PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Cross-sectional Study Examining the Status of Intrinsic Capacity Decline in Community-dwelling Older Adults in China: Prevalence, Associated Factors, and Implications for Clinical Care
AUTHORS	Ma, Lina; Chhetri, Jagadish K; Zhang, Li; Sun, Fei; Li, Yun; Tang, Zhe

VERSION 1 – REVIEW

REVIEWER	Kelly Virecoulon Giudici Institute of Aging, Gerontopole, Toulouse University Hospital, Université Toulouse III Paul Sabatier.
REVIEW RETURNED	21-Sep-2020

GENERAL COMMENTS	The manuscript “Status of Intrinsic Capacity Decline in China: Prevalence, Associated Factors, and Implications for Clinical Care” measured intrinsic capacity among a representative sample of Chinese community-dwelling older adults. English use and writing are generally adequate, but a final revision before resubmission is encouraged in order to correct some typos. One major concern that demanding attention of the authors comprises how some references are cited in sentences where they seem not to be corresponding to the content. Specific comments and suggestions are presented below. 1. Abstract - For easier comprehension, I suggest using “IC decline” instead of “DIC” over the entire manuscript. - Objectives: Please briefly mention the population of the study. - Results: cross-sectional analyses do not allow inferring any causal relationship. Please avoid using terms that could indicate causality, as in line 33-34: “(…) and osteoarthritis independently influenced DIC.” - Conclusion: the sentence “DIC prevalence in China is high” should only be kept if the study was performed with a population-based nationally representative sample. Since this is the case (as the reader can find out by reading the text), please indicate this fact when presenting participants (lines 23-26). 2. Introduction - Lines 74-75: Authors affirm that “few studies have focused on the combined components” of IC and mention only two references. However, other publications have also evaluated combined components of IC, as: Huang et al. (J Am Med Dir Assoc 2020;S1525-8610:30570-3); Daskalopoulou et al. (BMC Med Res Methodol 2019; 19:225); Giudici et al. (Exp Gerontol. 2019; 127:110716); Giudici et al. (Maturitas 2020;141:39-45);

	Gutiérrez-Robledo et al. (J Nutr Health Aging. 2019;23:788-795). - Lines 84-85: After mentioning that “few studies have focused on the combined components” of IC, authors say “Although the concept of IC has been used in several observational studies 11 (…)” – However reference 11 (Barreto et al., 2020) does not correspond to this affirmation. Please revise the sentence. 3. Methods - Line 95: “Further details regarding the CCGAS have been reported.” – Please provide a reference to this sentence. - Line 112: Please provide minimum and maximum score for SPPB. - Lines 127-129: What is the justification to choose BMI as the measure of vitality? Please provide some background to justify this choice. - Authors considered low BMI of a signal of IC decline, however what about very high BMI status (obesity)? Is there any evidence to support that participants with obesity (a well-known condition affecting functional abilities and several metabolic conditions) should be considered as presenting “intact vitality” (i.e. scoring 2 points)? - Line 128: Please provide a reference for the Asian BMI cutoffs. - Lines 138-143: Please provide minimum and maximum score for GDS. - Please explain how weighted percentages were determined. 4. Results - Line 169: Please provide the p-value in: “The prevalence of DIC was higher in women than in men (43.0% vs. 36.7%).” - Line 170: Please provide the p-value for “by 1.36 times”. - Please provide p-values for all results. - Line 181: please clearly indicate what are the categories related to the numbers given in the parentheses. 5. Discussion - Lines 218-220: Please avoid repeating numerical results in the Discussion. Authors can give a more general summary, if needed, as: “The frequency of participants with decline in IC domains varied from 11.1% (cognition) to 17.8% (locomotion).” - Lines 232-233: “We found that IC score decreased with increasing age” – This sentence induces the reader to wrongly infer causality, as this was a finding from a longitudinal analysis, which was not the case. Please revise to make it clear that you are reporting cross-sectional associations. Line 237: “(…) were independent factors influencing DIC (…)” and line 241: “Since the above associated factors independently affected the persons’ IC (…)” – my previous comment also applies here. Please avoid inferring causality. - Please cautiously revise the entire manuscript to be sure that no terms inappropriately infer causality for your results. - Lines 247-248: Reference 22 (Whiley et al., 2014) does not seem to correspond to the information that “IC predicted the incidence of loss of ADLs and IADLs”. Please carefully revise this sentence and also check if all other references are correctly cited in the manuscript. - Lines 251-252: I am not sure that reference 11 (Barreto et al., 2020) supports the thought that “IC may be considered as an evolution of frailty”. IC and frailty are different concepts and IC was not developed to be an evolution of frailty. Please revise.

	- Lines 269-277: The Conclusion paragraph should also present a concise answer to the objectives of the study. Please revise.

REVIEWER	Finbarr Martin King's College London, UK None declared with authors I am a member of the WHO Clinical Consortium on Healthy Ageing, which supports various work to do with the WHO ageing work including the construct of Intrinsic Ageing which is the focus of this paper.
REVIEW RETURNED	28-Sep-2020

GENERAL COMMENTS	General points on the main messages of this paper This is an interesting paper examining the use of the fairly new concept of Intrinsic Capacity, developed by the WHO. It describes its operationalisation using pre-existing survey data correlation with various relevant health states to seek associations. To this extent it is to some extent a validity study of the Intrinsic Capacity (IC) concept, although the authors to do present it as either a feasibility, or a validity study. Whilst neither of these aspects were explicit objectives of the study, it might be worth mentioning them in the Discussion. Overall, the paper is written clearly and makes reasonable conclusions. I recommend checking your report with recommendations of the with STROBE checklist for cross sectional studies. Specifically points 9 (Describe any efforts to address potential sources of bias) and 10 (Explain how the study size was arrived at) may need expansion. Some specific points: the term “decreased intrinsic capacity (DIC)” is problematic. It is likely that most people will have at some earlier point in their lives the highest score the authors presented (10/10) but this is not certain and so the suggestion of a decrease is not justified. There may be preferable term? There are however some suggestions made in the Discussion section which may be reasonable opinions of the authors but are not direct inferences from the findings. Specifically, Line 248 “…further indicates the need for worldwide implementation of prevention of DIC” – This may be a good idea but is not demonstrated by this study. A reasonable suggestion might be that evaluations of implementation using IC as a focus may be worthwhile. Line 256 “cross-sectional design limit generalizability”. Perhaps its main limitation is not being able to conclude causality. Line 273 “Consistent with the WHO recommendations, this study indicates screening and interventions should be provided, especially for vulnerable participants” Again, may be a good idea, but this study does not indicate that screening using IC would lead to benefits. So, this should be presented as a suggestion not a conclusion. Introduction and background – This is relevant, precise, and clear with adequate references. Methods- Sample-OK but we need a reference for the China Comprehensive Geriatric Assessment Study (CCGAS, 2011–2012) Methods-Assessors Do the authors have any data on the inter-

	rater reliability? Methods-Demographic/lifestyle etc: the following categories are not defined and therefore could not be adequately reproduced: low income, no exercise, less meat intake. The term “poor marital status” is unscientific and perjorative and should be replaced by “unmarried” Methods-Measure of IC Most are based on the WHO assessment guidance (with the reference). The limitation f BMI for vitality/nutrition is acknowledged. The definition of DIC based on the GDS score is not clear. Self-report for hearing impairment is not recommended method from WHO so should be mentioned as a small limitation. It may mis-estimate hearing loss. Methods-clinical “outcomes” The 0.25 threshold for FI is higher than generally recommended by Rockwood and colleagues. Methods – missing data “A total of 6,867 older adults were included, and 1,040 participants were excluded because of missing data, resulting in 5,823 with complete IC data included in this analysis” The authors present no comparison of basic characteristics to assess potential bias. Methods-Analyses Was there any a-priori significance value of bivariate associations for inclusion into the Logistic regression? Results The average IC score was 9.14±1.304- but skewed: therefore, I suggest present as median and range The reported prevalence of falls is very low, Is there any comparative data from China which would suggest this is reliable?

VERSION 1 – AUTHOR RESPONSE

Replies to Reviewer 1

Reviewer: 1
Comments to the Author
The manuscript “Status of Intrinsic Capacity Decline in China: Prevalence, Associated Factors, and Implications for Clinical Care” measured intrinsic capacity among a representative sample of Chinese community-dwelling older adults. English use and writing are generally adequate, but a final revision before resubmission is encouraged in order to correct some typos. One major concern that demanding attention of the authors comprises how some references are cited in sentences where they seem not to be corresponding to the content. Specific comments and suggestions are presented below.
1. Abstract
- For easier comprehension, I suggest using “IC decline” instead of “DIC” over the entire manuscript.
Response: Thank you for your comment. The “DIC” was replaced with “IC decline’ in the revised version.

- Objectives: Please briefly mention the population of the study.
Response: Thank you for your comment. We have added the population of the study in the objectives. “… among community-dwelling older adults in China.”

- Results: cross-sectional analyses do not allow inferring any causal relationship. Please avoid using terms that could indicate causality, as in line 33-34: “(…) and osteoarthritis independently influenced DIC.”
Response: Thank you for your comment. We have revised in the entire manuscript.

“Age, northern residence, low education, being unmarried, low income, less exercise, less meat intake, insomnia, memory loss, urinary incontinence, constipation, slowness, chronic obstructive pulmonary disease, and osteoarthritis were related to IC decline.”

- Conclusion: the sentence “DIC prevalence in China is high” should only be kept if the study was performed with a population-based nationally representative sample. Since this is the case (as the reader can find out by reading the text), please indicate this fact when presenting participants (lines 23-26).
Response: Thank you for your comment. We have added the description of the study in the participants.

2. Introduction
- Lines 74-75: Authors affirm that “few studies have focused on the combined components” of IC and mention only two references. However, other publications have also evaluated combined components of IC, as:
Huang et al. (J Am Med Dir Assoc 2020;S1525-8610:30570-3);
Daskalopoulou et al. (BMC Med Res Methodol 2019; 19:225);
Giudici et al. (Exp Gerontol. 2019; 127:110716);
Giudici et al. (Maturitas 2020;141:39-45);
Gutiérrez-Robledo et al. (J Nutr Health Aging. 2019;23:788-795).
Response: Thank you for pointing out these literatures which we might have missed earlier. We have added the above references in the revised version.
“Since each component is associated with adverse health events in older people recent studies have focused on these components in combination 6,7,16,8–15.”

- Lines 84-85: After mentioning that “few studies have focused on the combined components” of IC, authors say “Although the concept of IC has been used in several observational studies 11 (…)” – However reference 11 (Barreto et al., 2020) does not correspond to this affirmation. Please revise the sentence.
Response: We have corrected our mistake in the revised version.

3. Methods
- Line 95: “Further details regarding the CCGAS have been reported.” – Please provide a reference to this sentence.
Response: We have added the references.
“Further details regarding the CCGAS have been reported 19–21.”

- Line 112: Please provide minimum and maximum score for SPPB.
Response: The total score range of the short physical performance battery was 0-5, which we have added in the revised manuscript.

- Lines 127-129: What is the justification to choose BMI as the measure of vitality? Please provide some background to justify this choice.
Response: Thank you for your comment. In the ICOPE: guidance for person-centered assessment and pathways in primary care, it was recommended to use weight loss and loss of appetite to screen vitality and nutrition assessment tools to assess vitality. Unfortunately, we don’t have the above items in the CCGAS cohort.
As BMI is an important item in nutrition assessment tools, furthermore, some literatures use other variables such as BMI(1,2), grip strength(1,3), MNA(2), abdominal circumference(2), loss of appetite and weight loss (4) to assess vitality, thus we choose to use BMI to measure vitality.
We have acknowledged it as a limitation of this study in the discussion: “vitality was defined by BMI instead of specific nutritional assessment, which may have affected some of the results. Since most of

the diabetic patients in our study had a higher BMI, this could explain the lower IC decline in participants with diabetes.."
References:
1. Gutiérrez-Robledo LM, García-Chanes RE, Pérez-Zepeda MU. Allostatic Load as a Biological Substrate to Intrinsic Capacity: A Secondary Analysis of CRELES. J Nutr Heal Aging 2019;23(9):788–95.
2. Alexia Charles, Fanny Buckinx, Médéa Locquet, Jean-Yves Reginster, Jean Petermans, Bastien Gruslin OB. Prediction of Adverse Outcomes in Nursing Home Residents According to Intrinsic Capacity Proposed by the World Health Organization | The Journals of Gerontology: Series A | Oxford Academic. J Gerontol A Biol Sci Med Sci 2019
3. Giudici KV, de Souto Barreto P, Guerville F, et al. Associations of C-reactive protein and homocysteine concentrations with the impairment of intrinsic capacity domains over a 5-year follow-up among community-dwelling older adults at risk of cognitive decline (MAPT Study). Exp Gerontol 2019;127.
4. Ramírez-Vélez R, Correa-Bautista JE, García-Hermoso A, Cano CA, Izquierdo M. Reference values for handgrip strength and their association with intrinsic capacity domains among older adults. J Cachexia Sarcopenia Muscle 2019;10(2):278–86.

- Authors considered low BMI of a signal of IC decline, however what about very high BMI status (obesity)? Is there any evidence to support that participants with obesity (a well-known condition affecting functional abilities and several metabolic conditions) should be considered as presenting "intact vitality" (i.e. scoring 2 points)?
Response: Thank you for your comment. BMI cutoffs were based on Malnutrition Universal Screening Tool ('MUST'). We agree with the reviewer that obesity condition affects the functional abilities and is associated with worse outcomes. However, in the "Integrated care for older people (ICOPE): guidance for person-centred assessment and pathways in primary care." Released by WHO, the vitality domain is used to assess the nutritional status, and several tools are recommended to assess nutrition including MNA, DETEMINE nutrition risk assessment, MUST, and SNAQ. In this study, the BMI cutoffs were based on MUST tool which may be considered as a major of vitality.

- Line 128: Please provide a reference for the Asian BMI cutoffs.
Response: BMI cutoffs were based on Malnutrition Universal Screening Tool ('MUST').
Elia M. Screening for malnutrition: A multidisciplinary responsibility. Development and Use of the Malnutrition Universal Screening Tool ('MUST') for Adults. Redditch: BAPEN; 2003.

- Lines 138-143: Please provide minimum and maximum score for GDS.
Response: Thank you for your comment. We used GDS-30 items scale, hence, the total score range of GDS was 0-30.

- Please explain how weighted percentages were determined.
Response: Thank you for your comment. The weighted percentages were determined using the national standard population composition ratio as at the Sixth National Census (2010).

4. Results
- Line 169: Please provide the p-value in: "The prevalence of DIC was higher in women than in men (43.0% vs. 36.7%)."
- Line 170: Please provide the p-value for "by 1.36 times".
- Please provide p-values for all results.
Response: We have provided P values for all the results.

- Line 181: please clearly indicate what are the categories related to the numbers given in the parentheses.

Response: We have added the categories in the revised version.

5. Discussion
- Lines 218-220: Please avoid repeating numerical results in the Discussion. Authors can give a more general summary, if needed, as: “The frequency of participants with decline in IC domains varied from 11.1% (cognition) to 17.8% (locomotion).”
Response: Thank you for pointing out our error. We have made corrections.

- Lines 232-233: “We found that IC score decreased with increasing age” – This sentence induces the reader to wrongly infer causality, as this was a finding from a longitudinal analysis, which was not the case. Please revise to make it clear that you are reporting cross-sectional associations.
Response: We have made corrections.

Line 237: “(…) were independent factors influencing DIC (…)” and line 241: “Since the above associated factors independently affected the persons’ IC (…)” – my previous comment also applies here. Please avoid inferring causality.
- Please cautiously revise the entire manuscript to be sure that no terms inappropriately infer causality for your results.
Response: We have revised the manuscript and made corrections accordingly.

- Lines 247-248: Reference 22 (Whiley et al., 2014) does not seem to correspond to the information that “IC predicted the incidence of loss of ADLs and IADLs”. Please carefully revise this sentence and also check if all other references are correctly cited in the manuscript.
Response: We apologize for the error. We have revised the reference in the new version.
Beard JR, Jotheeswaran AT, Cesari M, Araujo De Carvalho I. The structure and predictive value of intrinsic capacity in a longitudinal study of ageing. BMJ Open 2019;9(11):e026119.

- Lines 251-252: I am not sure that reference 11 (Barreto et al., 2020) supports the thought that “IC may be considered as an evolution of frailty”. IC and frailty are different concepts and IC was not developed to be an evolution of frailty. Please revise.
Response: Thanks so much for your constructive suggestion. The reference of Barreto et al., 2020 supports the sentence that “IC creates a bridge between geroscience and healthy aging”. The original sentence in Barreto’s paper: “The intrinsic capacity framework: creating the bridge between Gerosciences and healthy aging” (Page 2, line 10-11).
IC may be considered as an evolution of frailty, it was cited from Drs. Giulia Belloni and Matteo Cesari’s paper entitled “Frailty and Intrinsic Capacity: Two Distinct but Related Constructs” (https://www.frontiersin.org/articles/10.3389/fmed.2019.00133/full). We have cited this reference in the revised manuscript. The original paragraph in Drs. Giulia Belloni and Matteo Cesari’s paper: “Under certain aspects, IC might be considered as a sort of evolution of frailty. The development of frailty into something “new” is motivated by multiple reasons as: (1) the need of disseminating the comprehensive approach to the older person beyond the perimeter of geriatric medicine (even in countries where geriatricians are relatively absent), (2) the necessity to provide a positive connotation to the aging phenomenon (thus focusing on functions rather than on deficits), (3) the importance of working on trajectories instead of focusing on arguable cross-sectional cut-points, and (4) the attempt to anticipate as much as possible the self-empowerment of the individual for his/her health status (thus supporting preventive strategies in the community).”

- Lines 269-277: The Conclusion paragraph should also present a concise answer to the objectives of the study. Please revise.
Response: Thanks so much for your constructive suggestion. We have revised the conclusion paragraph.

Replies to Reviewer 2

Reviewer: 2
Comments to the Author
General points on the main messages of this paper
This is an interesting paper examining the use of the fairly new concept of Intrinsic Capacity, developed by the WHO. It describes its operationalisation using pre-existing survey data correlation with various relevant health states to seek associations. To this extent it is to some extent a validity study of the Intrinsic Capacity (IC) concept, although the authors to do present it as either a feasibility, or a validity study. Whilst neither of these aspects were explicit objectives of the study, it might be worth mentioning them in the Discussion.
Response: Thank you so much for your constructive suggestion. We have added a sentence “This study also validated the IC concept in a Chinese population” in the conclusion.

Overall, the paper is written clearly and makes reasonable conclusions. I recommend checking your report with recommendations of the with STROBE checklist for cross sectional studies.
Specifically points 9 (Describe any efforts to address potential sources of bias) and 10 (Explain how the study size was arrived at) may need expansion.
Response: Thanks so much for your valuable comments. We have checked the manuscript with the STROBE checklist and added some methods.

Some specific points: the term “decreased intrinsic capacity (DIC)” is problematic. It is likely that most people will have at some earlier point in their lives the highest score the authors presented (10/10) but this is not certain and so the suggestion of a decrease is not justified. There may be preferable term?
Response: Thanks so much for your comments. We used the term “IC decline” instead of “decreased intrinsic capacity (DIC)” in the new version.

There are however some suggestions made in the Discussion section which may be reasonable opinions of the authors but are not direct inferences from the findings.
Specifically,
Line 248 “…further indicates the need for worldwide implementation of prevention of DIC” – This may be a good idea but is not demonstrated by this study. A reasonable suggestion might be that evaluations of implementation using IC as a focus may be worthwhile.
Response: We understand the reviewers point and have revised it in the new version.

Line 256 “cross-sectional design limit generalizability”.
Perhaps its main limitation is not being able to conclude causality.
Response: Yes, indeed. We have revised it in the new version.
“First, the primary limitation is being unable to establish causality due to the cross-sectional design. Further longitudinal studies with a larger sample size are urgently required.”

Line 273 “Consistent with the WHO recommendations, this study indicates screening and interventions should be provided, especially for vulnerable participants”
Again, may be a good idea, but this study does not indicate that screening using IC would lead to benefits. So, this should be presented as a suggestion not a conclusion.
Response: Thank you for your suggestion. We have made changes as suggested.

Introduction and background – This is relevant, precise, and clear with adequate references.
Response: Thank you.

Methods- Sample-OK but we need a reference for the China Comprehensive Geriatric Assessment Study (CCGAS, 2011–2012)

Response: We have added references for the CCGAS.

Methods-Assessors Do the authors have any data on the inter-rater reliability?
Response: We are sorry that we don't have any data on the inter-rater reliability.

Methods-Demographic/lifestyle etc: the following categories are not defined and therefore could not be adequately reproduced: low income, no exercise, less meat intake. The term "poor marital status" is unscientific and perjorative and should be replaced by "unmarried"
Response: We have revised according to your suggestions.

Methods-Measure of IC Most are based on the WHO assessment guidance (with the reference). The limitation f BMI for vitality/nutrition is acknowledged. The definition of DIC based on the GDS score is not clear. Self-report for hearing impairment is not recommended method from WHO so should be mentioned as a small limitation. It may mis-estimate hearing loss.
Response: Because we used pre-existing survey data, some items such as nutrition assessment and hearing assessment were not available. The psychological domain was assessed by GDS in previous studies (1,2). Some other studies used self-reported hearing impairment to assess hearing as well (1,4).
As BMI is an important item in nutrition assessment tools, furthermore, some literatures use other variables such as BMI(1,3), grip strength(1,2), MNA(3), abdominal circumference(3), loss of appetite and weight loss (4) to assess vitality, thus we choose to use BMI to measure vitality (again this approach is not our best choice, rather a proxy for vitality in the absence of specific measure).
We have addressed it as a limitation in this study in the discussion section: "vitality was defined by BMI instead of specific nutritional assessment, which may have affected some of the results. Since most of the diabetic patients in our study had a higher BMI, this could explain the lower IC decline in participants with diabetes.." "Fourth, self-reporting for hearing impairment may misestimate hearing loss (although WHO recommends whisper test for assessing hearing loss)."
References:
1.Gutiérrez-Robledo LM, García-Chanes RE, Pérez-Zepeda MU. Allostatic Load as a Biological Substrate to Intrinsic Capacity: A Secondary Analysis of CRELES. J Nutr Heal Aging 2019;23(9):788–95.
2.Giudici KV, de Souto Barreto P, Guerville F, et al. Associations of C-reactive protein and homocysteine concentrations with the impairment of intrinsic capacity domains over a 5-year follow-up among community-dwelling older adults at risk of cognitive decline (MAPT Study). Exp Gerontol 2019;127.
3.Alexia Charles, Fanny Buckinx, Médéa Locquet, Jean-Yves Reginster, Jean Petermans, Bastien Gruslin OB. Prediction of Adverse Outcomes in Nursing Home Residents According to Intrinsic Capacity Proposed by the World Health Organization | The Journals of Gerontology: Series A | Oxford Academic. J Gerontol A Biol Sci Med Sci 2019
4.Ramírez-Vélez R, Correa-Bautista JE, García-Hermoso A, Cano CA, Izquierdo M. Reference values for handgrip strength and their association with intrinsic capacity domains among older adults. J Cachexia Sarcopenia Muscle 2019;10(2):278–86.

Methods-clinical "outcomes" The 0.25 threshold for FI is higher than generally recommended by Rockwood and colleagues.
Response: Thanks so much for your comment. The 0.25 threshold for FI was cited from Rockwood(1,2), we have added the reference in the new version.
1. Kenneth Rockwood, Melissa Andrew, Arnold Mitnitski. A comparison of two approaches to measuring frailty in elderly people. J Gerontol A Biol Sci Med Sci. 2007 Jul;62(7):738-43.
2.Song X, Mitnitski A, Rockwood K. Prevalence and 10-year outcomes of frailty in older adults in relation to deficit accumulation. J Am Geriatr Soc. Apr;58(4):681–7.

Methods – missing data “A total of 6,867 older adults were included, and 1,040 participants were excluded because of missing data, resulting in 5,823 with complete IC data included in this analysis” The authors present no comparison of basic characteristics to assess potential bias.
Response: Thanks so much for your comment. We have added the supplementary material - Table S1 to compare the included participants and excluded participants. The excluded participants were older and frail, had more chronic diseases, and lower scores in each domain, we have stressed it as a limitation in the discussion section.

Methods-Analyses Was there any a-priori significance value of bivariate associations for inclusion into the Logistic regression?
Response: We have added a sentence in the statistical analysis section: “Those with a P-value less than 0.05 were included in the multivariable model.”.

Results
The average IC score was 9.14±1.304- but skewed: therefore, I suggest present as median and range
Response: Thank you so much for your comment. We have revised in the revised manuscript: the median (IQR) score was 10(1).

The reported prevalence of falls is very low, Is there any comparative data from China which would suggest this is reliable?
Response: Thanks so much for your comment. A recent study conducted in long-term care facilities in China showed the incidence of falls was 13.5% (1). A systematic review showed that the incidence of self-reported falls was lower in Chinese older people than in Caucasian older people (2).
The reported prevalence of falls is very low, because all the participants were from community, and participants who could not complete IC assessment was excluded in the data analysis.
References:
1. Yu Jiang, Qinghua Xia, Peng Zhou, Shuo Jiang, Vinod K Diwan, Biao Xu. Falls and Fall-Related Consequences among Older People Living in Long-Term Care Facilities in a Megacity of China. Gerontology. 2020 Oct 6;1-9.
2. Marcella Mun-San Kwan, Jacqueline C T Close, Alfred Kwok Wai Wong, Stephen R Lord. Falls incidence, risk factors, and consequences in Chinese older people: a systematic review. J Am Geriatr Soc. 2011 Mar;59(3):536-43.

VERSION 2 – REVIEW

REVIEWER	Finbarr Martin King's College London, UK
REVIEW RETURNED	09-Dec-2020

GENERAL COMMENTS	Thank you for the extensive revisions to this manuscript. I think it now reads clearly and is much improved.