

From: [UNSW Research Integrity](#)
To: [Mohamed Helmy](#); helmy.m@protonmail.com
Cc: [UNSW Research Integrity](#); [PVC Track In Insight CRM](#)
Subject: ALM-000002684 – Further queries regarding Preliminary Assessment
Date: Friday, 8 April 2022 3:42:31 PM

CONFIDENTIAL

Dear Dr Helmy,

RE: Preliminary Assessment into alleged breaches of *UNSW Research Code of Conduct*
Our Ref: ALM-000002684

I hope that this email finds you well.

As I have previously advised, I am currently examining the material relevant to this matter. In particular, I have carefully read the relevant sections of your report titled 'Report on Research and academic misconduct by STROKOG member and others in Singapore' (the **Report**) dated 18 January, 2021.

My current inclination is to recommend to the Designated Officer that they consider exercising their discretion to dismiss the complaint under section 3.7 of the *UNSW Research Misconduct Procedure*. The purpose of this email is to outline my reasoning and to provide you with the opportunity to make any submissions in reply.

You may recall the allegation tables previously sent to you on Thursday 21 May 2021 by my colleague, Dimitrios Pandioras, which referred to 11 allegations, based upon clarifications that you provided by email on 23 March 2021. What follows below is my review of each allegation, including the basis upon which I consider that each allegation should be dismissed.

- 1. Allegation 1: a) Misleading content and/or conclusions in publication, and b) the paper is a duplicate publication regarding Yatawara et al. 2020b: Yatawara C, Guevarra A, Ng KP, Chander R, Kandiah N. Interactions Between Acute Infarcts and Cerebrovascular Pathology Predict Poststroke Dementia. *Alzheimer Dis Assoc Disord.* 2020 Jul-Sep;34(3):206-211. doi: 10.1097/WAD.0000000000000384. PMID: 32483018.**

Your concerns regarding Allegation 1a were outlined in the Report on p. 41, section 13. However, this section of the Report merely contends that several papers published by Kandiah "are at best poorly aligned, at worst self-contradictory". Conflicting, poorly aligned, or even contradictory results *between publications* do not amount to misleading dissemination of research under the *UNSW Research Code of Conduct*. Principle 1 (Honesty) found in the *UNSW Research Code of Conduct* requires UNSW researchers to truthfully and accurately report their results, even if these results conflicted with their own prior studies or conclusions.

Allegation 1b outlined on p. 38 of your Report requires a review of Yatawara et al. 2020b. My review revealed that the study participants, design, and analysis were distinct from previous papers. Moreover, plagiarism detection software did not identify any overlap

with previously reported results. Accordingly I find that Yatawara et al. 2020b does not duplicate or plagiarise Yatawara et al. 2020a or Yatawara et al. 2018b.

2. **Allegation 2: a) Misleading content and/or conclusions in publication, and b) the paper is a duplicate publication; and c) falsifying research data regarding, Yatawara et al.**

2020a:

Yatawara C, Guevarra AC, Ng KP, Chander R, Lam BYK, Wong A, Mok V, Kandiah N. The role of cerebral microbleeds in the incidence of post-stroke dementia. J Neurol Sci. 2020 May 15;412:116736. doi: 10.1016/j.jns.2020.116736. Epub 2020 Feb 15. PMID: 32088471.

My views on Allegation 2a are the same as for Allegation 1a above.

Allegation 2b, like Allegation 1b, is contained on p. 38 of your Report. My views regarding Allegation 2b are similar to Allegation 1b. There are clear differences between the studies and there is insufficient textual overlap that could amount to plagiarism or duplicate publication. Moreover, I note that Allegation 2c contradicts Allegations 1b and 2b namely that Yatawara et al. 2020a duplicates Yatawara et al. 2020b.

Allegation 2c is based on p. 40, section 11 of the Report, clarified by your email of 23 March 2021 where you claim under the header of “Misrepresentation and putative falsification in Yatawara et al. 2020a” that the use of Boston criteria by Yatawara et al. 2020a was inappropriate because Boston criteria were applied to post-mortem brains in a prior study (Knudsen et al. 2001). As you wrote in your email of 23 March 2021, “In other words, data in Yatawara et al. 2020a (falsely) applies post-mortem Boston criteria to (unknown) imaging criteria in living persons.” While I was able to confirm that Yatawara et al. 2020a do cite Knudsen et al. 2001 (doi: 10.1212/wnl.56.4.537), Knudsen et al. 2001 do not claim that the Boston criteria can only be used in post-mortem tissue. In fact, they state that they “performed clinical-pathologic correlation” and conclude “the diagnosis of probable CAA can be made **during life** with high accuracy” [emphasis mine]. Moreover, according to Greenberg and Charidimou 2018 (doi: 10.1161/STROKEAHA.117.016990), “Introduction of the imaging-based Boston criteria for diagnosis of CAA in the 1990s allowed a diagnosis of probable CAA in **living patients with no available brain tissue** and substantially moved the field from the pathologist’s realm to the clinicians” [emphasis mine]. Accordingly I find this allegation is misconceived.

3. **Allegation 3: The paper is a duplicate publication, regarding Wong et al. 2020a: Wong FCC, Yatawara C, Low A, Foo H, Wong BYX, Lim L, Wang B, Kumar D, Ng KP, Kandiah N. Cerebral Small Vessel Disease Influences Hippocampal Subfield Atrophy in Mild Cognitive Impairment. Transl Stroke Res. 2021 Apr;12(2):284-292. doi: 10.1007/s12975-020-00847-4. Epub 2020 Sep 7. PMID: 32894401.**

This allegation is contained on p. 38 of your Report. I have reviewed the above cited paper and can identify clear differences between it and other publications, for example, in the design of the study and its participants and results. Furthermore, plagiarism detection software does not indicate any substantial textual overlap that would indicate plagiarism or a duplicate publication.

4. **Allegation 4: a) Misleading content and/or conclusions in publication, and b) the paper**

**is a duplicate publication regarding following paper, Low et al. 2019b:
Low A, Ng KP, Chander RJ, Wong B, Kandiah N. Association of Asymmetrical White Matter Hyperintensities and Apolipoprotein E4 on Cognitive Impairment. J Alzheimers Dis. 2019;70(3):953-964. doi: 10.3233/JAD-190159. PMID: 31306121.**

Regarding Allegation 4a, your concerns are outlined in the same section of your Report (p. 41, section 13) as Allegations 1a and 2a. The only reference to Low et al. 2019b on p. 41 simply describes its result. There is no evidence of any misleading reporting within Low et al. 2019b, nor would differences between Low et al. 2019b and other studies amount to misleading conduct or dissemination of research.

As for Allegation 4b, I have reviewed Low et al. 2019b and neither I, nor plagiarism detection software, can identify sufficient overlap in text or within the paper (including its design and analysis), to indicate that it is a duplicate publication. Moreover, the discussion on p. 41 of your Report, where you summarise its findings (section 13.v), demonstrates its differences with other papers – for example, it is the only paper in your Report which you have cited as referring to asymmetry.

5. **Allegation 5 was dismissed at your request, based on your emails of 27 May 2021 and 11 June 2021.**
6. **Allegation 6 was dismissed at your request, based on your emails of 27 May 2021 and 11 June 2021.**
7. **Allegation 7: a) Misleading content and/or conclusions in publication, and b) the paper is a duplicate publication regarding following paper, Guevarra et al. 2020:
Guevarra AC, Ng SC, Saffari SE, Wong BYX, Chander RJ, Ng KP, Kandiah N. Age Moderates Associations of Hypertension, White Matter Hyperintensities, and Cognition. J Alzheimers Dis. 2020;75(4):1351-1360. doi: 10.3233/JAD-191260. PMID: 32417773.**

Allegation 7a is outlined in the same section as Allegations 1a, 2a, and 4a, i.e. p. 41 of the Report. However, section 13.iii does not identify what is allegedly misleading within Guevarra et al. 2020, only that it found “poor cognitive performance *is associated* with hypertension” [emphasis from Report]. I refer to and repeat my finding as for Allegation 1a, 2a, and 4a.

Allegation 7b is outlined in the same section as Allegations 1b, 2b, and 4b. In your email of 23 March 2021, you indicated that Guevarra et al. 2020 duplicates Kumar et al. 2020 (doi: 10.1089/brain.2020.0784) and Low et al. 2019b. As indicated previously, I cannot identify sufficient overlap in aspects of the paper, including the text, design, or analysis, that would support a finding that it is a duplicate publication.

8. **Allegations 8, 9, and 11: Misleading conclusions and duplicate publication regarding Lo et al. 2019, Lo et al. 2020, and Sachdev et al. 2017:
Lo JW, Crawford JD, Desmond DW, Godefroy O, Jokinen H, Mahinrad S, Bae HJ, Lim JS, Köhler S, Douven E, Staals J, Chen C, Xu X, Chong EJ, Akinyemi RO, Kalaria RN, Ogunniyi A, Barbay M, Roussel M, Lee BC, Srikanth VK, Moran C, Kandiah N, Chander RJ, Sabayan B, Jukema JW, Melkas S, Erkinjuntti T, Brodaty H, Bordet R, Bombois S, Hénon H,**

Lipnicki DM, Kochan NA, Sachdev PS; Stroke and Cognition (STROKOG) Collaboration. Profile of and risk factors for poststroke cognitive impairment in diverse ethnoregional groups. *Neurology*. 2019 Dec 10;93(24):e2257-e2271. doi: 10.1212/WNL.00000000000008612. Epub 2019 Nov 11. PMID: 31712368; PMCID: PMC6937495.

Lo JW, Crawford JD, Samaras K, Desmond DW, Köhler S, Staals J, Verhey FRJ, Bae HJ, Lee KJ, Kim BJ, Bordet R, Cordonnier C, Dondaine T, Mendyk AM, Lee BC, Yu KH, Lim JS, Kandiah N, Chander RJ, Yatawara C, Lipnicki DM, Sachdev PS; STROKOG Collaboration*. Association of Prediabetes and Type 2 Diabetes With Cognitive Function After Stroke: A STROKOG Collaboration Study. *Stroke*. 2020 Jun;51(6):1640-1646. doi: 10.1161/STROKEAHA.119.028428. Epub 2020 May 14. PMID: 32404039.

Sachdev PS, Lo JW, Crawford JD, Mellon L, Hickey A, Williams D, Bordet R, Mendyk AM, Gelé P, Deplanque D, Bae HJ, Lim JS, Brodtmann A, Werden E, Cumming T, Köhler S, Verhey FR, Dong YH, Tan HH, Chen C, Xin X, Kalaria RN, Allan LM, Akinyemi RO, Ogunniyi A, Klimkowicz-Mrowiec A, Dichgans M, Wollenweber FA, Zietemann V, Hoffmann M, Desmond DW, Linden T, Blomstrand C, Fagerberg B, Skoog I, Godefroy O, Barbay M, Roussel M, Lee BC, Yu KH, Wardlaw J, Makin SJ, Doubal FN, Chappell FM, Srikanth VK, Thrift AG, Donnan GA, Kandiah N, Chander RJ, Lin X, Cordonnier C, Moulin S, Rossi C, Sabayan B, Stott DJ, Jukema JW, Melkas S, Jokinen H, Erkinjuntti T, Mok VC, Wong A, Lam BY, Leys D, Hénon H, Bombois S, Lipnicki DM, Kochan NA; STROKOG. STROKOG (stroke and cognition consortium): An international consortium to examine the epidemiology, diagnosis, and treatment of neurocognitive disorders in relation to cerebrovascular disease. *Alzheimers Dement (Amst)*. 2016 Nov 18;7:11-23. doi: 10.1016/j.dadm.2016.10.006. PMID: 28138511; PMCID: PMC5257024.

Your email of 27 May 2021 suggests to me that you agree that these allegations should be dismissed because you expressed admiration for Lo and Sachdev, praising them as “forthright”, claiming that they “acted ethically and scientifically”, and describing the articles covered by these allegations as “important contributions to the scientific record” and “of exemplary nature and exceptional quality”.

My own review of these papers and the relevant sections of your Report indicate to me that the inclusion of COAST and NNI studies by Lo, Sachdev, and colleagues could not possibly be construed as misleading because:

- a. On p. 39 of your Report, Dong et al. 2012 (doi: 10.1136/jnnp-2011-302070) is mentioned as “a key reference for COAST” used by STROKOG. However, section 4 on p. 39 of your Report only summarises Dong et al. 2012. Dong et al. 2012 is never mentioned again.
- b. On p. 39 of your Report, Kandiah et al. 2011 (doi: 10.1016/j.jns.2011.07.013) is mentioned as a “a key reference for NNI” used by STROKOG. However, the criticisms of Kandiah et al 2011 amount to, at most, differences of scientific opinion and interpretation.
 - i. For example, in section 6 on p. 40 of your Report, you cite Debette and Markus 2010 (doi: 10.1136/bmj.c3666) to argue “that white matter hyperintensity was (is?) an incidental finding, meaning MRI is not prescribed or indicated for the putative presence of white matter

hyperintensities”. However, Debette and Markus 2010 explicitly identify both clinical and research implications, “In terms of research, white matter hyperintensities could constitute a potentially useful intermediate marker for the identification of new risk factors for stroke and dementia.” Since Kandiah et al. 2011 were doing research, studying white matter hyperintensities using MRI appears to be a reasonable course of action.

- ii. Sections 7-8 (p. 40) of your Report reference Fazekas et al. 1987, but do not explain why Kandiah et al. 2011 is misleading. Even if using MRI to study dementia over 20 years after Fazekas et al. 1987 stated that reports on the topic were “anecdotal and controversial” this does not make the approach misleading – it would in fact be exactly what Fazekas et al. 1987 called for, “More information is needed to evaluate fully the potential of MR in the diagnosis of dementia”.
 - iii. Section 9 (p. 40) of your Report goes on to criticise Kandiah et al. 2011 for not defining DSCH, but this error appears to be, at worst, typographical. It is clear from cross-referencing the DWMH statistics in Table 2 with the DSCH statistics in-text on p. 94 of Kandiah et al. 2011, that these are the same data.
 - iv. Similarly, other sections make criticisms of Kandiah et al. 2011, but even if these were substantiated, they would not amount to misleading conduct. For example, the arguments in section 10, p. 40 of your Report about whether “white matter disease” is correct terminology (and therefore the consequences for data inclusion/exclusion), would depend on the audience and context of the word ‘disease’. ‘Disease’ can be reasonably understood to have meanings varying from any kind of pathology to specific conditions defined by diagnostic standards, such as the World Health Organization’s International Classification of Diseases. Moreover, a study of white matter disease that excluded participants with white matter disease (as section 10 seems to suggest), could not exist.
- c. Therefore, I believe that the criticisms outlined in your Report amount to, at most, honest differences in judgement or unintentional errors. In my view they could not constitute a breach of the Research Code of Conduct.

Finally, I have reviewed Dong et al. 2012 and Kandiah et al. 2011 for duplicate publication. I did not find sufficient overlap in aspects of the paper including the text, design, or analysis, that would indicate that it is a duplicate publication.

9. Allegation 10 was dismissed at your request, based on your emails of 27 May 2021 and 11 June 2021.

In summary, my careful review of the relevant publications and literature, together with the results from plagiarism detection software do not produce any evidence to support the allegations that you have submitted. If you have further information that you consider substantiates your allegations **please provide me with your response by Wednesday, 20 April 2022**. If I do not hear from you by this date, I will assume that you do not have further information and I will proceed to contact the Designated Officer with my recommendation to dismiss the complaint.

Yours sincerely,

Shaun Khoo
Senior Case Manager

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