Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-206-AC1, 2017

Interactive comment on "Modelling biogeochemical processes in sediments from the north western Adriatic Sea: response to enhanced POC fluxes" *by* Daniele Brigolin et al.

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Received and published: 27 July 2017

Anonymous Referee #1

The present version of the manuscript is relatively well written and the objectives are well stated. The subject is very interesting and the approach used may be appropriate. However, the methods are not clearly detailed enough to allow the reader to appropriately follow what is done and how. In addition, there are many points on the form that are confusing: the use of different parameters to define (potentially) the same thing that may have different meaning in the literature (e.g., POC, Corg, OC%,...). The caption of the figures/tables that are not in agreement with what is written in the text or what

is seen on the figure/table,... I tried to address most of them in the specific comment section below.

We thank the reviewer for all the detailed and useful comments made. Our answer to the three general points, and to specific points related are reported below. We tried to track within this document most of the changes performed on the text, when this was not possible, changes were implemented only on the manuscript (ms), and reference reported in the present document. Reference to the initial submission and to the updated version of the manuscript were detailed. When not stated, we imply reference to the revised version. The revised version of the ms is provided as a supplement.

Overall, my most important comments concern: 1) the way bioirrigation is accounted in the early diagenetic model, which is one of the two parameters used to calibrate the model With respect to this point, in the new updated version of the manuscript (ms), the following elements were included: - The formulation used to define the profile of the bioirrigation rate has now been added in Table 2 (formulation for bioturbation was also specified in Table 2); - A sentence pointing to the methodology used to include bioirrigation in the BRNS tool, used for developing the EDM was added in the methods section (ln 7-11, pg 4); - A plot showing the shape of bioirrigation rates in the two profiles was included in the appendix (Appendix, Figure A3)

2) the representativity of one porewater profile (and of 1 or 2 porewaters samples) to entirely support the bioirrigation process

We acknowledge that 2 cores cannot define very strongly bio-irrigation in this area, but, as this process is everywhere in coastal sediments, and we report numerous macrofauna, bio-irrigation should be active there. In order to support this point, as suggested by the reviewer, we included in the text a list of species which were found at the two stations within the thesis work by Colla (2017), and that we expect to play an active role in bioirrigation. Bioirrigators were ranked based on the reworking potential of each taxa, according to the approach proposed by Solan et al. (2004).

We would like to remark that two sets of profiles indicating the same type of distribution were available for each core: DIC and NH4, moreover the effects could be corroborated by the shape of the profile of another chemical species (SO42-), available from the same cores. This was stated in the ms (page 9 line 15, previous ms version):"The shape of the DIC–NH4+ profiles indicates bio-irrigation (Meile et al, 2001; Canavan et al., 2006), although the deeper increase is not visible in our data profiles due to limited penetration of the cores. Indeed, the very limited increase in concentration profiles in the first centimeters can only be linked to input of bottom water with lower DIC and NH4+ by irrigation, given the large recycling intensity in surface sediments as exemplified by O2 profile."

Based on the reviewer comment, we also edited the conclusions, in order to communicate that uncertainty on the coefficients could be large, due to the lack of data. The sentence is the following: "Indeed, the early diagenesis model calibration led to an estimation of enhanced and more shallow bioirrigation underneath the farm which were confirmed by independent data on macrofauna composition collected at the study site. We remark that, based on the number of cores available, it was not possible to assess quantitatively the uncertainty related to these coefficients, which estimation would allow to characterize more strongly bioirrigation in this area."

Solan, M., Cardinale, B. J., Downing, A.L., Engelhardt, K.A.M., Ruesink, J.L., Srivastava, D.S., 2004. Extinction and Ecosystem Function in the Marine Benthos. Science 306, 1177-80.

3) the representativity of the measured O2 profiles and derived diffusive flux carried out under ex situ conditions without any stated precautions to be representative of in situ conditions. All the discussion/conclusion relies on those simplifications and potentially non representative data, that strongly limits its credibility.

Oxygen microprofiles were measured with many precautions. First, we made sure that the cores recovered for the microprofile analysis were undisturbed, they were kept

closed until the turn in the harbor where the field camp was organized. As the water temperature and the air temperature did not vary by more than 2°C at this time of the year with the cloudy conditions present in these sampling days, we did not use the cryostat to control temperature but monitored the temperature at the start of the experiment and at the end. Furthermore, as these waters are near saturation with oxygen, we bubbled air (as stated in “Material and Methods” section) in the core in order to gently stir and to maintain a constant O2 concentration at saturation in the overlying water. This procedure was already used by our grouop with success in previous publications (see Khalil et al., 2013, Aquatic Geochemistry; Cathalot et al., 2015, PlosOne) We rewrote the Material and Method section in order to introduce these elements: “Sediment were sampled at stations IN1 and EST1 in June 2015 (respectively on 23/06 and 24/06). Undisturbed cores were collected by means of an Uwitec corer (10 cm diameter; 20 cm avg. penetration depth). Water was sampled 2 m above the bottom by means of a Niskin bottle, for dissolved oxygen, salinity and temperature determinations. Cores were immediately brought back to the field camp and prepared for microprofiling, which was conducted a few hours after coring. As the temperature of the outside air was within a few degrees of the water teperature during the cloudy sapmpling days (23°C in air versus 21°C in the water), the temperature was not controlled using the available cryostat, but monitored at the start and end of the measurements, and showed minimal variations. Cores were bubbled with air during measurements to allow aeration and gentle stirring. As the bottom waters were saturated with oxygen, bubbling maintained the proper in situ O2 conditions. Microprofiling was conducted with a Unisense motorized microprofiler. Four oxygen microprofiles were performed using 100 μm tip microsensors which were calibrated by a two-points method: Winkler titration of the overlying water (with a precision of 2 permil) and zero-oxygen signal in the anoxic layer below the oxic zone.”

Specific comments: Title: - define POC in the title or if you assume that it is 100% evident, it should be at least defined in the abstract and in the manuscript the first time it is used? The only place where it is defined is at L27, p3, clearly too late for the reader. Thanks. Particulate Organic Carbon was reported in the title. POC acronym was defined within the abstract, as well as in in the introduction.

Introduction L2, p2: please, precise the difference between faeces and pseudo- faeces? Do you assume that both materials are included in OM3 pool (deposited pool) defined later? It may help the reader to better understand. –

The sentence was modified as follows: “However, the production of faeces and pseudofaeces (excess particles rejected by palps before ingestion) leads to a net transfer of organic matter from the water column to the surface sediment”; correct, both materials are included in the same OM3 pool, this was better specified at line 14 pg 4.

L6, p2: replace the last coma by "and" corrected

L9, p2: correct "Estuarine" corrected

L14, p2: what "degree of deposition" means? Is it a characteristic of the deposition which is influenced by the local hydrodynamic? Yes, in order to be more precise the sentence was rephrased, and now reads as follows: “Based on these works it was possible to have a clearer mechanistic understanding of the relationship between the values of flux and the area affected by organic deposition and the different farming conditions (in terms of local hydrodynamics and farm characteristics – depth; geometry).”

Materials and Methods

L29-39, p2: This part is a little bit confusing: it seems to announce the organization of the Materials and Methods section but the following parts do not follow such structuration. There is no information allowing to link the POC production and the POC deposition (i.e., the deposition model). I suggest to improve this part in order to fit the following parts of the section (potentially separating the modeling approaches and the experimental approaches used to calibrate the models), or to remove it from here and resume it at the end of the introduction section. – Thanks for this useful suggestion. Lns 29-39 were integrated in the last part of the introduction, by extending the con-

tent of lines 21-24, pg 2 (original lines numeration). The text now reads as follows: "In this work, a longline mussel farm located in the north western Adriatic Sea was regarded as a local source of perturbation of natural organic matter downward fluxes. Average yearly increase in Particulate Organic Carbon (POC) flux induced by the mussel farm throughout the year was first quantified by applying a biogeochemical model of POC production and deposition (mussel faeces and pseudofaeces), coupled to two sediment trap deployments, which were carried out at the beginning and at the end of mussels farming cycles, with the aim of corroborating model predictions. Outputs of this first model, were subsequently used in early-diagenesis model simulations (one steady-state and one transient), which were constrained by the observed field data in the sampled cores at two stations (pristine and impacted): bioirrigation parameters and ratio among degradation pathways were estimated on the basis of model application. Measurements included O2 micro-profiling, porosity and micro-porosity, pore waters NH4+, SO42-, and Dissolved Inorganic Carbon (DIC)."

L3, p3: remove "see" before Figure (to be applied to all the manuscript) –

change implemented

L18-19, p4: What is the impact of neglecting Fe and Mn biogeochemical related processes on the total biogeochemical processes? Are these processes really negligible? This could be the case as sulfate reduction process appear negligeable (from SO42- profiles and in spite what your modeling result tend to indicate). In addition you do not have any NO3- data that could also be an important process in the area studied. -

Thanks. Including Mn and Fe related processes would definitely increase the EDM realism, but also its complexity. Mn and Fe dynamics were present in a previous modelling work applied in a close area, located in the Northern Adriatic (Brigolin et al., 2011, cited in the text). However, with respect to the present work, in this other case, a larger database was available for constraining the model (pg 4, lns 18-20 - initial version of the ms). This model limitation was stated explicitly in the methods section of the ms, and a remark concerning the potential development of the model was also present in the discussion (pg 9, lns 43-44 - initial version of the ms) " A more precise estimation of the fate of this oxygen could be obtained by introducing in the model FeS precipitation, for which at least Fe^{2+} measurements in pore waters would be required.". With respect to NO_3, we acknowledge that this would be a relevant information for constraining nitrification/denitrification, and this was highlighted in the discussion (pg 10, lns 5-9 - initial version of the ms) " Higher NH_4^+ concentration predicted at station EST1 with respect to field data could be explained with a higher rate of nitrification at this station. However, in the calibration performed within this work, the kinetic constant for nitrification was kept at its original value (Table A4), due to the lack of data concerning NO_3^-."

L1, p5: what is POM? Precise the relation we with? POC you assume. – We added POM and TSS extended name. TSM was replaced by TSS, in order to homogenize this term throughout the manuscript (see our reply below). POM was determined independently, as part of the survey described in Brigolin et al. (2009), and not by assuming a defined POC:POM ratio.

L1, p5: what is TSM? Is it the same as TSS? Please homogenize throughout the manuscript and if there is a difference between these parameters, clearly explain it. Correct. This was checked, see the question above.

L2, p5: Why do you cite Rampazzo et al., 2013 as you mention that POC/TSM was extracted from the same previously cited paper (i.e., Brigolin et al., 2009)? Thanks. For the sake of clarity the citation was removed.

L2, p5: what is AE? Absorption Efficiency (AE)

L5-6, p5: what is the influence of the farm on the current within the farm? This is the current within the farm that will drive the transport and deposition of particulate from the farm. This is not explained within the deposition model section. I think this is important for the reader. Please explain. This engineering aspect, although of interest, has not been properly investigated yet. To the best of our knowledge, multiple current meter

measurements within the same longline suspended mussel farm are not available, and model predictions regarding the local effect of structures on currents would be complex to validate. Similar effects are currently neglected in most deposition models also for fish cages (which nets are expected to have more relevant impacts on the current flows). We added a synthetic remark on the fact that the aspect is not included in the model (model theory paragraph 2.2, ln 35 pg 3).

L5-10, p5: How the annual variability of the current was obtained as only 6-7 months (March-September) of measurement is available? – Thanks. In order to perform this step, we followed the outline adopted by Jusup et al (2007). In the new version of the manuscript additional details were included in the description of the methodology, based on the suggestions by reviewer #2. The text now reads as follows (p5): " Modelling deposition requires an input time series of water velocity at an hourly time step. These data were provided on the basis of a current meter deployment carried out between March and September 2010 at a station located approximately 500 m from the NE edge of the farm (Boldrin A. pers. comm., see Fig. A2). Current meter data were first processed by means of a classical harmonic analysis, in order to extract tidal components as well as long-term residual means (Pawlowicz et al., 2002). On the basis of the procedure proposed by Jusup et al. (2007), the residual currents were therefore edited randomly for short periods of time in order to reproduce the variability recorded from current meter measurements during extreme events (i.e. storms). Number of events was imposed on the basis of the 2010 current time series, and of previous current meter deployments available for this area (Rampazzo et al., 2013; Giovanardi et al., 2003). Effects of tide and storm events were therefore accounted in the final time series, while short-period fluctuations related to turbulence were accounted for by the deposition model, as reported by Jusup et al. (2007)."

L15, p5: I guess you refer to the POC downward flux here ("Initial values of POC for the calibration...")? Please precise. – corrected

L17-18, p5: what do you mean by transient conditions? Is it the second EDM model that is mentioned at L4, p2? Is it a completely different one? Correct. The same model structure was used, although boundary conditions, and initial conditions, were specific for this station, and irrigation parameters were independently calibrated for this site. We added a reference to this in the last part of the introduction (Ln 28 pg 2), and within this section (Ln 19 pg 5) “The model, which had the same structure of the EDM run at EST1, was run for 20 years (time of activity of the farm) . . .”.

L21, p5: Diffusive O2 fluxes were assessed from the profile in the surface sediment? Accounting for water temperature and salinity, and tortuosity in sediment? Please precise Diffusive O2 fluxes were assessed from measured oxygen profiles in the sediment by considering the oxygen gradient within the thin diffusive boundary layer. Temperature and salinity corrections were accounted for, based on measurements performed on bottom water samples. Porosity was taken into account, and the calculation of the diffusion coefficients was based on Andrews and Benett (1981).

Andrews, D. and Bennet, A.: Measurements of diffusivity near the sediment-water interface with a fine-scale resistivity probe. Geochim. Cosmochim. Acta 45, 2169-2175, 1981.

Text was changed as follows:" Diffusive oxygen uptake was calculated from profiles (both model and data, see section 2.5 below) by means of the 1-D Fick’s first law of diffusion. These fluxes were assessed from oxygen profiles by considering the oxygen gradient within the thin diffusive boundary layer. Temperature and salinity corrections were accounted for, based on measurements performed on bottom water samples. Porosity was taken into account, and the calculation of the diffusion coefficients was done in accordance with Andrews and Benett (1981)".

L24-25, p5: it appears that the end of the rearing cycle is the last days of August while the beginning is early September. Does this mean that the harvesting of the mussels occurs within these few days each year? Does the activity of harvesting induce a strong increase of the concentration of suspended matter that may remain several

days within the water column and may impact the material trapped within the sediment traps deployed at the beginning of the new rearing cycle? Thanks. As reported in section 2.1, the farmed area covers about 2 km2, and mussel within this area are normally harvested within July-September, after a rearing cycle lasting a single year. This represents the average situation, which was considered for the POC production/deposition modeling, in order to conceptualize the mussel cultivation cycle and parameterize its features. In fact, the time table of activities can present some variability, according to sea conditions, market request, and farmer strategy. In the specific case, during the August 29-31, 2014 experiment mussels were still on the ropes without strong ongoing recollecting activity (we performed traps deployment over the week-end, in order to minimize the noise). The second experiment was performed one year later (September 11-13, 2015) some days after the beginning of the cycle (this second deployment was also performed over the week-end).

L34-35, p5: only one sediment core was collected per station. How the spatial heterogeneity can be addressed with only one core?

We agree with the reviewer that one core per station is not enough to capture spatial heterogeneity. It was beyond the scope of this study to understand the heterogeneity of the area and we concentrated on collecting enough porewater and solid phase data on each core to constrain the processes by using our diagenetic model. This was already a large amount of work and we could not achieve more than one core in the time of the study. We still believe that our approach is valid because of the joint use of measurements and model which allow to estimate process rates and compare them, which is a more robust and integrative output than just comparing pore water profiles.

L35-36, p5: Did the profiling experiment carried out at in situ temperature? If not, please precise at least the in situ vs ex situ temperature.

See response above, the temperature in air and in the water (23°C versus 21°C) were very close and temperature was not altered much during the measurement.

L35-36, p5: Does the bubbling was performed using ambient air? Is it coherent with the in situ oxygen concentration?

Bubbling was conducted with ambient air, as the bottom water of the sites were well-oxygenated : 223 and 229 μmol O2/l which correspond to 100% saturation in Mediterranean seawater (38 permil at 20-21°C).

L41, p5: how the porewaters were extracted?

Porewaters were extracted using Rhizons® (Seeberg-Elverfeldt et al. 2005) which are porous soil samplers operated with depression. The cores were sampled under N2 within 4 hours after coring. The text was modified to include this method: "Porewaters were extracted within 4 hours after coring in a glove bag under N2 using Rhizons® (Seeberg-Elverfeldt et al., 2005)."

Results

L17, p6: What is CV? "Coefficient of Variation", the extended name was included in the ms

L21, p6: why to chose days 10 and 360? Because they correspond to the min and max situation? Precise. Correct, we rephrased the sentence as follows: " Maximum organic carbon fluxes predicted at day 10 and 360, representative of the situation at the beginning and at the end of the growth-out cycle, are 2.5 and 13.3 mmol C m-2 d-1, respectively."

L22, p6: why the footprint is related to the presence of lines here? The shape of the farm appear also clearly on the deposition map (with or without the lines visible). With this comment we would like to point out that, based on model results, the shape of the lines is more clearly visible on the deposition footprint on some days with respect to others (which are characterized by specific conditions in term of currents, and amount of biodeposits exiting the farm - this aspect is additionally explored in the answer to reviewer #2).

L22, p6: "is clearly visible at days 120 and 360" It is also the case the day 10. With respect to this point, we are not in agreement with the reviewer, since the map at day 10, if examined alone, and not in the context of the whole figure, seems not to present a clear univocal pattern related to the presence of the mussel lines.

L25, p6: OC% not defined and no information about its analysis. In Table 1 "TSS" seems to be Total mass fluxes, OC% seems to be Corg(%) and POC flux seems to be Total Carbon flux? Please be consistent all throughout the manuscript. We thank the reviewer for spotting this aspect. Names in Table 1 and in the ms has been checked for consistency. Methods for measuring POC fluxes and the Total mass fluxes were stated in section 2.4, the text reads as follows: "Upon collection traps content was filtered through pre-combusted (450°C, 4h) and pre-weighed Whatmann GF/F filters. For total mass flux determination, filters were dried at 60 âŮęC for 24 h and re-weighed. For POC determination, filters were stored at -20°C until analysis, which was carried out by means of a Thermo Elementar Analyzer (Flash - EA 1112), after acidification with HCl for removing carbonates. The percentage of organic carbon on total mass (OC%) was calculated from POC fluxes and total mass fluxes."

L30, p6: if you decide to call the station outside the farm a "reference station" please use this terminology all through the manuscript. Thanks. We removed the term "reference", and opted for a more detailed description: " Early diagenesis processes underneath the farm and at a nearby station located outside the farm influence"

L33, p6: no information about how porosity was measured. Porosity was measured from the weight loss upon drying at 60°C. The first weighting was performed just after sample collection to minimize water loss. The water loss was then converted to porosity using sediment average dry bulk density and salt correction. A sentence was added in the revised version: "Porosity was obtained by measuring the weight loss upon drying until at 60°C until constant weight. Porosity was recalculated from this weight loss using salt correction and dry bulk density."

L36, p6: "Oxygen shows a quasi- monotonous decrease in concentration" you mean downward in the sediment? yes. This was specified in the ms.

L37, p6: what do you mean by low variability? thanks, the word "low" was replaced by the word "limited"

L4-5, p7: I clearly doubt that the trend in the only one DIC profile (and the two subsurface samples) reported can indicate anything about bioirrigation.

Thanks. We would like to remark that two sets of profiles indicating the same type of distribution were available: DIC and NH4, moreover the effects could be coroborated by the shape of the profile of another chemical species (SO42-), available from the same cores. This was stated in the ms (page 9 line 15, previous ms version):"The shape of the DIC–NH4+ profiles indicates bio-irrigation (Meile et al, 2001; Canavan et al., 2006), although the deeper increase is not visible in our data profiles due to limited penetration of the cores. Indeed, the very limited increase in concentration profiles in the first centimeters can only be linked to input of bottom water with lower DIC and NH4+ by irrigation, given the large recycling intensity in surface sediments as exemplified by O2 profile."

Based on the important reviewer's comment (see also general comment #2, and comment L13-14, p7, below), we included in the new version of the ms additional information on potential bio-irrigators. Details are available above, in the reply to general comment #2, here we report the text which was included in the new version of the ms, pg 9 : ". We remark here that α0 and xirr1 were the only two parameters calibrated at IN1, and they suggest a higher infauna activity, shifted towards the surface at this site. This feature was independently confirmed by a set of macrobenthos samples collected at the two stations as a part of a complementary study (Colla, 2017). Macrobenthos samples showed a higher diversity (48 vs 31 taxa recorded) and abundance (on average 1900 vs 1000 ind. m-2) at IN1 with respect to EST1, accompanied by the presence of larger organisms (0.065 g ind.-1 at IN1 versus 0.034 g ind.-1 at EST1). This is in

agreement with the expected influence of biodeposition from mussel culture (McKindsey et al., 2011). Species recognized as important bioturbators (as Lagis koreni, Glycera unicornis, Sipunculus nudus, Eunice vittata, Hilbigneris gracilis, Amphiura chiajei, Ensis minor, Dosinia lupinus, Tellina distorta, Nassarius incrassatus) were present in both samples, accounting for approximately 18% of the total abundance at EST1 and for the 35% at IN1."

L7, p7: O2 with 2 in index (same all through the manuscript) corrected

L13-14, p7: Is the bioirrigation expected to decrease the porewater concentrations at a precise depth? I think there is a need to better explain how bioirrigation is taking into account in the model. Usually, the bioirrigation rate is applied over a depth interval with the intensity decreasing (linearly or not depending the model) with depth from a maximum value at the surface to a 0 value at a specific depth. This induces a dilution effect of the porewater by the overlying water decreasing with depth. Is this not the case here? If not, please precise this aspect. Thanks, this was the case, although the formulation used was a double exponential (see Table 2), which allowed to represent a sharp decrease of bioirrigation from its topmost value to 0. A similar shape, although obtained by adopting a different, discontinuous, function was reported by Canavan et al. (2006). We fully agree with the reviewer about the lack of details with respect to this point on the previous version of the ms. In the new one the following elements were added: - The formulation used to define the profile of the bioirrigation rate has now been added in Table 2 (formulation for bioturbation was also specified in Table 2); - A sentence pointing to the methodology used to include bioirrigation in the BRNS tool, used for developing the EDM was added in the methods section (ln 7-11, pg 4); - A plot showing the shape of bioirrigation rates in the two profiles was included in the appendix (Appendix, Figure A3)

In addition, information on the macrofauna species and bioturbation behavior may help to define the way to account for bioturbation (sediment reworking + bioirrigation) processes (in Colla, 2017?). Thanks for this comment, with respect to this point please see our reply in the point above (L4-5, p7). We added information on this at pg 9.

L14, p7: I'm not agree with this affirmation. The range of concentration between measured and modeled profiles are similar but the vertical trends clearly differ. The sentence was rephrased, and now reads as follows: "In general, simulated profiles reasonably agree with observed concentration values, although differences between model and data vertical trends are visible, in particular at station EST1, were predicted NH_4^+ exceeds observed concentrations."

L17-20, p7: Figure 7 seems to show the diffusive O_2 fluxes for IN and EST stations. I can't see any comparison between modeled and measured data. Modeled data were represented by the black dot, in the new version we propose to use a more distinguishable marker (see new Figure 7).

L17-20, p7: how the measured ex situ O_2 profiles, and assessed O_2 diffusive fluxes, may be used here to gain information on the oxygen uptake in situ since: 1) profiles seems to haven't been performed at in situ temperature and in situ oxygen content, and 2) under sunlight influence that could be clearly higher than under in situ condition (observation microphytobenthos production that is know to strongly impact the diffusive fluxes at the sediment-water interface as well as the O_2 penetration depth). This is a crucial step that may to be clearly addressed. -

With respect to this important point, please see above our replies to the general point #3, and to the following specific aspects: L 21, p5; L35-36, p5.

L22-23,p7: the three pools of OM: you actually mean the total organic matter that correspond to 2 pools in EST station and 3 pools in IN station? yes, we rephrased the sentence as follows: " Figure 8 shows the partitioning among mineralization pathways, indicated by the electron acceptors, of the total organic matter."

L24-25: this is not what is reported in Figure 7. Thanks, the correct reference here is Figure 8.

Discussion

L24, p8: What is ED? It should be EDM as previously. For consistency, we used EDM throughout the ms.

L24-32, p8: You said that modeled POC flux from EDM agrees with POC flux measurements from a sediment trap. (that already was the subject of the previous section) but then try to explain why there is a factor 2 difference. We attempted to explain which we think could be the reasons behind this difference at lines 34-39, pg 8 (previous version). Based on this comment from the referee, we decided to change the introductory sentence, and now the text reads as follows: " Absolute values of the POC fluxes obtained from the sediment trap experiments can be cross-compared to the values estimated through the inverse use of the EDM. The value of 11.6 mmol C m-2 d-1, at EST1 accounts for approximately 50 % of the flux measured from the traps (22 mmol C m-2 d-1 on average). This difference can be primarily ..."

L12-13, p9: This is an important point.

Table 1: - title do not correspond to what can be seen in the Table - please add a mention specifying the stations that are inside the influence of the farm and those outside the farm (reference?) and gather results for stations IN and EST to help the reader. - be careful to the significant digits. - homogenize the position of the text inside the cell Thanks. Suggested changes have been implemented both on the Table, and on Table caption.

Table 2: - Second line corresponds to POC deposition I guess. This should be clearly mentioned. - please add a mention specifying the data for inside the influence of the farm and this outside the farm (reference?) to help the reader. - Thanks. Suggested changes have been implemented How can you assume that the CNP ratios are the same in both stations. Based on the close location of the two stations we considered reasonable to assume homogeneous CNP ratio for the background fluxes (OM1, OM2), which was based on available field data for this area (Brigolin et al., 2009) - no field

data were available to distinguish OM1 and OM2 - Due to the temporal variability of faeces to pseudofaeces ratio, and the temporal variability of CNP for each of these two types of biodeposits (both included in the OM3 pool), we considered difficult to estimate reliably the average CNP composition, and therefore proceeded with the conservative assumption of maintaining the CNP ratio of OM3 equal to the one of the remaining pools.

- mineralization rates reported correspond to total (OM1 + OM2 + OM3) organic carbon? yes, this was specified in the table in the new version of the ms

Figure 2: The caption is clearly no detailed enough? What are the red bubbles? The black rectangles? The blue lines? Thanks. We rewrote the caption, detailing the different parts of this cartoon figure.

Figure 4: Please add the simulation day on the figure. It will help the reader to follow the writing. This change was implemented.

Figure 5: - specify "por" in the caption or write "porosity" on the figure. Same for DIC. - I doubt the unity of porosity is % as mentioned. - the term "micro-profile" is only applicable to O2 and eventually to porosity profiles. Thanks, both figure and captions were changed based on the reviewer's suggestions.

Figure 6: precise this caption. Thanks, we extended the description in the caption.

Figure 7: - caption of the figure 7 is unrelated to the figure 7 - write on the figure (noted Figure 7) that it correspond to O2 diffusive fluxes (if this is well the case). Thanks, caption and figure were both modified.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2017-206/bg-2017-206-AC1-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-206, 2017.