women represent 54% of the study population (Table 1 panel 1). Respondents are relatively well distributed across the three study areas, with 36%, 35% and 29% of them coming from the South, North, and Central region of Malawi, respectively.

2.2 Experimental design

In 2004, the HIV testing during the survey coincided with an experiment that exogenously varied costs and benefits to learn the results, available two to four months after testing, at mobile clinics located randomly within respondents' villages. Respondents were offered financial incentives of varying amounts to obtain their test results ranging from 0 to 300 Malawian Kwachas (MWK, about USD 2.80), the latter corresponding to approximately two days worth of work of wages at that time.[7] Respondents who obtained their HIV results received the amount of the financial incentive offered to them at the time of testing.[8] In addition to the financial incentives, the location of the health clinics where respondents could pick up their test results was also randomized relative to the village center in which they live to make sure no respondent had to walk too far to pick up

effects of learning HIV status on economic behaviors two years after the 2004 experiment. In our study, we analyze what happens a few months after the intervention to overcome concerns related to selective attrition.

[5] 82% of the respondents had never participated in any HIV counseling and testing (Delavande and Kohler, 2012).

[6] The relatively low HIV+ prevalence in our sample compared to other estimates at that time is explained by the rural study area and exclusion of high HIV prevalence peri-urban areas in Malawi (Obare *et al.*, 2009).

[7] Respondents were offered the opportunity to receive monetary incentives after the specimens were collected.

[8] Respondents obtaining their HIV test results, irrespective of their HIV status, received HIV counseling about safe-sex practices, including abstinence and condom use, and were also offered condoms. Those who received a positive test result were referred to the nearest permanent clinic for further counseling.

Table 1: Summary statistics of the study sample

	All *(N = 2,823)*		***HIV−*** *(N = 2,650)*		***HIV+*** *(N = 173)*	
	Mean (1)	Sd (2)	Mean (3)	Sd (4)	Mean (5)	Sd (6)
			Individual characteristics			
Male	0.457	0.498	0.463	0.499	0.364	0.483
Age	39.205	13.948	38.918	13.991	43.601	12.510
South	0.357	0.479	0.353	0.478	0.416	0.494
North	0.348	0.476	0.355	0.479	0.243	0.430
			2004 Experiment			
Any incentive	0.764	0.425	0.765	0.424	0.751	0.433
Incentive (amount in $)	0.985	0.901	0.988	0.901	0.940	0.907
Distance (in km)	2.001	1.259	2.012	1.255	1.837	1.314
Learned HIV status	0.689	0.463	0.691	0.462	0.653	0.477
			Attrition in vital status information			
2006 information	0.976	0.152	0.977	0.150	0.965	0.184
2008 information	0.954	0.209	0.956	0.205	0.925	0.264
2010 information	0.938	0.241	0.939	0.240	0.931	0.255
2018-19 information	0.826	0.379	0.823	0.382	0.879	0.328
			Outcome variables			
Alive in 2006	0.989	0.105	0.994	0.078	0.910	0.287
Alive in 2008	0.967	0.180	0.977	0.151	0.806	0.396
Alive in 2010	0.947	0.225	0.958	0.200	0.770	0.422
Alive in 2018-19	0.870	0.336	0.888	0.315	0.612	0.489

Note: The sample is derived from all respondents who got tested for HIV in 2004 and have no missing values in basic demographic characteristics in the 2004 survey. "Sd" stands for standard errors and "N" for the number of observations. The number of observations available for each outcome variable (bottom panel) can be obtained by multiplying the sample sizes in the header of the table with the corresponding attrition rates for a given survey year.

their results. More information on the 2004 research design is provided in Thornton (2008) and Obare *et al.* (2009).

The second panel of Table 1 provides summary statistics of variables related to the 2004 HIV testing experiment. 76.4% of the respondents received a financial incentive, with similar rate across HIV– and HIV+ (76.5% vs 75.1%, p-value=0.705 for a two-sided test of equal means), showing the randomization of the financial incentive across these two groups is well distributed. On average, respondents received the equivalent of about $0.99, the financial equivalent of a bit less than one day's worth of work at that time. Again, the average incentive amount by HIV status is distributed evenly ($0.99 vs $0.94, p-value= 0.494). Respondents, on average, live two kilometers away from the health clinic where they could obtain their HIV test results. The histograms of financial incentives and distance to clinics are presented in the Appendix (Figure A.1). 68.9% of the individuals in our

sample who were tested for HIV in 2004 learned their HIV status at results clinics, at rates similar across HIV status (69.1% vs 65.3%, p-value=0.314).

2.3 Vital status information

MLSFH respondents were followed in 2006[9], 2008 and 2010, and again in 2018–19.[10] During each MLSFH wave, information on the vital status of each respondent who participated in the 2004 survey was collected. Using this information, we generate indicators of survival in 2006, 2008, 2010, and 2018/2019 which we use as main outcome variables.[11]

The third panel of Table 1 summarizes the availability of vital status information (e.g., attrition) of respondents in each of the four survey years of the analysis. Out of the 2,823 individuals who tested for HIV in 2004, the vital status—that is whether they were dead or alive—is known in 2006 for 97.6%. Over time, information attrition rises but remains relatively low with 95.4% and 93.8% of respondents with known vital statuses in 2008 and 2010, respectively. Fifteen years after the initial HIV testing, we know vital status information for 82.6% of the sample. Attrition is very similar across HIV status. We discuss attrition further below as it relates – and in our case is not significantly associated – to our exogenous financial incentives and distance to HIV results centers. We also provide robustness checks for our results accounting for attrition.

The bottom panel of Table 1 presents summary statistics on survival, showing stark differences between those who tested HIV+ and HIV– in 2004. Almost all (98.9%) individuals who tested for HIV in 2004 survived until 2006. This rate is much lower for those who tested HIV+ in 2004 (91%). Corresponding statistics show that 96.7%, 94.7% and 87.0% of the respondents in the overall study sample survived until 2008, 2010 and 2018/2019, respectively. Survival rates are much lower among HIV+ respondents (80.6%, 77.0% and 61.2% for 2008, 2010 and 2018/2019, respectively).

2.4 Follow-up Survey in 2005

A few months after the HIV results were made available, in January–February 2005, a follow-up survey in two of the three regions (Balaka and Rumphi) was implemented (Thornton, 2008). We use information from this 2005 survey to explore potential mechanisms through which learning one's HIV status affects survival. The crucial advantage of using this information is that it was collected just a few months after respondents had the opportunity to learn their test results and therefore before the treatment could significantly affect mortality.[12] One disadvantage is that the

[9] A 2007 survey followed respondents identified as migrants in 2006. We combine information on these migrants with respondents from the 2006 survey wave.

[10] After 2010, the MLSFH focused on mature adults (age 45 and above) for several waves (2012, 2013, 2017 and 2018). Respondents who were not interviewed in 2018 were interviewed in 2019 (Kohler *et al.*, 2020).

[11] In cases where a respondent was not located during a survey wave, efforts were made to contact their family members and members of the village to ascertain whether the individual was temporarily absent, had migrated, or had passed away. Unfortunately, the year of death is unknown which prevents us from conducting a standard survival analysis.

[12] The first full MLSFH survey was conducted two years after the intervention, in the summer of 2006, by which time already 9% of HIV+ individuals had died.

2005 follow-up survey was implemented only in two of three regions, thus reducing the sample size to 1,491 individuals.[13] The survey included questions related to drinking behaviors, health worries, economic uncertainty, time preferences, and life expectancy. Table A.1 in the Appendix presents basic descriptive statistics of the variables used in the analysis. We proxy mental distress and anxiety with a set of questions in which respondents were asked whether they had been worried in the past two months.[14] To capture economic distress and uncertainty, respondents were asked to what extent they agree with statements of worry.[15] We create a measure of time preference by deriving the amount of MKW an individual reported (hypothetically) needing to be compensated in one month, to give up 500 MKW in the present. As a measure of risky behaviors, we use information on alcohol consumption.[16] Finally, respondents were asked how long they believed they would live, from which we derive a measure of subjective life expectancy (Delavande and Kohler, 2016; Ciancio *et al.*, forthcoming).[17]

In our analysis, we create an index for each domain (worry, economic uncertainty, time preference, risky behaviors, and subjective life expectancy). For worries and economic uncertainty, we take the sum of each item. For risky behaviors, due to lack of variation in our extreme drinking behaviors, we create a dichotomous variable that takes the value one if respondents admitted to having drunk alcohol in the past 12 months, and zero otherwise. Time preference and life expectancy have only one variable for each domain. In the Appendix, we show results for each variable separately.

3 Empirical Strategy

3.1 Econometric Specification

Our goal is to estimate the causal effect of learning one's HIV status on the probability of survival. In typical settings, a regression of survival on learning one's HIV status is likely to suffer from endogeneity bias because individuals who decide to learn the result of their HIV tests, irrespective of whether they are HIV+ or not, are likely to be different from those who do not. These differences,

[13] There is some selective attrition from the 2005 follow-up survey, with individuals who learned they were *HIV–* more likely to participate than those who did not learn their HIV– status ($\beta = 0.257$, p-value< 0.001). Importantly, however, this difference in attrition is not observed for those who were *HIV+* ($\beta = -0.096$, p-value$= 0.648$). Participation of HIV+ respondents is also uncorrelated with our set of instrumental variables. Selective attrition is therefore unlikely to be important for those who are HIV+, and we present robustness for attrition for all of our analyses.

[14] These worries include: (a) HIV, (b) health problems (other than HIV), (c) paying school fees, (d) finding or earning enough money, and (e) having enough food. Of these, 7.1%, 37.2%, 20.9%, 82.8% and 93.1% of the respondents reported being worried about these items, respectively.

[15] These statements include: (a) "My family often does not have enough food", (b) "I often don't have enough money for basic essentials", (c) "Only God can determine the future", (d) whether it would be easy for respondents to obtain a small amount of money (500 Kwachas, about USD 4.73) if needed, and (e) whether children will (not) take care of the respondents when old.

[16] 12.8% of the respondents report consuming alcohol in the past 12 months, 6.3% report being drunk at least once during the previous week (9.4% among HIV+) with 5.2% report being drunk during (non-Friday) weekdays. About 65% of the individuals who reported having consumed alcohol in the past 12 months also report having been drunk at least once during the previous week.

[17] The average reported remaining life expectancy (life expectancy minus age in 2005) is 26.7 years for HIV– but only 19.7 years for HIV+ individuals, a gap of 7 years.

if correlated with the probability of survival, will bias the estimate, making any causal claims hard to defend. To circumvent this endogeneity, we instrument the probability of an individual learning their HIV status with a set of instrumental variables that were randomly assigned to respondents, specifically: the financial incentives and distance to health clinics. Because receiving an HIV+ or HIV– diagnosis is different, we distinguish between these two effects and predict our two endogenous variables (learning HIV+ status and learning HIV- status) with our set of instrumental variables interacted with HIV status in 2004.

Following the specification used in Delavande and Kohler (2012) and Thornton (2008), the set of instrumental variables we use in our analysis includes: an indicator of being offered any financial incentive, the amount of the financial incentive offered and its squared term, distance to the clinic where individuals could pick up their test results and its squared term. The specific model we estimate is as follows:

$$Survival = \alpha_1 + \beta_1 LearnHIVneg + \beta_2 LearnHIVpos + X\gamma_1 + \epsilon_1$$

$$LearnHIVneg = \alpha_2 + Z\delta_2 + X\gamma_2 + \epsilon_2$$

$$LearnHIVpos = \alpha_3 + Z\delta_3 + X\gamma_3 + \epsilon_3$$

where $LearnHIVpos$ is the interaction of learning one's HIV status with being HIV+ in 2004 and similarly $LearnHIVneg$ is the interaction of learning one's HIV status with being HIV– in 2004. These two variables are the key independent variables of interest. We predict these two variables with our set of exogenous instruments, their interactions with HIV status in 2004, and a set of predetermined demographic variables X, which include age, age-squared, sex, region fixed effects, and HIV status. β_1 and β_2 are the coefficients of interest and represent the causal effects of receiving an HIV– and HIV+ diagnosis, respectively, on the probability of survival. The standard errors are clustered at the village level to account for correlation of survival among individuals living within the same village.[18] We estimate this IV model using GMM.[19]

3.2 Validity of the instruments

The experiment randomized financial incentives and distance to the clinic, which we use as instrumental variables to predict whether an individual learns their HIV status. Ideally, the incentives and distance should be orthogonal to baseline individual characteristics. To test for orthogonality, we regress baseline individual characteristics of age, gender and HIV status, on the set of instruments.[20] Results are shown in Table A.2. None of the instruments predicts baseline characteristics of individuals in our sample, although we find a relationship between distance and the probability of

[18] Respondents in our study sample are living in 121 villages.

[19] In the case of overidentified models, IV-GMM cluster robust estimates have been shown to be more efficient than standard 2SLS estimates (Baum, 2007).

[20] More specifically, we estimate the following specification: $X_i = Z_i\gamma + \alpha_d + \epsilon_i$ with $X_i = \{sex_i, age_i, hiv+_i\}$ and $Z_i = (any\_incentive_i, incentive_i, incentive_i^2, distance_i, distance_i^2)$. α_d are district fixed-effects and we cluster the standard errors at the village level.

being HIV+. This is likely due in part to the smaller sample of HIV+ respondents. The association between distance and HIV+ disappears when allowing these associations to differ by region, as one would expect given how the randomization was implemented (see Table A.3 in the Appendix).[21] Overall the baseline characteristics are similar by financial incentive and distance to the clinic.

Table A.4 in the Appendix shows the first stages of the IV regressions when considering our two endogenous variables separately. Effective F-statistics following Olea and Pflueger (2013) reported at the bottom of each column show that they are reasonably strong enough to predict learned HIV results (Staiger and Stock, 1997; Stock and Yogo, 2005).

3.3 Attrition

While the overall attrition rate in vital status, at 6% six years after the experiment, is notably low compared to studies in similar contexts, there is still a potential concern regarding differential attrition across treatment groups. However, our findings remain very robust to using econometric models that account for selective attrition. First, using our baseline instrumental variable specification, learning to be HIV– or HIV+ is unrelated to the availability of vital status information in any of the years these data are available (Table A.5). Second, our estimated treatment effects on survival using our main specification outlined above are robust to using inverse probability weighting (Table A.6), where the weights are calculated using the probability of observing vital status information conditional on individuals controls and instruments for a given survey year. Finally, akin to Lee (2002) and Horowitz and Manski (2000, 1998), we bound treatment effects by randomly assigning vital status to respondents with missing data, allowing the mortality rates to be different across those who are HIV–, those who are HIV+ and learned about their status, and those who are HIV+ and did not learn about their status.[22] Again, estimated treatment effects are very robust to this simulation exercise. Overall, we find that selective attrition, if any, does not influence our main results.

4 Results

We first present the main effects of learning HIV results on survival across each wave of vital statistics data collection (2006, 2008, 2010, 2018/19) and then examine potential mechanisms using the 2005 follow-up data, collected two to four months after HIV results were made available in 2004.

[21] The locations of the health care clinics depend on the geographical features of the villages and districts. The Northern region – Rumphi – is much more mountainous and sparse than the two other regions and has a lower prevalence of HIV+.

[22] More specifically, we set the mortality rate of those who are HIV– at the rate we observe in our data (2.3% in 2008, 4.2% in 2010, and 11.2% in 2018) and then compute our estimates for *all the various possible combinations of mortality rates* among those who are HIV+ and learned about their status, and those who are HIV+ and did not learn about their status. Practically, we simulate our data by randomly assigning living status to observations with missing outcomes a 100 times *for each possible combination of mortality rates* and report the mean of the corresponding distributions of the parameters of interest. More details are presented in Appendix A.1.

Table 2: The effects of learning HIV status on survival

Respondent is alive in:	2006		2008		2010		2018-19	
	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	IV (8)
Learn HIV–	0.005	0.001	0.006	0.002	0.013	-0.007	0.009	0.006
	(0.003)	(0.007)	(0.006)	(0.014)	(0.009)	(0.014)	(0.015)	(0.024)
Learn HIV+	-0.054	-0.110	-0.114**	-0.183*	-0.111*	-0.231**	-0.157*	-0.227*
	(0.038)	(0.089)	(0.055)	(0.111)	(0.060)	(0.113)	(0.082)	(0.128)
Observations	2756		2694		2649		2332	
Mean outcome: did not learn HIV–	0.992		0.977		0.955		0.891	
Mean outcome: did not learn HIV+	0.946		0.885		0.849		0.729	

Note: The table shows estimates of the effect on the probability of being alive for various years. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$: reported in parentheses are standard errors that are clustered at the village level. All specifications control for HIV status, sex, age and its square, and region fixed effects. The instruments to predict whether respondents learned their HIV status are: whether respondents received any financial incentive, the amount of the incentive, the square of the amount of the incentive, the distance to the health clinic where respondents can obtain their results, the square distance, as well as all of their interaction with HIV status. The last two rows correspond to the mean outcome for those who did not learn their HIV status conditioning on their status.

4.1 Effects on Survival

Table 2 reports the OLS and IV regression results on the likelihood of survival. We focus our attention on the IV results and also report the OLS estimates for completeness. IV estimates should be interpreted as local average treatment effects (LATE) as the effects are driven by those who respond to the incentives.

Two years after the HIV test experiment –when overall survival is still at 99 percent for HIV– individuals–, there is no effect of learning HIV– results on survival. Survival among those who are HIV+ is lower than survival for those HIV- and the effect of learning HIV+ results on survival is negative and large (11 percentage points) but is not statistically significant at conventional levels (p-value=0.217).

In 2008, four years after the experiment, we find a negative and large effect on decreased survival by 18 percentage points, after learning one is HIV+. The effect is statistically significant at the 10% level (p-value=0.099). We do not find any effect of learning HIV– results on survival, with coefficients that are small and close to zero also in the following years. The results for those who were HIV+ are persistent in 2010, with an effect on decreasing survival of 23 percentage points, which is statistically significant at the 5% threshold (p-value=0.041).

Around 15 years after the experiment, the effects of learning one is HIV+ persist with approximately the same magnitude (23 percentage points) but with a slightly higher p-value (0.077) possibly due to a somewhat smaller sample size. The gap in mortality rate between those who learned and those who did not learn about their HIV+ status therefore remained constant once testing and ART became free and widely available in the late 2000s in rural Malawi.[23]

[23]We could have expected ignorance about own HIV+ status to increase survival in the short term with no corresponding difference in survival 15 years later. The introduction of ART in 2007 may have allowed the individuals who survived those initial years after the experiment to survive much longer.

The small sample size of HIV+ individuals prevents us from doing a credible heterogeneity analysis but in the next section, we explore possible pathways that explain the effects on survival using outcomes from the 2005 follow-up survey.

4.2 Mechanisms

Our key finding in the prior section is that learning HIV+ results has a negative effect on subsequent survival. Several behavioral mechanisms could underlie this outcome. First, it could deteriorate mental health leading to worse health behaviors and worse economic conditions which in a context of extreme poverty could increase mortality. Some studies have indeed shown that mental health predicts mortality (Keyes and Simoes, 2012). Second, an upward revision of the likelihood of being HIV+ could lead to fatalism (Kerwin, 2020) which in turn could lead to more risky health behaviors, a decrease in protective behavior, and lower demand for healthcare (Oster *et al.*, 2013a; Fang *et al.*, 2007). Moreover, if the HIV status of a particular individual becomes known within a community, this could lead to stigmatization and marginalization, which could affect other members of the family as well, leading to deterioration in social and economic life. Finally, learning to be HIV+ could change time preferences and investment decisions leading to risky health behaviors and poor (forward-looking) decision-making, which could in turn affect chances of survival (Lawless *et al.*, 2013; Chao *et al.*, 2009; Fuchs, 1980). To test possible pathways through which receiving an HIV+ diagnosis could affect survival, we use information from the follow-up survey which was collected a few months after the HIV test results were made available.

Table 3 presents the effects of learning one's HIV status on various possible pathways using data from the 2005 followup survey. To account for multiple hypothesis testing, we also report adjusted p-values following Simes' method, a step-up method that allows us to address the issue of false discovery (Simes, 1986; Newson, 2010; Benjamini and Yekutieli, 2001).

First, receiving an HIV– diagnosis has no discernible impact on any of the outcomes. The coefficient on receiving an HIV– diagnosis on worries is small (-0.289) compared to the mean. While the p-value is 0.029 in the main specification, the coefficient becomes statistically insignificant when correcting for the false discovery rate. Looking at each worry separately in Table A.7, we find that those who learn they are HIV– are less worried about AIDS and in part less worried about other health problems which is what one would expect.

Receiving an HIV+ diagnosis increases worries by 0.84 (or 33.3% of the mean of HIV+ who did not learn their results) and economic uncertainty by 0.93 (22.4%). Relative to those who were HIV+ but did not know about it, individuals who learned they were HIV+ reported needing an additional USD 5.30 (77.3%) in one month in order to give up USD 4.73 in the present, which shows a large shift in valuing the present relative to the future. We also find that those who learn they are HIV+ are 21 percentage points more likely to drink alcohol in the past 12 months. All of these results survive the corrections for multiple hypothesis testing.

Looking at each component separately, we find a particularly strong effect of learning to be HIV+ on worries about AIDS and other health problems (Table A.7), and in the uncertainty of not

Table 3: The effects of learning HIV status on various mechanisms

	Sum of worries	Sum of economic uncertainties	Time preference	Drink alcohol past 12 months	Subjective life expectancy
	(1)	(2)	(3)	(4)	(5)
Learn HIV-	-0.289	-0.185	-1.102	0.057	2.481
	(0.133)	(0.243)	(1.548)	(0.038)	(2.008)
p-value	0.029**	0.447	0.477	0.130	0.217
Simes correction	0.146	0.477	0.477	0.326	0.361
Learn HIV+	0.840	0.934	5.297	0.211	-1.745
	(0.363)	(0.390)	(1.906)	(0.102)	(3.601)
p-value	0.021**	0.017**	0.005***	0.039**	0.628
Simes correction	0.035**	0.035**	0.027**	0.048**	0.628
Observations	1474	1474	1427	1441	1270
Mean outcome: did not learn HIV–	2.414	4.545	10.973	0.127	27.713
Mean outcome: did not learn HIV+	2.526	4.158	6.856	0.105	18.056

Note: The table shows estimates of the effect on various mechanisms. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$: reported in parentheses are standard errors that are clustered at the village level. All outcomes variables were measured in 2005 as part of a follow-up survey a few months after the intervention. Descriptive statistics of the outcome variables are detailed in Appendix Table A.1. All specifications control for HIV status, sex, age and its square, and region fixed effects. The instruments to predict whether respondents learned their HIV status are: whether respondents received any financial incentive, the amount of the incentive, the square of the amount of the incentive, the distance to the health clinic where respondents can obtain their results, the square distance, as well as all of their interaction with HIV status. The last two rows correspond to the mean outcome for those who did not learn their HIV status conditioning on their status. Time preference corresponds to the amount of USD an individual reported (hypothetically) needing to be compensated in one month, to give up USD 4.73 in the present.

having enough food for the family (Table A.8). Looking at each variable for alcohol consumption (Table A.9), the effect on alcohol intoxication is positive but generally not statistically significant with the exception of being drunk during weekdays, which has been shown to be prompted by tension reduction and coping beliefs, as opposed to weekend drinking that is facilitated by sociability and enhancement beliefs (Lac and Luk, 2019; Lau-Barraco *et al.*, 2016; Studer *et al.*, 2014).

5 Discussion and Conclusions

Expanding the diagnostic capacity for early detection of diseases and health risks is key to improving health around the world. However, when the screening and detection of severe diseases or health risks are not accompanied by access to effective treatment, individuals who are informed about diseases during screenings may feel hopeless and become fatalistic. In these cases, the well-intended health screening expansion may be counterproductive.

In this paper, we measure the causal effect of HIV testing on survival by leveraging a randomized experiment that incentivized knowledge of HIV status and 15 years of vital status data in a context where treatment was not initially available. We find that an HIV+ diagnosis leads to lower survival compared to HIV+ individuals who do not learn their status. These effects amount to a reduction in survival of about 23 percentage points among those who learned they were HIV+ after 6 and 15 years. Using data from a follow-up survey conducted a few months after HIV tests were available, we find evidence that receiving an HIV+ diagnosis resulted in increased anxiety, economic uncertainty,

risky health behaviors and a change in time preferences, all of which could play a role in increasing mortality. While learning to be HIV– should be "good news" and potentially lead to positive outcomes, we do not find any effect of receiving HIV– results on survival or any other dimension explored in the follow-up survey. This null effect possibly reflects the fact that individuals can get infected shortly after being tested reducing the value of the information.

Overall, the results in this paper suggest that health information may have important unintended consequences and recipients of this information need guidance and support throughout the management of the disease. This is not to say that information should not be provided from a social welfare perspective. There are potential benefits to knowing that life expectancy has shortened to make the right adjustments in terms of savings and investments and putting affairs in order for the family. In the case of infectious diseases, there is also the additional benefit of preventing other people's illness and death. HIV is a prominent example where these benefits are important.[24] Finally, providing information may just be the ethical thing to do.

The findings of this paper are also important as the disease burden in developing countries is increasingly shifting to non-communicable diseases (NCDs) (GBD 2019 Disease Collaboration, 2020; Kämpfen *et al.*, 2018). Diagnostic and screening capacities for NCDs are lagging in many developing countries (McNerney, 2015). The situation is improving for some NCDs for which effective treatments have been developed (e.g., hypertension) (Yadav *et al.*, 2021), while it remains severely lagging for NCDs that require complex treatments (e.g., many cancers). At the moment, there often is a mismatch between the possibility to detect and treat diseases, and this is particularly the case in sub-Saharan African (SSA) countries that have not been able to develop robust and broadly accessible NCD-focused health treatment delivery (Pai and Temesgen, 2017; Jacobson *et al.*, 2013). For example, the global health community is currently promoting increased screening for hypertension (Schutte *et al.*, 2021) in many low- and middle-income countries, despite concerns in the former—but possibly also in the latter—countries that screened individuals may not be able to effectively respond to new information about underlying health risks by accessing treatments (Ibrahim and Damasceno, 2012). This implies that many people will receive "bad news"—that is, information about an underlying health risk without being able to access treatment—and, as a result, many may struggle to cope. This mismatch between screening and treatment capacities could also lead to information avoidance—i.e., opting out of screening and diagnostic tests—if information enters directly in their utility function (Golman *et al.*, 2017). Our paper highlights the importance of matching the treatment capacities of the healthcare system to the progress in health screening as part of global health programs. Our analyses show that the failure to do so may result in unintended effects of information; in our case, persistent elevated mortality of HIV+ individuals who are informed about their HIV status.

[24] Using the same exogenous variation in HIV status knowledge, Thornton (2008) shows that those who learn to be HIV+ are more likely to purchase condoms, which has important consequences for the spread of the disease.

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A Appendix

A.1 Selective attrition

We provide in this section more details about the simulation exercise we put in place to assess the importance of selective attrition in our study. Our simulations allow to bound parameters of interest by randomly assigning living status to missing data. More specifically, we compute lower and upper bounds of our causal estimates by allowing mortality rates to be different across three groups: (i) those who are HIV-, (ii) those who are HIV+ and learned about their status, and (iii) those who are HIV+ and did not learn about their status. We set the mortality rate of those who are HIV- at the rate we observe in our data (2.3% between 2004 and 2008, 4.2% between 2004 and 2010, and 11.2% between 2004 and 2018) and then compute our estimates for *all the various possible combinations of mortality rates* among groups (ii) and (iii). The advantage of this approach is that, while we assume a common mortality rate among those who are HIV-, we do not impose ex ante any specific mortality rate for those who are HIV+ and exhaust all possible mortality rate alternatives for these two groups. Practically, we simulate our data by randomly assigning living status to observations with missing outcome a 100 times *for each possible combination of mortality rates* and report the mean of the corresponding distributions of parameters of interest.

Figure A.2, Figure A.3, and Figure A.4 present the results of our simulation exercise in which we randomly assign living status to observations with missing outcome a 100 times *for each possible combination of mortality rates (among those who are HIV+)*. As explained above, we set the mortality rates to 2.3% for 2008, 4.2% for 2010, and 11.2% for 2018 for those who are HIV- and then compute our estimates for *all the various possible combinations of mortality rates* among those who are HIV+ and did not learn about their status and those who are HIV+ and learned about their status. For the 2008 simulation, among those with missing living status information, there were 8 individuals who were HIV+ and did not learn about their HIV status, and 5 who were HIV+ and learned about their seropositivity. For the 2010 (2018) simulation, among those with missing living status information, there were 7 (12) individuals who were HIV+ and did not learn about their HIV status, and 5 (9) who were HIV+ and learned about their seropositivity.

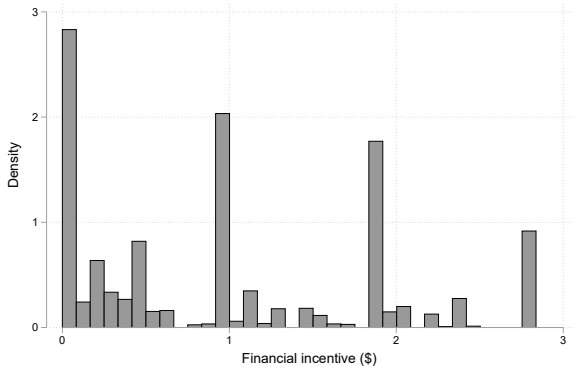
Figure A.2, Figure A.3, and Figure A.4 show the means of 100 simulations for each possible combination of mortality rates among those who were HIV+ whose living status information was missing. Each marker represents the mean of 100 simulated treatment effects. The x-axis shows the survival rate among those who were HIV+ and did not learn about their HIV status while the markers in the legend show the mortality rate among those who were HIV+ and did learn about their HIV status. The horizontal light (dark) grey line represents the effect of learning one's HIV- (HIV+) derived in our benchmark specification (Table 2). One can see in Figure A.2 that irrespective of the possible combinations of the survival rates among those who were HIV+ at baseline, we get large negative effects on survival among those who learned that they were HIV+ in 2008. While the effects for those who learned that they were HIV- are constant and very close to the effect estimated in our benchmark specification (the various light grey markers almost perfectly overlap), the causal effects of learning one's HIV+ monotonically increases (gets closer to 0) as we increase the survival rate among those who learned that they were HIV+ while keeping constant the survival rate among those who did not learn their seropositivity, as represented by the positive slopes of the lines connecting each type of markers.

From our simulation exercise, we clearly observe a negative effect of learning one's HIV+ on the probability of survival in 2008, irrespective of the mortality rate we assume for those who are HIV+. It is worth noting that the confidence intervals derived from the means of our simulated effects for those who learned that they were HIV+ do not overlap with 0, which shows that our effects are precisely estimated. Results from the 2010 and 2018 simulations (Figure A.3 and Figure A.4, respectively) show very similar patterns than those presented in Figure A.2 and further reinforces the robustness of our benchmark results.

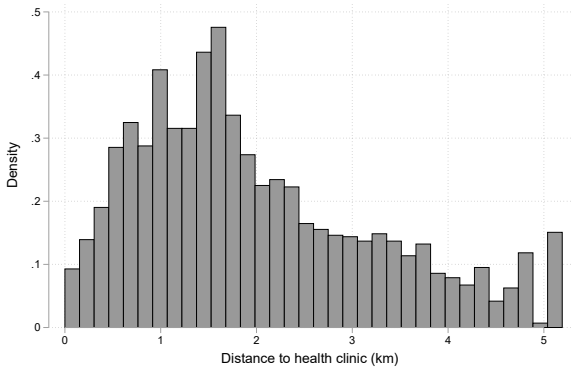
A.2 Figures

Figure A.1: Histograms of the instrumental variables used in this study

(a) Financial incentives

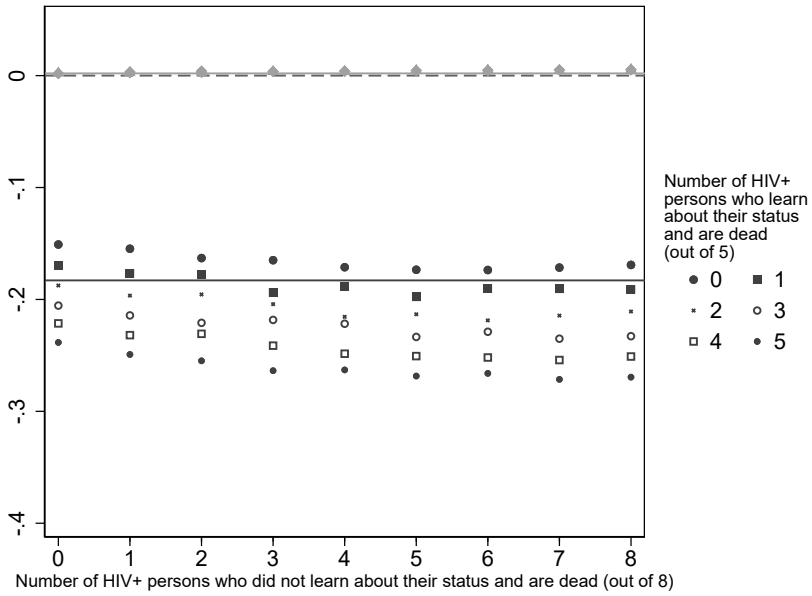


(b) Distance to health clinic



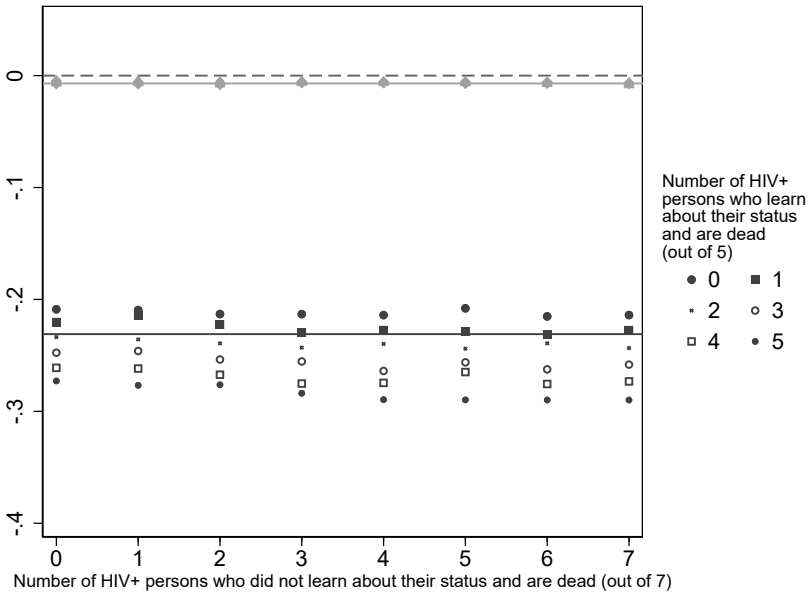
Note: The plot on the left (a) shows the histogram of the financial incentive in $ that respondents received in case they decided to receive their HIV test results. This plot of the right (b) shows the histogram of the distance between the place where the respondents live and the health clinic where respondents could pick up their HIV test results. The location of the health clinics was randomly selected in the respondents village where they live. We use both financial incentives and distance to health clinic as instrument variable to predict whether respondents learn about their HIV test results.

Figure A.2: **Causal effects estimated with random allocation of living status based on 100 simulations for each possible combination of mortality rates – 2008**



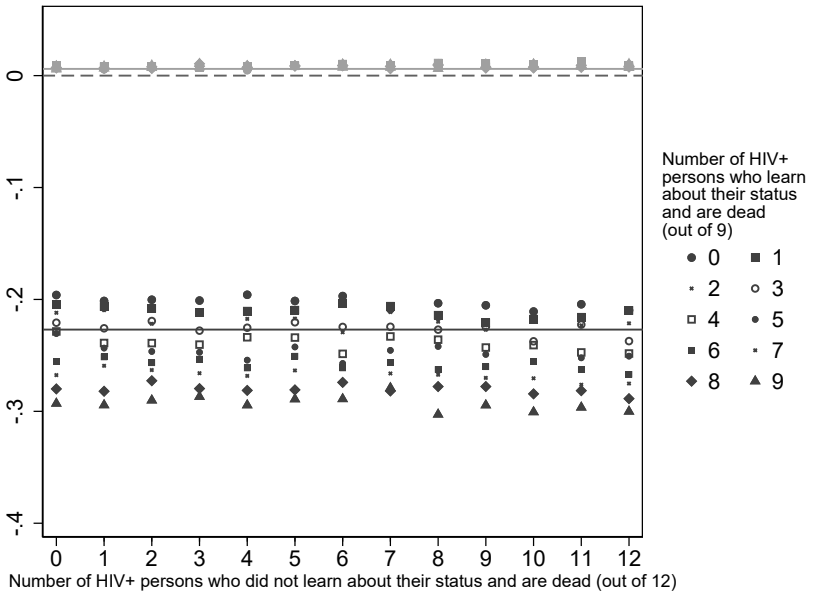
Note: The symbols represent the mean causal effects of learning HIV- (light color) and learning HIV+ (dark color) on the probability of being alive in 2008 based on our 100 simulations. There were 129 observations with missing living status in 2008, among which 116 were HIV- and 13 were HIV+. Among the 13 individuals who were HIV+, 5 learned about their status and 8 did not. We set the mortality rate of those who are HIV- at the rate we observe in our data (2.3% between 2004 and 2008). Symbols (in the legend) represent different mortality rate among individuals who were HIV+ and learned about their HIV status. The x-axis shows the mortality rate among those who were HIV+ and did not learn about their HIV status. The horizontal lines represent our benchmark estimates reported in Table 2.

Figure A.3: **Causal effects estimated with random allocation of living status based on 100 simulations for each possible combination of mortality rates – 2010**



Note: The symbols represent the mean causal effects of learning HIV- (light color) and learning HIV+ (dark color) on the probability of being alive in 2010 based on our 100 simulations. There were 174 observations with missing living status in 2010, among which 162 were HIV- and 12 were HIV+. Among the 12 individuals who were HIV+, 7 learned about their status and 5 did not. We set the mortality rate of those who are HIV- at the rate we observe in our data (4.2% between 2004 and 2010). Symbols (in the legend) represent different mortality rate among individuals who were HIV+ and learned about their HIV status. The x-axis shows the mortality rate among those who were HIV+ and did not learn about their HIV status. The horizontal lines represent our benchmark estimates reported in Table 2.

Figure A.4: **Causal effects estimated with random allocation of living status based on 100 simulations for each possible combination of mortality rates – 2018-2019**



Note: The symbols represent the mean causal effects of learning HIV- (light color) and learning HIV+ (dark color) on the probability of being alive in 2018 based on our 100 simulations. There were 491 observations with missing living status in 2018-2019, among which 470 were HIV- and 21 were HIV+. Among the 21 individuals who were HIV+, 9 learned about their status and 12 did not. We set the mortality rate of those who are HIV- at the rate we observe in our data (11.2% between 2004 and 2018). Symbols (in the legend) represent different mortality rate among individuals who were HIV+ and learned about their HIV status. The x-axis shows the mortality rate among those who were HIV+ and did not learn about their HIV status. The horizontal lines represent our benchmark estimates reported in Table 2.

A.3 Tables

Table A.1: Summary statistics of the 2005 study sample

	All			***HIV−***			***HIV+***		
	Mean	Sd	N	Mean	Sd	N	Mean	Sd	N
Sum of worries	2.423	1.028	1474	2.401	1.017	1408	2.879	1.144	66
Worries about AIDS-related health problem	0.071	0.256	1471	0.063	0.242	1405	0.242	0.432	66
Worries about other health problems	0.372	0.484	1472	0.364	0.481	1406	0.545	0.502	66
Worries about paying school fees	0.209	0.407	1470	0.205	0.404	1404	0.288	0.456	66
Worries about finding or earning enough money	0.931	0.254	1472	0.930	0.255	1406	0.939	0.240	66
Worries about having enough food	0.828	0.378	1465	0.828	0.378	1399	0.833	0.376	66
Sum economic uncertainties	4.513	1.117	1474	4.513	1.118	1408	4.500	1.099	66
Not easy to get MKW500	0.864	0.343	1474	0.864	0.343	1408	0.879	0.329	66
Future is uncertain	0.747	0.435	1473	0.746	0.436	1407	0.773	0.422	66
Family not enough food	0.693	0.461	1464	0.693	0.461	1399	0.692	0.465	65
Not enough for basic essential	0.910	0.287	1472	0.910	0.287	1406	0.909	0.290	66
Only God determines future	0.967	0.180	1471	0.967	0.180	1405	0.970	0.173	66
Children take care of my health when old	0.341	0.474	1472	0.344	0.475	1406	0.288	0.456	66
Time preference - USD in a month (vs 4.73 USD today)	10.855	10.286	1427	10.923	10.367	1364	9.366	8.269	63
Drink past 12 months	0.128	0.334	1441	0.125	0.331	1375	0.182	0.389	66
# days drunk last week	0.165	0.746	1411	0.166	0.754	1347	0.141	0.560	64
At least 1 day drunk last week	0.063	0.243	1411	0.062	0.241	1347	0.094	0.294	64
At least 2 days drunk last week	0.043	0.202	1411	0.044	0.205	1347	0.016	0.125	64
Drunk during weekdays (except Friday)	0.052	0.222	1411	0.051	0.221	1347	0.062	0.244	64
Subjective life expectancy at age	26.383	16.006	1270	26.668	16.065	1218	19.712	12.985	52

Note: The sample is derived from all respondents who got tested for HIV in 2004 and were interviewed in 2005 as part of the intervention follow-up survey. Information was collected only among respondents living in Rumphi and Balaka. "Sd" stands for standard errors and "N" for the number of observations. "Worries" and "uncertainties" are all dichotomous variables. Sum of "worries" also includes "Other" worries that respondents could report. 'Drunk on weekdays" is a dichotomous variable that takes the value 1 if a respondent admitted to have been drunk on Monday, Tuesday, Wednesday, and/or Thursday. Variables in **bold** are part of the main mechanisms reported in Table 3.

Table A.2: Exogeneity of the instruments

	Male (1)	Age (2)	HIV+ (3)
Any incentive	-0.050	1.342	-0.003
	(0.039)	(1.032)	(0.018)
Incentive	0.062	0.379	-0.013
	(0.057)	(1.381)	(0.022)
Incentive squared	-0.017	-0.315	0.004
	(0.019)	(0.451)	(0.008)
Distance in km	-0.028	-0.234	-0.032**
	(0.020)	(0.971)	(0.016)
Distance squared	0.007*	0.118	0.005*
	(0.004)	(0.195)	(0.003)
Constant	0.482***	37.442***	0.113***
	(0.027)	(1.154)	(0.023)
Observations	2823	2823	2823
R^2	0.002	0.005	0.006

Note: The table shows estimates of the effect on the instruments on predetermined variables. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$: reported in parentheses are standard errors that are clustered at the village level. All specifications control for region fixed effects.

Table A.3: Exogeneity of the instruments – interactions with region

	Male (1)	Age (2)	HIV+ (3)
South	-0.031	-1.213	-0.050
	(0.066)	(2.452)	(0.050)
North	-0.050	-1.216	-0.043
	(0.070)	(2.456)	(0.054)
Any incentive	-0.006	-0.388	-0.013
	(0.057)	(1.718)	(0.028)
Incentive	0.050	1.453	-0.016
	(0.089)	(2.195)	(0.039)
Incentive squared	-0.022	-1.002	0.004
	(0.033)	(0.761)	(0.014)
Distance in km	-0.077	0.642	-0.052
	(0.050)	(1.263)	(0.039)
Distance squared	0.014	-0.278	0.011
	(0.013)	(0.261)	(0.010)
South × Any incentive	-0.106	3.787	0.040
	(0.084)	(2.306)	(0.043)
South × Incentive	-0.035	-1.149	-0.017
	(0.128)	(3.011)	(0.053)
South × Incentive squared	0.033	0.680	0.009
	(0.044)	(1.009)	(0.020)
South × Distance in km	0.081	-1.267	0.029
	(0.059)	(2.135)	(0.045)
South × Distance squared	-0.013	0.426	-0.007
	(0.014)	(0.404)	(0.010)
North × Any incentive	-0.055	1.044	-0.020
	(0.102)	(2.674)	(0.049)
North × Incentive	0.091	-0.776	0.053
	(0.148)	(3.778)	(0.063)
North × Incentive squared	-0.027	0.879	-0.017
	(0.048)	(1.247)	(0.020)
North × Distance in km	0.052	-1.268	0.021
	(0.062)	(2.401)	(0.050)
North × Distance squared	-0.005	0.664	-0.007
	(0.015)	(0.496)	(0.011)
Constant	0.521***	38.833***	0.135***
	(0.051)	(1.438)	(0.039)
Observations	2823	2823	2823
R^2	0.006	0.011	0.008

Note: The table shows estimates of the effect on the instruments on predetermined variables. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$: reported in parentheses are standard errors that are clustered at the village level. Reference category corresponds to observations from the Center region (Mchinji).

Table A.4: First stage regressions by survey year

	2006		***2008***		***20010***		***20018-19***	
	Learn HIV– (1)	Learn HIV+ (2)	Learn HIV– (3)	Learn HIV+ (4)	Learn HIV– (5)	Learn HIV+ (6)	Learn HIV– (7)	Learn HIV+ (8)
Any incentive	0.275***	-0.001	0.274***	-0.002	0.280***	-0.001	0.270***	-0.002
	(0.031)	(0.002)	(0.032)	(0.002)	(0.032)	(0.002)	(0.034)	(0.002)
Incentive	0.252***	0.000	0.258***	0.001	0.258***	0.001	0.266***	0.001
	(0.038)	(0.001)	(0.041)	(0.001)	(0.040)	(0.001)	(0.042)	(0.001)
Incentive squared	-0.055***	0.000	-0.057***	-0.000	-0.057***	-0.000	-0.059***	-0.000
	(0.012)	(0.000)	(0.012)	(0.000)	(0.012)	(0.000)	(0.013)	(0.000)
Distance in km	-0.046	0.000	-0.047	0.001	-0.043	0.000	-0.047	0.001
	(0.029)	(0.001)	(0.029)	(0.001)	(0.030)	(0.001)	(0.034)	(0.001)
Distance squared	0.006	0.000	0.006	-0.000	0.005	0.000	0.006	-0.000
	(0.005)	(0.000)	(0.005)	(0.000)	(0.006)	(0.000)	(0.006)	(0.000)
HIV+	-0.414***	0.538***	-0.415***	0.551***	-0.408***	0.575***	-0.422***	0.604***
	(0.041)	(0.109)	(0.040)	(0.107)	(0.040)	(0.106)	(0.045)	(0.106)
Any incentive × HIV+	-0.301***	0.216*	-0.298***	0.213*	-0.308***	0.223*	-0.297***	0.219*
	(0.031)	(0.118)	(0.033)	(0.119)	(0.032)	(0.114)	(0.036)	(0.126)
Incentive × HIV+	-0.226***	0.468***	-0.229***	0.517***	-0.225***	0.508***	-0.236***	0.510***
	(0.040)	(0.141)	(0.043)	(0.149)	(0.042)	(0.144)	(0.044)	(0.153)
Incentive squared × HIV+	0.049***	-0.149***	0.050***	-0.164***	0.049***	-0.167***	0.051***	-0.160***
	(0.012)	(0.046)	(0.013)	(0.047)	(0.013)	(0.046)	(0.014)	(0.049)
Distance in km × HIV+	0.049	-0.235**	0.053*	-0.251***	0.050	-0.277***	0.053	-0.318***
	(0.031)	(0.104)	(0.031)	(0.096)	(0.031)	(0.091)	(0.035)	(0.085)
Distance squared × HIV+	-0.006	0.041**	-0.007	0.045**	-0.006	0.049***	-0.006	0.058***
	(0.006)	(0.019)	(0.005)	(0.018)	(0.006)	(0.017)	(0.007)	(0.016)
Constant	0.326***	-0.017	0.328***	-0.014	0.314***	-0.011	0.292***	-0.012
	(0.068)	(0.018)	(0.070)	(0.018)	(0.070)	(0.019)	(0.079)	(0.020)
Observations	2756	2756	2694	2694	2649	2649	2332	2332
Effective F-statistics	129.97	10.55	129.88	12.35	132.27	12.86	108.67	14.12

Note: The table shows estimates of the first stage. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$: reported in parentheses are standard errors that are clustered at the village level. All specifications control for sex, age and its square, and region fixed effects.

Table A.5: The effects of learning HIV status on the availability of vital status information

Living status information available in:				
	2006	2008	2010	2018-19
	(1)	(2)	(3)	(4)
Learn HIV–	0.003	-0.013	-0.023	0.011
	(0.010)	(0.016)	(0.016)	(0.031)
Learn HIV+	0.010	-0.074	-0.074	0.040
	(0.038)	(0.079)	(0.076)	(0.094)
Observations	2823	2823	2823	2823

Note: The table shows estimates of the effect on the probability of having vital status information for various years. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$: reported in parentheses are standard errors that are clustered at the village level. All specifications control for HIV status, sex, age and its square, and region fixed effects. The instruments to predict whether respondents learned their HIV status are: whether respondents received any financial incentive, the amount of the incentive, the square of the amount of the incentive, the distance to the health clinic where respondents can obtain their results, the square distance, as well as all of their interaction with HIV status.

Table A.6: Attrition corrected treatment effects: Inverse probability weights

Respondent is alive in:				
	2006	2008	2010	2018-19
	(1)	(2)	(3)	(4)
Learn HIV–	0.002	0.002	-0.008	0.008
	(0.006)	(0.014)	(0.014)	(0.024)
Learn HIV+	-0.112	-0.182*	-0.226**	-0.238*
	(0.089)	(0.110)	(0.112)	(0.128)
Observations	2756	2694	2649	2332

Note: The table shows estimates of the effect on the probability of being alive for various years when applying inverse probability weights. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$: reported in parentheses are standard errors that are clustered at the village level. All specifications control for HIV status, sex, age and its square, and region fixed effects. The instruments to predict whether respondents learned their HIV status are: whether respondents received any financial incentive, the amount of the incentive, the square of the amount of the incentive, the distance to the health clinic where respondents can obtain their results, the square distance, as well as all of their interaction with HIV status.

Table A.7: The effects of learning HIV status on economic worries

Worried about:	AIDS-related health problems (1)	Other health problems (2)	Paying school fees (3)	Having enough food (4)	Having enough money (5)	**Sum of worries** (6)
Learn HIV–	-0.083***	-0.121*	0.067	-0.066	-0.016	-0.289**
	(0.030)	(0.064)	(0.046)	(0.046)	(0.025)	(0.133)
Learn HIV+	0.297***	0.396**	-0.129	0.266*	-0.013	0.840**
	(0.083)	(0.172)	(0.142)	(0.157)	(0.026)	(0.363)
Observations	1471	1472	1470	1465	1472	1474

Note: The sum of worries includes "Other" worries that respondents could report. The table shows estimates of the effect on various worries that respondents reported in 2005. We control for the number of components that are included in the sum. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$: reported in parentheses are standard errors that are clustered at the village level. All outcomes variables were measured in 2005 as part of a follow-up survey a few months after the intervention. Details about the outcome variables used including sample descriptive statistics are detailed in Appendix Table A.3. All specifications control for HIV status, sex, age and its square, and region fixed effects. The instruments to predict whether respondents learned their HIV status are: whether respondents received any financial incentive, the amount of the incentive, the square of the amount of the incentive, the distance to the health clinic where respondents can obtain their results, the square distance, as well as all of their interaction with HIV status.

Table A.8: The effects of learning HIV status on economic uncertainties

Economic uncertainties:	Not easy to get MKW500 (1)	Future uncertain (2)	Not enough food (3)	Not enough for essentials (4)	God determine future (5)	Children take care (6)	**Sum of economic uncertainties** (7)
Learn HIV–	0.001	-0.085**	-0.166***	-0.034	0.026	0.017	-0.185
	(0.031)	(0.042)	(0.048)	(0.035)	(0.018)	(0.063)	(0.243)
Learn HIV+	0.141	0.081	0.387**	0.009	0.072**	0.134	0.934**
	(0.110)	(0.154)	(0.164)	(0.075)	(0.031)	(0.215)	(0.390)
Observations	1474	1473	1464	1472	1471	1472	1474

Note: "Children" includes both "sons"' and "daughters"' help. The table shows estimates of the effect on various economic uncertainties that respondents reported in 2005. We control for the number of components that are included in the sum. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$: reported in parentheses are standard errors that are clustered at the village level. All outcomes variables were measured in 2005 as part of a follow-up survey a few months after the intervention. Details about the outcome variables used including sample descriptive statistics are detailed in Appendix Table A.3. All specifications control for HIV status, sex, age and its square, and region fixed effects. The instruments to predict whether respondents learned their HIV status are: whether respondents received any financial incentive, the amount of the incentive, the square of the amount of the incentive, the distance to the health clinic where respondents can obtain their results, the square distance, as well as all of their interaction with HIV status.

Table A.9: Effects of learning HIV status on drinking behaviors

	Drink alcohol past 12 months	# of days drunk last week	At least 1 day drunk last week	At least 2 days drunk last week	Drunk on weekdays
Learn HIV–	0.057	0.074	0.035	0.026	-0.007
	(0.038)	(0.074)	(0.028)	(0.018)	(0.024)
Learn HIV+	0.211**	0.188	0.045	0.046	0.099**
	(0.102)	(0.126)	(0.070)	(0.029)	(0.041)
Observations	1441	1411	1411	1411	1411

Note: "Drunk on weekdays" is a dichotomous variable that takes the value 1 if a respondent admitted to have been drunk on Monday, Tuesday, Wednesday, and/or Thursday. The table shows estimates of the effect on drinking behaviors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$: reported in parentheses are standard errors that are clustered at the village level. All outcomes variables were measured in 2005 as part of a follow-up survey a few months after the intervention. Details about the outcome variables used including sample descriptive statistics are detailed in Appendix Table A.3. All specifications control for HIV status, sex, age and its square, and region fixed effects. The instruments to predict whether respondents learned their HIV status are: whether respondents received any financial incentive, the amount of the incentive, the square of the amount of the incentive, the distance to the health clinic where respondents can obtain their results, the square distance, as well as all of their interaction with HIV status.