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Interactive comment on "Long-distance electron transport occurs globally in marine sediments" *by* Laurine D. W. Burdorf et al.

Anonymous Referee #3

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The manuscript describes the world wide distribution of cable bacteria in marine sediments. It's a thorough work and interesting to the readers of the journal. Major comments: 1) A major concern is the style of the manuscript. It is written in a very lenghty, prosaic style with numerous repetitions between the materials, results, and discussion parts. The manuscript can be easily shortened to half. 2) The materials, results, and discussion are not well seperated but appear very mixed. Lots of materials are presented in the results etc. 3) The manuscript leaves the impression that LDET is the most dominant type of sulfur metabolism in marine sediments which might be true or not. It should be compared to the classical redox sequence model which has been found almost everywhere until recently and the overall importance of the two models should be discussed. 4) I suggest to not use the term eSOX. The electricity component is added by humans. The bacs don't generate electricity. The fact that humans can

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measure an electric field under very high activities and high resistance of the sediment does not mean that an electric field is always generated in nature. It's a very catchy term but not scientifically correct in my eyes. 5) The data can be presented in a more systematic way like in table 1. I would not present all the detailed figures which all show the same content. This would also contribute to a drastic shortening of the manuscript.

Specific comments: 6) Page 5, line 17-30: repetition 7) P6, l 8-11: repetition 8) P6, l 29-P7, l4: should be in discussion 9) P 7, l 8: what is modified precisely? 10) P8, l 12-17: repetition 11) P8, l 24-31: should be in discussion 12) P9, l 1-9: should be in Materials and is a repetition 13) P9, l 11: I suggest to need abbreviate OPD. Makes text more difficult to read. 14) L 12: what delta pH? Delta between what values? 15) L 14: replace oxygen penetration depth by oxic zone 16) L 15-22: This comparison cannot be done because the gradients and the zones change over time. Even for lab incubations such a comparison would only be feasible if the profiles are measured at distinct times and if it were shown before that the generation of the profile at a certain time is reproducibly and predictable. However, these gradients are dynamic and even collapse after a certain time. The situation is even worse in the field, as the kinetics of the profile formation are not known and the time point of sampling within such a built up or collapse time period is not defined. 17) L 24-30: Materials 18) P9, l 2-14: Materials 19) L 28: repetition 20) L 32-33: delete sentense 21) From here on I stopped indicating that parts belong to Materials or other sections 22) P13, l 9: crucial player: what is crucial? Any estimates of the quantitative importance? 23) L 10: marine sediments: is that only coastal or all? 24) L 8-19: repetition 25) L 23: fine-grained: data on sediment properties lacking in Materials part. A characterization of the sediments of all sites would be good. 26) P 19, l 6-18: This is obviously a new experiment which should be described in materials and in results accordingly. 27) P 19: Conclusion is more a second abstract than a conclusion. 28) Figure 1: unnessassary abbreviations are introduced which are not used at any other place in the manuscript. 29) Figure 5: name sites (1-4) the same as the panels (A-D) 30) Fig. 5: Indications of presense seems arbitrary. I suggest presenting the data in a table to make it comparable. Electron micrographs do not

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show ridges here! 31) Fig. 9: New experiment –> materials? Results?

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