PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Association between adverse childhood experiences and adult diseases in older adults: A comparative cross-sectional study in Japan and Finland
AUTHORS	Amemiya, Airi; Fujiwara, Takeo; Shirai, Kokoro; Kondo, Katsunori; Oksanen, Tuula; Pentti, Jaana; Vahtera, Jussi

VERSION 1 - REVIEW

REVIEWER	Cynthia Levine Northwestern University United States
REVIEW RETURNED	05-Jul-2018

GENERAL COMMENTS	Thank you for the opportunity to review this manuscript, "Association between adverse childhood experiences and adult diseases in older adults: A comparative cross-sectional study in Japan and Finland." The manuscript examines the relationship between adverse experiences in childhood and multiple health outcomes in older adulthood (self-rated health, health behaviors, and diagnosis of chronic diseases of aging) in samples of Japanese and Finnish adults. I appreciated the use of samples from two different countries and the use of multiple different health outcomes. I have the following questions about the manuscript that could be addressed in a revision: 1. The introduction could have done more to situate the current research in the past literature (i.e., to explain how the current research was the same or different from past work). Specifically, the introduction cites multiple studies that looked at the relationship between adverse childhood experiences and adult health. I appreciated the description of how Japan and Finland were the same as and different from each other, but I thought it would also be helpful to describe how/whether each was similar to or different from countries where this work had been done in the past. 2. The Japanese participants were not eligible to receive benefits from public long term care insurance services. I'm not sure who in Japan would be eligible for such services, but does this mean that the sample is largely from higher or lower socioeconomic backgrounds? How might this affect the results? 3. Were the analyses conducted separately for Japan and Finland (i.e., separate regression analyses run for each country)? If so,

	why not include both samples in the same analyses and then test the ACE x country interaction predicting each health outcome? The discussion makes the claim that the pattern of results and strength of associations were similar across countries, but the way to formally test that is to test this interaction? 4. Related to point 3, if the analyses are run separately for each country, there does not seem to be quite as much of a need to make the measures exactly equivalent across countries (since, after all, the questions were asked differently in many cases, which is understandable). It seems that it might be helpful to at least include some sensitivity analyses to test whether the results hold up if the variables are left in their original form (e.g., self-rated health left as a linear scale, rather than dichotomized). 5. The items that assessed fear of family seemed especially different in the two countries (fear vs. witnessing abuse). I appreciate that this is going to happen when researchers draw from two large datasets designed by different people, but it seems worth at least acknowleding this as a limitation in the discussion. Minor points: 1. The introduction mentions the gini coefficient. This is a widely used statistic, but it might be helpful to explain what it is briefly, just in case some readers are unfamiliar iwth it. 2. 1/5 of the Japanese sample were asked about ACEs. Was this 1/5 of the sample randomly chosen, or were they selected based on some other criterion?

REVIEWER	Dr Kat Ford; Hannah Grey Bangor University, Wales
REVIEW RETURNED	27-Jul-2018

GENERAL COMMENTS	Dear Authors, Thank you for the opportunity to review this manuscript. The paper presents a unique comparison of ACE prevalence across countries, however, we have highlighted a number of issues which we think the manuscript needs to address. Abstract The abstract needs clarity, such as line 22 - authors do not state that the 10,353 participants are from Finland. Further, lines 28 & 43 - the authors do not define the category 'number of ACEs' and therefore it is not clear what this refers to, for example, any number of ACEs or ACE count i.e. 2 to 3 ACEs? Authors should be consistent throughout the abstract and manuscript when reporting findings in the text and use of percentages i.e. Line 40: state what 'half' refers to (i.e. 50.0%). The results section of the abstract does not state what the reference categories used in the logistic regressions are, nor do they report the significance values. Further the abstract should be strengthened to reflect other comments within the discussion section on the paper.

	The current reported strengths and limitations are neither. For example, the authors state they have used data from two studies – it is not clear whether this is a strength or a limitation (and why). Further this should emphasize why the paper is unique i.e. in providing a comparison between two countries. Introduction The authors should aim to avoid language that is overly causative when discussing the associations that exist between ACEs and unhealthy behaviours (line 14) and further in the paper. The authors state that it is not known whether ACEs and their impact on health vary by country (line 23). A number of studies have explored ACEs within and across different countries (for example, Bellis et al., 2014; Hughes et al., 2017). However, the comparison between Japan and Finland, and looking at older adults in particular, is unique and should be emphasised more. The comparisons between Japan and Finland could be strengthened by providing more information on the cultural and social differences/similarities between the two countries, e.g., how much does equality differ within countries? Are there different levels of im/migration in the two countries? I would also like to see information on ACE prevalence and the prevalence of childhood maltreatment or separation and divorce in Japan and Finland as other studies have identified. There needs to be more onus on why the manuscript is new and original. Methods The methods section lacks clarity on some issues, which makes it difficult to understand. We feel the paper would be greatly improved by adding clarity on the following: • It is stated the data are from surveys conducted among older adults – define age range, average age etc. The final sample size, and sample sizes of both countries as well as demographic information regarding gender, age, employment status should be clearly reported within the findings. • Significant differences (if any) across study populations (could be presented in table one). No discussion is given to whether the characteristics of the sample is representative of the countries. • Information on how the data from the studies were derived. It is assumed that the study is a secondary analysis of an existing dataset but this is not overtly mentioned and the data collection methods are not fully explained e.g. face to face, postal, telephone surveys, self-completion, etc. • The numbers of individuals excluded from analysis at each point should be clarified (line 54; page 6). • Discussion on how missing data was handled i.e. some individuals were excluded, some included in analysis, some dummy? Why not exclude all who were unable to provide full data for each variable used in the analysis – this is a limitation and should be recognised as such. (e.g. page 9, line 17 states that no, yes and cannot say were all coded as missing. Line 28 states that missing data were coded as not having a specific ACE). It should be considered that if a response is missing to an ACE variable then that should be excluded from analyses, as the ACE could have occurred but not have been reported. Another example of missing data being treated differently is given on page 11, line 14. • Limitations of methods: The JAGES study collected data from individuals who were not eligible to receive benefits from

	public long-term care insurance providers. Would excluding this group bias the sample in any way? • I have concerns around the ethical approval for the research. The authors report that ethics was approved for the original studies; but does this also include the use of the data for secondary analysis. If this study is indeed a secondary analysis of an existing dataset, then was further ethical approval required/sought to conduct the combined analysis present in the current study. The paper does not discuss other ethical considerations which I would expect to be incorporated such as how the data were stored, anonymity and confidentiality of personal information. • There is a (recognised) disparity in how ACEs were measured across studies. It is not evident to the reader, how they are different between studies/countries, and then how these have been recoded. It would be useful to the authors to present this as a table either within the paper itself or as supplementary material. For example – we would envisage this to contain: questions asked, response options and how this was coded as a positive ACE score. This is the same for measurement of health and health behaviours. Any differences between the surveys should be acknowledged within the limitations section of the paper and including any implications for interpretation of the results. • Fear of a family member; witnessing domestic abuse; and physical abuse have seemingly been coded into one variable. The authors should state why they did this, as these categories could be quite distinct. Explanation is needed on whether physical abuse relate to physical abuse between parents or abuse against the child (line 29). As per previous comment it would be advantageous to see (in text or as supplementary material) how the questions were phrased/asked in order to increase repeatability of the study and explore how valid the questions used were. • Concern is raised with regards to the summary variable called 'number of ACEs'. Line 29 states that this included 0-3 ACEs, meaning the analyses would have included those who do not have any adversities with those who have all 3. This may be an error and might intend to mean 1 to 3 ACEs but it is not clear from the text or the results what 'number of ACEs' refers to. ACE literature generally categorises ACEs in 0, 1, 2-3 and □4 ACEs. There is no discussion of how these are grouped, justifications for grouping or reference to literature on cumulative adversity within this section. • Page 10, Line 20 onwards: It would be useful to have more information on how information was derived from the National Cancer Registry and the Social Insurance Institution of Finland. There is ambiguity surrounding how this data was collected from a separate source. Covariates As noted above, a discussion of other potential covariates that could potentially be included would enhance the paper. Further to this, more thought and discussion is needed into the grouping of variables e.g. 'retired and never worked'. Results The tables could be enhanced with clearer headings and a thorough proof reading to check the footnotes e.g. AOR's not ORs or RR. It is assumed that figures in bold in the tables are significant, but this is not stated anywhere in the paper and could be made clearer, as could the level of significance (e.g. $p<0.05$,

	p<0.001 etc). It is also not clear what the reference category being used in each of the analyses are. Discussion • It is felt that the discussion would benefit from a change in the structure, which would provide more clarity and transparency whilst giving more credence to the unique aspects of the study. More information is needed on the contribution of each ACE and cumulative ACEs to health and behaviours between and within each country to enhance the comparative aspects of the study. • Whist interesting, the discussion relating to World War 2 does not feel appropriate or in keeping with the rest of the text. Further to this, no references are provided to back-up the claims made about the effect of World War 2. • Survival bias is a key limitation of this unique study due to the elderly sample, this should be discussed in more detail. • The ACE prevalence and odds ratios listed are low compared to other studies. In an international meta-analysis, Hughes et al (2017) found a moderate odds ratio for smoking (2.82, 95% CI 2.38–3.34) in people with four or more ACEs compared to those with no ACEs. The authors should be mindful of not overstating their results, as the odds ratios found are relatively weak. There could also be discussion here that the prevalence within these results are low due to only measuring 3 ACEs in the first instance (see later comments about transparency of only measuring 3 ACEs). The concluding remarks need to clearly reiterate the unique contribution that this paper makes, and the potential impact or consequences that these findings could present. There are a number of limitations that could be included, for example: • Potential confounding factors that are not included in the study which could have contributed to health and health behaviours, such as levels of deprivation/inequality, ethnicity. • The authors mention in the discussion that ACE literature uses a number of other ACEs; this is quite a distinguishable difference and more attention needs to be paid to this point. • Many of the variables were self-reported. Associated problems of accuracy are not discussed or raised as a limitation. There are a few spelling mistakes as well as discrepancies in referencing within the text and therefore a thorough proof read is required. Suggested references to include: Bellis, M. A., Hughes, K., Leckenby, N., Jones, L., Baban, A., Kachaeva, M., ... & Raleva, M. (2014). Adverse childhood experiences and associations with health-harming behaviours in young adults: surveys in eight eastern European countries. Bulletin of the World Health Organization, 92, 641-655. Hughes, K., Bellis, M. A., Hardcastle, K. A., Sethi, D., Butchart, A., Mikton, C., ... & Dunne, M. P. (2017). The effect of multiple adverse childhood experiences on health: a systematic review and meta-analysis. The Lancet Public Health, 2(8), e356-e366.

REVIEWER	Katie Hardcastle Senior Public Health Researcher Public Health Wales Wales UK
REVIEW RETURNED	30-Jul-2018

GENERAL COMMENTS	Thank you very much for the opportunity to review this manuscript. Whilst the overall premise of the paper has potential in offering a cross-cultural comparison of ACEs in a less well-studied cohort (older adults), the submission is limited by inappropriate statistical analyses and a concerning lack of transparency with regards to its methodology. It is my view that the authors have not captured or subsequently built upon the most up to date literature regarding ACEs (many of the given references are not appropriate), health impacts and risk and resilience factors. Therefore it is unclear what this paper, which only reflects relatively few ACE measures, adds to this ever expanding evidence base at this time. The below comments are intended to offer some explanation as to my decisions on the attached review checklist and hopefully further offer some suggestion as to how the authors may look to develop this work for a future submission. Research objectives (1): • The paper offers two independent examinations of the relationships between a small number of adverse childhood experiences and health outcomes. However, it does not sufficiently draw this data together to offer a comparison between the two countries (as proposed). Generally the authors do a reasonable job of outlining the ways in which the three data sets differ. But for me, the differences/inconsistencies outweigh the similarities and it is not clear why it was felt that these data sets could reasonably be brought together. Any discussion of the social and cultural environments in these two countries is very limited and does not adequately set the scene for any subsequent comparisons. Later we see there are differences in the prevalence of different ACEs across the two countries; however, the paper does not fully explore this. The decision to focus on older adults is important (based on the current field) but not explained. • The vast majority of ACEs literature considers a wider spectrum of ACEs. Is it appropriate to build this paper on such literature using only 3 ACEs? Methods (4-6): • The methods section of this manuscript is a particular area of concern. A paper like this would benefit considerably from a supplementary table that clearly outlines commonalities or how the studies differed. Authors need to be much more explicit about each of the following: sampling methods; response rates (of the original study; detailed for the age cohort selected); timing of the collection of different variables; methods of data collection (e.g. paper surveys, online, face-to-face, in the respondent's home or other location etc); processes for consent, withdrawal. • Are the samples representative? There are many unaddressed challenges. For example, the Finnish sample is heavily skewed towards female participants. Why were different ages used across the different studies? Prevalence of cancer seems very low (Table 1) and appears inconsistent with data from the Global Cancer Observatory or other sources. • The handling of some of the study variables appears a little haphazard. For example, for the JAGES data, a dichotomous yes/no response to the selected ACEs is described as 'frequent' fear in a family (with no measure of frequency). ACEs are not

	clearly defined, nor is the bounds of 'childhood' (i.e. what ages?). For self-rated health, fair is dichotomised to poor in the Japanese data, but 'moderate' is dichotomised to good in the Finnish data. Can this be justified or is there precedent? A 'history of being diagnosed with cancer' lacks detail (does this refer to a current diagnosis; how would this be reflected for people that were, for example, in remission). Smoking and working status also lack clarity. Analyses and presentation of results (7,9-10): • A very small proportion of the sample have all three of the ACEs analysed here, which has implications for analysis by number of ACEs. Alternative focus may be appropriate (any vs none) but at the very least this should be reflected in limitations. • A sample characteristics table is provided (Table 1) and the supporting text includes discussion of ways in which the two countries are 'similar'. It appears that there are considerable differences in demographics. However, the authors have not used any bi-variate statistics to consider these differences. • Crucially, it is unclear to me why the authors have chosen to run a series of separate models rather than: (a) including country as a variable in the model; and (b) including all covariates in one model. No p values are provided throughout. Discussion and limitations (11-12): • Throughout the paper, use of the term 'similar' is concerning with no supporting statistical significance. Generally the discussion lacks a response to that 'so what' question and introduces concepts such as the impact of WWII with no justification or thorough explanation of its relevance. Much of the discussion appears to be focused around resilience, but there is no reference to the vast emerging literature on resilience factors. Whilst tentative links are made such as with health service use, again theories and evidence for the impact of adversity on help seeking etc are not referenced. • Authors state that these results suggest ACEs have a 'remarkable' impact on health. However, ORs presented here are lower than reported elsewhere (e.g. Hughes et al SR in the Lancet). There should be a recognition and exploration of this. • There is a significant lack of detail for the limitations described. For example, it is not made clear at all why being derived from a highly developed country is a limitation. Many of the factors concerning the samples mentioned above should be reflected in the limitations. Other general comments: • Please note that the references appear out of sync in places. Generally the standard of English is acceptable, but there are inconsistencies and errors that have not been picked up during proof reading that would need attention.

VERSION 1 – AUTHOR RESPONSE

Reviewers' Comments to Author:

Reviewer: 1
Reviewer Name: Cynthia Levine
Institution and Country: Northwestern University, United States
Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors belowThank you for the opportunity to review this manuscript, "Association between adverse childhood experiences and adult diseases in older adults: A comparative cross-sectional study in Japan and Finland." The manuscript examines the relationship between adverse experiences in childhood and multiple health outcomes in older adulthood (self-rated health, health behaviors, and diagnosis of chronic diseases of aging) in samples of Japanese and Finnish adults. I appreciated the use of samples from two different countries and the use of multiple different health outcomes. I have the following questions about the manuscript that could be addressed in a revision:

1. The introduction could have done more to situate the current research in the past literature (i.e., to explain how the current research was the same or different from past work). Specifically, the introduction cites multiple studies that looked at the relationship between adverse childhood experiences and adult health. I appreciated the description of how Japan and Finland were the same as and different from each other, but I thought it would also be helpful to describe how/whether each was similar to or different from countries where this work had been done in the past.

Response: We appreciate the reviewer's comments. We agree that it is important to describe how and whether Japan and Finland were similar to or different from countries where the recent studies of adverse childhood experiences had been done in the past. We have added the following sentences:

"According to a systematic review, most of the recent studies evaluating the impact of multiple ACEs on health throughout life were performed in the United states (US) and the United Kingdom (UK), with only a few studies conducted in other countries.1 The US and the UK are members of the OECD, as are Japan and Finland. Japan, Finland, and the UK employ a universal healthcare system, whereas the US does not. In terms of inequality, of 37 OECD countries the US ranked 34th, and the UK ranked 30th (e.g., the Gini coefficient was 0.36 in UK [2015] and 0.39 in US [2015]).2 Thus, there is more income inequality exists in the US and the UK than in Japan and Finland." (p7)

We also added the following sentences to explain how the current study was different from the recent studies:

"Recent studies which investigated the effect of ACEs on health throughout life had some limitations. First, most studies were performed in the US and UK1, with few studies conducted in Asian 3or Nordic countries 4 5. Second, only one study focused on older adults6. Third, few studies compared the effects of ACEs in different countries1 7." (p8)

2. The Japanese participants were not eligible to receive benefits from public long term care insurance services. I'm not sure who in Japan would be eligible for such services, but does this mean that the sample is largely from higher or lower socioeconomic backgrounds? How might this affect the results?

Response: We appreciate the reviewer's questions. In Japan, all individuals with functional disabilities are eligible for long-term care insurance services regardless of socioeconomic backgrounds. We have added the following sentence.

"The Japanese data were from the Japan Gerontological Evaluation Study (JAGES), which comprises community-dwelling individuals aged 65 years and older from 30 municipalities (in 14 municipalities the entire population was surveyed, whereas in the remaining 16 municipalities, random sampling was performed) who were not eligible to receive benefits from public long-term care insurance services (e.g., who were without functional disability). (p8)

“Tenth, the data from Japan (e.g., data from JAGES) excluded those with functional disability; therefore, the association between ACE and health throughout life might be underestimated in Japan.” (p28, limitation of the current study)

3. Were the analyses conducted separately for Japan and Finland (i.e., separate regression analyses run for each country)? If so, why not include both samples in the same analyses and then test the ACE x country interaction predicting each health outcome? The discussion makes the claim that the pattern of results and strength of associations were similar across countries, but the way to formally test that is to test this interaction?

Response: We appreciate the reviewer’s recommendations; however, we conducted the analyses separately because we could not access the Finnish datasets to pool data.

4. Related to point 3, if the analyses are run separately for each country, there does not seem to be quite as much of a need to make the measures exactly equivalent across countries (since, after all, the questions were asked differently in many cases, which is understandable). It seems that it might be helpful to at least include some sensitivity analyses to test whether the results hold up if the variables are left in their original form (e.g., self-rated health left as a linear scale, rather than dichotomized).

Response: We agree that a sensitivity analysis using the original form would be helpful to understand the results. However, a 4-point Likert scale was used in Japan and a 5-point Likert scale was used in Finland. Therefore, to compare the result of the two countries, we dichotomized the answers of self-rated health. Questions and answers were shown in Supplemental Table S1 and S2.

5. The items that assessed fear of family seemed especially different in the two countries (fear vs. witnessing abuse). I appreciate that this is going to happen when researchers draw from two large datasets designed by different people, but it seems worth at least acknowledging this as a limitation in the discussion.

Response: We admit that the assessment of fear in the family in the two studies were different, and therefore we added the following sentence in the limitations.

“Fourth, the assessment of fear of family in JAGES and FPS and HeSSup were different, which may result in heterogeneity between study estimates.” (p27, limitation in the current study)

Minor points:
1. The introduction mentions the gini coefficient. This is a widely used statistic, but it might be helpful to explain what it is briefly, just in case some readers are unfamiliar iwth it.

Response: Thank you for your comments. We have added the following sentence:

“However, the two countries differ in terms of equality (e.g., the Gini coefficient [a measure which represents the income distribution of a country's residents8] was 0.33 in Japan [2012] and 0.26 in Finland [2015]).”(p7)

2. 1/5 of the Japanese sample were asked about ACEs. Was this 1/5 of the sample randomly chosen, or were they selected based on some other criterion?

Response: It was 1/5 of the sample that was randomly chosen. We have added the following sentence:

“The data used in this study were from participants (n = 137,736, response rate = 71%) aged ≥65 years, one-fifth of whom were randomly chosen and questioned for information on adverse experiences in childhood (n = 25,928) in 2013.” (p9)

Response to comments from Reviewer 2:

Reviewer: 2
Reviewer Name: Dr Kat Ford; Hannah Grey
Institution and Country: Bangor University, Wales
Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below
Dear Authors,
Thank you for the opportunity to review this manuscript. The paper presents a unique comparison of ACE prevalence across countries, however, we have highlighted a number of issues which we think the manuscript needs to address.

Abstract
1. The abstract needs clarity, such as line 22 - authors do not state that the 10,353 participants are from Finland. Further, lines 28 & 43 - the authors do not define the category ‘number of ACEs’ and therefore it is not clear what this refers to, for example, any number of ACEs or ACE count i.e. 2 to 3 ACEs?

Response: We appreciate the reviewer’s comments. The “number of ACEs” means the cumulative number of ACEs treated as an ordered categorical variable. We have clarified the abstract accordingly:

“A total of 13,123 adults from Japan (mean age, 69.5 years) and 10,353 adults (mean age, 64.4 years) from Finland were included in this study.” (p3)

“Logistic regression was used to examine the associations of each of, any number of, and the cumulative number of ACEs (parental divorce, fear in the family, and poverty in childhood; treated as ordered categorical variables) with poor self-rated health, cancer, heart disease or stroke, diabetes mellitus, smoking, and body mass index.” (p3)

2. Authors should be consistent throughout the abstract and manuscript when reporting findings in the text and use of percentages i.e. Line 40: state what ‘half’ refers to (i.e. 50.0%).

Response: We agree that that we should be consistent throughout the abstract and manuscript when reporting findings. We amended the sentences as follows:

“Of the respondents, 50% in Japan and 37% in Finland reported having experienced at least one of the measured ACEs.” (p3)

“Of the respondents, 50% in Japan and 37% in Finland reported having experienced at least one ACE.” (p15)

“Concerning socioeconomic status, 50% of the respondents in Finland and 24% of respondents in Japan were educated for 12 years or more. Of participants in Finland, 70% were currently working, whereas 66% of participants in Japan were currently not working.” (p17)

3. The results section of the abstract does not state what the reference categories used in the logistic regressions are, nor do they report the significance values.

Response: We appreciate the reviewers' comments. Regarding reference categories, we added the following sentences (written in red):

"Logistic regression was used to examine the associations between each of (reference: no ACE), any number of (reference: no ACE), and cumulative number of ACEs (parental divorce, fear in the family, and poverty in childhood; treated as ordered categorical variable) with poor self-rated health, cancer, heart disease or stroke, diabetes mellitus, smoking, and body mass index. Models were adjusted for sex, age, education, marital status, and working status." (p3)

Regarding significant values, we consider that 95% confidence intervals more precisely convey the strength of relationship between variables, whereas a P value does not because a P value is only one number (Rothman, 2012). The upper and lower values of a 95% confidence interval is sufficient to determine a P value (Rothman, 2012). As information of the significance values might duplicate the information given by 95% confidence intervals, we consider significance values to be unnecessary in the results section of the abstract.

Reference:
Rothman, K. J. (2012). Epidemiology: an introduction. Oxford university press.

4. Further the abstract should be strengthened to reflect other comments within the discussion section on the paper.

Response: We appreciate the reviewers' comments. We have amended the abstract as follows:

"Associations between ACEs and self-rated health, adult diseases, and health behaviours were similar among older adults in Japan and Finland. Although the results are potentially subject to recall and survival bias, this international comparative study suggests that the impact of ACEs on health is noteworthy and consistent across cultural and social environments." (p4)

5. The current reported strengths and limitations are neither. For example, the authors state they have used data from two studies – it is not clear whether this is a strength or a limitation (and why). Further this should emphasize why the paper is unique i.e. in providing a comparison between two countries.

Response: We appreciate the reviewers' comments. We amended the limitations and strengths sections as follows:

• The strength of this study is that it is an international comparative study that investigated the impact of adverse childhood experiments (ACEs) on the health of older adults in different cultural and social environments, Japan and Finland, using harmonised data.
• The limitation of this study is that this study was a cross-sectional study, and therefore differential recall and selection bias cannot be ruled out. Survival bias is also possible because the participants were older adults.
• Another limitation of this study is that the pooled data of the two countries were not accessible, and therefore interactive effects of the countries and ACE on adult health were not clear.

6. Introduction
The authors should aim to avoid language that is overly causative when discussing the associations that exist between ACEs and unhealthy behaviours (line 14) and further in the paper.

Response: We agree with the reviewer comment that we should avoid overly causative language. We amended the sentence as follows:

“An increasing number of studies have investigated the association between adverse childhood experiences (ACEs), including long-term financial difficulties, parental divorce, and fear of a family member,[1] and unhealthy behaviours (e.g., obesity,[1, 2] alcohol consumption, smoking, and lower levels of physical activity[1]), adult diseases (e.g., cardiovascular disease, diabetes,[1, 3] stroke, cancer,[1] and depression[1, 2]), and even early death.” (p6)

7. The authors state that it is not known whether ACEs and their impact on health vary by country (line 23). A number of studies have explored ACEs within and across different countries (for example, Bellis et al., 2014; Hughes et al., 2017). However, the comparison between Japan and Finland, and looking at older adults in particular, is unique and should be emphasised more. The comparisons between Japan and Finland could be strengthened by providing more information on the cultural and social differences/similarities between the two countries, e.g., how much does equality differ within countries? Are there different levels of im/migration in the two countries? I would also like to see information on ACE prevalence and the prevalence of childhood maltreatment or separation and divorce in Japan and Finland as other studies have identified. There needs to be more onus on why the manuscript is new and original.

Response: We appreciate the reviewers’ comments and suggestions. We have mentioned Gini coefficient, immigration policies, divorce rate, and prevalence of ACEs in Japan and Finland. To emphasize the knowledge gap between recent studies and the current study, we also mentioned the limitations of recent studies. We have added the following sentences:

“Although some recent studies have investigated the impact of ACEs on adult health1 9, it is not known whether the impact of ACEs on older adult health varies by country, although the pathways linking childhood adversities with adult health likely depend on cultural or social environments.[5–7] Therefore, international comparisons of countries with different cultural and/or social environments in childhood but similar welfare state regimes may provide further understanding of the mechanisms underlying ACEs and older adult health.” (p6)

“Regarding immigration, immigration policies in Japan and Finland were different at the time of this study: 1.3% of the total Japanese population, whereas 6.2% of the Finnish population were international migrants in 2017.10 Another cultural difference between the two countries was divorce rate, with a divorce rate per 1000 of 1.7 in Japan [2017]11 and 2.5 in Finland [2015]12. Finally, the prevalence of ACEs also differed between Japan and Finland, with 37% of participants reporting at least one ACE in a previous Japanese study with a participant mean age of 73 years old 13, and 61% of participants reporting at least one ACE in a Finish study with a mean study population age of 48 years.14” (p7)

“According to a systematic review, most of the recent studies evaluating the impact of multiple ACEs on health throughout life were performed in the United states (US) and the United Kingdom (UK), with only a few studies conducted in other countries.1 The US and the UK are members of the OECD, as are Japan and Finland. Japan, Finland, and the UK employ a universal healthcare system, whereas the US does not. In terms of inequality, of 37 OECD countries the US ranked 34th, and the UK ranked 30th (e.g., the Gini coefficient was 0.36 in UK [2015] and 0.39 in US [2015]).2 Thus, there is more income inequality exists in the US and the UK than in Japan and Finland.” (p7)

“Recent studies which investigated the effect of ACEs on health throughout life had some limitations. First, most studies were performed in the US and UK1, with few studies conducted in Asian 3or

Nordic countries 4 5. Second, only one study focused on older adults6. Third, few studies compared the effects of ACEs in different countries1 7." (p8)
1134 561 7

8. Methods
The methods section lacks clarity on some issues, which makes it difficult to understand. We feel the paper would be greatly improved by adding clarity on the following:
• It is stated the data are from surveys conducted among older adults – define age range, average age etc. The final sample size, and sample sizes of both countries as well as demographic information regarding gender, age, employment status should be clearly reported within the findings.

Response: We appreciate the reviewers' comments. We have reported sociodemographic characteristics in Table 1 regarding the final sample size of both countries, average age and standard deviation, gender, and working status. Please see page 15.

• Significant differences (if any) across study populations (could be presented in table one). No discussion is given to whether the characteristics of the sample is representative of the countries.

Response: We appreciate the reviewer's comments. Significant differences across study population were shown in Table 1. We have added the following sentences.

"The participants of the JAGES might be healthier than the average older Japanese population because they were living in a municipality where municipal officers were interested in and decided to participate in the JAGES." (p9)

"The participants of the FPS were individuals who were or who had been public sector employees; thus, they did not fully represent the general Finnish population of the same age." (p9)

"The participants of the HeSSup might be representative of the Finnish population. 15" (p10)

• Information on how the data from the studies were derived. It is assumed that the study is a secondary analysis of an existing dataset but this is not overtly mentioned and the data collection methods are not fully explained e.g. face to face, postal, telephone surveys, self-completion, etc.

Response: We have added the following sentence:

"Self-administered survey questionnaires were delivered by post to those who were listed in a ledger of individuals insured for long-term care." (p8)
"Self-administered survey questionnaires were used in the FPS study and HeSSup study." (p10)

• The numbers of individuals excluded from analysis at each point should be clarified (line 54; page 6).

Response: We have added the following information:

"Participants with missing data on any ACEs (n=1,158), self-rated health (SRH) (n=325), body mass index (BMI) (n=483), and smoking (n=163) were excluded." (p9)

• Discussion on how missing data was handled i.e. some individuals were excluded, some included in analysis, some dummy? Why not exclude all who were unable to provide full data for each variable used in the analysis – this is a limitation and should be recognised as such. (e.g. page 9, line 17 states that no, yes and cannot say were all coded as missing. Line 28 states that missing data were

coded as not having a specific ACE). It should be considered that if a response is missing to an ACE variable then that should be excluded from analyses, as the ACE could have occurred but not have been reported. Another example of missing data being treated differently is given on page 11, line 14.

Response: We appreciate the revisers' recommendations. We excluded those with missing data on any ACEs, self-rated health, BMI, and smoking. Regarding covariates (education, marital status, working status), we included those with missing data in order to keep the sample size.

We apologise: the sentence 'For the summary variable, missing data were coded as not having that specific AC.' was a mistake. Missing data on any ACEs were excluded. We have deleted that sentence (p12).

We also apologise that the sentence '"the response categories "no", "yes", and "cannot say" were coded as missing variables."' was another mistake. We have amended the sentences as follows:

"the response categories "no", "yes", and "cannot say" were coded as dummy variables." (p12)

• Limitations of methods: The JAGES study collected data from individuals who were not eligible to receive benefits from public long-term care insurance providers. Would excluding this group bias the sample in any way?

Response: In Japan, those with functional limitations are eligible to receive benefits from public long-term care insurance providers, regardless of individual characteristics such as socioeconomic status. The participants of our study might be healthier than the average health status of older people in Japan because they were without functional limitations. We amended and added the sentences as follows:

"The Japanese data were from the Japan Gerontological Evaluation Study (JAGES), which comprises community-dwelling individuals aged 65 years and older from 30 municipalities (in 14 municipalities the entire population was surveyed, whereas in the remaining 16 municipalities, random sampling was performed) who were not eligible to receive benefits from public long-term care insurance services (e.g., who were without functional disability)." (p8)

"Fifth, the participants of the JAGES were without functional disability, and thus might be healthier than the average Japanese older population. Therefore, the results of the study might be underestimated." (p27)

• I have concerns around the ethical approval for the research. The authors report that ethics was approved for the original studies; but does this also include the use of the data for secondary analysis. If this study is indeed a secondary analysis of an existing dataset, then was further ethical approval required/sought to conduct the combined analysis present in the current study. The paper does not discuss other ethical considerations which I would expect to be incorporated such as how the data were stored, anonymity and confidentiality of personal information.

Response: We thank the reviewers for these comments. We added the sentences as follows.

"The studies include ethical approval for these secondary analyses. The information about data management and handling is relevant and available for the study administrators in each country." (p10)

• There is a (recognised) disparity in how ACEs were measured across studies. It is not evident to the reader, how they are different between studies/countries, and then how these have been recoded. It

would be useful to the authors to present this as a table either within the paper itself or as supplementary material. For example – we would envisage this to contain: questions asked, response options and how this was coded as a positive ACE score. This is the same for measurement of health and health behaviours. Any differences between the surveys should be acknowledged within the limitations section of the paper and including any implications for interpretation of the results.

Response: We appreciate the reviewers' comments. We added supplemental table S1 and S2 which explain the measurement of ACEs and health and health behaviours. We have added the following sentence:

"The questions regarding each ACE are shown in Supplemental table S1." (p12)

"The questions regarding health and health behaviours are shown in Supplemental table S2." (p13)

"Sixth, there is a disparity in the measurement of ACEs and health and health behaviours across JAGES, FPS, and HeSSup. The differences in measurement might result in heterogeneity of the results." (p27)

• Fear of a family member; witnessing domestic abuse; and physical abuse have seemingly been coded into one variable. The authors should state why they did this, as these categories could be quite distinct. Explanation is needed on whether physical abuse relate to physical abuse between parents or abuse against the child (line 29). As per previous comment it would be advantageous to see (in text or as supplementary material) how the questions were phrased/asked in order to increase repeatability of the study and explore how valid the questions used were.

Response: The questions used to measure ACEs are shown in Supplemental table S1. The authors discussed and reached the conclusion that 'Fear of a family member' could be considered to consist of violence to the child (e.g., themselves), to the mother (or to the father), or rarely to other family members such as siblings or grandparents. We have amended the following sentence.

"We categorised those who responded having experienced "being witness to domestic violence" and/or "physical abuse" as having "frequent fear in a family" in order to make these ACEs comparable to those in the FPS and HeSSup studies. Violence to the child (physical abuse) and to the mother (witnessing domestic violence) may both result in 'fear of a family member', and we therefore coded witness of domestic violence or physical abuse as 'fear of a family member'." (p11)

• Concern is raised with regards to the summary variable called 'number of ACEs'. Line 29 states that this included 0-3 ACEs, meaning the analyses would have included those who do not have any adversities with those who have all 3. This may be an error and might intend to mean 1 to 3 ACEs but it is not clear from the text or the results what 'number of ACEs' refers to. ACE literature generally categorises ACEs in 0, 1, 2-3 and ≥4 ACEs. There is no discussion of how these are grouped, justifications for grouping or reference to literature on cumulative adversity within this section.

Response: We appreciate the reviewers' comments. 'Number of ACEs' refers to cumulative number of ACEs, e.g. 0, 1, 2 and 3 ACEs. Cumulative number of ACEs has been shown to be related to greater risks of adverse health outcomes (Felitti VJ et al, 1998; Anda RF et al, 2006); therefore, we conducted analysis with cumulative number of ACEs. We assessed only three ACEs, therefore, we could not categorize ACEs as 0, 1, 2-3 and ≥4 ACEs. We did not group any ACEs to conduct statistical analysis. We have amended the sentences as follows:

"For the present study, the three ACEs were analysed separately and also as a summary variable (0, 1, 2, and 3 ACEs)." (p12)

“Logistic regression analysis was performed to examine the association of each ACE, any ACE, and the cumulative number of ACEs (e.g., 0, 1, 2, and 3 ACEs) with health outcomes.” (p14)

Reference:
Felitti, V. J., Anda, R. F., Nordenberg, D., Williamson, D. F., Spitz, A. M., Edwards, V., & Marks, J. S. (1998). Relationship of childhood abuse and household dysfunction to many of the leading causes of death in adults: The Adverse Childhood Experiences (ACE) Study. American journal of preventive medicine, 14(4), 245-258.

Anda, R. F., Felitti, V. J., Bremner, J. D., Walker, J. D., Whitfield, C. H., Perry, B. D., ... & Giles, W. H. (2006). The enduring effects of abuse and related adverse experiences in childhood. European archives of psychiatry and clinical neuroscience, 256(3), 174-186.

• Page 10, Line 20 onwards: It would be useful to have more information on how information was derived from the National Cancer Registry and the Social Insurance Institution of Finland. There is ambiguity surrounding how this data was collected from a separate source.

Response: We added the following sentence:

“In FPS and HeSSup, we used personal identification codes, assigned to all Finnish citizens, to link the respondents to their records in national health registers.” (p13)

9. Covariates
As noted above, a discussion of other potential covariates that could potentially be included would enhance the paper. Further to this, more thought and discussion is needed into the grouping of variables e.g. ‘retired and never worked’.

Response: We considered other potential covariates such as household income; however, those other covariates were not similarly available in all cohorts. We have added the following sentence.

“Seventh, considering other covariates such as levels of inequality, current and previous household income, environmental risks (e.g., parental smoking), or genetic variation was not feasible due to data availability.” (p27)

Regarding working status, we added the following sentence.

“Regarding working status, those never worked was not included in FPS because FPS is a study of individuals who were public sector employees and represented a wide range of occupations. Therefore, we combined ‘retired and never worked’ as ‘not working’ in Japan to be comparable with studies in Finland.” (p14)

10. Results
The tables could be enhanced with clearer headings and a thorough proof reading to check the footnotes e.g. AOR’s not ORs or RR. It is assumed that figures in bold in the tables are significant, but this is not stated anywhere in the paper and could be made clearer, as could the level of significance (e.g. $p<0.05$, $p<0.001$ etc). It is also not clear what the reference category being used in each of the analyses are.

Response: We have amended the table headings as follows:

“Table 2. Association between adverse childhood experiences and poor self-rated health and

diseases among older adults in Japan and Finland." (p19)
"Table 3. Association between adverse childhood experiences and former and current smoking among older adults in Japan and Finland." (p22)
"Table 4. Association between adverse childhood experiences and body mass index among older adults in Japan and Finland." (p23)

"OR; odds ratio" (p22, the footnotes of Table 3.)
"Bold text indicates statistically significant with a p-value less than 0.05." (p20, p22, p23)

11. Discussion
• It is felt that the discussion would benefit from a change in the structure, which would provide more clarity and transparency whilst giving more credence to the unique aspects of the study. More information is needed on the contribution of each ACE and cumulative ACEs to health and behaviours between and within each country to enhance the comparative aspects of the study.

Response: We appreciate the reviewers' comments. We have added the following sentences.

"To our knowledge, this is the first study to compare the impact of ACEs on health among older adults between two countries." (p24)

"The strength of these associations was weak or modest, and similar between the two countries." (p24)

"Our finding, highlighting the association between ACEs and worse adult health, is consistent with those of other studies. Although the association was weak or modest, this might be due to survival bias. Regarding SRH, the association between fear in the family in childhood and poor SRH among older adults was slightly stronger than that for parental divorce or childhood poverty in Japan; however, in Finland, a similar difference was not observed. This result suggests that the prevention of, detection of, and follow-up provided for childhood abuse or intimate partner violence in Japan may not have been as adequate those in Finland. Alternatively, physical punishment might be more pervasive in Japan 16 than in Finland; physical punishment was not forbidden in Japan, but was legally forbidden in Finland. Regarding cancer, childhood poverty was associated with cancer among older people in Japan, but this association was not observed in Finland. Childhood poverty in Japan might lead to poverty in adulthood, which may result in delays in medical check-ups or consultations. Despite provision of universal health care in Japan, individual payment of medical expenses is at least 10%, even among older adults.
Generally, the strength of these associations was similar between the two countries." (p24)

• Whist interesting, the discussion relating to World War 2 does not feel appropriate or in keeping with the rest of the text. Further to this, no references are provided to back-up the claims made about the effect of World War 2.

Response: According to the reviewers' comments, we deleted the sentence regarding World War 2.

• Survival bias is a key limitation of this unique study due to the elderly sample, this should be discussed in more detail.

Response: We have added the following sentence:

"Eighth, the results might be subject to survival bias. The average age of the study participants was 69.5 in Japan and 64.4 in Finland. As a result, people who had died before the current study due to ACE-related diseases or health risk behaviours would not have been included in the current study.

Therefore, the results of the current study may underestimate the health effects of ACEs." (p28)

• The ACE prevalence and odds ratios listed are low compared to other studies. In an international meta-analysis, Hughes et al (2017) found a moderate odds ratio for smoking (2.82, 95% CI 2.38–3.34) in people with four or more ACEs compared to those with no ACEs. The authors should be mindful of not overstating their results, as the odds ratios found are relatively weak. There could also be discussion here that the prevalence within these results are low due to only measuring 3 ACEs in the first instance (see later comments about transparency of only measuring 3 ACEs).

Response: We appreciated the reviewers' comments. We have added the following sentences:

"Similarly, ACEs were associated with smoking, which is consistent with the findings of a previously conducted international comparison research in eight eastern European countries7 and other studies in the United States (US), 17 18 United Kingdom (UK),19 and Finland9, although this association was weak in the current study." (p26)

"A meta-analysis showed a moderate odds ratio for smoking among those with four or more ACEs compared with those with no ACEs.1 The association was weaker in the current study than the results of the meta-analysis, possibly because (1) there were only three ACEs measured in the current study, and (2) the association may have been underestimated because of early death due to smoking and ACEs (e.g., survival bias)." (p26)

•
The concluding remarks need to clearly reiterate the unique contribution that this paper makes, and the potential impact or consequences that these findings could present.

Response: According to the reviewers' comments, we amended the concluding remarks.

"In conclusion, this is the first study to investigate the association between ACEs and health and health behaviour among older people in two countries, Japan and Finland. In Japan and Finland, the relationship between ACEs and health were similar for SRH, specific diseases, and smoking. The impact of ACEs on BMI was stronger in Finland than in Japan. These results suggest that there is a notable association between ACEs and health among older people, and that this association remains consistent even in countries with different historical and cultural heritages." (p28)

There are a number of limitations that could be included, for example:
• Potential confounding factors that are not included in the study which could have contributed to health and health behaviours, such as levels of deprivation/inequality, ethnicity.

Response: According to the reviewers' comments, we added the following sentence:

"Seventh, considering other covariates such as levels of inequality, current and previous household income, environmental risks (e.g., parental smoking), or genetic variation was not feasible due to data availability." (p27)

• The authors mention in the discussion that ACE literature uses a number of other ACEs; this is quite a distinguishable difference and more attention needs to be paid to this point.

Response: According to the reviewers' comments, we added the following sentence:

"Third, we did not assess other ACEs such as sexual abuse, neglect, childhood neighbourhood deprivation, or family disfunction (i.e., mental disorder of a family member, or incarcerated family

member). Further studies are necessary to investigate the impact of other ACEs, as well as more than three ACEs, on the health of older adults." (p27)

• Many of the variables were self-reported. Associated problems of accuracy are not discussed or raised as a limitation.

Response: According to the reviewers' comments, we added the following sentence:

"Ninth, all the ACEs were self-reported; therefore, there may be recall or reporting biases." (p28)

There are a few spelling mistakes as well as discrepancies in referencing within the text and therefore a thorough proof read is required.

Response: According to the reviewer's comment, the paper was proofread by a native speaker.

Suggested references to include:

Bellis, M. A., Hughes, K., Leckenby, N., Jones, L., Baban, A., Kachaeva, M., ... & Raleva, M. (2014). Adverse childhood experiences and associations with health-harming behaviours in young adults: surveys in eight eastern European countries. Bulletin of the World Health Organization, 92, 641-655.

Hughes, K., Bellis, M. A., Hardcastle, K. A., Sethi, D., Butchart, A., Mikton, C., ... & Dunne, M. P. (2017). The effect of multiple adverse childhood experiences on health: a systematic review and meta-analysis. The Lancet Public Health, 2(8), e356-e366.

Response to comments from Reviewer 3:

Reviewer Name: Katie Hardcastle

Research objectives (1):

1. The paper offers two independent examinations of the relationships between a small number of adverse childhood experiences and health outcomes. However, it does not sufficiently draw this data together to offer a comparison between the two countries (as proposed). Generally the authors do a reasonable job of outlining the ways in which the three data sets differ. But for me, the differences/inconsistencies outweigh the similarities and it is not clear why it was felt that these data sets could reasonably be brought together. Any discussion of the social and cultural environments in these two countries is very limited and does not adequately set the scene for any subsequent comparisons. Later we see there are differences in the prevalence of different ACEs across the two countries; however, the paper does not fully explore this. The decision to focus on older adults is important (based on the current field) but not explained.

Response: As for rationale of investigation of Japan and Finland, we added the following：

"Japan and Finland are both members of the Organization for Economic Co-operation and Development (OECD). The two countries employ a universal healthcare system [8 9] and provide free

education to those aged 6 to 15 years.[10] According to the World Happiness Report, the level of social support received (measured by having someone to count on in times of trouble) is relatively high in both countries (92.3% and 94.8% in 2015, in Japan and Finland, respectively).[11] However, the two countries differ in terms of equality (e.g., the Gini coefficient, a measure which represents the income distribution of a country's residents,[12] was 0.38 in Japan in 2014 and 0.26 in Finland 2015). Out of 37 OECD countries, Japan ranked 22th and Finland ranked 7th in 2015 in terms of equality.[13] Furthermore, immigration policies the two countries were different at the time of this study; international migrants made up 1.3% of the total Japanese population, whereas 6.2% of the Finnish population were international migrants in 2017.[14] Another cultural aspect that is between the two countries was divorce rate; the divorce rate in Japan in 2017 was 1.7/1,000 people,[15] and in Finland it was 2.5/1,000 people in 2015.[16] Finally, the prevalence of ACEs also differed between Japan and Finland, with 37% of participants (mean age of 73 years old) in a Japanese study reporting at least one ACE,[17] and 61% of participants (mean age of 48 years old) in a Finnish study reporting at least one ACE.[18]

According to a systematic review, most of the recent studies evaluating the impact of multiple ACEs on health throughout life were performed in the United states (US) and the United Kingdom (UK), with only a few studies conducted in other countries,[19] such as Asian [20]or Nordic countries.[21 22] The US and the UK are also members of the OECD. However, while Japan, Finland and the UK employ a universal healthcare system, the US does not. Further, only one study focused on older adults.[23] The purpose of this study was, therefore, to examine the associations and related risk factors between ACEs and adult diseases in older adults in Japan and Finland." [page 6-8]

As for rationale of focusing of older adults, we added the following:

"Although some recent studies have investigated the impact of ACEs on adult health, it is not known whether ACEs has an impact on the health of older adults. The investigation of this topic is important to confirm the long-term adverse effects of ACE and manage ACEs.." [page 6]

2. The vast majority of ACEs literature considers a wider spectrum of ACEs. Is it appropriate to build this paper on such literature using only 3 ACEs?

Response: Variables on ACEs which could be harmonized were only 3 ACEs, thus we used 3 ACEs. The limited number of ACEs were discussed as limitation as follows:

"Third, we did not assess other ACEs such as sexual abuse, neglect, childhood neighbourhood deprivation, or family disfunction (i.e., mental disorder of a family member, or incarcerated family member), and thus the number of ACEs were limited to only three. Further studies are necessary to investigate the impact of other ACEs on the health of older adults." [page 27]

Methods (4-6):

3. The methods section of this manuscript is a particular area of concern. A paper like this would benefit considerably from a supplementary table that clearly outlines commonalities or how the studies differed. Authors need to be much more explicit about each of the following: sampling methods; response rates (of the original study; detailed for the age cohort selected); timing of the

collection of different variables; methods of data collection (e.g. paper surveys, online, face-to-face, in the respondent's home or other location etc); processes for consent, withdrawal.

Response: Following the comment, details of sampling method, response rate, timing of collection of variables, methods of data collection, process for consent, and withdrawal were amended as follows:

"The data in this study were collected from surveys conducted among older individuals in Finland and Japan. The Japanese data were from the Japan Gerontological Evaluation Study (JAGES), which comprises community-dwelling individuals aged 65 years and older from 30 municipalities (in 14 municipalities the entire population was surveyed, whereas in the remaining 16 municipalities, random sampling was performed) who were not eligible to receive benefits from public long-term care insurance services (e.g., those without functional disability). Self-administered survey questionnaires were delivered by post to those who were listed in a ledger of individuals insured for long-term care. The participants of the JAGES might be healthier than the average older Japanese population because they were living in a municipality where municipal officers were keen to participate in the JAGES. The data used in this study were from participants (n = 137,736, response rate = 71%) aged ≥65 years, one-fifth of whom were randomly chosen and questioned for information on adverse experiences in childhood (n = 26,229) in 2013. The participants of the current study were restricted to an age range of 65-74 years (n = 15,070). Participants with missing data on any ACEs (n=1,158), self-rated health (SRH) (n=325), body mass index (BMI) (n= 483), and smoking (n=163) were excluded.

The Finnish data were drawn from two prospective cohort studies: the Finnish Public Sector (FPS) study[24] and the Health and Social Support (HeSSup) study.[25] The FPS study included employees representing a wide range of occupations working in ten towns and six hospital districts. The participants of the FPS were individuals who were at the time of the study, or had previously been, public sector employees; thus, they did not fully represent the general Finnish population of the same age. The FPS data used in this study were derived from employed and retired participants in the 2008/2009 survey, and included information on self-reported ACEs (n = 42,877, response rate = 69%). For this study, all FPS study respondents aged ≧60 years (n = 7,169) were selected. The HeSSup study targeted a sample representative of the Finnish population in four age groups (20–24, 30–34, 40–44, and 50–54 years), in 1998. Therefore, the participants of the HeSSup may be representative of the Finnish population.[26] In the 2012 follow-up survey, information on self-reported ACEs was obtained from 11,924 participants (response rate = 78%). Of them, those in the oldest age group (64–68 years) were selected (n = 3,184). Self-administered survey questionnaires were used in the FPS and HeSSup studies. The two Finnish cohorts were pooled. The studies together included 13,123 (6,214 men and 6,909 women) participants from Japan and 10,353 (3,201 men and 7,152 women) participants from Finland." [page 8-10]

4. Are the samples representative? There are many unaddressed challenges. For example, the Finnish sample is heavily skewed towards female participants. Why were different ages used across the different studies? Prevalence of cancer seems very low (Table 1) and appears inconsistent with data from the Global Cancer Observatory or other sources.

Response: We can not say that the current data is representative sample. As we focused on older adults, we aim to limit the age range over 65 for Finnish sample, but it yield quite low sample size, thus we included age 60 or over for Finish sample. Due to non-representativeness, cancer prevalence

may seem inconsistent from Global Cancer Observatory. These points were discussed as limitation as follows;

“Finally, the data from Japan (e.g., data from JAGES) excluded those with functional disability; therefore, the association between ACE and health throughout life might be underestimated in Japan. Further, FPS and HeSSup were not representative sample, thus prevalence of diseases, such as cancer, may be different from other studies.” [page 28]

5. The handling of some of the study variables appears a little haphazard. For example, for the JAGES data, a dichotomous yes/no response to the selected ACEs is described as ‘frequent’ fear in a family (with no measure of frequency). ACEs are not clearly defined, nor is the bounds of ‘childhood’ (i.e. what ages?). For self-rated health, fair is dichotomised to poor in the Japanese data, but ‘moderate’ is dichotomised to good in the Finnish data. Can this be justified or is there precedent? A ‘history of being diagnosed with cancer’ lacks detail (does this refer to a current diagnosis; how would this be reflected for people that were, for example, in remission). Smoking and working status also lack clarity.

Response: To clarify ACE variables in details for each data, we made supplementary table 1 as follows:

As for self-rated health, due to Japanese culture, we used 4-Lickert scale to avoid that majority of sample mark the “middle” choice, and based on discussion with authors, we decided to fair is dichotomised to poor in the Japanese data, but ‘moderate’ is dichotomised to good in the Finnish data.

As for covariares, working status was clarified as follows:

“Regarding working status, those who had never worked was not included in FPS because FPS is a study of individuals who were at the time of study, or had previously been, public sector employees and represented a wide range of occupations. Therefore, we combined ‘retired and never worked’ as ‘not working’ in Japan to be comparable with the studies in Finland.” [page 14]

As for smoking status, we asked exactly as follows, current, former, and never, based on survey responses. We have no further information on number of cigarette.

Analyses and presentation of results (7,9-10):

6. A very small proportion of the sample have all three of the ACEs analysed here, which has implications for analysis by number of ACEs. Alternative focus may be appropriate (any vs none) but at the very least this should be reflected in limitations.

Response: Following the comment, small number of ACEs were discussed as limitation as follows.

“Third, we did not assess other ACEs such as sexual abuse, neglect, childhood neighbourhood deprivation, or family disfunction (i.e., mental disorder of a family member, or incarcerated family member), and thus the number of ACEs were limited to only three. Further studies are necessary to investigate the impact of other ACEs on the health of older adults.” [page 27]

7. A sample characteristics table is provided (Table 1) and the supporting text includes discussion of ways in which the two countries are ‘similar’. It appears that there are considerable differences in demographics. However, the authors have not used any bi-variate statistics to consider these differences.

Response: Following the comment, we added bivariate comparison between these data in Table 1.

8. Crucially, it is unclear to me why the authors have chosen to run a series of separate models rather than: (a) including country as a variable in the model; and (b) including all covariates in one model. No p values are provided throughout.

Response: Due to restriction of sharing data of FPS and HeSSup, we can not pool these data, thus we can not run the analysis in one pooled data, which preclude to put country as variable in the model.

Discussion and limitations (11-12):

9. Throughout the paper, use of the term ‘similar’ is concerning with no supporting statistical significance. Generally the discussion lacks a response to that ‘so what’ question and introduces concepts such as the impact of WWII with no justification or thorough explanation of its relevance. Much of the discussion appears to be focused around resilience, but there is no reference to the vast emerging literature on resilience factors. Whilst tentative links are made such as with health service use, again theories and evidence for the impact of adversity on help seeking etc are not referenced.

Response: Following the comment, interpretation of “similar” results were revised, deleting WWII stories, adding resilience literature, as follows:

“Generally, the strength of these associations was similar between the two countries. The consistency of our results in harmonised cohorts from two different countries suggests that childhood adversities affect health similarly in these two societies. In spite of the differences in the cultural and social environments of older adults in these countries,[33 34] it is interesting to note that the associations observed were similar. A likely explanation could be the presence of universal healthcare systems in

these countries, which offer adequate medical treatment for diseases over one's life span, and likely lead to the attenuation of the impact of ACEs on health later in life. This might explain the lower OR of ACEs for diseases comparing previous studies, because universal health care system might be effective to protect older adults with ACEs. In addition, the presence of equal free educational opportunities in both countries may explain the similar impact of ACEs on adult health, as educational attainment can attenuate the impact of ACEs on later health.[35] The presence of high social support,[36] cultural engagement, access to trusted adults[37] in both countries may also attenuate the impact of ACEs on later health through the enhancement of resilience, described as the ability to adapt to adverse environment.[38] The effect of ACEs on health, in different educational or healthcare systems, requires further study." [page 25]

10. Authors state that these results suggest ACEs have a 'remarkable' impact on health. However, ORs presented here are lower than reported elsewhere (e.g. Hughes et al SR in the Lancet). There should be a recognition and exploration of this.

Response: Following the comment, the part was amended and citing previous important studies as follows:

"Although the association was weaker compared with other studies,[5-7 19] this might be due to survival bias as we focused on older adults." [page 24]

11. There is a significant lack of detail for the limitations described. For example, it is not made clear at all why being derived from a highly developed country is a limitation. Many of the factors concerning the samples mentioned above should be reflected in the limitations.

Response: Following the comment, we clearly mentioned about the reason why being derived from a highly developed country is a limitation, and other limitation part was amended as follows:

"There are several limitations to this study. First, this was a cross-sectional study conducted among older adults. Thus, differential recall and selection bias cannot be ruled out, especially if those with a disease were more likely to recall ACEs or if those with the most difficult ACEs did not participate. Second, these results, although based on harmonised measures, were derived from only two egalitarian developed countries, which preclude to generalize the findings to other countries. Further studies are warranted to investigate the association between other ACEs and adult disease in different cultural settings, and in low- and middle-income countries. Third, we did not assess other ACEs such as sexual abuse, neglect, childhood neighbourhood deprivation, or family disfunction (i.e., mental disorder of a family member, or incarcerated family member), and thus the number of ACEs were limited to only three. Further studies are necessary to investigate the impact of other ACEs on the health of older adults. Fourth, the assessment of fear of a family member in the JAGES, FPS and HeSSup was different, which may result in heterogeneity between study estimates. Fifth, the participants in the JAGES were without functional disability, and hence might be healthier than the average Japanese older population. Therefore, the results of the study might be underestimated. Sixth, there is a disparity in the measurement of ACEs and health, and ACEs and health behaviours,

across the JAGES, FPS, and HeSSup. The differences in measurement might result in heterogeneity of the results. Seventh, considering other covariates such as levels of inequality, current and previous household income, environmental risks (e.g., parental smoking), or genetic variation was not feasible due to data availability. Eighth, the results might be subjected to survival bias. The average age of the study participants was 69.5 years in Japan and 64.4 years in Finland. People who passed away before the current study from ACE-related diseases or health risk behaviours would not have been included in the current study. Therefore, the results of the current study may underestimate the health effects of ACEs. Ninth, all the ACEs were self-reported. Therefore, there may be recall or reporting biases. Finally, the data from Japan (e.g., data from JAGES) excluded those with functional disability; therefore, the association between ACE and health throughout life might be underestimated in Japan. Further, FPS and HeSSup were not representative sample, thus prevalence of diseases, such as cancer, may be different from other studies." (page 27-28)

Other general comments:

11. Please note that the references appear out of sync in places. Generally the standard of English is acceptable, but there are inconsistencies and errors that have not been picked up during proof reading that would need attention.

Response: Following the comment, references are rearranged and proof reading by native English speaker as well.

References

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VERSION 2 – REVIEW

REVIEWER	Dr Kat Ford; Hannah Grey School of Health Sciences, Bangor University, UK
REVIEW RETURNED	31-Oct-2018

GENERAL COMMENTS	Thank you for the opportunity to review your updated and revised manuscript, we feel that the manuscript has improved greatly, however there are a few further points that we feel require further clarity and/or justification. 1. We still feel that the manuscript could further situate the current study within past research. The authors need further clarity on what is novel about this research and what it adds to existing knowledge as opposed to the limitations of current research. 2. The additional demographic information on the two countries in the introduction is useful however the inclusion of information on the prevalence of childhood maltreatment or policies relating to its reduction in both countries would also enhance and situate the current study. 3. The authors should clearly state the aims and objectives at the end of the introduction. 4. Can the authors please clarify the meaning of the following sentence on page 9: “The participants of the JAGES might be healthier than the average older Japanese population because they were living in a municipality where municipal officers were interested in and decided to participate in the JAGES”. Additional

	references may be required on page 9 regarding how municipal officers might lead to better health for older adults. 5. The authors have updated the ethics statement; however, we would like to see additional information on how confidentiality and anonymity were handled given the use of personal identifiers to match data sources in the Finnish sample. 6. We are still unsure as to why never worked and retired have been coded together as not working – there may be significant differences between those who have worked and then retired and those who have never worked in the sample. 7. The samples have significantly different socio-demographics and ACE prevalence. We felt that this and the potential impact on results could be further highlighted in the discussion. 8. The disparities within the ACE questions asked across studies (e.g. witnessing or experiencing violence with having a fear of a family member) possibly require further interrogation when interpreting the results. 9. We appreciate the additional information and changes the authors have made. However, we do feel that the discussion could be restructured to provide a stronger argument and more clarity to compare the impact of ACEs on older adults between the two countries. Some differences between the two countries are not discussed (e.g. diabetes). 10. There are a number of points in the limitations that could be brought together and combined where they relate to similar issues, i.e. limitations of ACE. An example of this is points 8 and 10 which both relate to potential groups who might have been excluded from the research sample. 11. The conclusion should to be strengthened to show the policy implications. Further it currently mentions historical heritage, which has not been discussed elsewhere in the manuscript. Overall points: • The manuscript would benefit from a thorough proof read. • There are two reference lists and the original should be deleted. • Tables needs tidying up (e.g. consistency in decimal places, capitalisation of headings/subheadings) and clarity in table titles and footnotes.

REVIEWER	Cynthia Levine Northwestern University, USA
REVIEW RETURNED	03-Nov-2018

GENERAL COMMENTS	Thank you for the opportunity to review the revised manuscript, which I think has been improved. I had the following remaining comments: 1. I appreciated the expanded review of differences between FInland and Japan, but I thought it would also be useful to discuss why or why not these differences might matter for the relationship between ACEs and health (i.e., to connect these differences to the content of the paper). For instance, the authors note that there are differences in immigration into the two countries. Would immigrants or the presence of immigrants in a country be more likely to make ACEs matter for health?

	2. I also appreciated that the revised manuscript does more to situate the current work in the past literature by explaining how Japan and Finalnd differ from the US and UK (where most past work has been conducted). However, again, I thought it would strength the manuscript to explain more about why those differences might matter or not (similar to point 1 above). Relatedly, I still thought the paper could do more to address the question of why Japan and Finland important countries to study. The authors do explain that Japan and Finland have less inequality than the US and UK, but it might help to explain how inequality relates to the relationship between ACEs and health. I actually think it's really important to not assume the research findings from the US and UK automatically extend to other countries (sometimes findings from one country don't replicate in another), so I really appreciate the research described in this paper, but I think it would be useful to articulate its importance even more in the introduction. 3. The abstract notes, "• Another limitation of this study is that the pooled data of the two countries were not accessible, and therefore interactive effects of the countries and ACE on adult health were not clear." This addresses a question that I had about a previous version of the manuscript. However, I did not see this limitation mentioned in the text of the manuscript, unless I missed it. I think that it would be important to note in the manuscript itself. Minor points: - In the second to last paragraph of the introduction before the methods, there are 2 periods after the word Finland. - On page 14, there is a sentence that reads, "Regarding working status, those never worked was not included . . ." It should be were not included

REVIEWER	Katie Hardcastle Public Health Wales, Wales
REVIEW RETURNED	04-Feb-2019

GENERAL COMMENTS	Thanks to the authors and the editors for the opportunity to review the revised manuscript. Whilst I can appreciate that the authors have responded to many of the comments provided by myself and other reviewers, in many cases I felt that the manuscript could benefit considerably from more detailed revisions, rather than simple references to key terms in the text. For me, the following still require more clarity: 1. Research objectives – The title of the manuscript frames it as a comparative study, however comparing the two countries does not appear to be indicated as a research objective (instead, at the foot of the introduction, the authors refer to examining the associations IN each country). As highlighted by all three reviewers, some of the challenges as a reader of this manuscript come from the way in which similarities and differences between the two countries are discussed. Ultimately, it is not clear why these two countries are combined in this manuscript and more justification is needed. Whilst authors have now indicated that they were unable to pool the data from the two countries (in order to

	use the desired method of analysis in which country is included as a variable in multivariate analyses or an interaction term is used), this remains a weakness of the manuscript. I wonder if the authors have considered whether it would be more appropriate to consider data from either Japan or Finland and focus more on the use of an older adult sample as the 'added value'? 2. Methods – The authors are commended for improvements to the methods section. However, in my view the section still does not clearly identify this as an analysis of secondary data. There is a continued need to describe the included studies/data sources more consistently, which would really go a long way to improving clarity for the reader (and making sense of some of the ways in which data analysis is restricted – as above). I would also like to see authors revisit key aspects that still appear to be missing, such as the handling of missing data for Finland. 3. Data selection - If the data from the two countries are to be used together, could the authors consider ways in which they could improve the alignment of the two data sets? For example, the choice to use different age ranges for the two samples (i.e. not using 60+ for both). As those who have never worked are excluded from the FPS, could the authors also exclude never worked from the other sample (rather than combining with retired)? 4. Differences between study populations – Authors now frequently mention ways in which the samples MAY differ from the general populations in Japan and Finland. Rather than surmising, could the authors look for any national representative data to reference this? 5. Based on the data provided, authors should take care in referring to the association between ACEs and BMI in Japan and may want to consider re-wording. 6. Discussion – I would like to see authors support some of their discussion and suggested mechanisms with evidence. For example, different findings for self rated health have been linked to differences in prevention and detection of child abuse and intimate partner violence between Japan and Finland. Is there any evidence to support this? The same applies to the discussion about cancer. 7. Limitations – Thank you to the authors for being more explicit with limitations and including many of those suggested by reviewers. Often, however, there is a lack of detail and the implications of these limitations are not discussed. For example, authors outline the data being limited to three ACEs as a limitation – could they expand on why this is a limitation and how it many affect the findings and interpretation? Why might those with a disease be more likely to recall ACEs (as the authors suggest)? Is there any evidence to support this? Many limitations now actually appear in the methods section, which could be lifted to the suitable section. There are multiple instances in which limitations are directly duplicated even within this section.

VERSION 2 – AUTHOR RESPONSE

Response to Reviewer's comments

Reviewer: 2
Reviewer Name: Dr Kat Ford; Hannah Grey

Institution and Country: School of Health Sciences, Bangor University, UK
Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below
Thank you for the opportunity to review your updated and revised manuscript, we feel that the manuscript has improved greatly, however there are a few further points that we feel require further clarity and/or justification.

1. We still feel that the manuscript could further situate the current study within past research. The authors need further clarity on what is novel about this research and what it adds to existing knowledge as opposed to the limitations of current research.

Response: Following the comments, we added following sentence to situate the current study within past research and added novelty more clearly:

“Although there is an increasing number of studies that have investigated the association between adverse childhood experiences (ACEs, such as long-term financial difficulties, parental divorce, and fear of a family member) and unhealthy behaviours (e.g., obesity, alcohol consumption, smoking, and lower levels of physical activity), adult diseases (e.g., cardiovascular disease, diabetes, stroke, cancer, and depression), and even early death,1-4 few studies have investigated whether ACEs has an impact on the health of older adults. Because of the rising number of older adults in the world, it is therefore necessary to elucidate the risk factors for diseases among older people.
Further, to address the impact of ACEs on health of older people, it is also crucial to elucidate the commonality of the association, because the pathways linking childhood adversities with adult health are likely to be dependent on cultural or social environments.5-7 Therefore, a comparison of countries with different cultural and/or social environments in childhood, but with similar welfare state regimes, may provide further understanding of the underlying mechanisms of ACEs and older adult health. According to a systematic review, most of the recent studies evaluating the impact of multiple ACEs on health were performed in the United States (US) and the United Kingdom (UK), with only a few studies conducted in other countries,8 such as Asia9 or Nordic countries.10 11 Because the US and UK are developed countries with high inequality, there is a need to confirm the association in developed but relatively equal, egalitarian countries, such as Japan or Finland.” (page 6)

2. The additional demographic information on the two countries in the introduction is useful however the inclusion of information on the prevalence of childhood maltreatment or policies relating to its reduction in both countries would also enhance and situate the current study.

Response: Following the comment, demographic information on the two countries were added as follows:

“Japan (population: approximately 127 million) and Finland (population: approximately 5.5 million) are members of the Organization for Economic Co-operation and Development (OECD). The two countries employ a universal healthcare system 12 13 and provide free education to those aged 6 to 15 years.14 According to the World Happiness Report, the level of social support received (measured by having someone to count on in times of trouble) is relatively high in both countries (92.3% and 94.8% in Japan and Finland in 2015, respectively).15 However, the two countries differ in terms of equality (e.g., the Gini coefficient, a measure which represents the income distribution of a country's residents,16 was 0.38 in Japan in 2014 and 0.26 in Finland 2015). Out of the 37 OECD countries, Japan ranked 22th and Finland ranked 7th in 2015 in terms of equality.17 Furthermore, immigration policies in the two countries were different at the time of this study; international migrants made up 1.3% of the total Japanese population, whereas 6.2% of the Finnish population were international migrants in 2017.18 Moreover, the divorce rate in Japan in 2017 was 1.7/1,000 people,19 whereas it

was 2.5/1,000 people in Finland in 2015.20 Finally, the prevalence of ACEs also differed between Japan and Finland, with 37% of participants (mean age of 73 years old) in a Japanese study reporting at least one ACE,21 and 61% of participants (mean age of 48 years old) in a Finnish study reporting at least one ACE.22 Corporal punishment is not forbidden in Japan, but in Finland, it has been prohibited by law since 1983. In summary, both Japan and Finland are developed and egalitarian countries, but their differences in terms of inequality, immigration percentage, divorce rate, or policy on corporal punishment might contribute to the differential impact of ACEs on diseases in older adults. For example, deetiolated social capital due to inequality23 might contribute to stronger impact of ACEs on health in older adults." (page 6-7)

3. The authors should clearly state the aims and objectives at the end of the introduction.

Response: Following the comment, we added the clear aims of the study as follows:

"Therefore, the purpose of this study was to examine and compare the association between ACEs and adult diseases, including unhealthy behaviours such as smoking, in older adults in Japan and Finland." (page 7)

4. Can the authors please clarify the meaning of the following sentence on page 9: "The participants of the JAGES might be healthier than the average older Japanese population because they were living in a municipality where municipal officers were interested in and decided to participate in the JAGES". Additional references may be required on page 9 regarding how municipal officers might lead to better health for older adults.

Response: We sorry for the confusion, the sentence was amended as follows:

"The participants of the JAGES might be healthier than the average older Japanese population because one of the inclusion criteria to participate in the JAGES study is not receiving long-term care." (page 9)

5. The authors have updated the ethics statement; however, we would like to see additional information on how confidentiality and anonymity were handled given the use of personal identifiers to match data sources in the Finnish sample.

Response: We did not use personal identifiers in the study. To make clear, following sentence was added:

"We did not handle any personal identifiers in the analysis." (page 10)

6. We are still unsure as to why never worked and retired have been coded together as not working – there may be significant differences between those who have worked and then retired and those who have never worked in the sample.

Response: We agree, but to harmonize the variables in JAGES and Finland study, it is needed due to response items in JAGES.

7. The samples have significantly different socio-demographics and ACE prevalence. We felt that this and the potential impact on results could be further highlighted in the discussion.

Response: We agree, the point was added in Discussion as below:

"Sixth, considering other covariates such as levels of inequality, current and previous household

income, environmental risks (e.g., parental smoking), or genetic variation was not feasible due to data availability. In fact, education level was higher in the Finnish sample than in the Japanese sample." (page 28)

8. The disparities within the ACE questions asked across studies (e.g. witnessing or experiencing violence with having a fear of a family member) possibly require further interrogation when interpreting the results.

Response: Following the comment, we added below caviate to interpret the results as follows:

"The strength of these associations was weak or modest, and similar between the two countries, although careful interpretation is needed because the assessment of ACEs in Japan and Finland was different." (page 24)

9. We appreciate the additional information and changes the authors have made. However, we do feel that the discussion could be restructured to provide a stronger argument and more clarity to compare the impact of ACEs on older adults between the two countries. Some differences between the two countries are not discussed (e.g. diabetes).

Response: Following the comment, we added below argument in Discussion:

"On the contrary, "fear of a family member" showed significant positive association with cancer and diabetes in Finland, which was not observed in Japan. This might be due to the difference in the assessment of fear of a family member. In the Finnish study, it was asked as it is, while in JAGES, witness of domestic violence was used as a proxy measurement of fear of a family member. It may be possible that in Japan, witness of domestic violence may not always induce fear of a family member, say, if the child was used to it. Thus, in Japan the association was weak. Alternatively, fear of a family member may not necessarily be a risk factor for adult disease in Japan because of the rich social network, especially among kin relatives,45 46 which provides an environment for children to escape from a fearful family member. Further study using the same question is needed to confirm whether the discrepancy is due to the difference in the assessment of social environment." (Page 27)

10. There are a number of points in the limitations that could be brought together and combined where they relate to similar issues, i.e. limitations of ACE. An example of this is points 8 and 10 which both relate to potential groups who might have been excluded from the research sample.

Response: Following the comment, limitations saying similar points were combined as follows:

"There are several limitations to this study. First, this was a cross-sectional study conducted among older adults. As ACEs were self-reported, differential recall bias cannot be ruled out. Recent review showed poor agreement between prospective and retrospective assessment of childhood maltreatment.47 However, in contrast, other review studies reported that the validity of retrospective assessment of ACEs is acceptable.48 49 Second, these results, although based on harmonised measures, were derived from only two egalitarian developed countries, which preclude the generalisation of the findings to other countries. Further studies are warranted to investigate the association between other ACEs and adult disease in different cultural settings, and in low- and middle-income countries. Third, we did not assess other ACEs such as sexual abuse, neglect, childhood neighbourhood deprivation, or family disfunction (i.e., mental disorder of a family member, or incarcerated family member), and thus the number of ACEs were limited to only three. The limited number of ACEs precluded to assess stronger impact of ACEs on adult diseases, as a previous meta-analysis revealed.8 Further studies are necessary to investigate the impact of other ACEs on the health of older adults. Fourth, there is a disparity in the measurement of ACEs and health, and ACEs

and health behaviours, across the JAGES, FPS, and HeSSup. The differences in measurement might result in heterogeneity of the results. More specifically, the assessment of fear of a family member in the JAGES, FPS and HeSSup was different, which may result in heterogeneity between study estimates. Fifth, the participants in the JAGES did not have functional disability, and hence might be healthier than the average Japanese older population. Therefore, the results of the study might be underestimated. Alternatively, the results might be subjected to survival bias. The average age of the study participants was 69.5 years in Japan and 64.4 years in Finland. People who passed away before the current study from ACE-related diseases or health risk behaviours would not have been included in the current study. Therefore, the results of the current study may underestimate the health effects of ACEs. Further, FPS was a not representative sample, thus the prevalence of diseases, such as cancer, may be different from other studies. Sixth, considering other covariates such as levels of inequality, current and previous household income, environmental risks (e.g., parental smoking), or genetic variation was not feasible due to data availability. In fact, education level was higher in the Finnish sample than in the Japanese sample. Finally, we were unable to pool the data of the two countries due to restriction on the Finnish data, and therefore interactive effects of the countries and ACE on adult health were unclear." (Page 27-28)

11. The conclusion should to be strengthened to show the policy implications. Further it currently mentions historical heritage, which has not been discussed elsewhere in the manuscript.

Response: Following the comment, we amended the conclusion paragraph as follows:

"Nonetheless, this is the first study that investigated the association between ACEs and health and health behaviour among older people in two countries. In Japan and Finland, the relationship between ACEs and health was similar for SRH, specific diseases and smoking. The impact of ACEs on BMI was stronger in Finland than in Japan. These results suggest a notable association between ACEs and health among older people, and that this association remains consistent even in countries with a different social environment. Based on these findings, health policy to address ACEs is needed to prevent future diseases among older adults." (Page 29)

Overall points:
• The manuscript would benefit from a thorough proof read.
• There are two reference lists and the original should be deleted.
• Tables needs tidying up (e.g. consistency in decimal places, capitalisation of headings/subheadings) and clarity in table titles and footnotes.

Response: These points were amended.

Reviewer: 1
Reviewer Name: Cynthia Levine
Institution and Country: Northwestern University, USA
Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below
Thank you for the opportunity to review the revised manuscript, which I think has been improved. I had the following remaining comments:

1. I appreciated the expanded review of differences between FInland and Japan, but I thought it would also be useful to discuss why or why not these differences might matter for the relationship between ACEs and health (i.e., to connect these differences to the content of the paper). For instance, the authors note that there are differences in immigration into the two countries. Would immigrants or the presence of immigrants in a country be more likely to make ACEs matter for health?

Response: We agreed, we added more hypothesis or rational of the comparison of Japan and Finland as follows:

“In summary, both Japan and Finland are developed and egalitarian countries, but their differences in terms of inequality, immigration percentage, divorce rate, or policy on corporal punishment might contribute to the differential impact of ACEs on diseases in older adults. For example, deetiolated social capital due to inequality23 might contribute to stronger impact of ACEs on health in older adults.” (page 8)

2. I also appreciated that the revised manuscript does more to situate the current work in the past literature by explaining how Japan and Finalnd differ from the US and UK (where most past work has been conducted). However, again, I thought it would strength the manuscript to explain more about why those differences might matter or not (similar to point 1 above). Relatedly, I still thought the paper could do more to address the question of why Japan and Finland important countries to study. The authors do explain that Japan and Finland have less inequality than the US and UK, but it might help to explain how inequality relates to the relationship between ACEs and health. I actually think it's really important to not assume the research findings from the US and UK automatically extend to other countries (sometimes findings from one country don't replicate in another), so I really appreciate the research described in this paper, but I think it would be useful to articulate its importance even more in the introduction.

Response: Following the comments, we added the importance of this study, using Japan and Finland, as follows:

“Further, to address the impact of ACEs on health of older people, it is also crucial to elucidate the commonality of the association, because the pathways linking childhood adversities with adult health are likely to be dependent on cultural or social environments.5-7 Therefore, a comparison of countries with different cultural and/or social environments in childhood, but with similar welfare state regimes, may provide further understanding of the underlying mechanisms of ACEs and older adult health. According to a systematic review, most of the recent studies evaluating the impact of multiple ACEs on health were performed in the United States (US) and the United Kingdom (UK), with only a few studies conducted in other countries,8 such as Asia9 or Nordic countries.10 11 Because the US and UK are developed countries with high inequality, there is a need to confirm the association in developed but relatively equal, egalitarian countries, such as Japan or Finland.” (Page 5-6)

3. The abstract notes, "Another limitation of this study is that the pooled data of the two countries were not accessible, and therefore interactive effects of the countries and ACE on adult health were not clear." This addresses a question that I had about a previous version of the manuscript. However, I did not see this limitation mentioned in the text of the manuscript, unless I missed it. I think that it would be important to note in the manuscript itself.

Response: Following the comments, we added the point in limitation as follows:

“Finally, we were unable to pool the data of the two countries due to restriction on the Finnish data, and therefore interactive effects of the countries and ACE on adult health were unclear.” (page 29)

Minor points:
- In the second to last paragraph of the introduction before the methods, there are 2 periods after the word Finland.
- On page 14, there is a sentence that reads, "Regarding working status, those never worked was not included . . ." It should be were not included

Response: Following the comments, we amended.

Reviewer: 3
Reviewer Name: Katie Hardcastle
Institution and Country: Public Health Wales, Wales
Please state any competing interests or state ‘None declared’: None declared.

Please leave your comments for the authors below
Thanks to the authors and the editors for the opportunity to review the revised manuscript. Whilst I can appreciate that the authors have responded to many of the comments provided by myself and other reviewers, in many cases I felt that the manuscript could benefit considerably from more detailed revisions, rather than simple references to key terms in the text. For me, the following still require more clarity:
1. Research objectives – The title of the manuscript frames it as a comparative study, however comparing the two countries does not appear to be indicated as a research objective (instead, at the foot of the introduction, the authors refer to examining the associations IN each country). As highlighted by all three reviewers, some of the challenges as a reader of this manuscript come from the way in which similarities and differences between the two countries are discussed. Ultimately, it is not clear why these two countries are combined in this manuscript and more justification is needed. Whilst authors have now indicated that they were unable to pool the data from the two countries (in order to use the desired method of analysis in which country is included as a variable in multivariate analyses or an interaction term is used), this remains a weakness of the manuscript. I wonder if the authors have considered whether it would be more appropriate to consider data from either Japan or Finland and focus more on the use of an older adult sample as the ‘added value’?

Response: Following the comments, we reconstructed the significance of this study in Introduction, and added that we could not pool the data in limitation as follows.
“Corporal punishment is not forbidden in Japan, but in Finland, it has been prohibited by law since 1983. In summary, both Japan and Finland are developed and egalitarian countries, but their differences in terms of inequality, immigration percentage, divorce rate, or policy on corporal punishment might contribute to the differential impact of ACEs on diseases in older adults. For example, deetiolated social capital due to inequality23 might contribute to stronger impact of ACEs on health in older adults.
Therefore, the purpose of this study was to examine and compare the association between ACEs and adult diseases, including unhealthy behaviours such as smoking, in older adults in Japan and Finland.” (page 7)

“Finally, we were unable to pool the data of the two countries due to restriction on the Finnish data, and therefore interactive effects of the countries and ACE on adult health were unclear.” (page 29)

2. Methods – The authors are commended for improvements to the methods section. However, in my view the section still does not clearly identify this as an analysis of secondary data. There is a continued need to describe the included studies/data sources more consistently, which would really go a long way to improving clarity for the reader (and making sense of some of the ways in which data analysis is restricted – as above). I would also like to see authors revisit key aspects that still appear to be missing, such as the handling of missing data for Finland.

Response: Thank you very much for raising important point, we added the following sentences:

“For this study, all FPS study respondents were aged ≥60 years, and those who provided information on any ACEs, SRH, BMI, and smoking were selected (n=7,169).” (page 10)

“Of them, those in the oldest age group (64–68 years) who provided data on any ACEs, SRH, BMI, and smoking were selected (n = 3,184).” (page 10)

3. Data selection - If the data from the two countries are to be used together, could the authors consider ways in which they could improve the alignment of the two data sets? For example, the choice to use different age ranges for the two samples (i.e. not using 60+ for both). As those who have never worked are excluded from the FPS, could the authors also exclude never worked from the other sample (rather than combining with retired)?

Response: Because JAGES inclusion criteria is 65+ years old, and FPS have very few sample aged 65+, we used different age criteria. And HsSSup had never worked, and these variables were adjusted, we consider that this is appropriate data selection.

4. Differences between study populations – Authors now frequently mention ways in which the samples MAY differ from the general populations in Japan and Finland. Rather than surmising, could the authors look for any national representative data to reference this?

Response: To the best of my knowledge, JAGES is the only study which investigate ACEs among older adults. Thus, it is not possible to use other representative study in Japan, because such studies do not include ACEs.

5. Based on the data provided, authors should take care in referring to the association between ACEs and BMI in Japan and may want to consider re-wording.

Response: We agree the comments, thus the result description on the association between ACEs and BMI in Japan was rewarded as follows:

“BMI was positively associated with each type of ACE, and with the number of ACEs in Finland. This positive association was also significant in Japan.” (Page 20)

6. Discussion – I would like to see authors support some of their discussion and suggested mechanisms with evidence. For example, different findings for self rated health have been linked to differences in prevention and detection of child abuse and intimate partner violence between Japan and Finland. Is there any evidence to support this? The same applies to the discussion about cancer.

Response: Following the comment, we amended the explanation on the mechanism as follows:
“Considering that the prevalence of intimate partner violence in Japan was 0.1%, while 0.25% in Finland in the OECD report,32 this result can be interpreted as the detection of intimate partner violence may not be as adequate in Japan compared with Finland, and thus the problem might be unresolved and showed stronger impact for SRH in older age.” (page 23)

“Childhood poverty in Japan might lead to poverty in older age,29 which may result in delays in medical check-ups or consultations. Previous study has shown that adults with lower health literacy, which is more likely to happen among those living in poverty, are less likely to use healthcare services in Japan.34” (page 24)

7. Limitations – Thank you to the authors for being more explicit with limitations and including many of those suggested by reviewers. Often, however, there is a lack of detail and the implications of these limitations are not discussed. For example, authors outline the data being limited to three ACEs as a limitation – could they expand on why this is a limitation and how it many affect the findings and interpretation? Why might those with a disease be more likely to recall ACEs (as the authors

suggest)? Is there any evidence to support this? Many limitations now actually appear in the methods section, which could be lifted to the suitable section. There are multiple instances in which limitations are directly duplicated even within this section.

Response: Following the comment, more detailed explanation on limitations were added as follows:

“There are several limitations to this study. First, this was a cross-sectional study conducted among older adults. As ACEs were self-reported, differential recall bias cannot be ruled out. Recent review showed poor agreement between prospective and retrospective assessment of childhood maltreatment.47 However, in contrast, other review studies reported that the validity of retrospective assessment of ACEs is acceptable.48 49 Second, these results, although based on harmonised measures, were derived from only two egalitarian developed countries, which preclude the generalisation of the findings to other countries. Further studies are warranted to investigate the association between other ACEs and adult disease in different cultural settings, and in low- and middle-income countries. Third, we did not assess other ACEs such as sexual abuse, neglect, childhood neighbourhood deprivation, or family disfunction (i.e., mental disorder of a family member, or incarcerated family member), and thus the number of ACEs were limited to only three. The limited number of ACEs precluded to assess stronger impact of ACEs on adult diseases, as a previous meta-analysis revealed.8 Further studies are necessary to investigate the impact of other ACEs on the health of older adults. Fourth, there is a disparity in the measurement of ACEs and health, and ACEs and health behaviours, across the JAGES, FPS, and HeSSup. The differences in measurement might result in heterogeneity of the results. More specifically, the assessment of fear of a family member in the JAGES, FPS and HeSSup was different, which may result in heterogeneity between study estimates. Fifth, the participants in the JAGES did not have functional disability, and hence might be healthier than the average Japanese older population. Therefore, the results of the study might be underestimated. Alternatively, the results might be subjected to survival bias. The average age of the study participants was 69.5 years in Japan and 64.4 years in Finland. People who passed away before the current study from ACE-related diseases or health risk behaviours would not have been included in the current study. Therefore, the results of the current study may underestimate the health effects of ACEs. Further, FPS was a not representative sample, thus the prevalence of diseases, such as cancer, may be different from other studies. Sixth, considering other covariates such as levels of inequality, current and previous household income, environmental risks (e.g., parental smoking), or genetic variation was not feasible due to data availability. In fact, education level was higher in the Finnish sample than in the Japanese sample. Finally, we were unable to pool the data of the two countries due to restriction on the Finnish data, and therefore interactive effects of the countries and ACE on adult health were unclear.” (page 26-28)