# Abstract

Female sex workers (FSW) face structural barriers to HIV-service access, however the effect of their mobility is uncertain. Using cross-sectional data from 2,839 FSW in 14 sites in Zimbabwe, we explored the association between mobility (number of trips, distance, duration) in the past 12 months and five HIV-service access outcomes: exposure to community mobilisation, clinic attendance, HIV testing, antiretroviral treatment initiation, and viral suppression (<1000 copies per mL). We used modified-Poisson regression, and natural effects models to estimate how trip frequency was mediated by distance and duration away. Each additional trip in 12 months was associated with increased community-mobilisation-event attendance (adjusted RR: 1.08, 95% CI: 1.04-1.12) and attending clinic two-or-more times (adjusted RR: 1.02, 95% CI: 1.00-1.05). There was little evidence of any other associations, or of mediation. Our findings are consistent with literature that found the effects of mobility to vary by context and outcome. This is the first study to consider many FSW-mobility and HIV-service-access measures together. Future research on mobility and health-related behaviour should use a spectrum of measures.

*Keywords*: Sex work; migration; mobility; healthcare; HIV

# Background

Female sex workers everywhere are at higher risk of acquiring HIV [1] and other STIs than women of similar age, while also having high burdens of mental health problems [2], alcohol and substance misuse [3], and physical and sexual violence [4]. Suitable, non-stigmatising, healthcare services can help alleviate multiple health burdens; however, for sex workers, access to services is often insufficient [5]. Also, HIV-positive female sex workers who know their status and are supplied with medication and social support can suppress their viral load and prevent onward transmission [6]. Linking HIV-negative female sex workers to effective, acceptable, and appropriate prevention technologies with support can also reduce the number of new infections due to sex work [7].

While there are little empirical data from representative samples, there are reasons to suspect that sex workers may be more mobile than women of similar age. Selling sex can require moving to find clients and those willing to pay higher prices in response to fluctuation in the number of men and their disposable income [8,9], or simply because clients may be more likely to hire women they have not seen before, at a higher price. Sex work is rarely protected by minimum wage legislation, therefore over-supply and competition may put pressure on prices, precipitating further movement. The most sex is sold in places with the most clients (e.g. cities, mines, army bases, truck stops), therefore sex workers born in small towns and rural areas may live away from their family home and may, like other rural-to-urban migrants, travel to visit family [8]. Sex workers often face stigma, for example by family members, and may move to avoid identification [10,11]. They are also often harassed, physically assaulted, and raped by police, clients, partners, and pimps [12], and moving may be used to escape dangerous contexts [9].

There is evidence that mobility can disrupt treatment regimes for people who live with HIV [13]. However, a recent systematic review of mobility and healthcare use among sex workers found mixed results, with mobility strongly associated with healthcare barriers in some contexts (e.g. an adjusted odds ratio for mobility and ART interruption of 5.19 [14]), but weakly or not associated in most [15]. There was broadly consistent evidence that mobility increased barriers to general contact with the healthcare system [16–18], but the one study that looked at HIV-testing and counselling (HTC) found no evidence of an association between testing uptake and having worked overseas or in other provinces (the study’s mobility measure) [19]. Five studies reported the association between aspects of treatment and mobility: one found no association between mobility and ART initiation [20], which was the only study conducted in Africa. Although two studies found mobility was associated with ART interruption [14,21], two others found no association with viral load [22,23]. The inconclusiveness of the evidence may be partly due to the use of single binary measures of mobility, leading to coarse-grained results and ruling-out dose-response analysis. Three studies asked about mobility at any time in the past, making the association with healthcare access measured over 6 to 12 months imprecise and sometimes implausible. In most of the studies, the measure of mobility could also indicate history of migration, which although related to mobility is a different concept [24] with broader implications for healthcare access [26], such as legal restrictions. Also, none of the studies investigated more than one measure of healthcare access on the treatment and prevention ‘cascades’ [27,28].

Programming for sex workers is often supported by activities needed to overcome structural barriers [29], which presents additional opportunities for mobility to influence access to healthcare. For example, many programmes train peer educators to work through social networks [30], and mobility may affect social engagement and accessibility for peer education. However, since mobility may help sex workers escape certain barriers, it could improve healthcare access; for example, travel to places where women are not identified as sex workers may reduce sex-work-related stigma [11]. Increasing the variety of places visited may increase exposure to better, accessible, services, or social networks with more pro-active health-seeking norms. Understanding and mitigating any negative effects of sex-worker mobility may help achieve health equity and HIV treatment goals such as 90:90:90 [31], and explore the challenges mobility may pose for universal testing and treatment [32].

The aim of this paper is to estimate the association between mobility and healthcare access and use among female sex workers in Zimbabwe. At the time of data collection, Zimbabwe had a network of female-sex-worker friendly services and rapidly improving government health system. While Zimbabwe was unusual for having national coverage of sex-worker-friendly clinics providing STI treatment, HTC and referral, and condom provision, we hypothesised that higher levels of mobility would be associated with lower HIV-related healthcare access and use, and that the associations would vary by the type of mobility.

# Methods

The data were collected in 2016 in 14 sites in Zimbabwe as part of a cluster-randomised controlled trial called the ‘SAPPH-IRe’ trial [33]. Nationally, HIV treatment was provided by government-run clinics to which women were referred from outreach and testing programmes. The government-run records systems did not allow records to be temporarily transferred between sites, and therefore did not offer continuity if women moved for short periods. Female-sex-worker-specific sexual-health services were available at all sites as part of the *Sisters with a Voice* programme (henceforth ‘*Sisters*’). The *Sisters* programme had been running since 2009, and in 2016 was operating in 36 sites where approximately 75-85% of Zimbabwean sex-workers were expected to live, and where the programme saw 18,539 women that year. The *Sisters* programme used a unique identifier that could link records between sites so that women attending in different places could have continuity, if they chose. The *Sisters* programme did not offer on-site ART initiation; however, during the trial a large international NGO was offering pre-exposure prophylaxis (PrEP) and antiretroviral treatment initiation and support in the same location as the *Sisters* programme in the seven sites in the intervention arm of the trial.

The 14 trial sites were were purposively selected from the 36 *Sisters* sites, spaced at least 90 kilometres apart. The 14 sites included district capitals, farming towns, mining towns, trucking halts, a colliery, and an army base. A representative sample of approximately 200 sex workers was recruited through respondent-driven sampling (RDS) [34] in each site, interviewed and tested for HIV, and, if living with HIV, for viral load. The interviews took place in a private setting after written consent was given. Blood samples were air-dried on filter papers and tested using AniLabsytems EIA kit (AniLabsystems Ltd, OyToilette 3, FIN-01720, Finland). If HIV antibodies were detected then the sample was tested for HIV viral load using NucliSENS EasyQ HIV-1 v2.0; this confirmed HIV positive status and quantified the viral load. Samples with a positive HIV antibody test using Anilab EIA, but an undetectable viral load, were tested with a confirmatory ELISA (Enzygnost Anti-HIV 1/2 Plus ELISA, Germany).

Women were asked about up to five places that they had visited and worked in sex work in the past 12 months, and up to five places where they visited but did not work. They were asked to recall details about each place in reverse chronological order for each type. We did not ask whether they returned to the place of interview between visits. Therefore, because women may have travelled between visited locations, we calculated the median length of all of the possible routes for each type of up to five journeys. This technique has been shown to reduce positive outliers (i.e. the most likely to be overestimates) without substantially changing the overall median distances [8]. Mobility was incidental to the primary purpose of the SAPPH-IRe trial, and it was unclear what answers would be socially desirable; however, we were unable to verify the validity of the self-reported measures. We quantified mobility in terms of the number of trips made, the total number of days away, the total distance travelled. We also qualified the journeys, by the motivation for moving (sex work / not sex work); whether the journey crossed borders; whether a *Sisters* clinic was within 5km (journeys in Zimbabwe only); whether healthcare was used during the visit; whether travel was to a different major linguistic area of Zimbabwe, and whether women worked in sex work during the visit.

We collected data on a number of healthcare-use outcomes in the past 12 months. Although not always considered part of healthcare services, we included questions about engagement with community mobilisation because this was a common and evidence-supported auxiliary service for healthcare interventions targeting female sex workers [29]. We asked about the number of attendances at the *Sisters* clinics, dichotomising the number of visits into fewer than two times or two or more times in the last year. We measured whether participants living with HIV knew their status, and if HIV-negative women knew their status and had tested in the past 3 months. All women were asked about taking PrEP (‘Are you currently taking PrEP to prevent HIV?’), and included only HIV-negative women living in the seven intervention-arm sites where PrEP was available in the denominator in the analysis. Women who reported being HIV-positive were asked if they ever started taking ART. Viral suppression was used as a measure of treatment adherence among women who reported ever starting treatment; a ‘suppressed’ viral load was defined as having fewer than 1000 copies per mL of blood as per WHO cut-off guidance for low-and-middle-income countries [35]. All of the healthcare-use measures except viral load were subject to potential social desirability bias.

Women were asked their age (categorised as 18-19 years, 20-24, 25-29, 30-39, $\geq $ 40), highest level of education completed (no formal education, primary, secondary), marital status (married, divorced, widowed, never married), the number of children they supported, and how long ago they started sex work (categorised as 0-2 years, 3-6, $\geq $ 7). To screen for depression, we used the Patient Health Questionnaire for depression (PHQ-9) with a cut-off of $\geq $ 11 that was based on recent validation of the PHQ-9 in Zimbabwe [36]. We defined ‘substance misuse’ as reporting drinking alcohol four or more times a week, or taking drugs to get high any time in the past 12 months. We defined ‘good’ social support as reporting ‘good’ or ‘very good’ relations with other sex workers at site of interview and agreeing or strongly agreeing that colleagues would provide support if one refused to have sex with a client without a condom or if a client became aggressive or violent. Women were coded as food insecure if they reported that in the past four weeks there was ever no food to eat of any kind in the house because of lack of resources to get food. Women were asked if they had a cellphone and how much of their livelihood was earned through sex work, which we coded as ‘all’ if sex work was the only source, and ‘not all’ if any other source of income was available. We defined women as having experienced sex-worker-related stigma if she agreed or strongly agreed that because of work as a sex worker she has lost respect or standing in the community, thinks less of herself, or has felt ashamed; or, alternatively if she answered ‘a few times’ or ‘often’ in response to any of the scenarios in, ‘because of work as a sex worker: people have talked badly about me or healthcare workers talked badly about me or I have been denied health services or I have been verbally insulted, harassed and/or, threatened, or I have felt that people have not wanted to sit next to me, for example on public transport, at church or in a waiting room’. We asked about experience of violence from regular clients and intimate partners, and sexual assault from the police, and coded women as having recently experienced violence if they reported that a husband, boyfriend, sexual partner, client, or police had hit, slapped, pushed, shoved, or otherwise physically hurt her in past month, or if she had been raped by police in past month.

## Statistical analysis

We estimated the risk ratios (RR) associated with linear changes in the mobility measures and the healthcare-use measures using Poisson regression with robust standard errors [37]. We scaled the duration-away RR to correspond to the effect of each additional 30 days away, and distance-travelled RR to correspond to the effect of each additional 100km travelled. To explore potential non-linearity, we categorised the exposures into three or more categories and fitted these as factors. The number of trips was categorised as zero (baseline), one, two, and three or more; the total duration away as none (baseline), 1-14 days, 15-30 days, and 31 or more days, and distance as zero kilometres (baseline), 1-99, 100-499, and 500 or more kilometres. Since the survey-weighted models do not produce a true likelihood, we could not use likelihood-ratio tests to compare the models with categorical exposures to models with the exposures modelled as linear. Instead we inspected the results visually, using a novel method for presenting many ordered categorical results simultaneously, based on Edward Tufte’s ‘sparklines’ [38]. Solid lines connected the point-estimates for each level of each exposure, with the 95% confidence intervals as grey-shaded polygons. To account for the multi-site RDS, all analyses included fixed-effects for site (coefficients not reported) and were RDS-II-weighted [39] using the inverse of the site-normalised imputed visibility [40]. We used the *survey* package [41] in R version 3.5.1 (2018-07-02) [42], which allows sandwich standard errors to be used with the survey weights needed to account for the RDS design.

In multivariate analysis we adjusted for covariates selected using a directed-acyclical graph (DAG, see **Appendix 2**) to help avoid over-adjustment by more covariates than necessary (reducing the degrees of freedom) or ‘colliders’ that can bias the effect estimate [43]. None of the potential confounders were assumed to be on the causal pathways between the exposure and the outcome. Conditional independence for the mobility exposures and healthcare outcomes required adjustment for age (as continuous), education, marital status, years in sex work, number of children supported, food insecurity, whether owns a cellphone, only has sex work for income, is living with HIV, has screened positive for depression, has experienced sex-work-related stigma, has experienced violence, has misued substances, and reports having available social support.

To explore the independent effects of the different aspects of mobility, we used mediation analysis. We assumed that the number of trips increased the duration away and the total distance travelled, and that the distance travelled increased the duration away. To simplify, we restricted the analysis to women who made at least one journey, otherwise the distribution of the distance travelled had many zeros and would have required more complex modeling, for example ‘hurdle’ models [45]. We explored mediation for all outcomes, not only for models with evidence of effect since mediated effects can act in opposite directions and cancel-out [46]. The outcome *Y* under different nested counterfactual regimes can be denoted as $Y\left(a,M\_{1}\left(a'\right),M\_{2}\left(a',M\_{1}\left(a″\right)\right)\right)$, where *a*, $a'$, and $a″$ denote levels of the exposure *A*, e.g. 0 and 1 for a binary exposure, and $M\_{1}(x)$ and $M\_{2}(x)$ denote the values the mediating variables take when up-stream causal variables are set to *x*. With two mediators that are causally related there are 32 possible decompositions of the causal effect [47]. We focused on the three ‘natural’ effects that sum to the total effect: (1) the direct effect $A\rightarrow Y$ of the number of visits independent of distance or duration (2) the effect through distance $A\rightarrow M\_{1}Y$ including onward effects through duration, and (3) the effect through duration $A\rightarrow M\_{2}\rightarrow Y$ not due to effects on distance [47,48].

We used ‘natural effect models’ that allow for multiple mediators, introduced by Steen *et al* 2017 [49]. The approach requires one of the mediators to be modelled; we chose to model the natural log of the total distance travelled because the distribution among women who had travelled approximated a normal distribution, while the total duration away could not be transformed to resemble a common distribution. Following the steps in Steen *et al*, we generated three working models: a linear model for the log distance (mediator 1) conditional on the number of trips, a negative binomial model of the number of trips (exposure), and a modified-Poisson model with RDS-II weighting for the healthcare outcomes conditional on the exposure and both mediators. Confounding variables and site-level dummy variables were included in all three models. We predicted the 10% and 90% percentile number of trips for each individual conditional on the confounders using the negative-binomial distribution estimated by the working model for the number of trips. The dataset was ‘expanded’, i.e. duplicated nine times, and we added the auxiliary variables *a*, $a'$, and $a″$ for the levels of the exposure within the nested counterfactuals, with each taking the observed exposure level or one of the predicted quantiles according to the configuration described in Steen *et al*. We calculated the weights for the natural effects model using the working model for the log distance (mediator 1), and updated these with the working model for the number of trips (exposure), conditional on the confounders, so the weights could be used to estimate population-average effects in the natural effects model. The working model for the outcome was used to impute the nested counterfactuals when the exposure was set to the value of *a* for each row of the expanded dataset. After fitting the natural effects model using a weighted-regression of the imputed outcomes on *a*, $a'$, $a″$, and the confounding variables, we linearly combined the terms to estimate the natural direct effect $A\rightarrow Y$, the natural indirect effect with respect to the log distance $A\rightarrow M\_{1}Y$, and the natural indirect effect with respect to the total duration away $A\rightarrow M\_{2}\rightarrow Y$. We used 500 bootstrap resamples for the entire procedure to estimate the (standard normal) 95% confidence intervals while accounting for uncertainty in the estimation of the working models.

The planned analysis included many effect estimates: 10 exposures with 7 outcomes analysed as continuous and as categorical, bivariate and multivariate, plus mediation analysis. The exposures were conceptually related and the outcomes were likely to be highly correlated with one another. Therefore, simple adjustments to the statistical tests or to the width of the confidence intervals would be conservative and possibly misleading [50]. Rather than emphasise the size of any one of the *p*-values, or even any one of the effect estimates, we planned to qualitatively jointly interpret the results. Our analysis was pre-specified in a statistical analysis plan. We had planned to look at the effects of mobility on status awareness in all women but subsequently decided that we should present the results separately by HIV status; we also specified that effects on viral suppression would be measured in all women living with HIV, but later restricted the analysis to women who reported taking ART so as to more precisely estimate effects on adherence. Based on our overall hypothesis, we expected to see the following relationships with the measures of mobility:

1. stronger negative associations with viral suppression among HIV-positive women, because adherence to ART requires daily action to avoid a rebound in viral load
2. stronger negative associations for journeys that included or were motivated by sex work, because these may be more disruptive and spent in less secure locations
3. no (or positive) association for visits where healthcare was used
4. stronger negative associations for visits at places without a *Sisters* site within 5km
5. stronger negative associations for time spent in another country or in a different linguistic area
6. stronger negative associations for visits to mines, farms, or growth-points
7. a negative dose-response between the number of trips, duration away, and distance and healthcare access.

# Results

The sociodemographic, healthcare-use, and mobility measures are shown in **Table 1**. There were 2,839 women who took part in the survey. There was little missing data: 21 women were missing data on violence, 10 on social support, 11 on knowledge of HIV status, and 50 on the location of at least one of the places visited in the past 12 months. The majority (64%) were divorced, 2,376 had at least one child (84%), 1,175 were food insecure (42%), and 1,536 reported having social support (55%).

Only 889 women took part in community mobilisation (32%), while 1,487 had attended a *Sisters* clinic two or more times in the past 12 months (53%). There were 1,168 women who tested negative for HIV, of whom 757 (66%) had tested within the last 3 months. Only 17% of HIV-negative women in the intervention-arm reported taking PrEP. Of the 1,671 women who tested positive for HIV, 1,342 knew their status (80%). Of these, 1,153 reported that they were taking ART (86%); of those, 974 had a viral load of 1000 copies or fewer per mL (85%).

Most women (2,250, 80%) had taken a trip in the past 12 months, and they had travelled a median 2 times (IQR: 1 to 3). 2,217 women stayed overnight at least once (79%), spending a median 28 days away (IQR: 7 to 82). Women reported spending more time away where they worked in sex work than when they did not, and spending similar amounts of time within 5km or further from *Sisters* sites. Few women travelled outside of Zimbabwe (497, 17%), or to a different linguistic area (359, 13%). The median total distance travelled by women who made at least one trip was 679km (IQR: 319 to 1,257).

Complete cases were included in the analysis (2,725, 96%). The prevalence of the outcomes is shown by levels of the factor variables and separately for zero and non-zero categories of the continuous variables in **Table 2** and **Table 3**. Risk ratios are shown in **Table 4**, age-adjusted for the sociodemographics and fully-adjusted for the mobility exposures.

Many of the sociodemographic variables were associated with the healthcare use outcomes. Younger women had generally poorer healthcare use, for example only 42/60 (72%) of 20-24 year old women living with HIV were virally suppressed, compared to 327/368 (89%) of women aged 40 or more. Women living with HIV with secondary education were less likely to know they were positive than women without any formal education (76% v.s. 87%, age-adjusted RR: 0.90, 95% CI: 0.83 to 0.96). The prevalence of the outcomes was similar across marital statuses. For all outcomes asides from recent testing for women testing HIV-negative, women who had spent two or fewer years in sex work had lower healthcare use, for example, only 76% of these women were virally suppressed as compared to 88% of women who had worked in sex work for seven or more years. Each additional child was associated with higher attendance at community mobilisation (age-adjusted RR: 1.11, 95% CI: 1.03 to 1.20), and recent testing among HIV-negative women (age-adjusted RR: 1.08, 95% CI: 1.01 to 1.15). Women who reported owning a mobile phone had higher attendance at community mobilisation meetings (age-adjusted RR: 1.26, 95% CI: 1.04 to 1.53), clinics (age-adjusted RR: 1.32, 95% CI: 1.16 to 1.51), PrEP use (age-adjusted RR: 1.62, 95% CI: 0.96 to 2.71), and ART initiation (age-adjusted RR: 1.10, 95% CI: 1.00 to 1.20). Reported food insecurity and sex-work-as-sole-source-of-income were not associated with any of the healthcare use outcomes. Women living with HIV were more likely to take part in community mobilisation (age-adjusted RR: 1.17, 95% CI: 1.03 to 1.34) and attend clinics (age-adjusted RR: 1.20, 95% CI: 1.09 to 1.31). Women who scored higher than ten on the PHQ-9 were less likely to take part in community mobilisation than women who scored lower (age-adjusted RR: 0.76, 95% CI: 0.63 to 0.93), or to have taken PrEP (age-adjusted RR: 0.59, 95% CI: 0.34 to 1.02). Experiencing sex-work-related stigma was associated with lower PrEP use (age-adjusted RR: 0.62, 95% CI: 0.43 to 0.90), but not associated with other outcomes. Alcohol and substance misuse was also associated with lower PrEP use only (age-adjusted RR: 0.52, 95% CI: 0.32 to 0.86); experience of violence was not associated with any outcome. Having social support available was associated with higher attendance at community mobilisation meetings (age-adjusted RR: 1.16, 95% CI: 1.02 to 1.33), clinics (age-adjusted RR: 1.11, 95% CI: 1.02 to 1.21), and more recent testing among HIV-negative women (age-adjusted RR: 1.11, 95% CI: 1.00 to 1.23), but not with PrEP use (age-adjusted RR: 1.02, 95% CI: 0.70 to 1.48) or any healthcare-use outcomes for women living with HIV.

Overall, women who made at least one trip, spent any days away, or travelled any distance in the last 12 months had similar prevalences of the outcomes as women who did not move (**Table 3** and **Table 4**). However, there was evidence of a positive association between some of the mobility measures and community mobilisation and clinic attendance. Women who made trips that included use of healthcare were more likely to have taken part in community mobilisation (40%) than women who did not (30%). Lines *a*-*e* in the upper left panel of **Figure 1** show that the adjusted risk ratio for number of trips and attendance at community mobilisation were strengthened as the number of trips, of all kinds, increased. The trend in the figure was corroborated by the association with each additional trip made, with an adjusted risk ratio of 1.08, 95% CI: 1.04 to 1.12, with the strongest association for trips that included healthcare use (adjusted RR: 1.18, 95% CI: 1.06 to 1.30). The association between days away of any type and community mobilisation were close to 1.0, although the linear model may not have been appropriate since the effects were largest for women who had spent 15-30 days away in the past year for most of the measures of time away, and less strong for 31 or more days away (**Figure 1**). Each additional 100km travelled in the past 12 months was associated with attending community mobilisation, with an adjusted risk ratio of 1.01 (95% CI: 1.01 to 1.02), although this effect was largely due to the higher prevalence among women who travelled 500km or more (line *q* in upper-left panel of **Figure 1**).

Women who travelled for sex work were more likely to have attended a clinic two or more times (58%) than women who did not (48%). The association was strongest for higher numbers of sex-work-motivated visits (line *e* in the upper-second-left panel of **Figure 1**), and the adjusted risk ratio for each additional visit for sex work was 1.04, 95% CI: 1.01 to 1.08. Days away when travelling for sex work was associated with clinic attendance (line *g*), although this was not aparent in the linear model and each additional 100km travelled in the past 12 months was also not associated (adjusted risk ratio 1.00, 95% CI: 1.00 to 1.01, and line *q*). The effects for the other outcomes were close to the null (**Table 4**) and there were no discernible patterns in the categorised effects shown in the remaining five panels of **Figure 1**.

The results of the mediation analysis are shown in **Table 5**. If these associations can be assumed to be causal, then the effect of taking trips on taking part in community mobilisation was mostly direct (adjusted $RR\_{A\rightarrow Y}$: 1.07, 95% CI: 1.01, 1.12), with a much smaller amount acting through the distance travelled or the duration away. For attendance at clinics, the direct effect (adjusted $RR\_{A\rightarrow Y}$: 1.02, 95% CI: 0.98, 1.06) was similar to the indirect effect through the distance travelled (adjusted $RR\_{A\rightarrow M\_{1}Y}$: 1.02, 95% CI: 0.99, 1.05), with none of the effect acting through time away alone. None of the other null effects were altered by adding mediating variables: the effects had not ‘cancelled out’.

# Discussion

We few associations between mobility and healthcare access among female sex workers in Zimbabwe. Unexpectedly, mobility was associated with increased probability of taking part in community mobilisation and attending a *Sisters* clinic more than two times in 12 months. We found no evidence of associations between mobility and HIV testing, PrEP use, ART initiation, or viral suppression. Very little of the association between the number of trips and community mobilisation and clinic attendance was mediated by the distance or time spent away. Overall, our findings do not support the hypothesis that mobility leads to lower healthcare access among sex workers in Zimbabwe as we observed none of the patterns we expected.

Our study was strengthened by multi-faceted measures of mobility, and a range of outcomes relevant to HIV prevention and care for sex workers in Zimbabwe. We had little missing data and were able to geolocate most of the places named in the dataset. We estimated risk ratios that are more easily interpreted than odds ratios [51]. A log-binomial model may have been more appropriate than the modified Poisson model, but it often fails to converge [52] and the modified-Poisson model is less prone to bias when the prevalence of the dependent variable is high [53], as it was in our analysis. We did not explore the functional form of non-linear associations, for example by fitting log-transformed data, which may have produced more precise estimates using all the data [54]. Instead, we used multiple categories of the quantitative mobility exposures to adequately distinguish between high and low exposure; as a result, the contrasts for low-intensity exposures were likely to be close to the null, as we found.

Our study has limitations. The data were cross-sectional and relied on recall of mobility and most of the healthcare outcomes. This may have led to non-differential bias of the effects toward the null. Although developed in consultation with peer-educators, the questionnaire tool for measuring mobility may have allowed different interpretations of what constituted spending time away from the location of interview. From the data, it does not appear that women who moved home, were homeless or in unstable housing, or who stayed in different parts of the same municipal area reported these as ‘mobility’; these may have effects on healthcare access, and may be included in measures of mobility in other studies. Most of the outcomes were self-reported and susceptible to social-desirability bias. This may have particularly affected answers to questions about community mobilisation and *Sisters* clinic attendance because the data-collection teams were associated with the organisation that delivered these services. Social-desirability bias may have led to over-reporting, however the effect estimates would be unbiased unless the bias was also associated with reported mobility. Mobility and healthcare access are both deeply embedded in structural, social, and psychological factors and it is unlikely that we were able to account for all of these factors in our adjusted analysis, allowing residual confounding to potentially explain the associations that we observed. Although we adjusted for important potential confounders, such as the number of dependents and common mental health disorders, the positive associations between mobility and community mobilisation activities and clinic attendance may be due to a ‘healthy-migrant’ effect [55] – conceptually analogous to the healthy worker effect [56] – where mobility depends on a self-selection process involving individual and structural factors that are difficult to measure and positively affect health or healthcare use, such as general health or self-efficacy. Ideally, we would have used longitudinal data for mediation analysis [57], however following sex workers for long periods is difficult, and the ordering of the variables – number of trips, distance, time away – is more conceptual than temporal. Elements of the survey design may have excluded mobile women: only women who had lived in the same area for six months or more were eligible for the survey; the surveys were conducted over two weeks so women who were travelling would not have been available to participate, and recruitment was through peer contacts which may exclude women who regularly travel. The estimates of association with healthcare access may be weakened because the most highly mobile women may have been excluded. Also, without an unbiased estimate of the prevalence of mobility, it is difficult to estimate the population impact of mobility on healthcare access, e.g.the population attributable fraction.

The range of results that we found add to our recent systematic review of mobility and healthcare access for female sex workers [15]. Ours is the first study to find a positive association between mobility and community mobilisation and attendance at clinics for female sex workers. This contrasts with other stidues [17,18], including a study in Vancouver, Canada, that found an association between mobility and reported barriers to accessing healthcare (adjusted odds ratio: 1.79, 95% CI: 1.12 to 2.86) [16]. We did not find that mobility was associated HIV status awareness, which was consistent with Bach Xuan *et al.* 2013 [19]. A study in South Africa also found that ART initiation was not associated with mobility [20]. Our findings regarding viral suppression are inconsistent with studies that found mobility associated with poorer ART adherence among female sex workers [14,21], but consistent with two small studies that found no association with viral load in lower-prevalence contexts [22,23].

Zimbabwe has a national network of sex-worker-friendly clinics: might this explain the lack of evidence of a negative effect of mobility on healthcare use? We qualified our mobility exposures in three ways to address this question. First, we excluded mobility where healthcare was used, and did not find an association. Second, we excluded visits to locations within 5km of a *Sisters* clinic, and again we found no association. Third, we investigated the effects of travel abroad – were the *Sisters* clinic does not operate – and found no association. Our results do not support the interpretation that the effect of mobility in Zimbabwe was modified by the presence of the *Sisters* network. However, it is possible that the *Sisters* programme has empowered women to be better networked overall and more motivated to use healthcare when needed; further research could explore how interventions such as community mobilisation interact with the effects of mobility.

We cautiously interpreted the association between mobility and attendance at community mobilisation meetings and clinics as causal. Caution was warranted because we did not correct confidence intervals for the number of contrasts, and there were risks of recall, misclassification, and confounding biases (e.g. healthy-migrant effect), plus potential for reverse causation. Furthermore, we found that little of the effect of taking trips on community mobilisation and clinic attendance was mediated by distance or time away, which is difficult to reconcile with the interpretation that taking trips *caused* these outcomes to change. However, there were features of the results that supported a causal association, such as dose respose (higher levels of mobility were usually more strongly associated with the community mobilisation and clinic attendance than lower levels) and that reports of using healthcare while travelling tended to strengthen the associations. In contrast, we are more confident that the lack of association between mobility and viral suppression was not due to bias or reverse causation since viral load was measured objectively at the time of interview. As for the general absence of negative associations between mobility and healthcare use: we can only offer tentitive interpretation. It is possible that sex workers are taking advantage of multiple points of contact with the healthcare system, although as we discuss above our analysis does not confirm this interpretation. It is also possible that while mobility may substantially increase the relative instability for non-sex workers, sex workers’ lives in Zimbabwe are highly unstable for multiple reasons and therefore insesitive to mobility. On the contrary, given the high levels of engagment with healthcare found in our survey, it is possible that sex workers in Zimbabwe have developed coping strategies to deal with the various structural inequalities that they face that can be useful for overcoming the effects that mobility would be expected to have on sustained healthcare access.

We explored multiple aspects of mobility over a 12-month period, which may explain some of the disparities with previous research. However, our approach was not exhaustive: we did not explore aspects of mobility at the extremes of temporal and distance measures. For example, unstable housing, daily commuting, migrant status, or residential mobility over more than a 12-month period. We did not explore the effect of mobility over the life-course [58], or longer-term patterns such as seasonality and periodicity [59]. These omissions demonstrate the wide variety of aspects of mobility, even before considering the possible omitted qualifiers, e.g. mode of transport or type of accommodation, and the enormous array of possible *intersecting* qualities of mobility, such as long-distance-short-stay-with-family trips or short-frequent-working trips. The need for a nuanced approach to studying the effects of mobility has been called for when researching structural-drivers of HIV risk [60]; the same arguments apply to studying effects on other health-related behaviours. Latent-class analysis using the data in the present study identified five classes of mobile female sex worker based on multiple features of mobility [8]. These classes were not used here to ensure comparability with previous literature. The validity of future research on mobility would be improved by developing nuanced and conceptually coherent constructs to describe the lived experience of mobility by sex workers, drawing on qualitative and quantitative approaches.

As mobility increases, along with HIV-treatment availability and the reliance on adherence to ART or PrEP to reduce population incidence, mobile populations will require appropriate healthcare services to reduce the potential effects of increasing population viral load [61]. As well health impacts on sex workers, the potential for mobility to affect viral suppression when working at sites that are associated with high client volumes and condomless sex could reduce the effectiveness of universal-test-and-treat to reduce onward transmission of HIV in sex-work networks.

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