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A New Approach to Measuring Partnership Concurrency and its Association with HIV Risk in Couples

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Abstract

Empirical estimates of the association between concurrent partnerships (CP) and HIV risk are affected by non-sampling errors in survey data on CPs, e.g., because respondents misreport the extent of their CPs. We propose a new approach to measuring CPs in couples, which permits assessing how respondent errors affect estimates of the association between CPs and HIV risk. Each couple member is asked (1) to report whether s/he has engaged in CPs and (2) to assess whether his/her partner has engaged in CPs, since their couple started. Cross-tabulating these data yields multiple classifications (with varying combinations of sensitivity/specificity) of the CPs of each couple member. We then measure the association between CPs and HIV outcomes according to each classification. The resulting range of estimates is an indicator of the uncertainty associated with respondent errors. We tested this approach using data on 520 matched couples drawn from the Likoma Network Study. Results suggest that existing tests of the concurrency hypothesis are affected by significant uncertainty.

Keywords

Concurrent partnerships; HIV transmission; Measurement error; Survey data; Malawi; HIV prevention

Introduction

The role of concurrent partnerships (CPs) in driving HIV transmission in sub-Saharan countries has been heatedly debated [1–5]. CPs are sexual partnerships that overlap in time, whereas “serial” relationships are consecutive relations that are separated in time by a gap [6]. Theoretical models suggest that CPs can accelerate the spread of HIV because they increase the size of the sexual networks that connect members of a population at any point in time [7]. In doing so, they interact with biological factors that affect the transmissibility of the virus. In particular, CPs increase the chance that a seronegative individual comes into contact with a recently infected individual while the latter is still in the high-infectivity and often asymptomatic phase of acute infection [8, 9]. In contrast, the time gap between serial partnerships is generally long enough that the new partnership only begins after an individual has progressed into the chronic infection phase, and is thus less infectious.

Despite these plausible causal pathways between CPs and HIV spread, empirical investigations of the “concurrency hypothesis” have obtained conflicting results. Some studies have found an association between CPs and HIV prevalence [10–12] or HIV serodiscordance in couples [13], but others have failed to find an association between CPs and various measures of HIV risk. In a comparative study of 4 African cities [14], the highest rates of partnership concurrency were found in cities with the lowest levels of HIV prevalence. The relationship between HIV prevalence and the practice of polygyny (an institutionalized form of concurrency) also appears to be negative, both in ecological [15] and individual-level [16] studies. Finally, in a recent South African study, the incidence of HIV among women was not significantly higher in communities with a high prevalence of CPs among men [17].

Some commentators have suggested that the debate about the role of CPs in sub-Saharan epidemics should be “put to rest” [18, 19], but there are various reasons that may explain why an empirical association between CPs and HIV risk has not yet been found in SSA. These reasons include, first, gaps in our theoretical understanding of the effects of CPs. Indeed, the mathematical models, which initially suggested that CPs had a dramatic impact on epidemic growth, did not consider correlated factors that may amplify, mitigate or even offset the effects of CPs. Such factors include, for example, the presence of STDs [20], reduced condom use in CPs [21], male circumcision [22, 23], concomitant changes in sexual mixing patterns [24, 25] or decreased frequency of sexual intercourse during CPs [15, 26, 27].

Second, the lack of association between CPs and HIV risk in empirical studies may be due to inadequate study designs. For example, most empirical studies seek to measure the association between CPs and the risk of HIV acquisition faced by the individual who engages in CPs him/herself [14, 28]. Morris [29] argued that such designs failed to capture the specific effects of CPs: CPs increase the risk of HIV transmission to uninfected partners not the risk of HIV acquisition of the person who engages in CPs [7, 30]. In order to capture the specific effects of CPs on HIV transmission, rather than the effects of having multiple partnerships (either serially or concurrently) on HIV acquisition, epidemiological studies must thus measure HIV outcomes among the partners of individuals who engage in CPs. Several US studies of the impact of CPs on STIs have followed this approach (e.g., [31, 32]). In sub-Saharan settings, on the other hand, few studies of CPs and HIV risk include HIV outcomes measured among partners. Tanser et al. [17] is a notable exception.

Finally, the lack of association between CPs and HIV risk may be due to the poor quality of data on partnership concurrency. Until recently, empirical studies of CPs and HIV risk frequently used different definitions of concurrency [2]. This limited the interpretation of observed associations between measured CPs and HIV. It also prevented comparisons of study results across multiple populations. UNAIDS addressed this issue, by recommending that CPs be defined as “overlapping sexual partnerships where sexual intercourse with one partner occurs between two acts of intercourse with another partner” [33].

On the basis of this definition, CPs can be measured empirically using two different approaches. The “direct” approach entails asking respondents whether they have had sex

with any other person while they were still sexually involved in a relationship with a recent partner. For example, respondents may be asked whether they had more than one ongoing partnership at the time of the survey or whether they had other sexual partners during their marriage with their current spouse. The “indirect” approach, on the other hand, relies on the comparison of the reported dates of first and last sex in each of the most recent partnerships of a respondent [34]. If the partnership intervals defined on the basis of those dates overlap, then partnerships are classified as concurrent. Using this strategy, respondents are not asked to explicitly admit to having several ongoing relationships at a given point in time. Instead, they are classified as having CPs or not by the researcher, at the analysis stage. UNAIDS recently recommended the indirect approach to measuring the prevalence of CPs in a population [33], because it may limit social desirability biases in CP data [34] and also because it permits calculating the duration of partnership overlap—an important determinant of the impact of CPs on HIV spread [30, 35, 36].

Unfortunately, both measurement approaches are imperfect. The indirect measure of CPs, for example, is affected by large amounts of missing data in reports of dates of first and last sex [37], as well as date heaping [34]. The sexual partnership histories on which it builds are also highly unreliable and incomplete. In studies of couples where both partners are interviewed, disagreements about start and end dates of a relationship are frequent [38–40]. Similarly, network studies during which all members of a population are interviewed show that some sexual partnerships (e.g., recent short-term relations) are frequently omitted in sexual partnership histories collected during survey interviews [40, 41]. By comparison, the direct approach to measuring CPs elicits more complete data [37] and may be more reliable [40]. However, it does not permit measuring the duration of overlap between CPs. When used to measure the prevalence of concurrency at the time of the survey, it may also be affected by issues of censoring [42]: respondents may report being in ongoing relationships with partners with whom they will in fact never have sex again. In this context, the direct concurrency measure may over-estimate the prevalence of CPs [33].

The consensus among HIV epidemiologists and demographers is thus that the predictive value of survey data on CPs is likely low [43]. Respondents who truly engaged in CPs may be misclassified as being in serial relationships according to survey data (low specificity), whereas respondents engaged in serial relations may be mistakenly classified as having CPs (low sensitivity). The pattern of errors in survey reports of CPs may also be highly gendered. Men are thought to “swagger” during interviews about their sexual behaviors [41], so their survey reports of CPs may yield a (possibly large) number of false positives (i.e., men in serial partnerships are wrongly classified as having CPs). On the other hand, women are more “secretive”, so their survey reports of CPs may include false negatives (i.e., women in CPs are wrongly classified as being in serial relationships). This may not always be the case, however: men (particularly when married) may also under-report the number of CPs they engage in [40, 44].

Several methodological studies conducted in sub-Saharan settings have investigated how such reporting errors may affect estimates of CP prevalence. In doing so, they have suggested that the UNAIDS-recommended indicator likely under-estimates the extent of CPs in populations where HIV is hyper-endemic [40, 44–47]. In the absence of a gold

standard measurement of CPs (e.g., a biomarker) however, no study has investigated how reporting errors affect measures of the association between CPs and HIV risk in empirical studies. This is so even though the effects of measurement errors on tests of the “concurrency hypothesis” are likely complex. On the one hand, misclassifications of CPs in survey data may lead to “attenuation bias” when the frequency of misclassification does not vary by HIV status of the respondent and/or his partner. In this case of “non-differential” errors, the odds ratio (or other measures of relative risk) is biased towards the null hypothesis of no effects of CPs on HIV risk [48, 49]. Non-differential errors in survey reports of CPs could thus largely explain the lack of empirical association between CPs and HIV risk in sub-Saharan studies [50]. On the other hand, errors in survey reports of CPs may also vary by HIV status of the respondent or that of his/her partner. For example, respondents who are aware that they are infected with HIV may be less likely to report having engaged in CPs to avoid stigma. Alternatively, respondents who acquired HIV may be more likely to infer from their own serostatus that their spouse/partner engaged in CPs and subsequently transmitted HIV to them. Such “differential” misclassifications may lead to under or over-estimates of the association between CPs and HIV risk [51, 52].

We propose a new approach to measuring CPs in couples. It permits assessing the impact of reporting errors on empirical tests of the concurrency hypothesis, even in the absence of a gold standard measure of CPs. It requires a sample of “matched” couples, i.e., couples in which both members are interviewed (e.g., [53, 54]). Both partners in the matched couple are asked two simple questions. First, they are asked whether they, themselves, had another partner (i.e., a CP) during the course of their relationship with the matched partner. Then they are asked to assess how likely it is that their matched partner had another partner (i.e., a CP) during the same timeframe. By cross-tabulating these reports on the CPs of each partner, we can devise multiple CP classifications, which have varying combinations of sensitivity and specificity in capturing CPs. We then measure the association between CPs and HIV outcomes according to each of these classifications. The highest and lowest estimates obtained through this analysis form “bounds”, which quantify the uncertainty linked to reporting errors in estimates of the association between CPs and HIV risk. These bounds differ from confidence intervals (CIs) in that they capture non-sampling errors due to misreporting of CPs, whereas CIs only capture sampling variation.

Samples of matched couples are increasingly enrolled in studies of the risk factors of HIV infection, e.g., in the demographic and health surveys (DHS, e.g., [55]), in cohort studies of the factors of HIV transmission (e.g., [56]), or in intervention trials (e.g., [57]). Questions similar to the ones we propose have been asked previously in couples' surveys in the US (e.g., [58]) and in sub-Saharan settings [53, 59, 60]. They have however only been used to investigate whether individuals are aware of their partners' sexual networks [58, 61] or assess whether partners agree on reports of couple-level risk behaviors, e.g., date of last sex or condom use [53, 59, 62, 63]. They have not been used to improve the measurement of CPs and inferences about the effects of CPs on HIV outcomes.

Data and Methods

Data Sources

We implemented this approach during the second round of the Likoma Network Study (LNS), a study of the networks of heterosexual relationships that connect the population of a small island of Lake Malawi [64, 65]. All inhabitants aged 18–49 years old were asked to (1) list their 5 most recent sexual partners using audio computer-assisted interviewing (ACASI, [64]), and (2) answer questions about their relationship with each of these partners. All nominated partners were identified in rosters obtained through a household census, in order to reconstruct the population-level sexual networks of the island [64, 65]. In 520 sexual relationships, both partners were interviewed. We refer to these relationships as “matched relationships” or “matched couples”. They constitute the primary analytical sample of this paper. We describe the selectivity of the matched relationships compared to the rest of the sexual networks elicited in Likoma [65].

Couples' Reports of Concurrent Partnerships

During the LNS, respondents were first asked (yes/no) whether they—themselves—had another partner (i.e., a CP) during each of the relationships they listed. They were then asked to assess how likely it was that their partner had another partner (i.e., a CP) during their relationship with the respondent. Respondents were given 4 answer options for this question: they could answer either “yes”, “yes, I suspect”, “I don't know”, or “no”. These two questions are direct assessments (i.e., they do not rely on date reports) of the cumulative prevalence of CPs during the course of the matched relationship. They do not permit estimating the duration of overlap between matched couples and CPs because respondents were not asked to estimate how long they—themselves—or their matched partner had been in a relationship with a CP. In addition, the dates of first and last sex during each relationship were only ascertained using broad categories, e.g., within last month/within last year/more than a year ago. From such data, we thus cannot estimate the duration of overlap between two partnerships with precision.

If both the male partner (noted m) and the female partner (noted f) reported that m had CPs during their relationship, then we say that m had “known CPs”. If m reported having CPs during his relationship with f , but f either reported that m did not have CPs, didn't know whether m had CPs or only suspected that m had CPs, we say that m had “secret CPs”. If m did not report CPs, but f reported that m had CPs during their relationship, we say that m had “divulged” CPs. If m did not report CPs, but f suspected that m had CPs during their relationship, we say that m had “suspected” CPs. If m did not report CPs, but f didn't know whether m had CPs during their relationship, we say that m had “possible” CPs. Finally, if both m and f reported that m did not have CPs during their relationship, we say that m was “faithful”. We created similar CP categorizations for the female partners in matched relationships.

Using the CP categorizations defined above, we then derived 5 binary classifications of each partner according to whether or not they had any CP during the course of the matched relationship (Table 1). In the narrowest classification, we considered that only men or

women categorized as having “known CPs” actually had CPs. We hypothesize that this classification likely has high specificity but low sensitivity in detecting CPs among survey respondents: a (possibly large) number of individuals who engaged in CPs will be misclassified as having been in a serial relationship. On the other hand, few will be misclassified as having been in CPs when in fact their relationship was serial. In the broadest classification, we considered that only men or women categorized as faithful did not have CPs. Those having either known, secret, divulged, suspected or possible CPs were all classified as having CPs. We hypothesize that this classification likely has high sensitivity but low specificity, i.e., a number of individuals who did not engage in CPs during the matched relationship will be misclassified as having had CPs. The other intermediate classifications are defined in Table 1 below. Classification #2 constitutes the classification derived from survey data self-reported by individuals. We first describe the distribution of known, secret, divulged, suspected and possible CPs among members of matched couples, by gender. Then we estimate the prevalence of CPs according to each CP classification in Table 1.

Validating Couples' Reports of Concurrent Partnerships

In the absence of a gold standard measure of CPs, we used data reported by other LNS participants (i.e., outside of the matched couple) as a proxy against which to evaluate CP classifications obtained from couples' reports. Indeed, because the LNS included all 18–49 years old inhabitants of Likoma in its sampling frame, the CPs that an individual had engaged in during a matched relationship were potentially also reported by other LNS participants with whom s/he engaged in those CPs (Fig. 1). We thus defined a dummy variable for each partner in a matched relationship (“network-reported CPs”), which takes value 1 if the partner was reported by other network members (i.e., outside of the matched relationship) and 0 otherwise. The network-reported data is not a gold standard because (1) not all network members were interviewed during the LNS (e.g., because of absence, refusals or because they resided on the mainland), and (2) some interviewed network members may not have reported all their sexual partnerships [40, 64]. Some CPs of individuals in matched couples may be missed by network-reported data. In comparing couple-reported and network-reported data on CPs, we thus call *pseudo-sensitivity* the number of individuals classified as having CPs by couples' reports divided by the number of individuals reported as a partner by other LNS participants. We call *pseudo-specificity* the number of individuals classified as not having CPs by couples' reports divided by the number of individuals who were not reported as a partner by other LNS participants. We calculate the pseudo-sensitivity and pseudo-specificity of each CP classification.

Measuring the Association Between Couple-Reported CPs and HIV Outcomes

We used data from two home-based HIV testing and counseling campaigns (HTC [66, 67]) to determine the HIV status of each partner in a matched couple. The variable describing HIV status of each partner is coded as infected = 1 versus not infected = 0. Individuals with indeterminate test results were coded as not infected. We then describe the selectivity of the HIV testing and counseling campaigns by comparing matched relationships in which the male/female partner was tested to other matched relationships. To do so, we used χ^2 tests of association for categorical variables and *t* tests for continuous variables (e.g., age). Finally,

among matched relationships for which HIV outcome data were available for at least one of the two partners, we measured the association between CPs and HIV infection according to each CP classification. We considered HIV status separately by gender and we tested whether it was associated with the CPs of one's partner (rather than one's own CPs). In doing so, we improved on previous studies, which measured solely the association between CPs and HIV outcome of the person engaging in CPs [29].

We used logistic regression models, with controls for partner and relationship characteristics that may be associated with HIV outcomes and the likelihood that one's partner engaged in CPs. Partner characteristics included continuous variables describing the age of the male and female partners. Relationship characteristics included dummy variables indicating whether the relationship was current at the time of the survey, whether it was marital, whether both partners were residents of the same household, and whether the relationship had started more than a year prior to the survey. Standard errors were adjusted for clustering of observations by individual (i.e., one individual may have been engaged in multiple matched relationships).

Our analyses of the association between CPs and HIV outcomes are illustrations of the use of the new measurement method, rather than true tests of the concurrency hypothesis. This is so because of data limitations. The LNS indeed only includes data on prevalent HIV infections rather than incident HIV infections. As a result, it is not possible to ascertain precisely when those who tested positive acquired HIV infection. For example, when measuring the association between prevalent HIV infection among women and the CPs of their matched male partner, observed infections may not only be attributable to the CPs of their matched male partner. Women may also have acquired HIV infection (1) prior to the start—or after the end—of the matched relationship, (2) during the matched relationship but prior to the time when their matched male partner engaged in CPs, and (3) during the matched relationship but from an external partner rather than from the matched partner.

Results

The matched relationships included in this study represented less than one quarter of all relationships constituting the Likoma sexual networks (Fig. 2). Compared to the rest of the networks, the matched relationships were more frequently marital (418/520, 80.4 %) and ongoing at the time of the survey (432/520, 83.1 %). They had also often started more than a year prior to the survey (413/520, 79.4 %), and frequently included partners who co-resided in the same household (309/520, 59.4 %). The sample of matched relationships we analyzed included 472 men and 505 women. Among men, 425 were involved in only one matched relationship (90.0 %), 46 were involved in 2 matched relationships (9.8 %) and 1 was involved in 3 matched relationships (0.2 %). Among women, 491 (97.2 %) were involved in one matched relationship, vs. 13 (2.6 %) in 2 matched relationships and 1 (0.2 %) in 3 matched relationships.

Women were categorized as faithful in 28.1 % of matched relationships (Table 2) whereas this was the case for only 17.6 % of men. Women had possible CPs in 23.3 % of matched relationships, suspected CPs in 25.6 %, divulged CPs in 8.7 %, secret CPs in 12.7 %, and

known CPs in 1.6 %. For men, these proportions were 15.7, 22.4, 15.5, 18.9 and 9.9 %, respectively.

The prevalence of CPs among women in matched relationships ranged from 1.6 to 71.9 % depending on the classification used (Table 3). The range stretched from 9.9 to 82.4 % among men. In all classifications, males were more likely to be classified as having had CPs than women. The pseudo-sensitivity of CP classifications of the female partner increased from 2.7 % (classification #1) to 74.1 % (classification #5), whereas the pseudo-specificity declined from 98.8 % (classification #1) to 28.7 % (classification #5, Table 3). Among male partners, the pseudo-sensitivity of CP classifications increased from 13.6 % (classification #1) to 88.7 % (classification #5), whereas the pseudo-specificity declined from 92.1 % (classification #1) to 20.8 % (classification #5).

Women in 464 matched relationships were tested for HIV (88.8 %). There were few significant differences in partner and relationship characteristics between matched relationships in which the female partner was tested and matched relationships in which she was not (Table 4). Men were tested for HIV in 377 matched relationships (72.5 %). Similarly, there were few significant differences in partner and relationship characteristics between matched relationships in which the male partner was tested and matched relationships in which he was not. In particular, the age difference between partner was larger in relationships in which the man was not tested ($p = 0.02$). Men were infected in 43 relationships (11.4 %), and women were infected in 67 matched relationships (14.4 %).

The estimated odds of HIV infection among women were not significantly associated with their matched male partner's CPs (Fig. 3). There was however large variation in the point estimates of the odds ratio, with uncertainty bounds ranging from 0.81 in classification #2, to 1.82 according to classification #5. The risk of HIV infection among men in matched relationships was significantly associated with the CPs of their female partner in only one of the 5 CP classifications (Fig. 3, classification #5). Across classifications #1–#4, the odds ratio of male HIV infection associated with CPs of their partner ranged from 1.85 (classification #4) to 2.5 (classification #1). In the least sensitive classification however, the odds ratio of male HIV infection associated with CPs of their matched female partner increased to 3.39 (95 % CI 1.18–9.73).

Discussion

In this study, we proposed a new approach to measuring concurrent partnerships in couples. This approach relies on two simple survey questions, which ask each partner in a couple (a) to report whether they—themselves—had CPs during a given relationship and (b) to rate how likely it is that their partner had CPs during this relationship. By cross-tabulating answers to these questions, these data then permit constructing multiple classifications of the CPs of each partner. The most restrictive CP classification has high specificity but low sensitivity in capturing CPs, as shown by comparison with the sexual network data collected on Likoma Island. In contrast, the least restrictive classification has high sensitivity but low specificity. This measurement approach thus improves on current approaches to CP measurement because it permits quantifying the uncertainty in (a) measures of the

prevalence of the CPs and (b) estimates of the association between CPs and HIV risk. Such uncertainty is due to respondent errors, rather than sampling errors. It can thus not be reduced by increased sample sizes.

In our application of the couples' approach to measuring CPs and their association with HIV outcomes using the Likoma Network Study, we found fairly wide uncertainty bounds around concurrency parameters. The classification of CPs based on self-reported survey data alone often yielded low odds ratio estimates, whereas higher estimates were obtained using more sensitive but less specific classifications of CPs. For example, we found a large and statistically significant association between female CPs and the risk of prevalent HIV infection among their male partners when using classification #5, whereas lower estimates were obtained using classification #1 through #4 (Fig. 3). The uncertainty bounds surrounding estimates of the association between male CPs and female HIV risk, on the other hand, spanned a range from OR = .82 (i.e. protective) to OR = .81 (i.e., risk-enhancing). This application of our new measurement method thus suggests that the results of current empirical tests of the concurrency hypothesis (e.g., 14, 17) should not be over-interpreted. The null results obtained in those tests may be attributable to misclassifications in survey data on CPs, rather than to a true lack of association between CPs and HIV risk. The calculation of uncertainty bounds as described in this paper should be incorporated in future tests of the concurrency hypothesis.

Our study has a number of limitations. *First*, the results we present are not a formal test of the concurrency hypothesis because our data only include measures of prevalent HIV infections. They do not permit characterizing HIV transmission events with precision. This would require longitudinal follow-up to measure HIV incidence [17] or genetic linkages of viral strains [2, 68]. Our estimates of the association between CPs and HIV infection (Fig. 3) are thus potentially affected by disconnects between the period during which an individual may have become infected with HIV and the period during which CPs are measured [69, 70]. Some of the infected partners in matched relationships may have been infected prior to entering that relationship or after the matched relationship ended. They may also have been infected by their own CPs, rather than by their partner's CPs. The odds ratios we show in Fig. 3 thus illustrate the uncertainty in estimates of the association between CPs and HIV risk linked to measurement error, rather than measure the true effects of CPs on HIV transmission. *Second*, in the absence of a gold standard measurement of CPs, we cannot estimate the true sensitivity/specificity of each classification. As a result, we still cannot rule out that CPs are more/less prevalent than what we observe in each of the CP classifications defined in Table 1. The uncertainty attributable to respondent errors in estimates of the association between CPs and HIV risk can thus be larger than we estimate here. *Third*, the sample of matched samples drawn from the LNS is comprised primarily of stable relationships (e.g., marital). Such relationships may not be representative of the broader set of relationships during which HIV may be transmitted (e.g., casual relationships). Future studies should include larger samples of short-term non-marital relationships and test whether the effects of CPs on HIV risk vary across types of relationship. *Fourth*, the couples' approach to measuring CPs currently only includes a limited number of CP classifications ($n = 5$). This is so because the survey questions we used allowed only a small

number of possible answers. We could potentially devise additional classifications if respondents were asked to rate the likelihood of CPs during a relationship on a more detailed scale (e.g., by attributing a score from 1 to 10, with 1 representing the lowest possible likelihood of CPs and 10 representing the highest possible likelihood). The use of more detailed response categories may also help elicit respondents' perceptions of their matched partner's sexual networking more accurately [71, 72]. *Fifth*, we used a direct measure of CPs, which does not allow measuring the duration of overlap between CPs, even though this parameter is an important dimension of HIV transmission dynamics [7, 9, 36]. Future investigations of this measurement approach should thus be extended to include questions about the duration of overlap between matched relationships and CPs. *Sixth*, the couples' approach to measuring CPs may not allow applying calibration techniques commonly used in epidemiology to obtain unbiased estimates of relative risks in the presence of measurement errors (e.g., [73]). This is so because such techniques usually require either a gold standard measurement or two imperfect but independent measurements of the predictor of interest. Couples' reports of CPs, on the other hand, cannot be considered independent because partners may discuss their partnership(s) and other HIV-related risks with each other. They may also share common attitudes towards reporting behaviors during surveys. *Seventh*, our validation approach requires data on CPs reported by other network members. Such data may rarely be available outside of a small number of populations where network studies have been conducted. This should not however preclude the use of the couples' approach to CP measurement in other settings. Measurement tools and approaches are frequently validated in a small number of test settings where reference data are available before being adopted more broadly. Finally, some of the variation in estimates of the association between CPs and HIV risk may be due to the incorrect identification of matched couples, i.e., instances where 2 partners are believed to be linked in a matched couple when in fact they have not been in a relationship [64].

Conclusion

Despite these limitations, this new approach to measuring CPs can significantly strengthen our inferences about CPs and HIV risk. Couples' datasets are increasingly collected in a number of contexts including (a) large population-based surveys (e.g., the Demographic and Health Surveys, [54]), (b) small-scale intensive health and demographic surveillance systems (HDSS, [74]) and (c) cohort studies of couples initiated to evaluate the effects of various interventions on HIV transmission (e.g., [57, 75]). In each of these contexts, the couples' approach to measuring CPs could easily be integrated. It could improve estimates of the independent effects of CPs on HIV risk. It may also reduce bias in estimates of the effects of other HIV risk factors when data on CPs are used as “control” variables. Finally, it may result in a better understanding of the interactions between CPs and other HIV prevention interventions (e.g., ART, male circumcision, HTC).

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Fig. 1.

Study design for the validation of couples' reports of concurrent partnerships. *Notes* A and B are partners in a matched couple and appear in *dark grey*, whereas other LNS participants appear in *white*. The matched relationship is depicted by the *dotted line* between the two *grey circles*, i.e., both A and B reported each others as partners. The *solid arrows* that point towards the other partner indicate that each partner in the matched couple is asked to report his/her partner's potential CPs, whereas the self-pointed *solid arrows* indicate that each partner in a matched couple is asked to report his/her own CPs during the matched couple. The *dashed arrows* originating from other LNS participants indicate that members of a matched couple may occasionally be reported as partner by members of their sexual network. We use data from other LNS participants to validate the CP classifications in Table 1. In that case, for example, we would classify B as having “network-reported” CPs, but not A

Fig. 2.

Spine plot of the selectivity of the sample of matched couples *Notes* the spine plot combines an *horizontal bar* graph, which shows the proportion of matched relationships among all the relationships that constitute the Likoma Networks; and a *vertical bar* graph which shows the distribution of relationship characteristics among matched couples and non-matched couples (i.e., “only one partner interviewed”)

Fig. 3.

Odds ratios of HIV infection among women/men associated with concurrent partnerships of the matched partner, by CP classification and gender. *Notes* estimated odds ratios of HIV infection are obtained from logistic regression in which we also control for partner and relationship characteristics. *Error bars* represent 95 % confidence intervals. *Error bars* are clipped at 10 for estimates associated of the effects of female CPs on male risk of HIV infection obtained from classification #1. The *x-axis* is plotted on a logarithmic scale

Table 1
Definition of the classifications of concurrent partnerships used in this study

Classification	Men/women classified as having CPs if they have
#1	Known CPs
#2	#1 + secret CPs
#3	#2 + divulged CPs
#4	#3 + suspected CPs
#5	#4 + possible CPs

In this context, sensitivity refers to the proportion of male/female partners who truly had CPs during the matched relationship who were classified as having CPs by a given classification. Specificity refers to the proportion of male/female partners who truly did not have CPs during the matched relationship who were classified as not having CPs by a given classification

Table 2
Distribution of respondents in matched couples according to their CP behavior, by gender

Self-reports Partner reports	Women		Men	
	Yes, had CPs N (%)	No, no CPs N (%)	Yes, had CPs N (%)	No, no CPs N (%)
Yes	8 (1.6)	54 (8.7)	51 (9.9)	80 (15.5)
Yes, I suspect	25 (4.8)	132 (25.6)	38 (7.3)	116 (22.4)
I don't know	25 (4.8)	120 (23.3)	27 (5.2)	81 (15.7)
No	16 (3.1)	145 (28.1)	33 (6.4)	91 (17.6)

Dark grey known CPs, Lighter grey secret CPs, lightest grey divulged CPs, vertical shading = suspected CPs, diagonal shading possible CPs, white faithful respondent. Percentages in parentheses are cell percentages. They sum to 100 % across all cells relating to women/men

Table 3
Estimated prevalence of concurrency and pseudo-validity of CP classifications according to couples-reported data

	Women			Men		
	CP prevalence	Pseudo-sensitivity	Pseudo-specificity	CP prevalence	Pseudo-sensitivity	Pseudo-specificity
Classification #1	1.6 (0.7, 3.0)	2.7 (0.5, 7.6)	98.8 (97.1, 99.6)	9.9 (7.4, 12.7)	13.6 (8.9, 19.5)	92.1 (88.7, 94.7)
Classification #2	14.3 (11.4, 17.7)	20.5 (13.5, 29.2)	87.4 (83.7, 90.5)	29.0 (25.1, 33.1)	32.8 (25.9, 40.2)	73.0 (68.0, 77.7)
Classification #3	23.1 (19.5, 26.9)	29.5 (21.2, 38.8)	78.7 (74.4, 82.6)	44.4 (40.1, 48.8)	54.2 (46.6, 61.7)	60.7 (55.3, 65.9)
Classification #4	48.6 (44.3, 53.1)	55.4 (45.7, 64.8)	53.2 (48.2, 58.2)	66.8 (62.6, 70.8)	77.4 (70.5, 83.3)	38.7 (33.5, 44.1)
Classification #5	71.9 (67.8, 75.7)	74.1 (65.0, 81.9)	28.7 (24.3, 33.4)	82.4 (78.9, 85.6)	88.7 (83.1, 93.0)	20.8 (16.6, 25.5)
N	516			518		

The figures in parentheses represent the lower and upper bounds of 95 % confidence intervals. The pseudo-specificity and pseudo-sensitivity are calculated by comparison to the network-reported data contained in the LNS (see Fig. 1)

Table 4
Characteristics of matched relationships by HIV testing status of the female/male partner

	Women			Men		
	Tested	Not tested	P value	Tested	Not tested	P value
Age ^a						
Of the female partner	27.4 (7.0)	29.3 (7.7)	0.06	27.5 (7.2)	27.9 (6.6)	0.65
Of the male partner	31.8 (8.0)	33.7 (1.11)	0.10	31.6 (8.2)	33.0 (7.5)	0.09
Difference	4.4 (4.3)	4.4 (5.2)	0.96	4.1 (4.4)	5.1 (4.7)	0.02
Relationship status ^b						
Dissolved	79 (17.0)	9 (16.1)	0.86	66 (17.5)	22 (15.4)	0.56
Ongoing	385 (83.0)	47 (83.9)		311 (82.5)	121 (84.6)	
Relationship type ^b						
Non-marital	92 (19.8)	10 (17.9)	0.73	73 (19.4)	29 (20.3)	0.81
Marital	372 (80.2)	46 (82.1)		304 (80.6)	114 (79.7)	
Residence ^b						
Households	190 (40.9)	21 (37.5)	0.62	158 (41.9)	53 (37.1)	0.32
SAME household	274 (59.1)	35 (62.5)		219 (58.1)	90 (62.9)	
Date of first sex ^b						
< year ago	93 (20.0)	14 (25.0)	0.39	82 (21.8)	25 (17.5)	0.28
1 year ago	371 (80.0)	42 (75.0)		295 (78.2)	118 (82.5)	
Interval between interviews ^b						
Same day	165 (35.6)	20 (35.7)	0.91	139 (36.9)	46 (32.2)	0.53
1–29 days	161 (34.4)	18 (32.1)		125 (33.2)	54 (37.8)	
30 days	138 (29.7)	18 (32.1)		113 (30.0)	43 (30.1)	
HIV status ^b			–			–
HIV-negative	392 (84.5)	–		333 (88.3)	–	
HIV-positive	67 (14.4)	–		43 (11.4)	–	
Indeterminate	5 (1.1)	–		1 (0.3)	–	
N	464			377		

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The numbers in these rows include the mean age among tested and not tested partners, whereas the numbers in parentheses are standard deviations. P values are based on a t test of the difference in age between tested and not tested partners

The numbers in these rows are cell counts and numbers in parentheses are column percentages. P values are based on a χ^2 test of the difference between tested and not tested partners