PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	The impact of the asylum process on mental health: a longitudinal study of unaccompanied refugee minors in Norway
AUTHORS	Jakobsen, Marianne; Meyer DeMott, Melinda; Wentzel-Larsen, Tore; Heir, Trond

VERSION 1 - REVIEW

REVIEWER	Cécile Rousseau McGill University, Canada
REVIEW RETURNED	21-Dec-2016

GENERAL COMMENTS	This longitudinal study examines the effect of placement facility and asylum application decision on the mental health of unaccompanied refugee minor in Norway. The research is overall well designed. Attrition is quite significant at 26 months (50%), although this is to be expected with this hard to reach and very mobile youth population. The data analysis plan is appropriate for the temporal design. It is not clear however if, and how, the issue of missing data was handled (a part from looking at differences in baseline data). This would need to be clarified. Also, in terms of design, the authors specify that the youth included in the expressive arts intervention group did not participate to the study. Was this group different in any ways? (i.e.: more engaged, or manifesting more problems?). The data collection method is age appropriate and nicely organized. The results are unsurprising and in line with the existing literature on asylum seekers (see Steel among others). The discussion needs however some important reworking. As presented in the discussion and in the abstract, placement in a low support facility is the main factor associated with higher levels of psychological distress. However it also seems clear that being categorized as an adult (and transferred in such a facility) increased significantly the risk of claim refusal. A fact which was certainly known in the community and among the youth. It is thus very difficult (if not impossible) to dis-entangle the effect of the placement facility from the despair/changes in expectations associated with a reduced chance of obtaining asylum. The interpretation of results need to emphasize more this clustering of risk factors. The discussion on the ethical and clinical problems associated with age determination processes is warranted. It invites to shift the attention from a narrow focus on age to a consideration of physiological-psychological dimension of development in the

	transition from adolescence to early adulthood. It could be interesting to mention the position of some pediatric associations (Sweden among others) opposing age determination processes. In conclusion this is an interesting contribution to the field which could be enhanced through revisions.

REVIEWER	Zachary Steel School of Psychiatry, University of NSW, Australia
REVIEW RETURNED	31-Dec-2016

GENERAL COMMENTS	This is an important study undertaken with a difficult to access highly vulnerable population. The paper has important human rights implications documenting the adverse impact of immigration policies on mental health and protection outcomes for unaccompanied minors. In particular the study documents the adverse impact of highly contentious biological age assessments on unaccompanied minors and young adults. I have a few minor recommendations for improving the manuscript. The authors describe this as a male convenience sample but I do not think this does justice to the sampling frame adopted. While the study does not adhere to a random sample or consecutive recruitment form the asylum centre it is closest to this type of sampling . It may be more therefore be more accurate to say that "The sample in this study was recruited from an asylum reception centre for unaccompanied asylum-seeking adolescents between ages 15 and 18 years, which was the only one in Norway at this time." to replace the first sentence. There should be a discussion of the likely representativeness of the sample in the strengths and limitations section If available can the authors provide additional reason for lost to follow up – particularly those deported under immigration procedures. The data analysis approach seemed to be appropriate and robust The statistics on caseness in Table 2 for distress and PTSD are not clear for example for distress what does the 92 mean is it 92/132 which is not 46%. These caseness figures should also be described in the results. The scale on figures 1 and 2 should be adjusted to reflect the full range of the measures – this would provide a more accurate pictorial representation.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Cécile Rousseau
Institution and Country: McGill University, Canada
Please state any competing interests: None declared

Please leave your comments for the authors below

This longitudinal study examines the effect of placement facility and asylum application decision on the mental health of unaccompanied refugee minor in Norway.
The research is overall well designed.

Attrition is quite significant at 26 months (50%), although this is to be expected with this hard to reach and very mobile youth population. The data analysis plan is appropriate for the temporal design. It is not clear however if, and how, the issue of missing data was handled (a part from looking at differences in baseline data).
This would need to be clarified.
This has been clarified on page 8.
Also, in terms of design, the authors specify that the youth included in the expressive arts intervention group did not participate to the study. Was this group different in any ways? (i.e.: more engaged, or manifesting more problems?).
More information is added on page 4-5.

The data collection method is age appropriate and nicely organized.
The results are unsurprising and in line with the existing literature on asylum seekers (see Steel among others). The discussion needs however some important reworking. As presented in the discussion and in the abstract, placement in a low support facility is the main factor associated with higher levels of psychological distress. However it also seems clear that being categorized as an adult (and transferred in such a facility) increased significantly the risk of claim refusal. A fact which was certainly known in the community and among the youth.
It is thus very difficult (if not impossible) to dis-entangle the effect of the placement facility from the despair/changes in expectations associated with a reduced chance of obtaining asylum. The interpretation of results need to emphasize more this clustering of risk factors.
Thanks for these very relevant comments. We have expanded the discussion on this topic on pages 15-16.

The discussion on the ethical and clinical problems associated with age determination processes is warranted. It invites to shift the attention from a narrow focus on age to a consideration of physiological-psychological dimension of development in the transition from adolescence to early adulthood. It could be interesting to mention the position of some pediatric associations (Sweden among others) opposing age determination processes.
We agree that this is a problematic legal/medical/dental-area, and we are aware that both professional organizations and ethics committees from different countries have been critical towards this practice. We have added some more comments on p.17, but prefer not to name specific countries.

In conclusion this is an interesting contribution to the field which could be enhanced through revisions.

Reviewer: 2
Reviewer Name: Zachary Steel
Institution and Country: School of Psychiatry, University of NSW, Australia
Please state any competing interests: None declared

Please leave your comments for the authors below

This is an important study undertaken with a difficult to access highly vulnerable population. The paper has important human rights implications documenting the adverse impact of immigration

policies on mental health and protection outcomes for unaccompanied minors. In particular the study documents the adverse impact of highly contentious biological age assessments on unaccompanied minors and young adults.

I have a few minor recommendations for improving the manuscript. The authors describe this as a male convenience sample but I do not think this does justice to the sampling frame adopted. While the study does not adhere to a random sample or consecutive recruitment form the asylum centre it is closest to this type of sampling. It may be more therefore be more accurate to say that "The sample in this study was recruited from an asylum reception centre for unaccompanied asylum-seeking adolescents between ages 15 and 18 years, which was the only one in Norway at this time." to replace the first sentence.
This has been done, p. 4.

There should be a discussion of the likely representativeness of the sample in the strengths and limitations section
We have expanded the discussion to include this, p.16.

If available can the authors provide additional reason for lost to follow up – particularly those deported under immigration procedures.
We have limited information here, but have included what we have, p.5.

The data analysis approach seemed to be appropriate and robust

The statistics on caseness in Table 2 for distress and PTSD are not clear for example for distress what does the 92 mean is it 92/132 which is not 46%. These caseness figures should also be described in the results.
Thanks for discovering and pointing out these mistakes in our manuscript. The figures were calculated for an earlier version of the article, based on data from a larger population, and by mistake they were not recalculated for the present version. This has been corrected (table 1), and the numbers are described in the results, p. 6-7.

The scale on figures 1 and 2 should be adjusted to reflect the full range of the measures – this would provide a more accurate pictorial representation.
We agree that this would be more accurate. New figures will be submitted.

VERSION 2 – REVIEW

REVIEWER	Cécile Rousseau McGill University, Montreal, Quebec Canada
REVIEW RETURNED	09-Feb-2017

GENERAL COMMENTS	The manuscript is very much improved and the authors have nicely responded to the reviewers request, in the limits permitted by the data. The paper is now a good contribution to the litterature on UM.

REVIEWER	Zachary Steel School of Psychiatry UNSW St John of God Health Care Richmond Hospital Black Dog Institute Australia
REVIEW RETURNED	23-Feb-2017

GENERAL COMMENTS	Thank you for the opportunity to review this study a second time. The revisions have helped to improve the manuscript. I do have a few additional comments for the authors yto considered which are all designed to help to improve what I think is an extremely important study that will be seminal in the field. 1. Can the authors include information on any baseline demographic and symptom differences in those lost to followup. 2. I am not entirely sure why the authors have only used HSCL total scores in the mixed regression analyses. If PTS is to form the focus of a separate paper then this is fine but should be made more clear. If the results are not to be reported elsewhere I would encourage the authors to consider reporting on HTQ PTS; HSCL Dep; HSCL anxiety in the missed models. 3. As a reader there are challenges in interpretation of the mixed model results as the variables being used in the adjusted models change at each time point. The subscript note to tables 3-5 that they were "Adjusted for whether subjects participated in initial 5 week expressive arts group-intervention" is unclear as the participants section seemed to suggest that this group was not included. The authors should make it clear in each table the specific variables used to adjust the adjusted model at that time point as listed in the methods. 4. The authors are correct to highlight the difficult interpretation due to the confounding of age assessments, asylum outcomes and psychological symptoms. In addition to the possible knowledge the young asylum seekers may have had about an association with their asylum claims it is also possible that the asylum interviews were more adversarial for those who had adverse age assessments. Indeed this adverse age assessment may have been used to question testimonial credibility in the asylum assessment process, a possibility that should be noted in the manuscript. 5. I think it is worth noting in the final section that despite over half of this sample having marked symptoms of PTSD at a level suggesting a positive diagnosis, access to psychiatric care was not evident for any of the participants.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Cécile Rousseau
Institution and Country: McGill University, Montreal, Quebec Canada Please state any competing interests: None declared

Please leave your comments for the authors below

The manuscript is very much improved and the authors have nicely responded to the reviewers request, in the limits permitted by the data. The paper is now a good contribution to the litterature on UM.

Reviewer: 2
Reviewer Name: Zachary Steel
Institution and Country: School of Psychiatry UNSW, St John of God Health Care Richmond Hospital, Black Dog Institute, Australia Please state any competing interests: Nil

Please leave your comments for the authors below

Thank you for the opportunity to review this study a second time. The revisions have helped to improve the manuscript. I do have a few additional comments for the authors yto considered which are all designed to help to improve what I think is an extremely important study that will be seminal in the field.

1. Can the authors include information on any baseline demographic and symptom differences in those lost to followup.

There are participants lost to follow-up at all the different time points, and even a few who reappeared. When we compared the ones who have completed all four assessments (n=51) with those who missed out on one occasion or more (n=87) there are no significant differences. We have included one sentence about this in the article.p.5-6

2. I am not entirely sure why the authors have only used HSCL total scores in the mixed regression analyses. If PTS is to form the focus of a separate paper then this is fine but should be made more clear. If the results are not to be reported elsewhere I would encourage the authors to consider reporting on HTQ PTS; HSCL Dep; HSCL anxiety in the missed models.

We chose to use only one outcome variable for regression analyses (HSCL total) out of concern for the size and the numbers of tables. HSCL Dep and HSCL Anx have been shown to have weak specificity and sensitivity for the respective diseases in highly symptomatic populations (Jakobsen, Johansen, & Thoresen, 2011; Turner, Bowie, Dunn, Shapo & Yule, 2003). However, the total score is known to be a good indicator of general psychological distress. The PTSD-scores are used in another publication (submitted) with a larger n (including all individuals participating in the intervention study).

3. As a reader there are challenges in interpretation of the mixed model results as the variables being used in the adjusted models change at each time point. The subscript note to tables 3-5 that they were “Adjusted for whether subjects participated in initial 5 week expressive arts group-intervention” is unclear as the participants section seemed to suggest that this group was not included.

This was a mistake, and should have been deleted from an earlier version of the article. We are grateful that this mistake was discovered.

The authors should make it clear in each table the specific variables used to adjust the adjusted model at that time point as listed in the methods.

We have changed the heading for tables 3-5, and included a comment in “methods” p.6.

4. The authors are correct to highlight the difficult interpretation due to the confounding of age assessments, asylum outcomes and psychological symptoms. In addition to the possible knowledge the young asylum seekers may have had about an association with their asylum claims it is also possible that the asylum interviews were more adversarial for those who had adverse age assessments. Indeed this adverse age assessment may have been used to question testimonial credibility in the asylum assessment process, a possibility that should be noted in the manuscript.

This has been done, p.16.

5. I think it is worth noting in the final section that despite over half of this sample having marked symptoms of PTSD at a level suggesting a positive diagnosis, access to psychiatric care was not evident for any of the participants.

We agree that this is a serious concern, p.17.