PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) 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

TITLE (PROVISIONAL)	Multimorbidity in a marginalised, street-health Australian population: A retrospective cohort study
AUTHORS	Brett, Tom; Arnold-Reed, Diane; Troeung, Lakkhina; Bulsara, Max; Williams, Annalisse; Moorhead, Robert

VERSION 1 - REVIEW

REVIEWER	Marjan van den Akker Maastricht University, Department of Family Medicine, The Netherlands
REVIEW RETURNED	14-May-2014

GENERAL COMMENTS	To my knowledge, this is the first study on prevalence and patterns of multimorbidity among marginalised and homeless people. A useful extension of knowledge. I have some questions and remarks for the authors: - the conclusion as formulated in the abstract is that the prevalence of multimorbidity in the street health population is lower than that in the mainstream population. I think the more relevant conclusion is that - taking into account the age and sex distribution - the prevalence of multimorbidity is definitely higher in the street health population. - I would like to read some more about the organisation of the street health, such as accessibility, record keeping etc. I now have the feeling that I'm lacking context to really grap the results. - From page 5, lines 25-26 I understand that in the street health population only patients were included who presented with conditions that appeared to be ongoing. What was the procedure for the mainstream population? If those procedures were different, is it still legitimate to compare the prevalence of multimorbidity between those two groups? - Why were Aboriginal and non-Aboriginal patients only compared in the street health cohort? - I would prefer table 4 simplified, without B and SE (since these do not add information to the OR + CI.

REVIEWER	Susan Smith RCSI Medical School, Dublin Ireland
REVIEW RETURNED	22-May-2014

GENERAL COMMENTS	Thank you for asking me to review this paper looking at

	multimorbidity in a marginalized street health population in Australia. The authors have undertaken a very detailed analysis of conditions and condition severity in a cohort of street health patients and can compare this to a cohort of mainstream patients attending a general practice in the same region. To my knowledge this is the first such analysis of mutimorbidity in a street dwelling population though there has been previous work published on multimorbidity in younger illicit drug users, though not necessarily street dwelling (see http://www.biomedcentral.com/1471-2296/10/25 I have the following comments/ suggested revisions: 1. Abstract: the results from the aboriginal patients are based only on an analysis of the street dwelling population and this should be made more explicit in the abstract 2. I found it surprising that the strengths and limitations were presented in a figure at the start of the paper rather than incorporated in the discussion and even if this is a journal style issue, this should also be addressed in the discussion as potential confounding is such an important part of bias in observational studies 3. The authors do acknowledge this but it could be discussed more- comparing the two cohorts is a little problematic due to the differences in their attendance rates. This is particularly important in multimorbidity as the accrual of formal diagnoses of conditions depends in part on regular attendance with care providers and record keepers. Also – seems strange that there were absolutely no aboriginal patients in the mainstream cohort or is this information not recorded in this group? 4. Methods: How was the sub-set for analysis in the mainstream practice cohort determined? I was unclear as to the age criteria – the ranges suggest that all ages including babies were eligible for inclusion in both cohorts but it seems unlikely there would be similar proportions of children in both groups 5. Methods: should include some description of how severity is rated 6. Methods and results_ were there unique patient identifiers for the street health cohort – or could there be double counting given the time frame over which data collection took place and is there any possibility of cross over when street health people are housed? 7. I was surprised that conditions such as skin infections were being coded as ongoing conditions in the street cohort and not convince of the justification for doing this – perhaps discussion could consider the effect this had on condition counts in this group 8. As the numbers are big relatively small differences in proportions between the 2 groups are statistically significant but I am not sure as to the service or policy significance of a difference between 5% and 1% in proportions in the severe category and 34% vs 21% in moderate category using the severity index – could be worth discussing what these results mean for service delivery and policy more explicitly. 9. Discussion: I think for an international readership, there needs to be more discussion of where these results fit into international literature and a focus on policy implication – what is different about mutimorbidity in street health people? Do those caring for them need to have more psychiatric expertise or include psych team members etc. The authors state that their findings have implications for the design and delivery of services but would be useful if they could be more specific as to how their findings change current evidence on health care for street living people in general

VERSION 1 – AUTHOR RESPONSE

Reviewer 1

1. The conclusion as formulated in the abstract is that the prevalence of multimorbidity in the street health population is lower than that in the mainstream population. I think the more relevant conclusion is that - taking into account the age and sex distribution - the prevalence of multimorbidity is definitely higher in the street health population

Our original description was based on crude prevalence rates. We have included an age-sex adjusted prevalence calculation for the mainstream sample standardized to the street health cohort. This confirms that the prevalence of the street health cohort is significantly higher than the mainstream cohort.

See: Page 3, Lines 4-6.

2. I would like to read some more about the organisation of the street health, such as accessibility, record keeping etc. I now have the feeling that I'm lacking context to really grasp the results.

This has been amended and expanded in the Introduction and Method. There was no restricted access to the Street Health service and occasionally non-marginalised patients made opportunistic visits to the service. These visits were recorded but if no chronic medical conditions present, they rated zero on CIRS. The service operates in a converted van. Staff includes various combinations of a GP plus a nurse, Aboriginal outreach worker and reception person. All attendances were recorded in the electronic record system.
See: Page 5, Lines 40-50 and Page 6, Lines 9-11, 41-48
3. From page 5, lines 25-26 I understand that in the street health population only patients were included who presented with conditions that appeared to be ongoing. What was the procedure for the mainstream population? If those procedures were different, is it still legitimate to compare the prevalence of multimorbidity between those two groups?

The total population of attendees in both the Street Health and the mainstream practice were included. However, Multimorbidity within a particular anatomical domain was only noted to be present if the information in the records suggested the condition was ongoing/chronic and rated according to the CIRS. Some of the electronic records in the street health cohort were limited in the information recorded and, in contrast with mainstream practice, collateral information in the form of hospital discharge summaries or other specialist reports was not always present. Patients in both cohorts with no evidence of any chronic conditions scored 0 on CIRS. We included chronic skin conditions as they featured prominently in many presentations.

See: Page 6 Line 7-15 and Page 11 Lines 35-38

4. Why were Aboriginal and non-Aboriginal patients only compared in the street health cohort?

It is stated in the Abstract and method section under Data Analysis that there were no data on Indigenous status in the mainstream cohort for comparison.

See: Page 2 Line 49-51, Page 7 Line 54-56

5. I would prefer table 4 simplified, without B and SE (since these do not add information to the OR + CI.

We have removed the columns for B and SE as requested.

See: Page 15, Line 3-23

Reviewer 2

1. Abstract: the results from the aboriginal patients are based only on an analysis of the street dwelling population and this should be made more explicit in the abstract

We have added a sentence to the abstract following the results for Aboriginal patients stating that "there was no comprehensive data on Indigenous status in the mainstream cohort for comparison".

See: Page 2 Line 49-51
2. I found it surprising that the strengths and limitations were presented in a figure at the start of the paper rather than incorporated in the discussion and even if this is a journal style issue, this should also be addressed in the discussion as potential confounding is such an important part of bias in observational studies

We followed the suggested layout for the BMJ but have addressed the possible confounding issue in the discussion. It is more likely that possible opportunistic attendees at the Street Doctor service could have had a diluting effect on the Multimorbidity prevalence rather than the reverse.

See: Page 16, Lines 9-15, Page 11, Lines 35-38, Page 17, Lines 3-10

3. The authors do acknowledge this but it could be discussed more- comparing the two cohorts is a little problematic due to the differences in their attendance rates. This is particularly important in multimorbidity as the accrual of formal diagnoses of conditions depends in part on regular attendance with care providers and record keepers.

The data extractors (two GPs and two medical students) had considerable experience from earlier work on our two practice Multimorbidity paper. Where there were concerns re particular CIRS scores, the problem was resolved through discussion and consensus with the lead author.
Data extractors were aware of possibility of duplication in medical records and strategies were employed to minimize this.

See: Page 6, Line 20-47, Page 16, Line 9-15.

Also – seems strange that there were absolutely no Aboriginal patients in the mainstream cohort or is this information not recorded in this group?

It is stated in the method section under Data Analysis that was no comprehensive data on Aboriginal status in the mainstream cohort for comparison purposes. The information was recorded for some but not for all.

See: Page 2 Line 49-51, Page 7 Line 54-56

4. Methods: How was the sub-set for analysis in the mainstream practice cohort determined? I was unclear as to the age criteria – the ranges suggest that all ages including babies were eligible for inclusion in both cohorts but it seems unlikely there would be similar proportions of children in both groups

We included data on the total patient population attending a mainstream practice over a 6-month period (1 July to 31 December 2008) as the comparator group. We have made this clearer in the Study Setting section of the Methods.

See: Page 6, Line 12-15.

5. Methods: should include some description of how severity is rated

We have included additional information about the calculation of CIRS scores and Severity Index classifications under 'Data Extraction' in the Methods section.

See: Page 6, Line 28-38.

6. Methods and results_ were there unique patient identifiers for the street health cohort – or could there be double counting given the time frame over which data collection took place and is there any possibility of cross over when street health people are housed?

This has been covered under Method 'Data Extraction'

See: Page 6, Line 43-48.

7. I was surprised that conditions such as skin infections were being coded as ongoing conditions in the street cohort and not convince of the justification for doing this – perhaps discussion could consider the effect this had on condition counts in this group

This has been outlined further and explained in Results and Discussion sections.
See: Page 11, Line 35-38 and Page 17, Line 3-10

8. As the numbers are big relatively small differences in proportions between the 2 groups are statistically significant but I am not sure as to the service or policy significance of a difference between 5% and 1% in proportions in the severe category and 34% vs 21% in moderate category using the severity index – could be worth discussing what these results mean for service delivery and policy more explicitly.

This has now been addressed in the discussion and conclusion.

See: Page 17, Lines 34-38, Page 18, Lines 32-44

9. Discussion: I think for an international readership, there needs to be more discussion of where these results fit into international literature and a focus on policy implication – what is different about

multimorbidity in street health people? Do those caring for them need to have more psychiatric expertise or include psych team members etc. The authors state that their findings have implications for the design and delivery of services but would be useful if they could be more specific as to how their findings change current evidence on health care for street living people in general

This has been addressed in Discussion and Conclusion.

See: Page 17, Line 18-30, Page 18 Lines 23-43

VERSION 2 – REVIEW

REVIEWER	Marjan van den Akker Maastricht University, dept of Family Medicine, school Caphri, Maastricht, the Netherlands / KU Leuven, dept of General Practice, Leuven, Belgium
REVIEW RETURNED	23-Jun-2014

- The reviewer completed the checklist but made no further comments.

REVIEWER	Susan Smith Department of General Practice RCSI Dublin Ireland
REVIEW RETURNED	20-Jun-2014

GENERAL COMMENTS	The authors have addressed many of my original comments but a new issue has arisen as a result of the age-sex adjustments made in the results as suggested by one of the reviewers. These are the results that should be presented in the abstract rather than presenting it both ways. Likewise it may be preferable that the results section should focus on the age-sex adjusted rates rather than the crude rates which are also presented in Fig 1. This is particularly relevant given the different age ranges in each cohort and the fact that he age-sex adjustment actually changes the results I also have remaining concerns about the differences in conditions included in each cohort and struggle with the concept that an infectious condition like scabies is included as a chronic condition in the street cohort when it is not included in the mainstream group. It is not possible to tell from the results what the impact of this difference is as skin conditions are included within the musculoskeletal group. The authors do acknowledge this on page 17 but do not quantify it and I think the authors should present some estimate of the effect of this on the results – it may not be much but should be addressed as limitations and explored further. In relation to my original comment (no 3) on the link between attendance and accrual of diagnoses – the authors seem to have missed my point as I was not commenting on data extraction but the process of documentation and recording of conditions in each setting.

VERSION 2 – AUTHOR RESPONSE

Reviewer 2
1.The authors have addressed many of my original comments but a new issue has arisen as a result of the age-sex adjustments made in the results as suggested by one of the reviewers. These are the results that should be presented in the abstract rather than presenting it both ways.

The abstract has now been amended and the results presented.
Page 2 lines 22-39.

2. Likewise it may be preferable that the results section should focus on the age-sex adjusted rates rather than the crude rates which are also presented in Fig 1. This is particularly relevant given the different age ranges in each cohort and the fact that the age-sex adjustment actually changes the results.

The prevalence reporting in the results section and Figure 1 has been amended to focus on the age-sex adjusted rates.
Page 10 lines 24-52
Figure 1

3. I also have remaining concerns about the differences in conditions included in each cohort and struggle with the concept that an infectious condition like scabies is included as a chronic condition in the street cohort when it is not included in the mainstream group. It is not possible to tell from the results what the impact of this difference is as skin conditions are included within the musculoskeletal group. The authors do acknowledge this on page 17 but do not quantify it and I think the authors should present some estimate of the effect of this on the results – it may not be much but should be addressed as limitations and explored further.

Given that at the time of data collection, the CIRS collapses chronic disease ratings into body systems domains, it is not possible to estimate proportion of musculoskeletal/integumental domain that related to chronic skin problems. This limitation and relevance has been addressed on page 11 lines 46-50 and page 16 lines 31-40.

4. In relation to my original comment (no 3) on the link between attendance and accrual of diagnoses – the authors seem to have missed my point as I was not commenting on data extraction but the process of documentation and recording of conditions in each setting.

The likely impact of differences in attendance on accrual of diagnoses has been addressed on page 6 lines 41-46 and page 16 lines 18-23.

Reviewer 1
No comments from reviewer

VERSION 3 - REVIEW

REVIEWER	Susan Smith RCSI Dublin
REVIEW RETURNED	01-Jul-2014

GENERAL COMMENTS	The authors have fully addressed my previous comments.