Author's response to reviews

Title: Attachment and coping in psychosis in relation to spiritual figures
Attachment and religion in psychosis Philippe Huguelet1, M.D. Sylvia Mohr1, Ph.D., Isabelle Rieben2, Ph.D., M.A., Roland Hasler1, Ph.D., Nader Perroud1, M.D., Pierre-Yves Brandt2, Ph.D. 1 University Hospital of Geneva and University of Geneva, Division of Adult Psychiatry, Rue du 31-Decembre 8, 1207 Geneva / Switzerland 2 Lausanne University, Faculty of Theology, BFSH 2, 1015 Lausanne/ Switzerland Pr Philippe Huguelet (corresponding author) University Hospital of Geneva Department of Mental Health and Psychiatry, Division of General Psychiatry Secteur Eaux-Vives Rue du 31 Decembre 8, 1207 Geneva/ Switzerland Tel: +41 22 382 31 03 Fax: +41 22 382 31 05
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Author's response to reviews: see over
Dear Editor,

Thank you very much for your reply to our submission of the above-mentioned manuscript and for the reviewer’s comments. Please find enclosed the revised version of our paper and our responses to the four reviewer’s comments indicating how we have revised the manuscript.

Reviewer: ilanit Hasson-Ohayon
Reviewer’s report:
Major comments:
1- The paper lacks focus. Is it on Trauma and psychosis, attachment and religion with association with symptoms? Does it test two competitive models of attachment to a spiritual figure? Is it exploratory in nature or are there any specific hypotheses? Is it on coping as mentioned only in the title?

Indeed this is a large work exploring both attachment style and attachment to religious figures. In order to clarify this issue (also keeping in mind point 3),
   1) subtitles have been added in text;
   2) title of the paper has been modified;
   3) hypotheses have been changed in order to be more specific (see point 5).

2- The word causality or casual appear more than 10 times in the text. Although the authors mention the problems of inferring causality in their study and in others, I suggest totally avoiding suggesting that attachment style is a risk factor for psychosis. The study with small N, cross-sectional, therefore causality is not an issue. Also, in the literature attachment style, as well as additional variables that express bonding aspects, are perceived as affecting the illness trajectory and process and not to cause the illness.

This remark is in accordance with both literature and our findings, and is worth by itself to be included in the paper. We choose to underline our results by adding it in the discussion section, because the causality issue persists in psychiatry:

“Therefore, unreliable attachment affects the illness trajectory and process and is not a cause of the illness.”

3- Editing: in relation to lack of focus, the paper is very hard to follow and the writing is sometimes very vague (e.g. conclusion in the abstract – what is the one underlying dimensions? ; page 11 line 180 the prevalence of attachment, need to have styles followed), In the discussion the paragraphs are too long to follow, and the theme is lost, in the introduction (e.g. page 6) conceptual connection between paragraphs is lacking…

(see point 1)

4- The choice to test attachment to a spiritual figure qualitatively with the AAI is not clear. Please base the rationale for this choice. There are available validated instrument that assess attachment to god. In addition, the literature review should include recent studied that discussed attachment to god.

The choice for the AAI was lead by previous research using this instrument with psychotic patients, and by the unavailability of scales in French for attachment to spiritual figures in clinical populations. This has been added in the method section:

“As no validated questionnaire exists in French to assess attachment to spiritual figures among psychotic patients”

5- Aims and hypotheses should be better formulated. To explore attachment style is a too vague statement.

(see point 1)

6- Reliabilities of instrument are missing (alphas for scales and inter-rater with regard to the interview).

All the interviews were conducted by a well-trained PhD-level psychologist (IR). The AAI responses were coded according to Mary Main’s methodology by a blind certified coder. Diagnosis was estimated by comparisons of data from medical chart, current patient’s psychiatrist, and the SKID interview. These precisions were added in the method section.

7- Discussion: as mentioned before this part is too long and not focused. Should be edited extensively.
(see point 1)

Minor comments:
1- How patients were randomly selected? And why the control group is half than the clinical one. Any rationale for this?

The random selection of patients was specified:
All patients meeting diagnosis criteria according to medical records were listed. Participants were randomly selected from this list.

The only rationale for the size of the sample group is feasibility in terms of researchers’ time resources.

2- Page 12 – a table must be numbered and referred to in the text.
Done

3- Page 16 autism better defined as a neurodevelopmental disorder.
Corrected

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

**Quality of written English:** Needs some language corrections before being published

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

**Reviewer:** Rebecca Bargenquast

**Reviewer's report:**

- **Major Compulsory Revisions**
  1. The rationale for the study as described in the Background section is not clear. Please specify why this study is important, and what previous research it is being built upon. The aims of the study are clear but it is unclear why researching this area is important – Will it help us improve treatments for psychosis? Will it give us a better understanding of the disorder?

    Indeed this is a large work exploring both attachment style and attachment to religious figures. In order to clarify this issue hypotheses have been changed in order to be more specific.

- **Minor Essential Revisions**
  2. Please review language used in the Abstract. For example:
     - Under Results in the Abstract – ‘Interviews revealed the transformation of internal working models within the relation to a spiritual figure’ should read ‘Interviews revealed the transformation of internal working models within relation to a spiritual figure’.
     - Under Conclusions in the Abstract – ‘The attachment theory…’ should be ‘Attachment theory…’

    Language has been revised.

  3. Please review language used in the Background section. For example:
     - “… by avoiding to feel the need for closeness or avoiding…” should read “… by avoiding feeling the need for closeness or avoiding…” (line 14)
     - Remove “on psychotic patients” (line 24)
     - ‘Unsecure attachment’ should be ‘insecure attachment’ (error also present in Discussion section)
     - ‘Effects’ should be ‘affect’ (line 40)
     - ‘Figure’ should be ‘figures’ (line 74)

    Language has been revised.

  4. Revise the first sentence under section ‘Coping with spiritual figures and other characteristics’. It is difficult to understand due to length.

    Sentence has been clarified.

  5. The second paragraph of the Discussion introduces new literature (not mentioned in the Background section). I think the research on attachment and mentalization, and family communication and psychosis is better included in the Background section, and possibly linked to your rationale for the study.

    This new literature was added to reflect on our clinical experience after having interviewing the patients. It is why it is introduced in the discussion section.
6. See first paragraph of Discussion section ‘Attachment and trauma’ – second line should read ‘hinder us from’ rather than ‘hinder from’. Language has been revised.

7. See second paragraph of Discussion section ‘Attachment and trauma’ – first line should read ‘attachment style’ rather than ‘attachment’s style’ Language has been revised.

- Discretionary Revisions
8. I think it would be helpful to orient readers by specifying the aims of the study in the Abstract.

Aims of the study were added in abstract.

9. I suggest using words for numbers less than 10 instead of numbers (e.g. 2/3 should be two thirds).
Done

10. The multiple subheadings in the Discussion section makes it read more like multiple small studies rather than one study into attachment, spirituality, and psychosis. I suggest revising the presentation of points in the Discussion section so the information presented is more integrated, and linked back to the original rationale for the study (which needs to be clarified, see point 1).

The aims of the study have been clarified and the discussion structured according to it.

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Needs some language corrections before being published

Reviewer: Susanne Harder
Reviewer's report:
Major Compulsory Revisions
This study explores attachment to a religious figure in a sample of individuals suffering from chronic psychosis (N=30) and a small control sample (N=18) exploring two opposing hypothesis: The correspondence hypothesis suggesting that early child-parent interactions correspond to a person’s relation to a spiritual figure and the compensation hypothesis suggesting that an insecure attachment history would lead to a strong religiousness/spirituality as a compensation for the lack of felt security. The role of attachment in psychosis is gaining interest internationally and the role of religious beliefs in psychosis is a clinically important topic for understanding the role of religious beliefs as either a mental support system helping in time of suffering in contrast to being part of the illness for example in the form of negative delusional religious beliefs. Thus, I find the topic of this study important and relevant, but I find that the manuscript needs major revisions:

1. Abstract and Introduction. The rationale for the study is not well defined. There are several foci i.e. the role of attachment in both development and treatment of
psychosis as well as the association to symptoms and trauma and the role of attachment to spiritual figures. A clearer focus as well as a more well defined rationale for the chosen focus is needed.

Indeed this is a large work exploring both attachment style and attachment to religious figures. In order to clarify this issue hypotheses have been changed in order to be more specific. The aims have been added in the abstract.

A. in the methods section p 8 line 105 it states that all patients were religiously involved. As the control sample was matched to the clinical group, it is not clear why it was not possible to recruit 30 matched controls, and why the study ended up with only 18 in the control group.

Qualitative data needs high resources for coding and analysis. So 20 control were selected, 18 were healthy. This has been explicated:

“Due to resource limitations for qualitative analyses, twenty potential controls were recruited and screened for Axis I psychiatric diagnoses. Two had to be excluded due to meeting criteria of a psychotic disorder.”

B. p 9 line 138. It states her that “As the AAI led to a high level of distress for most of our patients, disorganization, a category of attachment sometimes considered in the attachment literature, was not systematically explored during the interview with respect to the patient’s wellbeing” as the same time at page 10 line 152-155 it states that “Childhood traumatic experiences in relation to attachment figures were extracted from the AAI with a focus on the following points: Multiple traumatic experiences, Separation from first attachment figure, Repeated separation from first attachment figure, Sexual abuse, Violence from parents, Parental psychiatric Disorder traumatic for the subject.” As the main category of disorganized attachment in the AAI is the “Unresolved category” which is assessed based on the questions related to trauma and abuse it is not clear to me which questions were excluded in the AAI in order to not systematically explore disorganization “ during the interview with respect to the patients wellbeing”.

No questions were systematically excluded, rather the researcher conducted the interview with clinical sensibility to each patient.

This was explained précised: “was not systematically explored during the interview according to clinical sensibility to patients’ states. “

c. Further if the questions related to trauma and abuse was assessed in the AAI,
why did they not code the U / unresolved category of the AAI and instead choose to make their own system for extracting trauma from the AAI? No rational for this was reported.
See point b.

d. no data on reliability was provided for any of the measures.

All the interviews were conducted by a well-trained PhD-level psychologist (IR). The AAI responses were coded according to Mary Main’s methodology by a blind certified coder. Diagnosis was estimated by comparisons of data from medical chart, current patient’s psychiatrist, and the SKID interview. These precisions were added in the method section.

3. Results:
a. As the trauma data is very exploratory and not based on a standardized measure such as the “unresolved” in the AAI, I would recommend this data for only descriptive purposes and not used in statistical analyses. (Table 4)
Even if based on clinical interview only, those data are worth to be reported.

B. line 225. It is not clear to me what is meant by a “stable” attachment to a spiritual figure. This need to be defined.

This was a mistake corrected by “secure”.

c. Only associations between symptom severity and attachment pattern to religious figure is reported. This makes it difficult to get an impression of the importance of the spiritual attachment compared to the normal attachment patterns. Is the attachment to at religious figure adding anything not covered by the normal attachment pattern? As most of the participants showed a correspondent attachment pattern to attachment figures and spiritual figures, I would have liked to see the association between symptom and attachment pattern both in relation to spiritual figures and attachment figures.

Those data have been added:
“According to the BPRS, total score was significantly lower in patients with a secure attachment model compared to those with other attachment styles (see Table 3); specifically for anxiety (m=2.33+/-.82 Vs. m=4.23+/1.02, T= 4.18, p<0.001), depression (m=2.33+/-.121 Vs. m=3.86+/1.08, T= 3.00, p<0.01), suspiciousness (m=1.50+/-.84 Vs. m=3.50+/1.10, T= 4.11, p<0.001), unusual thought content (m=1.83+/-.204 Vs. m=3.50+/1.44, T= 2.30, p<0.05), and motor retardation (m=1.00+/-.00 Vs. m=1.95+/1.05, T= 2.21, p<0.05).”

And:
Concerning the relationship between attachment and symptoms, it appeared that patients who featured a secure attachment to primary care giver and/or to a spiritual figure had a better symptom profile for somatic concern (Secure N=11 Vs. Unsecure N=17: m=1.73+/-.90 Vs. m=2.94+/1.56, T= 2.33, p< 0.05), anxiety (m=2.73+/-.90 Vs. m=4.53+/-.87, T= 5.26, p<0.001), depression (m=2.82+/-.25 Vs. m=4.00+/-.106, T= 2.69, p<0.01), guilt (m=1.55+/-.52 Vs. m=2.88+/-.62, T= 2.64, p<0.05), suspiciousness (m=2.00+/1.18 Vs. m=3.76+/-.90, T= 4.47, p<0.001), conceptual disorganization (m=1.27+/-.47 Vs. m=2.47+/-.42, T= 2.69, p<0.05), emotional withdrawal (m=1.55+/-.82 Vs. m=2.59+/1.33, T= 2.33, p<0.05) and motor retardation (m=1.09+/-.30 Vs. m=2.18+/-.07, T= 3.25, p<0.001). However, beyond
this quite large and heterogeneous symptom profile for this subgroup, hallucinations profile was quite similar across subgroups (m=2.91+/-.2.12 Vs. m=2.88+/-.1.41, T= 0.04, p<0.97).

4. Discussion.
The discussion is not enough centered around the data and findings reported in the study, but tend to cover much broader areas not adequately supported by the data. It would improve by being shortened considerable and more focused on the primary outcome of the study, the role of attachment to spiritual figures compared to normal attachment pattern, and the two hypotheses explored in this regard.

Discussion has been reorganized according to the specific aims of the study.

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Needs some language corrections before being published
Statistical review: No, the manuscript does not need to be seen by a statistician.

Reviewer: Hamish McLeod
Reviewer's report:
- Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
p.4 line 52 – researches should be “research”
p.11, line 181 – “patients displayed same levels of…” should be “the same…”
p.17, line 311 – “unsecure” should be “insecure” and again on lines 315 and 316
p.17 line 322 – “relation” should be “relationships”
p.17 line 326 – “researches” should be “research”
p.17 line 329 – “natures” should be “nature”
p.18 line 355 – “adhesion” should be “adherence”
p.22 line 443 – “transversal” should be “cross-sectional”
p.22 line 447 – “built” should be “build”
Language has been revised.

For ease of reading - Table 1 and 2 should indicate which between group differences are significant (even though this is described in the text)
Table 2 has been completed.

- Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)
Overall, this is a small study with mixed methods elements (e.g. the use of transcript quotes to exemplify attachment states of minds relative to spiritual figures). It addresses the interesting functional question of whether IWMs are adjusted to compensate for poor primary attachment experiences or whether the early formed attachment style corresponds to the style exhibited with spiritual figures. This interesting and relatively novel set of questions is drowned out somewhat by the over-inclusive discussion. In my view, presenting the study as more clearly a pilot exploration of the interface between attachment style and spiritual functioning would be more appropriate. The very small cell sizes for some sub-groups and analyses makes the data very preliminary. A briefer, more
focused report will probably stimulate some interest and be of use to future researchers.

Some specific amendments and the corresponding manuscript locations are as follows:

p.8 the paragraph describing the recruitment of the healthy control comparator group is very unclear and should be thoroughly revised. The recruitment method has been clarified.

p.13-14, lines 224-239 – The section describing BPRS item level comparisons between patients with different attachment styles and then comparing patients combined with controls is presented without primary data, measures of dispersion, effect sizes etc. These data should be presented more clearly.

Those data have been added:

According to the BPRS, total score was significantly lower in patients with a secure attachment model compared to those with other attachment styles (see Table 3); specifically for anxiety (m=2.33+/-.82 Vs. m=4.23+/-.02, T= 4.18, p<0.001), depression (m=2.33+/-.21 Vs. m=3.86+/-.08, T= 3.00, p<0.01), suspiciousness (m=1.50+/-.84 Vs. m=3.50+/-.10, T= 4.11, p<0.001), unusual thought content (m=1.83+/-.04 Vs. m=3.50+/-.14, T= 2.30, p<0.05), and motor retardation (m=1.00+/-.00 Vs. m=1.95+/-.05, T= 2.21, p<0.05).

And:

Concerning the relationship between attachment and symptoms, it appeared that patients who featured a secure attachment to primary care giver and/or to a spiritual figure had a better symptom profile for somatic concern (Secure N=11 Vs. Unsecure N=17: m=1.73+/-.90 Vs. m=2.94+/-.56, T= 2.33, p< 0.05), anxiety (m=2.73+/-.90 Vs. m=4.53+/-.87, T= 5.26, p<0.001), depression (m=2.82+/-.125 Vs. m=4.00+/-.106, T= 2.69, p<0.01), guilt (m=1.55+/-.52 Vs. m=2.88+/-.62, T= 2.64, p<0.05), suspiciousness (m=2.00+/-.18 Vs. m=3.76+/-.90, T= 4.47, p<0.001), conceptual disorganization (m=1.27+/-.47 Vs. m=2.47+/-.142, T= 2.69, p<0.05), emotional withdrawal (m=1.55+/-.82 Vs. m=2.59+/-.33, T= 2.33, p<0.05) and motor retardation (m=1.09+/-.30 Vs. m=2.18+/-.07, T= 3.25, p<0.001). However, beyond this quite large and heterogeneous symptom profile for this subgroup, hallucinations profile was quite similar across subgroups (m=2.91+/-.12 Vs. m=2.88+/-.41, T= 0.04, p<0.97).

p.14 line241-253 – The correspondence and compensation models are tested by comparing cell frequencies for matching or mismatch between attachment states of mind for primary care giver and spiritual figure relationships. However, the frequencies are not clear. The attached table depicts what I extracted from the written material but this appears incomplete. Please revise for clarity.

Parts of the discussion read more like a literature review and cover material that is less directly related to the primary aims and predictions of this study. This detracts from the sections where the authors attempt to carefully analyse and critique the results and link them to previous research (e.g. the consistent association between psychosis and trauma).

Discussion has been reviewed.
Level of interest: An article of limited interest
Quality of written English: Needs some language corrections before being Published
Language has been revised.

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

We thank the reviewer for his/her careful revision and we hope that this revised version will make a valuable contribution to “BioMed Central”.

Yours sincerely,

On behalf of the authors,

Pr. Ph. Huguelet, MD