PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Disparities in health and access to health care between asylum seekers and residents in Germany: a population-based cross-sectional feasibility study
AUTHORS	Schneider, Christine; Joos, Stefanie; Bozorgmehr, Kayvan

VERSION 1 - REVIEW

REVIEWER	Mark Harris University of New South Wales Australia I work in a voluntary capacity as medical officer in a clinic run by a non government organisation for asylum seekers in Sydney.
REVIEW RETURNED	21-Jun-2015

GENERAL COMMENTS	This is an important paper. It is one of very few that have attempted to quantify need and access to health care by asylum seekers. The paper is well written. The low participation rate deserves comment – 25.4% of those approached and 15.3% of the eligible population registered in the three counties. This is comparable with that found in other countries. It is likely to be for a number of reasons: 1. Languages not covered by the translations available (as discussed by the authors); 2. Lack of literacy of those approached in their own language; or 3. Fear of governments and anxiety arising from persecution in their country of origin, transit or destination. All three factors should be acknowledged and discussed in the paper. Supplementary table S1 contains information on the highest degree of education and should be included in the main paper as it is important in comparing the data in this study with other studies. The proportion with low or primary school education only (42.5%) suggests some participation bias may have occurred. It would also be useful to discuss the mix of government and non-government funded health care services available to asylum seekers. In many countries non-government and charitable organisations provide physical and/or mental health services to asylum seekers without charge. Is this the case in the counties studied? For example if psychological services were provided by these organisations it might account for some of the increased use of these services in the sample. Regardless there should be some more discussion of the very high rates of access to psychotherapist especially in young people and what local service factors may influence this. This may indeed relate to the severity of mental

	illness or suicidality but more description of how and what psychological services are available to asylum seekers would be useful. It may also be useful to cross-tabulate use of psychotherapist and hospitalisation (although the numbers may be too small). A minor methodological issue is that the recruitment occurred in three counties. It would be useful to have some analysis of any differences between counties. There is a grammatical error in the last sentence of the discussion (page 22, line 17: “expectable”).

REVIEWER	Anton Kunst Department of Public Health AMC University of Amsterdam The netherlands
REVIEW RETURNED	13-Jul-2015

GENERAL COMMENTS	1. The sample size seems too small for stratified analysis by age, gender or other characteristics. Resulting 95% confidence intervals are very broad. I suggest removing the stratified analysis, or interpreting the results with great caution. That is, avoid qualifying some results as ‘non-significant are non-significant’ when the power was too low to demonstrate even large differences. Moreover, be very cautious in mentioning the results of stratified analysis in the Abstract or “Conclusions” at the end. 2. The value of referring to ECHI indicators is overrated. This is an empirical analysis that uses health interview survey data to compare two population groups. The data should be judged against ability to assess inequalities, i.e. their validity, comparability and precision. Whether or not the indicators happen to correspond to some European indicator list is of secondary or tertiary importance. 3. It is unclear whether the survey questionnaire used in the survey among AS respondents is identical to the questionnaire used in the survey in the general Germen population. These questionnaire should be identical. If not, the resulting bias should be evaluated in detail. 4. The very low non-response rates affect the validity of the results. This limitation should be discussed as well. 5. All regression analysis should control for age and gender. Otherwise, the comparisons are highly biased because of the younger age structure of the AS population. “Crude” analysis are too crude for assessing inequalities in health and health care. 6. Regression analysis using health care use as outcome should also control for health variables. This is the common way to take into account ‘need’ and thus measure ‘horizontal inequity’. The approach taken now, stratifying by a health variable, is uncommon, statistically inefficient, and incomplete (because it does not take into account differences in other health variables). 7. This study is considered a ‘proof of concept’ according to the Discussion section. The main value of this study may indeed be in

	its methodology rather than the empirical results (because of many potential biases). The authors may reflect on the lessons learned with regards to future attempts to measure and compare the health and health care use of AS.

VERSION 1 – AUTHOR RESPONSE

Referee 1
This is an important paper. It is one of very few that have attempted to quantify need and access to health care by asylum seekers. The paper is well written. The low participation rate deserves comment – 25.4% of those approached and 15.3% of the eligible population registered in the three counties. This is comparable with that found in other countries. It is likely to be for a number of reasons: 1. Languages not covered by the translations available (as discussed by the authors); 2. Lack of literacy of those approached in their own language; or 3. Fear of governments and anxiety arising from persecution in their country of origin, transit or destination. All three factors should be acknowledged and discussed in the paper.

** We agree with the reviewer's comment and have accordingly elaborated the reasons for low participation rates in the discussion section (p23).

Supplementary table S1 contains information on the highest degree of education and should be included in the main paper as it is important in comparing the data in this study with other studies. The proportion with low or primary school education only (42.5%) suggests some participation bias may have occurred.

**We have shifted the information on the highest degree of education to the main paper. It is possible that the wording of the questions might have caused a participation bias towards higher educated people, but the proportion of asylum seekers with low or primary school education (42.5%) is in our opinion not really low which disproves a meaningful filtering effect by education. There is no representative data on educational achievement of asylum-seekers entering Germany to compare with.

It would also be useful to discuss the mix of government and non-government funded health care services available to asylum seekers. In many countries non-government and charitable organisations provide physical and/or mental health services to asylum seekers without charge. Is this the case in the counties studied? For example if psychological services were provided by these organisations it might account for some of the increased use of these services in the sample.

**The reviewer raises a very important point. We can confirm that in the study areas no non-governmental funded health care services, particularly psychological services, are available to asylum seekers. But we cannot rule out that existing services have been used in other counties. We have therefore discussed the possibility that NGO provided services may account for the increased use of these services in the sample (p20).

Regardless there should be some more discussion of the very high rates of access to psychotherapist especially in young people and what local service factors may influence this. This may indeed relate to the severity of mental illness or suicidality but more description of how and what psychological services are available to asylum seekers would be useful.

**We agree with the reviewer's comment and therefore we adressed the high access rates to psychological services in the discussion section (p20), outlining the high prevalence and severity of mental diseases.

It may also be useful to cross-tabulate use of psychotherapist and hospitalisation (although the numbers may be too small).

**In light of the small sample sizes (and in light of the concerns of the second reviewer regarding stratified analysis in small samples) we would tend to refrain from further exploratory cross-tabulations.

A minor methodological issue is that the recruitment occurred in three counties. It would be useful to have some analysis of any differences between counties.

**We have provided a descriptive analysis by counties in the appendix (Table S8), but we have refrained from more analytical approaches in light of the small number of counties and the overall small sample size.

There is a grammatical error in the last sentence of the discussion (page 22, line 17: "expectable").

**We have reworded this sentence.

Referee 2

1. The sample size seems too small for stratified analysis by age, gender or other characteristics. Resulting 95% confidence intervals are very broad. I suggest removing the stratified analysis, or interpreting the results with great caution. That is, avoid qualifying some results as 'non-significant are non-significant' when the power was too low to demonstrate even large differences. Moreover, be very cautious in mentioning the results of stratified analysis in the Abstract or "Conclusions" at the end.

**We agree with the reviewer that the study was under-powered given the exploratory character. Therefore we have refrained from emphasising non-significant results throughout the paper and interpreted point-estimates and 95% CIs in light of the small power. We have stressed in the discussion that the results of the stratified analyses shall be interpreted in an exemplary way and do not represent an absolute validity for Germany or the Federal State.
We have also dropped results of the sex-stratified analysis from the abstract/conclusion.

2. The value of referring to ECHI indicators is overrated. This is an empirical analysis that uses health interview survey data to compare two population groups. The data should be judged against ability to assess inequalities, i.e. their validity, comparability and precision. Whether or not the indicators happen to correspond to some European indicator list is of secondary or tertiary importance.

**We see the value of applying ECHI in the potential comparability and availability of data in several European countries. Therefore we highlighted the potential of these indicators to be used in health interview surveys among asylum-seekers in other countries as well. Whether or not this potential holds against the criteria mentioned by the reviewer (ability to assess inequalities, validity, comparability and precision) was out of scope of the empirical part of this manuscript. We therefore agree with the reviewer that the ECHI might not be of highest importance in the current paper. We have changed the title and conclusions accordingly and have included in the discussion a section to emphasize that the ability of these indicators in measuring inequalities between/within countries needs yet to be proved in future studies.

3. It is unclear whether the survey questionnaire used in the survey among AS respondents is identical to the questionnaire used in the survey in the general Germen population. These questionnaire should be identical. If not, the resulting bias should be evaluated in detail.

**The questionnaire used in the survey among asylum seekers is designed using identical questions which were used in the survey among the German reference population. Wording of the questions in German language is identical. We added this relevant information in the methods section.

4. The very low non-response rates affect the validity of the results. This limitation should be discussed as well.

**We have discussed the low response rates with respect to validity of results in the discussion (p23)

5. All regression analysis should control for age and gender. Otherwise, the comparisons are highly biased because of the younger age structure of the AS population. "Crude" analysis are too crude for assessing inequalities in health and health care.

**It is important to note that the stratified analysis were calculated exclusively by cross tabulations. Regression analyses could not be performed because microdata on the german population were not yet available as a public use file when the study was conducted. Owing to the exploratory character of the study we decided to perform cross tabulations using published data for the reference population rather than suspending analyses until publication of microdata became available.
We acknowledge that scaling up this study requires regression analyses using microdata. We specified the methodology accordingly, emphasized the exclusive use of cross-tabulations to avoid misunderstandings, and discussed this as a further study limitation (p24).

6. Regression analysis using health care use as outcome should also control for health variables. This is the common way to take into account 'need' and thus measure 'horizontal inequity'. The approach taken now, stratifying by a health variable, is uncommon, statistically inefficient, and incomplete (because it does not take into account differences in other health variables).

**We acknowledge the incomplete nature of the applied statistical approach. As mentioned above, all analyses were performed by cross tabulations due to a lack of timely access to micro data on the German reference population. We have emphasised this in the discussion to avoid misunderstandings (p24).

7. This study is considered a 'proof of concept' according to the Discussion section. The main value of this study may indeed be in its methodology rather than the empirical results (because of many potential biases). The authors may reflect on the lessons learned with regards to future attempts to measure and compare the health and health care use of AS.

**As the reviewer correctly points out our intention was to provide a methodology in order to assess the health status of asylum seekers. We have further stressed that the additional value of this study lies in the methodology rather than in the findings and that findings shall be interpreted in light of the exploratory study design (p20).

VERSION 2 – REVIEW

REVIEWER	Anton Kunst Department of Public Health University of Amsterdam Netherlands
REVIEW RETURNED	27-Aug-2015

GENERAL COMMENTS	The authors have responded adequately to my comments on the methodology used in the previous version of this manuscript. I have some minor comments to the text of the new version. 1. Even though the authors have now revised the value of ECHI for their own work, they still could describe the role of ECHI in their manuscript with greater accuracy. The authors describe ECHI as "measure" (in the strengths and limitation box) or a "proof of concept" (page 7). Yet, ECHI is nothing else than a list of indicators to be used for health monitoring system, not a measurement instrument to be used as such in interview surveys. 2. In tables 2 and 3, ORs tend to be larger (i.e. more deviant from 1.00) in age-stratified analyses as compared to all-age crude analyses. This discrepancy indicates the substantial bias that is inherent to the crude analyses for all ages together. The authors should recognise this confounding bias. 3. The authors may need to check the OR as presented in tables 2 and 3. For example, at the pre-final row of table 3, the OR is 12.57, which seems too high compared to the corresponding prevalence rates of 34.6% and 9.3%. 4. Table 4 could be presented in less detail. Distinction by gender does not seem informative. 5. In the second paragraph of the Discussion, the emphasis on "health voucher" is not supported by the limited number of respondents who mention this to be a barrier to health care use (Table 4). Several other factors appear to be more important. 6. At the prefinal line of page 19, at "4 to 12 times", the authors seem to interpret ORs in terms of prevalence rate ratios. The authors will recognise that such an interpretation may be misleading. 7. The text from the last paragraph of page 20 to third paragraph of page 22 should be preceded by the evaluation of limitation of their data. The authors cannot make recommendations regarding the future application of their method without a detailed account of the limitations of this method. 8. Related to this, given the many limitations to their method, it seems that the authors should be primarily concerned with generating new estimates with greater internal and external validity for Germany (or for other countries). Preparing international comparisons (as discussed at page 22) are a bridge too far. International overviews are even still hard to make for well-established migrant groups. 9. Reference 29 is a publication unavailable to an international audience and not published in a national scientific journal. At page

	21/22, the authors should not make far-reaching conclusions on the basis of this obscure publication only.

VERSION 2 – AUTHOR RESPONSE

Referee 1

1/ Even though the authors have now revised the value of ECHI for their own work, they still could describe the role of ECHI in their manuscript with greater accuracy. The authors describe ECHI as "measure" (in the strengths and limitation box) or a "proof of concept" (page 7). Yet, ECHI is nothing else than a list of indicators to be used for health monitoring system, not a measurement instrument to be used as such in interview surveys.

**We specified the meaning and value of ECHI throughout the paper. We did not regard ECHI as a proof of concept but rather the general approach. We have shifted the term "proof of concept" to precede references to ECHI to avoid such misunderstandings. We have dropped the reference to ECHI in the strengths and limitations box, where the link with "measure" was indeed misleading.

2/ In tables 2 and 3, ORs tend to be larger (i.e. more deviant from 1.00) in age-stratified analyses as compared to all-age crude analyses. This discrepancy indicates the substantial bias that is inherent to the crude analyses for all ages together. The authors should recognise this confounding bias.

**We presented this confounding bias as a further limitation of our study (p.25) and emphasized the relevant confounding by age in the results section (p.17, bottom).

3/ The authors may need to check the OR as presented in tables 2 and 3. For example, at the pre-final row of table 3, the OR is 12.57, which seems too high compared to the corresponding prevalence rates of 34.6% and 9.3%.

**We thank the reviewer for locating this mistake in our calculation. Indeed, the correct OR for AS in age-group 18-24 is 5.17 instead of 12.57. We eliminated this error in tables and our results and adjusted all statements related to this wrong estimate. We checked and corrected all further calculations and guarantee that they are correct now.

4/ Table 4 could be presented in less detail. Distinction by gender does not seem informative.

**We agree with the reviewer's comment and removed redundant data from table 4.

5/ In the second paragraph of the Discussion, the emphasis on "health voucher" is not supported by the limited number of respondents who mention this to be a barrier to health care use (Table 4). Several other factors appear to be more important.

**We discussed the fact that AS experience other factors (such as financial barriers) more restricting than the actual denial of a health care voucher (P. 20).

6/ At the prefinal line of page 19, at "4 to 12 times", the authors seem to interpret ORs in terms of prevalence rate ratios. The authors will recognise that such an interpretation may be misleading.

**We have reworded the sentence to avoid misleading interpretations.

7/ The text from the last paragraph of page 20 to third paragraph of page 22 should be preceded by the evaluation of limitation of their data. The authors cannot make recommendations regarding the future application of their method without a detailed account of the limitations of this method.

**We have shifted the section “strengths and limitations” to page 22 to precede the sections in which we discuss the need to scale up such analyses with valid and reliable approaches. We have also re-emphasized the exploratory character and the methodological limitations of this study.

8/ Related to this, given the many limitations to their method, it seems that the authors should be primarily concerned with generating new estimates with greater internal and external validity for Germany (or for other countries). Preparing international comparisons (as discussed at page 22) are a bridge too far. International overviews are even still hard to make for well-established migrant groups.

**We have dropped the emphasis on the potential use in international studies and have stressed the need to validate this concept on a national level before conducting international comparisons.

9/ Reference 29 is a publication unavailable to an international audience and not published in a national scientific journal. At page 21/22, the authors should not make far-reaching conclusions on the basis of this obscure publication only.

**The publication is an official national health monitoring report of the Swiss Health Agency. To the best of our knowledge, the findings of the report are not published elsewhere. We agree with the reviewer that the insights are unavailable to an international audience. Since we have reduced the emphasis on international comparisons, we have decided to drop the reference to Switzerland completely.