Reviewer’s report

Title: Sex and Sport: Chlamydia Screening in Rural Sporting Clubs

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Reviewer: M. Jacques Nsuami

Reviewer’s report:

Fabian YS Kong, Jane S Hocking, Chris K Link, Marcus Y Chen and Margaret E Hellard BMC Infectious Diseases Sex and Sport: Chlamydia Screening in Rural Sporting Clubs

General Comment

The authors have made a tremendous effort to answer different points I made on the previous version of the manuscript and portions of this revised version read more clearly as a result. However, a major issue has arisen in the Results section that will need to be addressed (see Major compulsory revisions below). The essence of this submission was well captured in the abstract, but the Results section departs from that essence probably by the fact that the authors had the opportunity to collect more data as the screening became feasible, and the Discussion follows the lack of focus of the Results presented. Consequently, the manuscript that could have been straight to the point as is the abstract has been weakened by a Result and a Discussion sections that can be ridden of unnecessary materials.

Major compulsory Revisions

Participants

1. This section states that men and women were eligible if they were aged 16 to 25 years but Table 1 has the age distribution as 15 to 29 years. Please reconcile these discrepancies.

Results section

To me, the entire section can survive the paper without Tables 1 and 2. Below are the rationales behind that view.

1. Table 1. This table is only referenced in the text to tell what participation rate on the night of screening was, and this information is given at the bottom of the table without fitting the table format, followed by asterisks that are explained in footnotes. This data presentation indicates that this information could be given directly in the text and be removed from the table to avoid repetition. Except for the number of males and females that I was able to use from the table in understanding where the prevalence rates presented in the Results came from, the remainder of the data in the table has no bearing on the manuscript, as evidenced by the fact that authors have used or referred to none of them in the
text except displaying them in this table. Because any relevant data in this table can be presented directly in the text and any irrelevant data left out, Table 1 can therefore be deleted.

2. Table 2. This table is displaying the prevalence and odds ratios for chlamydia infection by numbers of sexual partners in the past 3 and 12 months and overall tells us that the odds of infection tended to increase with the numbers of sexual partners. A couple of points: first, this is not new information. Whenever data on sexual partners are available, we know and expect that the odds of chlamydia infection will tend to increase with the numbers of sexual partners. Second, we only have small numbers of cases (19 males and 9 females) that are broken up into 2 by 3 strata, producing wide confidence intervals only to show what we expect in a manuscript that aimed at determining the feasibility of establishing a chlamydia testing outreach program, the prevalence of chlamydia, and the acceptability of the program. Because the 2.45 and 3.23 odds ratios given in the text cannot be found in this table but still tell us what is in the table, and determining risk factors for infection was not the main aim of this study, the information given in the text about the odds of infection increasing with the numbers of sexual partners is sufficient by itself without displaying Table 2.

Chlamydia prevalence

1. The authors must provide here the numerators and denominators used to generate these prevalence measures, such as x% (n/m; 95% CI). If there were 527 sexually active participants that included 121 females and 426 males (see Demographic details subsection), and 28 positive cases were identified including 19 males and 9 females (see Chlamydia prevalence subsection), then the overall prevalence among sexually active participants according to these figures would be 5.1% (28/527), 7.4% (9/121) among sexually active females and 4.5% (19/426) among sexually active males. It is unclear why authors are including participants who are understood to have never engaged in vaginal or anal intercourse in the calculation of chlamydia prevalence, and still refer to that as prevalence among sexually actives. The overall prevalence of 3.9% among sexually active participants, 5.6% in sexually active females and 3.5% in sexually active males are indeed coming from all 709 participants, 161 females and 548 males. These must be clarified.

2. Following the ambiguity of how sexually actives have been used in calculations of prevalence, it must be clarified how the concept has been used in calculating odds ratios. For example, denominators of sexually active used in calculations of odds ratios in Table 2 for males with new female partner in last 3 months (n=535) and for females with new male partner in last 3 months (n=157) are bigger than the totals of sexually actives given in text (426 males and 121 females, respectively).

Discussion

First page of section
1. Par 1, line 3: “The prevalence of chlamydia in sexually active participants was 3.9%”. See comment above on how unclearly the prevalence in sexually active participants was calculated.

2. Middle section of page: There is in the middle portion of this page three sub-portions that were initially separated each by 2 returns that are now separated each by 1 return (not sure if these are three paragraphs or one) that talk about “data which was representative of young people reported in the literature”, “the prevalence in males was similar to estimates in overseas population-based weighted estimates”, and “the proportions who reported having ever being previously diagnosed with chlamydia in our study was representative and similar to those reported in the nationally representative Australian Study of Health…” The authors here appear to concern themselves too much about their findings being representative of some other estimates. But findings from this study do not have to be representative of anything. This study was concerned about whether or not a chlamydia screening could be implemented in local sporting clubs, and this was done (29 clubs participated); it was concerned about whether people attending the sporting clubs that agreed to participate would accept the screening, and they did (95% of those attending the clubs the night of the screening were screened); it was concerned about what the prevalence among those who participated (yield of screening) would be, it was 3.9% (or 5.1% depending on what denominator is used) which for chlamydia is a high yield. The goal of this study was not to generate estimates that would achieve representativeness of some kind. The very idea that this “study was a feasible means for collecting prevalence data and data which was representative of young people reported in the literature” is an inaccurate restatement of the feasibility the study sought to determine, and the representativeness discussed here can be challenged by the authors’ later discussion of the limitations of this study, where they state in the last page of the Discussion that their recruitment sites reduces the generalizability of their results, and the true population effect of the type of screening they conducted was unknown given the limited data on their study population. So the whole exercise on this portion of the discussion trying to show that the authors’ prevalence or odds ratios are representatives of some other findings is not making findings of this paper any more valid or the paper any stronger. The strength of this paper is in having shown that chlamydia screening was done (feasibility) and it was well accepted by participants to which it was offered (acceptability). Even if no infection was detected, this would only have indicated a low yield of screening in this setting and would not undermine the feasibility and acceptability of the screening. The whole middle of the page can be either deleted altogether or shortened substantially to take the reader directly to the following portions that discuss aspects that affected/could affect participation (i.e., at the clubs and at individuals levels) and the yield of screening (i.e., screening all clubs participants or only sexually active). As I indicated earlier, the abstract is a good guide for conciseness.

Second page of section

1. Par. 2 and 3: If the authors were able to state in the Methods that “all potential
clubs were identified and contacted by telephone or email using local contacts” and “participants were then provided with a ‘show bag’ with condoms and educational material about STIs and available sexual health services. Participating clubs were provided with refreshments for all club members after training”, then they had the opportunity to list everything else they did to make the screening feasible. If media were used to solicit participation and participants were served meals and given autographed footballs, then there in the Methods were the place and time to exhaustively specify that telephone, email, radio and local newspaper were used to recruit sporting clubs, and after training autographed footballs and meals were provided. When such details emerge in the discussion, the reader can then place them in proper perspective and not start wondering where all these are coming from. The discussion is therefore not where these crucial details of the program should be clarified, as the authors’ response to my earlier related comment says. The Methods section is. That is where anyone interested in implementing a screening in a setting or population close to that of the authors will turn to and learn from the authors’ experience how they made it all happen.

2. The previous comment applies equally for statements such as “this created interest in another club to participate, especially given the benefits provided to its members”. What projects’ benefits that the authors are referring to that they know swayed people’s opinion towards participating? If there is anything the authors did that improved participation whether at the club or at the individual level, that is Methods. If the reader cannot place statements and thoughts expressed in the discussion in proper context in the paper, even if they are in context in the authors’ mind, they will appear to the reader out of context, as I previously indicated in my previous comments.

Previous comment:
Page 9
3. Par. 3, line 3: “…it encouraged non-participating clubs in the same region to participate…” This is unclear.

Authors’ response
• The manuscript has been altered to clarify this of non-participating clubs.

My new comment
The manuscript still reads “…it encouraged non-participating clubs in the same region to participate…”. Do the authors mean it encouraged clubs that initially declined to participate?

Limitations
1. The authors did not mention in the Discussion the 24% participation rates at the club level. Just as the authors have commented on the 95% participation rates at the individual level and the incentives that made individual participation a success, they must comment on the lower acceptance/participation rate of sporting clubs and what to make of it, essentially the potential implications on the
yield of screening and on the generalizability of the prevalence estimates that are reported.

Minor Essential revisions

Chlamydia prevalence
1. Line 3: “with only participant (male) …” Something is missing here.

Last Page of Discussion
1. Top par, line 9-10: “There was less females…” There were fewer females….

Discretionary Revisions

Although the following were recommendations which the authors could choose to ignore, in their response they stated to have chosen to make the changes while they actually didn’t and the manuscript still reads in those places as before.

My earlier comments and the corresponding authors’ responses follow:

Page 2
1. “Conclusion”, last line: It would be helpful to specify that it is “to screen, treat and educate young people ‘for STI’…”

Authors’ response:

• The manuscript has been altered to accommodate all of the suggested changes.

Page 3
1. Par. 1, line 8: “screening is necessary to effectively control ‘chlamydia’ transmission.” Although this is a thought that can be easily and has in fact been largely accepted, and screening does indeed increase early detection and treatment of asymptomatic and unsuspected STIs, but can we, after careful consideration, say that screening effectively controls STI transmission? Can this statement be seen as overstretching the role of screening in STI transmission?

Authors’ response:

• In Australia the proportion of people in the age range at greatest risk of an STI (16-29 years) remains low (Regan et al JID 2008;198:349-58). STI screening has the potential to impact on chlamydia transmission, particular if the level is high and it occurs regularly, by reducing the prevalence of the disease in the population. That being said, the manuscript has been altered to reflect the uncertainty of the impact of chlamydia screening on the population

My new comment here:

I underlined “on the population” to stress the fact that contrary to the authors’ response, my earlier comment was on the authors’ stated role (impact) of screening in controlling chlamydia transmission. Since the statement I commented on in the Introduction has not changed and the authors could choose to ignore the suggestion, I don’t know what part of the manuscript the authors
have altered in response to this specific comment.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests