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Interactive comment on "Variable phytoplankton size distributions reduce the sensitivity of global export flux to climate change" *by* Shirley W. Leung et al.

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Apologies - The author comment file titled "Response to Reviewer 1" should actually be "Response to Reviewer 3." This document is the actual author response to Reviewer 1.

For this journal's review process, authors are expected to post a response to all reviewer comments before revising the actual manuscript. Based on these author responses, the editor either invites the authors to submit a revised manuscript or directly rejects the manuscript. We therefore do not yet include a revised manuscript along with answers to the following comments. See more details on the process here:

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https://www.biogeosciences.net/peer_review/interactive_review_process.html.

In the paragraphs below, all reviewer comments will be italicized, while author responses will be in normal font.

This manuscript examines the influence of incorporating information on phytoplankton size into a biogeochemical model alters predicted carbon export in response to climate change. The overall result that the influence of climate change is damped by incorporating size information is intriguing and worthy of publication. Generally, the manuscript is clear and well-written., and represents a useful addition to the literature.

We thank the reviewer for recognizing the contribution our study makes and for their constructive comments, which we believe will improve the manuscript if a revised version is invited.

Abstract:

Line 20: the altered export values are reported without any reference to what the baseline simulation is, i.e. report what is the predicted decline in export without the size considerations, and what is it with the size included

Good point. We will change the sentence from:

“This negative feedback mechanism (termed the particle size-remineralization feedback) slows export decline over the next century by ~14% globally and by ~20% in the tropical and subtropical oceans, where export decreases are currently predicted to be greatest.”

to:

“This negative feedback mechanism (termed the particle size-remineralization feedback) slows export decline over the next century by ~14% globally **(from -0.29 GtC/year to -0.25 GtC/year)** and by ~20% in the tropical and subtropical oceans, where export decreases are currently predicted to be greatest.”

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Line 22: "more robust predictions" – How do you know these predictions are more robust than the baseline? There is no model validation presented (which I don't mind for this manuscript, but "more robust" can't be asserted in this case).

Good point. We will change the sentence to recognize that including the feedback may be important for future predictions, without implying that the feedback is certain to make these predictions more accurate. We will thus change the sentence from:

"Thus, incorporating dynamic particle size dependent remineralization depths into Earth System Models will result in more robust predictions of changes in biological pump strength in a warming climate."

to:

"Thus, incorporating dynamic particle size dependent remineralization depths into Earth System Models **may be important when predicting** changes in biological pump strength in a warming climate."

Introduction:

Line 41: how do the models cited here handle size or sinking speed (if they do at all)? i.e. do these models also already include a size-based parameterisation which means that the predictions are ~equivalent to yours?

Discussion of how ESMs handle size/sinking speed is contained in Lines 59-68, but is relatively brief. In a revised manuscript, we will further expand on how these models work and the different approaches they take, as requested by other reviewers as well.

Line 86-87: a fundamental assumption in this study is that small phytoplankton = small particles. A critical assessment from observational data of whether this is true, and when this assumption might break down, should be included. For example, how might TEP production or fragmentation affect the size structure of particles?

We answer this question and the following Line 91-92 comment together below.

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Line 91-92: references needed for the assertion that small picophytoplankton = small particles, and same for large.

The following is in response to both of the above comments.

The driving mechanism behind the particle size feedback is the relationship between export production and particle size, which we determine empirically from remote sensing data in our study. Our model setup simply computes particle size based on this empirical relationship to export, and makes no explicit assumption about the root cause of the relationship. We hypothesize that the export/particle size relationship arises from plankton community structure simply because this seems like an intuitive mechanism, and is supported by correlative evidence: large particles and large phytoplankton taxa are both generally more dominant in regions of high productivity and export (Cram et al., 2018; Hirata et al., 2011), and we therefore find it reasonable to assume that large phytoplankton aggregate (either directly or by grazing) into large particles. However, this needn't be true for the particle size feedback to hold. Any other mechanism that gives rise to the observed export/particle-size relationship would give rise to the same feedback. In the revised manuscript, we will be more careful about distinguishing between the explicit assumptions and relationships "baked in" to our model, and the mechanisms that we are hypothesizing give rise to those relationships.

We agree completely with the reviewer that TEP production or fragmentation would affect the size structure of particles. Lack of resolution of these processes is a limitation of our study. However, data to constrain these processes are limited and adding TEP and fragmentation would make for a substantially more complex model. Our group is currently working on modeling fragmentation in an ongoing project. We will add more emphasis on these limitations and look forward to future studies examining the importance of these other processes.

Line 97: there's a dawning realisation that Stokes law rarely holds for marine particles e.g. https://aslopubs.onlinelibrary.wiley.com/doi/abs/10.1002/lno.11388 This should be

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acknowledged here.

We will change the sentence from:

"Past work has also firmly established a strong positive relationship between particle size and sinking speed in the ocean (Alldredge and Gotschalk, 1988; Smayda, 1971) (although there are exceptions to this rule, particularly in the Southern Ocean – see McDonnell and Buesseler (2010))."

To:

"Past work has also **suggested a positive relationship** between particle size and sinking speed in the ocean (Alldredge and Gotschalk, 1988; Smayda, 1971), although **there appear to be complications and exceptions to these rules (Cael and White, 2020; Laurenceau-Cornec et al., 2019),** particularly in the Southern Ocean (McDonnell and Buesseler, 2010)."

Added references:

Cael, B.B. and White, A.E., 2020. Sinking versus suspended particle size distributions in the North Pacific Subtropical Gyre. Geophysical Research Letters, p.e2020GL087825.

Laurenceau-Cornec, E.C., Le Moigne, F.A., Gallinari, M., Moriceau, B., Toullec, J., Iversen, M.H., Engel, A. and De La Rocha, C.L., 2020. New guidelines for the application of Stokes' models to the sinking velocity of marine aggregates. Limnology and Oceanography, 65(6), pp.1264-1285.

Line 127: but wouldn't a shift to smaller particles also result in less C sequestration at depth as the C just goes round and round in the upper mesopelagic being readily recycled and re-entrained to surface? Also, Figure 2d – the caption acknowledges that smaller particles leads to greater recycled nutrient supply. This wouldn't increase C sequestration (or CO_2 drawdown) as that depends on the resupply of preformed nutrients which isn't affected by the size considerations used here, at least on the

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timescales considered.

We agree with the reviewer, and make a similar point in our conclusions section. Ocean carbon storage does not just depend on export, but on the sequestration timescale of the exported carbon (Boyd et al., 2019). The particle size feedback helps maintain export, but also results in shallow remineralization and therefore a shorter carbon sequestration timescale. However, our manuscript is solely focused on future changes in carbon export, not ocean carbon storage. Export is a critical process in its own right, even when decoupled from changes in carbon storage, because it is the source of nutrition to the mesopelagic twilight, and therefore determines the productivity of heterotrophic communities, including commercial fisheries. For this reason, carbon export is one of the key variables that is focused on in ocean biogeochemistry forecasts. While changes in the biological pump may also drive changes in ocean carbon storage, these will manifest over longer timescales than changes in export, and will likely be overwhelmed by the effects of anthropogenic CO_2 uptake and solubility-driven outgassing. A detailed exploration of changes in carbon storage is therefore beyond the scope of the current paper, and is only discussed in the introduction and conclusions. However, the reviewer's comment has highlighted the fact that we have not drawn the distinction between carbon export and storage clearly enough. We will revise the manuscript to make this clearer, and incorporate some more detailed discussion of the implications of the PSR feedback for carbon storage in the conclusions section.

Methods:

Line 217: just curious to know why Laws and Dunne estimates of export ratio, rather than others such as Henson et al. 2012 or Siegel et al. 2014 were used

Good question. We use the Laws and Dunne relationships because Weber et al. (2016) showed that when these algorithms, including Henson et al. (2012) were applied to satellite NPP, they gave the best matches to a range of in situ export estimates based on tracer/mass balance approaches. Seigel et al. (2014) was not used because

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it provides no simple formula that can be applied to NPP to estimate export. Instead, they use an ecosystem model that makes its own assumptions about grazing, particle size, etc., and the export ratio is an emergent property of the model.

Line 227: I couldn't find where the in situ observations mentioned here had come from

We will change the sentence from:

"When reporting most-likely values, we weight the nine map sets according to how well each map set's annual mean export matches in situ observations within each region defined here (Table S3; see Weber et al. (2016) for derivation of weighting factors)."

To:

"When reporting most-likely values, we weight the nine map sets according to how well each map set's annual mean export matches in situ oxygen **and mass balance-based observations (Reuer et al., 2007; Emerson, 2014)** within each region defined here (Table S3; see Weber et al. (2016) for derivation of weighting factors)."

Added references:

Reuer, M.K., Barnett, B.A., Bender, M.L., Falkowski, P.G. and Hendricks, M.B., 2007. New estimates of Southern Ocean biological production rates from O_2/Ar ratios and the triple isotope composition of O_2. Deep Sea Research Part I: Oceanographic Research Papers, 54(6), pp.951-974.

Emerson, S., 2014. Annual net community production and the biological carbon flux in the ocean. Global Biogeochemical Cycles, 28(1), pp.14-28.

Line 238-240: is temporal autocorrelation accounted for here? I guess the seasonal cycle in beta and En are similar which will affect the linear regression. Also, are beta and En independent? How are pixels/regions with non statistically significant regressions dealt with?

For the purposes of this study, autocorrelation should not pose a problem. Autocorrela-

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tion only poses a problem if one is trying to determine how much of the relationship between two variables is because they are actually related and how much is just because both are seasonal or slowly varying over time. Seasonality and coincidental variation, from our perspective, both contribute to the relatedness of these two variables and so we want to include both in the calculation of their relationship. Indeed, the seasonal cycle in beta and En greatly affect the linear regression and are an important part of the measured effect/relationship. Their correlation with one another on the shortest available/reasonable timescales is what we were after here, which includes looking at how they vary with one another over months and seasons. We then assume that this relationship that holds on the monthly/seasonal timescale also holds on shorter timescales (i.e., on the timescale in which phytoplankton turn over/change community structure). We will make a note of this assumption. We also computed regression coefficients using monthly anomalies rather than raw monthly values and got similar values.

Beta and En are independent, but related measurements in that beta is derived from particulate backscattering spectra, while En is based on chlorophyll concentrations (as well as SST and euphotic zone depths). Though both particulate backscattering spectra and chl concentrations are ultimately derived from normalized water leaving radiances observed by SeaWiFS, the ways in which they glean information from these radiances are quite different. We will add mention of this in the revised manuscript.

The following further describes how beta is computed. First, the particulate backscattering spectra is computed from the slope connecting particulate backscattering coefficients at 490, 510, and 550 nm. These coefficients are in turn derived from normalized water leaving radiance at these same 3 wavelengths. To convert from a particulate backscattering spectra to beta, Mie modeling is used to establish a physical relationship/lookup table between the two variables (η and ξ in Kostadinov et al. (2009)).

Chlorophyll concentrations, on the other hand, are typically computed as follows: [chl] = 10 ^(a + b*R + c*R ^2 + d*R ^3 + e*R ^4), where R = log10(maximum normalized water leaving radiance ratio out of 443 nm:555 nm, 490 nm:555 nm, and 510 nm:555

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nm), and a-e are constants.

We average over the larger regions in order to avoid generating/using single grid cells with insignificant regressions. That is, avoidance of insignificant and spatially over-resolved/over-specified regression coefficients was the primary reason for averaging over regions.

Line 266: in reality I suspect dbetaobs/dEnobs could vary seasonally. Might be worth a caveat on that point in the discussion? Actually, I suspect that some of the strange behaviour in the SAZ might be due to a seasonal effect or a time lag between changes in phytoplankton size and particle export. It would be helpful to the reader to include some example annual time series of a region showing the PP, export and beta to illustrate how they interact. It would also inform on potential time lags between PP, export and beta.

As was discussed above, we in fact use the variability over the seasonal cycles of beta and En to see how they change in concert over the seasons. Again, this is part of their computed relationship, encapsulated in dbetaobs/dEnobs. We thus compute the relationship between beta and export by in effect exploiting their seasonal variations.

The relationship between beta and export could also be much more non-linear than assumed here. Furthermore, dbetaobs/dEnobs could also vary over time as ocean physics and nutrient limitation conditions are altered by climate warming, for example. Thus, our approach represents a tractable, but simplified approximation. We will make an additional note of this.

We have attached below figures of example beta and En time series over various regions, along with a map showing where the sample points were located within each region. dbetadobs/dEnobs values are calculated over the entire SeaWiFS period (Sep 1997 - Dec 2010), but we show only a random subset of these years for visual clarity. We will add a reduced version of these figures into the supplementary material. (In particular, we will add 1 supplementary figure of export and beta time series in the

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SAZ over all export algorithms, and 1 supplementary figure of export and beta time series over all regions using only 1 example export algorithm.) One can see that there is likely not much of a time lag in the SAZ. Furthermore, from time series over all of the regions, one can indeed see that the vast majority of the variance in both beta and export occurs over seasonal (rather than interannual, say) timescales; thus, it is really their seasonalities that allow us to define dbetadobs/dEnobs in the first place and there is not enough data to get more granular than this (i.e., to subseasonal scales looking at differences between seasons).

Results and Discussion:

A point that should be acknowledged somewhere is that the results presented here are of course still dependent on the details of the model parameterisation and choices.

Good point. We will add this note.

Line 301-303: references needed here

We will add the missing reference to Bopp et al. (2013).

Line 322: isn't this 21% rather than 18%? I found the use of the word "visually" here and on line 307 confusing. It made me think that you had estimated the values by eye rather than calculating them. I suggest just dropping 'visually'.

The 18% reduction was calculated as follows: (export increase w/ feedback - export increase w/o feedback) / (export increase w/o feedback) = (0.23 - 0.28 molC/m ^2/yr) / (0.28 molC/m ^2/yr) = -17.9%.

We thank the reviewer for pointing out that this is confusing - we will drop the word "visually."

Line 346: some C:P ratio must be used here too? Couldn't find where that was mentioned. Does this formulation also assume that all nutrients supplied are regenerated? I think it assumes that all nutrients supplied are turned into PP (which is fine in nutrient

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limited regions), and then are all exported i.e. and e-ratio of 1?

We do not assume that all nutrients supplied are regenerated. Nutrient concentrations are the sum of preformed and regenerated nutrients. We will add a note of this to our existing text in Section 2.1.1, which is quoted below:

"This scheme calculates phytoplankton growth rates as a function of observed annual-mean temperatures (Locarnini et al., 2010) and solar radiation levels (Rossow Schiffer, 1999), along with modeled PO4 3-. We thus explicitly model phytoplankton production in terms of phosphorus consumption and regeneration. We then use an empirical, spatially variable relationship between particulate C-to-P ratios and phosphate concentrations (Galbraith Martiny, 2015) to convert phytoplankton production into units of carbon. It is assumed that 10% of phytoplankton production is routed directly to dissolved organic matter in the euphotic zone, with the remainder becoming particulate organic matter (Thornton, 2013)."

Line 376-378: I'm not sure this "visually" statement helps the reader's understanding here

We will drop the word "visually."

Line 415-426: specify the direction of +ve/-ve changes in the caption. At the moment it's a bit confusing as +ve indicates a reduction

Great idea. We will add this specification.

Figure 1a: specify in the caption that higher beta values = smaller plankton (or mark with arrows on the colour bar)

Great idea. We will add this specification.

Figure 6a: rather than having the right y-axis in remin depth x 100, just write out the numbers in full – it's clearer

We thank the reviewer for this helpful attention to detail. We will change the labels as

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requested.

Figure S1: specify in the caption or legend that higher beta values = smaller plankton

We will add this specification.

Figure S2: add the key to the PP and e-ratio model abbreviations into the caption here

Good point. We will add the key.

Table S1: Define the parameter names in this table.

We will add the parameter names.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-156, 2020.

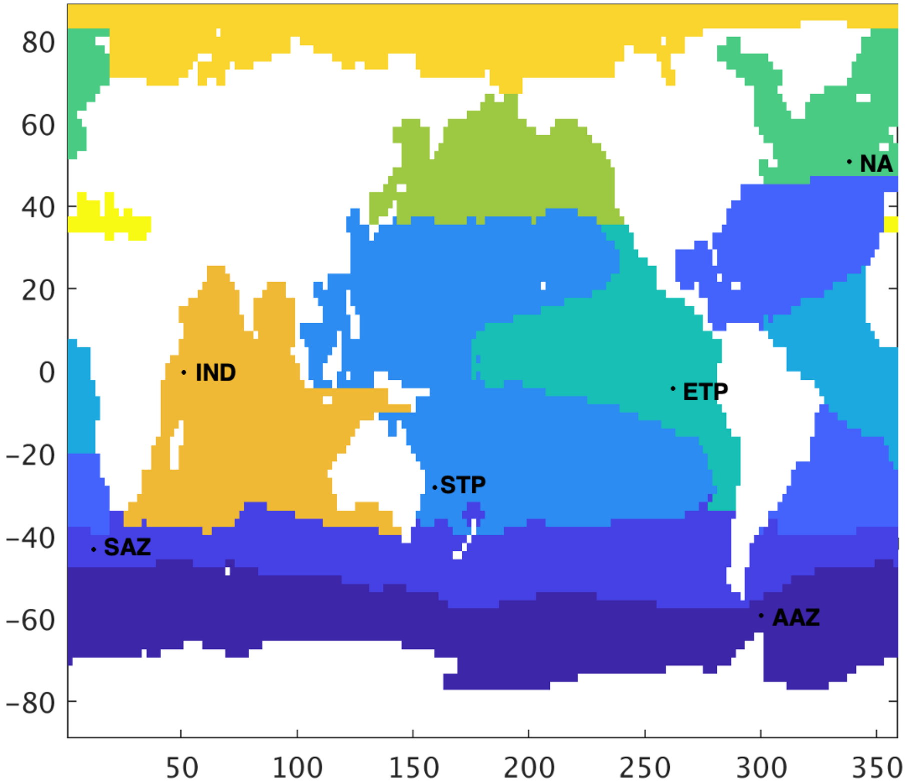

Fig. 1. Map of example time series locations and their associated regions. Each different color on the map delineates a different region as defined in the paper. Corresponding time series are in Figs. 2-8.

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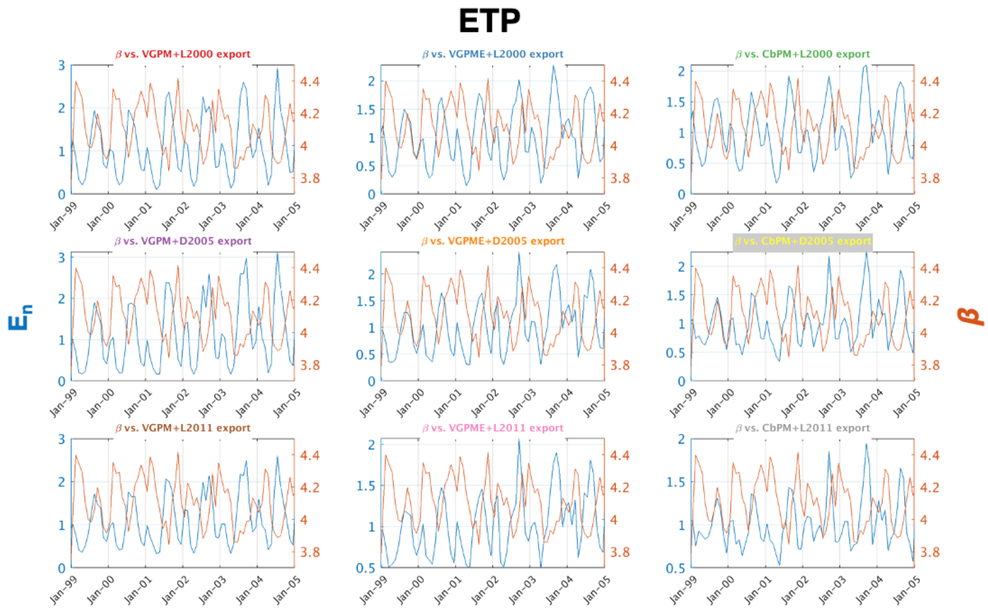

Fig. 2. Example beta and time-mean normalized export time series in the given region.

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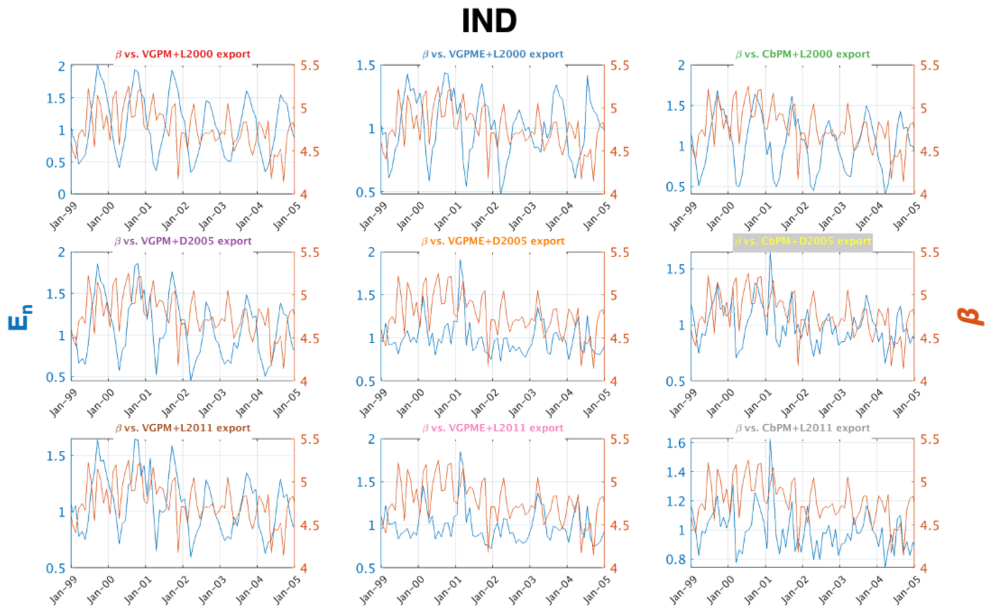

Fig. 3. Example beta and time-mean normalized export time series in the given region.

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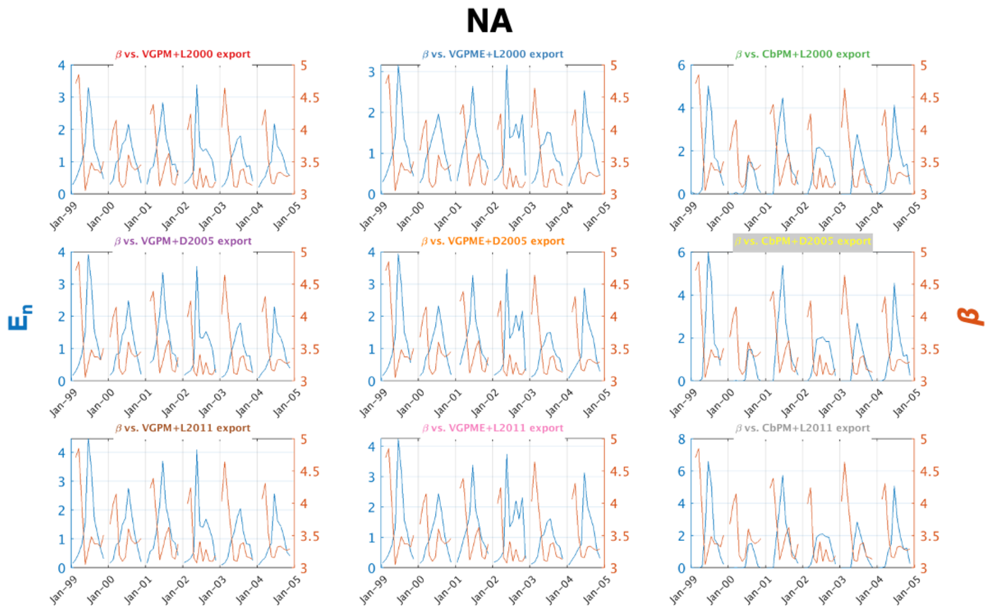

Fig. 4. Example beta and time-mean normalized export time series in the given region.

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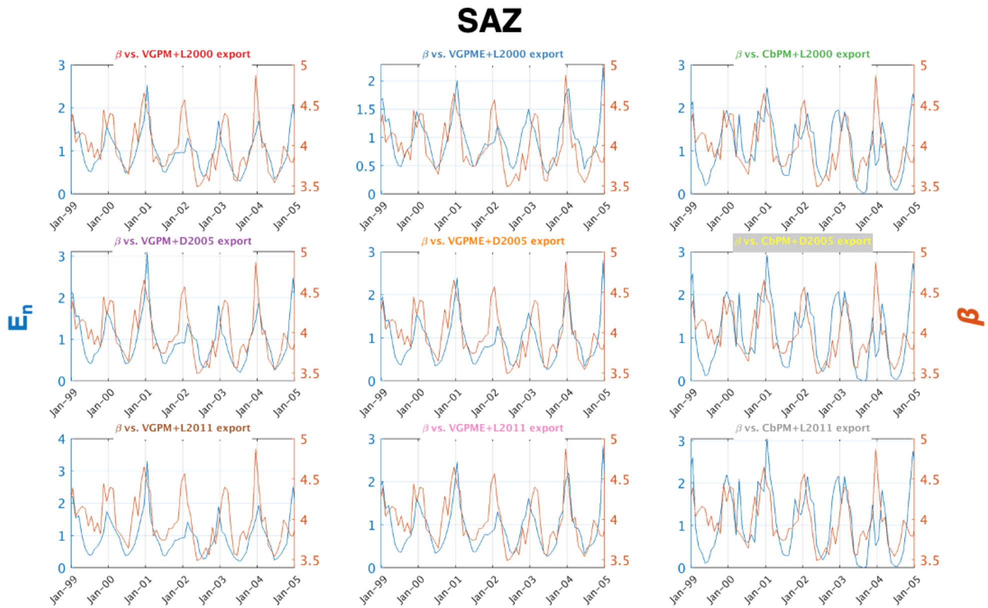

Fig. 5. Example beta and time-mean normalized export time series in the given region.

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Fig. 6. Example beta and time-mean normalized export time series in the given region.

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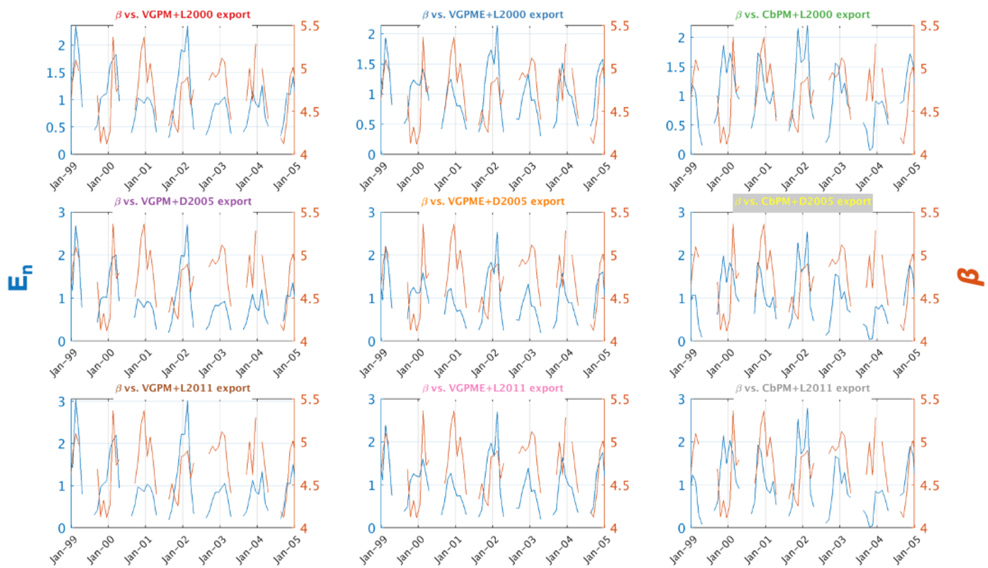

Fig. 7. Example beta and time-mean normalized export time series in the given region.

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