

Interactive comment on “Biogeochemical processes in sediments of the Manfredonia Gulf (Southern Adriatic Sea): early diagenesis of carbon and nutrient and benthic exchange” by F. Spagnoli et al.

Anonymous Referee #3

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Review of Spagnoli et al 2004-11-04

General

I found the paper to be at a relatively immature stage. There are many English mistakes which at times, make the paper hard to follow. In some instances the methods may be questionable or are often not described at all. I also found many of the figures to be poorly prepared, with strange, confusing and I think incorrect scales. The data presented are not well interpreted and the discussions of the data generally lead nowhere, in some instances ending in statements such as ‘we have no data on this’. and this hypothesis needs further study.’

I think the data could warrant publication with a careful and insightful analysis of the

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data, but as it stands the paper offers no new interesting scientific insights. No attempt is made to seriously analyse the data. This could be done on an ecosystem scale. For example what might the significance of benthic nutrient regeneration be to the gulf of Manfredonia? Or on a more local biogeochemical level. How do calculated diffusive fluxes match the measured fluxes for example? What are the respiration quotients of the system? Can Alkalinity fluxes help explain any imbalance? Analysis of this aspect might shed some light on the importance of suboxic mineralization pathways which the authors mention. If bio-irrigation is the main transport mechanism as the authors claim in the abstract with no evidence, then the measured O₂ fluxes should be well excess of the O₂ fluxes calculated from the diffusive O₂ profiles. These are just some examples of how the authors might get the most out of their data set and come to some interesting conclusions.

Specific and technical comments

Title Ę: early diagenesis of carbon and nutrientsĘ

Abstract Line 1. Ęresponsible for the recyclingĘ L 20 It is well known L10 coexistence Study area L3 Weastern (eastern or western?) L4 In the southern Adriatic, however, open watersĘ L8 Is there any permeability data for the sandy sediments? This may be relevant given the current interest in permeable sediments.

Methods L26 why are the Eh data not shown? L3 The process used to remove pore-water from the sediment most likely resulted in some loss of TCO₂, particularly at low pH values likely to be encountered in the sediment. Do the authors have any data supporting the validity of this approach? L16,17 Deployed? Ęat each site to replicate measurements L22 Method for Cs analysis? L1. ‘Organic N was assumed to equal total nitrogen.’ No total N or organic N data are shown. What was the method used? L7 I think these ions were determined by ion chromatography? What was the column/instrument used. L9 parameter L14 two deployments

Results and discussion

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Solid phase measurements I suggest separating the results and discussion as is usually done. L24 (Fig 3a) decreased exponentially below the interface at both stations, and L26 Applying a constant L5 It is well known L7 maximum concentrations.. L9 Thorium activities were also used as a tracer L18 from the sediment water L22 at about 20 cm at both sites L29 similar to those calculated L1 this sentence is not clear.

Porewater profiles L6 anoxic conditions occur within a few cm depth. L7,8 Thus the sedimentary L9 O2 profiles suddenly appear, they are not mentioned in the methods. Where they in situ or ex-situ? L10 and at depths L10 the authors term depths below 1cm suboxic where the concentrations of O2 are low. I would rather refer to the zone where Mn and Fe reduction occur, below the oxic zone as suboxic, as the authors themselves later do. L13 To me, it appears as if nitrification is occurring in the anoxic zone of the sediment the way the graph is currently labeled. L14 I agree denitrification is the most likely NO3 consuming process, but, anammox, dissimilatory nitrate reduction to ammonium and nitrate assimilation by bacteria are also possible processes. In the absence of any measurements, I don't think this can be referred to as denitrification. L18 If the data is questionable then it should not be presented. It is also possible the nitrate came from the cellular pool within the sediment see for example

“Rysgaard S, Glud RN, Risgaard Petersen N, Dalsgaard T (2004) Denitrification and anammox activity in Arctic marine sediments. *Limnol Oceanogr* 49:1493-1502”

which very nicely illustrates this phenomenon.

L22 that yield L25 .. is observed at site S2. L27 maximum. L27 at greater depth.. L3-4 These lines are very unclear and need to be reformulated. L7 of these metals under anaerobic conditions.. L8 The pore water SO4 2- profiles show rather constant trends and display L12. Again reference is made to denitrification with no evidence. L13 use “consistently” instead of “monotonically”. L18 “increase” instead of “improve” L18 we have no L20 the word diffuses upwards is used. If the authors claim that

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bioirrigation is taking place then advection is probably the dominant process. Perhaps “transported” would be a better term. L21-22. The authors claim most of the NH_4^+ is oxidized before it reaches the sediment water interface. I see no evidence for this in the ammonia profiles. Some sort of flux calculation based on the porewater profile might be useful here (at least to get a minimum flux, bioirrigation will of course increase the fluxes. A comparison of diffusive and total O_2 fluxes would again be useful here to get an idea of the likely impact of bioirrigation and enhanced biodiffusion (sometimes called bioturbation) on the total fluxes). I also think some sort of brief comparison of the N efflux with that expected from redfield stoichiometry might be appropriate here, at least to get some sort of an indication of the remineralisation of N with respect to C. L31 In summary

Chamber data L14 use “rather” instead of “pretty” (colloquial) L18 Ammonia, TCO_2 and PO_4 fluxes. L19 The term degradative organic matter products is very loose! Be more specific. L20 The term significant is used without reference to a statistical test. This term is usually associated with statistical tests, better to use another term such as greatly. L22 Current data on.. L26 at station

Conclusion L5-10. The authors make conclusions about the processes taking place although none have been measured! I agree there is evidence for denitrification, but it is just assumed to occur. Sulfate reduction could also be taking place at low rates deeper within the sediment.

Fig 2 As the caption stands there should be 10 data points, there are more than this.

Fig 5 same comment as for fig 2. The scale on this graph is very strange and not easy to read.

Fig 6 This figure is extremely unclear and I would suggest it be broken up so that not so many variables are on the same axis. As I understand it the O_2 fluxes are extremely high $\sim -50 \text{ mol m}^{-2} \text{ d}^{-1}$! Define nutrients in the caption The sulfate fluxes as shown are nonsense $\sim \text{nmol m}^{-2} \text{ d}^{-1}$. How can this be meaningfully measured with a sulfate

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background concentration of 24mM? I would omit these fluxes, unless the authors have a good reason to include them. What is the error associated with each measurement. It is customary to show the uncertainty in the flux based on the standard error of the linear regression slope. How do the O₂ TCO₂ fluxes balance? What about alkalinity data?

Interactive comment on Biogeosciences Discussions, 1, 803, 2004.

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1, S339–S343, 2004

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