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Interactive comment on "The Oxic Degradation of Sedimentary Organic Matter 1.4 Ga Constrains Atmospheric Oxygen Levels" *by* Shuichang Zhang et al.

D. E. Canfield

dec@biology.sdu.dk

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In what follows, we offer a brief response to the main criticisms of Reviewer 2. The main criticism of this reviewer is reproduced below:

"This paper aims to determine oxygen levels prevailing in oceans 1.4 Gyr ago. This topic is of interest and debated as evidenced by the numerous publications in highrank journals. As stressed by the authors, the use of the chromium isotopes as a redox indicator can be discussed implying the need for complementary or new approaches as the one published by the same authors in PNAS (Zhang et al., 2016). However, this manuscript shares the same conclusion than Zhang et al., 2016. There are too many similarities between the two manuscripts to fully consider this one as a new manuscript.

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Discussion paper

In my opinion, the manuscript must refocus on the approach rather than the already published conclusion."

With all due respect to the reviewer's concerns, we are puzzled by the idea that "there are too many similarities between the two manuscripts[this manuscript and an earlier one by our group published in PNAS] to fully consider this one is a new manuscript" because ".. .this manuscript shares the same conclusion than Zhang et al. 2016." We stress: 1) that there is no consensus on levels of oxygen during the Mesoproterozoic Era. Basically, there are a pair of chromium isotope studies suggesting very low levels of atmospheric oxygen, and our PNAS contribution suggesting much higher levels, 2) that the relationship, therefore, between the history of atmospheric oxygen and animal evolution is currently unresolved, 3) the current manuscript uses a completely different methodology to establish similar oxygen concentrations to the PNAS paper on a completely different section of the Xiamaling Formation, 4) the methodology explored in this manuscript is completely novel and is the first to evaluate the consequences of oxygen exposure on carbon preservation during the Precambrian.

Therefore, we view this manuscript as a novel contribution to an important scientific debate. The fact that our two contributions, utilizing completely different methods, offer similar conclusions as to levels of Mesoproterozoic atmospheric oxygen strengthens the idea that atmospheric oxygen levels were higher than predicted from chromium isotope studies and reinforce the idea that sufficient oxygen for animal respiration was available in the environment long before the evolution of animals themselves.

We might understand the reviewer's concerns if the scientific problem we were exploring was well resolved and trivial. However, this is not the case with the Mesoproterozoic history of atmospheric oxygen and its relationship to animal evolution.

We also offer a brief response to some of the other reviewer's concerns.

The reviewer state "The two studies are only distinguished by (i) the studied geological units and (ii) the determination of oxygen exposure time and penetration depth"

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Discussion paper

Yes, the studies do look at different parts of the Xiamaling Formation, but the methodology used to constrain atmospheric oxygen is completely and fundamentally different. In the PNAS paper we utilized a water column model to determine the minimum levels of atmospheric oxygen required to allow bottom water oxygenation. In the present manuscript we utilize a sediment diagenetic approach to explore the minimum levels of bottom water oxygen required to generate the amounts of carbon oxidation needed to reproduce the HI values we observe in the sediments.

The reviewer states: "Unfortunately and as stressed by the authors (L550), there is no precise dating of unit 1 in the Xiamaling Formation. Hence, the main original conclusion of the manuscript (persistent atmospheric oxygen over million years) is not convincing especially in the light of the alternating gray (oxidizing conditions) and black shales (anoxic conditions; see Zhang etal., 2016)."

Yes, there is no precise dating of unit 1. However, there is a well-dated interval of 8 million years separating unit 3, from which the calculations of oxygen concentration in the PNAS paper were derived, and 220 meter in unit 2. There is a further 180 meter of stratigraphy between the last dated interval in unit 2 and the interval in unit 1 where we make our oxygen calculations in the present paper. Therefore, even if this 180 m of stratigraphy deposited instantaneously (which is completely unreasonable) we observe elevated oxygen over a time interval separated by 8 million years. We agree that we have not determined persistent oxygenation over millions of years (although this is likely) but that oxygen is found in intervals spanning millions of years. We believe that this point is significant and will be better developed in the revised manuscript.

Outside of these comments, the reviewer's comments are very helpful, and together with those of reviewer 1, will help to significantly improve the manuscript.

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