

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

### ARTICLE DETAILS

|                            |  |
|----------------------------|--|
| <b>TITLE (PROVISIONAL)</b> | Demographic and Social Correlates and Indicators for Behavioural Compliance with Personal Protection Among Chinese Community-Dwellers During COVID-19: A Cross-sectional Study |
| <b>AUTHORS</b>             | Xu, Xin; Chew, Kimberly; Xu, Xiaolin; Wu, Zhihua; Xiao, Xiaohua; Yang, Qian  |

### VERSION 1 – REVIEW

|                        |                                    |
|------------------------|------------------------------------|
| <b>REVIEWER</b>        | Jeffrey Lees<br>Harvard University |
| <b>REVIEW RETURNED</b> | 03-Jul-2020                        |

|                         |  |
|-------------------------|--|
| <b>GENERAL COMMENTS</b> | <p>I enjoyed reading this timely manuscript which examines compliance with personal protective behaviors meant to prevent the spread of COVID-19 amongst a national sample of Chinese community-dwellers. Below I list several thoughts, concerns, and suggestions for improving upon the manuscript as written. They are listed in order from what I consider most pressing to least.</p> <p>1. Perhaps my largest concern with the manuscript is the lack of clarity surrounding many of the measures and analyses. I applaud the manuscript for its conciseness, but the dedication to conciseness often left me without critical information needed to access the analyses and findings. Two things would help clarify much of what I ask for below. First, the authors provide a document titled “Study_Protocol” (which I translated into English using Google Docs, so note the possibility for mistranslation). Yet this document seems not to be the study protocol (i.e. the survey) itself, but rather the grant application used to fund myriad sets of studies, this one included. It provides very little information about this specific study and the data collected for this manuscript. I would strongly suggest that the authors include a copy of the survey participants took so that both reviewers and future consumers of this manuscript can see the precise measures. Second, I would strongly suggest that the authors include with their submission the data (i.e. a CVS file) and analyses (SPSS files). Including the survey, data, and analyses with the manuscript would better adhere to BMJ Open’s commitment to transparency, and allow readers to better understand the survey. With all that said, below I list the study/analysis details that ought to be clarified in the manuscript.</p> <p>a. How the analyses are conducted is quite unclear. The authors report analyses of “overall behavioral compliance,” but it is unclear what this variable is precisely. Is it simply a binary variable denoting whether participants complied with all four behaviors? If so, then why is it analyzed using linear regression whereas all the other analyses use logistic regression? Additionally, since said</p> |
|-------------------------|--|

|  |  |
|--|--|
|  | <p>logistic regressions show divergent effects based on specific behaviors (i.e., the opposite gender effects), this itself calls into questions the validity of examining compliance with all four behaviors together.</p> <p>b. Another area needing of clarity is when/whether the findings presented are from multiple regressions. Table 2 is a prime area where this is both unclear and affects the interpretation of results. Are all the odd-ratios reported from single predictor logistic models, or are there many non-significant predictors in the models but not being show?</p> <p>c. How variables are being measured also needs clarity. Age and regional risk exposure, for example, are written about as blocks (e.g., the age group 21-49), but are they analyzed this way in the logistic regressions, or are they analyzed as linear variables (which in my opinion they should be)?</p> <p>2. My second area of concern relates to the authors' interpretations of their results. I agree with their primary interpretation that their gender/age effects are likely explained by "middle-age" men who are leaving the home to work. However, there are times when the authors stray from this interpretation. For example, in several places (e.g. the top and bottom of page 8) the manuscript invokes evidence that men engage in more risky behaviors. The evidence in this manuscript to me is opposed to the "risky behavior" interpretation. While men do violate the stay-at-home order more, they also are more likely to engage in other preventive behaviors, which suggests that while they're going out to work more, they're aware that their behavior is risky and taking steps to mitigate that risk. I would suggest that the authors be more consistent throughout the discussion in centering social norms/male breadwinner interpretation.</p> <p>3. I would suggest that the introduction be expanded upon to provide more information about the context. For example, the specific restrictions put in by the Chinese government are not clear, and the citation provided does not allow one to actually read something that provides such details. Moreover, what specific punishments one might receive for violating guidelines isn't mentioned at all. I would suggest to the authors that they write the introduction such that someone reading it 10 years in the future (hopefully far removed from the current pandemic circumstances) can readily understand the context participants in the survey found themselves in during February 2020 in China.</p> <p>4. Lastly, below are several minor thoughts/notes.</p> <p>a. The sampling section on page 4 says this was conducted over two surveys. Why two? Were they identical?</p> <p>b. At the bottom of page 7 it mentions "males were found less likely to be compliant with the social-distancing order (72% vs. 74%)...", yet the data on Figure 1 don't match those numbers.</p> <p>c. Page 8 mentions data on both why participants violated stay-at-home orders, and their levels of anxiety. Why aren't those data included in the primary analyses?</p> <p>d. Page 9 mentions Maslow's Hierarchy of Needs. That specific theory is generally considered outdated amongst psychologists. Instead I'd suggest citing Baumeister &amp; Leary (1995, Journal of Personality and Social Psychology) for evidence in favor of the idea that belonging is a fundamental human need.</p> |
|--|--|

|  |   |
|--|---|
|  | <p>I sincerely hope that my comments are helpful to the authors. If the issues I detail in point #1 above are addressed, I will be in a better position to assess the validity of the analyses and findings. I look forward to reading the revised manuscript.</p> <p>-Jeffrey Lees</p> |
|--|---|

|                        |   |
|------------------------|---|
| <b>REVIEWER</b>        | Daniel Allington<br>King's College London, UK |
| <b>REVIEW RETURNED</b> | 12-Aug-2020                                   |

|                         |  |
|-------------------------|--|
| <b>GENERAL COMMENTS</b> | <p>The data collected are very valuable and interesting. However, I struggled to understand and evaluate the analysis because it was not well reported or explained. I also could do nothing with the study protocol, as it was presented in the original language only and without an English translation.</p> <p>Because the main analytic method used was regression, I would have appreciated two tables of regression coefficients for all the independent variables: one for the linear regression and one for the logistic regression. Having only some coefficients reported in the text meant that I couldn't see the whole picture. Also, I was puzzled as to why both linear regression and logistic regression were used: I presume that the linear regression was a linear probability model, in which case the two regressions were essentially doing the same thing. Maybe this was done for reasons of interpretability, i.e. because it's easier to compare coefficients in a linear probability model, but you need a logit to calculate odds? But there was no real explanation, so I couldn't be sure what was going on. Furthermore, when Chi-square tests of association between individual protective measures were reported, there was no indication of whether associations were positive or negative. I also found it confusing that there were apparently two stratified analyses of age, one with very wide bands (e.g. 21-49) and one looking at smaller bands within bands (e.g. 31-40). Results of these analyses were presented selectively in the text rather than exhaustively in tables, so it was all very difficult to make sense of. One minor point: if the odds ratio is close to 2.00 (page 8, paragraph 2), that means that that the odds of the event are twice as high, but not that the event is twice as likely.</p> <p>There are also some language errors that should be corrected. The title, for example, should read "Behavioural Compliance with Personal Protection among Chinese Community-Dwellers During COVID-19: Correlates and Indicators" (not "in Chinese" or "the COVID-19"). To continue in this vein, in the second paragraph of the introduction, it says "masking-wearing" instead of "mask-wearing". Although the language errors were at a surface level, meaning that I was able to perceive the writers' intended meanings, this would create a poor impression on readers.</p> <p>Lastly, while this may be the first study to look at the effects of demographic variables on compliance with COVID-19 rules in China, it's definitely not the first one to look at this in the whole world, so a comparison with recent studies done in other countries would be appreciated. This will have consequences for the interpretation of findings. For example, the discussion of aspects of Confucian culture as an explanation for findings with regard to gender was very interesting, but account needs to be taken of</p> |
|-------------------------|--|

|  |  |
|--|--|
|  | <p>whether researchers in other countries have found similar effects or not. If they have, then we may not need Confucian culture as an explanation.</p> <p>In summary, I feel that some very good and important work has been done in designing the data collection and carrying out the study, but that it has not been written up in a form that is yet suitable for publication.</p> |
|--|--|

### VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1

Reviewer Name: Jeffrey Lees

Institution and Country: Harvard University

Competing interests: None declared

Please leave your comments for the authors below

I enjoyed reading this timely manuscript which examines compliance with personal protective behaviors meant to prevent the spread of COVID-19 amongst a national sample of Chinese community-dwellers. Below I list several thoughts, concerns, and suggestions for improving upon the manuscript as written. They are listed in order from what I consider most pressing to least. Thank you.

1. Perhaps my largest concern with the manuscript is the lack of clarity surrounding many of the measures and analyses. I applaud the manuscript for its conciseness, but the dedication to conciseness often left me without critical information needed to access the analyses and findings. Two things would help clarify much of what I ask for below. First, the authors provide a document titled "Study\_Protocol" (which I translated into English using Google Docs, so note the possibility for mistranslation). Yet this document seems not to be the study protocol (i.e. the survey) itself, but rather the grant application used to fund myriad sets of studies, this one included. It provides very little information about this specific study and the data collected for this manuscript. We previously had an incorrect understanding of how extensive the content of the protocol required by the journal was, and hence have submitted the whole application. We have retracted the document and submitted an original Mandarin version together with a translated version of the survey to the journal. Please see supplemental material: "COVID-19 Survey".

I would strongly suggest that the authors include a copy of the survey participants took so that both reviewers and future consumers of this manuscript can see the precise measures. Second, I would strongly suggest that the authors include with their submission the data (i.e. a CVS file) and analyses (SPSS files). Including the survey, data, and analyses with the manuscript would better adhere to BMJ Open's commitment to transparency, and allow readers to better understand the survey. We thank the reviewer for his suggestion. We are more than happy to submit our survey, dataset, and analysis output together with a syntax file to the journal to facilitate better understanding among our readership.

a. How the analyses are conducted is quite unclear. The authors report analyses of "overall behavioral compliance," but it is unclear what this variable is precisely. Is it simply a binary variable denoting whether participants complied with all four behaviors? If so, then why is it analyzed using

linear regression whereas all the other analyses use logistic regression? Additionally, since said logistic regressions show divergent effects based on specific behaviors (i.e., the opposite gender effects), this itself calls into questions the validity of examining compliance with all four behaviors together.

We defined overall compliance as the sum of 4 individual protective behaviours. We therefore generated a 5-point scale which was analysed as a continuous variable in the linear regression model. On the other hand, as all respective protective behaviors were rated as 0 (no), and 1 (yes), they were treated as binary outcomes in the logistic regression model.

We have added in a description on page 6 under the Methods section, to define overall behavioural compliance:

“Overall compliance is defined as the sum of compliance score on 4 protective behaviours, rated at 0 (none), 1 (compliant with 1 behaviour), 2 (compliant with 2 behaviours), 3 (compliant with 3 behaviours), and 4 (compliant with all 4 behaviours).”

b. Another area needing of clarity is when/whether the findings presented are from multiple regressions. Table 2 is a prime area where this is both unclear and affects the interpretation of results. Are all the odd-ratios reported from single predictor logistic models, or are there many non-significant predictors in the models but not being shown?

The reviewer is correct that many non-significant predictors in the model were not shown. We have updated the table and included all predictors we included in the analysis for overall behavioural compliance. Please see the new Table 2 on page 7:

Table 2. Indicators of overall behavioural compliance levels

|                                      | Beta (95% CI)       |
|--------------------------------------|---------------------|
| Age                                  | 0.06 (0.003, 0.012) |
| Gender (female)                      | -0.07 (-0.15, 0.01) |
| Regional Risk-exposure               | 0.16 (0.13, 0.21)   |
| Timeliness for Policy Implementation | 0.02 (-0.003, 0.01) |

Boldface indicates  $p < 0.05$ .

We have also updated the table for compliance with individual mitigation measures; please see Table 3 on pages 8-9:

Table 3. Indicators for compliance with respective mitigation measures

|                                      | Compliance vs. Non. Compliance<br>OR (95%CI) |
|--------------------------------------|--|
| Home Quarantine                      |  |
| Gender, Female                       | 1.66 (1.38-1.97)                             |
| Age                                  | 0.99 (0.98-0.999)*                           |
| Regional Risk-exposure               | 0.99 (0.90-1.08)                             |
| Timeliness for Policy Implementation | 1.003 (0.99-1.02)                            |
| Mask-Wearing                         |  |
| Gender, Female                       | 0.56 (0.46-0.67)                             |
| Age                                  | 1.03 (1.02-1.04)*                            |
| Regional Risk-exposure               | 1.41 (1.28-1.54)                             |
| Timeliness for Policy Implementation | 0.99 (0.98-1.01)                             |
| Temperature-taking                   |  |
| Gender, Female                       | 0.79 (0.66-0.95)                             |
| Age                                  | 1.005 (0.995-1.02)*                          |
| Regional Risk-exposure               | 1.40 (1.28-1.54)                             |

|                                      |                       |
|--------------------------------------|-----------------------|
| Timeliness for Policy Implementation | 1.016 (0.998 -1.035)* |
| Hand-sanitising                      |                       |
| Gender, Female                       | 0.88 (0.74-1.05)      |
| Age                                  | 1.02 (1.01-1.04)*     |
| Regional Risk-exposure               | 1.39 (1.27-1.52)      |
| Timeliness for Policy Implementation | 1.01 (0.995-1.03)     |

Boldface indicates significance,  $p < 0.0125$

\*. Trend to significance ( $0.0125 < p < 0.05$ )

c. How variables are being measured also needs clarity. Age and regional risk exposure, for example, are written about as blocks (e.g., the age group 21-49), but are they analyzed this way in the logistic regressions, or are they analyzed as linear variables (which in my opinion they should be)?

We thank the reviewer for his advice. We have revised the Methods section under “Statistical Analysis” to specify that we used age as a continuous variable (see page 6):

“Linear regression models were employed to examine the association between overall compliance to 4 mitigation measures and demographics, including age as a continuous variable, and gender, as well as social determinants such as regional risk exposure and days to regional implementation of mitigation measures.”

We have also re-done the analysis using age as a continuous variable and reported the beta value in the manuscript. Age and regional risk exposure level remained as significant indicators in the model. Please see page 7:

“Linear regression analysis was done to examine the indicators for total compliance with mitigation measures. Among all demographics and social indicators, age ( $\beta$ [95%CI]=0.06[0.003-0.012] and regional risk-exposure ( $\beta$ [95%CI]=0.155[0.131-0.209]) were independently associated with an increased level of behavioural compliance.”

2. My second area of concern relates to the authors’ interpretations of their results. I agree with their primary interpretation that their gender/age effects are likely explained by “middle-age” men who are leaving the home to work. However, there are times when the authors stray from this interpretation. For example, in several places (e.g. the top and bottom of page 8) the manuscript invokes evidence that men engage in more risky behaviors. The evidence in this manuscript to me is opposed to the “risky behavior” interpretation. While men do violate the stay-at-home order more, they also are more likely to engage in other preventive behaviors, which suggests that while they’re going out to work more, they’re aware that their behavior is risky and taking steps to mitigate that risk. I would suggest that the authors be more consistent throughout the discussion in centering social norms/male breadwinner interpretation.

We thank the reviewer for their comment. We have clarified our interpretation so that we are more consistent in our discussion centering the social norms/male breadwinner interpretation. Please see page 11:

“Our study lends some support to Zhong and colleagues’ findings that males were more likely to leave the house to go to crowded places during the outbreak in China<sup>8</sup>, but contradict in mask-wearing compliance. Higher likelihood of risk-taking behaviour in males was noted as an explanation for their non-compliance. On the contrary, we found that males were compliant to other preventive measures to mitigate risk. Notably, Zhong and colleagues gathered their data between Jan 27 and Feb 1 2020<sup>8</sup>, a week after the lockdown in China, three weeks earlier than when our data were collected.

Therefore, strict restrictions and public health education by authorities during the three weeks may have been effective and enabled males to engage in more preventive measures even though they were still leaving home for work. Moreover, in a separate survey conducted as part of the large project, we found a higher percentage of men leaving the house for essential services, amongst which 53% reported moderate levels of anxiety, therefore demonstrating that levels of anxiety were high enough to encourage males to comply with other protective measures when they were out of the

house or when they were not practising social distancing. Thus, all these coupled with the importance of filial piety and the male breadwinner role in Confucianism may explain why males were more non-compliant to social distancing orders during the outbreak.”.

3. I would suggest that the introduction be expanded upon to provide more information about the context. For example, the specific restrictions put in by the Chinese government are not clear, and the citation provided does not allow one to actually read something that provides such details. Moreover, what specific punishments one might receive for violating guidelines isn't mentioned at all. I would suggest to the authors that they write the introduction such that someone reading it 10 years in the future (hopefully far removed from the current pandemic circumstances) can readily understand the context participants in the survey found themselves in during February 2020 in China.

We thank the reviewer for their suggestion. After the announcement of the home-quarantine order, the implementation was executed to the maximum level of stringency. Individuals who violated the guidelines would receive strict education and immediate correction from local authority from various levels (street and community management, to district and city-level management.) We agree with the reviewer that such information should be detailed in the main body. Hence we have included the specific restrictions in the Introduction on page 3, and included a clearer citation from the WHO-China Joint Mission:

“Notably, prevention and control measures were implemented in three phases: 1) suspension of intra-city and intercity transportation, and strict control of importation and exportation of COVID-19 cases from Wuhan and other provinces, 2) delaying the severity and rise in cases through several safety measures, 3) decreasing clusters, using standardised protocols and execution of “scientific evidence-based policy”. Examples of such measures include the closure of wet markets, contact tracing, temperature-taking, health declarations, quarantine, disallowing large gatherings, and implementation of strict travel restrictions.<sup>1 2”</sup>.

We have also added in the detailed regulations implemented even prior to the home quarantine order. See page 4:

“Hence prior to the enforcement of the nationwide home quarantine order, the Chinese government announced a series of precautionary regulations, including 1) refusal of entry into public places without wearing a mask and obtaining a normal body temperature; 2) set-up of a detailed individual purchase record of fever/cough/flu-related medications in local pharmacies; 3) screening and a detailed registry of suspected cases with high fever in the community. The entire enforcement was accompanied by thorough public health education and promotion which started as early as late January. Violation of the above-mentioned regulations could result in further investigation or even legal liability.<sup>6</sup>

After the implementation of the four personal protective behaviours, including home quarantine, mask-wearing, temperature-taking, and hand-sanitising, non-compliance would lead to strict education and immediate correction from various levels of management, ranging from the street and community, to district and city levels of local authority.”

4. Lastly, below are several minor thoughts/notes.

a. The sampling section on page 4 says this was conducted over two surveys. Why two? Were they identical?

There were indeed two surveys as part of the large project. However, the other survey had a different focus (social media and mental health) in a different group of participants. We thank the reviewer for pointing out this ambiguous point. We have changed the description on page 5 under “Study design and sampling”:

“With a cross-sectional study design, a nationwide online survey on the behavioural compliance during COVID-19 were carried out during February 14-17, 2020, among Chinese citizens in China.

b. At the bottom of page 7 it mentions “males were found less likely to be compliant with the social-distancing order (72% vs. 74%)...”, yet the data on Figure 1 don’t match those numbers. We thank the reviewer for pointing out the error in numbers. We have revised Figure 1’s values to match the numbers:

c. Page 8 mentions data on both why participants violated stay-at-home orders, and their levels of anxiety. Why aren’t those data included in the primary analyses?

We agree with the reviewer that if mental health questionnaires were included in the survey and analysis, it would be very interesting. However, as our current study is one of the two separate surveys conducted during that period, mental health information was not collected during our survey.

Even though we could peek into the data of the second survey and make some assumptions, we did not have the empirical evidence to back such theory up. Hence, we only brought up this point during discussion. We have further clarified it under discussion on page 11:

“Moreover, in a separate survey conducted as part of the large project, we found a higher percentage of men leaving the house for essential services, amongst which 53% reported moderate levels of anxiety, therefore demonstrating that levels of anxiety were high enough to encourage males to comply with other protective measures when they were out of the house or when they were not practising social distancing.”

d. Page 9 mentions Maslow’s Hierarchy of Needs. That specific theory is generally considered outdated amongst psychologists. Instead I’d suggest citing Baumeister & Leary (1995, Journal of Personality and Social Psychology) for evidence in favor of the idea that belonging is a fundamental human need.

We thank the reviewer for the suggestion and have cited Baumeister & Leary instead of Maslow for their work on belonging being a fundamental human need. Please see page 12:

“According to the belongingness hypothesis posited by Baumeister and Leary, a sense of belonging is a fundamental human need that ultimately motivates and drives human behaviour.<sup>23</sup>”

I sincerely hope that my comments are helpful to the authors. If the issues I detail in point #1 above are addressed, I will be in a better position to assess the validity of the analyses and findings. I look forward to reading the revised manuscript.

-Jeffrey Lees

Reviewer: 2

Reviewer Name: Daniel Allington

Institution and Country: King's College London, UK

Competing interests: None declared

Please leave your comments for the authors below

The data collected are very valuable and interesting. However, I struggled to understand and evaluate the analysis because it was not well reported or explained. I also could do nothing with the study protocol, as it was presented in the original language only and without an English translation.

According to reviewers’ comments and suggestions, we have made extensive modifications to the analysis and results. We have also included our survey in its original form in both Mandarin and English. We have also submitted our dataset, output file and syntax file to the journal to facilitate a better understanding among the readership.



Because the main analytic method used was regression, I would have appreciated two tables of regression coefficients for all the independent variables: one for the linear regression and one for the logistic regression. Having only some coefficients reported in the text meant that I couldn't see the whole picture.

We thank the reviewer for their suggestion. We have added in a Table 2 to demonstrate the linear regression coefficients, and have changed Table 3 to include all indicators. Please see pages 7-9 for Tables 2 and 3:

Table 2. Indicators of overall behavioural compliance levels

|                                      | Beta (95% CI)       |
|--------------------------------------|---------------------|
| Age                                  | 0.06 (0.003, 0.012) |
| Gender (female)                      | -0.07 (-0.15, 0.01) |
| Regional Risk-exposure               | 0.16 (0.13, 0.21)   |
| Timeliness for Policy Implementation | 0.02 (-0.003, 0.01) |

Boldface indicates  $p < 0.05$ .

Table 3. Indicators for compliance with respective mitigation measures

|                                      | Compliance vs. Non. Compliance OR (95%CI) |
|--------------------------------------|---|
| Home Quarantine                      |   |
| Gender, Female                       | 1.66 (1.38-1.97)                          |
| Age                                  | 0.99 (0.98-0.999)*                        |
| Regional Risk-exposure               | 0.99 (0.90-1.08)                          |
| Timeliness for Policy Implementation | 1.003 (0.99-1.02)                         |
| Mask-Wearing                         |   |
| Gender, Female                       | 0.56 (0.46-0.67)                          |
| Age                                  | 1.03 (1.02-1.04)*                         |
| Regional Risk-exposure               | 1.41 (1.28-1.54)                          |
| Timeliness for Policy Implementation | 0.99 (0.98-1.01)                          |
| Temperature-taking                   |   |
| Gender, Female                       | 0.79 (0.66-0.95)                          |
| Age                                  | 1.005 (0.995-1.02)*                       |
| Regional Risk-exposure               | 1.40 (1.28-1.54)                          |
| Timeliness for Policy Implementation | 1.016 (0.998 -1.035)*                     |
| Hand-sanitising                      |   |
| Gender, Female                       | 0.88 (0.74-1.05)                          |
| Age                                  | 1.02 (1.01-1.04)*                         |
| Regional Risk-exposure               | 1.39 (1.27-1.52)                          |
| Timeliness for Policy Implementation | 1.01 (0.995-1.03)                         |

Boldface indicates significance,  $p < 0.0125$

\*. Trend to significance ( $0.0125 < p < 0.05$ )

Also, I was puzzled as to why both linear regression and logistic regression were used: I presume that the linear regression was a linear probability model, in which case the two regressions were essentially doing the same thing. Maybe this was done for reasons of interpretability, i.e. because it's easier to compare coefficients in a linear probability model, but you need a logit to calculate odds? But there was no real explanation, so I couldn't be sure what was going on.

We performed linear regression analysis using overall compliance level (sum of all 4 individual behaviours, with a total score ranging from 0 to 4) as the outcome variable. As for the logistic regression model, we looked at compliance to each individual behaviour. We have further clarified the definition of “overall behavioural compliance” on page 6:

“Overall compliance is defined as the sum of compliance score on 4 protective behaviours, rated at 0 (none), 1 (compliant with 1 behaviour), 2 (compliant with 2 behaviours), 3 (compliant with 3 behaviours), and 4 (compliant with all 4 behaviours).

#### Statistical Analysis

Linear regression models were employed to examine the association between overall compliance to 4 mitigation measures and demographics, including age as a continuous variable, and gender, as well as social determinants such as regional risk exposure and days to regional implementation of mitigation measures. Logistic regression models were applied to investigate the indicators for accordance with each individual mitigation measure. All analyses were performed using SPSS version 25 software, and statistical significance was determined as two-tailed p-value < 0.05. Bonferroni correction was employed to obtain an adjusted significance level for each protective behaviour:  $\approx 0.05/4 = 0.0125$ .”

Furthermore, when Chi-square tests of association between individual protective measures were reported, there was no indication of whether associations were positive or negative.

We have added in the direction of the chi-square results on pages 7-9 under “Compliance with individual protective measures”:

“Compliance with social distancing was positively associated with compliance with hand-sanitising ( $\chi^2 = 4.21$ ,  $p = 0.023$ ), but not with mask-wearing and temperature-taking ( $p = 0.07$  and  $0.08$ , respectively). Compliance with mask-wearing was positively associated with temperature-taking ( $\chi^2 = 493.11$ ,  $p < 0.001$ ) and hand-sanitising ( $\chi^2 = 498.55$ ,  $p < 0.001$ ). Compliance with temperature-taking was positively associated with hand-sanitising ( $\chi^2 = 802.16$ ,  $p < 0.001$ ).”

I also found it confusing that there were apparently two stratified analyses of age, one with very wide bands (e.g. 21-49) and one looking at smaller bands within bands (e.g. 31-40). Results of these analyses were presented selectively in the text rather than exhaustively in tables, so it was all very difficult to make sense of.

Due to limited space in the main manuscript, we did not report the results in table format but we have included the analysis output and syntax file together with the submission.

The text-format report of the result can be found on page 9:

“Interestingly, the mid-age group (21-50) was the most non-compliant age group for social-distancing, nevertheless also the most compliant group for other protective behaviours (Figure 2). Further stratified analysis showed that, in the mid-age group, those aged 31-40 were the least compliant to the social distancing order (OR=4.17, 95% CI=3.07-5.66), four times as low to stay at home compared to the most compliant age group (<21 years of age), yet the most compliant group for mask-wearing (OR=1.96, 95%CI=1.46-2.64), hand-sanitising (OR=2.24, 95% CI=1.70-2.96), and temperature-taking (OR=1.65, 95% CI=1.23-2.21) In addition, the 41-50 age group was found more compliant for mask-wearing (OR=1.88, 95% CI=1.24-2.87), whilst the 21-30 age group was found more compliant for hand-sanitising (OR=1.43, 95% CI=1.13-1.80), as compared to the younger adult group.”

One minor point: if the odds ratio is close to 2.00 (page 8, paragraph 2), that means that that the odds of the event are twice as high, but not that the event is twice as likely.

We thank the reviewer for the correction. We have made changes to the text on page 9: “those aged 31-40 were...., four times as low to stay at home...”

There are also some language errors that should be corrected. The title, for example, should read "Behavioural Compliance with Personal Protection among Chinese Community-Dwellers During COVID-19: Correlates and Indicators" (not "in Chinese" or "the COVID-19"). To continue in this vein, in the second paragraph of the introduction, it says "masking-wearing" instead of "mask-wearing". Although the language errors were at a surface level, meaning that I was able to perceive the writers' intended meanings, this would create a poor impression on readers.

We sincerely thank the reviewer for correcting the errors. We apologise for the poor impression we have left on our readership due to such language errors. We have changed the title as well as the typos in the manuscript and have checked thoroughly throughout the whole manuscript.

Lastly, while this may be the first study to look at the effects of demographic variables on compliance with COVID-19 rules in China, it's definitely not the first one to look at this in the whole world, so a comparison with recent studies done in other countries would be appreciated. This will have consequences for the interpretation of findings. For example, the discussion of aspects of Confucian culture as an explanation for findings with regard to gender was very interesting, but account needs to be taken of whether researchers in other countries have found similar effects or not. If they have, then we may not need Confucian culture as an explanation.

We thank the reviewer for the suggestion and have looked into studies done in other countries. We have expanded on our discussion on pages 10-11:

"Our findings support previous literature that reported males were more likely to leave their homes during the early stages of the COVID-19 outbreak in the Hubei Province and other parts of China.<sup>8</sup> Interestingly, recent studies investigating behavioural compliance to safety measures (including mask-wearing, isolation) outside of China during COVID-19 have mixed results.<sup>14-</sup>

<sup>18</sup> Nonetheless, explanations for non-compliance to safety measures centres around the level of knowledge and perception of the COVID-19 pandemic. Similarly, past pandemic research have shown that females are more likely to adhere to more avoidant behaviors such as wearing masks.<sup>8-10</sup>

<sup>19</sup> Conversely, we found that males are more likely to comply with these avoidant behaviours apart from social distancing. Our findings may be explained by the male breadwinner model that still exists in China's social fabric today despite the increasingly blurred gender roles in modern-day China.<sup>20</sup> In relation to Confucian culture, the over 2000-year old model posits a gender role divide between males and females where males undertake an 'outside' role and are expected to provide for the family, while females take on the caregiving role ('inside' role) to tend to household matters.<sup>21-24</sup> Furthermore, a strong emphasis is placed on filial piety, where providing and caring for one's elderly parents is an esteemed and obligatory duty.<sup>25</sup> Qian and Qian<sup>22</sup> reported greater happiness in males when they are employed and providing for the family compared to females' employment status. Therefore, nonconformity to expectations of social roles particularly in the economic aspect may inevitably affect the health and welfare of both males and females. As a result, the conformity to role expectations in Chinese society may explain the non-compliance by males to social distancing measures as they are expected to continue providing and caring for the family, including their parents, even during a public health crisis. Additionally, gender only affected social distancing compliance in people above 21 years old, an age group where most working-class fall into, hence supporting our findings where more males reported leaving their homes for work purposes. Our study lends some support to Zhong and colleagues' findings that males were more likely to leave the house to go to crowded places during the outbreak in China<sup>8</sup>, but contradict in mask-wearing compliance. Higher likelihood of risk-taking behaviour in males was noted as an explanation for their non-compliance. On the contrary, we found that males were compliant to other preventive measures to mitigate risk. Notably, Zhong and colleagues gathered their data between January 27 and February 1 2020<sup>8</sup>, a week after the lockdown in China, three weeks earlier than when our data were collected. Therefore, strict restrictions and public health education by authorities during the three weeks may have been effective and enabled males to engage in more preventive measures even though they were still leaving home for work. Moreover, in a separate survey conducted as part of the large project, we found a higher percentage of men leaving the house for essential services, amongst which 53% reported moderate levels of

anxiety, therefore demonstrating that levels of anxiety were high enough to encourage males to comply with other protective measures when they were out of the house or when they were not practising social distancing. Thus, all these coupled with the importance of filial piety and the male breadwinner role in Confucianism may explain why males were more non-compliant to social distancing orders during the outbreak.”

In summary, I feel that some very good and important work has been done in designing the data collection and carrying out the study, but that it has not been written up in a form that is yet suitable for publication.

**VERSION 2 – REVIEW**

|                        |   |
|------------------------|---|
| <b>REVIEWER</b>        | Jeffrey Lees<br>Clemson University, USA |
| <b>REVIEW RETURNED</b> | 30-Sep-2020                             |

|                         |   |
|-------------------------|---|
| <b>GENERAL COMMENTS</b> | <p>I enjoyed reading this revised manuscript examining compliance with COVID-19 preventive behaviors in a Chinese sample. I thank the authors for the greatly increased clarity regarding the analyses performed. Nonetheless, there is further clarity to be had. But more critically, the authors have yet to fully consider the inferences from and implications of their findings, and to appropriately integrate those implications into their analyses.</p> <p>Major Concerns</p> <p>1. In sum, I believe the authors have a compelling finding regarding disparate age/gender effects across compliance behaviors, yet they do not fully consider the significance of this or engage in parsimonious inferences about it. The inference that ought to be made here is simple: individuals who were most likely to be leaving the home for work/personal responsibilities prior to the pandemic (men, those of middle age) are the most likely to violate quarantine, but also seem to be taking the most precautions in other domains of preventive behaviors, in accordance with a desire to be safe while meeting their economic responsibilities. The authors do make this point, but the significance of this gets lost in what is essentially a lot of guesswork about filial piety, Confucian gender roles, psychological anxiety, and a sense of belonging. None of this is necessary, as the economic explanation is the most parsimonious, and most significant. The discussion section should be significantly revamped to reflect more parsimonious explanations.</p> <p>2. The findings are significant because they challenge a lot of work on COVID-19 preventive behaviors which treat compliance with those behaviors as (1) unidimensional, and (2) the result of context-less psychological processes like partisanship (in the USA), reactance, perceptions of effectiveness, social norms, etc. The results here show that economic conditions meaningfully drive divergent compliance behaviors. This result isn't fully integrated into the paper in many ways (e.g., the “conclusion” in the abstract still states that noncompliance is largely attributed to men and the middle-aged, but this oversimplifies the authors’ own result). Moreover, the fact that gender and age predict different effects depending on the preventive behavior belies the linear regression analysis performed. The authors’ own results suggest we should not be treating the behaviors equivalently, so the analysis where</p> |
|-------------------------|---|

|  |  |
|--|--|
|  | <p>the outcome is compliance with 0-4 of the behaviors is an invalid analysis and should be removed from the paper.</p> <p>3. In addition to these inferential issues, there's still lack of clarity around some of the analyses. The newly added analyses of age-groups at the end of the analysis section are unclear. It's not clear if these are from multiple regressions or not, or if they're reiterations of the analyses in Table 3. This wouldn't appear to be the case, as those analyses treat age as continuous instead of blocked. Moreover, the authors' need to justify using age-blocks, as their own analyses in Table 3 show that measuring age as a continuous variable (which is superior analytically) shows little to no relationship with preventive behaviors. If the authors are expecting curvilinear relationships (which their theorizing around mid-age individuals would suggest), then Table 3 should include exponential coefficients for age to test for such a relationship, rather than collapsing across meaningful variance by using arbitrary age groups.</p> <p>Minor Issues</p> <ol style="list-style-type: none"> <li>1. There are no survey instruments available on the BMJ portal where I access the revised manuscript and accompanying documentation. Please address.</li> <li>2. Several confidence intervals in Table 3 seem misreported. For example, the age confidence interval predicting temperature-taking is listed as 0.995-1.02, but also as significant at the &lt;0.05 level, which isn't possible with that confidence interval.</li> <li>3. In the first paragraph, the text added in for the revision is in past tense, but the remainder of the paragraph is present tense. Please address.</li> </ol> <p>Overall this revision substantially improves the manuscript in terms of clarity, although there's still a lack of clarity around some analyses. However, issues of inferential validity are still unresolved from the original draft, especially now that the results of the empirical analyses paint a clearer picture. I would suggest that the authors foreground the significance of their findings better, and provide more parsimonious explanations as to why they find what they find.</p> <p>I sincerely hope this review is helpful to the authors!</p> <p>-Jeffrey Lees</p> |
|--|--|

|                        |   |
|------------------------|---|
| <b>REVIEWER</b>        | Daniel Allington<br>King's College London<br>United Kingdom |
| <b>REVIEW RETURNED</b> | 19-Sep-2020   |

|                         |  |
|-------------------------|--|
| <b>GENERAL COMMENTS</b> | <p>This is much improved and I can now clearly see what was done. Some minor revisions are still required.</p> <p>The authors should make clear throughout that the outcome variables concern reported compliance rather than actual compliance. They should note the possible impact of social desirability bias, especially given that non-compliance is punishable by law. The authors might wish to argue that social desirability bias will have been mitigated by anonymity and by the online method of data collection.</p> |
|-------------------------|--|

I have ticked checkboxes indicating that methods are not described clearly enough for replication and that limitations are not discussed clearly. Both of these judgements relate to sampling. More detail must be provided on the sampling method. I see that the questionnaire was administered online, but the information 'the study team disseminated the survey questionnaire in multiple provinces' (p. 5) is insufficient. Who was the questionnaire disseminated to, and how were the recipients identified? If it was a self-selecting sample, which will be the case e.g. if the questionnaire was simply made available online, with a notice on social media, that's fine but the authors should say so clearly as that will have an impact on reliability and interpretation of results. Also, which provinces was the questionnaire disseminated in, and why those provinces in particular?

If it is (as I suspect) a non-probability sample such as a self-selecting sample, then p-values and confidence intervals are not strictly valid. It's fine to report them, but a statement should be added to the effect that they are not strictly valid, or that they are valid only under the assumption that the sample is equivalent to a random sample (which may or may not be the case).

Regression coefficients would be easier to interpret if all variables were standardised to the range 0-1. I don't think that this has been done (I make this assumption because the coefficient for age in table 2 is so low). If it has been done, please say so. If it has not been done, please consider doing so.

On p. 8, reference to 'advanced age' is made. I think this is an error: 'advanced age' means 'old age', and suggests that a banded age variable was used, whereas a continuous numeric variable has been used up until that point in the paper. If this is the case, please simply correct to 'age'. On the other hand, a banded age variable is used on the following page (p. 9), so it may be that such a variable is being referenced on p. 8 also. If that was the case, please clarify (and explain which band is meant by 'advanced age').

A table with odds ratios for every combination of individual protective behaviours and banded age categories would be much easier to interpret than the paragraph with statistics in parentheses on p. 9 (lines 28-43) and would give the reader a clearer understanding of all the possible effects.

I was surprised by the finding that age was negatively associated with compliance with home quarantine, given that elderly people have fewer reasons to leave their homes than working-age people (and often suffer from restricted mobility). Was this association possibly produced by people in middle age only? That is to say: is it an artefact of using linear regression to study a non-linear relationship? This is in fact what is suggested by figure 2. Some discussion of this in the text would be appreciated. (A table of odds ratios as described in the previous paragraph would also help reveal this sort of pattern efficiently.)

The conclusion is very thorough and appears supported by the findings. The discussion of social roles for working age males as an explanation for the effects of gender and age was particularly good, especially with the comparison with other researchers' findings. I was puzzled by the mention of the 'working class' (p. 11,

|  |   |
|--|---|
|  | line 13 and p. 12, line 3), as class was not mentioned as a demographic previously (and the authors state that they did NOT collect data on educational level or occupational status). Was this perhaps an error? (E.g. should it have been 'working people', i.e. people in employment?) |
|--|---|

## VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Please leave your comments for the authors below

I enjoyed reading this revised manuscript examining compliance with COVID-19 preventive behaviors in a Chinese sample. I thank the authors for the greatly increased clarity regarding the analyses performed. Nonetheless, there is further clarity to be had. But more critically, the authors have yet to fully consider the inferences from and implications of their findings, and to appropriately integrate those implications into their analyses.

### Major Concerns

1. In sum, I believe the authors have a compelling finding regarding disparate age/gender effects across compliance behaviors, yet they do not fully consider the significance of this or engage in parsimonious inferences about it. The inference that ought to be made here is simple: individuals who were most likely to be leaving the home for work/personal responsibilities prior to the pandemic (men, those of middle age) are the most likely to violate quarantine, but also seem to be taking the most precautions in other domains of preventive behaviors, in accordance with a desire to be safe while meeting their economic responsibilities. The authors do make this point, but the significance of this gets lost in what is essentially a lot of guesswork about filial piety, Confucian gender roles, psychological anxiety, and a sense of belonging. None of this is necessary, as the economic explanation is the most parsimonious, and most significant. The discussion section should be significantly revamped to reflect more parsimonious explanations.

We thank the reviewer for his comment and agree that the economic conditions are the most parsimonious and significant interpretation of our findings. Meanwhile, this driving force for compliance to preventive behaviours may be also partially explained by the cultural expectations of middle-aged males in Chinese society.

We have significantly revamped our discussion and included the reviewer's suggestions in our discussion on pp. 9-10: "Our findings thus demonstrate that the impact of economic conditions alongside the desire to remain safe may be the predominant drivers for the disparities in behavioural compliance. However, behind such a potential driving force lies cultural expectations that adult males are subjected to in Chinese society. From a cultural standpoint, the male breadwinner model still exists in China's social fabric today despite the increasingly blurred gender roles in modern-day China.<sup>20</sup> The over 2000-year old Confucian model posits a gender role divide between males and females where males undertake an 'outside' role and are expected to provide for the family, while females take on the caregiving role ('inside' role) to tend to household matters.<sup>21-24</sup> A strong emphasis is also placed on filial piety, where providing and caring for one's elderly parents is an esteemed and obligatory duty.<sup>25</sup> As a result, the conformity to role expectations in Chinese society may explain the non-compliance by males to social distancing measures as they feel more obligated to meet their economic responsibilities to continue providing for the family, even during a public health crisis. Our results challenge several work on COVID-19 preventive behaviours that view behavioural compliance singularly as the result of partisanship, perceptions surrounding its effectiveness and the infection risks.<sup>17 26</sup>" and p. 10:

“However, our study found that people in the mid-age group, especially those between 31-40 and 41-50 years old (Figure 2) were driving this significance of lower compliance with home quarantine order, as compared to younger adults <21 years of age. Notably, those above 50 years old had a higher likelihood of staying home similar to those below 21. A plausible explanation for the reduced social distancing compliance in the 31-50 age group is that a large number of these people may be salarymen and have to leave home for work. On the other hand, those above 50 may be aware of the risks involved and have fewer reasons to leave the house. Furthermore, people in the >50 age group may have reduced mobility function.<sup>28</sup> Hence, those between 31 to 50 years old have lesser compliance to home quarantine due to economic reasons where they have to go out to work compared to those under 21 where majority of them were likely to be high school or university students, hence could not access campus due to temporary shutdown of all schools nationwide during the epidemic.<sup>29</sup>”

2. The findings are significant because they challenge a lot of work on COVID-19 preventive behaviors which treat compliance with those behaviors as (1) unidimensional, and (2) the result of context-less psychological processes like partisanship (in the USA), reactance, perceptions of effectiveness, social norms, etc. The results here show that economic conditions meaningfully drive divergent compliance behaviors.

We agree with the reviewer that economic status may have been the predominant drive for disparities in compliance. We have revised the discussion section and included this argument on p. 10: “As a result, the conformity to role expectations in Chinese society may explain the non-compliance by males to social distancing measures as they feel more obligated to meet their economic responsibilities to continue providing for the family, even during a public health crisis. Our results challenge several work on COVID-19 preventive behaviours that view behavioural compliance singularly as the result of partisanship, perceptions surrounding its effectiveness and the infection risks.<sup>17 26</sup>”

This result isn't fully integrated into the paper in many ways (e.g., the “conclusion” in the abstract still states that noncompliance is largely attributed to men and the middle-aged, but this oversimplifies the authors' own result).

We have changed the conclusion in the abstract. Please see p. 2: “Male gender was associated with lower compliance with home quarantine yet higher compliance with mask-wearing and temperature-taking. The middle-age participants (31-50 years of age) had lower compliance with home quarantine order but higher with other measures. In light of the ongoing COVID-19 pandemic, public health authorities should tailor policy implementation to disparities in demographic and social indicators.”

Moreover, the fact that gender and age predict different effects depending on the preventive behavior belies the linear regression analysis performed. The authors' own results suggest we should not be treating the behaviors equivalently, so the analysis where the outcome is compliance with 0-4 of the behaviors is an invalid analysis and should be removed from the paper.

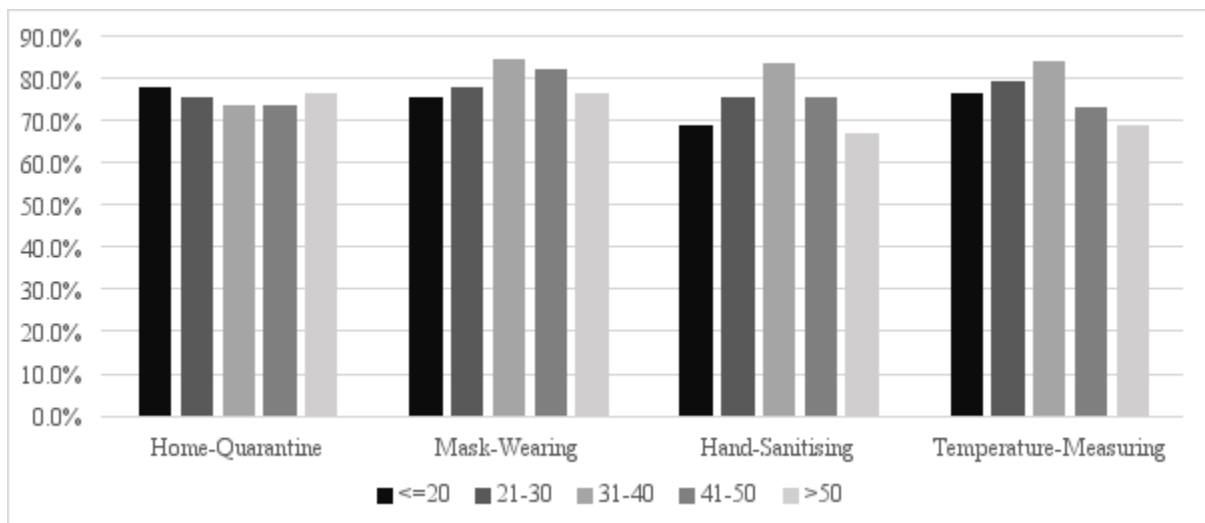
We took in the reviewer's advice and removed this part of the analysis. We instead focused on compliance with individual behaviours. We have therefore deleted table 2 along with the linear regression analysis.

3. In addition to these inferential issues, there's still lack of clarity around some of the analyses. The newly added analyses of age-groups at the end of the analysis section are unclear. It's not clear if



these are from multiple regressions or not, or if they're reiterations of the analyses in Table 3. This wouldn't appear to be the case, as those analyses treat age as continuous instead of blocked. Moreover, the authors' need to justify using age-blocks, as their own analyses in Table 3 show that measuring age as a continuous variable (which is superior analytically) shows little to no relationship with preventive behaviors. If the authors are expecting curvilinear relationships (which their theorizing around mid-age individuals would suggest), then Table 3 should include exponential coefficients for age to test for such a relationship, rather than collapsing across meaningful variance by using arbitrary age groups.

We realise the arbitrary grouping of age in the manuscript requires a compelling justification for such a grouping. We therefore took in the reviewer's comments and explored quadratic and exponential coefficients of age in the model. However, statistical significance was not observed (B=1.0, OR=1.0).



However, we recognise the importance of analysing the behaviour in different age-groups. Hence we generated a bar graph presenting distribution of age-specific behavioural compliance using age-blocks, with every 10 years of age as a block. From the bar graph we can observe a pattern of age-specific pattern for behavioural compliance. Subsequently we performed an additional analysis using age-blocks as a categorical indicator. The results can be found in Figures 2 and 3 and in text on p. 8:

Figure 2. Age-specific distribution for compliance with individual protective behaviours

“Whilst males were less prone to be compliant with home quarantine order, they were more likely to abide by the other 3 personal protective measures.

Interestingly, the mid-age group (21-50) was the most non-compliant age group for social-distancing, nevertheless also the most compliant for other protective behaviours (Figure 2).”

Figure 3. Adjusted ORs and 95% CI of different age-blocks for compliance with individual protective behaviours. Analysis controlled for gender, policy implementation days and risk exposure.

“Further analysis showed that, the 31-40 age group was less compliant to the home quarantine order (OR=0.71 [0.54-0.93]), compared to the reference group ( $\leq 20$ ; Figure 3). Yet they were compliant to

mask-wearing (OR=1.96, 95%CI=1.46-2.64), hand-sanitising (OR=2.24, 95% CI=1.70-2.96), and temperature-taking (OR=1.65, 95% CI=1.23-2.21). A similar pattern for compliance was also observed in the 41-50 age group, where they were less compliant to home quarantine (OR=0.67 [0.46-0.97]), nonetheless more compliant to mask-wearing (OR=1.88[1.24-2.87]) and hand-sanitising (OR=1.51[1.03-2.19]).”

#### Minor Issues

1. There are no survey instruments available on the BMJ portal where I access the revised manuscript and accompanying documentation. Please address.

We may have uploaded the file under the incorrect section in the portal. We have rectified this and uploaded the file as “Supplementary Material”.

2. Several confidence intervals in Table 3 seem misreported. For example, the age confidence interval predicting temperature-taking is listed as 0.995-1.02, but also as significant at the <0.05 level, which isn't possible with that confidence interval.

We have cross-checked the output file and rectified the table. Please see p. 7-8 for the new table (now Table 2). The numbers are in accordance with the output file we uploaded to the portal.

3. In the first paragraph, the text added in for the revision is in past tense, but the remainder of the paragraph is present tense. Please address.

We have changed the tense. Please see p. 3: “Notably, prevention and control measures have been implemented in three phases...”

Overall this revision substantially improves the manuscript in terms of clarity, although there's still a lack of clarity around some analyses. However, issues of inferential validity are still unresolved from the original draft, especially now that the results of the empirical analyses paint a clearer picture. I would suggest that the authors foreground the significance of their findings better, and provide more parsimonious explanations as to why they find what they find.

We appreciate the reviewer's sound input. Please see above response to these comments and suggestions.

#### Reviewer 2:

The authors should make clear throughout that the outcome variables concern reported compliance rather than actual compliance. They should note the possible impact of social desirability bias, especially given that non-compliance is punishable by law. The authors might wish to argue that social desirability bias will have been mitigated by anonymity and by the online method of data collection.

We thank the reviewer for pointing this out. We have clarified this on p. 11: “The study's outcome variables were also self-reported compliance instead of actual compliance, suggesting the potential impact of social desirability bias in under- or over-reporting compliance to safety measures.<sup>30</sup> However, the online mode of data collection and the anonymity of the survey may have mitigated such potential biases.”

I have ticked checkboxes indicating that methods are not described clearly enough for replication and

that limitations are not discussed clearly. Both of these judgements relate to sampling. More detail must be provided on the sampling method. I see that the questionnaire was administered online, but the information 'the study team disseminated the survey questionnaire in multiple provinces' (p. 5) is insufficient. Who was the questionnaire disseminated to, and how were the recipients identified? If it was a self-selecting sample, which will be the case e.g. if the questionnaire was simply made available online, with a notice on social media, that's fine but the authors should say so clearly as that will have an impact on reliability and interpretation of results. Also, which provinces was the questionnaire disseminated in, and why those provinces in particular?

We have revised the methods section to add in the sampling methods and the questionnaire dissemination approach. We have also rectified the statement on the province selection—the questionnaire was made available nationwide, with no specific selection for provinces. In the end, study participants in the present study were from 31 provinces and regions in the People's Republic of China.

Please see p. 5: "...disseminated the survey questionnaire nationwide in all 31 provinces and regions in China..." and "Study description and questionnaires were posted through various social media platforms, such as Wechat and Weibo, with a notice and invitation on these platforms for better visibility. All participants from this self-selecting sample provided electronic informed consent prior to taking the survey."

If it is (as I suspect) a non-probability sample such as a self-selecting sample, then p-values and confidence intervals are not strictly valid. It's fine to report them, but a statement should be added to the effect that they are not strictly valid, or that they are valid only under the assumption that the sample is equivalent to a random sample (which may or may not be the case).

We thank the reviewer for pointing this out and have clarified this under the limitations on p. 11: "A non-probability sample was used in this study, thus rendering the effect of p-values and confidence intervals not strictly valid, or valid only under the assumption that the sample is comparable to a random sample."

Regression coefficients would be easier to interpret if all variables were standardised to the range 0-1. I don't think that this has been done (I make this assumption because the coefficient for age in table 2 is so low). If it has been done, please say so. If it has not been done, please consider doing so.

We thank the reviewer for the suggestion. We did not standardise the variables. Following reviewer 1's advice, we have removed Table 2 and the related analysis. We instead focused on each individual behaviour. Please see pp. 7-8 for the results section.

On p. 8, reference to 'advanced age' is made. I think this is an error: 'advanced age' means 'old age', and suggests that a banded age variable was used, whereas a continuous numeric variable has been used up until that point in the paper. If this is the case, please simply correct to 'age'.

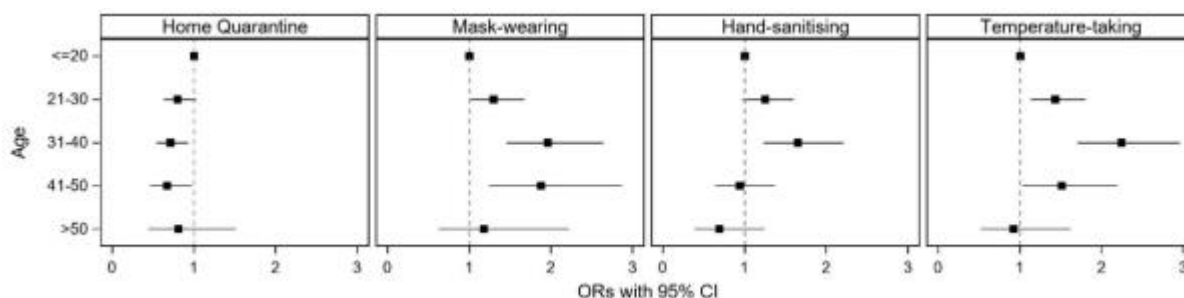
We have made the correction accordingly.

On the other hand, a banded age variable is used on the following page (p. 9), so it may be that such a variable is being referenced on p. 8 also. If that was the case, please clarify (and explain which band is meant by 'advanced age').

We have revised the analysis to provide more clarity. Please see pp. 7-8.

A table with odds ratios for every combination of individual protective behaviours and banded age categories would be much easier to interpret than the paragraph with statistics in parantheses on p. 9 (lines 28-43) and would give the reader a clearer understanding of all the possible effects.

We took in the reviewer's suggestion and generated a forest plot for better visibility and understanding.



Please see Figure 3 and text on p. 8: “Further analysis showed that, the 31-40 age group was less compliant to the home quarantine order (OR=0.71 [0.54-0.93]), compared to the reference group ( $\leq 20$ ; Figure 3). Yet they were compliant to mask-wearing (OR=1.96, 95%CI=1.46-2.64), hand-sanitising (OR=2.24, 95% CI=1.70-2.96), and temperature-taking (OR=1.65, 95% CI=1.23-2.21). A similar pattern for compliance was also observed in the 41-50 age group, where they were less compliant to home quarantine (OR=0.67 [0.46-0.97]), nonetheless more compliant to mask-wearing (OR=1.88[1.24-2.87]) and hand-sanitising (OR=1.51[1.03-2.19]).”

Figure 3. Adjusted ORs and 95% CI of different age-blocks for compliance with individual protective behaviours. Analysis controlled for gender, days to policy implementation and risk exposure.

I was surprised by the finding that age was negatively associated with compliance with home quarantine, given that elderly people have fewer reasons to leave their homes than working-age people (and often suffer from restricted mobility). Was this association possibly produced by people in middle age only? That is to say: is it an artefact of using linear regression to study a non-linear relationship? This is in fact what is suggested by figure 2. Some discussion of this in the text would be appreciated. (A table of odds ratios as described in the previous paragraph would also help reveal this sort of pattern efficiently.)

We apologise for the wrong impression caused. We have revised the analysis and included Figures 2 and 3 for further analysis showing compliance in each age group. As you can see in Figure 2 and the interpretation on p. 8, it is very likely that the middle-age groups were the primary drivers for the association in the analyses:

“Whilst males were less prone to be compliant with home quarantine order, they were more likely to abide by the other 3 personal protective measures.

Interestingly, the mid-age group (21-50) was the most non-compliant age group for social-distancing, nevertheless also the most compliant for other protective behaviours (Figure 2).

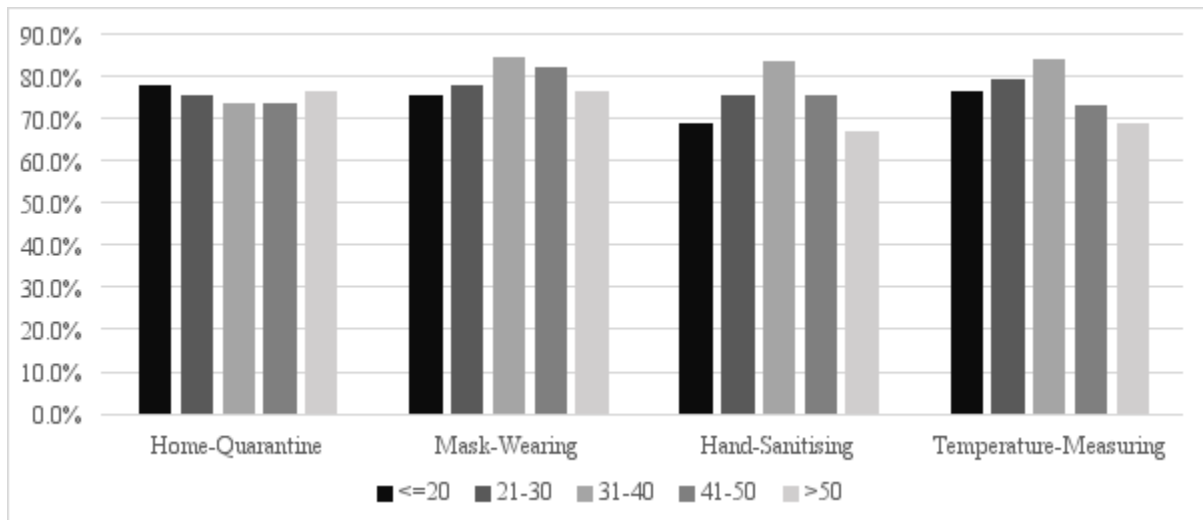
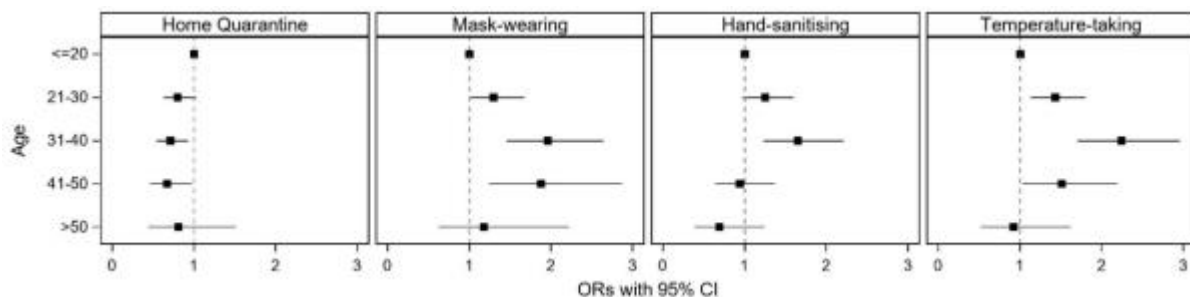


Figure 2. Age-specific distribution for compliance with individual protective behaviours



Further analysis showed that, the 31-40 age group was less compliant to the home quarantine order (OR=0.71 [0.54-0.93]), compared to the reference group ( $\leq 20$ ; Figure 3). Yet they were compliant to mask-wearing (OR=1.96, 95%CI=1.46-2.64), hand-sanitising (OR=2.24, 95% CI=1.70-2.96), and temperature-taking (OR=1.65, 95% CI=1.23-2.21). A similar pattern for compliance was also observed in the 41-50 age group, where they were less compliant to home quarantine (OR=0.67 [0.46-0.97]), nonetheless more compliant to mask-wearing (OR=1.88[1.24-2.87]) and hand-sanitising (OR=1.51[1.03-2.19]).

Figure 3. Adjusted ORs and 95% CI of different age-blocks for compliance with individual protective behaviours. Analysis controlled for gender, days to policy implementation and risk exposure.”

We have also included the possible explanation in discussion on p. 10: “However, our study found that people in the mid-age group, especially those between 31-40 and 41-50 years old (Figure 2) were driving this significance of lower compliance with home quarantine order, as compared to younger adults <21 years of age. Notably, those above 50 years old had a higher likelihood of staying home similar to those below 21. A plausible explanation for the reduced social distancing compliance in the 31-50 age group is that a large number of these people may be salarymen and have to leave home for work. On the other hand, those above 50 may be aware of the risks involved and have fewer reasons to leave the house. Furthermore, people in the >50 age group may have reduced mobility function.<sup>28</sup> Hence, those between 31 to 50 years old have lesser compliance to home quarantine due to economic reasons where they have to go out to work compared to those under 21 where majority of them were likely to be high school or university students, hence could not access campus due to temporary shutdown of all schools nationwide during the epidemic.<sup>29</sup>”

The conclusion is very thorough and appears supported by the findings. The discussion of social roles for working age males as an explanation for the effects of gender and age was particularly good, especially with the comparison with other researchers' findings. I was puzzled by the mention of the 'working class' (p. 11, line 13 and p. 12, line 3), as class was not mentioned as a demographic previously (and the authors state that they did NOT collect data on educational level or occupational status). Was this perhaps an error? (E.g. should it have been 'working people', i.e. people in employment?)

We thank the reviewer for pointing this out. Yes, it is an error and we have rectified it to "working people".

### VERSION 3 – REVIEW

|                        |   |
|------------------------|---|
| <b>REVIEWER</b>        | Jeffrey Lees<br>Clemson University, USA |
| <b>REVIEW RETURNED</b> | 18-Nov-2020                             |

|                         |  |
|-------------------------|--|
| <b>GENERAL COMMENTS</b> | <p>I am largely satisfied by the revisions made to the manuscript. I have a few minor points the authors should address, but otherwise I believe this manuscript is suitable for publication.</p> <p>Minor points:</p> <ol style="list-style-type: none"> <li>1. The (new) interpretations regarding economic considerations (and Confusion values) driving the distinct gender/age effects should be reflected in the "Conclusions" section of the abstract.</li> <li>2. Having now seen the questionnaire, the fact that the items ask about compliance behavior "today" needs to be noted clearly in the "Compliance of Mitigation Measures" section on pg. 5. This doesn't meaningfully affect the results, but it's an important methodological detail that needs to be in the manuscript.</li> <li>3. If the authors are going to mention graphs of data distribution (e.g., on page 8 regarding age) then those graphs should be included in the text.</li> </ol> <p>I thank the authors for their good work and careful consideration of our comments and concerns!</p> <p>-Jeffrey lees</p> |
|-------------------------|--|

|                        |   |
|------------------------|---|
| <b>REVIEWER</b>        | Daniel Allington<br>King's College London, UK |
| <b>REVIEW RETURNED</b> | 22-Nov-2020                                   |

|                         |   |
|-------------------------|---|
| <b>GENERAL COMMENTS</b> | <p>This is an important and useful study. Problems with earlier drafts have largely been solved. It is now much easier to understand what was done.</p> <p>Minor points:</p> <ol style="list-style-type: none"> <li>1) On p. 5, a numeric variable called 'Overall compliance' is defined but it doesn't seem to be used in the analysis (perhaps this was at one time used as the dependent variable in a linear regression?).</li> <li>2) On p. 8, it is stated that 'Compliance with home quarantine was positively associated with compliance with handsanitising ... but not with mask-wearing and temperature-taking (p=0.07 and 0.08, respectively)'. The p-values suggest that there may have been a</li> </ol> |
|-------------------------|---|

|  |   |
|--|---|
|  | <p>non-significant association, so it would be good to know whether it was positive or negative.</p> <p>3) I didn't notice this previously, but I was struck by the fact that the timeliness variable never had a significant effect. The authors could consider adding a comment on this.</p> <p>Possibly more important point:<br/>Currently, only odds ratios with 95% confidence intervals are reported for the logistic regression model, with a graphical indication of significance (use of bold type for p values below a certain threshold). I would usually expect to see regression models reported in more detail (i.e. with intercepts, and with the coefficient estimates, standard errors, and confidence intervals from which the odds ratios were derived, and also with actual p values). However, nobody ever really does anything with that extra detail, and it may be that this form of reporting is acceptable for this particular journal, so I leave it to the editor to decide whether the form of reporting used by the authors should be considered adequate.</p> |
|--|---|

### VERSION 3 – AUTHOR RESPONSE

Reviewer: 1

Comments to the Author

I am largely satisfied by the revisions made to the manuscript. I have a few minor points the authors should address, but otherwise I believe this manuscript is suitable for publication.

Minor points:

1. The (new) interpretations regarding economic considerations (and Confucian values) driving the distinct gender/age effects should be reflected in the "Conclusions" section of the abstract.

We have added a sentence in the "Conclusions" section of the abstract: "These findings may be supported by the economic considerations and the long-inherited Confucian values among Chinese."

2. Having now seen the questionnaire, the fact that the items ask about compliance behavior "today" needs to be noted clearly in the "Compliance of Mitigation Measures" section on pg. 5. This doesn't meaningfully affect the results, but it's an important methodological detail that needs to be in the manuscript.

We have changed the definition on page 5:

"mask-wearing, as defined by wearing a mask when leaving the residential address on the day of the survey, as per the Chinese government's regulation; temperature-taking, as defined by taking one's own temperature at least once on the day of the survey, as per the Chinese government's regulation; hand-sanitising, as defined by sanitising one's hands with a sanitiser with >75% alcohol on the day of the survey, as per the Chinese government's regulation."

3. If the authors are going to mention graphs of data distribution (e.g., on page 8 regarding age) then those graphs should be included in the text.

We have included these graphs (labelled Figure 1, Figure 2, Figure 3), and they have been uploaded separately under "Images" due to BMJ's figure submission requirements. We have linked these respective figures to their respective texts on page 8 to ensure that they will be included in the text.

I thank the authors for their good work and careful consideration of our comments and concerns!

We thank the reviewer for his time and comments.

Reviewer: 2

Comments to the Author

This is an important and useful study. Problems with earlier drafts have largely been solved. It is now much easier to understand what was done.

Minor points:

1) On p. 5, a numeric variable called 'Overall compliance' is defined but it doesn't seem to be used in the analysis (perhaps this was at one time used as the dependent variable in a linear regression?). We thank the reviewer for pointing out the excessive paragraph. We have now removed it from the main body.

2) On p. 8, it is stated that 'Compliance with home quarantine was positively associated with compliance with handsanitising ... but not with mask-wearing and temperature-taking ( $p=0.07$  and  $0.08$ , respectively)'. The p-values suggest that there may have been a non-significant association, so it would be good to know whether it was positive or negative.

The associations were in the positive direction. We have added it in the text on page 7: "...but not with mask-wearing and temperature-taking ( $p=0.07$  and  $0.08$  in the positive direction, respectively)"

3) I didn't notice this previously, but I was struck by the fact that the timeliness variable never had a significant effect. The authors could consider adding a comment on this.

We have added a comment on page 10: "Nevertheless, timeliness of policy implementation at provincial level did not have a significant impact on behavioural compliance in the present study. A plausible explanation could be that policy implementation was launched in a prompt manner, according to the outbreak spreading speed in each province. It is worth noting that most provinces announced and implemented the COVID policy within 15 days since the lockdown of Wuhan city.<sup>1</sup> Such equally speedy reaction at the governmental level may be the reason why there was no difference of policy implementation on personal protective behaviours among community dwellers in China."

Possibly more important point:

Currently, only odds ratios with 95% confidence intervals are reported for the logistic regression model, with a graphical indication of significance (use of bold type for p values below a certain threshold). I would usually expect to see regression models reported in more detail (i.e. with intercepts, and with the coefficient estimates, standard errors, and confidence intervals from which the odds ratios were derived, and also with actual p values). However, nobody ever really does anything with that extra detail, and it may be that this form of reporting is acceptable for this particular journal, so I leave it to the editor to decide whether the form of reporting used by the authors should be considered adequate.

We appreciate the reviewer's suggestion. We have included 4 complete tables for the logistic regression analyses as Supplemental Material, and have indicated it on page 8:

"See Supplemental Material 2 for complete regression model output. "

#### VERSION 4 – REVIEW

|                        |                                    |
|------------------------|------------------------------------|
| <b>REVIEWER</b>        | Jeffrey Lees<br>Clemson University |
| <b>REVIEW RETURNED</b> | 04-Dec-2020                        |



|                         |   |
|-------------------------|---|
| <b>GENERAL COMMENTS</b> | The manuscript is satisfactory in its current form. However, please note that I am unable to see through the Manuscript Central system any of the Figures the authors included in response to my previous comments, and therefore am unable to assess them. |
|-------------------------|---|

|                 |   |
|-----------------|---|
| <b>REVIEWER</b> | Daniel Allington<br>King's College London |
|-----------------|---|

|                        |             |
|------------------------|-------------|
| <b>REVIEW RETURNED</b> | 09-Dec-2020 |
|------------------------|-------------|

|                         |   |
|-------------------------|---|
| <b>GENERAL COMMENTS</b> | This is a very useful piece of research. I look forward to seeing it published. |
|-------------------------|---|