Dear Editor,

We are submitting a revised version of our manuscript entitled "Stressful life events and psychosocial correlates of pediatric inflammatory bowel disease activity". We thank you and the reviewers for the valuable comments and suggestions that we tried to address them all. The changes that we made following reviewers' suggestions are highlighted in yellow color in the text. Below you can find our reply to each comment made by the reviewers.

Thank you once again.

Kind regards,

George Giannakopoulos

REPLY TO REVIEWERS

Reviewed by 02445015

It is an interesting research, presents the influence of emotional states in children, congratulations.

We thank the reviewer for his/her feedback.

Reviewer No. 02530553

INTRODUCTION

Overall it is well written, with the exception of the aim and the hypothesis paragraph which I found somehow difficult to understand.

We thank the reviewer for this remark. Indeed, the aim and the hypothesis paragraph was somehow difficult to understand in the way it was previously written. We have improved the language of this paragraph in the revised version (see changes highlighted in yellow) and we hope that it is now easy to follow.

METHOD

Authors should describe which statistical procedure has been used in order to test hypothesis.

We thank the reviewer for this helpful comment. In the revised version we have clearly stated the statistical procedures that were used in order to test our hypothesis (see respective changes highlighted in yellow).

RESULTS

In this part of the paper many no significant results are considered: they must be deleted, suppressed. For example, those sentences must be corrected: - "Parents of children being in active state of the disease reported more life events (p = 0.005) and stressful life events (p = 0.048) during the past year, more mental health symptoms (p < 0.001) and marginally higher levels of family dysfunction (p = 0.061)" Parents of children being in active state of the disease reported more life events (p = 0.005) and marginally stressful life events (p = 0.048) during the past year, more mental health symptoms (p < 0.001) ". Authors cannot consider family dysfunction: the result is not significant. "Similarly, when the disease activity was

assessed through activity index, it was positively associated with life events (p = 0.04) and stressful life events (p = 0.07)". "Similarly, when the disease activity was assessed through activity index, it was positively associated with life events (p = 0.04)". Authors cannot consider stressful life events: the result is not significant.

We thank the reviewer for this important comment. Following his/her suggestion, we deleted from this part of the paper no significant results, and we now report only significant results at a level of ≤ 0.05 (see changes highlighted in yellow).

Moreover, it is rather difficult to understand, at the reading, paper AND tables without explanations of the statistical procedure description according to hypothesis (for example stressful life events appears significant in the table and not significant in the paper or where did come from the result p=.07? is this the result of correlation matrix? Logistic regression?). Authors should write the results part with subtitles according to their hypothesis.

We thank the reviewer for this helpful comment. Following his/her suggestion, we have written the results with subtitles according to our hypothesis in this revised version (see changes highlighted in yellow). Also, under Table 2

DISCUSSION

In this part, results interpretation appears quite excessive. For example: "This finding may reflect the fact that parents may underestimate the potential negative impact of some life events on their children wellbeing and functioning and consequently not report them as stressful." I don't understand the interpretation of this result: there was no assessment of life events by children. The authors should consider their results as they are even if they are not exactly as they have expected them.

We thank the reviewer for this important comment. In the revised version we wrote the interpretation of this result in a more balanced and understandable way and we commented more on this aspect (see changes highlighted in yellow).

<u>**Reviewer 02843425**</u>

The paper is interesting and well written. However, I have some comments on your study.

Participants

Please give also the absolute number of subjects on steroids.

We thank the reviewer for his/her suggestion. We give the absolute number of subjects on steroids in the revised version (see changes highlighted in yellow).

Results

I agree with your consideration on low power when stratify CD vs UC. You could avoid the description and said that this evaluation has not sufficient statistical power.

We thank the reviewer for his/her suggestion. We avoided the description of the results after stratifying according to diagnosis (CD vs. UC) in this revised version (see changes highlighted in yellow).

Steroid medication is associated with disease then patients on relapse should be more likely to use steroids......or viceversa as you wrote?

We thank the reviewer for his/her suggestion. We previously used the term "effect" according to statistical terminology. However, we acknowledge that the term is confusing in this context. Following reviewer's suggestion, we changed the term "effect" with the word "association" in this revised version (see change highlighted in yellow).

I agree with you when you said that avoid to test some issue on patients should be a limitation: the parents could ignore some aspects of the life of the patients, underestimate or overestimate other known stressors. At the same time young patients couldn't estimate correctly the power of different events when you use a questionnaire to detect them Please comment this aspect.

We thank the reviewer for this important comment. We added his input in the interpretation of the respective finding and commented on this aspect (see changes highlighted in yellow).