PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Mental health in the UK during the COVID-19 pandemic: Cross-sectional analyses from a community cohort study
AUTHORS	Jia, Ru; Ayling, Kieran; Chalder, Trudie; Massey, Adam; Broadbent, E; Coupland, Carol; Vedhara, Kavita

VERSION 1 – REVIEW

REVIEWER	Graham Meadows 1. Monash University, 2. The University of Melbourne, 3. Monash Health, Australia
REVIEW RETURNED	10-Jun-2020

GENERAL COMMENTS	Thank you for the opportunity to comment on this presentation of a piece of work which I suggest can add in a timely manner to understanding of effects of the current pandemic on mental health. I make a series of suggestions below as to how it could benefit from better acknowledgement of limitations inherent in the very biased sample, some broadening of the argument included, and some clarification at key points as set out below: P4 L18.'sequelae' is intended here I think. L28 '…mental health…' – probably emerging since submission the accumulating evidence is favouring likely long-term physical health problems and this could now be mentioned here. P5+ I can find no discussion of power as guidance for the sample size and attention to the STROBE checklist would be valuable. P6 LL9-22. Description of measures at this point I would see as inadequate. Where pre-existing scales were used these should be named here, not just referenced, and where scales were created for this work they should be here clearly described. L42. It will be easier to make sense of the binary variable classification if, as above, we have been informed to this point as to the nature of the scale. L49. I believe 'Robustness of models was…' would be more correct – for editorial attention. P7 L3. 'established cut-offs'; could benefit from a citation. Table 1. This could benefit from another column giving overall UK statistics so the reader better understands the sampling bias. P9 L29

	I would suggest the measures are not helpfully labelled 'outcomes' in this setting which is not of an intervention study, rather they are status measures. Table 2 In multiple cases here, the SD is greater than the mean suggesting distribution is non-normal. If a t-test (parametric) on untransformed data is to be used (and I note use of transforms later in the work) then a reference supporting robustness of t-testing (which I agree is probably supportable) given the degree of divergence from a Gaussian distribution would be desirable. P16 L7. There is a seeming anomaly here in that the given 95% CI spans 1 but the result is cited as significant at p<0.0001 PP17-19 The study limitations are acknowledged only late in the discussion and the authors have not in the estimation of this reviewer adequately qualified their conclusions in the light of these limitations. So I would suggest relocate the limitations section to the start of the discussion and then temper the conclusions accordingly with use of more tentative language. The limitations section should be stronger on the issue of response bias which is evidently a substantial risk-of-bias in the work. The findings might be very unrepresentative of the UK population and self-selection for distress and anxiety is a real possible source of bias. For instance, outside England the other Countries in the UK are under-represented – one of a number of sampling biases that would become clearer if my earlier suggestion regarding table 1 is taken up. I would suggest also that the discussion is unduly mental-health-services focused. That element is important but I suggest the work would benefit from a pause considering the bigger picture before narrowing scope. This may necessitate declaring some points that may seem obvious but represent assumptions about what is effectively modifiable and how. The issues here that are in scope for governments and communities go well behind mental health provision or effects of mental health on physical health and include the public health and whole-of-government approach to the impact of the pandemic. Since England has been more severely affected than other parts of the UK this might be one instance in which more people responded to the survey, and gave more anxious responses, where there was very genuinely more to be worried about. This could lead to consideration of a third column in Table 1 giving some selected information on COVID-19 case-rates at the start of the survey. Possibly for another paper it also could invite a different analysis taking classification of areas by COVID-19 case-rates, or mortality rates, as an independent variable. Most simply, and perhaps for this paper, it could be interesting to take the binary variable of England vs Rest of the UK to add into the regression models. This brings me to a connected point that key determinants of poorer mental health in the findings are perceived risk of COVID-19 and COVID-19 worry. These are listed as modifiable but the implications of this I suggest could be better followed through. So arguably the most important single thing that can be done to alleviate the effects of poor mental health may be to get the national disease outbreak - as part of the pandemic - under control such that worry and estimation of risk can realistically subside, and along with that, mental health can improve. The efforts of mental health services to ameliorate the consequences of COVID-19 may be much less significant than what may be achievable by improved

	and effective public health measures. This may be an important point to admit into the discussion of these issues as easing of restrictions is progressively considered and implemented. If such actions lead to further waves of infections then realistic anxiety, perhaps along with progressive demoralisation, may represent increasing threats to population mental health which may counteract mental health benefits for instance of lessened isolation or anything that mental health services can realistically achieve.

REVIEWER	Mila Kingsbury University of Ottawa, Canada
REVIEW RETURNED	17-Jun-2020

GENERAL COMMENTS	This study is a survey of mental health in the UK during the COVID-19 pandemic, and offers comparisons with population norms, as well as testing associations with certain modifiable and non-modifiable risk factors. Comparisons to population norms are interesting; however, with the data available, it is impossible to tell whether these differences are due to COVID-19 specifically. These norms are from a decade ago, and changes could reflect general population trends in mental health (as have been reported in other countries), reactions to other troubling recent local events (BREXIT), or self-selection into the study due to concerns about mental health. Most informative, obviously, would be comparisons to baseline mental health for these same individuals, though I recognize the impracticality of going backwards in time to collect such data. Without this baseline data, claims of causality, e.g., “the COVID-19 pandemic is having widespread and deleterious effects” are too strong. The examination of explanatory factors is also troubling. The only explanatory factors specific to the COVID-19 pandemic are “worry about” and “perceived risk” of contracting COVID-19. Other than these, is there any evidence to suggest we might expect the other explanatory factors (gender, BAME status, loneliness) to operate differently during the pandemic than otherwise? If not, I’m not sure these findings add much what is already known about risk factors for mood and anxiety symptoms outside of the pandemic context. Again, more informative might be an assessment of whether perceived loneliness or positive mood had changed since the beginning of lockdown. Other, more specific risk factors, such as loss of employment or childcare due to the pandemic, may have provided more insight into pandemic-specific risks. The framing of positive mood as a modifiable risk factor is additionally problematic. As measured, it may be confounded with depression, characterized by low positive affect. I was also confused as to why objective risk of contracting COVID, as presented in Table 1, was not included in the multivariable models. I was also unsure as to the distinction between perceived risk of contracting COVID and worry about contracting COVID, and concerned about possible co-linearity between these variables.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Graham Meadows
Institution and Country: 1. Monash University, 2. The University of Melbourne, 3. Monash Health, Australia Please state any competing interests or state ‘None declared’: None declared

Thank you for the opportunity to comment on this presentation of a piece of work which I suggest can add in a timely manner to understanding of effects of the current pandemic on mental health. I make a series of suggestions below as to how it could benefit from better acknowledgement of limitations inherent in the very biased sample, some broadening of the argument included, and some clarification at key points as set out below:

1. P4, L18.’sequelae’ is intended here I think.
Thank you. This has been corrected.

2. L28 ‘…mental health…’ – probably emerging since submission the accumulating evidence is favouring likely long-term physical health problems and this could now be mentioned here.
This has been added to line 13 of the Introduction.
3. P5+: I can find no discussion of power as guidance for the sample size and attention to the STROBE checklist would be valuable.
Apologies for this oversight. We have now included a section on sample size and a Strobe checklist.

4. P6: LL9-22. Description of measures at this point I would see as inadequate. Where pre-existing scales were used these should be named here, not just referenced, and where scales were created for this work they should be here clearly described.
Apologies. We have now provided details on all scales used in the Procedures section with measures created for this research presented in supplementary appendix 3.

5. L42. It will be easier to make sense of the binary variable classification if, as above, we have been informed to this point as to the nature of the scale.
We hope the description of the scales presented in the Procedures and supplementary appendix address this issue.

6. L49. I believe ‘Robustness of models was…’ would be more correct – for editorial attention.
Apologies. This has now been corrected.
7. P7: L3. ‘established cut-offs’; could benefit from a citation.
The relevant references have been added to the text as requested.
8. Table 1: This could benefit from another column giving overall UK statistics so the reader better understands the sampling bias.
As per our response to comment 3 from the Editor, we have added this information to Table 1.
9. P9 L29: I would suggest the measures are not helpfully labelled ‘outcomes’ in this setting which is not of an intervention study, rather they are status measures.
This has been modified as requested.

10. Table 2: In multiple cases here, the SD is greater than the mean suggesting distribution is non-normal. If a t-test (parametric) on untransformed data is to be used (and I note use of transforms later in the work) then a reference supporting robustness of t-testing (which I agree is probably supportable) given the degree of divergence from a Gaussian distribution would be desirable.
We thank the reviewer for this observation. We have amended our section on statistical analyses to explain why non-parametric or transformations could not be applied, and added a reference to support the robustness of t-tests. Although we still urge caution in the interpretation of findings.

11. P16: L7. There is a seeming anomaly here in that the given 95% CI spans 1 but the result is cited as significant at p<0.0001
In the case of continuous outcomes, if the 95% CI for a regression coefficient straddles 0 non-significance at P<0.05 is indicated. Nonetheless, the data reported in this and other tables has changed following a request by Reviewer 2 to add COVID-19 risk into our analytical models.

12. PP17-19: The study limitations are acknowledged only late in the discussion and the authors have not in the estimation of this reviewer adequately qualified their conclusions in the light of these limitations. So I would suggest relocate the limitations section to the start of the discussion and then temper the conclusions accordingly with use of more tentative language. The limitations section should be stronger on the issue of response bias which is evidently a substantial risk-of-bias in the work. The findings might be very unrepresentative of the UK population and self-selection for distress and anxiety is a real possible source of bias. For instance, outside England the other Countries in the UK are under-represented – one of a number of sampling biases that would become clearer if my earlier suggestion regarding table 1 is taken up.
As requested, we have moved the section on limitations to the start of the discussion, expanded on sources of sampling bias and have tempered the language throughout. Although we would note that our comparisons with UK census and ONS data in Table 1 indicate that on many parameters our cohort was largely representative of the UK population.

13. I would suggest also that the discussion is unduly mental-health-services focused. That element is important but I suggest the work would benefit from a pause considering the bigger picture before narrowing scope. This may necessitate declaring some points that may seem obvious but represent assumptions about what is effectively modifiable and how. The issues here that are in scope for governments and communities go well behind mental health provision or effects of mental health on physical health and include the public health and whole-of-government approach to the impact of the pandemic. Since England has been more severely affected than other parts of the UK this might be one instance in which more people responded to the survey, and gave more anxious responses, where there was very genuinely more to be worried about.
We would like to thank the reviewer for this comment and comment 16 below. Their steer is in fact wholly in keeping with our view that the findings may indicate an increased demand for mental health services, but that equally some of the correlates of psychological morbidity (such as worry about contracting the disease) could also be addressed by robust public health interventions such as contact tracing. We have added a consideration of these issues into our Discussion.

14. This could lead to consideration of a third column in Table 1 giving some selected information on COVID-19 case-rates at the start of the survey.
Thank you for this suggestion but we have elected not to include these data for several reasons. First, our data on COVID-19 cases is based on self-report only. Second, at this early stage of the pandemic COVID-19 testing was in fact very limited in the UK population. This is echoed in our sample where only 2.6% reported having had a COVID-19 test (n=81) and of these only 9 reported a positive test result (0.03% of the total cohort). This, we consider is likely to be an underestimate of the prevalence of the infection and reflects poor access to community testing at the time.

15. Possibly for another paper it also could invite a different analysis taking classification of areas by COVID-19 case-rates, or mortality rates, as an independent variable. Most simply, and perhaps for this paper, it could be interesting to take the binary variable of England vs Rest of the UK to add into the regression models.
We would like to thank the Reviewer for this suggestion. We agree this may be a useful analysis for future papers, in particular once we have follow-up data. At the time the data in this manuscript were collected, it was in the early stages of the pandemic and the differences between the four nations of

the UK were not as clear as they are now. This fact, and the low number of participants outside of England led us to conclude this analysis would not add value to the current manuscript.
16. This brings me to a connected point that key determinants of poorer mental health in the findings are perceived risk of COVID-19 and COVID-19 worry. These are listed as modifiable but the implications of this I suggest could be better followed through. So arguably the most important single thing that can be done to alleviate the effects of poor mental health may be to get the national disease outbreak - as part of the pandemic - under control such that worry and estimation of risk can realistically subside, and along with that, mental health can improve. The efforts of mental health services to ameliorate the consequences of COVID-19 may be much less significant than what may be achievable by improved and effective public health measures. This may be an important point to admit into the discussion of these issues as easing of restrictions is progressively considered and implemented. If such actions lead to further waves of infections then realistic anxiety, perhaps along with progressive demoralisation, may represent increasing threats to population mental health which may counteract mental health benefits for instance of lessened isolation or anything that mental health services can realistically achieve.
We agree completely with the Reviewer's view on this issue and have addressed this in the Discussion (please also see response to point 13 above).

Reviewer: 2
Reviewer Name: Mila Kingsbury
Institution and Country: University of Ottawa, Canada
Please state any competing interests or state 'None declared': None declared

This study is a survey of mental health in the UK during the COVID-19 pandemic, and offers comparisons with population norms, as well as testing associations with certain modifiable and non-modifiable risk factors.

1. Comparisons to population norms are interesting; however, with the data available, it is impossible to tell whether these differences are due to COVID-19 specifically. These norms are from a decade ago, and changes could reflect general population trends in mental health (as have been reported in other countries), reactions to other troubling recent local events (BREXIT), or self-selection into the study due to concerns about mental health.
On the advice of this reviewer we have explored again whether more recent normative data are available for these scales but were unable to find anything. We have, therefore, included a consideration of this issue as a limitation of the work in our Discussion. We have also included a consideration of how self-selection bias may have affected recruitment to the cohort (see also response to point 2, Reviewer 1).

2. Most informative, obviously, would be comparisons to baseline mental health for these same individuals, though I recognize the impracticality of going backwards in time to collect such data. Without this baseline data, claims of causality, e.g., "the COVID-19 pandemic is having widespread and deleterious effects" are too strong.
We agree with this observation and that it is an inherent limitation of all cohorts that were established in response to the pandemic. We have, as requested, tempered our language throughout the discussion in recognition of this issue and our cross-sectional design.

3. The examination of explanatory factors is also troubling. The only explanatory factors specific to the COVID-19 pandemic are "worry about" and "perceived risk" of contracting COVID-19. Other than these, is there any evidence to suggest we might expect the other explanatory factors (gender, BAME status, loneliness) to operate differently during the pandemic than otherwise? If not, I'm not sure these findings add much what is already known about risk factors for mood and anxiety symptoms outside

of the pandemic context. Again, more informative might be an assessment of whether perceived loneliness or positive mood had changed since the beginning of lockdown. Other, more specific risk factors, such as loss of employment or childcare due to the pandemic, may have provided more insight into pandemic-specific risks.
As noted in our manuscript, our selection of explanatory factors was predicated on identifying non-modifiable and modifiable characteristics associated with our mental health outcomes. Our rationale behind the former was to identify specific demographic characteristics which might increase the risk of mental health difficulties because they are associated with an increased risk of COVID-19 (e.g., age, gender, BAME status etc.) and so might operate differently during the pandemic. The value in these analyses is in providing an early signal as to who may be most in need of intervention. In this regard, we consider our data do offer some new insight. For example, despite the fact that worse outcomes due to COVID-19 are more common in men and older people, we observe that women and younger people were more likely to report mental health difficulties. Conversely, the measurement of modifiable factors was to provide an early indication of potential targets for future intervention. Here again we report, we believe for the first time, that the constellation of perceived loneliness, worry about contracting COVID-19 and low positive mood, appear to be strongly associated with mental health difficulties. We have edited our introduction to make the rationale behind the selection of explanatory factors clearer and in our revised discussion we examine how the identification of these factors highlights the potential for public health as well as mental health interventions to mitigate these mental health issues. We agree that follow-ups of this and other cohorts and prospective analyses will provide more insight in due course. We hope the above and the manuscript revisions clarify our selection of explanatory factors.

4. The framing of positive mood as a modifiable risk factor is additionally problematic. As measured, it may be confounded with depression, characterized by low positive affect.
We respectfully disagree with the Reviewer on this point. As we state in our introduction, positive mood is now widely regarded to confer direct effects on well-being, including in mental health disorders. Furthermore, the instrument we used to measure positive affect conceptualises positive and negative affect as being distinct. We cite further references in the Introduction to support the independence of positive affect.

5. I was also confused as to why objective risk of contracting COVID, as presented in Table 1, was not included in the multivariable models.
We would like to thank the Reviewer for this suggestion. We have now incorporated this into all our models. As you will see, although statistically significant, the addition of this variable does not change the overall findings of our analyses.

6. I was also unsure as to the distinction between perceived risk of contracting COVID and worry about contracting COVID, and concerned about possible co-linearity between these variables.
A priori we examined all regression models for multicollinearity during model fitting and did not find this to be an issue for these measures. We have added a sentence to the Methods to say that we checked the VIF (variance inflation factors) factors for all models and there was no problematic multicollinearity. The correlation between perceived risk and worry about contracting COVID was found to be only r=0.32

VERSION 2 – REVIEW

REVIEWER	Graham Meadows Monash University, The University of Melbourne, and Monash Health, Australia
REVIEW RETURNED	17-Aug-2020

GENERAL COMMENTS	In my view the authors have adequately addressed my comments. In the revision the term cohort has acquired increased prominence and I would suggest the authors could usefully clarify in what terms (see Last's, now Porta's dictionary of epidemiology, 6 ed) this is a cohort. It can be seen as a historical cohort, relying on self report, but is there intent - in another sense of meaning of a cohort design - to follow up this group with further observations? If so then some information on the research plan would be useful.

REVIEWER	Mila Kingsbury University of Ottawa, Canada
REVIEW RETURNED	18-Aug-2020

GENERAL COMMENTS	I thank the authors for their attention to my comments, particularly early attention to the study limitations in the discussion, softening of causal language, and inclusion of objective COVID-19 risk in the regression models. I also appreciate the new section detailing how public health measures may mitigate risk of mental health concerns. With respect to 'non-modifiable' risk factors, I appreciate your discussion of these as linked to COVID-19 risk and therefore pandemic-specific mental health concerns; however, given the cross-sectional nature of the study, I think it important to mention in the discussion that some of these (female sex; BAME background) constitute risk factors for mental health concerns more generally. Regarding 'modifiable' predictors: I agree with the authors that positive and negative affect are distinct constructs; however, I respectfully maintain that low positive affect (e.g., anhedonia) is often a feature or symptom of clinical depression. Indeed, the PHQ-9 includes an item assessing anhedonia. Especially given that you measure depressive symptoms and positive affect at the same time point, framing positive mood as a modifiable protective factor remains problematic in my view. Given that many (though admittedly not all) patients with depression experience anhedonia as a symptom, I would expect these two constructs to be correlated; showing an association between these two constructs is certainly not enough to suggest causality with direction of effect from positive mood to depressive symptoms Though the authors mention that the cross-sectional nature of the data precludes claims of causality, the discussion should explicitly address the possibility of reverse causation. As another example, worry about COVID-19 may indeed drive mental health difficulties; however, it is equally plausible that those predisposed to anxiety may be more likely to worry.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Graham Meadows
Institution and Country: Monash University, The University of Melbourne, and Monash Health, Australia

Please state any competing interests or state 'None declared': None declared.

In my view the authors have adequately addressed my comments. In the revision the term cohort has acquired increased prominence and I would suggest the authors could usefully clarify in what terms (see Last's, now Porta's dictionary of epidemiology, 6 ed) this is a cohort. It can be seen as a historical cohort, relying on self report, but is there intent - in another sense of meaning of a cohort design - to follow up this group with further observations? If so then some information on the research plan would be useful.
We apologise for not making this clear in the original manuscript. The intention is to follow-up the individuals in this study. We have made edits to the abstract, introduction, methods and discussion to make clear that the data reported reflect only the first wave of data collection and that the cohort was established to prospectively examine the mental health impact of the pandemic. We hope this clarifies this issue.

Reviewer: 2
Reviewer Name: Mila Kingsbury
Institution and Country: University of Ottawa, Canada
Please state any competing interests or state 'None declared': None declared

I thank the authors for their attention to my comments, particularly early attention to the study limitations in the discussion, softening of causal language, and inclusion of objective COVID-19 risk in the regression models. I also appreciate the new section detailing how public health measures may mitigate risk of mental health concerns.

With respect to 'non-modifiable' risk factors, I appreciate your discussion of these as linked to COVID-19 risk and therefore pandemic-specific mental health concerns; however, given the cross-sectional nature of the study, I think it important to mention in the discussion that some of these (female sex; BAME background) constitute risk factors for mental health concerns more generally.
As requested, we have added this point to our discussion and include a reference supporting this observation.

Regarding 'modifiable' predictors: I agree with the authors that positive and negative affect are distinct constructs; however, I respectfully maintain that low positive affect (e.g., anhedonia) is often a feature or symptom of clinical depression. Indeed, the PHQ-9 includes an item assessing anhedonia.

Especially given that you measure depressive symptoms and positive affect at the same time point, framing positive mood as a modifiable protective factor remains problematic in my view. Given that many (though admittedly not all) patients with depression experience anhedonia as a symptom, I would expect these two constructs to be correlated; showing an association between these two constructs is certainly not enough to suggest causality with direction of effect from positive mood to depressive symptoms

Though the authors mention that the cross-sectional nature of the data precludes claims of causality, the discussion should explicitly address the possibility of reverse causation. As another example, worry about COVID-19 may indeed drive mental health difficulties; however, it is equally plausible that those predisposed to anxiety may be more likely to worry.
As requested we have expanded the discussion around the cross-sectional design and explicitly mention the problem of reverse causality and explicitly mention the example of positive mood as noted by the reviewer.