

Ocean Sci. Discuss., author comment AC1
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Reply on RC1

Maike Iris Esther Scheffold and Inga Hense

Author comment on "There and back again, a journey of many pathways: conceptualising the marine organic carbon cycle" by Maike Iris Esther Scheffold and Inga Hense, Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-75-AC1>, 2021

We thank the referee for the valuable and detailed comments and suggestions. Please find below our responses (in bold) to all comments (not in bold).

Overall, while I am intrigued by the manuscript, and can envisage how its insights might be used, I cannot currently recommend it for publication. By being presented in a highly abstracted way, and by avoiding specific examples of its use, it feels as if any potentially interested users still have a mountain to climb. I do understand why the authors have made these choices for the manuscript. However, I suspect most readers will find the concept of interest, but will be dissuaded from pursuing it because it is unclear what next steps are needed to make best use of it.

We thank the referee for her/ his valuable feedback. While we see the referee's point, we cannot build our concept around an example as it is supposed to be abstract and general and be valid without a specific example. However, we will try to be more specific regarding possible applications of the concept, taking into account the referee's feedback on the possible usefulness of the concept, and will also try to include a small (within a reasonable dimension of the paper) example in the discussion. By this, we hope to provide a ladder helping the user to climb the mountain more easily.

In terms of its revision on this point, what I think would help it be more clearly relevant is its use in an example. From my own area – global-scale marine biogeochemical modelling – I could see it potentially providing a framework for identifying and, more importantly, *quantifying* major carbon flows between different models and, potentially, observations. In this way, the details of individual models could be ignored to focus instead on major pathways, including how these change across simulations of the present and future. Adapting the manuscript to include a clear instance of the use of its conceptual framework feels necessary to me.

See comment above.

Table 3's presentation of the conceptual model's "language" is necessary, but it seems very clunky (see comments below); I don't know how this can be avoided, however

We have not found a solution yet, but are carefully considering options, also referring to the referee's comment regarding the possible example of the

biological pump.

Abstract: the manuscript would do well to make clear somewhere that ocean uptake of anthro CO₂ has little to do with the biological focus of this manuscript; the opening statement of the abstract is implying that the reverse is true

We are not 100% sure that we understand this comment. We assume that "biological focus" refers to the part of our model based on photosynthetically fixed carbon? As the rest has not per se a pure biological focus, as we also include other sources of OC and physically-induced processes. However, we agree that we need to adjust the abstract, particularly with regard to the changes we plan for the introduction and discussion (see following answers).

Abstract: recalcitrant DOC is poorly defined at this point; this might not matter for the framework here, but the authors should note the work of Arrieta et al. (2015; <https://doi.org/10.1126/science.1258955>) on the bioavailability of low concentrations of DOC molecules

We already included their work in our concept in the detailed description of our model (L284). We refrain from adding complexity to the abstract, as we highlight the different concepts of what rDOC is at a later stage.

Abstract: regarding the absence of applications in this manuscript, one use this qualitative approach could be put to is creating a systematic structure of the ocean's carbon cycle that quantitative datasets or models can be aligned with; that would simplify both obs and models to allow simple comparisons of reservoirs and fluxes

We thank the referee for this comment. We will include this feedback in our motivation.

Pg. 1, ln. 16-18: this sentence is confusing; what does "it is the first concept" mean?; models of the marine system are not always structured around e.g. export; also, the span of processes here is already covered in some models; so it's unclear why this framework is special

We will reconsider and revise our introduction as we have obviously not been clear enough. However, we seem to have a different understanding of what a "concept" is (perhaps due to our use of the word "conceptual model") and what "explicit" means in this context. By "conceptual model" we mean an abstract idea that summarises and generalises findings and observations - in our case structures/a skeleton of path segments. By "explicit" we do not mean that the equations of a mathematical model are solved mathematically or numerically. "Explicit" here means that the concept is published (easily retrievable and available to the reader) and leaves no room for confusion or different interpretations in its presentation (verbal, visual, etc.). Thus, it contrasts with more implicit representations, such as mental models or incomplete schemata, which refer to something that is probably known but not directly described. We argue that a concept that is not explicit (in our sense) can be opaque because it presupposes that identical (or very similar) concepts exist in the scientific community.

At the same time, from our point of view, covering/adding processes is not the same as trying to condense and generalise structures. Our concept does not compete with existing (numerical) models or partial and specific concepts (as they have a very different purpose) but is the first concept to generalise structural patterns and general pathways of the entire OC cycle. While we agree

that (numerical) models of marine systems are not always structured around export but so far presented (visual) concepts often are.

Pg. 1, ln. 18-19: point 1 is good; it's what I've identified above; point 2 is also good, although it doesn't introduce anything new on this point; models usually include all of these exit points because they have to; in Earth system models, for instance, the budgeting of C is critical, so these exits are explicitly there (and usually monitored diagnostically)

As described above, we will revise our introduction. However, we would like to emphasise that we acknowledge that numerical models naturally take care of C-budgeting. From our point of view, however, a numerical model is not a concept that offers generalisation or synthesis but is always simplified to focus on the most important processes (as far as they are understood and identified) for the questions to be addressed; it provides specification, realism where possible, and (necessary) quantifications. A numerical model, for good reason, approximates observations as closely as possible and attempts to represent "reality" (as far as we can fully see and imitate it). Our model does not have this aim. It aims to be an abstract idea of this world by identifying functional structures allowing generalisation, synthesis, comparisons, and a structured integration of individual concepts and study results into the larger concept of the marine part of the carbon cycle.

Pg. 1, ln. 20-21: It **needs** to be quantitative if it's to help here; being qualitative and abstracted to basic processes will reduce its usefulness

We are rewriting this paragraph to be more specific.

Pg. 2, ln. 28-29: But pathway options are already explicitly included in quantitative models; those move C between different reservoirs, and between locations in the case of 3D models; the conception and representation of the biological pump and its elevation of interior ocean CO₂ is arguably already a "general pathway"

The biological pump is certainly a general concept, but it is only one part of the OC cycle within a larger picture and cannot be used to generalise the OC cycle.

Pg. 2, ln. 31: "inter alia" is a relatively uncommon latin expression; it might be better to replace with "among other things" for non-native English speakers

Agreed. Changed it, among other things.

Pg. 2, ln. 31-33: bit of bracket indiscipline here

We revised the sentence reducing brackets.

Pg. 2, ln. 45: remineralisation is effectively the respiration of organic carbon to inorganic DIC by bacteria or other microbes; respiration more or less by definition turns organic C into inorganic CO₂

We want to express with this sentence that existing explicitly presented graphical concepts of OC cycling often do not show what happens or could happen to the outcome of remineralisation/ respiration (DIC) and that the representations of these schemes are not congruent (respiration vs. remineralisation). Of course, one objection may be that these are only schemata and thus are less relevant. However, we think this represents a not insignificant misunderstanding of both the process of visualisation (a conscious decision of

which aspects to include and which not) and an underestimation of the power of illustrations (particularly in interdisciplinary, communication, and education contexts). Images are a visualisation of parts of or the entire mental concepts of the mapper. Differences could therefore mean that these concepts are different or similar because the decision of what to visualize masks the entire concepts. Incomplete and/ or non-congruent graphical concepts are at best not valuable for any other considerations other than the study they are presented in or at worst represent different concepts and lead to different understandings.

Pg. 2, ln. 48-50: this sounds like the authors are trying to give a more holistic description of the marine carbon cycle, but it is unclear how this will be achieved

We see the referee's point and will change it, as our main focus is not on a holistic description.

Pg. 3, ln. 59-60: this is getting a bit opaque now; also, the export arrow is necessarily thinner at depth because there is simply less material the deeper one goes - this is a simple function of material being added only at the surface

We agree that measurements, simulations, and observations show a decrease in material with increasing depth. The only points we would like to emphasise are that 1) export (a concept) represents the resulting flux of a process, or rather a composition of processes that are "lumped together" and contain quantitative and spatial information in their graphical representation. Other processes or pathways in the same graphical concepts often do not carry (relative) quantification or spatial information hindering a congruent representation of flows, processes, or pathways.

2) The export arrow and all representations of quantifications are not useful for a concept, which wants to synthesise processes and pathways and abstract their core/function. In our new introduction, we will clarify our points and perhaps refrain from using the term conceptual model and instead use concept.

Pg. 3, ln. 66-68: again, this sounds like it is merely making a case for a more holistic treatment of the C cycle, i.e. everything including the kitchen sink; noting that models, including conceptual ones, truncate the real world does not seem all that novel a point to make; and, as already noted, some models already go quite far towards including as much detail as is known

We do not make a case for a more holistic approach but for a systematically unified and synthesized concept that generalises structures and options within these structures. This concept, like all models, truncates the real world. Or even stronger, its aim is not to represent the real world as recognizable as possible but to abstract it. It might allow classifying and comparing what we see and model in a more structured and unified way, thereby providing an explicit conceptual ground for research design, model comparison, educational purposes, and science communication.

Pg. 3, ln. 81: what about dissolved inorganic carbon?; it's only the largest ocean reservoir

See comment below.

Pg. 3, ln. 82: So ... straightaway some real-world detail is being dispensed with?; that sounds a little less than holistic

Yes, that is a major limitation. As the reviewer also already indicate, all models

are simplifications. We cannot deliver one that takes it all. But as our main argument is also not a holistic approach but rather a generalised, synthesized, and congruent concept that is explicitly represented and up for discussions; we argue that we have to live with this shortcoming (for the time being).

Pg. 3, ln. 84: you might want to be very clear on what you mean by "species" here; it's obviously not "biological species", but it also appears not to be "chemical species" either; it's somewhere in between

True. We changed the term species to pools and added some additional information on the aspects we use to separate those fractions: If the processes or pathways considered depend on the size, volatility and lability of the OC, we operationally subdivide OC into different pools.

Pg. 3, ln. 85-85: any explanation for these arbitrary size limits?

That is correct. Thanks for pointing out that these numbers were wrong. We changed it to: DOC smaller 0.2 μm and POC larger 0.2 μm (Kharbush et al. 2020).

Pg. 3, ln. 88: again, rDOC may not be recalcitrant at all, merely at low concentration

We agree that low concentrations of DOC might be one explanation for the limited or more time-consuming utilisation of some DOC. However, there is, to our knowledge, still no consensus on the dominance or exclusiveness of the "dilution hypothesis" as for example presented and supported by Jannasch (1967) and Arrieta et al. (2015), and the "intrinsic stability hypothesis" supported for instance by Jiao et al. (2010) describing that a share of DOC is refractory because of its molecular structure (see discussion in Dittmar (2015) and Mentges et al. (2019)). We highlight this aspect in section 2.4 when showing that "normal" DOC production, such as sloppy feeding, can be part of the rDOC loop because the DOC is diluted. We therefore will further separately consider rDOC as we also state in line 193 that the rDOC loop is, although a timelier, part of other loops. To ensure that this aspect is highlighted again, we added some explanation into paragraph, where we first mention rDOC.

Pg. 3, ln. 88: 1.5 to 40,000 years is quite a span; again - any explanation for these arbitrary timespan limits?

These numbers refer to the times given by Hansell (2013) for semi-labile to ultra-refractory DOC following his definition of recalcitrant DOC pools. We added some more information on the division provided by Hansell (2013).

Pg. 4, ln. 91-92: you could note that very few models go to the bother of subdividing DIC into its constituent species

We agree that few do that, but since we think that it is also a limitation of our model, we do not want to justify it with other models having a similar limitation.

Pg. 4, ln. 99: the use of "particle" here might be confusing; in a marine context, this could mean an actual particle of marine snow

We will use the term OC compound for the parts in question.

Pg. 4, ln. 105-109: this is a big ask; if the conceptual model is not going to be quantitative, it has to do something special qualitatively to compensate; the introduction here has not made it clear what

We will address this point, taking into account the criticisms, but we hope that the points we have already made in our responses show what our model contributes.

Pg. 4, ln. 108: "a delay of consumers"?; this is opaque; I think it would make more sense if "a delay of" was deleted; however, I may be misunderstanding what "delay" means here

We changed it to spatio-temporal mismatch with consumers that favours sinking.

Pg. 4, ln. 111: "bases" -> "is based"

Changed.

Pg. 4, ln. 112: "non-exclusive"?; this might need a clarifying remark - suggesting that a review was non-exclusive implies that it read **all** of the literature

We apologise for this misleading sentence. We actually wanted to say more or less the opposite. We therefore change it to: unsystematic literature review.

Pg. 4, ln. 115-118: OK, this sounds good so far; obviously I'm immediately wanting to assign numbers ...

That would make us very happy. As our concept should be the discussion ground for ideas, concept exchange, studies etc.

Pg. 4, ln. 121: "under the given hygiene conditions"?; this is a strange qualifier to add without explanation; is this an oblique reference to the ongoing pandemic?; in any case, either explain or delete

We were referring to the ongoing pandemic. We will reconsider or delete the sentence.

Pg. 4, ln. 121: "example" -> "analogy"

Changed.

Pg. 5, ln. 138: "position change" is an interesting one given that this can have radically different drivers; it can be physical, biological-gravitational, biological-migrational, etc., each of which can have distinct consequences; for instance, dissolved OC moved by physics will not interact with sediments in the same way as OC sinking gravitationally; is this a problem?; by having multiple OC routes out of the surface, the diagram would suggest possibly not, but then is opaque on what these different routes cover

We agree that differences in process options lead to different products, interactions, temporal scales etc. That is why, we add process option that resolves the path segments in a second step.

Pg. 5, ln. 150: quite; per my previous point

See comment above.

Pg. 6, ln. 155: SLS = "surface layer space"?

Yes.

Pg. 6, ln. 154-164: sensible spatial breakdown

We agree.

Pg. 6, ln. 167: SRL -> SLRL; per SLS?

We agree that this is inconsistent. We will change it as recommended.

Pg. 6, ln. 167-176: this makes sense, but one has to concentrate with all the acronyms; I've two suggestions here; 1. maybe parse out the examples with in-line "equations" of flow pathways; i.e. SLS -> WCS -> SLS; 2. it feels like you need a syntax for using all of these space, process, pathway interactions - yes, you can write sentences with them, but having a consistent way of writing them might make it easier to follow what a particular example is doing

We will try to incorporate the referee's suggestion and add a flow syntax to the example.

Figure 1: as already noted, the significance ascribed here to rDOC formation is perhaps misplaced given that rDOC might only be recalcitrant because individual chemical constituents are at low concentration; the predominance of this process in this model seems disproportionate given that potentially more quantitatively important processes are downgraded and lumped into "OC remineralisation"

We disagree with this statement, as we consider the rDOC pool because of its size and importance to the climate system (Hansell et al. 2009). While the processes that lead to DOC are partly the same as those that produce rDOC, and "refractory" is a concept in itself - as are, in fact, the subdivisions into POC, DOC and trophic levels, to name but a few - there is a measurable pool of DOC that persists longer and accumulates throughout the water column in the system. As such, it is a unique pool that needs to be considered separately.

In our concept, it is not important whether these processes are quantitatively important. To return to the concept of different carbon pools, conceptually or operationally divided into DOC and POC. Again, very different molecules that are quantitatively very different are lumped together because they are conceptually similar within the abstract pools and different from the other pool. Analogously, we extract and generalise a function of processes based on similarities and differences. The process options allow a distinction between quantitatively relevant and irrelevant processes.

Pg. 14-16, Table 3: while this table tries to address a point I made earlier about allowing a consistent description of the pathways, the result seems very clunky to me; I can't immediately envisage a clear alternative, to be fair

See one of the first comments and comment below.

Pg. 14-16, Table 3: might a good example "process" for the breakdown illustrated here be the biological pump itself?; by looking at this, the authors could (a) give an example of how conventional understanding of a carbon cycle process can be translated into their conceptual model, and (b) illustrate the difficulties of doing so because of the associated complexity, and (c) thus emphasise the importance of a holistic viewpoint

We understand the referee's point and will try to include a small example in the discussion. But as described in our initial comments, we have deliberately not built our model around an example as it is an overarching abstract concept. Although we understand the point about the clumsiness of Table 3, we will not break it down for just one aspect of the OC cycle. Since one of our main points is

that while it is sufficient and useful to consider finite-length paths in modelling and observational studies, it is not in a general concept of the OC cycle.

Pg. 17, ln. 298: most existing models already incorporate this cyclic aspect; what's special here?

We see that we were not able to highlight that our proposed concept does not compete with numerical models, as they have very different purposes. We will make that point stronger.

Pg. 17, ln. 299-301: this sentence may describe how the process of export is *sometimes* described, but I think it's an exaggeration to suggest that this is "normal"; most modelling scientists are well aware of return pathways; to be honest, return pathways can even be highly visible at the surface, e.g. CO2 outgassing along the equatorial Pacific

We are adjusting our discussion. However, we want to emphasise that we do not question that much of what we present is known to most scientists in some way. However, we find that a synthesised, unified and explicit way is better than different ways and that most scientists leaves open the possibility that there are some who do not know. Explicit concepts have the great advantage of visibly summarising existing knowledge and bringing to life a new idea of how to describe, structure and systematise existing (mental) models so that we can refer to, discuss, discard and adapt them.

Pg. 17, ln. 301-303: being (as the authors keep stressing) purely qualitative, the conceptual model under discussion here cannot really help with this point much more than making it clear (which may, as already noted, be obvious for many researchers) that "what goes down must come up"

See above. Many researchers is not enough.

Pg. 18, ln. 342-345: where this conceptual model seems useful to me is in pointing to fluxes, and making it clear where they sit in a wider consideration of the cyclic pathways of carbon

We agree and will add this value feedback to our motivation.

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