Review 1 of the manuscript “Global climate change and the Baltic Sea ecosystem: direct and indirect effects on species, communities and ecosystem functioning” by Markku Viitasalo1 and Erik Bonsdorff submitted to Earth System Dynamics Discussion (https://doi.org/10.5194/esd-2021-73).

ESD specific reviewer statements

1. Does the paper address relevant scientific questions within the scope of ESD?

   Yes

2. Does the paper present novel concepts, ideas, tools, or data?

Not yet so much. There are new tools (or methods) to better bridge gaps in cross-compartment community structure analyses (e.g. by fuzzy coding or metabarcoding). But there are also new methods (namely compound specific isotope analyses of amino acid nitrogen, CSIA) that bridge gaps in cross-compartment functional diversity analyses more directly and which, different to other “multi trait” approaches, include significant reduction of complexity by addressing functional groups (according to Tilman 2001: phototrophs, mixotrophs, heterotrophs, herbivores, different levels of carnivores,) rather than individual traits or taxonomic groups. CSIA allows for the direct measurement of the mean trophic position of a field sample as one key cross-compartment functional trait (according to Tilman 2001) and of the dominant inorganic nitrogen source used for growth from a field sample as second key cross-compartment functional trait (Tilman 2001) for end-to-end analyses (e.g., physics to fish to human sectors, in sensu Peck et al., 2018) of food webs. Both traits can directly be determined from a single field sample independent from the compartment it comes from (e.g. mixed phytoplankton, mixed mesozooplankton, bivalves, herring, cod, seagulls) and can directly be used as “common currency” across all compartments (e.g., physics to fish to human sectors, in sensu Peck et al., 2018) to calibrate and validate current biogeochemical models. For example, no biogeochemical model yet accounts for the mass and energy loss for fish that must be related to the shift in mean trophic position of mesozooplankton from herbivory (TP of 2) to carnivory (TP of 3) during cyanobacterial blooms in the central Baltic Sea (see specific comment #) and which can help to explain the loss in apex predators in the Baltic Sea.

3. Are substantial conclusions reached?

   Yes, but they are sometimes inconsistent with parts in the earlier text. For example, I still don´t know if the research up to now points to N_2-fixing, unpalatable cyanobacteria as “winner” of cc or not. So conclusions should be somewhat refined.

4. Are the scientific methods and assumptions valid and clearly outlined?

   Generally yes. Yet, I feel like important empirical field based studies are still missing in this review as marked in the specific comments.

5. Are the results sufficient to support the interpretations and conclusions?

   See point 3.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Pls explain how you found and chose your reviewed literature (e.g. did you use google scholar, did you visit the webites of major Baltic Sea research institutes for most recent publications etc.?).

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Not applicable for an invited review article, I think.

8. Does the title clearly reflect the contents of the paper?

Authors should definitely add a definition of ecosystem functioning into your introduction or as a glossary, e.g. Ecosystem Functioning (Tilman 2001): The rate, level, or temporal dynamics of one or more ecosystem processes like primary production or nutrient gain or loss.

9. Does the abstract provide a concise and complete summary?

I will suggest a major revision and there might be changes in the abstract necessary due to that. For example, what about the internal P storage in the sediments that gets released under anoxic conditions? How will that influence the projection of future cyanobacterial blooms in the Baltic Sea?

10. Is the overall presentation well structured and clear?

The authors could more systematically show, which direct and indirect effects have been addressed by which kind of research. Generally, I feel that new knowledge based on empirical research did not always find its way into here, if the “titel” did not include certain key words. See also reply to 6.

11. Is the language fluent and precise?

    There are some minor spelling and grammar errors that need to be corrected for.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

    Not applicable.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

The structure could be sharpened a little bit maybe. For example, I find it more intuitive to start with phototrophs like phytoplankton and cyanobacteria rather than with “Microbial communities”. Also, cyanobacteria is a very broad group. Maybe palatable cyanobacteria should be differentiated from unpalatable ones, as well as those that do fix nitrogen from those that don´t fix nitrogen but only profit from leacking out of diazotroph nitrogen from the N2-fixing ones. More in the revised pdf attached.

14. Are the number and quality of references appropriate?

There are important papers missing at the moment, I think. Pls see the revised pdf for details.

15. Is the amount and quality of supplementary material appropriate?

Not applicable.

General comment

I enjoyed reading and reviewing this manuscript and I think it’s worth publishing. That's said, I think that there are still many improvements possible to make it even better. For example, the authors did not find yet a good balance between a “too detailed” revision of a study and a “to brief” review of a study. In many places, I have a quarrel with the statements of many studies, which are often generalized beyond recognition (anytime I request: “Pls be more specific”). Maybe the authors can add some details here and there, namely where mechanisms are mentioned but which are hardly explained in sufficient detail (pointed out below in the specific comments).

The scientific community currently is struggling to find “the right” definition of functional diversity and other terms and how to investigate functional diversity. I think it’s worth crediting the different approaches and the dissent. Some methods allow for an indirect approach to study functional diversity (those including taxa), others a direct approach (those including only functional groups). A review is a chance to point out methodological improvements over the last years and the review would gain relevance if some cross-compartment approaches are included here (e.g. specific comment #79).

Specific comments

1) p 1, ll. 3: add a definition of ecosystem functioning into your introduction or as a glossary, e.g. Ecosystem Functioning (Tilman 2001): The rate, level, or temporal dynamics of one or more ecosystem processes like primary production or nutrient gain or loss

2) p. 4, ll. 60-64: After Tilman (2001) the definition of ecosystem functioning (EF) is: The rate, level, or temporal dynamics of one or more ecosystem processes like primary production or nutrient gain or loss.

My question: How do you define EF in this review and which processes do you include in your review and which do you not include and why (e.g. are there other reviews out to refer to like "Wannicke, N., Frey, C., Law, C. S., & Voss, M. (2018). The response of the marine nitrogen cycle to ocean acidification. Global Change Biology, 24(11), 5031-5043.")? Pls add this information into your text.

Pls add a side note with definitions for the most prominent terms in your review, e.g.
-climate change
-ecosystem functioning
- trophic dynamics etc.

3) p. 4, ll. 68: wording→ „..more light onto (not into) the complex.."

4) p. 4, ll 67-69: I understand that there is an upcomming review on the cc projections associated with this review. Still, for the "stand alone status" of this review it would be very helpful, to specify here in more detail the projected "abiotic" changes that possibly are most important for the biology (maybe less the atmospheric forcing behind them) including namely salinity, temperature, stratification and oxygen as well as OA, nitrate and phosphate levels, the latter from both, rivers and the anoxic sediments. Important to include: How certain or uncertain can we be about them (e.g. in line 537-539)?

5) p. 4, ll. 70: add: „the" before year

6) p. 4, ll. 71: add „field based" before "responses"

7) p. 4, ll. 75: „food web dynamics", what do you mean by this? Pls specifiy.

8) p. 4, ll. 75: add „both directly in the field as well as in experimental studies." At the end of the sentence.

9) p. 4, ll. 76: What kind of modeling studies, pls specify.

10) p. 4, ll. 85: Why don´t you start with the autotrophic communities instead of the heterotrophic microbial community in Chapter 2?
Phototrophs form the base of the food web also for the heterotrophic microbial community that you seem to mainly refer to in Chapter 2.1.
Also, you should define, which organisms you mean with "microbial community", e.g. only heterotrophs?
What about marine viruses and fungi?

11) p. 4, ll. 87: add "(OA)" after ocean acidification

12) p. 4, ll. 90: How did community change? Be more specific.

13) p. 4, ll 91: Which OTUs? Be more specific.

14) p. 4, ll. 92: How did the microbial community respond? Be more specific.

15) p. 5, ll. 94: Give range also for CO2 as for Sal and Temp.

16) p. 5, ll. 99 delete bracket before “Berner”

17) p. 5, ll. 99 delete extra dot after et al.

18) p. 5, ll. 100-101 Changed drastically to what? Be more specific. What does a "high temperature community" look like?

19) p. 5, ll. 102-103 Which increase? Unclear where you refer to here.

20) p. 5, l. 104 (end of paragraph) What about the effect of other abiotic variables like stratification, oxygen, nitrate and phosphate on heterotophic bacteria, viruses and fungi?
Möller, L., Kreikemeyer, B., Gerdts, G., Jost, G., & Labrenz, M. (2021). Fish as a winter reservoir for Vibrio spp. in the southern Baltic Sea coast. Journal of Marine Systems, 221, 103574.
Rojas-Jimenez, K., Rieck, A., Wurzbacher, C., Jürgens, K., Labrenz, M., & Grossart, H. P. (2019). A salinity threshold separating fungal communities in the Baltic Sea. Frontiers in microbiology, 10, 680.

21) p. 5, ll. 106 Cyanobaceria is a wide field, namely in the BS. The ecology can be very different. Some are palatable (unicells also called picocyanobacteria seem to be palatabel for mesozooplankton), some are not (the large, filamentouse ones are hardly grazed directly, right?). So pls refine what you mean by cyanobacteria, and why they may (or may not) be problematic.

22) p. 5, ll. 109 Be more specific: not few but last 30? years?

23) p. 5, l. 110 Although with a clear gab between spring and summer blooms, right?

24) p. 5,, ll. 112-116 Is that true for all basins of the BS?

25) p. 5, l. 115 add summarized by:
Spilling, K., Olli, K., Lehtoranta, J., Kremp, A., Tedesco, L., Tamelander, T., ... & Tamminen, T. (2018). Shifting diatom—dinoflagellate dominance during spring bloom in the Baltic Sea and its potential effects on biogeochemical cycling. Frontiers in Marine Science, 5, 327.

Consider adding: Paul, A. J., Sommer, U., Paul, C., & Riebesell, U. (2018). Baltic Sea diazotrophic cyanobacterium is negatively affected by acidification and warming. Marine Ecology Progress Series, 598, 49-60.

26) p. 5, l. 117 Careful: There are hardly Cyanobacteria in the western BS. Pls add the basins that you refer to as this is unclear from the titel of your review, which includes the whole BS, not just the Baltic Proper and adjacent gulfs.

27) p. 5, l. 118 add: "comprising of diatoms" after “spring bloom”

28) p. 5, l. 118 add: "comprising of mainly unpalatable cyanobacteria" after “August”

29) p. 5, ll. 117-121 Here the text is unclear where you refer to the spring and summer blooms, respectively. The top down pressure probably refers to the spring bloom, as cyanobacteria are hardly grazed directly, right? This does not become clear in the text at the moment. Pls rephrase for clarity, e.g., by adding "on the diatom blooms in spring"

30) p. 5, ll. 120-121 You leave out the most interesting info here: change from which phytoplankton group to which other phytoplankton group?

31) p. 5, ll. 122-125 Consider moving into section 3.1. Climate change and primary production in the pelagial

32) p. 5, ll. 122-123 Again: be more specific, which changes in pelagic PP do you mean?

33) Nummer: 20 l. 124 Be more specific: Which ecosystem wide consequences do you mean?

34) p. 5, l. 125
What exactly do you mean with "food web dynamics"?

35) p. 5, l. 126 And with "climate" you mean what again?

36) p. 5, l. 125 add BS literature:
Kiljunen, M., Peltonen, H., Lehtiniemi, M., Uusitalo, L., Sinisalo, T., Norkko, J., ... & Karjalainen, J. (2020). Benthic-pelagic coupling and trophic relationships in northern Baltic Sea food webs. Limnology and Oceanography, 65(8), 1706-1722

37) p. 5, l. 124 Again you talk about changes without specifying which phytoplankton groups were replaced by which other groups. Pls specify.

38) p. 5, l. 127 switch community and biomass as so far you have mainly talked about community

39) p. 5, l. 128 Give an example for dominant species, pls.

40) p. 5, l. 128 Dominating in density (aka abundance) or biomass?

41) p. 5, l. 129 Add something like „..leading to a switch from group x to group y." after effect. Pls be more specific.

42) p. 6, l. 131 Pls define BSI

43) p. 6, l. 132 Pls, explain which change you mean, e.g. be more specific. Otherwise the reader has no clue of what quality the changes are that you review about.

44) p. 6, l. 132 Density or biomass (e.g. cell-carbon) wise community changes?

45) p. 6, l. 138 Do you mean phytoplankton or cyanobateria or both?

46) p. 6, ll. 140-141 How does that mechanism work? Pls explain in more detail. Really, evidence? Or rather indication? What kind of evidence do you refer to? Also, do you mean predicted or projected climate change?

47) p. 6, ll. 142-145 Consider moving into section 3.1. Climate change and primary production in the pelagial

48) p. 6, ll. 143-145 Pls revise sentence for correct grammar.

49) p. 6, ll. 143-145 Add detailed info, e.g.: "..increases by xy% compared to the control of ambient conditions."

50) p. 6, l. 143 Add „the" before „water"

51) p. 6, l. 148 Wording: „decrease in" or "release in grazing pressure from", your choice.

52) p. 6, ll. 150-154 Long sentence. Pls break up in shorter ones for better readability.

53) p. 6, l. 150 Have you defined at the beginning of your review, what you mean anytime you say CC? If not, pls add. If CC includes different variables in different parts of the text, I think you need to specify, which variables you refer to in each case.

54) p. 6, l. 151 add
Jerney, J., S. Suikkanen, E. Lindehoff and A. Kremp (2019). Future temperature and salinity do not exert selection pressure on cyst germination of a toxic phytoplankton species. Ecol. Evol. 9: 4443-4451, doi: 10.1002/ece3.5009

55) p. 6, ll. 150-154 convoluted sentence, pls revise for clarity.

56) p. 6, ll. 154-155 I don´t thing cyanobacteria per se are a problem. Unicells should not be a problem, right? Yet they ae cyanobacteria. The problem are large, unpalatable Cyanobacteria, which should be specified here and elsewhere.

57) p. 6, l. 155 Why another? Which is the first competitive advantage to begin with that you seem to refer to?

58) p. 6, ll. 154-155
This is contradictory to what is stated in ll. 603-605.

59) p. 6, l. 157 Consider the field study: Eglite, E., Wodarg, D., Dutz, J., Wasmund, N., Nausch, G., Liskow, I., et al. (2018). Strategies of amino acid supply in mesozooplankton during cyanobacteria blooms: A stable nitrogen isotope approach. Ecosphere, 9, e02135. https://doi.org/10.1002/ecs2.2135

60) p. 6, ll. 159-161 Like whom? Pls add species.

61) p. 6, l. 160 add "copepod" after small-sized

62) p. 6, l. 160: delete the comma after „small sized“

63) p. 6, l. 161 Give examples of marine taxa.

64) p. 6, l. 162 Give examples for brackish-water taxa.

65) p. 6, l. 164 add the following study to explain at least one underlying mechanism:
Dutz, J., & Christensen, A. M. (2018). Broad plasticity in the salinity tolerance of a marine copepod species, Acartia longiremis, in the Baltic
Sea. Journal of Plankton Research, 40(3), 342–355. https://doi.org/10.1093/plankt/fby013

66) p. 7, l. 167: Small scale impacts like what? Pls be more specific.

67) p. 7, l. 174: In which way are cladocerans and rotifers different functional groups e.g. after the definition of Tilman (2001)? They are different taxonomic groups but as stated it is not clear why they would represent a functional group. E.g. which specific function do they represent? Also: “Shift” from whom? Herbivory, omnivory or carnivory are functional groups. Do you mean eventually a shift from omnivorouse copepods to herbivorouse cladocerans and rotifers? I´m confused, pls clarify. Again, a definition how you define “functional group”, “functional diversity” etc is urgently needed in this review.
Again, I suggest definitions of terms as given in the glossary of “Tilman, D. (2001). Functional diversity. Encyclopedia of biodiversity, 3(1), 109-120.”

68) p. 11, l. 337
What about food quality and transfer efficiency of mass and energy?

69) p. 11, l. 338: After „fish“ add:
Limburg, K. E., & Casini, M. (2019). Otolith chemistry indicates recent worsened Baltic cod condition is linked to hypoxia exposure. Biology letters, 15(12), 20190352.
Möllmann C, Cormon X, Funk S, Otto SA, Schmidt J, Schwermer H, Sguotti C, Voss R, Quaas M (2021): Tipping point realized in cod fishery, Nature Scientific Reports, DOI: https://doi.org/10.1038/s41598-021-93843-z

70) p. 13, l. 390: Consider adding the following study to determine trophic efficiency in field samples, because it includes examples also from the Oeland upwelling and larger Baltic Proper in the Fig. 6 :
Weber, S. C., Loick-Wilde, N., Montoya, J. P., Bach, M., Doan-Nhu, H., Subramaniam, A., ... & Voss, M. (2021). Environmental regulation of the nitrogen supply, mean trophic position, and trophic enrichment of mesozooplankton in the Mekong River plume and southern South China Sea. Journal of Geophysical Research: Oceans, 126(8), e2020JC017110. namely Chapter:”4.4. Ecosystem-Specific Trophic Enrichment in Mesozooplankton”

71) p. 13, l. 390: trophic efficiency: Pls define in the text or glossary

72) p. 13, l. 390 At the moment it does not become clear why some processes are reviewed and others are missing. E.g. what do we know about key processes like nitrification, denitrification, N_2 fixation in a future Baltic Sea?

Suggested References from whichs´results at least some clues may be deduced:

Bartl, I., Hellemann, D., Rabouille, C., Schulz, K., Tallberg, P., Hietanen, S., & Voss, M. (2019). Particulate organic matter controls benthic microbial N retention and N removal in contrasting estuaries of the Baltic Sea. Biogeosciences, 16(18), 3543-3564.

Allin, A., Schernewski, G., Friedland, R., Neumann, T., & Radtke, H. (2017). Climate change effects on denitrification and associated avoidance costs in three Baltic river basin-coastal sea systems. Journal of Coastal Conservation, 21(4), 561-569.

Asmala, E., Carstensen, J., Conley, D. J., Slomp, C. P., Stadmark, J., & Voss, M. (2017). Efficiency of the coastal filter: Nitrogen and phosphorus removal in the Baltic Sea. Limnology and Oceanography, 62(S1), S222-S238.

Hellemann, D., Tallberg, P., Bartl, I., Voss, M., & Hietanen, S. (2017). Denitrification in an oligotrophic estuary: a delayed sink for riverine nitrate. Marine Ecology Progress Series, 583, 63-80.

Olofsson, M., Klawonn, I., & Karlson, B. (2021). Nitrogen fixation estimates for the Baltic Sea indicate high rates for the previously overlooked Bothnian Sea. Ambio, 50(1), 203-214.

Loick-Wilde, N., Weber, S. C., Eglite, E., Liskow, I., Schulz-Bull, D., Wasmund, N., ... & Montoya, J. P. (2018). De novo amino acid synthesis and turnover during N2 fixation. Limnology and Oceanography, 63(3), 1076-1092.

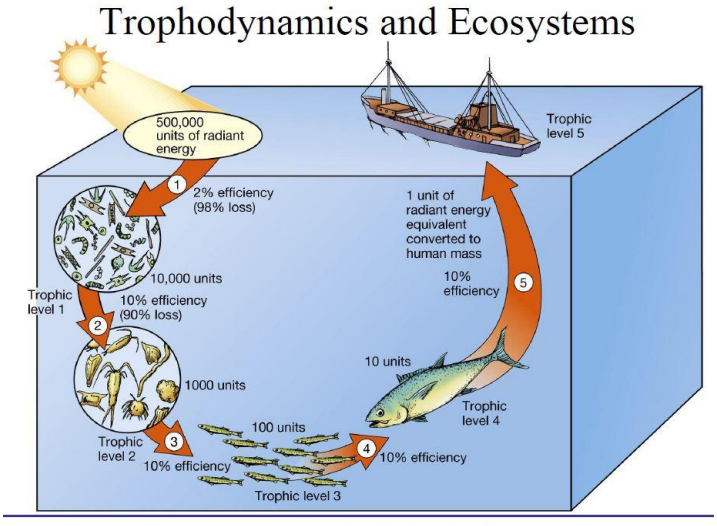
73) p. 13, l. 412-415: This is interesting! Pls explain the mechanism behind this at least briefly.

74)

75) p. 13, l. 413: Add „switches“ after „system“

76) p. 13, ll. 415-418: I don´t understand this. If the food web bases on heterotrophy rather than photoautotrophy, doesn´t that imply that less mass and energy is transfered to fish since the lower food chain is elongated (based on heterotrophs rather than autotrophs) leading to a higher trophic position in mesozooplankton (e.g. carnivorouse zoops instead of herbivorouse zoops, e.g. as documented Loick-Wilde et al. 2019)? Then e.g. eastern Baltic cod would also have a higher TP in such an, at times, heterotrophy based system, compared to a lower TP in western Baltic cod due to a a phototrophy based system, right? Pls clarify.

77) p. 15, ll. 461-467: I disagree with “warming induces a switch from a bottom-up controlled to a mainly top-down controlled system, which may result in increased zooplankton abundance and reduced phytoplankton biomass under warm temperature”. What consequences do you think it has for higher trophic levels like fish or sea birds if mesozooplankton switches from herbivory to carnivory due to increasing densities of unpalatable cyanobacteria in a future Baltic Sea? According to a simple biogeochemical model (see Figure below from T&T 2011), the decrease in mass and energy that is available for TPs above the mesozooplankton compartment should be massiv, shouldn´t it? Pls discuss in a larger context.



(Thurman & Trujillo, Fig. 14.22)

Figure 14.22 shows the passage of energy between trophic levels through an entire ecosystem, from the solar energy assimilated by the autotrophic plankton through all trophic levels to

piscivorous humans. From the assimilated chemical energy, a large fraction is converted by respiration into kinetic energy for sustaining life or is lost as heat, and what remains remains is available for growth and reproduction. Thus, only about 10% of the ingested by herbivores is available for the next trophic level. Since energy is lost at each trophic level, it takes thousands of smaller marine marine organisms to produce a single fish that can be so easily consumed during a meal! Source: Trujillo, A. P., & Thurman, H. V. (2011). Essentials of oceanography (10th edition). Pearson Education. pp. 551.

78) p. 15, ll. 461-467: Add the following field based study about the environmental regulations of a switch from herbivory to carnivory in mesozooplankton in the Baltic Sea in summer in this paragraph:
Loick-Wilde, N., Fernandez-Urruzola, I., Eglite, E., Liskow, I., Nausch, M., Schulz-Bull, D., et al. (2019). Stratification, nitrogen fixation, and cyanobacterial bloom stage regulate the planktonic food web structure. Global Change Biology, 25(3), 794–810. https://doi.org/10.1111/gcb.14546

79) p. 17, l. 535
There is also a significant knowledge gap about the chances of new methodological approaches. So how about including methodological improvements that allow for a significant reduction in trait complexity while considering intraspecific variations in biological samples and specifically allow for the calibration and validation of current biogeochemical models?

Using compound-specific isotope analyses (CSIA) of amino acid nitrogen, it is now possible to measure a continuous trophic position in any biological compartment (as opposed to discrete trophic levels) based on a single field sample, which integrates the assimilation of mass from all the trophic pathways leading to a top predator from different field locations. With this information, we can take the next step of relating the effective TPs e.g. of zooplankton to the environmental conditions measured in situ (Loick-Wilde et al., 2019), providing much needed insights into the mechanisms driving shifts in TP.

The strength of CSIA lies in providing information on both TP and N sources from a single organism/sample, which is achieved with a simple comparison of the $\delta^{15}N$ values of glutamic acid (Glu) and phenylalanine (Phe) amino acids (McClelland & Montoya, 2002; Mompean et al., 2016). While Glu is enriched in ^{15}N by ~8.0‰ per trophic transfer (Chikaraishi et al., 2009), the $\delta^{15}N$ of Phe remains nearly unchanged when the amino acid (AA) is transferred through the food web and thus reflects the isotopic composition of the primary producers (N-source measure, Chikaraishi et al., 2010). This approach largely eliminates potential sources of error in TP estimates associated with temporal and physiological decoupling between a consumer and its diet, and has been refined and confirmed in numerous field- and lab-based trophic studies over the last decade (reviewed by Glibert et al., 2019 and Ohkouchi et al., 2017).

The CSIA based N source identification and mean trophic position from cross-compartment analyses can directly be used to calibrate and validate current biogeochemical models and

allow for an end-to-end quantification e.g. of N inputs from N_2 fixation into apex predators like cod or sea birds.

80) p. 17, l. 535: Why do you refer only to salinity and stratification, what about the other abiotic variables like temperature, oxygen, OA, nitrate or phosphate inputs (either through rivers or from the anoxic sediments)?

81) p. 19, l. 604: The following review is less certain about a future decrease in cyanobacteria, pls discuss more controversial: Munkes, B., Löptien, U., & Dietze, H. (2021). Cyanobacteria blooms in the Baltic Sea: a review of models and facts. Biogeosciences, 18(7), 2347-2378.

What about the internal P storage in the sediments that gets released under anoxic conditions? Stigebrandt, A., Rahm, L., Viktorsson, L., Ödalen, M., Hall, P. O., & Liljebladh, B. (2014). A new phosphorus paradigm for the Baltic proper. Ambio, 43(5), 634-643.

82) p. 19, l. 608: What about empirical field observations/ research? Pls add.

83) p. 20, l. 623: add any missing field and lab studies as pointed out in the text here, too, when applicable.

Technical corrections
Just few, part of the specific comments!