Social smiles and laughter preschoolers

REVIEWER 1

S1. GENERAL EVALUATION

ARTICLE TYPE

Q 1

Does this manuscript conform to the definition below of Original Research articles? If not, please contact the Frontiers Editorial Office (editorial.office@frontiersin.org).

Original Research articles report on primary and unpublished studies and should describe the aims and hypothesis, methods, results and interpretation of the research. Original Research articles may also encompass confirming studies, as well as disconfirming results which allow hypothesis elimination, reformulation and/or report on the non-reproducibility of previously published results. The manuscript should include the following: Abstract, Introduction, Material and Methods, Results, Discussion.

Reviewer 1 | 09 Nov 2017 | 16:05

#1

Yes

LANGUAGE AND GRAMMAR

Q 2

Is the language, specifically the grammar, of sufficient quality? If no, please specify if the authors should send this manuscript to an expert in English editing and academic writing.

Reviewer 1 | 09 Nov 2017 | 16:05

#1

No

MAIN MESSAGE

Q 3

What are the main findings reported in this manuscript?

Reviewer 1 | 09 Nov 2017 | 16:05

#1

two main findings are reported :

- Significatly more smiles and laughs while watching funny videos in presence of peers compared to when watching alone. No difference between pairs and groups.

- this is not correlated or predicted by the rate of funiness of the material

S2. INDIVIDUAL SECTIONS

TITLE

Q 4

Does the title clearly and precisely reflect the findings of the manuscript, as described in the author guidelines?

Reviewer 1 | 09 Nov 2017 | 16:05

#1

Not entirely but this is true throughout the manuscript: we do not know whether authors are talkign about humorous or social laughter. Both are confounded here becauise the material is humorous and I do not think that the authors can separate them here or draw a cocnlusion on these categories as they claim in the discussion. I will return to this point in the following sections.

**Reviewers 1 & 3 both make similar points here. Our goal was not to compare social and humorous laughter as competing categories but to explore the social dimension of laughter. The comments of both reviewers reflect that our title was too vague and presentation were not clear enough about this. Additionally, we felt we should make it more explicit that design of the study is derived directly from the Antony Chapman’s 1973 paper “Social facilitation of laughter in children”. We thank the reviewers for highlighting these weaknesses and have made a number of changes to the presentation to address them.**

1. **We change our title to “Social facilitation of laughter and smiles in preschool children”**
2. **At the end of the abstract instead of**

**“This suggests …** **that it is the sociability of the situation, rather than its humour, that elicits laughter.”**

**We now say**

**“It supports the idea that laughter and smiles are primarily flexible social signals rather than reflexive responses to humour. “**

1. **At the end of the first paragraph of the introduction we say**

**“The current study sought to do so by adapting the methods of Chapman (1973) to measure smiling and laughter in preschoolers watch humorous videos on their own or in the company of their peers.”**

ABSTRACT

INTRODUCTION

Q 6

Please comment on the Introduction section. Key elements to consider:

- appropriateness of context

- purpose of study

Reviewer 1 | 09 Nov 2017 | 16:05

#1

It is not clear in the first paragraph of the introduction how laughing is distinct from other vocalizations and "instantly recognisable" and starting at what age can we recognize laughs in infants. Are there any studies on this?

Readily in the first paragrapgh, the authors talk about social laughter and readers may wonder if laughter can be non social. This becomes clearer in the fourth paragraph but the definitions come a bit late.

The comparative section is rather poor. There are many interesting and recent studies on laughter in non human primates that must be citedhere; especially the studies of Davila-Ross and more particularily one recent study of her doing FACS in chimpanzees to evaluate smiles and laughs; In addition, there is an intersting paper of Davila-Ross on social laughter and the difference between non social laughter that is relevant here and that also need to be cited.

In the fourth paragraph, the aithors say that it is difficult to define humour. Yet, there is a consensus in the litterature to define humore as a incongruous act (this is how the authors refer to it also in the dsicussion) accompagnied with social cues such as smiles or laughter. I think that the authors should give a definition to at least how they define humor here in their paper.

Laughter as a hypothesis for social bonding is an interesting speculation. There is a recent paper of Dunbar (2017) that should be cited in that respect.

The objectives of this study are not very clear; The authors cite a number of studies showing that laughjter is social and is elicited by the presence of others in adults and in children. If the only novelty of this work is the experimental approach to the problem (this is how the authors present their work at the end of the introduction), then why is it that observational studies are not sufficient in providing us with answers?

**Taking these points in turn.**

1. **We now stated more clearly that infant laughter widely reported to emerge by 4 months (Sroufe & Waters, 1976)**
2. **We have reorganized the flow of the introduction. The paragraph giving definitions of social laughter and humor is moved from fourth to second. It gives a clearer definition of humor.**
3. **This is followed by the discussion of Darwin. The comparative work come next and is expanded to cover the recent work of Davila-Ross et al. (We also add a later section on developmental work on humour by Mireault and others.)**
4. **Dunbar is now cited. We thank the reviewer for this suggestion as Dunbar’s social hypothesis makes it clear that group size is an important variable to considered.**
5. **The final paragraph is rewritten to make it clear why group size was included as a variable and why there were no non-humourous conditions.**

MATERIAL AND METHODS

Q 7

Please comment on the Material and Methods section. Key elements to consider:

- objective errors

- correct choice of methods

- comprehensive description of methods

- accuracy of procedures

- quality of figures and tables

Reviewer 1 | 09 Nov 2017 | 16:05

#1

The method used has some important flaws that prohibit the work to be published in its current form :

- What do the authors mean by majority? we know that humour is culture dependant. If some of the children do not have the same cultural background, this will introduce bias in the study.

- Again in the material section, it is not clear whether the authors are testing humorous or social laughter. This should be better defined throughout the manuscript and also in the material section. In addition, not everyone knows the Bernard Bear series. The authors should give examples of the humour used in this series and whether it contains language; Again understanding of younger and older children will not be the same depending on this.

- As for the design, it is not clear why two of the groupes have 6 children and one has 8; In addition, it would be nice if the same number of boys and girls particicpated in each group. In fact the authors do not mention any potential differences in percieving humor as a function of sex. The dynamic might not be the same with 6 or 8 children and with different number of girls and boys.

- The authors do not mention the intervals at which a child was tested within the 6 testing weeks.

- In order to draw conclusions on social and humorous laughter, the authors should test this on non humorous material. Otherwise the two categories are confounded.

1. **In participants section We now state “All children were British born and included 19 who were white and 1 who was mixed race.”**
2. **We try to make it clearer that social group size is an IV of interest.**
3. **A little more description of the Bernard the Bear cartoon has been added.**
4. **The large groups had to be different sizes because there were 20 participants. 6,6,8 was the most convenient choice. Genders were balanced in groups (Group A N=6 (3m, 3f), Group B N= 8 (4m, 4f) Group C N= 6 (2m, 4f). The pair conditions included 3x m+f, 3x m+m and 4 x f+f pairings.**

**We did not set out to investigate any gender effects so our design doesn’t no easily allow for analyses of group x gender dynamics. An additional confound here would be that the children all knew each other so pre-existing friendship bonds ought to be taken into account. We mention gender as area for further research in the discussion.**

Q 8

Are the statistical methods used valid? Key elements to consider:

- appropriateness of statistical test applied

- statistics applied on independent experiments (biological replicates)

- sample size

- statistical significance

- error bars

Reviewer 1 | 09 Nov 2017 | 16:05

#1

Yes

Video scoring : what is minor and majpr discrepancies?

**We now state explicitly that “A minor discrepancy was a scoring difference between coders of 3 or fewer per child per video.”**

RESULTS

Q 12

Please comment on the Results section. Key elements to consider:

- objective errors

- correct presentation of results

- quality of figures and tables

Reviewer 1 | 09 Nov 2017 | 16:05

#1

I think that in order to publish interesting and meaningful results, the authors should analyse what they mention in the discussion as further studies : eye contact, whether the laughs and smiles occur at the same time as humorous acts in the video, whether one child initiates the laugh and other follow or whether they all laugh at the same time,...

We do not know for example if children always look at the screen while laughing and this is very important because it tells us whether the phenomenon observed is due to a social dimension of sharing something or to a simple contagion.

Duration of laughter might also be interesting to analyse. Maybe when entrained to laugh or to smile, children laugh and smile longer than when alone.

Even though smiles occur more frequently in pairs and groups, they are not inexistent individually. Could it simply be a question of intentisty or there is a quantitive difference between watching alone and with others? I think that this point is interesting to discuss.

**We agree that** **analyses of eye contact, laughter initiation, contagion, duration and intensity would all be very interesting and as we say in the discussion we saw circumstantial evidence that all of these factors play a role. When we found our results we carefully considered if there were any exploratory analyses we could perform on our data investigating these additional variables.**

**Regrettably because these were not part of our initial hypotheses we did not design the study to reliably capture these rich time series data. For example, smile and laughter duration are hard to code from a single microphone and single camera angle in a noisy environment. We chose to focus on the more crude but robust interval based scoring we present here.**

**We now mention these limitation and future directions to the discussion.**

DISCUSSION

Q 15

Please comment on the Discussion section. Key elements to consider:

- adequate discussion of research questions or hypothesis (posed in introduction)

- conclusions supported by data

- exhaustive discussion of previously published material (in context to current study)

Reviewer 1 | 09 Nov 2017 | 16:05

#1

I think that the authors should do what they suggest in the disucssion as further analysis in order to draw some conclusions about their findings.

REFERENCES

Q 16

Is prior work properly and fully cited?

Reviewer 1 | 09 Nov 2017 | 16:05

#1

No

I suggested some recent work on laughing in non human primates.

S3. FINAL COMMENTS

Q 17

Please provide your further comments and overall recommendation to the authors, including the level of revisions (minor, moderate, substantial).

Reviewer 1 | 09 Nov 2017 | 16:05

#1

Overall, the authors raise a very intesrting question and the experimental design is very nice. However, it is frustrating to see that they did not go further in controling potential biases in the method and and analysing their videos in order to draw meaningful cocnlusions.

Beyond the fact that children laugh and smile more when they are together, we cannot draw further conslusions and we cannot know whether it is due to the material or not. I recommand that the authors complete their study with a homogenenous group and another control group with no humorous material and do further analysis to better understand the results.

**Taking your second point first. It is not easy to add a non-humorous condition while keeping our well-powered within subjects design. Humorous vs non-humorous video would have to be included as an additional independent variable and would double the number of sessions required (adding 3 non-humorous videos sessions for single, pair and group viewing). It is certainly not possible to do this with the original participants who are now over a year older. Testing another group of 20 aged matched controls as between subjects factor might be one solution but is beyond our resources to conduct. A non-humorous condition or video-free baseline as suggested by reviewer 3 are improvements to consider in future work.**

**We feel that the present paper has value of its own. We feel that the key finding here is a strong effect that children laughing and smiling much more in company than on their own., conceptually replicating Chapman (1973) in a much younger age group. Additionally, the surprising finding that larger group size does not lead to greater still laughter and smiling is an interesting challenge to the idea that the increase in laughter in social conditions is due to contagion.**

REVEIWER 2

S1. GENERAL EVALUATION

ARTICLE TYPE

Q 1

Does this manuscript conform to the definition below of Original Research articles? If not, please contact the Frontiers Editorial Office (editorial.office@frontiersin.org).

Reviewer 2 | 23 Nov 2017 | 12:07

#1

Yes

LANGUAGE AND GRAMMAR

Q 2

Is the language, specifically the grammar, of sufficient quality? If no, please specify if the authors should send this manuscript to an expert in English editing and academic writing.

Reviewer 2 | 23 Nov 2017 | 12:07

#1

No

There are many typos throughout. I don't think an expert is needed- just a thorough read-through.

**Two of the authors have proofread the manuscript before and after the changes listed here. Hopefully, this has caught most typos.**

MAIN MESSAGE

Q 3

What are the main findings reported in this manuscript?

Reviewer 2 | 23 Nov 2017 | 12:07

#1

20 2-4 year old children watched funny videos alone, in pairs, or in groups (6-8 children). Children smiled and laughed significantly more in pairs and groups than alone, but there was no difference between pairs. Funniness ratings did not relate to smiling/laughter.

S2. INDIVIDUAL SECTIONS

TITLE

Q 4

Does the title clearly and precisely reflect the findings of the manuscript, as described in the author guidelines?

Reviewer 2 | 23 Nov 2017 | 12:07

#1

Yes.

ABSTRACT

Q 5

Please comment on the Abstract section. Key elements to consider:

- appropriateness of context

- purpose of study

Reviewer 2 | 23 Nov 2017 | 12:07

#1

Overall this is good. “The social conditions showed significantly higher the numbers of laughs and smiles than individual viewing.” There’s an extra “the” in this sentence.

**Fixed**

INTRODUCTION

Q 6

Please comment on the Introduction section. Key elements to consider:

- appropriateness of context

- purpose of study

Reviewer 2 | 23 Nov 2017 | 12:07

#1

Overall I think this covered most of the literature well, and justified the study.

Page 3, line 29: “that begins at very” should be “that begin at a very…”

-Another typo the next line. Please check for grammar throughout.

-It is worth citing Gina Mireault’s recent work on humour/smiling/laughter in infants.

-I think it’s good that you cover laughter in infancy. You also cover some research on humour in toddlers/pre-schoolers in your discussion. I think it would be good to touch on this briefly in the intro too just to demonstrate that young children (ie those covering your study’s age range) do have an understanding of humour, justifying your study design.

**A paragraph discussing of work on laughter and smiles in relation to infant temperament and covering the development of humour has been added to the introduction**

MATERIAL AND METHODS

Q 7

Please comment on the Material and Methods section. Key elements to consider:

- objective errors

- correct choice of methods

- comprehensive description of methods

- accuracy of procedures

- quality of figures and tables

Reviewer 2 | 23 Nov 2017 | 12:07

#1

Overall I think this was well designed. I list specific points below.

Method

Participants- do you have an idea of the SES background of the children?

And how did you determine participant numbers (power analysis)?

Design: “A second independent variable viewing order children were divided into three streams A, B and C, tested in different orders over several sessions (see Table 1)” This isn’t grammatical and so is confusing.

-You look at number of laughs/smiles, but what about time? Because what if someone is, e.g., smiling throughout without a break?

-How did you assign children to pairs/groups? E.g, random assignment? Did they choose?

Coding:

-Can you clarify whether laughter would also be coded as smiling, or whether these are independent?

-I think time spent smiling/laughing might be a better indicator since you could smile/laugh a long time, but that would count only as once. However, it might not be a better indicator. It might be worth coding time for a small portion of data to see if it correlates highly with number of smiles/laughs.

-Agreement needs to be reported as cohen’s kappa (or maybe just intraclass correlation since the main point is the number of laughs/smiles)

1. **SES was not recorded. Although as stated most children were from middle class background.**
2. **Sample sizes were based the 20 per cell of Chapman (1973) but no explicit power calculations were performed prior to the experiement. These have been done and support the original choice with our sample giving use power of 0.9 to detect a similar effect with alpha = 0.05. This calculation has been added to the results section and is worked through explicitly in the analysis code provided in supplementary materials.**
3. **The description of the design and accompanying Table 1 have been revised.**
4. **We divide the 6 minutes of each video into 10 second blocks and count smiles / laughs in each block. Thus a single smile that lasted across more than one block could be counted more than once. In practice smiles are more transitory.**

**We did initially consider coding for lengths of smiles and laughs. But, as we said to reviewer 1, “****we did not design the study optimally to reliably these rich time series data. For example, smile and laughter duration are hard to code from a single microphone and single camera angle in a noisy environment. We chose to focus on the more crude but robust interval based scoring we present here.”**

1. **All assignments were random. This is now explicitly stated in the design.**
2. **Smiles and laughter were not double counted. If a smile was accompanied by a laugh it would only count as a laugh.**
3. **Correlations between coders are now listed.**

Q 8

Are the statistical methods used valid? Key elements to consider:

- appropriateness of statistical test applied

- statistics applied on independent experiments (biological replicates)

- sample size

- statistical significance

- error bars

Reviewer 2 | 23 Nov 2017 | 12:07

#1

Yes

The statistics are appropriate. However sample size calculations should be made (though perhaps reported in the participant section).

Figure 1: I’m not familiar with Tukey plots. Can you just clarify – why are the dots sometimes left/right of the middle line? Is that just to make it easier to see all the dots, or is there some other reason for it? Overall, I like the idea of showing all the individual data points, as well as the mean/CI.

I find it fascinating that smiles/laughs did not link to humour ratings. However, visually inspecting the graphs suggests it might if a larger sample size was used. Do you think there might be a large effect of socialness, and a small effect of funniness or response? Also, would this all change if you had an unfunny control video? Would that show a large effect size in comparison to a funny video?

1. **We include power calculations at the start of the results section. Since these were not done a priori, this seemed a better place for them than the participants section**.
2. **Yes, the jitter is just to make points easier to discriminate. We now explain this figure better in the caption.**
3. **The point about potential independent effects of video funniness (small) and social group size (large?) on laughter is very well made. We agree that this study cannot answer that point.**

**The conclusion of our experiment is that socialness has a large effect. Discovering how this interacts with humour is an important next step.**

**Therefore adding non-humorous comparison conditions would an important improvement for future work. Humour vs non-humorous would be an additional independent variable requiring 6 video sessions in all. This point and the limitations of the current design are now covered more clearly in the discussion .**

RESULTS

Q 12

Please comment on the Results section. Key elements to consider:

- objective errors

- correct presentation of results

- quality of figures and tables

Reviewer 2 | 23 Nov 2017 | 12:07

#1

See Q8.

DISCUSSION

Q 15

Please comment on the Discussion section. Key elements to consider:

- adequate discussion of research questions or hypothesis (posed in introduction)

- conclusions supported by data

- exhaustive discussion of previously published material (in context to current study)

Reviewer 2 | 23 Nov 2017 | 12:07

#1

The discussion is good overall. A few points below.

p.13 Mehu & Dunbar (2008)- It is interesting that this is at odds with your findings. I wonder if the set-up might make a difference, or if it is developmental. Watching a video might be more passive, such that all humour should derive from that stimulus alone. However in larger non-video based group interactions, perhaps there is more opportunity for social interactions or humour creation, leading to more smiling/laughter the more people there are

p.14 first paragraph – Good point about scale validity. I’ll re-raise my points about power, as well as the fact that all videos were funny (so differences in scale might not be representing very strong differences in funniness)

**Thank you for these comments. We have incorporated your remarks about the passive nature of our video viewing task into the discussion.**

REFERENCES

Q 16

Is prior work properly and fully cited?

Reviewer 2 | 23 Nov 2017 | 12:07

#1

Yes

Would be worth adding work by Gina Mireault.

**Done**

S3. FINAL COMMENTS

Q 17

Please provide your further comments and overall recommendation to the authors, including the level of revisions (minor, moderate, substantial).

Reviewer 2 | 23 Nov 2017 | 12:07

#1

I think this will be a great manuscript with moderate revisions, as listed above.

REVIEWER 3

S1. GENERAL EVALUATION

ARTICLE TYPE

Q 1

MAIN MESSAGE

Q 3

What are the main findings reported in this manuscript?

Reviewer 4 | 03 Jan 2018 | 19:10

#1

The authors present findings related to smiling and laughter in school children. Children express more smiling and laughter in response to humorous video clips when in the presence of one or more friends/other children. Neither the humour rating of the clip nor the number of peers present impacted smiling and laughter.

S2. INDIVIDUAL SECTIONS

TITLE

Q 4

Does the title clearly and precisely reflect the findings of the manuscript, as described in the author guidelines?

Reviewer 4 | 03 Jan 2018 | 19:10

#1

Yes, though it is a little vague.

**Agreed! We have changed our title to “Social facilitation of laughter and smiles in preschool children”**

**This also makes it more explicit that design of the study is derived from the Antony Chapman’s 1973 paper “Social facilitation of laughter in children”.**

ABSTRACT

Q 5

Please comment on the Abstract section. Key elements to consider:

- appropriateness of context

- purpose of study

Reviewer 4 | 03 Jan 2018 | 19:10

#1

The abstract accurately reflects the purpose, hypotheses, and findings from the study. See note in the "results" section about 8x and 3x more likely to laugh and smile in social groups.

INTRODUCTION

Q 6

Please comment on the Introduction section. Key elements to consider:

- appropriateness of context

- purpose of study

Reviewer 4 | 03 Jan 2018 | 19:10

#1

The Introduction is well written. I liked the historical and anthropological context within which the authors situated their study. I have several comments, however, about smiling and laughter as a construct.

In Mary Rothbart's temperament parlance, she uses "smiling and laughter" as one temperament scale. Paul Ekman's emotion theory includes only one "happy" domain. These researchers are both cited in the Introduction, yet the authors do not provide theoretical or methodological justification for splitting smiling and laughter into two separate constructs. There is also discussion of humourous and social laughter, but again, no justification for smiling vs. laughter.

**Thank you for this comment. We agreed that smiling and laughter are closely related constructs and in many circumstances they can be treated as a single value. One area where they are treated differently is in study of language and communication (see** **Haakana, 2010). Since we are interested in the social role of smiles and laughter we have done the same. As you rightly point out we did not justify this clearly. We have now done so.**

MATERIAL AND METHODS

Q 7

Please comment on the Material and Methods section. Key elements to consider:

- objective errors

- correct choice of methods

- comprehensive description of methods

- accuracy of procedures

- quality of figures and tables

Reviewer 4 | 03 Jan 2018 | 19:10

#1

The materials and methods are presented quite well. I do have several comments that relate to issues with the conceptual definitions and meaning of smiling and laughter.

1. I would like to see a control for children's "typical" level of positive affect (see comments in Introduction). For example, are affect ratings available outside the video clips? Did parents or teachers complete a child temperament questionnaire? Some children may be "driving" the observed increases in smiling and laughter in social situations, and temperamental positivity may colour funniness ratings of the videos.

2 Also, the authors admit that often smiling and laughter are related, but there is little discussion of the uniqueness each code. Perhaps a factor analysis or something would help readers understand the necessity of two codes rather than an overall score, if there is indeed a need to keep smiling and laughter separate.

3. With the small sample size,a thorough examination of how age related to smiling and laughter may be difficult. However, the range is quite large (2-4 years). Many developmental shifts, particularly in the executive function and cognitive control domains, occur during this time. This may greatly impact expressions of positivity and/or children's likelihood to regulate in social situations. Did age correlate with smiling, laughter, or funniness rating sof the videos?

**Taking your points in turn**

1. **Having some temperament measures would have been interesting. This was not included in the study. We now mention this suggestion in the discussion.**
2. **Keeping laughter and smiling analysis separate seemed the conservative approach. We did originally also analyse a combined score of “mirth” but left this out the results for succinctness.**

**Likewise it would have been interesting to have some comparison between laughing and smiling. However, they are not independent values we couldn’t see an appropriate statistical way to analyse their interrelations. Future work should certainly address this. We suspect that this will require a richer dataset and more sophisticated time series analyses. We mention this in the discussion.**

1. **The age range of ‘2-4 years’ is better expressed in months as ages were between 31 and 49 months. We have change the paper to reflect this. Mean age was 38.2, SD 4.2 months with 17 out of 20 children in range 36-44. We decided to include all 20 children as they were all tested together. Age did not correlate with laughter or smiles. We now include a table of correlations in the results section (Table 3).**

Q 8

Are the statistical methods used valid? Key elements to consider:

- appropriateness of statistical test applied

- statistics applied on independent experiments (biological replicates)

- sample size

- statistical significance

- error bars

Reviewer 4 | 03 Jan 2018 | 19:10

#1

Yes

The group design was well planned, as well as controlling for humour ratings of the video clips. However, with such small numbers in each group, I would like to see a discussion of power and sample size.

**Sample sizes were based the 20 per cell of Chapman (1973) but no explicit power calculations were performed prior to the experiement. These have been done and support the original choice with our sample giving use power of 0.9 to detect a similar effect with alpha = 0.05. This calculation has been added to the results section and is worked through explicitly in the analysis code provided in supplementary materials.**

Q 12

Please comment on the Results section. Key elements to consider:

- objective errors

- correct presentation of results

- quality of figures and tables

Reviewer 4 | 03 Jan 2018 | 19:10

#1

1. Please include table with basic descriptives and correlations for all study variables

2. For smiling and laughter, please provide estimates and p-values for each sphericity test.

3. The abstract says "8x" and '3x" more likely to laugh and smile in groups, but this isn't clear from the results section. The means appear to be larger in the smiling and laughter groups, but the abstract infers results from an odds ratio, which is not the statistical test actually conducted. Please clarify.

**1. Descriptive statistics and correlations, including for age, are now included in Tables 2 &3.**

**2. P-values and Mauchly’s W statistic are now both reported for all sphericity tests.**

**3. These numbers were a colloquial comparison of size of the means. We have rephrased the abstract and discussion to reflect this more clearly.**

DISCUSSION

Q 15

Please comment on the Discussion section. Key elements to consider:

- adequate discussion of research questions or hypothesis (posed in introduction)

- conclusions supported by data

- exhaustive discussion of previously published material (in context to current study)

Reviewer 4 | 03 Jan 2018 | 19:10

#1

On p. 14, line 357-358, this appears to be an incomplete clause.

On p. 15, line 390, I do not like the phrase "considered sufficient to clearly demonstrate." The authors may consider their operational definition sufficient, but I do not. Further, and in line with my previous comments, previous research often combines smiling and laughter (see Mary Rothbart's temperament questionnaires, Hill Goldsmith's Laboratory Temperament Assessment Battery). Again, the authors do not justify their use of separate constructs, particularly when most coding problems arose from confusing the two (p. 15, line 385). the authors state that future research could look at smiling and laughter as one construct and future research may do so...so why did they not provide correlations, factor analyses etc justifying their use of separate codes?

The authors only briefly differentiate between felt happiness and overt expressions of laughter/smiling. I think this is an important differentiation to discuss. Indeed, the fact that the funniness of the clip did not alter smiling and laughter, only group size did, is, I think, worth considering. If the authors are not measuring happiness or affect, why does the level of smiling and laughter a child expresses (if it is not happiness/positive affect) matter? The authors do not find that happiness is social or learned, only smiling...but how do we know the smiles are reflective of happiness, humour or positive emotions? From this paper, we don't. Is positive affect in children learned or social as well? Or only smiling? Perhaps tying the two ideas together may bolster the Discussion section.

***I think that the distinction between smiling/laughter and happiness/positive affect needs to be more clear, in the title, abstract, and discussion.***

**Thank you for your comments.**

1. **Our starting hypothesis (in line with Dunbar) is that laughter might be a social group signal. As such coding laughter and smiling separately seemed the most conservative starting point. The phrase “considered sufficient to clearly demonstrate” was meant to claim that both laughter and smiling are affected by the social situation (rather than to demonstrate that laughter and smiling are proved to be different constructions. We agree with the reviewer that we haven’t proved that and have rephrased that comment to avoid misunderstanding.**
2. **The discussion of the relationship between smiles and laughter and felt happiness was necessarily limited because our study did not include any (independent) measures of happiness or positive affect. We agree with the reviewer that this is an interesting point and we now consider it in greater depth.**

REFERENCES

Q 16

Is prior work properly and fully cited?

Reviewer 4 | 03 Jan 2018 | 19:10

#1

Yes

There are several typos in references, I imagine from a citation manager. Please double check formatting for references in text and in the reference section.

S3. FINAL COMMENTS

Q 17

Please provide your further comments and overall recommendation to the authors, including the level of revisions (minor, moderate, substantial).

Reviewer 4 | 03 Jan 2018 | 19:10

#1

I do believe this paper is interesting and could prove to be important in the increasing literature on well-being and positivity. With some additional discussion of power and sample size and the necessity of smiling vs. laughter, the method is sound. However, I think that there are some "big picture" pieces missing that prevent the paper from being ready to publish. Notably, not enough grounding in previous child "smiling and laughter" literature (see Rothbart and Goldsmith), lack of discussion on how smiling and laughter play into happiness/positive affect and why it is (or is not!) important to study simply overt expressions of smiling/laughter.

**Hopefully the comments immediately above address these concerns.**