

18 December 2017

Editor-in-Chief  
World Journal of Nephrology

Dear Editor-in-Chief,

**Re: Diabetic muscle infarction in end-stage renal disease: A scoping review on epidemiology, diagnosis and treatment (Manuscript No: 37050)**

We are grateful for the opportunity to resubmit our revised manuscript to World Journal of Nephrology. The manuscript has been revised in light of the helpful comments from the reviewers. We trust that the revision has provided more clarity as suggested.

Our response to the reviewers' comments are as follows:

Reviewer 02844701

1. MeSH terms or key words and the time period when the literature was published have been described (page 4).
2. The limitations of the current review has been written on page 12.

Reviewer 00505314

1. We thank the reviewer for the suggestion to compare the ESRD cohort with DMI and those without ESRD. We have added that comparison in the discussion section.
2. As recommended, calciphylaxis or calcific uraemic arteriolopathy (CUA) as a differential diagnoses to DMI have been discussed (page 10).

Reviewer 00503199

We thank the reviewer for the encouraging feedback.

Reviewer 02874819

1. The aim of the review has been clarified in the Abstract. Additional clarity has been provided regarding the focus of this review in the introduction section.
2. The incidence of DMI in patients with ESRD has not been determined and cannot be determined from a review of case reports and small case series. This has been explained in the discussion section.
3. We have explained how microvascular complications may have a role in the pathogenesis of DMI. It is not our intention to establish any definite

pathophysiological link between DMI and ESRD. Instead, the review aimed to highlight possible differences in characteristics or outcomes among ESRD patients with DMI.

4. The pathophysiology of DMI, particularly among patients with ESRD, has been explored utilising currently available data but we acknowledge that further research is still needed in this area.
5. We agree with the reviewer that there are potentially different characteristics and implications of DMI in the patients on dialysis and recipients of kidney transplant. We have drawn attention to these two different groups of patients in the presentation of data but the frequency of DMI in kidney transplant recipients is very low which limits analyses.
6. The two recommended case reports have been cited in the discussion section.

We would like to point out that the submitted manuscript is a scoping review and does not contain any statistical analyses requiring certification from a biostatistician. Likewise, the PRISMA checklist does not apply. In addition, both authors are native English speakers and hence we seek exemption from certification of language editing for non-native speakers.

We trust that the manuscript is now acceptable for publication in World Journal of Nephrology. We thank you again for the opportunity to revise this manuscript.

Yours faithfully,

Dr Tuck Y Yong  
Consultant physician