I appreciated the opportunity to review the manuscript “Psychological distress in French college students: Demographic, economic and social stressors. Results from the 2010 National Health Barometer” for BMC Public Health. The authors examined symptoms of psychological distress (PD) among 946 French college students, using data from a national survey. The authors found that 13.8% of students reported notable symptoms of psychological distress; economic stress, non-sexual assault in the past year and low social participation were associated with a higher likelihood of reporting notable PD, while studying legal science was associated with a lessened likelihood of reporting PD. These results are discussed within the context of programs to improve psychological functioning among college students.

The findings of this study are interesting and the access to recent data from a representative sample is particularly valuable. The contribution that the manuscript can make in this area is clear, but the manner in which it is currently written significantly limits its potential impact. The most notable concern is the manner in which the study’s findings are discussed. It is important for the authors to present their findings in a more contextualized, accurate manner by focusing on the actual ways in which their variables were assessed (e.g., contact with friends and family, participation in leisure time activities), and not the more general manner in which they have currently portrayed them (e.g., general social support). This caveat should be used when presenting the implications of such results, as well, as the authors appear to overextend the potential impact of their findings, given the broad manner in which the findings and implications are discussed.

Further, the authors should work to build a stronger case for this study and the variables included. They note that little work has been done assessing student distress, which is not true in a broader sense (e.g., within other countries), but may be true with respect to France, specifically. They should more clearly discuss the state of research in this area, as well as why the variables they included in their analyses are important, given that they included a great number of them. As written, the Introduction is too vague with respect to previous research in this area and does not serve to craft a strong rationale for the current study. Tying their findings into the existing literature in the Discussion is also important, as many discrepant findings (e.g., lack of PD among sexual assault
victims) are left unaddressed.

Yet, the findings presented are of interest and the manuscript should be revised for these issues, as well as the more specific points noted below. Provided the findings can be presented in a more contextualized and accurate fashion, the authors have the opportunity to provide interesting information in this area. Of note, a native English speaker should help with the revision process, as a number of grammar and syntax conventions make the manuscript difficult to read and understand clearly. Additionally, contractions should be removed throughout the manuscript and the inclusion of citations (i.e., numbers provided before or after punctuation) should be done consistently. Unless otherwise noted, the points made below are considered Major Compulsory Revisions.

Introduction:
1) As noted above, this section needs to be revised to provide more information about the research available on psychological distress among students in different populations. A more thorough discussion of reasons for possible differences in these findings, particularly given the different outcome measures used across studies, is warranted.

2) The authors should more clearly identify reasons for including the variables they selected in their analyses. The manner in which previous findings are discussed is too general (e.g., grouping life events, psychological symptoms, financial stress and year in college into a single summative sentence). Given the number of variables under study, it is important to ensure that they are all important to assess, particularly given the possibility of committing a Type I error. (Of note, a statistical correction for this possibility appears to have been used, which is a strength).

Method:
3) The authors should note how informed consent was obtained. (Minor Essential Revision)

4) The authors should provide some context for many students there are in France, and thus, what percentage of this population they obtained with their 946 participants. (Discretionary Revision)

5) The authors note that individuals in “collective dwellings” were not included in analyses. They should specifically note whether this excluded college dormitories or other common student residential options, as such an exclusion would notably impact the generalizability of their obtained sample. (Minor Essential Revision)

6) The procedure by which participants in a given dwelling were selected should be explained more clearly, and not just refer to the procedure used by another researcher. (Minor Essential Revision)

7) The authors should justify why they are using a cut-off of general self-reported psychological symptoms. This issue is important given concerns about dichotomous diagnostic criteria, particularly when looking at diffuse symptoms. It is unknown if this level of symptomatology reflects functional impairment (required for psychological diagnoses), and thus, represents the broader
construct of severe distress the authors represent that they assessed. Of note, the impact that this choice may have had on their results (e.g., lower prevalence of PD than expected) should be included in the Discussion.

8) The manner in which variables are labeled (e.g., “isolation” is whether “respondents lived alone in their own home”) is misleading at times. The authors must make clear throughout this section, as well as the rest of the manuscript, what exactly was, and was not, assessed. There are a number of limitations of the assessed variables (e.g., sexual violence was assessed using two broad, non-behaviorally-specific questions, social participation is framed as social support broadly, while opportunity of social contacts with friends and family is ignored).

9) The authors should be clear whether the life experiences variables assessed (e.g., non-sexual violence, serious illness of a parent) were assessed using close-ended questions. The wording of such questions may also be helpful, given that sexual violence was assessed in such a broad manner. (Minor Essential Revision)

10) It is unclear why life experience variables were assessed before the age of 18, but not after. (Discretionary Revision)

11) The changing timeframe for the life event variables should be made clearer, and discussed with respect to their limitations (i.e., lifetime sexual violence, non-sexual violence in the past year, life events prior to age 18). (Minor Essential Revision)

12) The manner of weighting the sample should be explained more clearly, and whether or not a stratified sample was used should be mentioned. (Discretionary Revision)

Results:

13) The inclusion of information about debt should be qualified with respect to whether or not students generally take out loans to pay for school in France. Given that this experience is relatively commonplace for many students in the US, the discussion of students in debt in this population needs further contextualization. Of note, though, the use of perceived personal economic strain is an interesting and valuable variable in this regard. (Minor Essential Revision)

14) The authors note a result with respect to “contact with friends or family in the last 8 days…” when, in fact, it was assessed within the past week. (Minor Essential Revision)

Discussion:

15) As described above, this section needs substantial revision to more accurately portray the obtained results. The authors are too broad in the manner in which they discuss their findings. While the results are interesting, they must speak to the actual constructs that were assessed (e.g., engagement in leisure time activities) and not the broader variables that may or may not be tapped by these operational definitions (e.g., social support more broadly).

16) The non-significant results and results that are inconsistent with the previous
literature (e.g., sexual violence, engagement with family and friends) should be addressed. The relations between these unexpected findings should be discussed in the context of the broader literature in this area, as well as the study’s limitations (e.g., limited assessment of these variables).

17) The discussion of major choice as a factor in PD is confusing as written (e.g., “…only first-year students from public universities in which the conditions of education, social and professional opportunities are more uncertain than in engineering or business schools that are included in our sample.”) This variable and its implications should be addressed more clearly.

18) The authors have some interesting ideas with respect to preventing PD among students by mobilizing social support, and the inclusion of possible interacting variables (e.g., female gender, economic strain), is useful. However, this discussion is too long and far-reaching for the results that were actually obtained, particularly given that only one of the social support-related variables was significant in the study analyses. This discussion should be condensed (it is brought up multiple times throughout the Discussion) and discussed within the context of what was actually found.

19) The comparison of this sample to unemployed individuals is surprising and does not seem warranted, given the notable differences between these groups.

Discretionary Revision

Level of interest: An article of importance in its field

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

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests.