Aid for AIDS and testing behavior: evidence from Malawi (2000-2016)

Abstract

What is the impact of foreign aid-funded HIV prevention programs on the testing decision? Since the 1990s, Malawi has successfully fought HIV, yet it still has the 8th HIV prevalence rate worldwide. There is limited evidence the impact of exposure to a HIV prevention program on screening decision. This study matches foreign aid-funded HIV prevention programs implemented between 1997 and 2017 (AidData and Ministry of Finance of Malawi) to 92,310 respondents from four Demographic and Health Survey (DHS) waves. It finds that exposure to HIV prevention programs has not increased the likelihood of getting tested. Instead, it has decreased the likelihood of being tested by 3 percent between 2004 and 2010. This impact varies according to the intensity of exposure. The analysis of mechanisms suggests that foreign aid-funded HIV prevention programs would have increased stigma without any impact on the level of knowledge about HIV, which was already high. The negative impact is stronger for men than women, encouraging further investigation into the responsibility of HIV testing among couples.

Keywords: testing behavior, HIV, foreign aid, stigma, Malawi.

JEL Codes: I12, I15, F35.

1 Introduction

Managing an epidemic lies on the individual's decision to get tested. Over the past 30 years, foreign aid-funded HIV prevention programs have received large financial resources to spread information and promote testing, particularly in Malawi. Malawi invested 17% of its total budget for HIV and AIDS programs in HIV prevention (i.e. USD 39,1 million in 2017). Despite a 39% drop in HIV prevalence rate from 2000 to 2017, Malawi is still among the countries most affected by HIV (Roser and Ritchie, 2020). According to Nunnenkamp and Öhler (2011), foreign aid failed at preventing new HIV infections. Yet, microeconomics studies assess prevention interventions (de Walque, 2007) succeeded in discouraging risky sexual behaviors (Dupas et al., 2018).

This paper investigates the impact of foreign aid-funded HIV prevention programs on screening behavior. HIV prevention programs refer to programs that share information, distribute contraceptive, and encourage screening. They reduce the direct and indrect costs of information and access to drugs. They may target the general public or specific population. While prevention programs should enhance screening, other parameters could hamper their positive impact. For instance in Malawi, the selection bias in the destination of health aid (Marty et al., 2017) may limit the marginal impact on the screening decision. Plus, HIV programs may signal a high HIV prevention rate and have negative spillovers on stigma, in areas where they settle (Yang et al., 2022).

A difference-in-differences estimates the impact of the foreign aid-funded prevention programs on testing. Like Knutsen et al. (2017), and Isaksson and Kotsadam (2018), the identification strategy takes advantage of HIV prevention programs' spatial and temporal variations to match geolocations from foreign aid-funded HIV prevention projects' and from the Malawi Demographic and Health Survey (DHS) 's respondents. The AidData database details roughly 90% of foreign aid-funded programs in Malawi between 1997 and 2012 (Peratsakis et al., 2012). I filled this dataset until 2017 with a dataset shared by the Ministry of Finance of Malawi. The DHS is a repeated cross-section dataset of four waves from 2000 to 2016.
Individuals exposed to HIV prevention programs at the date of their interview are compared to individuals living on a site that will receive an HIV prevention program after their interview. This strategy controls for unobservable time-invariant characteristics that may bias the estimates. The main assumption is that individuals exposed at the date of the interview are comparable to those who will be exposed later on. Individuals exposed to programs early may be different than those exposed later. This threat is controlled at several stages. The initial model includes year-fixed effects to allow time variation within and across the four waves of DHS. In the sensitivity analysis, regres-

sions are performed on sub-samples and reduced time lags between exposure and the interview.

Results show that individuals *Exposed* to a prevention program do not get more tested than those who are *To be Exposed* later. Instead, exposure to HIV prevention programs significantly decreased the likelihood of getting screened by 3 percent on average from 2004 to 2010. Stigma and gender seem to be the main mechanisms. Exposure to HIV prevention programs did not improve knowledge on HIV. However, the mediation analysis reveals that stigma drives 19% of the final impact. Instead, program implementation may have negative externalities by increasing fear of testing for those living in exposed areas. Eventually, the heterogeneity analysis finds that men are significantly less likely to be tested than women.

This paper contributes to understanding prevention programs' role on the fight against HIV. Previous studies explain the trends of HIV prevalence rate at the continental or national level (Oster, 2005, 2012; Greenwood et al., 2019). Other studies focus on the effect of preventive interventions on sexual behavior or HIV prevalence at the local level (Dupas, 2011; Sterck, 2014; Dupas et al., 2018; Kerwin, 2018). Wilson (2016) suggests that exposure to centres giving access to antiretroviral therapy increases the likelihood of being screened. Friedman (2018) reports that although ART sprawl in Kenya has reduced new HIV infections, it may have had negative spillovers on risky sexual behavior. This paper complements findings in Malawi (Thornton, 2008; Godlonton et al., 2015; Delavande et al., 2014; Kerwin, 2018; Derksen et al., 2022), and gives evidence on the prevention programs' impact on testing.

Second, this paper aligns with the literature on the behavioral determinants of HIV testing. Delavande et al. (2014) show that community intolerance towards HIV-positive people raises the social cost of seropositivity and diminishes risky sexual behaviors. The risk of being recognized and stigmatized by their kinship also impacts HIV screening (Bond et al., 2002). In a randomized control trial (RCT) in rural Malawi, Derksen et al. (2022) finds that an adverse selection dynamic biases HIV testing rates. People who believe to be HIV-negative are more likely to get tested, while doubtful individuals avoid close HIV-testing centers for fear of meeting acquaintances and being stigmatized. They choose distant health centers to get tested, increasing transport and time costs. Additionally, the study's findings align with Yang et al. (2022). The authors conducted a RCT in Mozambique to assess the impact of a PEPFAR HIV prevention program on testing, knowledge and stigma. They find that the PEPFAR-funded program significantly and negatively affects HIV testing rates. Surprisingly, prevention interventions led to misinformation and worsens HIV-related stigmatizing attitudes. In its working paper, Kerwin (2018) higlights that some prevention programs may overestimate the likelihood of contracting HIV in Malawi. This paper contributes to understanding the prevention programs' adverse effect on stigma. Programs can have an information effect and a signal effect. The information effect increases the level of knowledge about HIV and should increase testing. Conversely, the signal effect suggests that the HIV rate is high in the area where the program is implemented. Since knowledge levels are very high in Malawi, it is more likely that the presence of the program creates apprehension about the risk of contracting HIV and discourages testing.

Eventually, this study explores the impact of foreign aid on economic development (Ndikumana and Pickbourn, 2017; Marty et al., 2017; Knutsen and Kotsadam, 2020; Khomba and Trew, 2019) and health outcomes (Ssozi and Amlani, 2015; Odokonyero et al., 2018; Kotsadam et al., 2018). This paper contributes to the discussion on the impact of foreign aid and its potential adverse effects (Easterly, 2006; Deaton, 2013; Dreher et al., 2017). Marty et al. (2017) find that foreign aid received by Malawi reduced malaria prevalence. Rajlakshmi and Becker (2015) find that health and water aid decreased disease severity and diarrhea incidence, respectively.

This paper proceeds as follows. Section 2 restates the context of foreign aid and HIV testing in Malawi. Sections 3 and 4 describe the data and the empirical strategy, respectively. Section 5 discusses the main results with sensitivity analysis. Section 7 explores the channels through which HIV prevention programs affect testing behavior.

2 Context

Malawi has achieved significant progress in the fight against HIV over the past 20 years, although it has the ninth HIV prevalence rate in 2019. Preventive interventions substantially helped in reversing the spread of HIV. Since 2000, the HIV prevention strategy has accounted for a substantial share of the budget of Malawi's Ministry of Finance. Between 2005 and 2016, the budget allocated to HIV prevention and treatment increased more than threefold, from \$66 Million to \$230 Million (Roser and Ritchie, 2020).

Foreign aid has supported the worldwide expansion of HIV prevention and testing policies, specifically in Malawi, where the HIV budget has been donor-dependent for many years. The Global Fund and PEPFAR, the two largest HIV funders, were established in 2002 and 2003, respectively, and their funding activities have strengthened

HIV prevention and testing policy. In 2017/18, international aid, the Global Fund, and the Government of the United States of America represented 92% of spending on HIV in Malawi (UNAIDS, 2019).
An increasing number of health centers and clinics have opened screening services. Other prevention strategies reached the most isolated people through radio, television, community health workers, or mobile clinics for HIV testing.

The HIV prevalence rate declined from 15% in 2000 to 8% in 2019 (Roser and Ritchie, 2020). The HIV incidence rate dropped by 71% between 2000 and 2016, from 110,400 newly infected people to 31,772 newly infected people (see figureıA1.2). In 2000, 114,591 people were sick with HIV, and 78,502 people died of HIV. In 2016, they were 103,371 sick with HIV and 24,495 dying from it (see figure A1.1). However, about 18% of women and 35% of men were never tested in 2017, and the testing rate is lower among the poorest people (Office/Malawi and ICF, 2017).

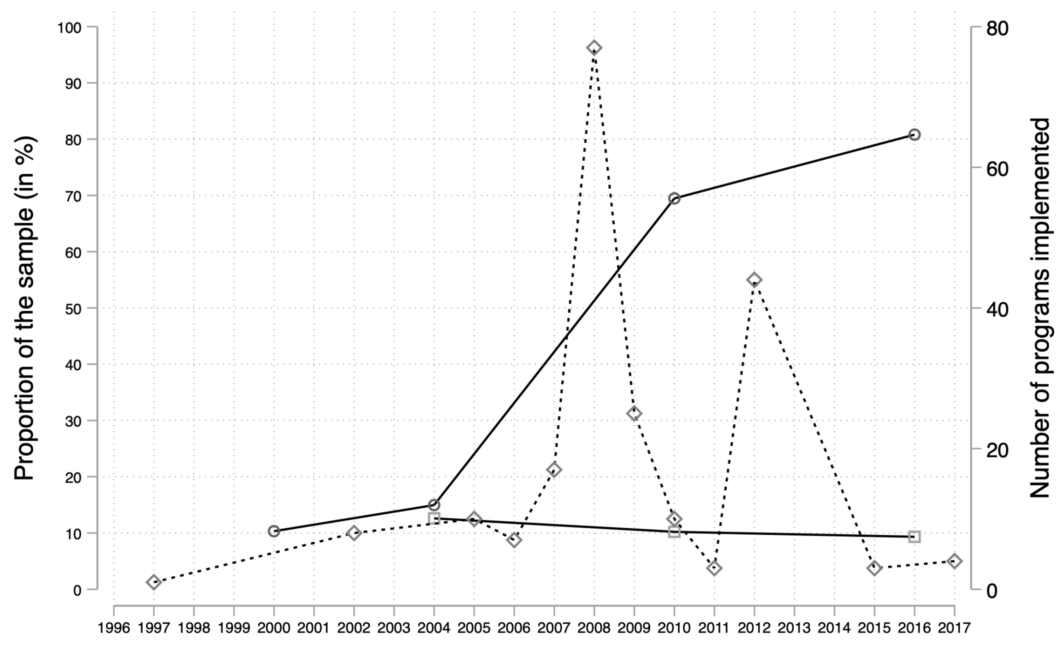
Testing is key in the fight against HIV. It increases the likelihood of being under ART treatment for seropositive people and reduces the likelihood of engaging in risky sexual behavior (Thornton, 2008; Delavande and Kohler, 2012; Greenwood et al., 2019). Being tested also encourages peers to do so (Godlonton and Thornton, 2012). The government of Malawi has encouraged preventive actions to overcome some barriers to screening, such as the distance to the screening center or the waiting time for results (Ministry of Finance of Malawi, 2000, National Aids Commission of Malawi, 2011, 2015). Thus, testing is free of charge in Malawi.[1] In 2003, Malawi achieved a breakthrough by introducing the mandatory screening of pregnant women during antenatal visits as part of programs for the prevention of mother-to-child transmission of HIV (PMTCT), which has increased HIV testing among women (WHO, 2014).[2] One of the most supported initiatives has been anonymous testing in health centers. This strategy intended to reduce the risk of stigma but quickly showed its limitations, particularly in the follow-up of patients who tested positive. Although the government eventually revoked anonymous testing, some health centers still propose it (Bernardo et al., 2017). Additionally, the roll-out of centres giving access to antiretroviral therapy may certainly contribute to the increase of HIV testing. In a longitudinal analysis of the roll-out of the national ART program in the Tutume district of Botswana, Warwick (2006) show that the number of HIV tests significantly increased fivefold once ART became available locally. Roura et al. (2009)'s qualitative analysis highlights that access to ART can increase testing because HIV is less perceived as a fatal disease, alleviating

[1] There is an indirect cost to testing. The average test cost is about twice the average daily wage, whether in rural areas (Sande et al., 2018) or urban areas (Maheswaran et al., 2016).

[2] In 2016, mother-to-child prevention prevented 13,662 infections.

the existing self-stigma. In the context of an RCT, Derksen et al. (2022) demonstrates that information about the effectiveness of ART is key to encouraging individuals to go for testing. However, being on ART is an external sign of recognition of the disease that can lead to new negative attitudes or stigma. The fear of being recognized and stigmatized is still one barrier to HIV testing (Derksen et al., 2022; Young and Zhu, 2012). Greenwood et al. (2019) analyze HIV policies in Malawi where foreign aid has funded prevention programs. Some policies have proven to be effective. Others have had a negative impact on HIV spread because of the heterogeneity in individuals' behavior. However, there is no quantitative evidence on the impact of the roll-out of ART at national level for Malawi, to our knowledge. All in all, HIV testing and direct access to ART are key to reduce HIV prevalence and incidence Granich et al. (2009). Between 2000 and 2016, the number of people tested increased from 10% to 81% (figure 1.1) and figure A1.3 pictures a positive and concave relationship between the number of HIV prevention programs and testing at the district level.

Figure 1.1: Testing behavior, HIV prevalence and HIV-related programs (1997 to 2017)



Note: Author's graph based on AidData database and Malawi Demographic and Health Surveys. It represents HIV-related programs' trends in Malawi, testing, and HIV prevalence. The left y-axis gives the proportion of individuals who tested for HIV and the proportion of seropositive people at each survey round (2000, 2004, 2010, 2016). The HIV prevalence has only been available since 2004 when DHS questionnaires included blood sample tests. The right y-axis gives the number of HIV-related programs funded by foreign aid by the date of implementation of the agreement.

3 Data

This paper aims to analyze the impact of exposure to HIV prevention programs on testing behavior. It matches the geolocation of 92,310 respondents from Malawi Demographic and Health Surveys (DHS) and the location of the HIV prevention programs funded by foreign aid from the Aid Management Platform (AMP) database.

The DHS survey is sampled in two stages, representative at the national level and urban/rural level. On average, 78% of the sample are women of reproductive age (15–49 years) who were either permanent household residents or visitors who slept there the night before the survey and were eligible for participation. In one-third of the households, all men aged 15–54 years were eligible for participation if they were either permanent household residents or visitors who slept there the night before the survey. Individuals are asked about socio-demographic characteristics as well as their sexual health and behavior. Their answers are used as the main outcomes: HIV testing and HIV status (see figure 1.1). Testing is monitored with respondents who were asked whether they already *"Got tested"*. Additionally, HIV tests were run over sub-samples of DHS interviews from 2004 to 2016 to assess respondents' HIV status. Respondents will be split into three groups, described later in this section, for the identification strategy: *Never Exposed*, *Exposed*, and *To be Exposed.* As shown in the table A1.2, the groups are balanced. The group *Exposed* and *To be Exposed* respectively counts 12,249 and 10,305 respondents, while those *Never Exposed* are 70,952.

Table 1.1 presents the summary statistics of the sample per group, and table A1.1 presents the balance checks between the three groups. The tables show that respondents are, on average, 28 years old, have four years of education, live mostly in rural areas (at 80%), and are married or in a couple (70%). People who are *Never Exposed* live on average 5 km from a health center, half as close as those who are *Exposed* or *To be Exposed.* Nearly 88% of the sample has had sex, with the average age of first intercourse being 14 years. In all three groups, nearly 12% of individuals reported extra-marital sex in the past year. The HIV prevalence rate is lower in the *Never Exposed* group, at 9%, compared to 13% and 14% in the *Exposed* and *To be Exposed* groups. The main difference is in the level of screening. Fifty-six percent of those who were *Never Exposed* reported having been screened, compared with 74% of those who were *Exposed* and 27% of those who were *To be Exposed.* Respondents will be geographically associated with HIV prevention programs thanks to their geolocation given by DHS.

Table 1.1: Summary statistics - By group of exposure

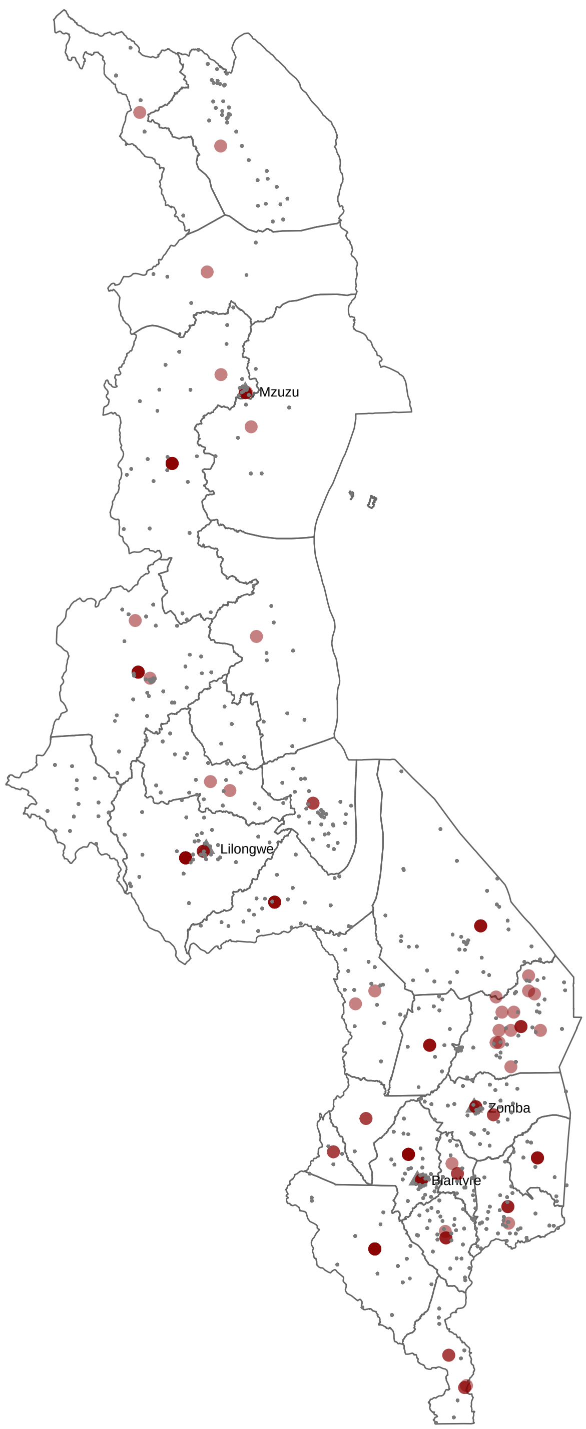
	(1) Groups of Exposure			
	Never Exposed	Exposed	To be Exposed	Total
Demographic				
Respondent's current age	28.24	28.80	27.59	28.24
	(9.62)	(9.83)	(9.33)	(9.62)
Gender	0.23	0.23	0.21	0.22
	(0.42)	(0.42)	(0.41)	(0.42)
Years of education	3.76	3.78	3.52	3.74
	(2.64)	(2.57)	(2.64)	(2.63)
Rural	0.85	0.74	0.70	0.82
	(0.35)	(0.44)	(0.46)	(0.38)
Health facilities, 10km radius	5.42	9.73	11.49	6.65
	(8.84)	(15.63)	(15.98)	(11.15)
Distance to the nearest HIV-prevention program	46.34	3.50	7.73	41.64
	(36.61)	(1.58)	(1.87)	(37.05)
Marital status:				
Never married	0.23	0.27	0.22	0.23
	(0.42)	(0.44)	(0.41)	(0.42)
Married	0.61	0.55	0.64	0.61
	(0.49)	(0.50)	(0.48)	(0.49)
Living together	0.05	0.06	0.02	0.05
	(0.22)	(0.24)	(0.15)	(0.22)
Widowed	0.03	0.03	0.03	0.03
	(0.16)	(0.18)	(0.17)	(0.16)
Divorced	0.04	0.05	0.05	0.04
	(0.20)	(0.22)	(0.22)	(0.20)
Not living together	0.04	0.04	0.03	0.04
	(0.19)	(0.19)	(0.18)	(0.19)
Sexual Behavior				
Already had sexual intercourse	0.88	0.87	0.89	0.88
	(0.33)	(0.34)	(0.31)	(0.33)
Age of first sexual intercourse	14.47	14.17	14.60	14.45
	(6.11)	(6.18)	(5.83)	(6.09)
Sex with someone else than partner last 12 months	0.11	0.13	0.12	0.11
	(0.31)	(0.34)	(0.33)	(0.32)
Sex with someone else than partner last 3 intercourse	0.02	0.01	0.02	0.02
	(0.12)	(0.11)	(0.15)	(0.12)
HIV outcomes				
Ever been tested for aids	0.56	0.71	0.24	0.54
	(0.50)	(0.45)	(0.43)	(0.50)
Date of last HIV test:				
Less than 12 months	0.42	0.43	0.46	0.42
	(0.49)	(0.50)	(0.50)	(0.49)
12 to 23 months	0.11	0.09	0.13	0.10
	(0.31)	(0.29)	(0.34)	(0.30)
More than 24 months	0.48	0.48	0.41	0.47
	(0.50)	(0.50)	(0.49)	(0.50)
HIV status - DHS test	0.09	0.13	0.14	0.10
	(0.29)	(0.33)	(0.35)	(0.30)
Observations	70952	12249	10305	93506

Note: Means of covariates at individual level, reported by group of exposure and for the full sample. The standard deviation is in parentheses.

Table A1.4 presents a summary of the foreign aid-funded programs monitored by the AidData's Malawi Geocoding Project (AMP) (Peratsakis et al., 2012). This dataset tracked and reported foreign aid activities by compiling information provided by donors to the Ministry of Finance (MoF) of Malawi, such as Chinese financial transfers to Africa (Khomba and Trew, 2019; Marty et al., 2017). The original dataset gathers nearly 90% of foreign-funded projects from 1997 to 2012 in various sectors such as agriculture, education, infrastructure, and health. I updated it for health programs until 2017, thanks to the AMP website and a dataset shared by the Malawi MoF. The final dataset details 561 projects in 2522 unique places: name, purpose, geolocation, funder, starting date, annual commitment, and actual disbursement. Eighty-seven projects for 304 unique locations (see figure 1.2) pursue an HIV-related goal: prevention, human resources management, scientific research, and construction of health facilities.[3]

[3] Eventually, one can reasonably consider that this dataset captures between 70% to 80% of HIV prevention actions implemented in Malawi.

Figure 1.2: Foreign aid-funded HIV prevention programs in Malawi (1997-2017)



Note: Author's graph based on the AidData and Malawi Demographic and Health Surveys databases. Grey dots indicate the location of the survey clusters of the various DHS waves, and red shaded dots indicate the location of HIV prevention programs. The redder it is, the more programs are settled in the location.

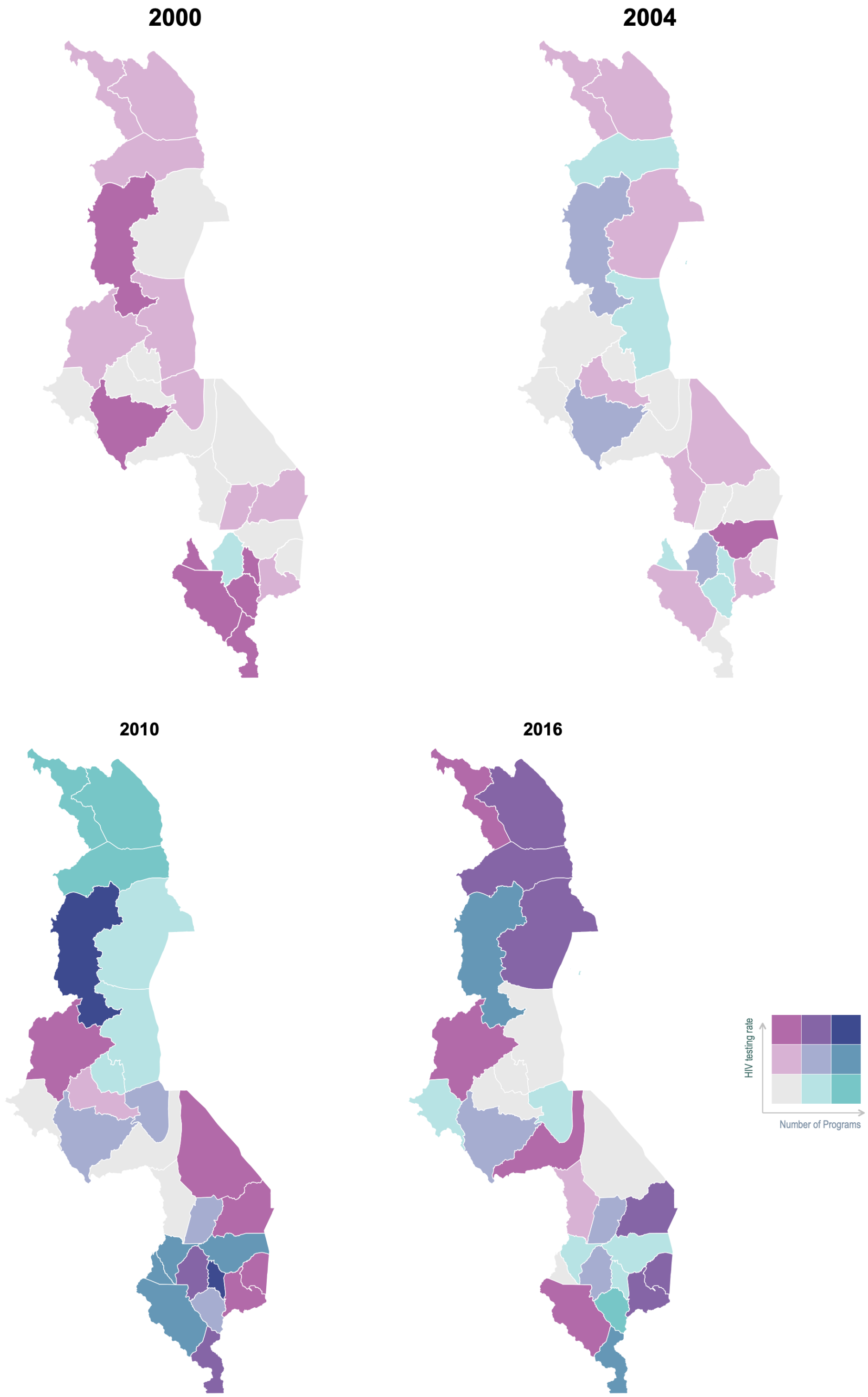
Some HIV-related programs do not aim for prevention and may be an investment in equipment and logistics for an HIV care center or training doctors. They have an indirect impact on information and HIV prevention. Thus, I audited each project with online information, and I defined a basic index to set three categories of HIV-related projects. Each project is rated using three values: 0 for HIV-related projects with no purpose of prevention, 1 for HIV-related projects that indirectly impact prevention, and 2 for HIV-related projects with prevention as an explicit primary focus (see table A1.4). In the end, 87 are HIV-related programs, among which 29 projects with 141 unique locations strictly target HIV prevention programs. Programs implemented at the national level or without any location details are excluded. In the raw database, projects are sorted by geolocation precision, scaled from 1 (exact location) to 8 (central government projects). The sample is then restricted to programs with the exact geolocation scaled 1 to 3.[4] The dataset identifies 17 HIV prevention projects with 118 unique locations (see table A1.16). All in all, 32 locations were at level 1 (precise location), 2 at level 2 (up to 25km displacement), and 84 at level 3 (district level). This variation could bias the estimates. Section 6 reports estimates after a robustness test that restricts the sample to individuals *Exposed* or *To be Exposed* to programs at level 1. Table A1.16 shows that programs have been gradually established in 25 of Malawi's 28 districts.

The heat maps in figure 1.3 represent the correlation between the testing rate and the number of programs per 1000 inhabitants at the district level in maps. They represent the correlation between the programs implemented between $t-4$ and $t0$ and the screening rate at t. For example, the map for 2004 illustrates the correlation between the number of programs per 1000 inhabitants implemented between 2001 and 2004 with the testing rate measured by DHS in 2004. The level of screening rates and the number of programs per 1000 inhabitants is always adjusted to the annual level. In other words, the intensity of the colors should only be observed at the level of each map and not between maps. In the case of a perfectly linear relationship, one would observe the colors on the diagonal running from the bottom left square to the top right square. Yet, the maps in Figure 1.3 do not indicate consistency in correlation across

[4]Strandow et al. (2011) describes the geocoding methodology. At level 1, "The coordinates correspond to an exact location, such as a populated place or a physical structure such as a school or health center. This code may also be used for locations that join other locations to create a line such as a road, power transmission line, or railroad". At level 2, "The location is mentioned in the source as being "near", in the "area" of, or up to 25 km away from an exact location. The coordinates refer to that adjacent location". At level 3, "The location is, or is analogous to, a second-order administrative division (ADM2), such as a district, municipality or commune". The location of level 3 of Malawi is the district's capital.

time. Only one region received programs in 2000, but its screening rate was not high relative to the national average. From 2004 onward, some regions have received more programs, and this is correlated with a high screening rate afterward. Figures A1.3 and A1.4 present the plots of linear and local polynomial correlations between HIV prevalence and the number of programs at the district level. It shows the correlation between HIV prevalence in t, and the number of programs per 1,000 inhabitants implemented years before. The relationship between HIV prevalence and the number of programs at the district level is slightly positive.

Figure 1.3: Map - Correlation between programs implemented and testing rate at district level (2000 - 2016)



Note: Author's map based on DHS and AidData databases. These maps represent the correlation between the HIV screening rate at $t0$ ($t0$ being the survey year) and the number of programs per 1000 inhabitants installed between $t0$ and $t+3$ (for 2000 and 2004) or $t+5$ (for 2010 and 2016) at the district level. The testing rate is based on the response to the question: "Have you ever been tested for HIV?" The number of programs is measured by the number of programs per 1000 inhabitants at the district level. In other words, the intensity of the colors should only be observed at the level of each map and not between maps. In the case of a perfectly linear relationship, one would observe the colors on the diagonal running from the bottom left square to the top right square.

The treatment variable is the geographical exposure to HIV prevention programs. The assumption is that individuals close to an HIV prevention program are likelier to receive information and incentives to get tested directly or indirectly. They receive it directly if the intervention directly targets them. They receive it indirectly if the intervention targets one of their kins (family, friends). The geographic matching is a proxy for exposure to HIV prevention programs. The programs' point coordinates are linked to individuals surveyed in the DHS, using the distance between the clusters' latitude and longitude of respondents to HIV prevention projects. DHS clusters are randomly moved by 2 to 5km in urban and rural areas. The median distances were 15 and 30km for urban and rural areas. In light of the literature, 15km and 30km are quite long distances that increase the risk of including non-exposed individuals in the *Exposed* group.

Bilinski et al. (2017) find that patients living in the Neno district (Malawi) have a higher probability of dropping their HIV treatment above an 8km distance to their health center. Palk et al. (2020) highlight that an increased travel distance is associated with a decreased HIV treatment initiation and retention. They also study the impact of transport costs on the retention of HIV treatment. They use a one-hour distance as the reference distance to a health facility for HIV treatment. Following them, I decided to set a one-hour walk in an urban area, which corresponds to 5km, a third of the average distance in the urban area. In rural areas, programs may use the car to cover different villages and a greater distance. I decided to set the buffer at 10km in rural areas, as it corresponds to a third of the average distance in the rural area. These cutoffs ensure a statistical power for the *Exposed* and *To be Exposed* groups and the precision of the estimates, though it is somewhat arbitrary[5]. Larger bandwidths are used for sensitivity analysis in section 6.

4 Empirical strategy

The impact of exposure to HIV prevention programs is estimated thanks to a difference-in-differences strategy used in Knutsen et al. (2017) and Isaksson and Kotsadam

[5]Friedman (2018) estimates the impact of Antiretroviral drug access on sexual health behavior. She justifies her choice to set the distance to the ARV center to 8km: "Eight kilometers is chosen to maximize power as it is the closest distance to the median. This generates balance between the treatment and control groups that maximizes the precision of the estimates. This distance (approximately 5 miles) is also a reasonable distance to walk for routine medical care. For robustness, the analysis is repeated using different distance cutoffs with nearly identical results. [...] The threshold of 8 km was chosen because it is near the median in order to maximize power, but - like any other distance cutoff - it is somewhat arbitrary."

(2018).[6] The spatial distribution of HIV prevention projects may be driven by local material or community support such as a health facility. Marty et al. (2017) show that pre-existing health infrastructures attract health projects in Malawi. One might expect that people living closer to health facilities are more demanding of and/or responsive to counseling, medical care, condoms, and/or tests for HIV because of low access costs to testing centres. The empirical strategy tries to overcome the endogeneity issue in the location of HIV prevention programs.

The empirical strategy relies on the spatial and temporal variation of the implementation of HIV prevention projects. It takes advantage of the repeated cross-section database and follows areas sampled before and after the implementation of the projects. It compares three groups of individuals: those exposed to HIV prevention programs in a 5/10km buffer before the interview ($Exposed$), those who To be Exposed to HIV prevention programs in a 5/10km buffer after the interview ($TobeExposed$), and those who are not, were not, and will not be exposed ($Never\ Exposed$, i.e. the control group, see table A1.2). The group $TobeExposed$ controls for unobservable time-invariant characteristics that may bias the selection. The hypothesis is that areas receiving HIV prevention programs at different periods - individuals $Exposed$ and $TobeExposed$ - are attractive for the same determinants. Consider the following baseline regression:

$$Y_{i,d,t} = \alpha + \Gamma Exposed_{i,t} + \lambda TobeExposed_{i,t} + \upsilon_d + \delta_t + \gamma \cdot X'_{i,t} + \varepsilon_{i,d,t} \tag{1.1}$$

Where Y is a discrete variable for the testing behavior or HIV status of an individual i in a district d of year t. It equals 1 if the person got tested or is seropositive and 0 otherwise. The vector of variables X' controls individuals' characteristics, namely age, gender, marital status, wealth, religion, and education level. The covariates matrix also includes the distance to the nearest health facility (see table A1.1). The dummies $Exposed$ and $TobeExposed$ denote the current or future exposure to an HIV prevention program, respectively. $Exposed$ variable equals 1 if a program was implemented in a 5km buffer around an individual i living in an urban area or a 10km buffer around an individual i living in a rural area, before the DHS survey. Otherwise, it takes the value 0.[7] The variable $TobeExposed$ is equal to 1 if an HIV prevention program will settle in the 5/10km buffer around the individual i after the DHS survey is realized, and

[6] The estimation strategy is not so common in Economics but quite close to the strategy that uses the planned location of road infrastructure vs the actual location to correct for endogenous placement of infrastructure. See Bird and Straub (2014); Donaldson (2018); Milsom (2021).

[7] Migrants are not exposed if they arrived after implementing an HIV prevention program.

0 otherwise. The variable *Exposed* takes precedence over *TobeExposed*. The latter cannot take the value 1 if the former does already. For instance, consider a three-year intervention implemented in an urban area from 2009 to 2012. An individual surveyed in 2004 in an urban area and living 3.6km away from the project's location has *Exposed* = 0 and *TobeExposed* = 1. A respondent surveyed in 2010 and living 4km away has variables *Exposed* = 1 and *TobeExposed* = 0. Groups *Exposed* and *TobeExposed* are respectively compared to the control group. The control group defines individuals living further than 5 or 10 km away who are or will never be exposed. Finally, the regression has district υ_d and year δ_t fixed effects to control for the general screening trend. The final outcome is the difference between the coefficients *Exposed* and *TobeExposed*, though it does not explicitly appear in the equation. It measures the effect of exposure to an HIV prevention program.

Recent findings on the difference-in-differences show that using the two-way fixed effects (TWFE) estimator with staggered treatment adoption may bias estimates if there are heterogeneous treatment effects (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). The estimator is biased if the TWFE relying on time-variation in treatment is a weighted average of two comparisons, including one using already treated units as a control group for not-yet-treated units (Goodman-Bacon, 2021). The setting tackles this threat by using *never treated* individuals for the control group for the two comparisons (*Exposed* and *TobeExposed*). An additional sensitivity test supports the empirical findings in section 6. The *Exposed* and *TobeExposed* groups will be restricted to individuals exposed to HIV prevention programs within a year of the survey, reducing the time-varying effect of the treatment.

Following Cameron and Miller (2015), the error term $\varepsilon_{i,d,t}$ is clustered at the level of the cluster survey (a village, a town, or a neighborhood, depending on whether it is an urban or rural area). It is assumed that the within-cluster correlation of the regressors is not equal to 0. The treatment status is a dummy variable based on a 5 to 10 km radius around the respondents, the latter being gathered in DHS geographical clusters. This survey criterion infers the overlapping of radius exposure.[8] The regression includes the DHS sampling weight.

[8]As 40% of clusters show treatment heterogeneity, there is a weak correlation between a cluster and being exposed to an HIV prevention program.

5 Results

5.1 Main results

Table 1.2 presents the results of the linear probability model - with and without fixed effects.

Table 1.2: Exposure to HIV prevention programs

	Testing behavior		HIV status	
	Got tested		Blood test result (DHS)	
	(1)	(2)	(3)	(4)
Exposed	0.14***	0.00	−0.01	−0.00
	(0.006)	(0.006)	(0.007)	(0.008)
To be exposed	−0.32***	0.03***	0.02*	0.01
	(0.006)	(0.008)	(0.010)	(0.011)
Control	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	Yes
Difference in differences	0.46***	−0.02***	−0.03***	−0.02
F-test: active-inactive=0	4475.45	6.82	6.57	1.78
p-value, F-test	0.00	0.01	0.01	0.18
Mean dep. var	0.54	0.54	0.10	0.10
R-squared	0.18	0.45	0.09	0.09
No. of observations	92310	92310	33167	33167

Note: *** p <.01, **p <.05, * p <.1 All estimates include controls for age, gender, marital status, wealth, rural/urban, distance to the nearest health center, and district fixed effect. Columns (1) and (3) do not include time-fixed effects, and columns (2) and (4) include year-fixed effects. All estimates are weighted using DHS sampling weights. The standard errors in parentheses are clustered at the level of the survey's clusters. The main outcome is in the bottom part of the table, named "Difference-in-Differences". It indicates the difference between the coefficients "Exposed" and "To be Exposed". The F-test and the p-value of the F-test are presented in the bottom section.

The upper part of the table shows the impact of exposure to a prevention program. Columns (1) & (3) do not include year-fixed effects, and they align with the hypothesis that exposure to HIV prevention programs increases testing. Individuals living in areas exposed to an HIV prevention program appear to be more likely to be screened and less likely to be HIV-positive.

Columns (2) and (4) include year-fixed effects. The effect is negative but non-significant on the probability of being HIV-positive. Exposure to HIV prevention programs is positive for both those exposed and those who To be Exposed, but it is not significant for the former. However, individuals *exposed* to HIV prevention programs are not more likely to get tested, although the coefficient is positive. In contrast, individuals who *will be exposed* are 2.8 percentage points more likely to be tested than those who are

never exposed.
The lower part of table 1.2 shows that the effects persist with the double difference strategy. Individuals exposed to HIV prevention programs are significantly less likely to be tested by 2.4 percentage points. Individuals who *will be exposed* tend to get more tested than those *exposed*. The results are consistent with Yang et al. (2022) who find a stronger and significant negative effect of 10.5 percentage points on HIV testing. The estimation is all the more surprising that there is no evidence of any impact on the HIV prevalence rate. Different explanations are further explored in section 7, including HIV knowledge and stigma.

Despite this specific identification strategy, results might be biased if foreign funders looked for places with higher HIV-test compliance year after year. Areas *to be exposed* would get a different pre-trend than areas *exposed*. Section 6 presents robustness tests.

5.2 Intensity of exposure

The impact of exposure varies according to the number of programs, the nature of programs, exposure before or after first sexual intercourse, or gender. This section investigates the impact heterogeneous impact of HIV prevention programs on testing behavior relative to intensity and gender.

Intensity as the number of programs

The first regression examined the extensive margin impact of exposure to HIV prevention programs. The concentration of HIV prevention programs may enhance exposure impact on testing behavior. The following regression includes intensity as a continuous variable measuring the number of programs ($NoPrograms$) within a 5/10km buffer. The coefficient of the double difference is now calculated from the terms $Exposed_{i,t} * NoPrograms_{i,t}$ and $TobeExposed_{i,t} * NoPrograms_{i,t}$.

$$Y_{i,d,t} = \alpha + \lambda_1 Exposed_{i,t} + \lambda_2 (Exposed_{i,t} * NoPrograms_{i,t}) + \pi_1 TobeExposed_{i,t} + \\ \pi_2 (TobeExposed_{i,t} * NoPrograms_{i,t}) + \mu NoPrograms_{i,t} + \upsilon_d + \delta_t + \gamma \cdot X'_{i,t} + \varepsilon_{i,d,t} \tag{1.2}$$

Table A1.7 shows that individuals *exposed* to HIV prevention programs are less likely to get tested than those in the control group, but the coefficient is not significant.

However, people *exposed* are more likely to have been tested than those *who will be tested*, although the estimate is not significant.

Intensity as the nature of programs

The study focuses on HIV prevention programs. Yet, many other HIV programs range from medical training to building health centers, and these programs are complementary to fighting HIV spread. Thus, exposure to HIV programs could be more intense if it includes every type of HIV program. Model 1.1 tests the effect of exposure to any HIV programs.

Table A1.7 suggests that exposure to any HIV program reduces the negative impact of exposure to HIV prevention programs. Indeed, the coefficient (-0.3 percentage points) is negative but smaller and non-significant.

Intensity as exposure relative to first sexual intercourse

Finally, intensity is analyzed as exposure to HIV prevention early in sexual life. The assumption is that people will be more aware of sexual health and get more screened if they are exposed to an HIV prevention program early in their sexual life. Although there is evidence of the positive HIV prevention impact on young people (Dupas, 2011; Dupas et al., 2018; Friedman, 2018), it is not always clear how it varies according to sexual experience and which effect it has on screening.

$$Y_{i,d,t} = \alpha + \Gamma Expbeforesex_{i,t} + \lambda TobeExposedbeforesex_{i,t} + +\upsilon_d + \delta_t + \gamma \cdot X'_{i,t} + \varepsilon_{i,d,t} \tag{1.3}$$

I use the age of the first sex intercourse is the threshold for sexual experience. The sample is split in three groups:(1) People *exposed* live in a 5/10km buffer and had their first sexual intercourse after the implementation of the program ($Expbeforesex$); (2) People $To\ be\ exposedbeforesex$ live in a 5/10km buffer where the program will be implemented after the interview and after their first intercourse ($TobeExposedbeforesex$); (3) People $Never\ exposed$ - the control group - already had their first sexual intercourse and live further than 5/10km buffer or are living with 5/10km buffer but had their first sexual intercourse before the HIV prevention program starts.

Table A1.7 estimates that individuals exposed before their first report are significantly less likely (5.2 percentage points) to have been tested than those who $will\ be\ exposedbeforesex$. This result is based on two assumptions. First, the variable to be $will\ be\ exposedbeforesex = 1$ if an individual is a virgin on the interview date and *will be exposed* after. The respondent cannot predict the age at which he will lose his virginity. This assumes that they will remain a virgin between the time of the interview and the time the program is implemented. This assumption cannot be verified. The second assumption is that exposure to the program does not delay the time of first sexual intercourse. Section 7.3 tests this hypothesis. We observe that *exposed* and $TobeExposed$ individuals had their first intercourse at average ages that are not significantly different.

5.3 Testing behavior and gender

Since 2000, women's screening has increased faster than men's. This difference is due to free and mandatory screening of pregnant women, implemented in 2003, which does not benefit men de facto. The following model tests the gender gap by including an interaction between the treatment variable and a binary variable for gender. It equals 1 if the respondent is a man and 0 if the respondent is a woman. First, it includes the full sample, and then it excludes women who had already been pregnant.

$$\begin{aligned} Y_{i,d,t} = \alpha + \Gamma_1 Exposed_{i,t} + \Gamma_2 (Exposed_{i,t} * Gender) + \lambda_1 TobeExposed_{i,t} + \\ \lambda_2 (TobeExposed_{i,t} * Gender) + \phi Gender + \upsilon_d + \delta_t + \gamma \cdot X'_{i,t} + \varepsilon_{i,d,t} \end{aligned} \tag{1.4}$$

Table A1.9 shows that HIV prevention programs had different impacts according to gender. Exposure to a prevention program generally increased the probability of being tested, but not for men. In the top part of the table, men *exposed* are significantly 6.5 percentage points less likely to be tested after exposure to an HIV prevention program than women. As for the baseline regression, this result is compared to men who *will be exposed*. The bottom of the table presents the double difference result comparing the screening trend of men *exposed* to those who *will be exposed*. Men exposed to a prevention program are significantly 1.6 percentage points less likely to be screened after exposure than women. Column (2) supports the assumption that the gender gap may be due to free screening for pregnant women. Men are no less likely to be screened than women who have never had a pregnancy and therefore never had access to free screening. Different mechanisms may be at play and deserve to be explored later. On the one hand, this highlights that there is a gender difference in the screening cost. For women, this cost is covered either by the obligation to be screened or by the fact that it is free. On the other hand, the fact that the obligation is gendered may shift the screening responsibility to women. Indeed, although screening is individual and personal, it would be interesting to question the dynamics of screening in couples. Could it be that some people do not get tested because their spouse has had an HIV test result? One could investigate further whether the difference in testing is more marked within married couples with children than within married couples without children. The sociology literature shows the inequality in the assumption of household tasks within the couple, called “cognitive labor” (Daminger, 2019) or mental load. Further investigation could focus on a “testing load”, defined as the transfer of screening responsability from men to women.

6 Robustness

The sensitivity tests reveals the temporality of the effect of exposure to HIV prevention programs. The negative impact of exposure to HIV programs is limited to people exposed to programs implemented between 2004 and 2010. Plus, the effect of exposure to HIV prevention programs vanishes once the time period of exposure is reduced to one or two years around the survey.

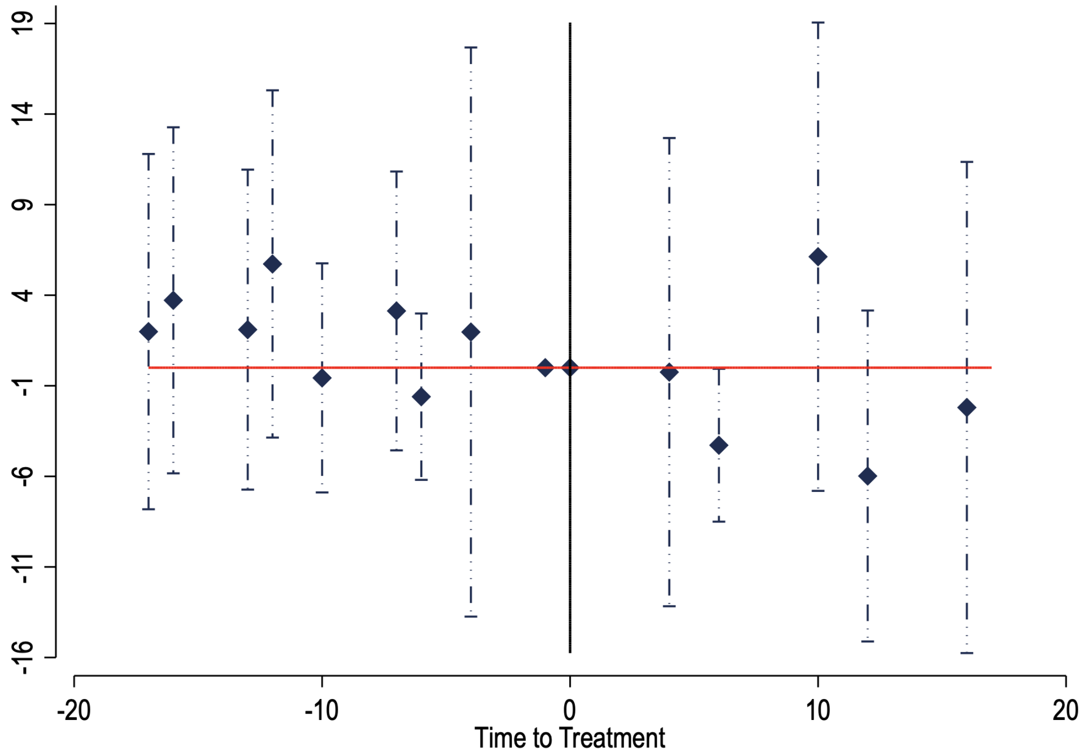
The first threat is relative to the parallel trend assumption. The structure of the DHS database and the identification strategy are a challenge to test the pre-existing trends. Ideally, a panel database would allow to observe pre- and post-exposure variation at the individual level. However, the DHS is a repeated cross-sectional database, without a panel.
The DHS enumeration area could be thought as the substitute level to observe pre- and post-exposure variation. Unfortunately, the different DHS waves select different enumeration areas from one survey to another. Even if they would visit the same enumeration area, the identification number would not be harmonized across surveys in order to protect the anonymity of the respondents. Another possibility would be to match the geolocation across survey waves. However, DHS already alters the real geolocation between 2 and 5km, in addition to not visiting the same enumeration areas. I therefore decide to build a quasi panel at the district level. It is the first smallest level at which DHS sampling is representative. In addition, the AidData databases give programs that are geolocated up to the district level. The choice of the district level will result in a loss of precision but remains robust for the pre-trend. As suggested, I conduct an event study using the 2000 and 2004 surveys as a baseline for districts that did not receive any programs at those dates. The event study presents the coefficient

of the following equation:

$$Y_{d,t} = \upsilon_d + \delta_t + \Sigma_{j=-16}^{16} \beta_j Pr_{d,t+j} + \gamma X'_{d,t} + \varepsilon_{d,t}$$

Where Y is the HIV testing rate at district level d and time t. X' is a vector of factors at district level and at time t: average years of school, average age, gender ratio, average wealth index, average distance to the first health center, proportion of Catholics and proportion of couples (married or living together). Treatment is defined as receiving an HIV-prevention program for the first time. The specification control for year and district fixed effects. $Pr_{d,t}$ is the event study indicator variable equal to one if an HIV-prevention programs was implemented in a district d years ago t.
Figure 1.4 graphs the HIV testing rate by year at district level, with respect to the treatment. The plotted estimates depict the differential trends in screening over up to 16 years before and after the implementation of any HIV-prevention program. There are no noticeable trends in the pre-treatment period.

Figure 1.4: Robustness - Event study for pre-trend



Note: The figure presents the estimates of Σ from equation 6. The treatment is defined as a district receiving an HIV prevention program for the first time. Year of HIV prevention program implementation is normalized to zero. The year before the HIV prevention program implementation is omitted. Dashed segments are 95 percent confidence intervals. The specification control for year and district fixed effects. Control variables include average years of school, average age, gender ratio, average wealth index, average distance to the first health center, proportion of Catholics and proportion of couples (married or living together).

In the second step, I use another strategy to reinforce these results at the individual level without a panel. Kuecken et al. (2021) proposes to test the parallel trends by observing the trend of the control and treated groups in population sections that are not exposed to the treatment, which is the anti-malaria campaigns. Instead of looking at the trend over time, they look at the trend of the main outcomes across each age group. In my case, I cannot state that some individuals are more exposed than another within each group of exposure. Nevertheless, the subsample of pregnant women can be used as a counterfactual. Indeed, screening of pregnant women has been mandatory since 2003. One can therefore consider that the probability of screening relative to the number of kids should be the same whether women belong to the $Never\ Exposed$, $Exposed$ or $TobeExposed$ group. I represent the screening trend by number of births per woman and by exposure groups. I exclude the outliers and restrict the sample to women who had up to 10 kids (97% of women who ever had kids). Although the HIV testing is much lower in the $TobeExposed$ group, Figure A1.5 shows parallel trends across the different groups until 7 kids.

Recent evidence in the literature question the necessity of the parallel trends test (Bilinski and Hatfield, 2020; Kahn-Lang and Lang, 2020; McKenzie, 2020). However, table A1.1 shows imbalances in the means of control variables. The entropy balancing strategy could help to relax this assumption. The entropy balancing strategy computes a set of unit weights to balance the distribution of the covariates (Hainmueller, 2012a,b). Three conditions have to hold to obtain robust results. First, the balance constraints are consistent (dimensionality of the overlap). Second, all constraints present positive weights and avoid extreme balance constraints between groups (degree of overlap). Third, the control group is large enough not to reuse the control units several times (size of the data set). The means and standard errors of the pretreatment covariates and the sample size presented in table A1.1 respect these conditions. Thus, the following variables balance the samples: years of education, age, wealth, the distance to the nearest health infrastructure, gender, religion, and the number of births for women. The entropy balancing strategy successfully balances the pretreatment covariates (see table A1.10). The results (table 1.3) show that the negative impact of exposure to HIV prevention programs (by 2.7 percentage points) is still significant at 5%.

Table 1.3: Robustness tests

	Testing behavior						
	(1) Entropy matching	(2) Control group 30km	(3) Placebo	(4) Programme FE (2000-2004)	(5) Programme FE (2004-2010)	(6) Programme FE (2010-2016)	(7) Control group To be exposed
Exposed	0.01			0.02	0.01	0.00	0.01
	(0.007)			(0.019)	(0.010)	(0.007)	(0.010)
Exposed 10km - 30km		0.02					
		(0.009)					
To be exposed	0.03**			0.03***	0.04**	−0.01	
	(0.011)			(0.007)	(0.013)	(0.015)	
To be exposed 10km - 30km		0.03***					
		(0.009)					
Exposed to counterfactual projects			0.00				
			(0.005)				
To be exposed to counterfactual projects			0.00				
			(0.006)				
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Difference in differences	−0.03**	−0.02*	0.00	−0.01	−0.03**	0.01	
F-test: active-inactive=0	5.01	2.98	0.01	0.06	4.03	0.41	
p-value, F-test	0.03	0.08	0.95	0.81	0.05	0.52	
Mean dep. var	0.54	0.54	0.54	0.13	0.51	0.75	0.50
R-squared	0.38	0.45	0.45	0.07	0.35	0.24	0.42
No. of observations	92310	43916	92310	30895	44120	61415	22446

Note: *** p <.01, **p <.05, * p <.1 This table presents coefficients of robustness tests. Column (1) displays the result of the main regression after the entropy matching reweighting. In column (2), the control group is restricted to individuals living beyond a 30km bandwidth. Column (3) individuals *exposed* and who *will be exposed* are exposed to non-health foreign aid-funded programs. Columns (4), (5), and (6) replicate the main regression and are restricted to two consecutive survey waves. All estimates include controls for age, gender, marital status, wealth, rural/urban, and distance to the nearest health center. They include year and district fixed effects. All estimates are weighted using DHS sampling weights. The standard errors in parentheses are clustered at the level of the survey's clusters. The main outcome is in the bottom part of the table, named "Difference-in-Differences". It indicates the difference between the coefficients "Exposed" and "To be Exposed". The F-test and the p-value of the F-test are presented in the bottom section.

Another threat is related to the random location of DHS clusters. It induces a risk of measurement error. In column (2) of table 1.3, the sample is restricted to compare individuals in the 10km buffer to individuals living within a distance greater than 30km to HIV prevention programs. The results are robust, although the significance power decreases at 10%. In table D3, the cut-off bandwidths are increased by 1km between 11km and 25km. The coefficients are consistent and indicate that exposure to HIV prevention programs still reduces the likelihood of being tested from 11 to 19km.

Another concern would be that exposure to HIV prevention programs captures other determinants related to the area where programs are set up. For instance, they would locate close to other foreign aid-funded programs in urban, richer, more educated, and more densely populated areas. In column (3), individuals are matched with non-health foreign aid-funded programs used as counterfactuals. The AidData database provides the interventions' coordinates in various fields, such as agriculture or education, that are not related to HIV. Table 1.3 shows that the exposure to non-health foreign aid-funded programs does not impact testing behavior and excludes the risk of confounding variables.

Further, a potential issue is that the treatment effect may be driven by the timing of the implementation of foreign aid-funded HIV prevention programs. In the baseline results, the impact of exposure to an HIV prevention program is positive but non-significant. Programs implemented earlier may have a more significant impact - benefit by being the first program for HIV prevention. Programs implemented later would have a decreasing marginal impact in the same areas. Columns (4), (5), and (6) present the result of the regression on sub-samples of consecutive waves (2000-2004, 2004-2010, 2010-2016). For individuals *Exposed*, the results are consistently positive and non-significant. However, the estimator of the double difference is only significant between 2004 and 2010. Being exposed to HIV prevention programs decreased the probability of getting tested by 3 percentage points during this period.

The main result of the difference-in-differences could be caused by the difference between the two groups *Exposed* and $TobeExposed$. The sample is restricted to people who *will be exposed* as the group control to those who are *exposed*. The coefficient should not be significantly positive to confirm the negative relationship revealed in the main regression. Column (7) reports a positive but not-significant coefficient, supporting that individuals *exposed* do not get tested more than those who *will be exposed*.

Additionally, there is a concern about possible distribution of large time lags between interviews and program implementation for the $TobeExposed$ group. An individual *Exposed* in 2008 can be surveyed in 2010 but tested in 2007. Similarly, an individual $TobeExposed$ a year after the survey is not comparable to another *Exposed* four years after the survey. Table 1.4 presents a sensitivity test that reduces the sample to individuals *Exposed* or who $TobeExposed$ to programs implemented within 12 months and 24 months of the interview date. I use the information in the DHS on the last test date to adapt the main outcomes. The main outcome for HIV testing equals 1 if the individual exposed the year before the interview, declared he got tested in the previous year. Similarly for those exposed within 2 years before the survey. For the two cut-offs, individuals *Exposed* and $Tobeexposed$ got less tested than individuals $Never$ *Exposed*. However, the coefficient of the double difference is different from the one-year cut-off to the two-year cut-off. The former is positive while the latter is negative. These results suggest a non-linear effect of exposure to HIV prevention programs over

time. The third column confirms the sign of the double difference coefficient for the one-year cut-off. The sample in this column is restricted to programs with the exact geolocation. However, it shows a different coefficient in the single differences. *Exposed* and *To be Exposed* individuals were tested more than unexposed individuals. All in all, exposure to a program fails to significantly increase the likelihood of being screened.
Finally, the rate of exposure varies within the district but cannot be exactly illustrated because of some geolocations are not exact. Some projects are simply located in the district capital by default (84/118), underestimating the intra-district variability. The dataset gives 32 precise geolocations. The third column of table 1.4 presents the results of the regression on a sample reduced to individuals *Exposed* or who $TobeExposed$ to programs implemented within 12 months of the interview date and for which the exact geolocation is available. The new estimates show that being exposed to HIV prevention programs within a year or two years does not impact significantly the likelihood of getting testing.

Table 1.4: Robustness tests - Time lag

	Testing behavior		
	(1) Cut-off 1 year	(2) Cut-off 2 years	(3) One-year cut-off Precision 1
Exposed within 12 months	−0.00		
	(0.007)		
To be exposed 12 months	−0.00		
	(0.011)		
Exposed within 24 months		−0.02**	
		(0.008)	
To be exposed 24 months		−0.02	
		(0.013)	
Exposed within 12 months (P1)			0.02*
			(0.011)
To be exposed within 12 months (P1)			0.02*
			(0.009)
Difference in differences	0.00	−0.00	0.01
F-test: active-inactive=0	0.03	0.10	0.16
p-value, F-test	0.87	0.76	0.69
Mean dep. var	0.64	0.64	0.53
R-squared	0.14	0.17	0.21
No. of observations	58.17	58.17	50.34

Note: *** p <.01, **p <.05, * p <.1 This table presents the impact of exposure to HIV prevention program on screening. The model used is similar to equation 1.1. Individuals are said *Exposed* if they live in a 5/10km buffer around a program implemented before their last test (and not the interview date). People living in a 5/10km buffer of a program starting after their testing date (or interview date by default) are in the group *TobeExposed*. The difference is that the sample is restricted to people *Exposed* and *TobeExposed* within a year (column 1) or 2 years (column 2) before and after the survey. Column 3 adds a restriction, as it includes programs with a precise localisation (Precision 1 or P1) exclusively. People living further than a 5/10km buffer around the HIV prevention program are in the control group. The date of screening is estimated based on the date of the interview and the respondent's answer to: "When was your last test: a year ago? between 12 and 24 months? more than two years ago?". I take the previous year's date for those who answered "a year ago". I take the date 18 months ago for those who answered "between 12 and 24 months". I take the date two years ago for those who answered "more than 2 years ago". All estimates include controls for age, gender, marital status, wealth, rural/urban, religion, and distance to the nearest health center. They include year and district fixed effects. All estimates are weighted using DHS sampling weights. The standard errors in parentheses are clustered at the level of the survey's clusters. The main outcome is in the bottom part of the table, named "Difference-in-Differences." It indicates the difference between the coefficients "Exposed" and "To be Exposed." The F-test and the p-value of the F-test are presented in the bottom section.

Another table proposes an additional robustness test to control for the time lags. Respondents mentioned whether the last test occurred within the last 12 months, between 12 and 24 months, or beyond 24 months[9]. I restrict the definition of exposure. For those *Exposed* at the interview, the main outcomes equals 1 if the last testing

[9] I set a date 18 months ago for those who answered "between 12 and 24 months" and two years before for those who answered "more than 2 years ago".

happened within the year. It would equal 0 if the testing happened more than 12 months ago. Individuals exposed more than a year before the interview date are not in the sample anymore. Table A1.11 reports that individuals *exposed* and *who will be exposed* are 2.8 and 3.9 more likely to get tested than those never exposed. Although the coefficient of the difference-in-differences is negative, it is not significant.

7 Discussion on mechanisms

HIV prevention programs impact screening through different channels. Information spreading should increase people's knowledge about HIV and improve sexual behaviors and attitudes toward those who are - allegedly or knowingly - HIV positive. Table 1.5 presents the impact of exposure to HIV prevention on these mechanisms by replicating the main regression.

Table 1.5: Mechanisms

	(1) Mean	(2) Exposed	(3) To be exposed	(4) Difference in Differences	(5) *N*
Knowledge					
Knowledge score	6.31	0.03	0.03	-0.008	92766
		(0.031)	(0.039)		
Attitude					
Stigma	0.29	0.00	-0.01	0.016^{*}	92766
		(0.007)	(0.009)		
Sexual Behaviour					
All					
Age of those never had intercourse	17.05	-0.02	0.10	-0.122	10307
		(0.085)	(0.110)		
Age of first intercourse	19.95	0.07	-0.04	0.114	74889
		(0.259)	(0.279)		
Other sexual partner (extensive margin)	0.11	0.02^{***}	0.01	0.016^{***}	74889
		(0.005)	(0.005)		
Use condom in the last intercourse	0.10	0.01	0.00	0.005	64688
		(0.005)	(0.004)		
Men					
Paid for sex	0.18	-0.01	0.01	-0.026^{*}	17414
		(0.010)	(0.013)		

Note: *** p <.01, **p <.05, * p <.1 The table presents the results of the double difference by changing the primary outcomes for variables capturing the mechanisms that would explain why exposure to HIV prevention programs might negatively impact the testing decision: Knowledge, Attitude, Sexual behavior and Gender. The estimation for sexual behavior includes controls for age, gender, wealth, rural/urban, and distance to the nearest health center. It includes year and district fixed effects. The sample is restricted to respondents who have had intercourse at least once for the sexual behavior. The standard errors, in parentheses, are clustered at the survey cluster, except for the dependent variable "Stigma (proportion per cluster)". In column (4), The "Difference-in-Differences" indicates the difference between "Exposed" and "To be Exposed". The F-test and the p-value of the F-test are presented in the bottom section. Columns (2) and (3) present results on the baseline sample.

7.1 Knowledge

Knowledge is one of the main channels through which information shared by HIV prevention interventions may impact testing behavior. It is measured by an indicator from 0 to 8 that aggregates each correct answer to a set of questions about HIV transmission (see table A1.14).

The literature has assessed that information impacts the level of knowledge (Godlonton et al., 2015), although the behavioral change following HIV information remains heterogeneous (Gallant and Maticka-Tyndale, 2004). Wilson (2016) assesses that the impact of free counseling and testing is understated and would sharply increase testing behaviors. People exposed to HIV prevention should have a higher level of knowledge on the topic (Paul-Ebhohimhen et al., 2008). In their respective randomized control trials (RCT) in Malawi, Kerwin (2018) and Derksen et al. (2022) use information as a treatment and reveal its divergent effects on health behavior. Specifically, in Derksen et al. (2022), individuals receive information on the life expectancy of an HIV-positive person under ARV. The intervention increased average beliefs on the public benefit of ART and the annual testing rate in closer health facilities. Kerwin (2018) shows that providing information about the actual risk of HIV infection reduces the sexual activity of those with a higher HIV-infection risk estimation. The latter had a fatalistic bias, leading them to greater risky sexual activity. The dataset used in the current study does not detail the nature of the information spread. The analysis cannot assess any misinformation in the programs. Recent outcomes in the RCT run by Yang et al. (2022) reveal that misinformation spread in a simple exposure to an HIV prevention program heightens the stigmatizing attitude.

The level of knowledge is regressed on exposure to HIV prevention programs, using the main estimation strategy. Table 1.5 finds no difference in the level of knowledge between individuals *exposed*, *who will be exposed* or never exposed. The result is somewhat unsurprising, given that Malawi has implemented HIV education initiatives that have raised the overall knowledge to the highest ranking worldwide Roser and Ritchie (2020).

7.2 Stigma

The second hypothesis is that exposure to an HIV prevention program may worsen the stigmatizing attitude towards - allegedly or knowingly - HIV-positive individuals.

Adeneye et al. (2007) and Derksen et al. (2022) reports that stigma is one obstacle to HIV screening in their RCT in Nigeria and Malawi, respectively.

A prevention program should decrease the likelihood of stigma by increasing the level of information about the disease and its treatment. Figure A1.6 does show a correlation between knowledge and stigma. However, the presence of a program may also indicate a high prevalence of HIV-positive people. Getting screened or visiting a screening center could be interpreted as the behavior of someone with risky sex life or already being HIV-positive. For instance, in Derksen et al. (2022), being seen regularly at the hospital for screening may be a sign of HIV infection. HIV-positive individuals are at risk of discrimination, so people prefer to choose remote health centers to be screened (Bond et al. (2002), Malawi Journals Project).

Stigma is regressed on exposure. The final variable is a dummy equal to 0 (no answer denoting negative attitude towards HIV) or 1 (at least one answer denoting negative attitude toward HIV). In DHS, people are asked to describe how they would act in hypothetical settings where they would meet a seropositive person. This dataset has been used by Delavande et al. (2014) to measure social intolerance. Questions may suffer from social desirability bias which is controlled by the rotation in the formulation of the questions from one year to another(see table A1.15). Table 1.5 shows that people exposed to HIV prevention programs are more likely to declare negative behavior, i.e. stigma, at 1.6 percentage points (significant at 10 percent). A mediation analysis estimates the treatment effect due to stigma based on the indication of Acharya et al. (2016) and its application by Abebe et al. (2021) to calculate the Average Controlled Direct Effect (ACDE). The ACDE gives ceteris paribus the treatment's direct effect. The selected mediators are fixed to give a controlled direct effect, an alternative measure of the treatment's impact. This methodology allows exploring one mechanism on the assumption. The total effect is distinct from the effect of the mediator. Figure A1.7 illustrates that the impact of the exposure can be explained at 19 percent by changes in the stigmatizing attitude. The finding should be treated with some caution given the constraints of the DHS surveys. The level of stigma is measured after the screening date. However, the results are consistent with figure A1.7, which details the analysis of this mechanism.

7.3 Sexual behavior

Interactions between prevention, screening, and sexual behavior are ambiguous. One might think that prevention programs increase testing and reduce risky sexual behavior. However, screening can be done before engaging in sexual intercourse, or it can be consecutive to risky sexual intercourse. HIV-information programs have proven to reduce risky sexual behavior among young girls (Dupas, 2011; Duflo et al., 2015; Dupas et al., 2018) and Friedman (2018) shows that exposure to ART would increase demand for HIV testing. As a preventive measure, the increasing screening rate would allow young women to sero-sort their potential partners. The pregnancy rate (proxy of risky sexual behavior) increases because women are aware of their partner's status and do not engage in risky intercourse.

Demonstrating the causal relationship between prevention program exposure, sexual behavior, and testing behavior is beyond the scope of this paper. However, the final section opens avenues to new analysis and encourages the collection of panel data to test these hypotheses.

Age of first sexual intercourse

Individuals exposed before their first intercourse may delay the likelihood of engaging in sex, decreasing the likelihood of being screened - since there would be no benefit to screening before sexual activity[10]. The regression tests the hypothesis that: *Exposure > Sexual behavior > Screening*[11]. Table 1.5 indicates that people *exposed* were not older than those *who will exposed* when they had their first sexual intercourse. There is no difference for those who have not yet had sexual intercourse.

Sexual partners

Individuals exposed could avoid extramarital partners to reduce the risk of contracting HIV and would therefore have less incentive to get tested. The impact chain would be: *Exposure > Sexual behavior > Screening*. However, people *exposed* may also feel more protected by HIV programs and engage in sexual intercourse with different partners. The relationship would be: *Exposure > Screening > Sexual behavior*.

DHS interviewed respondents on whether they had sexual intercourse with someone other than their partner over the past 12 months (extensive margin). Table 1.5 reports

[10]However, 22% of those who never had sexual intercourses declare they have been tested for HIV.

[11]Where > means "change(s)".

no impact on the likelihood of having another partner. However, it indicates that individuals exposed are more likely to use a condom as whether they were more careful since they are not getting tested.

Paid for sex - Men

Eventually, men exposed were significantly less likely to pay for sexual intercourse. Further investigation will clarify if they tend to reduce risky sexual intercourse because they are getting less tested.

8 Conclusion

This paper proposes a quasi-experimental study to estimate the impact of foreign aid-funded HIV prevention programs on screening behavior in Malawi. It draws on two databases (Demographic and Health Survey and AidData) to show the effects of foreign aid in enhancing screening behavior. The empirical strategy takes advantage of the time and geographical variation in the implementation of the project. It matches the geolocations of DHS respondents to HIV prevention programs.

Estimates find that, on average, individuals living in areas exposed to prevention programs are 2.4 percentage points less likely to get tested. After sensitivity tests, exposure did not affect testing behavior except from 2004 to 2010, when it decreased the likelihood of getting tested by 3 percentage points. However, once the comparison of the groups is reduced to one or two years around the interview date, the negative effect is no longer significant. At best, therefore, it can be stated that exposure to internationally funded HIV prevention programs did not have a significant positive effect on the likelihood of being tested.

The heterogeneity analysis reveals that gender affects the impact of HIV prevention programs. Men are less likely to be screened than women, except when the sample is reduced to women who never gave birth. The origin of this gender gap deserve further study, specifically because Malawi has had a policy of mandatory screening for pregnant women only since 2013.

Mechanisms analysis shows behavioral spillovers of foreign aid-funded HIV prevention programs. People that have received the programs are also more likely to stigmatize others by 1.6 percentage points. However, being exposed did not increase the level of knowledge about HIV. One explanation is that there is little variation in HIV knowledge across the country. People have a high level of knowledge about HIV thanks to national investment in education for HIV. In another setting, like Mozambique, Yang et al. (2022) show that information disclosed may be misunderstood by those who receive it and that people are more likely to stigmatize after participating in prevention programs. However, the information disclosed by prevention programs needs to be more precisely documented in a further study, such as what was shared and to whom. The limited effect of prevention programs does not question the importance of disseminating information to encourage the adoption of preventive health behavior. Rather, these results reveal that the coordination of the funds' allocation is just as important as the microeconomic impact.

One can imagine that a randomized control trial would have been preferable to secure the experiment's internal validity. However, this choice may remain a second best choice. Randomized control trials require substantial financial expenses that would benefit from being directly invested in HIV treatment, screening or prevention programs. The first option would be a longitudinal database, that would allow to implement a more canonical two-way fixed effects to estimate the impact of foreign aid on testing. However,de Chaisemartin and D'Haultfœuille (2020); Callaway and Sant'Anna (2021); Goodman-Bacon (2021) highlight the risk of bias with a staggered treatment.

All in all, as new health priorities may threaten budget mobilization in the fight against HIV, accurate estimation of foreign aid effectiveness is crucial. Research could identify the channels that would explain it with more detailed data.

9 Bibliography

Abebe, G., Caria, A. S., Fafchamps, M., Falco, P., Franklin, S., and Quinn, S. (2021). Anonymity or distance? job search and labour market exclusion in a growing african city. *The Review of Economic Studies*, 88(3):1279–1310.

Acharya, A., Blackwell, M., and Sen, M. (2016). Explaining causal findings without bias: Detecting and assessing direct effects. *American Political Science Review*, 110(3):512–529.

Adeneye, A. K., Brieger, W. R., Mafe, M. A., Adeneye, A. A., Salami, K. K., Titiloye, M. A., Adewole, T. A., and Agomo, P. U. (2007). Willingness to Seek HIV Testing and Counseling among Pregnant Women Attending Antenatal Clinics in Ogun State, Nigeria. *International Quarterly of Community Health Education*, 26(4):337–353. Publisher: SAGE Publications Inc.

Allport, G. W., Clark, K., and Pettigrew, T. (1954). The nature of prejudice.

Bernardo, E. L., Fuente-Soro, L., Lopez-Varela, E., and Naniche, D. (2017). Anonymity in HIV testing: implications for public health. *The Lancet*, 390(10112):2546. Publisher: Elsevier.

Bilinski, A., Birru, E., Peckarsky, M., Herce, M., Kalanga, N., Neumann, C., Bronson, G., Po-Chedley, S., Kachimanga, C., McBain, R., and Keck, J. (2017). Distance to care, enrollment and loss to follow-up of HIV patients during decentralization of antiretroviral therapy in Neno District, Malawi: A retrospective cohort study. *PLOS ONE*, 12(10):e0185699. Publisher: Public Library of Science.

Bilinski, A. and Hatfield, L. A. (2020). Nothing to see here? Non-inferiority approaches to parallel trends and other model assumptions. arXiv:1805.03273 [stat].

Bird, J. and Straub, S. (2014). The Brasilia Experiment: Road Access and the Spatial Pattern of Long-Term Local Development in Brazil.

Bond, V., Chase, E., and Aggleton, P. (2002). Stigma, HIV/AIDS and prevention of mother-to-child transmission in Zambia. *Evaluation and Program Planning*, 25(4):347–356.

Callaway, B. and Sant'Anna, P. H. C. (2021). Difference-in-Differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Cameron, A. C. and Miller, D. L. (2015). A Practitioner's Guide to Cluster-Robust Inference. *Journal of Human Resources*, 50(2):317–372. Publisher: University of Wisconsin Press.

Daminger, A. (2019). The Cognitive Dimension of Household Labor. *American Sociological Review*, 84(4):609–633. Publisher: SAGE Publications Inc.

de Chaisemartin, C. and D'Haultfœuille, X. (2020). Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, 110(9):2964–2996.

de Walque, D. (2007). How does the impact of an HIV/AIDS information campaign vary with educational attainment? Evidence from rural Uganda. *Journal of Development Economics*, 84(2):686–714.

Deaton, A. (2013). *The Great Escape: Health, Wealth, and the Origins of Inequality*. Princeton University Press. Publication Title: The Great Escape.

Delavande, A. and Kohler, H.-P. (2012). The Impact of HIV Testing on Subjective Expectations and Risky Behavior in Malawi. *Demography*, 49(3):1011–1036.

Delavande, A., Sampaio, M., and Sood, N. (2014). HIV-related social intolerance and risky sexual behavior in a high HIV prevalence environment. *Social Science & Medicine*, 111:84–93.

Derksen, L., Muula, A., and van Oosterhout, J. (2022). Love in the time of hiv: How beliefs about externalities impact health behavior. *Journal of Development Economics*, 159:102993.

Donaldson, D. (2018). Railroads of the Raj: Estimating the Impact of Transportation Infrastructure. *American Economic Review*, 108(4-5):899–934.

Dreher, A., Lang, V. F., and Ziaja, S. (2017). Foreign Aid in Areas of Limited Statehood. *SSRN Electronic Journal*.

Duflo, E., Dupas, P., and Kremer, M. (2015). Education, HIV, and Early Fertility: Experimental Evidence from Kenya. *American Economic Review*, 105(9):2757–2797.

Dupas, P. (2011). Do Teenagers Respond to HIV Risk Information? Evidence from a Field Experiment in Kenya. *American Economic Journal: Applied Economics*, 3(1):1–34.

Dupas, P., Huillery, E., and Seban, J. (2018). Risk information, risk salience, and adolescent sexual behavior: Experimental evidence from Cameroon. *Journal of Economic Behavior & Organization*, 145:151–175.

Easterly, W. (2006). The White Man's Burden. *The Lancet*, 367(9528):2060. Publisher: Elsevier.

Friedman, W. H. (2018). Antiretroviral drug access and behavior change. *Journal of Development Economics*, 135:392–411.

Gallant, M. and Maticka-Tyndale, E. (2004). School-based HIV prevention programmes for African youth. *Social Science & Medicine*, 58(7):1337–1351.

Godlonton, S., Munthali, A., and Thornton, R. (2015). Responding to Risk: Circumcision, Information, and HIV Prevention. *The Review of Economics and Statistics*, 98(2):333–349. Publisher: MIT Press.

Godlonton, S. and Thornton, R. (2012). Peer effects in learning HIV results. *Journal of Development Economics*, 97(1):118–129.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Granich, R. M., Gilks, C. F., Dye, C., De Cock, K. M., and Williams, B. G. (2009). Universal voluntary HIV testing with immediate antiretroviral therapy as a strategy for elimination of HIV transmission: a mathematical model. *The Lancet*, 373(9657):48–57.

Greenwood, J., Kircher, P., Santos, C., and Tertilt, M. (2019). An Equilibrium Model of the African HIV/AIDS Epidemic. *Econometrica*, 87(4):1081–1113. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.3982/ECTA11530.

Hainmueller, J. (2012a). Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies. *Political Analysis*, 20(1):25–46. Publisher: Cambridge University Press.

Hainmueller, J. (2012b). Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies. *Political Analysis*, 20(1):25–46.

Isaksson, A.-S. and Kotsadam, A. (2018). Chinese aid and local corruption. *Journal of Public Economics*, 159:146–159.

Kahn-Lang, A. and Lang, K. (2020). The Promise and Pitfalls of Differences-in-Differences: Reflections on 16 and Pregnant and Other Applications. *Journal of Business & Economic Statistics*, 38(3):613–620. Publisher: Taylor & Francis _eprint: https://doi.org/10.1080/07350015.2018.1546591.

Kerwin, J. (2018). Scared Straight or Scared to Death? The Effect of Risk Beliefs on Risky Behaviors. SSRN Scholarly Paper ID 2797493, Social Science Research Network, Rochester, NY.

Khomba, D. C. and Trew, A. (2019). Aid and growth in Malawi.

Knutsen, C. H., Kotsadam, A., Olsen, E. H., and Wig, T. (2017). Mining and Local Corruption in Africa. *American Journal of Political Science*, 61(2):320–334. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/ajps.12268.

Knutsen, T. and Kotsadam, A. (2020). The political economy of aid allocation: Aid and incumbency at the local level in Sub Saharan Africa. *World Development*, 127:104729.

Kotsadam, A., Østby, G., Rustad, S. A., Tollefsen, A. F., and Urdal, H. (2018). Development aid and infant mortality. Micro-level evidence from Nigeria. *World Development*, 105:59–69.

Kuecken, M., Thuilliez, J., and Valfort, M.-A. (2021). Disease and Human Capital Accumulation: Evidence from the Roll Back Malaria Partnership in Africa. *The Economic Journal*, 131(637):2171–2202.

Maheswaran, H., Petrou, S., MacPherson, P., Choko, A. T., Kumwenda, F., Lalloo, D. G., Clarke, A., and Corbett, E. L. (2016). Cost and quality of life analysis of HIV self-testing and facility-based HIV testing and counselling in Blantyre, Malawi. *BMC Medicine*, 14(1):34.

Marty, R., Dolan, C. B., Leu, M., and Runfola, D. (2017). Taking the health aid debate to the subnational level: the impact and allocation of foreign health aid in Malawi. *BMJ Global Health*, 2(1):e000129. Publisher: BMJ Specialist Journals Section: Research.

McKenzie, D. (2020). Revisiting the Difference-in-Differences Parallel Trends Assumption: Part II What happens if the parallel trends assumption is (might be) violated?

Milsom, L. (2021). Spatial inequality of opportunity in West Africa. Publisher: University of Oxford.

Ndikumana, L. and Pickbourn, L. (2017). The Impact of Foreign Aid Allocation on Access to Social Services in sub-Saharan Africa: The Case of Water and Sanitation. *World Development*, 90:104–114.

NSO/Malawi, N. S. O. and Macro, I. C. F. (2011). Malawi Demographic and Health Survey 2010.

NSO/Malawi, N. S. O. and Macro, O. R. C. (2005). Malawi Demographic and Health Survey 2004.

Nunnenkamp, P. and Öhler, H. (2011). Throwing Foreign Aid at HIV/AIDS in Developing Countries: Missing the Target? *World Development*, 39(10):1704–1723.

Odokonyero, T., Marty, R., Muhumuza, T., Ijjo, A. T., and Moses, G. O. (2018). The impact of aid on health outcomes in Uganda. *Health Economics*, 27(4):733–745.

Office/Malawi, N. S. and ICF (2017). Malawi Demographic and Health Survey 2015-16.

Office/Malawi, N. S. and Macro, O. R. C. (2001). Malawi Demographic and Health Survey 2000.

Oster, E. (2005). Sexually transmitted infections, sexual behavior, and HIV/AIDS epidemic. *Quarterly Journal of Economics*, pages 467–515.

Oster, E. (2012). HIV and sexual behavior change: Why not Africa? *Journal of Health Economics*, 31(1):35–49.

Palk, L., Okano, J. T., Dullie, L., and Blower, S. (2020). Travel time to health-care facilities, mode of transportation, and HIV elimination in Malawi: a geospatial modelling analysis. *The Lancet Global Health*, 8(12):e1555–e1564. Publisher: Elsevier.

Paul-Ebhohimhen, V. A., Poobalan, A., and van Teijlingen, E. R. (2008). A systematic review of school-based sexual health interventions to prevent STI/HIV in sub-Saharan Africa. *BMC Public Health*, 8(1):4.

Peratsakis, C., Powell, J., Findley, M., Baker, J., and Weaver, C. (2012). Geocoded Activity-Level Data from the Government of Malawi's Aid Management Platform. Technical report, AidData and the Robert S. Strauss Center for International Security and Law, Washington D.C.

Rajlakshmi, D. and Becker, C. (2015). The foreign aid effectiveness debate: Evidence from malawi. aid-data working paper# 6. williamsburg, va: Aiddata.

Roser, M. and Ritchie, H. (2020). HIV / AIDS. *Our World in Data*.

Roura, M., Urassa, M., Busza, J., Mbata, D., Wringe, A., and Zaba, B. (2009). Scaling up stigma? The effects of antiretroviral roll-out on stigma and HIV testing. Early evidence from rural Tanzania. *Sexually Transmitted Infections*, 85(4):308–312. Publisher: The Medical Society for the Study of Venereal Disease Section: Health services research.

Sande, L., Maheswaran, H., Mangenah, C., Mwenge, L., Indravudh, P., Mkandawire, P., Ahmed, N., d'Elbee, M., Johnson, C., Hatzold, K., Corbett, E. L., Neuman, M., and Terris-Prestholt, F. (2018). Costs of accessing HIV testing services among rural Malawi communities. *AIDS Care*, 30(sup3):27–36. Publisher: Taylor & Francis _eprint: https://doi.org/10.1080/09540121.2018.1479032.

Ssozi, J. and Amlani, S. (2015). The Effectiveness of Health Expenditure on the Proximate and Ultimate Goals of Healthcare in Sub-Saharan Africa. *World Development*, 76:165–179.

Sterck, O. (2014). HIV/AIDS and Fatalism: Should Prevention Campaigns Disclose the Transmission Rate of HIV? *Journal of African Economies*, 23(1):53–104.

Strandow, D., Findley, M., Nielson, D., and Powell, J. (2011). *The UCDP and AidData codebook on georeferencing aid : Version 1.1*. Department of Peace and Conflict Research, Uppsala University.

Thornton, R. L. (2008). The Demand for, and Impact of, Learning HIV Status. *American Economic Review*, 98(5):1829–1863.

Tsai, A. C., Bangsberg, D. R., Kegeles, S. M., Katz, I. T., Haberer, J. E., Muzoora, C., Kumbakumba, E., Hunt, P. W., Martin, J. N., and Weiser, S. D. (2013). Internalized Stigma, Social Distance, and Disclosure of HIV Seropositivity in Rural Uganda. *Annals of Behavioral Medicine*, 46(3):285–294.

UNAIDS (2019). UNAIDS HIV Financial Dashboard. Library Catalog: hivfinancial.unaids.org.

Warwick, Z. (2006). The influence of antiretroviral therapy on the uptake of HIV testing in Tutume, Botswana. *International Journal of STD & AIDS*, 17(7):479–481. Publisher: SAGE Publications.

WHO (2014). Implementation of option b+ for prevention of mother-to-child transmission of hiv: the malawi experience.

Wilson, N. (2016). Antiretroviral therapy and demand for HIV testing: Evidence from Zambia. *Economics & Human Biology*, 21:221–240.

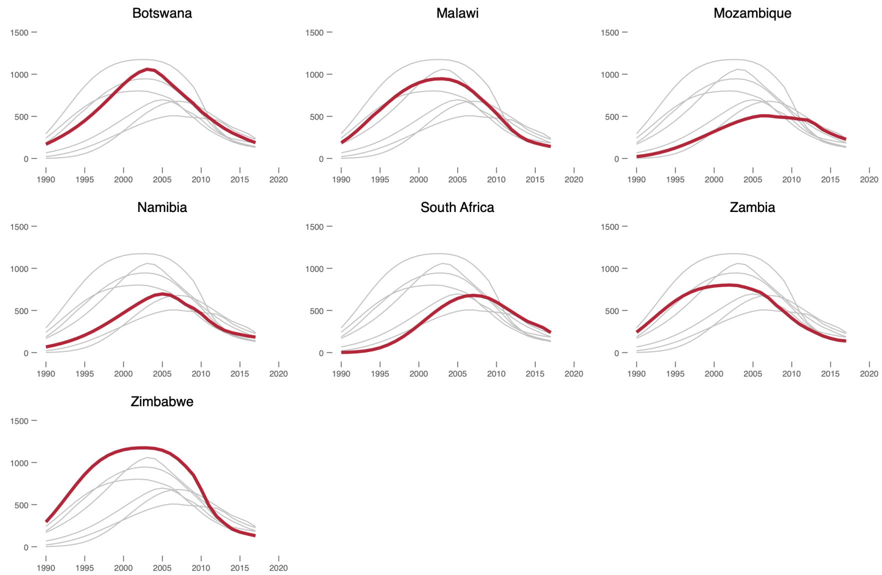
Yang, D., Allen IV, J., Mahumane, A., Riddell IV, J., and Yu, H. (2022). Knowledge, stigma, and hiv testing: An analysis of a widespread hiv/aids program. *Journal of Development Economics*, page 102958.

Young, S. D. and Zhu, Y. (2012). Behavioral Evidence of HIV Testing Stigma. *AIDS and Behavior*, 16(3):736–740.

Contents

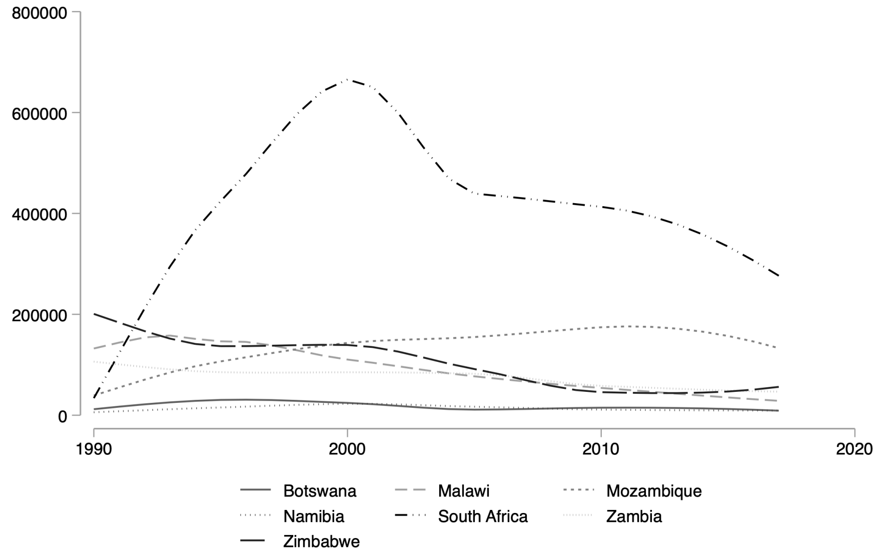
Appendix A1. Chapter 1

Figure A1.1: HIV death rate per 100 000 in Southern African countries



Note: Author's graph from the database of Roser and Ritchie (2020). The HIV death rate is the annual number of deaths from HIV per 100 000 people from 1990 to 2017. Selected countries: Botswana, Malawi, Mozambique, Namibia, South Africa, Zambia, and Zimbabwe.

Figure A1.2: HIV incidence rate per 100 000 in Southern African countries



Note: Author's graph from the database of Roser and Ritchie (2020). The HIV incidence rate is the annual number of new HIV cases from 1990 to 2017. Selected countries: Botswana, Malawi, Mozambique, Namibia, South Africa, Zambia, and Zimbabwe.

Table A1.1: Balance table - Exposed, To be Exposed, Never Exposed

	Never Exposed vs. Exposed	Never Exposed vs. To be Exposed	Exposed vs. To be Exposed
Demographic			
Age	-0.560***	0.641***	1.201***
	(0.094)	(0.101)	(0.128)
Gender	-0.005	0.018***	0.023***
	(0.004)	(0.004)	(0.006)
Years of education	-0.016	0.239***	0.255***
	(0.026)	(0.028)	(0.035)
Rural	0.112***	0.153***	0.041***
	(0.004)	(0.004)	(0.006)
Health facilities, 10km radius	-4.317***	-6.074***	-1.757***
	(0.099)	(0.106)	(0.211)
Distance to the nearest HIV-prevention program	42.842***	38.616***	-4.226***
	(0.482)	(4.849)	(0.211)
Marital status:			
Never married	-0.037***	0.010**	0.047***
	(0.004)	(0.004)	(0.006)
Married	0.067***	-0.028***	-0.095***
	(0.005)	(0.005)	(0.007)
Living together	-0.011***	0.028***	0.038***
	(0.002)	(0.002)	(0.003)
Widowed	-0.007***	-0.003*	0.004*
	(0.002)	(0.002)	(0.002)
Divorced	-0.011***	-0.010***	0.000
	(0.002)	(0.002)	(0.003)
Not living together	-0.002	0.003	0.005*
	(0.002)	(0.002)	(0.003)
Sexual Behavior			
Already had sexual intercourse	0.008**	-0.016***	-0.023***
	(0.003)	(0.003)	(0.004)
Age of first sexual intercourse	0.302***	-0.131**	-0.433***
	(0.065)	(0.066)	(0.085)
Sex with someone else than partner last 12 months	-0.024***	-0.014***	0.010**
	(0.003)	(0.003)	(0.004)
Sex with someone else than partner last 3 intercourse	0.003**	-0.009***	-0.011***
	(0.001)	(0.001)	(0.002)
HIV outcomes			
Ever been tested for aids	-0.154***	0.315***	0.469***
	(0.005)	(0.005)	(0.006)
Date of last HIV test:			
Less than 12 months	-0.015***	-0.044***	-0.029**
	(0.005)	(0.011)	(0.011)
12 to 23 months	0.015***	-0.026***	-0.040***
	(0.003)	(0.007)	(0.007)
More than 24 months	-0.000	0.069***	0.070***
	(0.005)	(0.011)	(0.011)
HIV status - DHS test	-0.033***	-0.046***	-0.013
	(0.004)	(0.007)	(0.009)

Note: The table presents the t-statistics with standard error is in parentheses. The mean difference is significant if *** p <.01, **p <.05, * p <.1.

Table A1.2: DHS composition per year

	Never Exposed	Exposed	To be Exposed	Total
Individual level				
2000	15.64	0.87	49.55	17.44
	[11099]	[107]	[5106]	[16312]
2004	14.78	6.74	35.40	16.00
	[10486]	[825]	[3648]	[14959]
2010	33.25	46.76	8.48	32.29
	[23593]	[5728]	[874]	[30195]
2016	36.33	45.63	6.57	34.27
	[25774]	[5589]	[677]	[32040]
Total	100.00	100.00	100.00	100.00
	[70952]	[12249]	[10305]	[93506]
	Control	Exposed	To be Exposed	Total
District level				
2000	30.43	1.75	52.38	24.59
	[7]	[1]	[22]	[30]
2004	34.78	12.28	35.71	24.59
	[8]	[7]	[15]	[30]
2010	13.04	42.11	7.14	24.59
	[3]	[24]	[3]	[30]
2016	21.74	43.86	4.76	26.23
	[5]	[25]	[2]	[32]
Total	100.00	100.00	100.00	100.00
	[23]	[57]	[42]	[122]

Note: Proportion of each wave in the final sample, at individual and district levels. The number of observations is in brackets.

Sources Office/Malawi and ICF (2017), NSO/Malawi and Macro (2011), NSO/Malawi and Macro (2005), Office/Malawi and Macro (2001).

Table A1.3: Summary statistics - By year of survey

	Survey year				
	2000	2004	2010	2016	Total
Demographic					
Respondent's current age	28.04	28.10	28.35	28.30	28.24
	(9.64)	(9.44)	(9.72)	(9.60)	(9.62)
Gender	0.19	0.22	0.24	0.23	0.22
	(0.39)	(0.41)	(0.43)	(0.42)	(0.42)
Years of education	3.39	3.51	3.89	3.87	3.74
	(2.72)	(2.70)	(2.63)	(2.54)	(2.63)
Rural	0.78	0.86	0.86	0.78	0.82
	(0.41)	(0.35)	(0.34)	(0.41)	(0.38)
Health facilities, 10km radius	8.00	7.17	5.78	6.54	6.65
	(12.74)	(10.91)	(10.71)	(10.73)	(11.15)
Distance to the nearest HIV-prevention program	86.98	67.22	31.84	27.84	41.63
	(47.76)	(38.36)	(29.15)	(23.43)	(37.04)
Marital status:					
Never married	0.21	0.20	0.24	0.26	0.23
	(0.40)	(0.40)	(0.43)	(0.44)	(0.42)
Married	0.67	0.66	0.57	0.59	0.61
	(0.47)	(0.47)	(0.50)	(0.49)	(0.49)
Living together	0.02	0.04	0.09	0.04	0.05
	(0.13)	(0.19)	(0.28)	(0.20)	(0.22)
Widowed	0.03	0.03	0.03	0.02	0.03
	(0.17)	(0.17)	(0.17)	(0.15)	(0.16)
Divorced	0.04	0.04	0.04	0.04	0.04
	(0.20)	(0.20)	(0.20)	(0.20)	(0.20)
Not living together	0.03	0.03	0.04	0.04	0.04
	(0.17)	(0.17)	(0.19)	(0.21)	(0.19)
Sexual Behavior					
Already had sexual intercourse	0.90	0.89	0.86	0.88	0.88
	(0.30)	(0.31)	(0.34)	(0.33)	(0.33)
Age of first sexual intercourse	14.71	14.73	14.26	14.34	14.45
	(5.78)	(5.92)	(6.47)	(5.90)	(6.09)
Sex with someone else than partner last 12 months	0.12	0.09	0.10	0.14	0.11
	(0.32)	(0.29)	(0.30)	(0.35)	(0.32)
Sex with someone else than partner last 3 intercourse	0.03	0.02	0.01	0.01	0.02
	(0.16)	(0.14)	(0.09)	(0.12)	(0.12)
HIV outcomes					
Ever been tested for aids	0.10	0.15	0.69	0.81	0.54
	(0.30)	(0.36)	(0.46)	(0.39)	(0.50)
Date of last HIV test:					
Less than 12 months	.	0.48	0.39	0.45	0.42
	(.)	(0.50)	(0.49)	(0.50)	(0.49)
12 to 23 months	.	0.25	0.06	0.14	0.10
	(.)	(0.43)	(0.23)	(0.35)	(0.30)
More than 24 months	.	0.27	0.55	0.41	0.47
	(.)	(0.45)	(0.50)	(0.49)	(0.50)
HIV status - DHS test	.	0.13	0.10	0.09	0.10
	(.)	(0.33)	(0.30)	(0.29)	(0.30)
Observations	16312	14959	30195	32040	93506

Note: Means of covariates at individual level, reported by year of survey and for the full sample. The standard deviation is in parentheses.

Table A1.4: Foreign aid-funded programs in Malawi (1997-2017)

Sector	**Geolocation level**	**Number of programs**	**Number of unique locations**
All programs	1 - 8	561	2522
All HIV-related programs	1 - 8	87	304
	1 - 3	38	237
Programs of prevention for HIV	1 - 8	29	141
	1 - 3	17	118

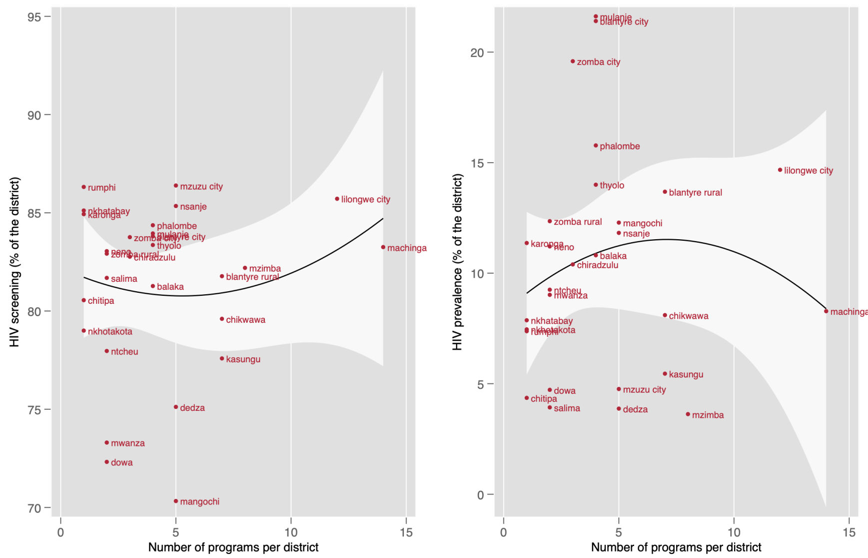
Note: Strandow et al. (2011) describes the geocoding methodology. Geolocation levels 1 to 3 include programs established at the district and traditional authority levels. At level 1, "The coordinates correspond to an exact location, such as a populated place or a physical structure such as a school or health center. This code may also be used for locations that join other locations to create a line, such as a road, power transmission line, or railroad". At level 2, "The location is mentioned in the source as being "near," in the "area" of, or up to 25 km from an exact location. The coordinates refer to that adjacent location". At level 3, "The location is, or is analogous to, a second-order administrative division (ADM2), such as a district, municipality or commune". The location of level 3 of Malawi is the district's capital. At level 4, "The location is, or is analogous to, a first order administrative division (ADM1), such as a province, state or governorate." At level 5, "the location can only be related to estimated coordinates, such as when a location lies between populated places; along rivers, roads and borders; more than 25 km away from a specific location; or when sources refer to parts of a country greater than ADM1 such as a National Park which spans across several provinces (e.g. Foret Classee de Gongon in Benin)". At level 6, "The location can only be related to an independent political entity, meaning the pair of coordinates that represent a country. This includes aid that is intended for country-wide projects as well as larger areas that cannot be geo-referenced at a more precise level." At level 7, the geolocation is "unclear". "The country coordinates are entered to reflect that subcountry information is unavailable." At level 8, "the location is estimated to be a seat of an administrative division (local capital) or the national capital." All programs geolocated at a level between 4 and 8 are established at the regional or national level. All in all, 32 locations were at level 1, 2 at level 2, and 84 at level 3.

Table A1.5: Distribution of programs per year and districts

	2000	**2004**	**2010**	**2016**	**2017**	**Total**
Total number of programs	1	8	54	32	23	118
Number of district receiving programs	1	4	20	11	19	25

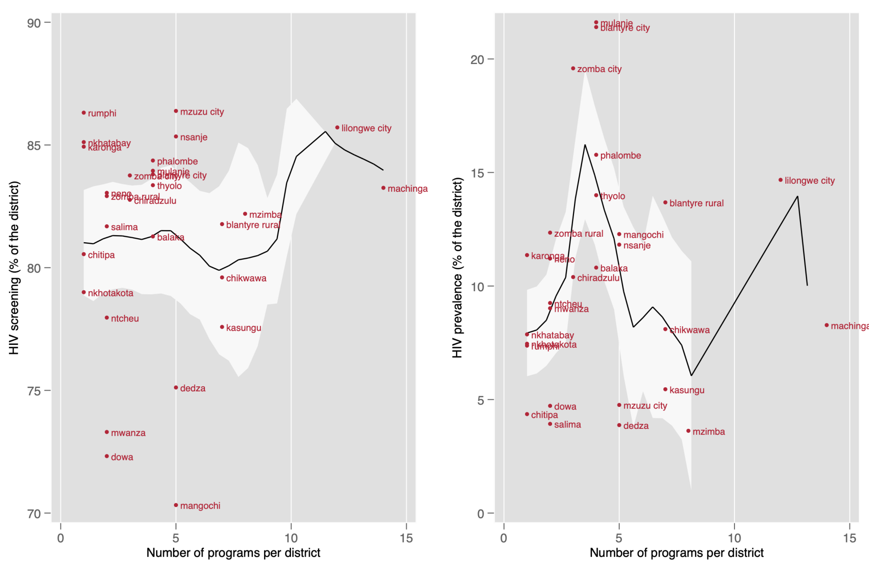
Note: The table shows the number of programs implemented at national and district level, at the mtime of the survey DHS survey from 2004 to 2016. I add those implemented in 2017. The distribution takes into account the projects' starting and ending dates. It also shows the number of districts receiving these programs per year. Malawi has 28 districts. The Northern Region includes the districts of Chitipa, Karonga, Likoma, Mzimba, Nkhata Bay, Rumphi. The Central Region includes the districts of Dedza, Dowa, Kasungu, Lilongwe, Mchinji, Nkhotakota, Ntcheu, Ntchisi, Salima. The Southern Region includes the districts of Balaka, Blantyre, Chikwawa, Chiradzulu, Machinga, Mangochi, Mulanje, Mwanza, Nsanje, Thyolo, Phalombe, Zomba, Neno. The following districts did not receive programs: Likoma, Mchinji, Ntchisi.

Figure A1.3: HIV prevention programs and testing behavior - quadratic regression (district level)



Note: The graph estimates a non-linear correlation at the district level between main outcomes and the number of HIV prevention programs implemented before the survey. The main outcomes are the ratio of people who declared already tested and the HIV prevalence rate.

Figure A1.4: HIV prevention programs and testing behavior - local polynomial regression (district level)



Note: The graph estimates a non-linear correlation at the district level between main outcomes and the number of HIV prevention programs. The main outcomes are the ratio of people who declared they already tested for HIV and the HIV prevalence rate.

Table A1.6: Exposure to HIV prevention programs - Logistic regression

	Testing behavior			HIV status		
	(1)	(2)	(3)	(4)	(5)	(6)
Distance to the nearest HIV-prevention program	−0.01***	−0.02***	−0.00*	−0.00***	0.00***	0.00
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Years of school		0.13***	0.10***		0.03***	0.04***
		(0.003)	(0.004)		(0.007)	(0.007)
Age		0.03***	0.03***		0.07***	0.07***
		(0.001)	(0.001)		(0.002)	(0.002)
Men		−0.61***	−0.74***		−0.63***	−0.64***
		(0.022)	(0.024)		(0.048)	(0.048)
Wealth		0.00***	0.00***		0.00***	0.00***
		(0.000)	(0.000)		(0.000)	(0.000)
Other than single		0.61***	1.01***		−0.30***	−0.31***
		(0.020)	(0.023)		(0.050)	(0.050)
Distance to the nearest health facility		−0.02***	−0.03***		−0.05***	−0.05***
		(0.004)	(0.004)		(0.010)	(0.010)
Catholic		−0.13***	−0.03		−0.04	−0.05
		(0.023)	(0.026)		(0.058)	(0.058)
Control	No	Yes	Yes	No	Yes	Yes
District FE	No	No	Yes	No	No	Yes
Year FE	No	No	Yes	No	No	Yes
Pseudo R-squared	0.10	0.21	0.35	0.00	0.11	0.12
No. of observations	68298	68279	68279	24840	24833	24833

Note: *** p <.01, **p <.05, * p <.1 This table presents the results of logistic regression of testing and HIV status on distance to nearest HIV prevention program. Columns (1) and (4) are a simple correlation between screening behavior and the primary outcomes. Columns (2) and (3) include control variables. Columns (3) and (6) add time-fixed effects. All estimates include controls for age, gender, marital status, wealth, rural/urban, religion, and distance to the nearest health center. Each column includes district fixed effects. All estimates are weighted using DHS sampling weights. The standard errors in parentheses are clustered at the survey cluster.

Table A1.7: Heterogeneity analysis - Intensity of exposure (HIV testing)

	Testing behavior		
	(1) Number of programs	(2) HIV programs	(3) Exposition before first intercourse
Exposed x Number of programs	0.01		
	(0.004)		
To be exposed x Number of programs	0.00***		
	(0.000)		
Exposed	−0.01		
	(0.009)		
To be exposed	0.01		
	(0.007)		
Exposed to any HIV program		0.01	
		(0.007)	
To be exposed to any HIV program		0.01*	
		(0.005)	
Exposed before sexual relationship			−0.07**
			(0.023)
To be exposed			−0.02
			(0.011)
Difference in differences	0.01	−0.00	−0.05**
F-test: active-inactive=0	2.39	0.14	4.37
p-value, F-test	0.12	0.71	0.04
Mean dep. var	0.54	0.54	0.56
R-squared	0.45	0.45	0.49
No. of observations	92310	92310	20161

Note: *** p <.01, **p <.05, * p <.1 The table shows the results of the heterogeneous analysis according to the degree of exposure intensity for HIV testing. Three categories of exposure intensity are distinguished: the number of programs (column 1); whether or not exposed before the first sex (column 2); and exposure to any HIV program beyond prevention programs (column 3).All estimates include controls for age, gender, marital status, wealth, rural/urban and distance to the nearest health center. They include year and district fixed effects. All estimates are weighted using DHS sampling weights. The standard errors in parentheses are clustered at the survey cluster. The "Difference-in-Differences" indicates the difference between "Exposed" and "To be Exposed". The F-test and the p-value of the F-test are presented in the bottom section.

Table A1.8: Heterogeneity analysis - Intensity of exposure (HIV status)

	HIV status		
	(1) Number of programs	(2) HIV programs	(3) Exposition before first intercourse
Exposed x Number of programs	0.01		
	(0.003)		
To be exposed x Number of programs	0.00		
	(0.002)		
Exposed	−0.02		
	(0.012)		
To be exposed	0.01		
	(0.011)		
Exposed to any HIV program		0.00	
		(0.007)	
To be exposed to any HIV program		0.02	
		(0.013)	
Exposed before sexual relationship			−0.01
			(0.017)
To be exposed			0.02
			(0.010)
Difference in differences	0.01	−0.02	−0.03
F-test: active-inactive=0	1.65	2.74	1.76
p-value, F-test	0.20	0.10	0.19
Mean dep. var	0.10	0.10	0.12
R-squared	0.10	0.10	0.09
No. of observations	33167	33167	20961

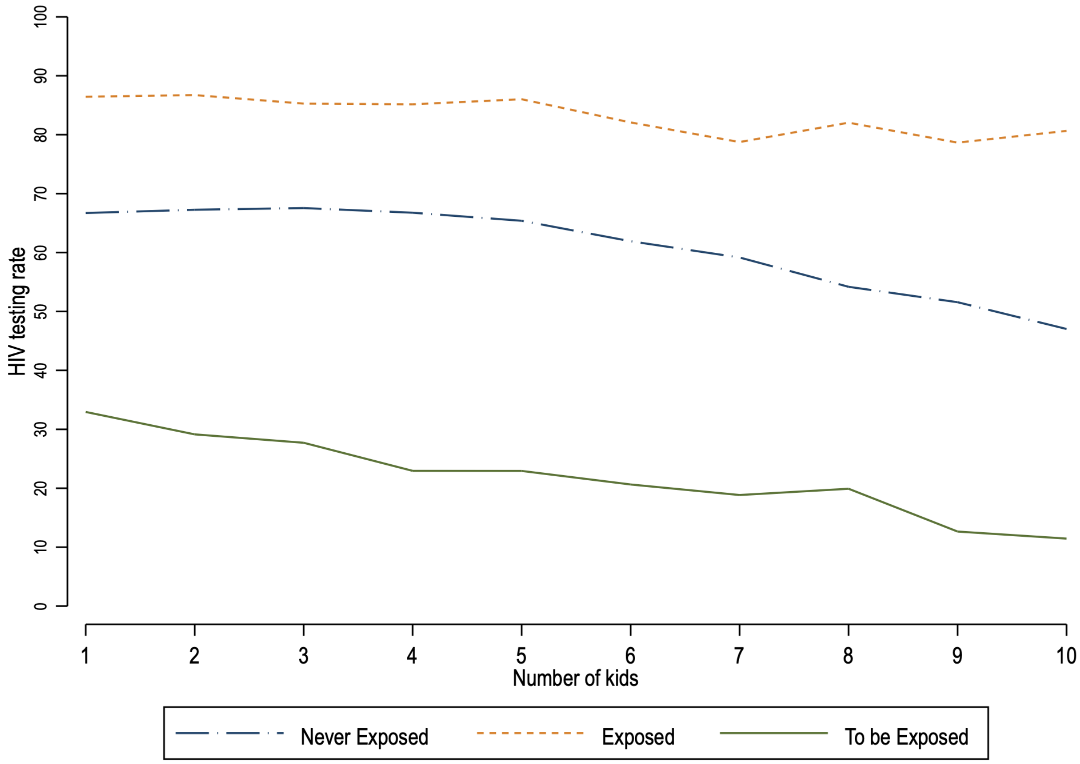
Note: *** p <.01, **p <.05, * p <.1 The table shows the results of the heterogeneous analysis according to the degree of exposure intensity for HIV status. Three categories of exposure intensity are distinguished: the number of programs (column 1); whether or not exposed before the first sex (column 2); and exposure to any HIV program beyond prevention programs (column 3). All estimates include controls for age, gender, marital status, wealth, rural/urban and distance to the nearest health center. They include year and district fixed effects. All estimates are weighted using DHS sampling weights. The standard errors in parentheses are clustered at the survey cluster. The "Difference-in-Differences" indicates the difference between "Exposed" and "To be Exposed". The F-test and the p-value of the F-test are presented in the bottom section.

Table A1.9: Heterogeneity analysis - Gender

	Testing behavior		HIV status	
	(1) Full sample	(2) Restricted sample	(3) Full sample	(4) Restricted sample
Exposed	0.02**	0.03	0.00	−0.02
	(0.007)	(0.018)	(0.010)	(0.019)
To be exposed	0.01	0.02	0.02	0.05
	(0.007)	(0.015)	(0.014)	(0.033)
Gender	−0.06***	0.04***	−0.02***	−0.04***
	(0.006)	(0.010)	(0.004)	(0.011)
Exposed x Gender (Male)	−0.07***	−0.05*	−0.01	0.02
	(0.014)	(0.023)	(0.010)	(0.021)
To be exposed x Gender (Male)	0.10***	−0.02	−0.01	−0.05
	(0.014)	(0.018)	(0.018)	(0.036)
Difference in differences	−0.17	−0.03	0.00	0.07
F-test: active-inactive=0	81.97	1.28	0.00	3.30
p-value, F-test	0.00	0.26	1.00	0.07
Mean dep. var	0.54	0.40	0.63	0.48
R-squared	0.45	0.27	0.10	0.08
No. of observations	92310	18662	33167	9336

Note: The table presents the results of the heterogeneous analysis by gender. *** p <.01, **p <.05, * p <.1 This estimation includes interaction term between the treatment and the gender to assess heterogeneous effect by gender. It controls for age, marital status, wealth, rural/urban, and distance to the nearest health center. It includes year and district fixed effects and is weighted using DHS sampling weights. Columns (1) and (3) include all respondents. Columns (2) and (4) are restricted to men and women who ever had sexual intercourse and excluded women who were pregnant at least once to control for the free testing policy for mothers. The standard errors in parentheses are clustered at the survey cluster. The "Difference-in-Differences" indicates the difference between "Exposed" and "To be Exposed". The F-test and the p-value of the F-test are presented in the bottom section.

Figure A1.5: Robustness - Parallel trend



Note: This figure represents HIV testing rate among three groups: *Never Exposed*, *Exposed* and *TobeExposed*. The sample is restricted to women who are eligible to mandatory and free HIV screening during their pregnancy, since 2003. The outcome is plotted against the group of number of births given.

Table A1.10: Robustness - Entropy balancing

Variables	**Treatment mean**	**Control mean (unweighted)**	**Standardized difference (before)**	**Control mean (weighted)**	**Standardized difference (after)**
Gender of respondent	0.23	0.22	0.02	0.23	0.00
Respondent's current age	28.80	28.10	0.07	28.80	0.00
Education in single years	6.33	5.31	0.26	6.33	0.00
Rural	0.66	0.84	−0.38	0.66	0.00
Distance to the first health facility	3.65	3.86	−0.07	3.65	0.00
Wealth index	52997	7515	0.32	52960	0.00
Total births	2.77	2.92	−0.06	2.77	0.00
Religion (being catholic)	0.19	0.21	−0.05	0.19	0.00

Note: The table shows the result of matching treatment and comparison groups' observations on their propensity scores. The entropy balancing strategy builds a new weighting, adjusting inequalities in representation with respect to the first and second moments of the covariate distributions. The table displays the standardized differences between the two groups before and after applying the entropy balancing weighting.

Table A1.11: Robustness - Date of the last test

	Testing behavior		HIV status	
	Got tested		Blood test result (DHS)	
	(1)	(2)	(3)	(4)
Exposed (precision 1)	0.11***	0.03**	−0.02	−0.02
	(0.009)	(0.011)	(0.011)	(0.013)
To be exposed (precision 1)	−0.34***	0.04**	0.02	0.01
	(0.009)	(0.012)	(0.016)	(0.019)
Control	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	Yes
Difference in differences	0.45***	−0.01	−0.04**	−0.02
F-test: active-inactive=0	1663.75	0.51	4.67	1.39
p-value, F-test	0.00	0.48	0.03	0.24
Mean dep. var	0.54	0.54	0.09	0.09
R-squared	0.16	0.45	0.10	0.10
No. of observations	70182	70182	25875	25875

Note: *** p <.01, **p <.05, * p <.1 This table presents the impact of exposure to HIV prevention program on screening (columns 1 and 2) and HIV status (columns 3 and 4). Contrary to the model presented in equation 1.1, the sample is restricted to people *Exposed* and *TobeExposed* within a year before and after the survey. Additionally, people are said *Exposed* if they live in a 5/10km buffer around a program implemented before their last test (and not the interview date). People living in a 5/10km buffer of a program starting after their testing date (or interview date by default) are in the group *TobeExposed*. People living further than a 5/10km buffer around the HIV prevention program are in the control group. The last date is estimated based on the date of the interview and the respondent's answer to: "When was your last test: a year ago? between 12 and 24 months? more than two years ago?". I take the previous year's date for those who answered "a year ago". I take the date 18 months ago for those who answered "between 12 and 24 months". I take the date two years ago for those who answered "more than 2 years ago". All estimates include controls for age, gender, marital status, wealth, rural/urban, religion, and distance to the nearest health center. They include year and district fixed effects. All estimates are weighted using DHS sampling weights. The standard errors in parentheses are clustered at the level of the survey's clusters. The main outcome is in the bottom part of the table, named "Difference-in-Differences." It indicates the difference between the coefficients "Exposed" and "To be Exposed." The F-test and the p-value of the F-test are presented in the bottom section.

Table A1.12: Robustness - Distance to HIV prevention program

	Testing behavior														
	(1) 11km	(2) 12km	(3) 13km	(4) 14km	(5) 15km	(6) 16km	(7) 17km	(8) 18km	(9) 19km	(10) 20km	(11) 21km	(12) 22km	(13) 23km	(14) 24km	(15) 25km
Exposed	−0.00	0.00	0.00	0.00	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
To be exposed	0.03***	0.03***	0.03***	0.02**	0.03***	0.02***	0.02***	0.02***	0.02***	0.02**	0.01*	0.01*	0.01	0.01*	0.01*
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Difference in differences	−0.03***	−0.03***	−0.02***	−0.02***	−0.02***	−0.02**	−0.01**	−0.01**	−0.01*	−0.01	−0.01	−0.01	−0.01	−0.01	−0.00
F-test: active-inactive=0	14.47	10.16	7.06	6.67	6.23	5.30	4.38	4.15	3.33	2.13	0.70	1.08	0.70	1.06	0.47
p-value, F-test	0.00	0.00	0.01	0.01	0.01	0.02	0.04	0.04	0.07	0.14	0.40	0.30	0.40	0.30	0.50
Mean dep. var	0.54	0.54	0.54	0.54	0.54	0.54	0.54	0.54	0.54	0.54	0.54	0.54	0.54	0.54	0.54
R-squared	0.36	0.36	0.36	0.36	0.36	0.36	0.36	0.36	0.36	0.36	0.36	0.36	0.36	0.36	0.36
No. of observations	93048	93048	93048	93048	93048	93048	93048	93048	93048	93048	93048	93048	93048	93048	93048

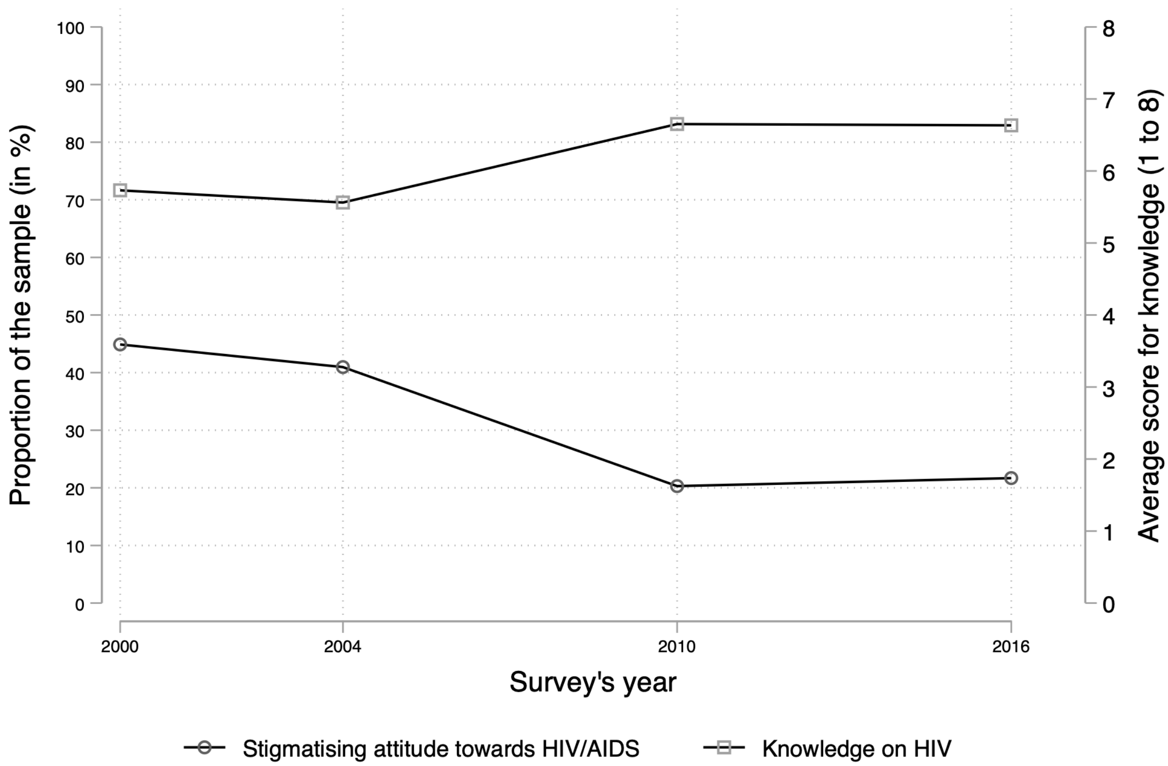
Note: The table replicates the main double-difference regression by expanding the distance bandwidth by 1km up to 25km. The results are robust up to and including 19km. *** p <.01, **p <.05, * p <.1 All estimates include controls for age, gender, marital status, wealth, rural/urban, religion and distance to the nearest health center. They include year and district fixed effects. All estimates are weighted using DHS sampling weights. The standard errors in parentheses are clustered at the survey cluster. The “Difference-in-Differences” indicates the difference between “Exposed” and “To be Exposed”. The F-test and the p-value of the F-test are presented in the bottom section.

Table A1.13: Robustness - Survey wave, Sample weighting and completion date

	Testing behavior			HIV status		
	(1) Survey 2000	(2) Sampling weight	(3) Completion date	(4) Survey 2000	(5) Sampling weight	(6) Completion date
Exposed	0.01	0.01*		−0.00	0.00	
	(0.007)	(0.005)		(0.008)	(0.006)	
To be exposed	0.03**	0.03***		0.01	0.02	
	(0.012)	(0.006)		(0.011)	(0.010)	
Exposed (completion date)			−0.01			−0.01
			(0.007)			(0.008)
To be exposed (completion date)			0.03***			0.02
			(0.007)			(0.009)
Difference in differences	−0.03	−0.01	−0.03	−0.02	−0.01	−0.03
F-test: active-inactive=0	4.73	3.06	9.82	1.83	1.45	3.36
p-value, F-test	0.03	0.08	0.00	0.18	0.23	0.07
Mean dep. var	0.64	0.54	0.54	0.63	0.63	0.63
R-squared	0.38	0.45	0.45	0.10	0.09	0.10
No. of observations	76160	92310	92310	33167	33167	33167

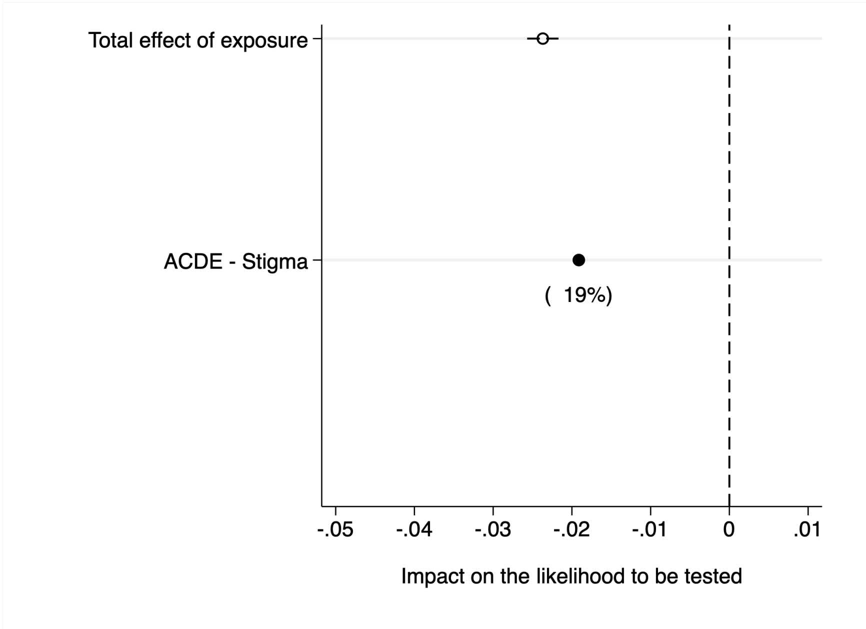
Note: *** p ¡.01, **p ¡.05, * p ¡.1 This table presents the impact of exposure to HIV prevention program on screening. All estimates include controls for age, gender, marital status, wealth, rural/urban, religion, and distance to the nearest health center. They include year and district fixed effects. All estimates are weighted using DHS sampling weights. The standard errors in parentheses are clustered at the level of the survey's clusters. The main outcome is in the bottom part of the table, named "Difference-in-Differences." It indicates the difference between the coefficients "Exposed" and "To be Exposed." The F-test and the p-value of the F-test are presented in the bottom section.
Columns (1) and (4) exclude the 2000 survey wave from the sample. Columns (2) and (5) do not use the sampling weight. Columns (3) and (6) include the end of the program in the regression. In other words, it is possible for an individual whose region was $Exposed$ to be $TobeExposed$ if the program ended before he or she was surveyed.

Figure A1.6: Knowledge level and stigmatizing attitude from 2000 to 2016



Note: The graph represents the trend of the knowledge level and stigmatizing attitudes over the years from 2000 to 2016 (DHS surveys). The stigmatizing attitude is a dummy taking the value 0 for those not declaring any stigmatizing attitude and 1 for those declaring at least one attitude stigmatizing seropositive people. See further information on table A1.15. The HIV knowledge score is rated from 0 to 8 based on the answer given to a list of questions on HIV.

Figure A1.7: Mediation analysis for stigma



Note: The graph presents the coefficient estimates at 90% confidence intervals of the impact of exposure to HIV prevention programs on the likelihood of getting tested. The total effect without mediation is reported in the coefficient "Total effect of exposure". The "ACDE - Stigma" coefficient says the Average Controlled Direct Effect (ACDE). I consider only one mediator here: stigma.

Table A1.14: Questions in DHS surveys - Knowledge on HIV

	Survey's year			
	2000	2004	2010	2016
Can people get the AIDS virus from mosquito bites?	x	x	x	x
Can people get the AIDS virus by sharing food with a person who has AIDS?	x	x	x	x
Is it possible for a healthy-looking person to have the AIDS virus?	x	x	x	x
Can the virus that causes AIDS be transmitted from a mother to her baby: During pregnancy?	x	x	x	x
Can the virus that causes AIDS be transmitted from a mother to her baby: During delivery?	x	x	x	x
Can the virus that causes AIDS be transmitted from a mother to her baby: By breastfeeding?	x	x	x	x
Can people get the AIDS virus because of witchcraft or other supernatural means?		x	x	x
Are there any special medications that a doctor or a nurse can give to a woman infected with the AIDS virus to reduce the risk of transmission to the baby?		x	x	x

Note: This table lists questions on HIV raised in DHS from 2000 to 2016. I keep questions raised at least in 3 different surveys to build an index equal to 8 when the respondent gets all answers and 0 when the respondent answers incorrectly to all of them.

Table A1.15: Questions in DHS surveys - Attitude towards HIV

Year of survey	Questions
2016	Would you buy fresh vegetables from a shopkeeper or vendor if you knew that this person had HIV?
	Do you think children living with HIV should be allowed to attend school with children who do not have HIV?
	Do you fear that you could get HIV if you come into contact with the saliva of a person living with HIV?
2010	Would you buy fresh vegetables from a shopkeeper or vendor if you knew that this person had the AIDS virus?
	In your opinion, if a female teacher has the AIDS virus but is not sick, should she be allowed to continue teaching in the school ?
2004	Would you buy fresh vegetables from a shopkeeper or vendor if you knew that this person had the AIDS virus?
	In your opinion, if a female teacher has the AIDS virus but is not sick, should she be allowed to continue teaching in the school ?
	Should persons with the AIDS virus who work with other persons such as in a shop, office, or farm be allowed to continue their work or not?
2000	Should persons with the AIDS virus who work with other persons such as in a shop, office, or farm be allowed to continue their work or not?

Note: This table presents the questions about the attitude towards HIV in the successive DHS surveys. The variable is a dummy equal to 0 (no answer denoting negative attitude towards HIV) or 1 (at least one answer denoting negative attitude toward HIV). Those questions are part of the DHS section entitled "HIV and other sexually transmitted infections." The latter includes other questions relative to feelings towards stigma that I exclude to build the variable on stigma. Stigma is a three-dimensional social and individual reality, but I only include those leading to enacted stigma. The enacted stigma "results when clandestine hostility and/or overt acts of discrimination are directed towards persons specifically because they possess the stigmatized attribute" (Tsai et al. (2013), Allport et al. (1954)). The internalized stigma "results when stigmatized persons come to accept these inhospitable attitudes as valid, thereby developing self-defacing beliefs and perceptions about themselves" (Tsai et al., 2013). The anticipation of stigma names people's expectations of their community's attitude against HIV-positive or assumed HIV-positive individuals. Thus, I select questions on enacted stigma because it is expected that they will be the ones that most closely reflect the translation of the feeling of stigma into real action and because they were repeated in all four waves of the survey. The questions related to the anticipation of stigma are not used since they were only asked in 2016.

Table A1.16: Description Foreign aid-funded HIV prevention programs in Malawi (1997-2017)

Name of the project	**Donor**	**Locations (district)**
Behaviours adopted that reduce fertility and risk of HIV/AIDS	USAID	Blantyre, Lilongwe, Mzimba, Zomba
Community based family planning (FP) and HIV/Aids services	USAID	Balaka, Chikwawa, Karonga, Kasungu, Mangochi, Nkhotakota, Phalombe, Salima
District-level implementation of the Malawi HoH PMTCT Programme	Norwegian Agency for Development Cooperation	Machinga, Mangochi
Extending Quality Improvement for HIV/AIDS in Malawi (EQUIP)	USAID	Lilongwe
HIV Prevention Communication	Canadian International Development Agency	Chikwawa, Kasungu, Mzimba
HIV Prevention for out of sch Adol & Yth	Federal Republic of Germany & Global Fund	Blantyre, Chikwawa, Dedza, Lilongwe, Mzimba
HIV Prevention in Sch Adolescents & Youth	United Nations Children's Fund	Balaka, Dedza, Lilongwe, Mzimba, Nsanje
JHU-BRIDGE	USAID	Blantyre, Chikwawa, Chiradzulu, Machinga, Mulanje, Mwanza, Neno, Nsanje, Phalombe, Thyolo, Zomba
Norwegian Church Aid-health training	Norwegian Agency for Development Cooperation	Blantyre, Chiradzulu, Kasungu, Lilongwe, Mulanje, Mzimba, Nsanje, Thyolo, Zomba
PMTCT and Paediatric AIDS	Canada	Balaka, Chikwawa, Chitipa, Dedza, Ksungu, Lilongwe, Mzimba, Nsanje, Ntcheu, Thyolo
Project Hope Malawi	USAID	Mulanje, Phalombe
Promote Normative Change	USAID	Balaka, Blantyre, Chikwawa, Chiradzulu, Kasungu, Machinga, Mangochi, Mulanje, Mwanza, Mzimba, Neno, Nsanje, Ntcheu, Phalombe, Salima, Thyolo, Zomba
Safeguarding Young People Programme - New	The European Union & Swiss Development Cooperation	Blantyre, Dowa
Safe Motherhood Project (TC)	UK Department for International Development	Chikwawa, Dedza, Mangochi, Nkhata Bay
Strengthening the Delivery, Coordination, and Monitoring of HIV Services in Malawi through Faith-Based Institutions	Centers for Disease Control & Prevention	Blantyre, Dowa, Kasungu, Mzimba
Strengthening expanded HIV/AIDS Counseling & Testing services in Malawi	Centers for Disease Control & Prevention	Blantyre, Kasungu, Mzimba
Strengthening expanded HIV/AIDS Counseling & Testing services in Malawi (MACRO)	Centers for Disease Control & Prevention	Blantyre, Lilongwe, Mangochi, Rumphi

Note: This table presents the name, the donor and the location of HIV prevention programs listed in the databases of Peratsakis et al. (2012) and the Ministry of Finance.