Reviewer's report

Title: Prevalence and correlates of positive mental health in Chinese adolescents

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Reviewer: Sharon Goldfeld

Reviewer's report:

It is encouraging to see papers from China focusing on the potential positive aspects of the lives of adolescents. Overall this paper provides important results from a large but non representative sample of Chinese adolescents. There are a number of revisions outlined below that will be important to address

There are a number of minor grammatical errors and word misuse (e.g. "diagnosed" as flourishing in the abstract or sometimes adolescents instead of adolescence and vice versa). An overview review of grammar and language is an important revision.

The authors have appropriately based their work on Keyes who has published widely in this area. However the summary of papers commencing on page 4 needs some attention with a more synthetic approach to describing the overall thrust of the papers together with some detail about the direction of variables ie is it lower or higher SES (and which SES factors?) and positive mental health-since many of the studies have many of these variables in common I suggest they are summarised and the references grouped

In Table 1 the ages of the population should be included so the current study may be put in context

The authors note that this is the first study of its kind for Chinese adolescents at least 4 times in the paper-it might be better to note in the discussion and note where there are gaps in the literature in the introduction

In the research aims I wonder if the authors had any hypotheses that might be specific to Chinese adolescents-it might be helpful to further know whether other developmental research on Chinese children had different results from other countries and whether there is thinking in the literature regarding cultural differences that were important to consider for this study.

In the methods it would be helpful to know a little more about Weifang. Later we learn it is a thriving area but something about its geography within China, population and general economics
would be helpful in understanding the population the sample was drawn from right up front in the methods

The authors should note how issues of potential responder bias were minimised in the distribution of the surveys-having surveys with names on them means there may be a lack of true reporting

Under "variables" it is unclear what "family form" (line 24) means

Under analysis-it would be important to include whether Keyes also combined the categories and if so why and if not why the authors felt this was appropriate

The analysis could be strengthened with some basic changes to the approach. There is some confusion with "sub-groups" and predictor variables (eg physical activity is not really a subpopulation of children). Table 2 would be better as a demographic table describing the population rather than the prevalence of positive mental health across each variables-prevalence could be added into a basic demographic table - given there is no adjustment I'm not sure chi-squared statistics add very much

Rather than a crude and then final model the authors could include a univariate and multivariate logistic regression model in the same table-including only those that are significant in the final multivariate model. This may simplify the results and make the approach more sensible.

The results text can be cut down given the availability of tables-only the highlights should be included in the text

It would be helpful to summarise the results in the beginning of the discussion rather than repeat what has been stated previously.

The authors have not really positioned this study in the context of other studies such that there is an overall understanding of what differentiates Chinese adolescents from other populations and therefore may be important for intervention (a point only really brought up in the conclusion)

The higher SES of the adolescents in this study probably explains a lot of the results therefore it would be helpful to see if other positive mental health studies (ie Table 1) were able to differentiate higher SES adolescents/adults and determine if the results were then similar. This is
a very significant limitation in terms of being representative of Chinese adolescents and should get more attention in limitations

In the conclusion the suggested areas of policy focus are dealt with fairly superficially—a couple of good examples of what might be possible would strengthen the paper

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Unable to assess

Are the conclusions drawn adequately supported by the data shown?
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Yes

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