

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

### ARTICLE DETAILS

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| <b>TITLE (PROVISIONAL)</b> | Factors mediating HIV risk among female sex workers in Europe: A systematic Review and ecological analysis |
| <b>AUTHORS</b>             | Platt, Lucy; Rhodes, Tim; Jolley, Emma; Hope, Vivian; Latypov, Alisher; Reynolds, Lucy; Wilson, David      |

### VERSION 1 - REVIEW

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| <b>REVIEWER</b>        | Baral, Stefan<br>John Hopkins University |
| <b>REVIEW RETURNED</b> | 22-Mar-2013                              |

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| <b>GENERAL COMMENTS</b> | <p>Thank you for having me involved in this review of a manuscript providing a meta-analysis of the burden of disease and associated determinants of HIV and STI infection among FSW in Europe. This is a well written manuscript completed by a group with significant expertise in the area. However, there are certain issues that could be addressed to improve the manuscript.</p> <p>One issue that I would like to better understand is why these data were not compared to ECDC data (yearly surveillance reports) that includes summation of HIV prevalence by country and by risk factor. In addition, the ECDC completed a review of people who inject drugs in Europe including an epi assessment—it would be helpful to cite this here given the importance of drug use characterized in these analyses. If there is a reason as to why this was not included, then please mention. If not, then I would suggest citing and potentially comparing your own reviews that what is reported by countries to see if significant discrepancy. The ECDC has little control over what is reported (similar to most such bodies such as CDC in US) and thus there may be an important discrepancy worth highlighting. The authors also highlight the Dublin Declaration which includes European version of indicators akin to UNGASS. I believe most countries have reported on these indicators, but not sure if yet public. Again, this may be worth citing and comparing if now available. Another option is to contact the ECDC for feedback.</p> <p>The authors also assert that heterosexual transmission is increasing in Europe : Considering the growing epidemics of HIV in Europe, evidence of increasing heterosexual transmission, and the significant overlap between sex work and drug injecting across the region, and especially in the East(11-13). However, the citations 11-13 are focused on Georgia and Ukraine. This is an important assertion and I am not sure that this is correct. If the authors feel comfortable with this assertion, than there is a need to prove this with increased citations that are more representative—such as ECDC reports. I do believe that there is evidence supporting increasing transmission among MSM but even this is contentious given the changing surveillance strategies in the region. I do not believe that there is consensus on trends of the HIV epidemic in Europe at the ECDC.</p> |
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|  | <p>The authors are thorough in the introduction describing the limitations of HIV surveillance broadly as well as in some of the settings included here. I do think that the authors could make more clear in the discussion that there is also the potential that FSW who use drugs are overrepresented in these studies—especially in the East since they use TLS or RDS with a focus on drug use venues or potentially seeds who use drugs. FSW who do not use drugs in these settings likely have similar burden of disease as FSW in Western Europe—and this is masked by pooling of data. I suggest that the authors explore this or at least consider including as a limitation.</p> <p>Minor<br/>Page 15, second paragraph under Ecologic Analysis<br/>This includes interpretation of data and would suggest cutting and integrating into discussion</p> <p>Since this represents a meta-narrative, consider reviewing the newly published RAMESES guidelines.</p> <p>Reference 5 refers to a review of HIV among MSM and not FSW.</p> |
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| <b>REVIEWER</b>        | Stockman, Jamila<br>UCSD, Division of Global Public Health, Department of Medicine |
| <b>REVIEW RETURNED</b> | 03-Apr-2013  |

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| <b>THE STUDY</b>        | None of the supplemental documents contain information that should be better reported in the manuscript or raise questions about the work.   |
| <b>GENERAL COMMENTS</b> | <p>It was a pleasure reviewing manuscript #bmjopen-2013-002836 entitled, “Factors mediating HIV risk among female sex workers in Europe: A systematic review and ecological analysis”. This review article is an extremely important contribution to the field of public health and more specifically, the field of HIV prevention research among female sex workers. This review is well-written and presented in a comprehensive manner. Minor suggestions are recommended to strengthen the manuscript.</p> <p>Abstract<br/>1) The results and discussion section should mention the findings relevant to STI prevalence and particularly the need to focus on sexual risk in addition to injecting risk practices, respectively.</p> <p>Introduction<br/>2) Minor editing is needed. Female sex workers is initially defined as “FSW” rather than “FSWs” and the acronym, “SWs” is used in other areas of the introduction.<br/>3) On pg. 5, lines 16-19, the term, “structural and environmental risk factors” is used? Are these used interchangeably because this is the first mention of “environmental risk factors?”</p> <p>Methods<br/>4) The abstract states that studies that were published from 2000 were selected but the methods section stated, “from 2005 to October 20, 2011” (pg. 5, lines 35-38; pg.6, lines 26-29). The correct time period should be accurately reflected in both sections.<br/>5) Inconsistencies with the final number of papers meeting the</p> |

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|  | <p>inclusion criteria are noted – in the methods section, the final count is “60 papers provided unique estimates of HIV and STI prevalence” and in the abstract, the final count is “63.”</p> <p>6) Minor editing is recommended. The period should be removed after the subheading, “Ecological Analysis.”</p> <p>Results</p> <p>7) Since the first aim of the review is “to assess the prevalence and incidence of HIV and STIs (Chlamydia, Syphilis, Gonorrhoea) among FSWs,” it is confusing to understand why the first section of the results focuses on HIV, violence and condom use, with STIs following this section. If there was limited STI prevalence data for the countries represented in the review and this is why this section is not presented first, this should be stated.</p> <p>Discussion</p> <p>8) Pg. 18, line 14, “female sex works” should be “female sex workers.” Review of this section should also make sure the plural form of FSWs is used rather than the singular form.</p> <p>References</p> <p>9) The reference list should be reviewed and edited accordingly. Some references appear to be missing the name of the journal (e.g., references 13, 30, 68).</p> |
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| <b>REVIEWER</b>        | <p>Dr Andrew Hinde<br/>Senior Lecturer in Population Studies<br/>Division of Social Statistics and Demography<br/>University of Southampton<br/>SOUTHAMPON SO17 1 BJ<br/>United Kingdom</p> |
| <b>REVIEW RETURNED</b> | 14-May-2013   |

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| <b>THE STUDY</b>        | <p>See attached report. The authors should decide whether their research question includes STIs other than HIV, or whether they wish to focus on HIV alone. Once they have done this, they should amend both abstract and text. The regression analysis needs some attention, especially the question of using weighted regressions. I also recommend removing part of the ecological analysis.</p>  |
| <b>GENERAL COMMENTS</b> | <p>This is a systematic review of 73 studies which have attempted to estimate the prevalence of HIV among female sex workers in Europe. The review is carried out following the PRISMA guidelines, and the methods used to select the studies included are clearly laid out. I think that a version of this paper would merit publication, but I have two major issues to raise and a few minor points which I think the authors should consider.</p> <p><b>Major points</b></p> <p>(1) I am unconvinced by some of the ecological analysis. I cannot see why a strong relationship should be expected between the HIV prevalence among sex workers and the GINI coefficient, or the male-female pay differential. The chain of causality seems long, and the GINI coefficient and the male-female pay differential poor proxies for what you are trying to measure. I am more sympathetic to the investigation of the relationship between HIV prevalence and the prison population, and certainly the proportion injecting drugs. I would be inclined to drop the analysis using the GINI coefficient and the male-female pay differential from Figure 4 and Table 3, and from</p> |

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|  | <p>the relevant parts of the text.</p> <p>Second, I could not find any indication of whether you used weighted regression and, if so, by what you weighted the data? On p. 7, ll. 19-24, you state that '[T]wo authors ... independently assessed the quality of the studies ... using a scoring system that graded the papers ...'. Did you use the scores to weight the linear regression analysis so that higher scoring studies were given more influence over the results? In fact, I could not find any discussion of how you used these scores in the analysis. What was the purpose of the scoring system (apart from following the PRISMA guidelines)? Did you weight the linear regression analysis by the sample size used in each study you included? I recommend you consider doing this.</p> <p>Third, you use as a covariate the 'numbers of sex worker specific services' (p. 16, l. 1), noting that '... HIV prevalence appears to decline as the number of sex worker specific services increase [sic]' (p. 16, ll. 2-3). Apart from the plural verb associated with a singular subject, I do not follow what you mean by, or how you measure, the 'numbers of sex-worker specific services'. What is a 'service'?</p> <p>(2) In the abstract, the objectives mention sexually transmitted infections (STIs) but the results and conclusions do not. If the analysis of STIs is one of your objectives, some results and conclusions about STIs should be mentioned. The text does not mention STIs until p. 5, ll. 10-11. On the basis of the title and the text of p. 4, readers would imagine it was just about HIV. If you really are interested in STIs other than HIV, I recommend re-drafting the text to include them more centrally.</p> <p><b>Minor points</b></p> <p>p. 5, l. 9. 'SWs' should be 'FSWs'. I do not think you study male sex workers.</p> <p>p. 7, ll. 25-27. p. 8, l. 29. What does the '&lt;10)' mean? Is it 'less than 10 per cent'?</p> <p>p. 17, l. 14. Define 'PWID'.</p> <p>p. 19, l. 15. Do you mean a 'multi-level' model of the kind described in, say, H. Goldstein, <i>Multilevel statistical models</i> (4<sup>th</sup> edition), London, Wiley, 2011, or are you using the term to mean a general multiple regression model? If the latter, please find another term to avoid confusing readers.</p> |
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## VERSION 1 – AUTHOR RESPONSE

Response to Reviewer 1: Stefan Baral.

We thank for the reviewer for his useful observations. We didn't compare the findings with ECDC data on yearly HIV case reports since this did not fall within the objectives of the study. Few countries in the region collect data on sex work as a risk factor in their HIV case reporting systems. The quality of the data collected and its completion is variable. Moreover, even if data on size of sex work population was also available for these countries, it would be difficult to assess HIV prevalence among sex workers using these data without data on proportion of HIV cases undiagnosed among sex workers. We could compare prevalence of HIV against prevalence among women with sexual transmission or among HIV cases among female PWID but again that would be beyond the remit of this paper.

We agree with reviewer that the evidence for increasing sexual transmission in the West of Europe suggests the main transmission is through sex with men or transmission from migrant populations from high prevalence countries. We have edited this sentence to more accurately reflect the data and emphasise that heterosexual transmission remains important in the West rather than increasing (see introduction, paragraph 3). We maintain that there is emerging evidence of increased heterosexual transmission in the East and have inserted more references to support this.

We did not review the UNGASS indicators. At the time of writing only 29 countries had reported history of HIV testing and condom use that would have been of relevance to the study. Problems with both indicators exist including inconsistent time frame used to measure last HIV test, lack of clarity over type of sex act condoms are used for with the last client, as well as consistent lack of denominator or description of sex workers. These problems make it difficult to conduct any systematic comparison across countries. We have added a sentence into the introduction (paragraph 2) describing these limitations to provide some context to why they were not used in the analysis.

We agree with Reviewer 1 that the use of TLS or RDS with a focus on recruiting street sex workers may result in overrepresentation of FSWs who inject drugs in the East. In the results we describe prevalence from individual studies alongside prevalence of injecting drug use in order to illustrate the range of prevalence measured in the East and the effect of injecting on HIV among FSWs. We have added in a sentence to acknowledge this potential overrepresentation as a limitation in the discussion (paragraph 6).

Minor:

We have moved this interpretation of the regression line and its implications to the discussion (paragraph 5) .

We thank the reviewer for alerting us to the newly published RAMESES guidelines. Since this review synthesizes quantitative data on the prevalence of HIV, STIs, violence and condom use we believe that the PRISMA guidelines are more appropriate to use rather than the RAMESES.

We have replaced Reference 5 with the correct reference to a systematic review examining increased odds of HIV among FSWs compared to a general populations sample of women.

Review 2: Jamila Stockman

We thank the Reviewer for her useful comments and have addressed the revisions as follows:

Abstract

1) We have summarized findings relating to STIs and the need to focus on sexual risk as well as injecting risk.

Introduction

2) We have corrected the acronym to show that female sex workers is summarized as FSWs and edited the text to ensure we consistently refer to FSWs rather than SWs.

3) We have edited this paragraph so that we are consistently referring to structural risk factors rather than environmental risk factors.

Methods

4) We have corrected the abstract to show that studies published from 2005 were searched systematically. We have also added a sentence in the methods section to qualify this strategy. Where no recent or only poor quality HIV estimates were identified for the study, we extended the search back to 2000 in order to maximize the number of estimates identified. This strategy was used to identify estimates from Italy, Spain, Moldova and Portugal. We have flagged this up as limitation that some estimates are derived from older studies than others (discussion, paragraph 6).

5) We have edited the abstract and methods so that the final count of papers is consistent.

6) The full stop has been removed after 'ecological analysis' in the methods section.

Results

7) We have reordered the results section so that prevalence of STIs are presented after HIV and in the same order as the objectives

Discussion

8) We have edited this section to say 'female sex workers' and changed the acronym to say FSWs.

References

9) We have edited the reference list.

Reviewer 3

We have addressed the reviewers comments and presented findings related to prevalence of STIs in the abstract.

Major points

1) Ecological analysis

We thank the reviewer for his insightful comments on the ecological analysis. We maintain that it is useful to examine the relationship between HIV prevalence among FSWs and the GINI co-efficient and the male to female pay differential despite the long chain of causality. We acknowledge in the limitations section that the ecological analysis is limited in that we cannot infer causality or relationships on an individual level and that the descriptive linear nature of the relationships examined are unlikely to be a true representation of complex, multi-level relationships.

One of the purposes of ecological analyses is to generate hypotheses for future research, considering the lack of epidemiological research that focuses on structural risk factors the analysis is a valuable contribution to the limited evidence base, reinforcing existing data on the importance of sex worker targeted services to reduce HIV as well as the complex relationship between HIV and inequalities in wealth and pay differentials that has been flagged up by previous research referred to in the methods section.

As described above, the quality of papers were scored where multiple estimates existed in order to select a 'best' estimate of HIV prevalence among FSWs that appeared to be the most representative at a country level. We have clarified the purpose of the scoring in the methods section. While we agree with the reviewer that it is correct to weight the linear regression models to adjust for different sample sizes, we did not do this since sample size was already accounted for in this scoring process. We have clarified in the methods section that sample size was one of the criterion for assessing quality (Methods, ecological analysis). In addition, we reran the linear regression models using an analytical weight to account for sample size and this did not have an effect on the findings. We do not present the outcome of this scoring although the range of estimates are presented in Table 1 (as described above).

2) STI estimates

We have revised the result and findings section of the abstract to present findings of STIs.

Minor points.

We have added into the text a clear definition of sex worker targeted services and have corrected the grammatical errors (see Results, paragraph on ecological analysis).

We have edited the acronym SW to FSW.

We have clarified that <10 is referring to a proportion by adding in %

The definition of PWID is given in the first paragraph of the introduction.

We confirm that we do mean multi-level model of the kind described in the reference given by the reviewer.