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The Impact of Unrestricted Transportation Passes on HIV Treatment and Service Outcomes: Results from a Randomized Controlled Trial among Unstably Housed People Living with HIV

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Research Article

Keywords: randomized clinical trial, housing instability, transportation, HIV, viral suppression, supportive services, case management

Posted Date: November 13th, 2026

DOI: https://doi.org/10.21203/rs.3.rs-11540634/v1

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Additional Declarations: No competing interests reported.

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Target Journal: BMC Public Health

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Abstract

Background: Unstably housed people living with HIV (PLWH) face barriers to achieving and maintaining viral suppression. Transportation instability limits access to medical care and supportive services. Prior research has proposed transportation passes as a potential intervention. However, the effectiveness of this approach has not been evaluated.

Methods: This prospective randomized controlled trial recruited 146 unstably housed PLWH from a community-based organization serving PLWH in a Midwest city. Participants randomized to the intervention group received an unrestricted public transit pass for 6 months, while the control group retained their current benefits. Surveys administered at baseline and six-month follow-up assessed housing and transportation instability and quality of life. Viral load lab results and social service utilization were extracted from administrative databases.

Results: Logistic regression found no evidence of an effect of the intervention on the study's primary outcome of viral suppression (OR .97; 95% CI: 0.47-2.01; p=.942) across multiple missing-data approaches. Models accounted for baseline indicators. Most secondary outcomes, including meals served, stable housing, employment, and quality-of-life scores, also showed no evidence of an effect. However, in a negative binomial model, the intervention affected the frequency of engagement with case management services, with the intervention arm having an estimated rate of recorded follow-up case management contacts 2.24 times that of the control arm (95% CI, 1.03 to 4.88; p = 0.042). Furthermore, stable housing and employment at follow-up clustered strongly in the intervention group (ratio of ORs: 48.9; p = .004).

Conclusion: Many supportive services are geared toward stabilizing PLWH experiencing homelessness to help them achieve and maintain viral suppression. However, transportation access is an oft-overlooked structural determinant of health. This study finds no evidence of a direct clinical effect of transportation passes during the follow-up period. Increased engagement with case management, though, is valuable and is associated with long-term health and stabilization benefits. The increased service engagement might warrant the relatively low cost of $16 per participant per month. The increased service engagement and exploratory stabilization findings suggest mechanisms by which transportation access might have clinical benefits at longer follow-up periods.

Trial Registration: The trial was prospectively registered with clinicaltrials.gov: NCT06483152 on July 3, 2024.

Keywords: randomized clinical trial; housing instability; transportation; HIV; viral suppression; supportive services; case management

Introduction

Homelessness and housing instability remain common among people living with HIV (PLWH) in the United States, with an estimated 9%–17% of PLWH experiencing unstable housing or homelessness [1]. Housing instability is strongly associated with poorer HIV outcomes and reduced access to timely and continuous HIV care [2–5]. Without safe and stable housing, immediate needs such as securing food, shelter, and personal safety may take priority over other long-term health needs. In a clinic-based study, researchers found that among participants experiencing homelessness, the adjusted odds of incomplete viral suppression were nearly four times higher than among housed participants (adjusted odds ratio, 3.84; 95% CI, 1.36–10.3) [6]. Similarly, national data from 2022 showed lower viral suppression among unstably housed PLWH than among stably housed PLWH (77.9% versus 90.6%) [1]. These disparities underscore the importance of identifying solutions to common barriers that restrict access to HIV care among PLWH experiencing housing instability.

Transportation access is a well-documented social determinant of health, especially among people experiencing homelessness or unstable housing [7]. Many people without stable housing rely on public transportation to access healthcare and other essential services. However, transportation costs, limited transportation availability, long travel distances, and difficulty in commuting to transit stops can make obtaining care difficult [8, 9]. This relationship is especially consequential for PLWH, whose short-term and long-term health depends on sustained engagement in medical care, regular laboratory monitoring, and uninterrupted access to ART [3]. When housing and transportation instability occur together, the known difficulty of accessing care may force PLWH to prioritize competing survival needs, leading to missed appointments, treatment interruptions, and lower retention in care, thereby impeding the achievement and maintenance of viral suppression [10, 11].

Transportation access may also affect engagement with other supportive services available to PLWH. Among people experiencing homelessness, transportation access has been identified as a barrier to receiving supportive services and utilizing available resources [9, 12]. Therefore, reliable and affordable transportation may lead to engagement with resources such as food assistance, shelters, housing assistance, and employment opportunities, all of which may influence health and stability [9]. One service designated to help PLWH navigate medical and social needs is Medical Case Management (MCM). MCM is a central component of Ryan White HIV/AIDS Program (RWHAP)-funded care and helps PLWH coordinate medical treatment while addressing social and structural barriers [13]. Transportation access may increase engagement with case managers by

making in-person services easier to reach. Because greater engagement with MCM has been associated with improved viral suppression and other HIV clinical outcomes [14, 15], MCM represents one potential pathway through which transportation access could ultimately influence health.

Although transportation is consistently identified as a barrier to HIV care, it has rarely been the primary focus of research [2, 11, 16–19]. Studies of PLWH who are out of care have documented transportation barriers alongside housing instability, poverty, stigma, substance use, and mental illness [20], prompting programs to respond with bus tokens, rideshare services, gas cards, and other transportation assistance [16]. However, these transportation interventions have generally been one part of multicomponent initiatives that also provide other support services such as case management, navigation, counseling, and financial incentives, limiting the ability to isolate the independent contribution of transportation assistance to clinical outcomes. For example, the Max Clinic found improvements in viral suppression when participants were provided with unrestricted bus passes alongside walk-in medical care, intensive case management, food assistance, cell phones, financial incentives, and cross-agency service coordination [21]. However, the investigators acknowledged that they could not determine which piece of the intervention led to their observed outcomes; therefore, the independent contribution of unrestricted transportation could not be confirmed. [21]. When transportation has been examined more directly, assistance has typically been restricted to travel for specific medical appointments rather than providing unrestricted mobility for healthcare, supportive services, and other daily living needs [22, 23].

Systematic reviews suggest that transportation assistance may reduce missed appointments, but the evidence remains insufficient to determine whether transportation assistance itself improves care engagement or downstream clinical outcomes [24–27]. Because transportation has generally been evaluated as only one component of broader interventions, the current literature calls for further research to isolate the independent effects of transportation assistance and clarify whether removing this specific barrier can improve HIV-related and social outcomes [24–27].

To determine the effects of transportation, we conducted a prospective randomized controlled trial (RCT) evaluating the impact of providing six months of unrestricted access to public transportation for PLWH experiencing homelessness or unstable housing. The primary objective was to determine whether the intervention improved viral suppression. Prespecified secondary outcomes included engagement with medical case management and meal services, housing stability, employment, and quality of life. We hypothesized that

removing transportation access as a barrier would increase engagement with healthcare and supportive services and, consequently, improve HIV and social outcomes.

Methods

This study was a prospective, two-arm, parallel-group, randomized controlled trial evaluating the impact of a transportation pass intervention on the health and quality of life of PLWH experiencing unstable housing. The study followed the Consolidated Standards of Reporting Trials (CONSORT) guidelines for methodology and reporting[28]. The trial was prospectively registered in clinicaltrials.gov (NCT06483152).

Participants were recruited between October 2024 and March 2025 from a community-based organization in a Midwestern city serving PLWH that offers clinical and supportive services and a drop-in day center that provides a weekday meal. Recruitment ended as planned once we reached our desired sample size. Participants were randomly assigned to either the intervention group, which received an unrestricted public transportation pass, or the control group, which received their current transportation benefits, if any. Participants were randomized using a block-randomization envelope method, in which every 4 envelopes contained two intervention assignments and two control assignments [29]. This ensured that participants were recruited evenly into each group throughout the recruitment period, minimizing time-based confounding variables. Blinding was not feasible due to the nature of the intervention, which required consistent monitoring of the transportation pass account, as described in the intervention description below. To minimize the effects of non-blinding, all participants, including those in the control group, were given a list of transportation benefits and programs, along with instructions on how to apply. The randomization sequence was generated by the second author. The first, second, and third authors enrolled participants and assigned the intervention and control groups. No deviations from the randomization sequence were identified.

Participants were eligible for the study if they were a client of the organization, which required proof of HIV diagnosis, and were at least 18 years of age. Participants were also required to self-report current homelessness or housing instability and regularly stay within the organization's metropolitan service area. This requirement ensured that participants could use the regional public transit system during the intervention period. Participants were screened for current homelessness and housing instability with a questionnaire developed in collaboration with the organization's housing case managers. Some participants did not meet the definition of literal homelessness (e.g., sleeping outside), but experienced housing instability through insecure access (e.g., "couch hopping"). These forms of housing instability corresponded with the kinds of survival strategies, including sex work, that many LGBTQ participants engaged in to temporarily achieve access to

insecure housing [30, 31]. Table 1 shows the percentage of the sample represented by each type of housing instability. Participants were excluded if they could not, with staff assistance, understand the consent form in English. Participants whose first language was not English were given assistance from staff in their primary language to answer questions about the meaning of words on consent forms or surveys.

Table 1. Baseline participant characteristics by randomized group

Characteristic	Overall (N = 146)	Control (n = 73)	Intervention (n = 73)
Demographic characteristics			
Age, years	42.4 (11.1)	41.9 (12.1)	42.9 (10.1)
Sex assigned at birth			
Male	120/145 (82.8%)	64/73 (87.7%)	56/72 (77.8%)
Female	22/145 (15.2%)	8/73 (11.0%)	14/72 (19.4%)
Other	3/145 (2.1%)	1/73 (1.4%)	2/72 (2.8%)
Missing	1/146 (0.7%)	0/73 (0.0%)	1/73 (1.4%)
Gender category			
Cis Male	99/143 (69.2%)	55/72 (76.4%)	44/71 (62.0%)
Cis Female	21/143 (14.7%)	8/72 (11.1%)	13/71 (18.3%)
Transgender woman	8/143 (5.6%)	4/72 (5.6%)	4/71 (5.6%)
Transgender man	1/143 (0.7%)	0/72 (0.0%)	1/71 (1.4%)
Transgender other	14/143 (9.8%)	5/72 (6.9%)	9/71 (12.7%)
Missing	3/146 (2.1%)	1/73 (1.4%)	2/73 (2.7%)
Race and/or ethnicity, selected			
White/Caucasian	52/142 (36.6%)	28/71 (39.4%)	24/71 (33.8%)
African-American or Black	64/142 (45.1%)	28/71 (39.4%)	36/71 (50.7%)
Hispanic or Latinx	20/142 (14.1%)	13/71 (18.3%)	7/71 (9.9%)
Native American or Indigenous	21/142 (14.8%)	9/71 (12.7%)	12/71 (16.9%)
Asian	4/142 (2.8%)	3/71 (4.2%)	1/71 (1.4%)
Native Hawaiian or Pacific Islander	3/142 (2.1%)	1/71 (1.4%)	2/71 (2.8%)
Other/write-in response	3/142 (2.1%)	1/71 (1.4%)	2/71 (2.8%)
Missing	4/146 (2.7%)	2/73 (2.7%)	2/73 (2.7%)

Characteristic	Overall (N = 146)	Control (n = 73)	Intervention (n = 73)
Sexual identity			
Gay or Lesbian	68/142 (47.9%)	38/72 (52.8%)	30/70 (42.9%)
Straight	43/142 (30.3%)	21/72 (29.2%)	22/70 (31.4%)
Bisexual or Pansexual	21/142 (14.8%)	8/72 (11.1%)	13/70 (18.6%)
Queer	8/142 (5.6%)	3/72 (4.2%)	5/70 (7.1%)
Another sexual identity	1/142 (0.7%)	1/72 (1.4%)	0/70 (0.0%)
Prefer not to say	1/142 (0.7%)	1/72 (1.4%)	0/70 (0.0%)
Missing	4/146 (2.7%)	1/73 (1.4%)	3/73 (4.1%)
Education			
High School or Less	72/144 (50.0%)	37/73 (50.7%)	35/71 (49.3%)
Some College	48/144 (33.3%)	25/73 (34.2%)	23/71 (32.4%)
College Degree (2+)	20/144 (13.9%)	10/73 (13.7%)	10/71 (14.1%)
Advanced Degree	4/144 (2.8%)	1/73 (1.4%)	3/71 (4.2%)
Missing	2/146 (1.4%)	0/73 (0.0%)	2/73 (2.7%)
Marital status			
Single	122/145 (84.1%)	58/73 (79.5%)	64/72 (88.9%)
Married	9/145 (6.2%)	4/73 (5.5%)	5/72 (6.9%)
Previously Married	14/145 (9.7%)	11/73 (15.1%)	3/72 (4.2%)
Missing	1/146 (0.7%)	0/73 (0.0%)	1/73 (1.4%)
Economic and housing characteristics			
Employed at baseline			
Yes	21/139 (15.1%)	14/70 (20.0%)	7/69 (10.1%)
No	118/139 (84.9%)	56/70 (80.0%)	62/69 (89.9%)
Missing	7/146 (4.8%)	3/73 (4.1%)	4/73 (5.5%)
Any income at baseline			
Yes	41/124 (33.1%)	25/64 (39.1%)	16/60 (26.7%)
No	83/124 (66.9%)	39/64 (60.9%)	44/60 (73.3%)
Missing	22/146 (15.1%)	9/73 (12.3%)	13/73 (17.8%)
Housing status			
Insecure Access	55/141 (39.0%)	29/72 (40.3%)	26/69 (37.7%)
Literal Homelessness	86/141 (61.0%)	43/72 (59.7%)	43/69 (62.3%)
Missing	5/146 (3.4%)	1/73 (1.4%)	4/73 (5.5%)
Days slept outside in prior month	6.0 (0.0–20.0)	3.0 (0.0–20.0)	14.0 (2.0–23.5)
Missing	16/146 (11.0%)	9/73 (12.3%)	7/73 (9.6%)

Characteristic	Overall (N = 146)	Control (n = 73)	Intervention (n = 73)
HIV characteristics			
Administratively observed HIV care			
Yes	110/144 (76.4%)	59/72 (81.9%)	51/72 (70.8%)
No	34/144 (23.6%)	13/72 (18.1%)	21/72 (29.2%)
Missing	2/146 (1.4%)	1/73 (1.4%)	1/73 (1.4%)
Administratively observed HIV care retention			
Yes	70/134 (52.2%)	32/66 (48.5%)	38/68 (55.9%)
No	64/134 (47.8%)	34/66 (51.5%)	30/68 (44.1%)
Missing	12/146 (8.2%)	7/73 (9.6%)	5/73 (6.8%)
Administratively observed viral suppression			
Yes	86/110 (78.2%)	49/59 (83.1%)	37/51 (72.5%)
No	24/110 (21.8%)	10/59 (16.9%)	14/51 (27.5%)
Missing	36/146 (24.7%)	14/73 (19.2%)	22/73 (30.1%)
Current HIV medication use, self-reported			
Yes	131/143 (91.6%)	66/71 (93.0%)	65/72 (90.3%)
No	12/143 (8.4%)	5/71 (7.0%)	7/72 (9.7%)
Missing	3/146 (2.1%)	2/73 (2.7%)	1/73 (1.4%)
Baseline supportive-service use			
Recorded meal units	11.0 (1.0–46.0)	9.0 (0.0–57.0)	11.0 (3.0–42.0)
Recorded case management assists	10.0 (5.0–18.0)	10.0 (4.0–18.0)	11.0 (6.0–16.5)
Baseline WHOQOL-HIV BREF domain scores			
Physical	12.6 (3.9)	12.6 (3.9)	12.7 (4.0)
Missing	20/146 (13.7%)	10/73 (13.7%)	10/73 (13.7%)
Psychological	12.2 (3.9)	11.9 (3.6)	12.5 (4.1)
Missing	21/146 (14.4%)	11/73 (15.1%)	10/73 (13.7%)
Level of independence	13.3 (3.7)	13.1 (3.4)	13.5 (3.9)
Missing	35/146 (24.0%)	20/73 (27.4%)	15/73 (20.5%)
Social relationships	11.9 (4.0)	11.6 (3.9)	12.2 (4.1)
Missing	25/146 (17.1%)	14/73 (19.2%)	11/73 (15.1%)
Environment	11.1 (3.1)	10.9 (2.9)	11.3 (3.3)
Missing	30/146 (20.5%)	19/73 (26.0%)	11/73 (15.1%)
Spirituality, religion, and personal beliefs	12.6 (3.9)	12.5 (3.9)	12.8 (3.8)
Missing	19/146 (13.0%)	10/73 (13.7%)	9/73 (12.3%)

Note. Values are n/N (%) unless otherwise indicated. N is the number of nonmissing responses for the characteristic within the column; missing values are shown separately. Age and WHOQOL-HIV BREF domains are mean (SD). Days slept outside and recorded meal units are median (Q1–Q3). Case-management assists are median (Q1–Q3) among participants with at least one baseline assist (32 overall; 17 control; 15 intervention). No baseline significance

tests were performed. Race and/or ethnicity was a multi-select item, so category percentages may total more than 100%. Rare free-text race responses are grouped as Other/write-in response. Rare free-text sexual identities are grouped as Another sexual identity. Administrative HIV care indicates at least one qualifying viral-load date within six months before baseline. Administrative care retention indicates at least two qualifying viral-load dates within 12 months before baseline and at least 90 days apart. Viral suppression is based on a qualifying baseline viral load below 200 copies/mL. Unavailable administrative data remain missing. WHOQOL-HIV BREF domain scores are observed complete-domain scores transformed to the 4-20 scale; higher scores indicate better quality of life. Abbreviations: Q1, first quartile; Q3, third quartile; SD, standard deviation. Housing categories combine street homelessness with shelters and temporary housing with couch surfing.

Sample Size

Administrative data indicated that a potential population of 379 PWLH met the inclusion criteria and had accessed a service at the organization in 2023. This population of potential participants had a viral suppression incidence of 70.8%. If the intervention met the 90% viral suppression goal of the END HIV 2030 plan, then standard normal binomial distribution sample size calculations at 80% power and a significance level of 0.05 would suggest a sample with 66 participants per group for a total of 132 participants. With an estimated 10% attrition rate, 73 participants were ultimately recruited per group, for a total of 146. In the end, 68.5% of participants completed a follow-up survey at six months through a combination of frequent reminders, consenting participants to multiple modes of contact, such as collecting social media and trusted contacts at initial screening, and internal database flags. Factors associated with increased retention, such as service utilization, were included in the multiple imputation model discussed below.

Intervention

Participants in the intervention group were given an unrestricted public transit pass by enrolling them in an organization-sponsored transit program in collaboration with the regional public transit authority. Participants were given a card they could swipe on public transit systems, which charged the study $1 per ride. Participants' accounts were automatically refilled when they had less than $10, effectively giving them unrestricted rides from their perspective. Participants were reminded that they could receive a replacement card should they lose it. 38 participants (52.1%) received a replacement card at least once over their six months of enrollment. The median number of replacements was 2, and one participant lost their card 12 times.

Control group

Participants in the control group received no additional transportation benefit, and their access to existing transportation benefits was not affected. Initially, participants receiving case management services could access a $20 monthly allowance of tokens; however, they lost access to this allowance during the study due to policy changes. Participants were provided with a list of transportation resources, including instructions on how to

access them. Staff at the organization occasionally assisted participants with accessing desired resources.

Data collection

Data were collected from participant surveys, state and organizational administrative records, and public-transit usage records. Participants completed a baseline survey before randomization and a follow-up survey approximately six months after enrollment. Surveys were administered by study staff via an electronic form or a paper copy and took approximately 20 minutes to complete. Participants received a $25 Visa gift card for completing each assessment. To mitigate anticipated barriers to timely follow-up participation, participants could provide multiple contact methods, including telephone numbers, email addresses, social media handles, and contact information for trusted individuals. We allowed participants a grace period extending from two weeks before to two weeks after their six-month completion date to complete the survey. We administered the follow-up survey remotely to participants who indicated that they may not be able to complete it in person during the follow-up period. Study staff also used internal database notifications to identify participants who returned to the organization during the follow-up period.

The baseline survey collected demographic characteristics, housing and employment status, transportation access and use, experiences of transportation-related barriers, and self-reported physical and mental health. Quality of life was assessed using the World Health Organization Quality of Life HIV Brief instrument (WHOQOL-HIV BREF), which includes 31 items covering six quality-of-life domains and two general items [32]. Responses were recorded on five-point scales, with higher domain scores indicating better quality of life.

The six-month follow-up survey repeated measures of housing, employment, transportation access, health, and quality of life to assess changes occurring during the study period. Questions capturing stable demographic characteristics at baseline were omitted from the follow-up survey.

Viral-load values were obtained from the state's implementation of a client-level electronic health and supportive services information system used by RWHAP recipients and providers. Administrative records from the study organization were used to obtain the numbers of recorded medical case-management visits and meals received during the six months preceding enrollment and the six-month intervention period. These administrative outcomes were available independently of participants' completion of the follow-up survey.

For participants assigned to the intervention group, the regional public transit authority provided electronic pass-use records. These records included the total number of rides and the number of unique days on which each participant used the transportation pass during the intervention period.

Outcome measures

The primary outcome was documented viral suppression at the six-month follow-up. Participants without documented viral suppression at follow-up were classified as having a negative HIV care outcome. Follow-up viral suppression was determined using the most recent available viral-load result collected during the six months preceding each participant's scheduled follow-up date. Participants with a viral load below 200 copies/mL were classified as virally suppressed. Participants without a documented viral load result and those with a viral load of 200 copies/mL or higher were classified as having a negative HIV care outcome [33]. Baseline viral suppression was defined similarly, using the most recent available viral load result collected during the six months preceding the baseline assessment.

The secondary outcomes were service use, stable housing and employment, and quality of life. We assessed baseline service use by summing the reported number of encounters for both meals and case management for the six months prior to enrollment. Similarly, follow-up service use summed up the encounters recorded during the six-month study period. Stable housing, employment, and quality of life were self-reported by participants on the baseline and 6-month follow-up survey. Quality of life across six domains was assessed using the WHO HIV Quality of Life brief version, comprising 26 items from the standard WHO HIV Quality of Life instrument and 5 items addressing HIV-specific issues. Participants answered questions on a 5-point scale, with higher scores indicating higher quality of life. Analyses multiply imputed missing values within a domain.

Data analysis

Statistical analyses were conducted using R version 4.6.1 in RStudio version 2026.09.0+174. Participant characteristics were summarized by treatment arm using frequencies and percentages for categorical variables. Baseline characteristics were examined descriptively. All statistical tests were two-sided, with an alpha level of 0.05, and estimates are reported with 95% confidence intervals (CIs).

The primary analysis evaluated viral suppression at the six-month follow-up using logistic regression and followed the intention-to-treat principle (ITT), with participants analyzed according to their randomized group. Participants without a documented viral-load result during the follow-up measurement window were treated as having a negative outcome in

the primary analysis. Two sensitivity analyses evaluated the robustness of this assumption. The first excluded participants without a documented follow-up viral load result, and the second imputed missing viral suppression outcomes. Results are reported as odds ratios.

Negative binomial regression was used to analyze the number of recorded medical case-management contacts and meal units during the intervention period, adjusting for the corresponding service-use measure during the six months preceding enrollment. Stable housing and employment at follow-up were analyzed using logistic regression, adjusting for the baseline employment status. Quality-of-life domain outcomes were analyzed using linear regression (ANCOVA), and general items were analyzed using ordinal logistic regression. Logistic regression results are reported as odds ratios (ORs), negative binomial regression results as rate ratios (RRs), quality-of-life domain results as adjusted mean differences, and ordinal logistic regression results as proportional odds ratios.

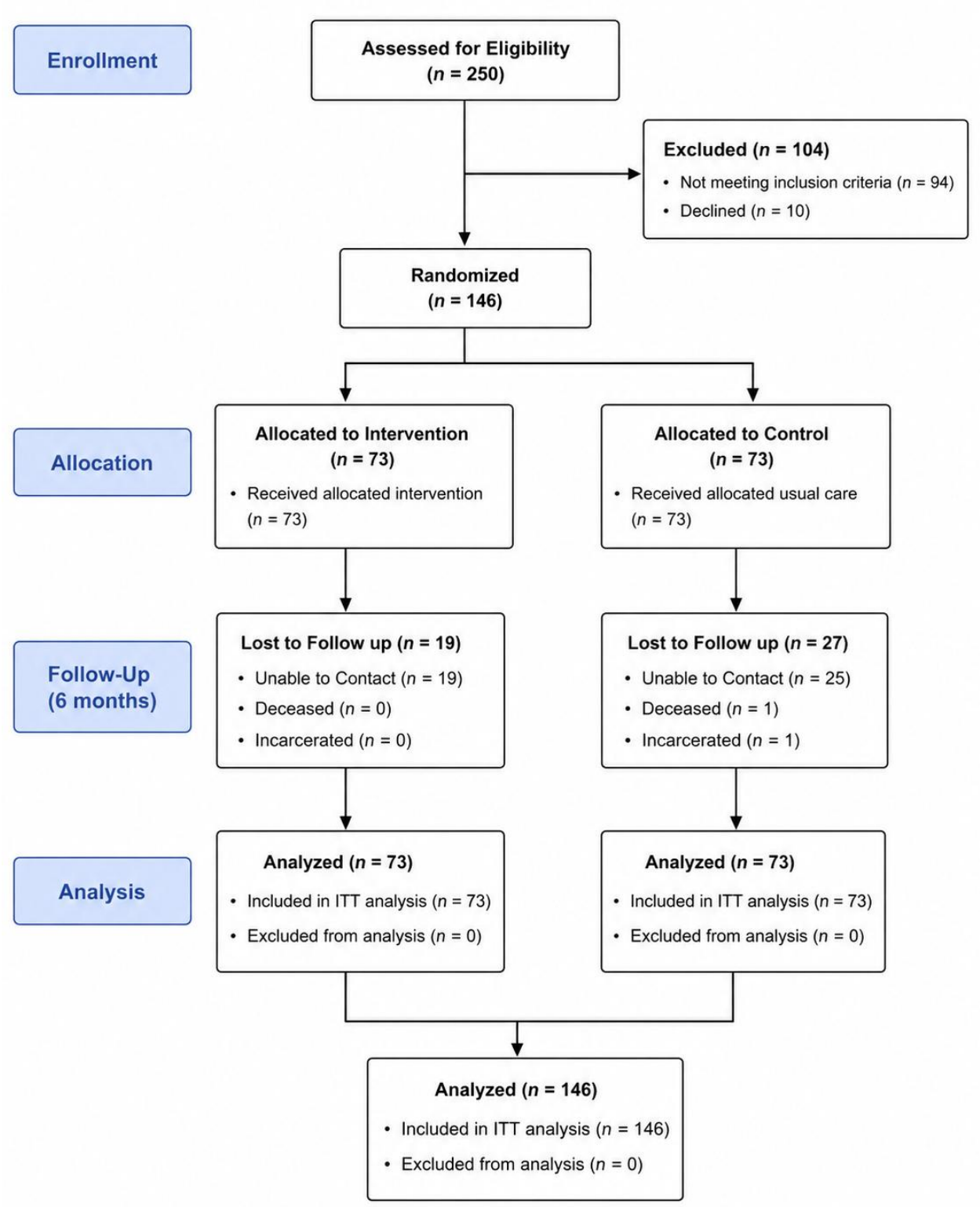
Analyses of transportation-pass use and the interaction between randomized group, housing, and employment were conducted after the primary analyses and are considered exploratory.

Missing data were addressed using Multiple Imputation by Chained Equations (MICE) under the assumption of missing at random (MAR). A total of 100 imputed datasets were generated. Analyses were conducted separately within each imputed dataset, and estimates and standard errors were combined using Rubin's rules.

Results

As shown in our CONSORT diagram (Figure 1), 250 clients at the Midwestern ASO approached the authors for eligibility screening. Of these, 104 were excluded (41.6%) for not meeting inclusion criteria (n = 94) or declined after reviewing the study procedures (n = 10). 73 participants were randomized to the intervention group and received the unrestricted transit pass. 73 participants were randomized to the control group. At follow-up, 19 participants in the intervention group (26%) and 27 in the control group (37%) were lost to follow-up and did not complete the second survey, including one participant who died and another who was incarcerated, for an overall retention rate of 68.5%. The death was unrelated to the study, and no harms were reported. There was an 11% difference in follow-up rates between the groups; it was not statistically significant (p = .215). All 146 participants were retained in the intention-to-treat (ITT) sample because many primary and secondary outcomes were obtained from administrative data and our analyses used multiple imputation to handle missing data.

Figure 1: CONSORT Diagram



Baseline Characteristics

Baseline characteristics of the sample broken down by randomization group are available in Table 1. Of note, the sample was overwhelmingly male (82.8%), consistent with the population at the ASO and the HIV incidence in the area. The majority of the sample was people of color and identified with an LGBTQ+ sexuality. The median number of days slept outside in the last 30 days in the sample was 6. Approximately two-thirds of the sample had no income of any kind at baseline.

Primary Clinical Outcome

The pre-specified primary analysis examined the clinical outcome of viral suppression at the 6-month follow-up (Table 2). Of the 73 participants randomized to the intervention group, 45 (61.6%) were virally suppressed. 28 had negative outcomes, including 10 (13.7%) who were not virally suppressed and 18 (24.7%) who had missing viral load labs. Of the 73 participants randomized to the control group, 49 (67.1%) were virally suppressed. 24 had negative outcomes, including 5 (6.8%) who were not virally suppressed and 19 (26.0%) who had missing viral load labs. There was no evidence that the intervention affected viral suppression (OR: 0.97; CI: 0.47 to 2.01; p = 0.942). Sensitivity analyses that restricted to observed outcomes by dropping missing viral loads or, conversely, that multiply imputed missing viral load outcomes, showed a similar pattern (Table S1).

Table 2. Viral suppression at six months by randomized group

Six-month status	Control (n = 73)	Intervention (n = 73)
Observed viral suppression	49/73 (67.1%)	45/73 (61.6%)
Observed viral nonsuppression	5/73 (6.8%)	10/73 (13.7%)
Viral load unavailable	19/73 (26.0%)	18/73 (24.7%)
Negative HIV Care outcome	24/73 (32.9%)	28/73 (38.4%)

Adjusted intervention effect

Analysis	N	Odds ratio (95% CI)	P value
Primary intention-to-treat	146	0.97 (0.47 to 2.01)	0.942

Values are n/N (%) using all randomized participants in each arm. Viral suppression is a qualifying viral load below 200 copies/mL. The first three rows are mutually exclusive; the fourth combines observed nonsuppression and unavailable viral loads. The primary logistic model adjusts for baseline viral suppression and days slept outside in the prior month. Unavailable baseline and follow-up viral-suppression values are coded as negative outcome; missing days slept outside are multiply imputed. CI, confidence interval.

Table S1. Viral suppression analyses under alternative missing-data assumptions

Analysis	N	Follow-up outcome handling	Odds ratio (95% CI)	P value
Primary intention-to-treat	146	Unavailable outcomes coded as negative outcome	0.97 (0.47 to 2.01)	0.942
Observed-outcome sensitivity	109	Only observed follow-up outcomes	0.67 (0.17 to 2.57)	0.553
Full-cohort MAR sensitivity	146	Missing outcomes multiply imputed	0.71 (0.18 to 2.75)	0.618

All models adjust for baseline viral suppression and days slept outside in the prior month. The primary analysis codes unavailable baseline and follow-up suppression as a negative outcome and imputes missing days slept outside. The observed-outcome sensitivity analysis includes 109 participants with observed follow-up viral loads (54 control; 55 intervention) and multiply imputes missing baseline covariates. The full-cohort sensitivity analysis includes all 146 participants and imputes missing baseline covariates and follow-up suppression under MAR. CI, confidence interval; MAR, missing at random.

Secondary Outcomes: Service Use

We pre-specified several secondary analyses to examine the effect of the intervention on outcomes that might plausibly affect later clinical outcomes, including social service usage and stabilization. We also pre-specified the examination of quality-of-life scores.

Administratively recorded service usage was extracted from internal ASO databases. Two services were pre-specified: meals and case management use (Table 3). There was no evidence of an effect of the intervention on the number of recorded meal units served (RR:

0.87; CI: 0.60 to 1.26; p = 0.452) when accounting for baseline meal units served during the preceding 6 months.

However, there was evidence that the intervention affected the number of case-management assists, which are recorded when a case manager has contact with a client. When accounting for baseline assists, participants in the intervention group had an assist rate 2.24 times that of the control group (RR: 2.24; CI: 1.03 to 4.88; p = .042).

Table 3. Supportive service use during six months of follow-up

Outcome	Control (n = 73)	Intervention (n = 73)	Adjusted effect (95% CI)	P value
Recorded meal units	51.4 (57.7) 33.0 (5.0–76.0)	52.9 (59.8) 31.0 (9.0–69.0)	RR 0.87 (0.60 to 1.26)	0.452
Case-management assists	4.6 (7.8) 0.0 (0.0–8.0)	6.5 (11.9) 0.0 (0.0–11.0)	RR 2.24 (1.03 to 4.88)	0.042

Arm summaries show mean (SD) on the first line and median (Q1–Q3) on the second. They include all randomized participants, including those with no recorded use. Each analysis includes 146 participants. Baseline service use covers the preceding six months. CI, confidence interval; Q1, first quartile; Q3, third quartile; RR, rate ratio; SD, standard deviation.

Secondary Outcomes: Stable Housing and Employment

Two pre-specified secondary outcomes related to participants' stability at follow-up were derived from the 6-month follow-up survey: stable housing and employment. As Table 4 shows, neither the stable housing model (OR: 0.86; CI 0.36 to 2.06; p = 0.740) nor the employment model (OR: 0.60; CI 0.20 to 1.83; p = 0.363) provided evidence of an effect.

Table 4. Housing stability and employment at six months

Outcome	Control (n = 73)	Intervention (n = 73)	Odds ratio (95% CI)	P value
Stable housing	17/42 (40.5%)	19/51 (37.3%)	0.86 (0.36 to 2.06)	0.740
Missing follow-up response	31/73 (42.5%)	22/73 (30.1%)	—	—
Employment	14/42 (33.3%)	9/50 (18.0%)	0.60 (0.20 to 1.83)	0.363
Missing follow-up response	31/73 (42.5%)	23/73 (31.5%)	—	—

Outcome counts are observed n/N (%) among participants with nonmissing responses; missing-response rows use the full randomized arm denominator of 73. Descriptive percentages are not imputed. Both effect estimates use all 146 randomized participants, estimated via multiple imputation under a missing-at-random assumption. The stable-housing model includes randomized group only; the employment model also adjusts for baseline employment. CI, confidence interval; —, not applicable.

We also modeled the interaction between housing and employment by randomized arm. The relationship between employment and housing at follow-up differed between arms. In

the control arm, employed participants had an estimated 0.36 times the odds of stable housing compared with non-employed participants (CI 0.08 to 1.69). In the intervention arm, employed participants had an estimated 17.63 times the odds of stable housing compared with non-employed participants (CI 1.82 to 170.97), because nearly all participants who achieved employment also achieved stable housing. This resulted in a ratio of ORs of 48.9 (p = .004). The pattern held in an observed-outcome restricted sensitivity analysis.

Secondary Outcomes: Quality of Life

We also pre-specified a secondary outcome related to a change in participants' quality of life. We administered the WHO HIV Quality of Life brief version at baseline and follow-up, which assesses quality of life for PLWH along six domains. As seen in Table 5, none of the six domains nor the two general items showed evidence of an effect.

Table 5. Quality of life at six months

WHOQOL-HIV BREF outcome	Effect	Estimate (95% CI)	P value	Holm-adjusted P value
Physical	AMD	-0.07 (-1.37 to 1.22)	0.912	1.000
Psychological	AMD	-0.71 (-1.83 to 0.41)	0.211	1.000
Level of independence	AMD	-0.24 (-1.47 to 1.00)	0.704	1.000
Social relationships	AMD	-0.92 (-2.45 to 0.60)	0.230	1.000
Environment	AMD	-0.05 (-1.19 to 1.10)	0.935	1.000
Spirituality, religion, and personal beliefs	AMD	0.03 (-1.30 to 1.36)	0.963	1.000
Overall quality of life	POR	0.90 (0.45 to 1.79)	0.760	0.760
General health	POR	0.71 (0.35 to 1.45)	0.344	0.688

All analyses include 146 randomized participants using full-cohort item-level multiple imputation under a missing-at-random assumption. Domain scores range from 4 to 20, with higher scores indicating better quality of life; domain effects are adjusted mean differences from baseline-adjusted ANCOVA. The two general items use baseline-adjusted ordinal models and proportional odds ratios for higher response categories. AMD, adjusted mean difference; ANCOVA, analysis of covariance; CI, confidence interval; POR, proportional odds ratio; WHOQOL-HIV BREF, World Health Organization Quality of Life HIV brief instrument.

Transportation Intervention Use

The primary and secondary analyses were ITT and used the entire sample. Given the number of null findings, we examined whether participants in the intervention group used the intervention. The local transit authority provided pass records, which we used to determine the number of rides and days used. 68 (93.2%) participants of the 73-person intervention sample used the pass during the 6-month observation period. The median participant rode 43 of the approximately 180 days (IQR: 16 to 79). When restricted to the

intervention group's outcomes, there was no statistical evidence of a dose-response relationship between pass-use frequency and outcome availability, viral suppression, or service usage (Table 6).

Table 6. Transportation pass use and exploratory associations within the intervention arm

A. Pass uptake and frequency

Measure	N	Descriptive result
Any recorded pass use	68/73	93.2% (95% CI 84.9 to 97.0)
Transaction days, all intervention participants	73	Mean 53.5 (SD 42.9); median 43.0 (IQR 16.0 to 79.0); range 0 to 142
Transaction days, participants with any use	68	Mean 57.5 (SD 41.9); median 46.5 (IQR 17.8 to 86.5); range 1 to 142

B. Pass-use frequency and outcome availability

Outcome	N	Effect	Estimate (95% CI)	P value	Holm-adjusted P value
Eligible follow-up viral-load result	73	OR	1.12 (0.97 to 1.29)	0.115	0.574
Follow-up survey completed	73	OR	1.03 (0.91 to 1.16)	0.664	1.000
Stable-housing outcome observed	73	OR	1.04 (0.92 to 1.17)	0.524	1.000
Employment outcome observed	73	OR	1.01 (0.90 to 1.14)	0.821	1.000
Any follow-up QOL item response	73	OR	1.03 (0.91-1.16)	0.664	1.000

C. Pass-use frequency and clinical and service outcomes

Outcome	N	Effect	Estimate (95% CI)	P value	Holm-adjusted P value
Viral suppression	73	OR	1.06 (0.94 to 1.19)	0.342	0.635
Recorded meal units	73	RR	1.04 (0.98 to 1.11)	0.173	0.518
Case-management assists	73	RR	0.93 (0.82 to 1.07)	0.317	0.635

All analyses are restricted to the intervention arm. Pass uptake is at least one recorded transaction day during the six-month benefit window; its 95% CI is a Wilson score interval without continuity correction. Frequency is the number of distinct days with any recorded transaction. The all-participant summary includes nonusers. Association estimates are per 10 additional transaction days. Panel B uses unadjusted logistic regressions for outcome availability. In panel C, viral suppression uses logistic regression adjusted for baseline viral suppression, with unavailable viral-suppression outcomes coded as negative outcomes. Meal units and case-management assists use negative-binomial regressions. Holm adjustment is applied separately to the five availability tests and the three clinical/service-outcome tests. CI, confidence interval; IQR, interquartile range; OR, odds ratio; QOL, quality of life; RR, rate ratio; SD, standard deviation.

Discussion

The results from this RCT did not provide evidence that providing unrestricted transportation passes improved viral suppression during the six-month follow-up period. There was also no evidence that these null results were due to participants not using the pass, or that there was a dose-relationship between pass use and viral suppression.

While most transportation interventions are designed to get clients to specific appointments, this intervention tested unrestricted public transit mobility that participants could use to meet multiple transportation barriers in their lives. While the intervention did not affect many of the pre-specified outcomes of interest, one pre-specified outcome showed a significant difference between the groups. Participants in the intervention group saw their case managers at a rate 2.24 times higher than those in the control group, after accounting for baseline contacts. However, the number of meals received from the organization's meal program did not increase.

We theorize that these services represent different kinds of needs that transportation barriers might influence differently. Food is a basic need. If someone is hungry, they might do whatever they can to obtain free food that day. Participants in both arms may have prioritized accessing food, even if they did not have transportation access. Furthermore, participants with the transit pass may have used it to access other food services or to allocate money toward food they would otherwise have spent on transportation, potentially reducing the number of meals they ate at the organization.

Conversely, case management is a client-driven, relationship-based service. Clients identify, with guidance from case managers, the items they want to work on, such as securing benefits, housing, medical care, employment, or other needs. Providing transportation may have reduced the logistical burden of remaining in contact with case managers. Alternatively, participants might have felt more empowered to engage in their work plans with their case managers if they had transportation that helped them achieve their goals.

There was no evidence of an effect on housing and employment, either individually or as a joint outcome. However, there was an interaction effect: housing and employment stabilization were more strongly clustered in the intervention group than in the control group. One possibility is that transportation created conditions that made progress in these domains mutually reinforcing, such as financial reallocation, increased geographic flexibility, or easier coordination of these activities. We caution that this analysis was exploratory. Future work should examine the potential robustness of stabilization outcomes.

While we believe that our work aligns with the broader literature on the importance of transportation as a structural determinant of health [34], our findings run counter to many studies suggesting that transportation passes would be an effective intervention for improving HIV clinical outcomes [24–27]. As a randomized controlled trial, our study provides the most rigorous evidence to date that isolates the effects of unrestricted transportation intervention. We find no evidence of an effect on the near-term clinical outcomes. However, increased engagement in case management services and exploratory outcomes in housing and employment stabilization are both associated with viral suppression. Stability leads to long-term health; the current study might not have followed participants long enough for clinical outcomes to emerge.

This work suggests that more research is needed to identify which services are most affected by transportation barriers and the mechanisms by which these barriers affect well-being, independence, and stabilization. While transportation access might increase use of some services, it might decrease use of others by increasing choice and independence and freeing up finances for other needs. More work is needed to square these results with the lack of evidence of an effect on quality of life between the groups.

Additionally, prior observational studies have associated MCM participation with greater engagement in HIV care and improved virologic outcomes [15]. Although findings did not establish that additional case management contacts improved clinical outcomes, it does have implications for HIV service use. The higher rate of case management contacts suggests that unrestricted transportation may help PLWH engage with RWHAP MCM. Because medical case managers coordinate HIV care, support treatment adherence, and help clients obtain needed health and supportive services, improved access to case management may be a meaningful intermediate step through which transportation access could influence engagement in HIV care and longer-term clinical outcomes.[13].

The increased engagement with medical case management observed in this study may have implications beyond HIV care. People managing other chronic conditions frequently require repeated healthcare visits, laboratory monitoring, access to medication, and coordination across health and social service systems. Additionally, transportation barriers are reported more frequently among patients with chronic disease and are associated with increased emergency department use and higher all-cause mortality across a range of age groups and diseases [35]. For these reasons, the observed increase in MCM use suggests that unrestricted transportation may help people with chronic conditions manage their health by improving access not only to medical appointments, but also to case managers, care coordinators, food resources, public benefits, and other supportive services that support ongoing disease management.

Our study may also have implications for services supporting people experiencing homelessness. Qualitative interviews with formerly homeless people and homeless service providers demonstrate that transportation is important for accessing essential resources such as food and healthcare, as well as the public benefits and supportive services needed to exit homelessness[12, 36]. Although the clustering of employment and stable housing in the intervention group was exploratory, it is consistent with the possibility that unrestricted transportation facilitates access to the resources and opportunities that support housing and employment stability. Similarly, increased MCM engagement in our study suggests that unrestricted transportation may increase engagement with providers who help clients access housing, employment, public benefits, and other supportive services. If this effect extends to people experiencing homelessness without HIV, increased engagement with case management could be one mechanism through which unrestricted transportation benefits could support housing stabilization.

There are several limitations to consider when integrating this study's evidence into the field. First, blinding was not possible because the transportation pass intervention required staff management, including replacing lost passes. This additional staff engagement might have had an independent effect on the results outside of the pass itself, though there was no evidence to suggest this. Second, the effect of the transportation pass on outcomes might not have appeared in this study because it was smaller than the effect size this study was powered to detect. Similarly, viral suppression outcomes might change on longer timescales. Third, the single-site design means that these outcomes might have been unique to this organization, or sensitive to the kinds of administrative data to which we had access. These limitations suggest that a larger, multi-sited RCT that follows participants for multiple years would provide more definitive evidence, especially since some results point towards near-term effects that could lead to viral suppression.

However, this project had many strengths. As a randomized controlled trial, we were able to control for many baseline correlates that might otherwise obscure the potentially subtle effects of transportation. Additionally, while we collected participants' perspectives through surveys, the main outcomes were derived from administrative data, enabling a more objective assessment of service engagement and clinical outcomes. Finally, this study was embedded within a holistic social service organization, which allowed a high participant retention rate for this hard-to-reach population. Recruitment and screening were both quick and high, with all 146 participants recruited within 6 months. Researchers would do well to embed more research within existing social service infrastructure.

Conclusion

In conclusion, our study contradicts a body of evidence on transportation barriers that suggested transportation passes as an effective intervention for improving HIV health outcomes. Transportation remains an important structural determinant of health. However, we found no evidence within our follow-up window that it led to clinical changes. Case-management service engagement was higher. There was also an interaction between housing and employment stabilization, with these outcomes clustering more strongly in the intervention group. These two effects might have an influence on clinical HIV outcomes at longer timescales. Future research should continue to explore ways to alleviate transportation barriers to health and to examine transportation passes as a low-cost intervention to improve certain forms of service engagement.

Declarations

Ethics approval and consent to participate

This study was approved by WCG IRB on July 2, 2024 (IRB tracking number 20242659; study number 1377510). Written informed consent was obtained before participation.

Consent for publication

Not applicable.

Availability of data and materials

The datasets generated and analyzed during this study cannot be shared, including in deidentified form, because sharing is prohibited under the terms of the study's institutional review board approval and participants' informed consent. The protocol is available upon request from the corresponding author.

Competing interests

The authors declare that they have no competing interests.

Funding

This study was funded by Gilead Sciences through its investigator-sponsored research program (sponsor protocol number IN-US-985-7161). Gilead Sciences had no role in study design, data collection, analysis, interpretation, manuscript preparation, or the decision to submit for publication. Gilead Sciences was provided the manuscript before submission and offered no comments.

Authors' contributions

JO conceived and designed the study, secured funding, supervised and conducted recruitment and data collection, conducted the analyses and literature review, and drafted substantial portions of the manuscript. GS contributed to the design of data collection instruments, conducted recruitment, assisted with data cleaning and analysis, and drafted a section of the manuscript. NB conducted recruitment and literature review and drafted sections of the manuscript. ZG conducted the literature review, drafted sections of the manuscript, and critically reviewed the manuscript and statistical analysis. All authors read and approved the final manuscript.

Acknowledgements

The authors thank their colleagues at The Aliveness Project who made this work possible or provided helpful feedback, including Joe Stansbury, Matthew Hartle, George Froehle,

James McMurray, and Matt Toburen. We also thank Metro Transit, especially Nate Frisbie-Krogh, for their assistance and support of this project, and Lora Alexander for her welcome support, encouragement, and feedback. We are grateful to the study participants for sharing their time and experiences.

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