Author’s response to reviews

Title: Effectiveness of a multimodal training programme to improve General Practitioners’ burnout, job satisfaction and psychological well-being

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Author’s response to reviews:

We have attached a document ("cover letter") where all our answers to reviewers’ requirements are more clearly presented than below.

Dear editors,

Before presenting our letter with a point-by-point response to the reviewers’ comments, we would like to indicate:

-Thank your overall considerations and comments. We would like to state that journal’s word limitation (for “Research article”) prevented us from providing some of the later requested
information. So, in order to address these issues, we would ask for permission to surpass this word limit.

-All changes (reformulations, additional words included so on) introduced into the manuscript are highlighted in green.

-The order of authors has not been altered.

1. Please Editor Comments:

1. Include the power calculation for the Sample size.

This is a very good observation that could make this paper more statistically rigorous.

A new chapter called Sample size (in Methods, page 15) has been introduced.

The power for the statistical analysis performed on principal outcome measures are provided in Results, page 17.

Finally, in Study Strengths and Limitations (Discussion, page 22) we have mentioned the effect of sample size on statistical significance and power.

2. Please have the text edited by a professional language editing service or a native English speaking colleague. There are many issues with grammar, wording, spelling, and/or punctuation that need to be addressed.

Before sending our paper to BMC Family Practice, the original manuscript was reviewed by a professional native speaker. We contacted UAB Idiomes Barcelona, from Universitat Autònoma de Barcelona (UAB).

After giving them notice of your complaints, we have resubmitted this manuscript to UAB Idiomes Barcelona.

BMC Family Practice operates a policy of open peer review, which means that you will be able to see the names of the reviewers who provided the reports via the online peer review system. We encourage you to also view the reports there, via the action links on the left-hand side of the page, to see the names of the reviewers.

Thank you for the information about the reviewers.
Reviewer reports:

Marie-Josée Fleury (Reviewer 1): This article is interesting and pertinent, but major revisions should be made.

Abstract (overall, the Abstract needs to be substantially rewritten for better clarity):

-The first paragraph of the abstract, in particular, is not clear, and needs to be rewritten.

We recognize this lack of clarity. We have modified this paragraph.

-The objectives of the study need to be more clearly stated,

Yes, thank you for your appreciation. The objectives clearly needed to be described more concisely. Hopefully, now they will be more precisely stated.

and should be removed from the method section.

I’m sorry, but I’m not sure about what do you really mean. The objectives are already located in Background.

-The results are also not clearly presented,

We agree; this paragraph had to be modified for a better understanding. We wish to clarify this issue.

and recommendations should be better specified among the conclusions.

Thank you for this observation; they had to be better linked and more accurately described.

Introduction:

-The objectives of this article are not clear.

Yes, thank you for your appreciation. They needed to be more clearly described. Hopefully, now they will be more precisely stated.

As well, a number of measures were studied, but they are not introduced appropriately in the introduction (e.g., access to care).

Yes, you are right; at first glance it could create some confusion. Now we have better introduced these measures in Introduction (page 9, the final paragraph).
Later, we have better presented the Outcome measures (Methods, page 14) and the debate about them in Comparison with Existing Literature (Discussion, page 20).

-More contextual information should be provided on the importance of improving the GP role in mental health.

We agree, in order to justify better the study’s purposes, we had to improve this contextual information. In this line, new data regarding GP–patient encounters focused on mental health is provided (internationally and in our clinical context). We have highlighted why they represent a large proportion of GPs’ clientele and workload that could increase in the coming future. So, the GP role in mental health will be crucial.

-The nature of the links among "burnout, job satisfaction and psychological well-being" of GP, and other measures relating to the effectiveness of their role in mental health should be more clearly described.

Yes, thank you for your appreciation. These work-related elements had to be better connected in order to facilitate the readers comprehension. Nonetheless, we would comment:

(a) Given the journal’s word limitation, we couldn’t fully expose the links among "burnout, job satisfaction and psychological well-being" in this paper; we thought that this broad topic would be beyond the scope of this paper and is actually better explained in other papers.

So, we initially decided to focus on other issues, and to bring about bibliography where these factors appear related. In any case, given reviewer’s suggestions, we have added new scientific literature that justifies better the link between these factors.

If this is not satisfactory for the editors and it is still regarded as a key issue, we will fully expose these relations in this paper.

(b) Regarding the links among measures, given the aforementioned associations between burnout, job satisfaction and psychological well-being, we chose those gold-standard assessment tools according to (primarily Spanish) related research studies to which we wanted to compare our results. This fact has been better explained in Outcome Measures, page 14).

According to the bibliography, the only unusual outcome measures are:

- The psychiatric interview (BPRS), what we actually consider as a novelty and an improvement.

- The administrative and health care indicators (for example the Total annual visits for all pathologies, so on): They were provided as indirect elements that could contribute GPs’ work-related health and well-being. They offer a broader perspective.

Although it is not ordinary, this kind of measures can be found in related studies; for instance:
In addition, all the variables studied needed to be linked within a coherent framework.

Yes we agree. Beside what has been exposed in the previous request, we would like to commend that:

- We have included a new model (the “Job demands control model”) and a better explanation (page 6-7) that links these variables in a coherent framework.

- We exposed the Collaborative Care Models (CCMs)’s framework in which these variables can be combined. We have better mentioned this issue in Introduction.

- Finally, as we commented earlier, we have added new scientific literature that justifies better the links among "burnout, job satisfaction and psychological well-being" (page 6, second paragraph).

A figure showing all the outcome variables studied could perhaps be added, and appropriately presented in the methods or introduction section.

We think this is a very good idea. So, all the instruments have been summarized in a new table (table 1).

- I believe that this study was conducted in a collaborative care context, but this is not quite clear. The underlying model should be adequately introduced, with a description of the intervention studied.

We agree with this proposal. The links between the CCMs and the MTP were not clearly described.

In Methods (page 10), we have reformulated both the Participants and Recruitment Strategy and Intervention Design. We have explained better how this program was conducted into the Collaborative care framework because it pretends strengthen cooperation between different health care providers in order to provide evidence-based and comprehensive treatment. We have provided more details about the mental health team’s tasks in primary care.

- A better contextualization of the GP role in mental health, and major problems identified in the literature should be provided, including relevant statistics (e.g. % of GP burnout, levels of job satisfaction, with a better description of these issues…)
Yes, definitively this information should be included for a better contextualization. In this line, Levels of burnout, job satisfaction and psychological well-being are introduced and discussed in page 6, along with a better description of these issues.

-A better literature review needs to be provided as well,

In connection with our previous comment, we recognize that besides a paragraph reformulation, a renewed bibliography should be provided. We expect to have satisfied your requirement.

, including a review of other interventions that may improve GP work in mental health,

Yes, this is a good point. It could provide even more contextual information. Nonetheless, given journal’s word limitation, our study’s objectives and the higher amount of interventions aimed at improving GPs’ psychological well-being (5,392 according to Murray and colleagues), we focused “only” on the psychological interventions.

In this line, beside applying a paragraph reformulation, new bibliography related to mindfulness and cognitive-behavioral interventions have been included.

If this is not satisfactory for the editors and it is still regarded as a key issue, we will fully expose other non-psychological interventions in this paper.

…and how the proposed intervention differs from previous experience. Much of the information provided in the introduction is, in fact, rather vague. The originality of the study should also be more clearly pinpointed

Yes, thank you for your appreciation. Nonetheless, after all the changes introduced in both Introduction and Methods regarding the MTP description, we expect to be more concise.

Moreover, we would like to remark all the original elementals that are commented in the text:

- CCM methodology was used in GPs, instead of patients (page 8).

- The (new) psychological approach: the Integrated Brief Systemic Therapy (IBST) training was tested for the first time in GPs, and within a CCM methodology (page 9).

Later, In Intervention Design (page 11) we explained how it was conducted.

-The use of unusual but interesting Measurements: we conducted a psychiatric interview; we gathered administrative quality of healthcare management indicators and GPs’ current psychiatric medication use.

-The last paragraph of the introduction is particularly unclear (one sentence has no verb), and should be rewritten. The objectives of the study should be very clearly stated.

We agree. This paragraph has been modified.
Method:

-More information on the study context should be provided for international readers. What ambulatory mental health services were studied,

Yes, thank you for your appreciation. Certainly, it needed to be more better described. We would like to clarify that there was only one ambulatory mental health service, our CASM Benito Menni (C/ Dr Pujadas, 36 Sant Boi de Llobregat, Barcelona). It provided assistance to these four Primary Care units. In order to avoid confusions, this paragraph has been reformulated for a better understanding (page 10).

We didn’t initially provide the names of both our ambulatory mental health service and the Primary Care units to make it anonymous and facilitate the double-blind peer review

Besides, in order to provide more contextual information about our mental health system, In Introduction we have explained that:

-Our health system is inside the primary care oriented health systems.

-Primary Health Care is the access gate for the majority of our mental health patients (GPs in particular) and how were they related with the overall health care system (or what was the importance of the organizations studied in relation to the healthcare system)?

We agree. This issue could also lead to misunderstandings. So, we have commented that such a territorial representation provides patients from urban and semi-urban areas. Sant Boi represents a significative metropolitan city for our Catalan health system.

If needed, we can introduce more contextual data.

-Statistics were provided on the inhabitants covered by the primary care units studied, but how representative was this population compared with other populations treated by GPs at the national or state levels?

Thank you for this observation; it could provide more contextual information for the readers. However, I’m sorry, but after checking the related psychological intervention’s surveys, I don’t think we can make any strong and reliable claim in relation to the comparation with other national or international populations; there’s a lot of variables that can interfere (year of assessment, socio-economic characteristics, type of health system, so on).

In any case, what we really know it that our representative sample was comparable to that offered by related intervention studies. Our sample is not, nor does it pretend to be, representative for all the country. Instead, it contains the majority of GPs from of a large population of Barcelona’s metropolitan area. This sector has very similar sociodemographic characteristics when compared to many other cities in the country.
This section should be more fully integrated, i.e. variables studied needed to be linked within a coherent, integrated analytical framework.

As commented earlier, the linkage between variables has been improved in Introduction (page 6).

If necessary, we could also disclose this topic in Methods, but we thought it would be more appropriate in Introduction.

This section could also be shortened, and the specifics of the instruments used presented in a table (and introduced very briefly in the text).

As commented earlier, all the instruments have been summarized in a new table (table 1).

- The intervention design should be presented at the beginning of the methods section before introducing the outcome variables (and instruments).

Thank you for this format recommendation. We have changed the text position.

This section needs also to be more detailed and more clearly written.

We agree. Maybe this section needed to be modified for a better readers' comprehension. Now, we have reformulated this section and new intervention’s details are presented.

In necessary, we could introduce more information

- I would remove from the text the point that the study did not receive any funding, as this is already mentioned under "Funding" following the article.

Yes, this is correct. We have removed this reiterative information.

Results/discussion:

- Please introduce the % response rate clearly in your article.

We recognize this information could be better reported. Now we have introduced both the general and each professional’s attendance rates.

- Percentage of sessions attended by clinical psychologists, psychiatrists and social workers are reported. The attendance rate for social workers (65%) was particularly low, especially since only one session was offered. It would be important to comment on these statistics in the discussion, as well as the potential impact of these results.

Yes, definitively a full discussion of this issue could improve the result’s analysis. Nonetheless, taking into account the amount of topics and results that could be debated in this paper and
maybe they might even possess higher impact on our study’s aims, we chose to focus on other issues.

If the editor still regard it as a key element, we could fully discuss attendance rates in discussion.

- The discussion points should also be reinforced with better linkage to the literature.

We agree with this proposal. The links between the results and the literature were weak. This paragraph has been modified and new references are provided.

- There is no conclusion. The article needs to conclude with a statement regarding the originality of the study, and presentation of the key results. Concluding with a description of what was presented as "Future Research and Development" is, in my view, inappropriate.

Yes, thank you for your appreciation. Now, we hope to have summarized better the key results of this new intervention and their clinical implications.

Aukse Endriulaitiene (Reviewer 2): The manuscript has the intention to investigate the effectiveness of an intensive multimodal training intervention aimed at improving GP's work-related health and psychological well-being. The idea might be valuable and interesting to the readers of the journal - researchers, as well as practitioners. The paper has many advantages - interdisciplinary approach, quasi-experimental design, clear description of contribution to the literature, relevant topic and use of different data source (independent reviewer, self-reports and objective administrative data). Provides some hints on how to develop and improve general practitioners professional training. Involvement of different specialists.

Thank you for your valuable comments

Still there are some concerns that should be revised before I can recommend this manuscript for publishing. Major concerns:

1) Clearer connections to the journal's scope should be described.

Yes, we agree. This is a fundamental issue. Nonetheless, we are confused about this appreciation. It is stated in BMC Family Practice’s webpage: “BMC Family Practice is an open access, peer-reviewed journal that considers articles on all aspects of primary health care, including clinical clinical management of patients, professional training, shared decision making....”. In this context, we have already declared our objectives in Introduction that actually fit editors’ requirements.
If needed, we could provide more linkages.

2) The authors do not define construct clearly and does not follow clear theory that would connect all measured constructs.

We agree. This issue has been addressed earlier with the previous reviewer; please check out Introduction’s changes (page 6).

The impression is that they took many variables and just tried to look if something related to GP functioning is improved after intervention.

We agree, but hopefully we have addressed this wrong impression. This issue has been also commented earlier with the previous reviewer; changes in Introduction’s and Outcome measures (Methods) have been introduced.

Clear conceptual and theoretical framework would increase the clarity and rationale. For example, the concept of psychological well-being (sometimes called well-being) is not defined, as well as job satisfaction or burnout. There are a lot of models how these constructs are defined in the literature of occupational health psychology, and it remains unclear how the authors use them in current paper. For example, the absence of psychiatric symptoms or burnout are not synonymous to well-being.

We recognize that a clear construct definition is necessary for a good evaluation. In this line, in Introduction, we have indicated this limitation (page 6).

Later, in Method, we already explained as we administered validated versions of these self-reported and hetero-applied measures. Related bibliography was also provided if the reader wanted to take a close look at the instruments’ validity (or how well the instrument measures what it is supposed to measure).

If this is not satisfactory for the editors and it is still regarded as a key issue, we will fully expose this topic in this paper.

The opinion about mental illness or patient management strategies seem to be out of the scope and not related to the title and purpose of the paper.

We recognize this issue had to better explained to avoid confusions. Now we have better introduced these issue in Introduction (page 9, the final paragraph). Later, in Discussion (page 21), we have introduced new details and bibliography that hopefully will clarify and justify better this opinions’ assessment.
3) The authors provide three - four different aims across the manuscript - abstract (first para, front page), abstract (page 5 line 13), page 8 line 46; page 9 line 16. This is very confusing for the reader.

Yes, we totally agree. This issue has been addressed earlier with the previous reviewer; please check out Introduction’s changes (page 9).

4) Training program (MMT) should be explained with more specific details. Like, on which theory it was based on and how; which constructs and how it was intended to change.

Thank you for this observation. In order to amend this limitation, we have introduced new specific details regarding the interventions (especially clinical psychology) and its coordination.

Regarding the theory, we would like to commend:

- Bibliography related to the psychological intervention (Integrated brief systemic therapy (IBST) was initially provided; here the reader can find information about its theoretical bases, guiding principles and change therapeutic processes.

- As it was the case of the links between Burnout, job satisfaction and psychological well-being, here again we cannot find any strong theory that connects the Clinical Psychology, Psychiatry and Social worker interventions. However, it has been justified in:

  (a) Introduction (page 8): we propose the Collaborative Care Models (CCMs) as the fundamental framework in which the psychology, psychiatry and social worker disciplines can be combined and consequently also the evaluation of their respective interventions.

  (b) In Methods, we have added that the intervention was coordinated by the clinical psychologist who was guided by his psychotherapy model (IBST), besides de CMMs.

  (c) In Introduction and Methods, we commented as the MTP was an ad hoc clinically rooted training program which emerged essentially from GPs’ demands and concerns related to their encounters on mental health; they represent a large proportion of GPs’ clientele and workload.

Was there a control for attendance in statistical analyses, I’m sorry, but I’m not sure about what do you really mean. Attendance rates by professional were not taking into account in any specific statistical analysis was there a control of factor that the program was delivered by different specialists.
Thank you for this observation. In our intervention there was only one psychiatrist, psychologist and social worker. We have introduced this fact in the text.

Please provide the explanation why authors think that this program is intensive and multimodal or based on brief systemic therapy.

We agree. Maybe these concepts should be better contextualized:

- Intensive: Because in many other related studies consider 9 weeks as Intensive. Even though there’s not any clear-cut point, at least from a psychological perspective, it makes sense. If the editors still consider it necessary, we could highlight this issue in the text.

- Multimodal: given our previous comments about CMMs and the new details introduced in Intervention Design, we think it’s appropriate to use this concept. If it is considered that somehow it could lead to misunderstandings, we could remove this label.

- based on brief systemic therapy: as it has been indicated in Intervention Design, all the interventions were coordinated by the clinical psychologist guided by his psychotherapy model and the CCMs.

It seems that different teachers used different methodologies - some focused on knowledge enhancement others on attitudes and skills; was this controlled in the analyses?

Thank you for this observation. In connection with our previous comment, yes the clinical psychologist coordinated all the interventions’ structure.

5) Reference list and literature review part should be extended with more recent publications (only one is 2017 in a current version, others are older); and with more direct focus on the constructs that are central of this study (like well-being, job satisfaction, etc.).

We totally agree; the literature had to be updated. That’s what we’ve done. In any case, we would like to point out that this paper was submitted at the end of last year.

Regarding the construct’s issue, we have commented this question earlier.

6) The conclusion - implication in page 22 line 41 (…our study suggests that MTP (from a BST approach) could be adopted as part of continuing professional training program...) is not supported with the results. Having in mind that the effects were minor, such statement is speculative and too strong.

Yes, definitively this paragraph has been reformulated with more humble statements.
Minor concerns:

* Overuse of abbreviations should be avoided.

We have reviewed the paper, reducing the use of abbreviations as much as we could.

* Figure 1 and 2 lack clarity.

Thank you for this observation. In this context, we have introduced improvements in the format of figure legends.