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Interactive comment on "Coastal processes modify projections of some climate-driven stressors in the California Current System" *by* Samantha A. Siedlecki et al.

Samantha A. Siedlecki et al.

samantha.siedlecki@uconn.edu

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We thank both reviewers for their insightful comments, which helped to improve the manuscript. Please find our point by point responses in the following proceeded by the word "RESPONSE". Please note that all of the figures and values in the tables were updated as the runs needed to be redone. One of the ensemble members required an update and so all the downscaled simulations including those forced by the ensemble mean were updated. The patterns and results did not change as a result of this despite the numbers changing.

Reviewer 1: GENERAL COMMENTS ON MANUSCRIPT bg-2020-279 This multi-

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model, downscaled projection of changes in the carbonate system by Siedlecki et al. represents a substantial and timely contribution to the biogeosciences community. It fits nicely within the scope of the journal and should be of interest to a large group of readers. The authors put their own work in the context of the previous literature and they use a sound methodology. The structure of the manuscript is intuitive, and the results are appropriately presented and discussed (for the most part; see below). The "multi-model" aspect of the manuscript is both a curse and a blessing. While we learn quite a bit from this intercomparison, there are multiple instances where the manuscript lacks crucial clarifications. I make several suggestions below on how to clarify the parts where I stumbled, and I believe that the manuscript will be substantially improved if the authors address those points.

I also note that the manuscript submission was rushed. Sections 3.6,3.7,4 repeatedly refer to a "Fig.8" that doesn't exist in the manuscript PDF. Line 85 refers to a "model evaluation provided as supplemental material" but there are no model evaluation in the Supplement. Line 195 refers to colors in Table 1 while Table 1 is in grayscale. Considering that there are 11 authors on the manuscript, I don't understand how none of them was willing to re-read the manuscript before submitting it? Reviewers are supposed to review the science, not proofread.

RESPONSE: We apologize for the confusion this caused the reviewer and appreciate you taking the time to communicate this to us. There was some reshuffling of figures that happened last minute. Specifically, a figure was moved to the supplement and a supplementary figure was moved into the paper. In addition, the journal (Biogeosciences) had requested we alter the table to be in greyscale after we had submitted the manuscript, so the text was not corrected. We have rectified the situation now. Figure 8 was meant to be Figure 7, which is included. References to Figure 8 have been eliminated and Figure 7 references added in their place. The text was updated to reflect that the table is now greyscale, and the model evaluation in supplement was altered to indicate that it is included in Figure 2. Thank you again for giving us the

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opportunity to clarify and improve the paper as a result.

SPECIFIC COMMENTS: MAJOR (1) The comparison between the projections of the 1-degree, 12km, and 1.5km models is a major focus of the manuscript. However, I'm still unclear as to what is compared to what. If I interpret lines 178-184 correctly, each model (1-degree, 12km, 1.5km) is considering a different time period: 1-degree: 1971-2000 (present) versus 2071-2100 (future) 12km: 1994-2007 (present) versus 2085-2100 (future) 1.5km: 2002-2004 (present) versus 2094-2096 (future) If I'm correct, then the differences in time periods contribute to the differences in model projections. I don't consider this mismatch in time periods a deal breaker, but I certainly think that it should be emphasized and discussed up front in the manuscript?

RESPONSE:The reviewer is correct in that the time periods do not match exactly. We have included a table below to clarify and added this information to Table 1 as suggested. The difference between the 12km and 1.5km simulations in time is smaller than the reviewer suggests, and so we have clarified this in the methods text in addition to adding it to Table 1.

While these additions do emphasize these differences a bit more, as the reviewer suggests, the differences in time do not account for the modification we find for the carbon variables. The difference between the carbon dioxide in the present (26 ppm) and future (112-136 ppm) between the 1 degree model and the 12 or 1.5 km simulations is not large enough to account for the modification we see in the runs. pCO2 from the 12km simulations in the upper 200 meters, for example, is 261 ppm greater than the 1 degree simulations project for the entire CCS. The modifications highlighted in Table 1 far exceed this difference in atmospheric carbon dioxide.

Model Time period (present, future) CO2 range (present, future) 1-degree 1971-2000 (present) versus 2071-2100 326-369 (present) ; 685-936 (future).

12 km [2002-2004 (present) versus 2094-2096 (future)] 372-377 (present); 802 (future). 1.5 km 2002-2004 (present) versus 2094-2096 (future) 371 (present); 800 (fu-

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ture)

To add to the confusion, line 185 says: "Comparisons between the 12 and 1.5kmresolution simulations were made with the same year span". What "comparisons" are we talking about? Are we referring to the present state shown in Figure 1? Or are we referring to all figures that show the 12km and the 1.5km together? (Figures 1-7?) What about Table 1 (which includes a "comparison" between the 12km and the 1.5km)? Again, this sort of comparison is supposed to be a major focus of the manuscript and thus these points must be clarified. I would recommend that the time span used for each model configuration be clearly stated in the caption of Table 1 and Figure 1-7.

RESPONSE: We appreciate the opportunity to clarify. The text was updated to reflect that the comparisons are between the downscaled models and the global models, but that the time frames on the downscaled simulations used the same time interval when this was done “Comparisons between the 12- and 1.5-km-resolution simulations and the 1 degree models were made using the same year span despite runs existing for a broader range of years for the 12-km simulation.” Table 1 and Figure captions were also updated with time spans as suggested.

(2) Figure 8, referenced multiple times in the text, doesn’t exist in the PDF document.

RESPONSE: We again apologize for the confusion this caused the reviewer and appreciate you taking the time to communicate this to us. Figure 8 was meant to be Figure 7, which is included. References to Figure 8 have been eliminated and Figure 7 references added in their place.

(3) Line 146: "The 12-km historical simulation forcing is described in Renault et al (in review)..." This reference does not exist in the "References" section and therefore is not available to the reviewers.

RESPONSE: We again apologize for this oversight and appreciate you taking the time to communicate this to us. We have added this reference to the list.

(4) Lines 155-156: "Initial and boundary conditions had the same kind of centennial trend addition for temperature, salinity, and all biogeochemical tracers (O2, nitrate, phosphate, silica, iron, dissolved inorganic carbon, alkalinity)." This statement is vague and leaves much to interpretation. What about adding one little table in the "Supplement" that details what "centenial trend" was assumed for each of those variables? Were the trends assumed constant in time (linear trend)? Constant in space?

RESPONSE: Boundary conditions came from an ensemble of CMIP5 members, each with their own trend. The centennial trend in each was referred to here. A reference was added to the manuscript where more information on this trend can be found (Howard et al. 2020, Table 1).

(5) In figure 3a,b,c, the authors are comparing temperature changes across 3 depths (surface, 0-200m, bottom) using 3 different colorscales. The fact that they use 3 different colorscales for the same variable (temperature) makes it unecessarily difficult to compare Figure 3a,b,c. The same problem arises in Figures 4 and 5 (pCO2, pH, Omega). It should be possible to find a compromise, meaning find a colorscale that works reasonably well for the three depths. If you don't make such a modification, it becomes unecessarily hard for the reader to get a sense of how the changes vary along the vertical dimension.

RESPONSE:Excellent idea. We have implemented this suggested change and have uploaded new figures for 3,4,5 with an updated color scale. Thank you again for helping to make the manuscript clearer.

SPECIFIC COMMENTS: MINOR (6) The text of the abstract uses the symbol Omega without defining what it represents.

RESPONSE: The text was updated with "saturation state" prior to the first use of Ω.

(7) Lines 48-50: "Warming impacts O2 in other ways, for example by raising organismal metabolic rates and accelerating O2 consumption, and by increasing water column

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stratification and thus reducing mixing and ventilation" Is temperature really playing a dominant role in water column stratification in this coastal system? Aren't river inputs and salinity playing a more important role?

RESPONSE:It depends on the season. During the summer months, temperature gradients generated by upwelling intensity largely drive the stratification, while in the winter/spring months when the discharge is highest in the N-CCS, salinity contributes to the stratification structure more. In these simulations, the total discharge was unaltered in the future, only the timing was changed to represent an earlier freshet. The resulting dynamics made warming very important.

(8) Lines 81-83: "We produce multi-model regionally downscaled climate projections of multiple climate-associated stressors (temperature, O2, pH, Omega, and CO2) that resolve coastal processes to create 100-year projections..." If I'm not mistaken, each model configuration (1-degree, 12km, 1.5km) is considering a different time span (lines 178-184), and only the 1-degree configuration corresponds exactly to a 100-year projection (it's more like a 90-year projection in the case of the 12km and the 1.5km models). I think you should include a âĹij symbol in front the "100- year projection" to acknowledge these differences between the 3 model configurations. The same comment applies to Line 179.

RESPONSE: The $\sim$ symbol was added to clarify the approximate time difference. Thank you again for this suggestion.

(9) Line 85: "The model evaluation, provided as supplemental material..." I don't see a model evaluation in the Supplemental material. Please delete this passage, or add a model evaluation to the Supplemental material.

RESPONSE: We apologize for the confusion this caused the reviewer and appreciate you taking the time to communicate this to us. There was some reshuffling of figures that happened last minute. Specifically, a figure was moved to the supplement and a supplementary figure was moved into the paper. The text has been updated to reflect

this change and the current location of the figures in the manuscript.

(10) Lines 101-103: On line 103, please add a note such as: "The CMIP models are further described in Section 2.3."

RESPONSE: We have implemented this suggestion.

(11) Line 126: Typo (...between mode resolutions...)

RESPONSE: We have corrected this typo, and thank the reviewer again for their vigilance.

(12) Lines 147-148: "The 12-km projection was forced by adding a monthly climatological difference between CMIP5 RCP 8.5 scenario forcing and the historical run forcing, averaged over 2071-2100 and 1971-2000, respectively" Doesn't this correspond to the "Delta approach" (or "Delta method")? Wouldn't it be worth mentioning it since it is a common approach for downscaling?

RESPONSE: Yes. We have edited the methods to reflect this change and referenced Alexander et al. 2020 for the method. Alexander, M. A., S. Shin, J. D. Scott, E. Curchitser, and C. Stock, 2020: The Response of the Northwest Atlantic Ocean to Climate Change. J. Climate, 33, 405–428, https://doi.org/10.1175/JCLI-D-19-0117.1.

(13) Line 170: "For the future conditions, atmospheric CO2 concentration (800 ppm), and future atmosphere..." Where is the 800ppm coming from? Is it the difference between 1971-2000 and 2071- 2100 in the median of the CMIP models? Please clarify this statement.

RESPONSE: That's approximately the value in 2085 under RCP 8.5 (range from the time period for the CMIP 5 models =685-936) for future.

(14) Lines 194-195: "is highlighted using pink (amplified) and blue (dampened) colors in Table 1" Please update the sentence (Table 1 is in grayscale).

RESPONSE: We apologize for the confusion. After we submitted the table in color,

the journal (Biogeosciences) had requested we alter the table to be in greyscale, but the already submitted text was not corrected. We have rectified the situation now and appreciate the reviewer's suggestion to do so.

(15) Line 202: "1.2.1 Subsubsection (as Heading 3)" Please delete.

RESPONSE: We have implemented this suggestion.

(16) Lines 206-207: "The largest temperature increase, nearly 3 degrees C, occurs on the shelf in both regions of the projections (Fig. 3" I think the sentence (as currently formulated) is misleading. The "shelf" is defined by bottom depths of *less* than 200m (lines 132-133). So the offshelf region in Figure 3b is representative of the depth interval 0-200m, while the "onshelf" region in Figure 3b could be representing something like 0-100m. Given a strongly stratified variable like temperature, the mismatch in depth can explain the constrast between the offshelf/onshelf regions in Figure 3b.

RESPONSE: We have removed this sentence entirely.

(17) Line 252: "All projections show an onshore-offshore trend in pCO2..." Since you are discussing spatial differences, I would suggest replacing the word "trend" by "gradient".

RESPONSE: We have implemented this suggestion.

(18) Lines 334-336: "When nitrate is included in the upwelling measure, as in BEUTI, there is a slight decline in the upwelling of nitrate (1-2%), commensurate with a decrease in nitrate at the surface in the N-CCS (Fig. 6)" Isn't this a circular reasoning? There is a decrease in surface nitrate (Fig.6), and when we take into account this decrease in our upwelling metric, we get a "slight decline in the upwelling of nitrate"?

RESPONSE: Notably, this sentence does not try to distinguish or attribute the cause of the decrease nitrate at the surface, but instead merely points out that the decrease in nitrate at the surface is consistent with a decline in the upwelling metric which includes nutrients (BEUTI). To further clarify this point we have altered the language of the

sentence to the following: ” When nitrate is included in the upwelling measure, as in BEUTI, there is a slight decline in the upwelling of nitrate (1-2%), consistent with a decrease in nitrate at the surface in the N-CCS (Fig. 6)”

(19) Lines 364-365: "On the shelves of the downscaled simulations, the source waters are further modified by coastal processes including increased productivity, freshwater delivery and denitrification." I don’t question the statement, but these are things that were not shown/demonstrated in the manuscript. Please add "(not shown)" at the end of the statement.

RESPONSE: We have implemented this suggestion

(20) Line 369: "3.7 Modification" Please replace this heading by something less cryptic, e.g., "Differences between the global and downscaled projections".

RESPONSE: We have implemented this suggestion but edited it slightly to read “Differences between the global and downscaled projections: the impact of including coastal processes in regional projections.”

(21) Line 401: "In our projections, the more realistic winds were different than in Dussin et al..." Please clarify this statement. I don’t understand what "more realistic winds" you are referring to.

RESPONSE: Our wind fields included monthly wind anomalies derived from the CMIP5 model outputs added to hourly wind fields from the Weather Research and Forecasting Model (the ROMS hindcast forcing). This leads to variability at multiple timescales (including the hourly to daily timescales important for gas exchange fluxes of biogeochemically relevant variables) combined with climate-driven shifts informed directly by the ESM outputs.

This is a "realistic" approach to the changing winds, as opposed to the approach in Dussin et al. 2019: "add 10% to the meridional wind, over the whole domain, only when blowing southward from early June to late September....The magnitude of the perturba-

tion is chosen to be consistent with models projecting increases in upwelling strength under climate change." However, climate models project a more complicated (and compensating) picture of wind-driven changes: e.g. in this and the related manuscript (Howard et al. 2020), across several CMIP5 models, large wind-driven increases in upwelling favorable winds are only found in the springtime northern and central CCS. But these increases are compensated by increased winter downwelling favorable winds (in the northern CCS), and summertime decreases in upwelling in the central CCS (where wind changes oppose upwelling, rather than strengthen it as assumed above).

In this context, the Dussin et al. 2019 approach to winds is less realistic, though still a valuable idealized model experiment. Indeed, the two approaches broadly led to similar model outcomes, which helps to reinforce the conclusions of Dussin et al. 2019 about the importance of remote biogeochemical forcing (and, as a corollary, the lower sensitivity to wind-driven changes).

(22) Lines 147-148 were describing a "Delta approach" where the 12km and 1.5km models use the same winds in "present" and "future" times, except for the addition of a "Delta" computed from a 1-degree resolution global model. Now Line 497 suggests something completely differentâĂŤthat the 12km and 1.5km models were directly using the winds from the 1-degree resolution global model: "The downscaled projections are driven by the same forcing as the global simulation" Which one is right? This must be clarified.

RESPONSE:The description of the winds in response to comment 21 (CMIP5 anomalies added to WRF hindcast winds) may help address this comment; the first description is correct, though another detail is that the wind-current coupling is parameterized rather than run as a fully coupled ocean-atmosphere (Renault et al. 2020 and references therein). The Deltas (CMIP5 2100-2000 changes) are interpolated to the higher resolution WRF grid, and added onto the hourly WRF wind fields. This description was added to the methods. “Atmospheric conditions including air-temperature at the sea surface, precipitation, and downwelling radiation are derived from an uncoupled

Weather Research Forecast model output (c3.6.1; Skamarock et al. 2008) as in Renault, Hall, & McWilliams (2016) and Renault et al. (2020) with more information in Howard et al (in review). To avoid the computational cost of a fully-coupled ocean-atmosphere model, wind and mesoscale current feedbacks are parameterized with a linear function of the surface wind stress as in Renault, Molemaker, et al. (2016). This linear relationship is supported by observations in the CCS (Renault et al. 2017).". The CMIP anomalies are taken at as high a resolution as possible, and are daily. The second statement at Line 497 commented on above was corrected to reflect this clarification "The downscaled projections are driven by the same delta forcing as the global simulation".

Just to clarify, the wind is not the dominant driver for many of the changes presented in this manuscript. So while the methods will be clarified as requested, the big-picture answers are insensitive to these details.

(23) Figure 4a,b,c: Would it be possible to add a little "tick mark" on the colorscale, indicating the projected change in atmospheric CO2 concentration between the "present" (1971-2000) and "future" (2071-2100) periods (according to the median of the CMIP models)? This would provide some perspective on the magnitude of the changes in paper pCO2 shown in Figure 4a,b,c.

RESPONSE: Yes, we have implemented this suggestion.

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Region	Global	CCS			N-CCS			
Model [Time Interval]; atmospheric CO_2 (present); atmospheric CO_2 (future);	A. 1 degree [1971-2000 (present) vs. 2071-2100 (future)]; 326-369 (present); 685-936 (future).	B. 1 degree [1971-2000 (present) vs. 2071-2100 (future)] 346 (present);	C. 12 km [2002-2004 (present) versus 2094-2096 (future)] 372-377 (present); 802 (future)	D. 12 km Ensemble spread	E. 1 degree [1971-2000 (present) vs.2071-2100 (future)]; 326-369 (present); 685-936 (future).	F. 12 km [2002-2004 (present) versus 2094-2096 (future)] 372-377 (present); 802 (future)	G. 1.5 km [2002-2004 (present) versus 2094-2096 (future)] 371 (present); 800 (future)	H. 12 km Ensemble spread
200m avg								
ΔTemp (°C)	1.97	1.63	1.81, 2.55*	1.38 to 2.24	2.21	1.95, 2.24*	2.32, 2.62*	1.55 to 2.35
ΔOxygen (ml/l)	-0.19	-0.52	-0.62, -0.56*	-0.52 to -0.72	-0.56	-0.72, -0.66*	-0.61, -0.50*	-0.52 to -0.92
ΔpCO_2 (μatm)	401	492	**759, 707***	682 to 836	527	**923, 922***	**721, 620***	780 to 1066
ΔpH	-0.297	-0.321	-0.333, -0.322*	-0.310 to -0.357	-0.332	-0.315, -0.327*	-0.322, -0.311*	-0.278 to -0.352
$\Delta\Omega_{ca}$	-0.85	-0.71	-0.70, -0.68*	-0.65 to -0.75	-0.62	*-0.47, -0.53**	*-0.58, -0.62**	-0.41 to -0.53
$\Delta\Omega_{ar}$	-1.33	-1.11	-1.11, -1.08*	-1.03 to -1.19	-0.99	*-0.74, -0.85**	*-0.93, -0.98**	-0.65 to -0.83
Surface								
ΔTemp (°C)	2.49	3.12	3.31, 3.27*	2.57 to 4.05	3.15	3.30, 3.26*	2.89, 2.86*	2.42 to 4.18
ΔOxygen (ml/l)	-0.23	-0.32	-0.35, -0.39*	-0.27 to -0.43	-0.38	-0.41, -0.41*	-0.40, -0.39*	-0.30 to -0.52
ΔpCO_2 (μatm)	379	392	**435, 432***	433 to 437	365	**429, 418***	**352, 344***	424 to 434
ΔpH	-0.309	-0.319	*-0.286, -0.291**	-0.285 to -0.287	-0.343	*-0.298, -0.297**	*-0.274, -0.271**	-0.296 to -0.300
$\Delta\Omega_{ca}$	-0.98	-0.96	*-0.90, -0.85**	-0.86 to - 0.94	-0.76	**-0.85, -0.85***	**-0.76, -0.71***	-0.82 to -0.88
$\Delta\Omega_{ar}$	-1.52	-1.50	*-1.42, -1.35**	-1.37 to -1.47	-1.21	**-1.35, -1.35***	**-1.21, -1.14***	-1.30 to -1.40
Bottom (<500m)								
ΔTemp (°C)	1.98	1.65	1.84, 2.33*	1.47 to 2.21	1.34	**1.75, 1.90***	**2.05, 2.35***	1.40 to 2.10
ΔOxygen (ml/l)	-0.22	-0.43	-0.56, -0.64*	-0.37 to -0.75	-0.63	-0.66, -0.68*	-0.60, -0.60*	-0.40 to -0.92
ΔpCO_2 (μatm)	432	505	**785, 883***	650 to 920	592	**965, 1038***	**840, 899***	776 to 1154
ΔpH	-0.306	-0.286	-0.259, -0.318*	-0.228 to -0.290	-0.333	*-0.271, -0.292**	*-0.279, -0.308**	-0.230 to -0.312
$\Delta\Omega_{ca}$	-0.68	-0.47	*-0.42, **-0.59****	-0.38 to -0.46	-0.32	-0.35, -0.38*	-0.43, **-0.50***	-0.30 to -0.40
$\Delta\Omega_{ar}$	-1.06	-0.74	*-0.66, **-0.93****	-0.59 to -0.73	-0.50	-0.56, -0.61*	-0.68, **-0.79***	-0.48 to -0.64

Fig. 1.

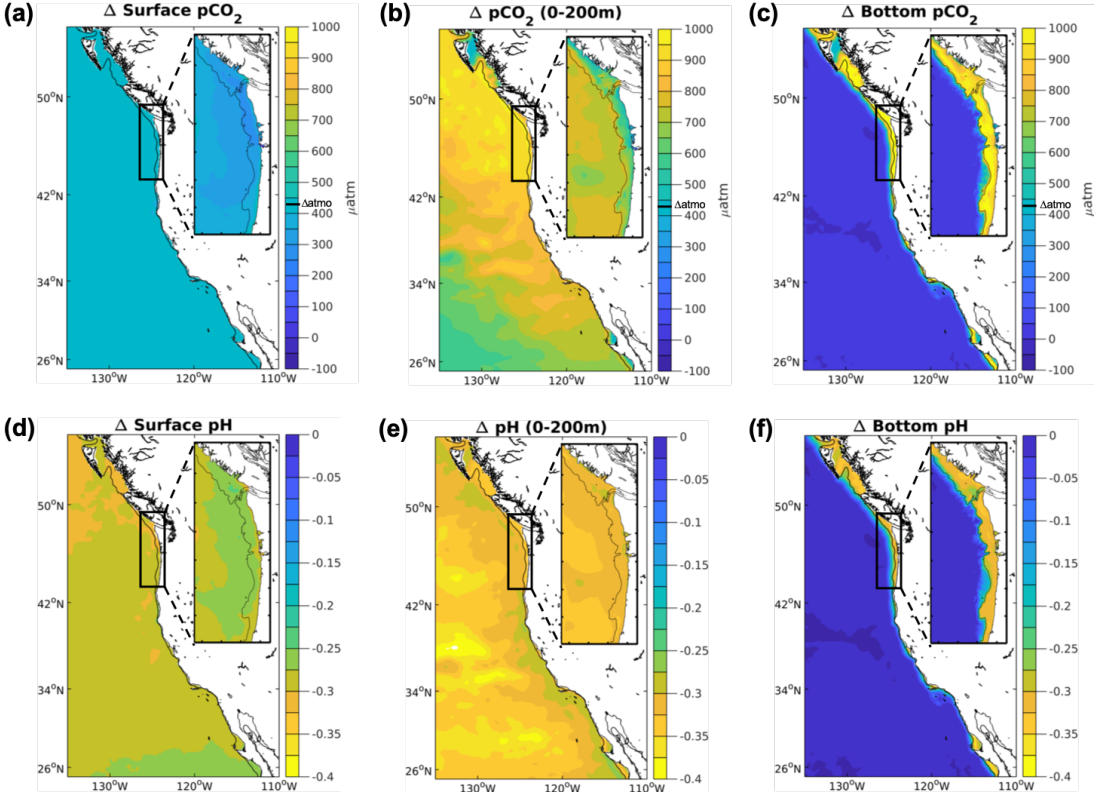

Fig. 2.

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