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Interactive comment on "PISCES-v2: an ocean biogeochemical model for carbon and ecosystem studies" *by* O. Aumont et al.

Anonymous Referee #1

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This manuscript provides a detailed description of the current state of PISCES. It is for the most part comprehensive and the authors are commended on their attention to details. This will document will be very important for users of PISCES and potential useful for the biogeochemical modelling community in general. I recommend publishing after some revisions.

General Comments:

1) This is not the venue for discussions about appropriate level of model complexity, but that said, Einstein said something about "Everything should be made as simple as possible, but not simpler". I did keep thinking this as I read through the large number of complex processes/parameterizations that were included in this version of the model: How many are actually needed? There are so many that the model could be

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looking like it was doing the right thing but for the wrong reason. As stated above, this is not the venue for that discussion, but I do think the authors need to provide more information and justification for the long list of complex processes/parameterizations included. There are four sensitivity experiments for four of these processes – this is good (though see my comments on these below). And two processes (complex iron and Kriest POM components) are referenced to papers which further explain the impact of these parameterization. However there are many other parameterizations with no explanation why they were needed, or how they improved the results. Admittedly there is always an explanation of what process the parameterization is capturing, but not why that process is included rather than many others that not included, and no explanation of how it helped the results. These parameterization require a large number of additional parameters – how well are those known? I presume that sensitivity studies were done for each of these parameterizations, and quite likely there are papers that describe these studies and justify the need for that additional complexity. I suggest that the authors include those references where available – as well as for all process provide a sentence or two justification for including the process: How did including that process improve the model?

Just some of these parameterizations are: prolonged dark periods (pg 1385)

acclimation of cells to local silicate (pg 1389)

stickness of cells (pg 1389)

demands of iron for photosynthesis, respiration and NO3/NO2 reduction (pg 1393)

variations in Si/C (pg 1393)

oxygen effects on mortality (pg 1394)

changing global grazing threshold (pg 1395)

flux feeding (pg 1397)

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upper trophic levels (pg 1397)

aggregation processes (e.g.pg 1400)

dissolution of silicain two phases (pg 1408) – especially since you state that this process is uncertain

variability of rain ratio (pg 1417) – yes you can turn this value to get good patterns of alkalinity, calcification – but for the right reason?

dissolution of silica in guts (pg 1418)

2) There are some parameterization not fully explained: e.g. the P/Km+P P loss term in Eq 1 etc

3) Some symbols are used for more than one thing making it confusing. e.g. "g" in Eq (1) and (2) – better to keep g for grazing and chose something else in Eq 2 "Z" for zooplankton and depth, and note that some places depth is z and other Z (the former would be better throughout, keeping Z for zooplankton) “I” is a bit confusing too – though I think it is mostly used for biomass in particular element “m”, especially when used for carbon content pg 1403 line 18 I think there are more - I suggest the authors go through and check carefully for those that could be confusing.

4) In the sensitivity experiment that are shown, it is not always clear that the results are "better" with the parameterization. And if they are not - then is it worth having the extra complexity? This section of the paper could be substantially improved to give a clearer message. The writing seems particularly unfocused. - In the LIGHT experiment I really struggled to figure out which of Eq 2a or b was the "standard" run. This was a long muddled section - and it was not clear if either parameterization was better than the other. It is indeed an interesting experiment and I would suggest rewriting to get the message across much clearer. For instance it is not clear that either one is "better" for the bloom timing looking at figure 19. - In the SIZE experiment, the authors show that there is lower Chl with the size implementation in high latitudes. Is this "better".

Satellite Chl estimates are likely not good in these regions (see e.g. Szeto et al, JGR, 116, C00H04, 2011). There is a difference in equatorial Pacific where diatoms have the ability to survive in SIZE. More should be made of this and less of the other "not necessarily better" issues.

5) The parameterization of half-saturation with cell size is linear with size following Eppley et al (1969) and yet other studies suggest there is a power law change with size (e.g. Litchman et al, Ecol Letters, 10, 1170-1181, 2007). Readers might be more familiar with this latter, so might be good to justify the linear assumption.

6) Where numbers are used in equations, it would be good to explain where they come from: e.g. "12" in Eq at bottom on pg 1389, numbers in Eq. 23b, 2.15 in Eq. 58b, 0.5 in Eq 66 (there are many more)

7) Relaxation of nutrients once a year (pg 1429, line 28). Is this done at one timestep once a year? Doesn't this cause a large jump? How large are these restoring? I find this a bit alarming given that you are also including lots of sources of nutrients and then you artificially just add/remove some relatively arbitrarily. How are we supposed to view the comparison to observations when there is a relaxation term? This needs substantial further discussion. 8) Minimum threshold for Chl in Pacific (pg 1433, line 27): Only in the Pacific? This seems arbitrary. Also seems to suggest that getting good nutrient concentrations needed a big "fudge". In which case all the evaluation is a bit suspect. Getting the "right" results for a wrong reason. I am uncomfortable with this – needs better justification.

Specific Comments:

pg 1377, line 26: remove "to be"

pg 1378: line 1: do you mean internal? not external?

pg 1380, line 10: explain "IOM"

pg 1381, line 3: I think you mean "nitrogen fixing" not "nutrient fixing" pg 1382, lines 10-16: I don't follow the explanation here. Either they are constant Redfield ratio or not? Given that there are different processes included it is maybe inaccurate to suggest that they are Redfield at any point?

pg 1384: Eq 1 should it be P in denominator?

pg 1384 (and onwards) – what is currency in the main equations (e.g. Eq 1 and 9)

pg 1384: which equation of 2 a or b is used in the standard run? (Would help for the sensitivity study)

pg 1388: muˆP_NO3 needs clearer explanation of what this variable is

pg 1395: line19 "to" should be "with"

pg 1396: lines 7-9. I don't quite follow this. So you calculate a "pretend" quota for the growth efficiency, but in reality they take up N/C in a constant proportion. This seems odd. Better explanation needed here. Also one of those parameterizations that it would be nice to see the sensitivity.

pg 1397: line 9 – grammar issues

pg 1398: Eq 32 and line 6: use of N confusing here... since N usually refers to a nutrient

pg 1400: line 8 – would be nice to have a reference for "a version of PISCES"

pg 1407: define Sfe and Bfe; also GOC turns up without being defined

pg 1425: what about iron deposition on top of ice?

Pg 1426, line 3: would be nice to know how different results are with simple versus complex sedimentation

Pg 1428: line 24: why Gm only poleward of 10degrees?

Pg 1431, line 13-15: Doesn't this suggest grazing is too high?

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Pg 1431, line25: Not sure why you use the word “Consequently” here.

Pg 1434, line 18: I believe the Dierssen and Smith (2000) algorithm is for coastal waters. I am not sure it is appropriate to use it for the whole Southern ocean. On the other hand I think is is generally thought that the standard algorithm does not do a good job in the Southern Ocean. I think just stating this is enough to explain that one expects Chl to be higher in the Southern Ocean than satellite products suggest.

Pg 1436, line 25: doesn’t this suggest too few diatoms?

Pg 1437, line 18: DIC doesn’t compare particularly well.

Pg 1443, line 14: As in point 4 above – are these results more realistic? Just being different doesn’t suggest that this parameterization is better or needed.

Pg 1445, line 1: Do you mean “chl” here, not “phytoplankton” – since next sentence you mention a large change in carbon biomass.

Pg 1445, line 9: grammar issues

Pg 1446, line 13. Not sure “reliable” is the right word here.

Pg 1447, line 10: grammar issues

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