Author's response to reviews

Title: Standard values and relationship specific validity of the Bielefeld Relationship Expectations Questionnaire (BFPE)

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Author's response to reviews: see over
Resubmission of manuscript

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Dear Dr. Cassady-Cain,
Dear Dr. Koutsos,

Please accept our profound apologies for the delayed submission. Thank you very much for considering our manuscript “Standard values and relationship specific validity of the Bielefeld Relationship Expectations Questionnaire (BFPE)”. In the following, we have addressed the issues raised by the reviewers point-by-point. Please note that the reviewers’ comments are typed in italics.

We look forward to hearing from you.

Sincerely yours,

Katja Petrowski, PhD
Comments by Dr Cassady-Cain:

The referees have indicated that your submission would benefit from better presentation and English usage. We recommend that you ask a native English-speaking colleague to help you copy-edit the paper. If this is not possible, you may need to use a professional copy-editing service. Examples are those provided by the Manuscript Presentation Service (www.biomedes.co.uk), International Science Editing (http://www.internationalscienceediting.com/) and English Manager Science Editing (http://www.sciencemanager.com/). BioMed Central has no first-hand experience of these companies and can take no responsibility for the quality of their service.

Thank you for your comments. The reviewers’ remarks and suggestions are highly appreciated. Following their advice, the ambiguous language as well as inconsistent and inappropriately presented material were eliminated. The manuscript was then copy-edited by a native English-speaking linguist. Hopefully, revising the original text has now clarified the different issues and improved the quality of the paper.

In addition, we are not sure that your authors’ all fulfill the criteria for authorship. Our policy on authorship is: An "author" is generally considered to be someone who has made substantive intellectual contributions to a published study. To qualify as an author one should 1) have made substantial contributions to conception and design, or acquisition of data, or analysis and interpretation of data; 2) have been involved in drafting the manuscript or revising it critically for important intellectual content; and 3) have given final approval of the version to be published. Each author should have participated sufficiently in the work to take public responsibility for appropriate portions of the content. Acquisition of funding, collection of data, or general supervision of the research group, alone, does not justify authorship. Please review your author list and amend as appropriate.

Thank you for explaining the above. We have now pointed out each individual author’s contributions correctly.

We would be grateful if you could address the comments in a revised manuscript and provide a cover letter giving a point-by-point response to the concerns. We look forward to receiving your revised manuscript by 22 February 2010. If you imagine that it will take longer to prepare, please, give us some estimate of when we can expect it.

We will now continue with the point-by-point treatment of the various concerns:

Reviewer #1:

In assessing the ms. in accord with the BMC guidelines, I considered the following points:
1. Is the question posed by the authors well defined?
Some of the questions were well defined, but others were not. Most specifically, the information regarding specificities for the various scales was not well presented at all, so that part of the results was very confusing.

Thank you very much for this comment. We have extended and clarified the background section and hope that it is no longer confusing.

2. Are the methods appropriate and well described?
In general, the methods appear to be appropriate. However, I have provided details below about how I think the methods were poorly described, and the authors should fix these problems.

This comment will be answered in detail below.

3. Are the data sound?
The data are the soundest part of this ms. The data derive from a nationally representative sample, so the sampling appears to be very strong – and the sample size is rather large.
However, I have concerns about some of the instruments used – more information is required to determine whether all of the measurement instruments are sound.

Thank you for pointing out the confusion produced by the lack of information. We have extended the descriptions of the instruments in the Methods section on page 9 -11.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
Yes it does.

Thank you!

5. Are the discussion and conclusions well balanced and adequately supported by the data?
I have concerns about certain results reported in the ms. – these are discussed in detail in my comments for the authors (see below). As a result, I am not sure that some of the conclusions are warranted, because the data may be faulty.

This comment will be answered in detail in the comments below.

6. Are limitations of the work clearly stated?
No. I think the authors tried to make such a statement (see my comments below). The authors made some statement about how the data may not be a good basis for general conclusions, and I think this statement is amiss.

The reviewer observed correctly that the limitation section had to be improved, which we extended on page 16. The comments are answered below.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
The authors cited their own prior research adequately as well as prior work using the BFPE, but the authors did not cite adequately prior research by others using other measuring instruments that is relevant to the current paper.

Yes, the reviewer is correct that the literature on attachment and the construct life satisfaction or social support was not well developed. We have extended this section in the introduction on page 6 and 7 as well as in the discussion on page 15.

8. Do the title and abstract accurately convey what has been found?
Yes.

Thank you!

9. Is the writing acceptable?
Not fully acceptable. There were numerous problems of wording and grammar. The authors should seek the advice of persons more well acquainted with English grammar and composition to fix these problems.

Thanks to the reviewer for this comment. Hopefully, all the incorrect grammar and wording errors have now been eliminated as a native speaker was engaged to revise the manuscript. Every one of the preceding points and our suggestions for improvement will be discussed in the following.

Major Compulsory Revisions
1. p. 4: The Bielefeld Relationship Expectations Questionnaire (BFPE) is the central topic of the current paper. Two or three issues. First, the BFPE has 3 scales (Fear of Rejection,
Readiness for Self Disclosure, and Conscious Need for Care), but the authors never indicated in this ms. how these three scales were developed. Were they derived using common factor analysis or principal component analysis? Some information here would be very valuable.

We are grateful for this advice. Consequently, amends have been made and the factor analyses of the BFPE and BFKE reported in more detail. However, we have tried to keep it brief as the exact procedure was already communicated by Pollack, Wiegand-Grefe & Höger (2008): The Bielefeld Attachment Questionnaires: Overview and empirical results of an alternative approach to assess attachment. Psychotherapy Research, March 2008; 18(2): 179-190.

Second, the authors should discuss how the three dimensions from the BFPE are related to two-dimensional representations obtained in a series of studies by Philip Shaver and colleagues. In the Shaver research, the two large dimensions are termed Anxiety and Avoidance. The traditional three types of attachment are then shown as follows: (a) Anxious attachment – high on Anxiety dimension; (b) Avoidant attachment – high on Avoidance dimension; and (c) Secure attachment – low on both Anxiety and Avoidance. The Shaver approach has been the basis of a large amount of research. How do the BFPE dimensions relate to the Shaver dimensions? What basis is there to argue that the BFPE dimensions are an improvement over the two Shaver dimensions?

This is a very helpful suggestion. We have now cited the study written in English [Pollack 2008], which reports the examining of the overlap between BFPE and AAS [i.e. Buschkämper 2008, unpublished master thesis]. As this study aims on discriminant and not convergent validity, we decided to keep it as brief as possible in the introduction section (pages 5/6) and in the discussion (page 15).

2. p. 7: The authors argued that the relation of attachment dimensions to general life satisfaction has “not yet been investigated.” I am not sure what to make of this claim. Are the authors referring to the relation between life satisfaction and attachment as assessed by the BFPE? If this is the claim, the authors should say so clearly. Prior research using other instruments has looked at general life satisfaction and attachment scores using other instruments. If the authors want to extend this research by using the BFPE, they should cite this prior research and offer hypotheses regarding how BFPE scores related to life satisfaction.

Obviously, our statement could be misinterpreted. Hence, we have unhesitatingly dissociated ourselves from the claim that there are no BFPE-life satisfaction studies in the actually existing literature investigating attachment-life satisfaction. Based on this framework, we have instead offered our own hypothesis (see introduction on page 11 and discussion on page 15).

3. p. 8: It is odd to say that “Approximately 756 men … and 753 women … participated …” Is this the approximate number or the precise number?

You are absolutely correct, we have fixed the oversight.

4. p. 8: The three dimensions – Fear of Rejection, Readiness for Self Disclosure, and Conscious Need for Care – might be better named. I am not sure just what names I would use, but I recommend that the authors find better terms – terms that are more directly related to attachment theory and research.

All three names were borrowed from an article by Höger in Pollack, Wiegand-Grefe & Höger (2008): The Bielefeld Attachment Questionnaires: Overview and empirical results of an alternative approach to assess attachment. Psychotherapy Research, March 2008; 18(2): pp. 179 -190. We had decided to go with these names as they had already appeared in a publication. However, subsequent definitions for each scale as well as item examples have been included in our revised manuscript in order to account for this comprehensible remark.
5. pp. 8-10: The ms. would be improved if the authors provided one or two items for each of the scales used. Particularly important are the dimensions obtained from the Social Support Questionnaire. Social support is measured in many different ways, and many of these ways are incompatible with one another. As a result, simply referring to an instrument as a measure of social support provides almost no indication of what is being measured. The authors should list some example items so readers would know what is being asked.

Thanks for this recommendation. We have added these items as a matter of course (see pages 10 -11).

6. p. 10: For the Life Satisfaction Questionnaire, the authors reported that 10 domains are assessed, with 7 questions per domain. How highly correlated are scores across the 10 domains? Is there a great deal of redundancy? Information here would be valuable.

A most helpful suggestion: The methods paragraph had to be rewritten anyway as the FLZ-module actually consists of 8x2 sub-scales. The range of inter-correlations for the analysed FLZ-sub-scales (in fact, only the 2x2 relationship-specific) are now reported in the results section (page 11).

7. p. 11, top half of page: The authors reported both the split-half reliability and the coefficient alpha for each scale. [Side note: the authors should use the term “coefficient alpha” for these reliability values, not Cronbach’s alpha – Cronbach (1951) wrote a prominent article about coefficient alpha, but coefficient alpha was proposed by someone else prior to this article – and even Cronbach calls it “coefficient alpha.”]. But, the authors never indicated how they computed the split-half reliability. If this was computed as the simple correlation between a sum score on one half of the items (e.g., even numbered items) and a sum score on the other half of the items (e.g., the odd numbered items), then the resulting correlation would have to be adjusted using the Spearman-Brown prophecy formula – because the total scale is twice as long as either split half. For the first scale – Fear of Rejection – the authors stated that the split-half reliability was .76. If this was not adjusted with Spearman-Brown formula, it should be. The Spearman-Brown (SB) adjusted value would be: SB adjusted reliability = \[\frac{2(r)}{1 + r}\] = \[\frac{2(.76)}{1 + .76}\] = .86. This value of .86 is the estimated reliability of the total scale. Incidentally, the .86 obtained in this fashion is identical to the coefficient alpha reported for this scale. This is not surprising, because coefficient alpha is often described as the average of all possible split halves. The authors need to provide more information here about what they did, because the current presentation does not allow an informed evaluation.

This remark was especially helpful for us in order to further improve the manuscript. We renamed the coefficient as proposed. The exact calculation of the reliabilities is now reported and discussed accordingly (see page 12).

8. p. 11: The authors reported sensitivities for each of the scales in Table 1, but never indicated “sensitivity for what.” I am not sure what to make of these purported sensitivities.

With this term, we actually meant confusion. We have now specified it as the corrected item-total correlation.

9. p. 11: The authors should report the magnitude of correlations among the 3 dimensions from the BFPE. They could report both the simple correlations among the 3 dimensions and then the correlations among the 3 dimensions corrected for attenuation. This would provide valuable information about how distinct these three dimensions are empirically.
Thank you for this advice. We have now reported and discussed the inter-correlations between the BFPE scales (see page 12).

10. pp. 11-12 and Table 2: The correlations between dimensions from the BFPE are important to report (as noted in the prior comment), and this importance is underscored here. Why? Because the correlations of the three BFPE dimensions with other variables do not show a strong pattern of discriminant validation. For example, Fear of Rejection correlates strongly positive with Conflict, but negatively with the remaining 3 scores from the PFB; Readiness for Self Disclosure shows the reversed pattern of correlations. Is this because the two BFPE scales are strongly negatively correlated with one another?

This point was well taken, we must admit. We have added the correlations between the BFPE-scales. The reviewer states the core of this finding, which had indeed not been pointed out clearly. This has been taken care of now (see page 12).

11. p. 12, middle of page: The authors discussed the differing levels of correlation of social support with the different BFPE dimensions, emphasizing one correlation (.43), and calling other correlations (-.32 and .20) only “weak.” But, the authors did not report tests of significance of the difference between these correlations. In terms of absolute magnitude, I am not sure that a correlation of .43 differs significantly from a correlation of .32.

We rewrote the whole results section and added a note about the table that refers to it (see pages 12 – 13).

12. p. 12 and Table 2: The authors reported only positive correlations of all BFPE scales with importance of family/children and with partnership/sexuality. Because other results in this table showed that BFPE Fear of Rejection correlated in a different direction (positive vs. negative) with other variables in comparison with both Readiness for Self Disclosure and Conscious Need for Care, I expected that some of the correlations with FLZ importance of family/children and partnership/sexuality would be positive and other correlations would be negative. Are the authors sure that the signs of all of these correlations are correctly reported?

Another useful hint. We have corrected this error.

13. p. 13, bottom half of page: The authors discussed reliability results presented in this ms. relative to values reported in prior studies. However, because I am unsure just how the present authors reported certain values, such as split-half reliabilities, I am not convinced that these points of discussion are correctly considered. Specifically, if the split-half reliabilities in the current study are lower than in prior research, but the coefficient alphas are about the same as in prior research, my conclusion is that the current authors probably reported erroneous estimates of split-half reliability. See Comment 7 above for more specifics.

Thank you very much for this helpful comment. The re-calculated coefficients have now been discussed (see page 12).

14. pp. 13-14: Sensitivities once again discussed in confusing fashion, as it is not clear to what these sensitivities refer.

Thanks for the remark, we corrected the confusing terms as explained earlier.

15. p. 15, middle of page: The authors begin a paragraph with a statement that “Based on the data from a representative sample, general conclusions cannot be drawn …” This is confusing at least, dead wrong at worst. Representative samples should be the basis for stronger
general conclusions than are results from small, non-representative, haphazard samples. Not sure what the authors were trying to say here – re-writing is essential.

This point is well-taken. The sentence has been deleted.

16. p. 15: the authors stated that information from clinical samples, standard values would be indispensable for broad clinical application. However, the authors provided no information in the current ms. about information from clinical samples, so I cannot see how this conclusion is justified based on information contained in the present ms.

Thanks so much for this comment. We have refined the paragraph to make our point more convincing.

Reviewer #2:

This manuscript reports data from a large sample pertaining to the Bielefeld Partnership Expectations Questionnaire. The attempt to provide such psychometric information is laudable. Nonetheless there are serious problems with the manuscript.

Major Compulsory Revisions
First, it was difficult to evaluate this manuscript as it communicates so poorly that, at times, it is virtually impossible to follow. This is important as few readers will bother to read it once they encounter such difficulties. This could merely be an issue of writing in a second language but it is one that urgently needs to be addressed. The authors would be well advised to recruit a native English speaker to help them craft a better manuscript.

We thank the reviewer for these remarks. We hope to have eliminated any inappropriate or confusing language as a native speaker has revised the manuscript.

Once this is done, there are substantive issues that need to be addressed. The most obvious concerns potential overlap between assessments, a problem that is rife in the relationship literature (see Journal of Marriage and the Family, 1987, 49, 797-809).

This point is well-taken. We have addressed it by explaining why another measurement, i.e. the BFPE, was invented, how it relates to existing ones, and where its progress may be arguable.

In addition, there is the issue of sentiment override (Weiss, 1980, Advances in family intervention, assessment and theory (Vol.1, pp. 229-271). Greenwich, CT: JAI Press), whereby spouses respond to questionnaire items about the partner and marriage, not so much in terms of their manifest content, but in terms of their sentiment towards the partner.

A very good suggestion: This argument has been considered as a limitation of our discussion.

Another concern is the nature of the sample. Are we really to believe that every person approached agreed to participate? But this is implied by the lack of data to the contrary.

Thanks for this advice. We have included the coverage rate.

Finally, the authors seem to make inferences that exceed their data. For example, they purport to assess behaviour (cf. “examines the behaviour as well as the experience” when in fact there is no solid data on behaviour. Self report =/= behaviour, it only equals self reports of behaviour which may or may not reflect behaviour in reality.

The reviewer is correct as a self-report is different from observable behaviour. Nevertheless,
the BFPE items refer to patterns of behaviour in attachment-specific situations, in as far as the participant has observed himself/herself and is able to remember it. We have corrected the cited paragraph on page 15 (conclusions).

In short, the manuscript communicates poorly and is limited to answers from questionnaires the very structure of which may have the relationships reported “built in.”

Reviewer #3:

This paper reports about the psychometric properties and the convergent validity of a new adult attachment measure, the Bielefeld Relationship Expectations Questionnaire (BFPE). A distinctive strong point of the study is the use of a large, nationally representative sample, which is not common in the attachment literature. I have some suggestions that may further improve the paper.

Major Compulsory Revisions
(1) The Introduction, in its current version, is difficult to follow and does not clearly convey the study rationale and aims. It should be reorganized to give the readers a clearer picture of the topic and of the specific aim of the study. Starting with a short description of attachment theory would be useful. Also, the reasons behind the decision to develop a new adult attachment measure should be presented in greater detail. Given that there is really a wide array of measures available to researchers, from interviews (e.g., Adult Attachment Interview, Current Relationship Interview) to questionnaires (e.g., Adult Attachment Scale, Experiences in Close Relationships) to short self-descriptive measures (e.g., Relationships Questionnaire), it would be important to specify what issues not adequately covered by existing measures the BFPE intends to address. This is not a trivial point, as there are indeed many instruments available to measure attachment in adults. In the sole field of romantic attachment, already a decade ago Brennan, Clark and Shaver (Brennan, K.A., Clark, C.L., & Shaver, P.R. (1998). Self-report measurement of adult attachment: an integrative overview. In J.A. Simpson & W.S. Rholes (Eds.), Attachment theory and close relationships (pp. 46-76). New York: Guilford Press) were able to factor analyse over 320 items drawn from existing self-report measures.

There are several noteworthy suggestions to be worked out. We have thus provided the reasons as stated in the original studies by Höger and colleagues (see Pollack, 2008) in the introduction on page 4.

(2) After introducing attachment theory and clarifying the reasons behind the decision to develop a new measure, it would be important to provide a more detailed description of the BFPE. While I imagine that it has been described in previous papers, it is nevertheless essential to make this paper completely understandable in its basic features by the readers without having to consult other articles. In particular, it would be useful to clarify what is the object of the assessment (parent-child relationships, romantic relationships, all close relationships including best friends, etc.), and to further elaborate on statements such as ‘the attachment patterns are operationalised as defined patterns of behaviour in attachment-relevant situations’. In this regard, some examples of BFPE items would be extremely useful to give the readers an idea of what is new in this measure with respect to existing questionnaires, many of which do ask respondents about their behaviour and experience in specific situations (e.g., with their romantic partner, or their parents). The sub-scales should also be briefly described.

We thank the reviewer for this comment. We rewrote the whole introduction section following the flow suggested by the reviewer (see page 4,5,6). In addition, the BFPE, its development, psychometric properties, and scales have been described in more detail in the methods section (see page 8).
The Introduction contains a description of five attachment patterns identified by cluster analysis of BFPE scores. A minor problem is the use of the term ‘avoiding’, when the term used in the literature to describe the patterns characterized by high avoidance is ‘avoidant’. A more serious issue is the statement that these five patterns correspond to the classical attachment classification, such as those of Ainsworth, Main, and their colleagues. First, it should be noted that the patterns identified by these authors relate to the parent-child relationship, either in the form of actual behaviour in infants (the Strange Situation) or as in the form of an ‘attachment state of mind’ in adults (the AAI), whereas it seems (judging from the names of the sub-scales) that the BFPE focuses on romantic relationships or close friendships, which suggests that Bartholomew and Horowitz model (Bartholomew, K. & Horowitz, L.M. (1991). Attachment styles among young adults: A test of a four-category model. Journal of Personality and Social Psychology, 61, 226-244) should be taken as a more appropriate reference for comparison of patterns. However, this model, as well as Ainsworth’s and Main’s models, postulates four main categories, rather than five. It would be important to devote effort to reconcile the finding of the cluster analysis with mainstream attachment literature, as the statement that the patterns identified by the cluster analysis of BFPW scores correspond to the usual patterns seems to be open to criticisms.

This was a very helpful remark. ‘Avoiding’ was renamed and other cluster names are now equal to those published by one of the BFPE’s authors [Pollack, Wiegand-Grefe & Höger (2008): The Bielefeld Attachment Questionnaires: Overview and empirical results of an alternative approach to assess attachment. Psychotherapy Research, March 2008; 18(2): pp.179 -190]. According to this citation, relevant overlaps to the AAS and the Bartholomew-model have been presented.

In the Introduction, it is stated that ‘it is still unknown how attachment patterns relate to specific patterns of social support in relationships’. In a similar vein, it is stated that studies of attachment and life satisfaction are lacking. However, this is not correct. Many studies have examined the relationship between attachment and social support (e.g., Collins NL, Feeney BC: A safe haven: an attachment theory perspective on support seeking and caregiving in intimate relationships. J Pers Soc Psychol 2000;78:1053-1073. Moreira JM, Silva MD, Moleiro C, et al: Perceived social support as an offshoot of attachment style. Pers Individ Dif 2003;34:485-501) and between attachment and life satisfaction (e.g., Merz EM, Consedine NS. The association of family support and well-being in later life depends on adult attachment style. Attach Hum Dev 2009;11:203-221; Hinnen C, Sanderman R, Sprangers MA. Adult attachment as mediator between recollections of childhood and satisfaction with life. Clin Psychol Psychother 2009;16:10-21). The cited references are only examples, as there are many relevant papers. It would be appropriate to mention these and other related papers and to discuss the study findings with reference to the extant literature.

Thank you for the help concerning this item. We have expanded the introduction (see pages 6/7) as well as the discussion accordingly (see pages 15/16).

Minor Essential Revisions

In the Introduction, the authors state that attachment theory was developed to explain psychopathology, that most assessment instruments were developed exclusively based on healthy samples, and that the BFPE has an advantage as it was developed using both healthy and clinical samples. On one hand, it would be useful to give some more details about the BFPE development in order to enable the reader to better understand this point. On the other hand, it should be acknowledged that, although Bowlby did extensively wrote about anxiety and depression (in reaction to separation and loss, respectively), many attachment theorists would agree that attachment theory was first and foremost developed to understand
normative development, although it does represent an excellent theoretical framework for psychiatry and clinical psychology.

This is absolutely correct, therefore, we have now described the BFPE in more detail and refined the statement about psychopathology (see page 4).

(6) In the Methods and Results section, the authors use the term ‘sensitivity’ to refer to a psychometric property of the BFPE. This term may puzzle the readers, because in the literature this term is usually used to indicate either the ability of a screening instrument to identify the diseased individuals as positives, or to indicate the ability of an instrument to detect a small but clinically meaningful change in health status (sensitivity to change or responsiveness). The authors should explain what they mean with ‘sensitivity’ in the context of this manuscript.

Thanks a lot for this remark, we have renamed every one of the confusing expressions.

(7) Some parts of the Discussion, i.e., those devoted to the inter-correlations between BFPE scores and the other measures collected in this study, should be further developed to discuss the study findings in the context of the large literature in the field of personality and social psychology, which needs to be examined and cited in more detail.

Taking this excellent advice, we have worked on this and refined the whole paragraph and its conclusions as well as the associated abstract sentences.

(8) On page 15, last paragraph, it is stated that, as compared with other attachment self-report measures, a specific feature of the BFPE is that ‘attachment dimensions as well as attachment patterns can be analysed’. This seems to be an overstatement, as there are other questionnaires (e.g., the ECR) that can be used to provide continuous measures of attachment dimensions (such as attachment-related avoidance and anxiety) and to classify individuals into the 4-category model of Bartholomew and Horowitz on the basis of commonly used cut-offs or of published discriminant functions. Indeed, the literature contains many examples of the use of continuous measure of attachment style to classify individuals into attachment categories. This point would need to be toned down.

The reviewer is absolutely correct. We have included the ECR on page 4, formulating the paragraph more precisely (see page 4).

(9) On page 9, 8th line, ‘prognostic validity’ may be better substituted with the more common ‘predictive validity’.

You are right, thank you!

(10) On page 14, third paragraph, 1st line, ‘received’ seems to be a typo and it should likely be substituted with ‘perceived’.

Of course, thank you!

Discretionary Revisions

(11) In table 2, the authors chose to present only correlations exceeding a cut-off of .30. While this improves the readability, in this way the readers cannot see the whole pattern of the results. A possibly better alternative would be to present all the results, highlighting (e.g., in bold) the more significant ones.

This is very good advice. Hence, all correlations have been presented. In regard to the FLZ, please note that we only analysed two of the FLZ-dimensions, i.e. those who relate to
partnership quality.