Response to referee #3. Svetla Hristova-Veleva

Referee #3:
Overview and recommendations:
The paper addresses important questions regarding improving the performance of Earth System Models (ESM) – the important tools to study and understand the complexity of the Earth's climate. Improving these models is a major goal of the science community as they can be a very valuable tool in studying the response of the Earth's system to anthropogenic forcing, providing guidance to policy makers.

Reply: Thanks.

Referee #3:
In particular, the paper investigates the impact of a new parameterization of CO2 emissions that the authors have recently developed, called the POPEM module (POpulation Parameterization for Earth Models). POPEM presents an important advancement in the way CO2 emissions are modeled, as it accounts dynamically for the changing emissions. Like previous research, POPEM uses population data as proxies for emission. What is unique to this new parameterization, though, is that it models the evolution of the population while previous research has relied on historical data, hence not being dynamical, preventing them from making reliable predictions for the future emissions and the response of the climate system.
Using this new parameterization (POPEM) presents an important advancement and this makes the described research very valuable.

Reply: Thanks.

However, before going forward one have to evaluate the performance and assess the impact of the new parameterization. Indeed, this is the goal of this paper.
The paper begins by describing what is unique about POPEM.
It then validates the stand-alone performance of POPEM by comparing its predication over a past 63 (and 70) -year period to existing data. The comparison is done globally but also by several regions. This validation is done in two ways: by comparing forecasted to observed population growth rates; and by comparing the forecasted to observed emission rates. The results show that despite the difficulty of predicting non-linear trends in the growth of population and emissions, POPEM preforms quite well. These comparisons give credibility to the POPEM forecasts, hence to its use in forecasting future scenarios.

Next, the paper uses a coupled ESM, the Community ESM (CESM) to evaluate the impact of POPEM. The evaluation focusses on the impact of POPEM on two very important, and difficult to predict, parameters of the Earth's system - the precipitation and the sea surface temperature (SST). The evaluation is done in two ways:

- *by comparing the results from a control run (using global CO2 concentration parameters that I believe are homogeneous – this needs clarification) to those*

from POPEM. This choice of model setups highlights the value of POPEM as it predicts the population (and the emissions) in every grid point, showing the impact and the importance of the spatial variability.

- By comparing both control and POPEM forecasts to actual observations (over a 20-year period for precipitation and 50-year period for SST).

The paper finds that:

- The global predictions for both parameters compare to the observations in a very similar way for the CONTROL and the POPEM simulations. Hence, the more realistic POPEM parameterization "does no harm". This is an important test and conclusion because it is occasionally the case that including more realistic parameterizations might degrade the performance of the forecasts for certain parameters. This is because often the models are "tuned" to predicting some of the parameters, giving the right answer for the wrong reason, and impacting negatively the forecasting of the non-tuned parameters when the more realistic parameterizations are employed.

- More importantly, the paper finds that using POPEM results in regional differences between its forecasts and that of the control run. Comparison to observations seems to suggest the POPEM produces better regional distribution of the precipitation. This is a very important conclusion, in my view. It does not seem to be well highlighted in the paper summary.

Overall, the paper addresses a very important topic. The approach is sound and uses a very good modeling framework. There is a very extensive set of references. The paper is presented in a fluent and precise language. However, there are several places where the paper could be improved, as detailed below.

Because of all that, I propose the paper be accepted with minor revisions.

Reply: Thanks for highlighting the main findings of the manuscript and for your detailed revision of the paper. Also, thanks for your suggestions and comments. We consider that they improve the global quality of the paper.

Referee #3:

Title: The current title is: "Improving the representation of anthropogenic CO_2 emissions in climate models: a new parameterization for the Community Earth System Model (CESM)"

I would suggest a modification to read "Improving the representation of anthropogenic CO2 emissions in climate models: Impact of a new parameterization for the Community Earth System Model (CESM)"

The reason is that main goal of the paper is not to describe the new parameterization but to evaluate its performance and impact.

Reply: Indeed, the suggested title describes more precisely the aim of the paper. Thanks. The title now reads:

> ***Improving the representation of anthropogenic CO_2 emissions in climate models: impact of a new parameterization for the Community Earth System Model (CESM).***

Referee #3: *Abstract*
"The results show that it is indeed advantageous to model CO_2 emissions and pollutants directly at model grid points rather than using the forcing approach". Please, reword as it is not clear (at this point) what is this forcing approach.

Reply: We rewrote the sentence to make the point clearer.

The text reads:

> *The results show that it is indeed advantageous to model CO_2 emissions and pollutants directly at model grid points rather than using the same mean value globally.*

Referee #3:
Introduction:
The reader would benefit from a more detailed description of the existing approaches to modeling CO2 emissions. What I gather from the paper is the following: there are two basic approaches that models use to account for CO_2 forcing:

- *a) using globally homogenous forcing;*
- *b) using non-homogenous, grid-point specific forcing. This one can be applied in several ways:*

1. using Representative Concentration Pathways (RCPs) that "are not fully-integrated socioeconomic parameterizations, but rather estimates for describing plausible trajectories of human climate change drivers .... They provide simplified accounts of human activities and processes, including population density and economic development, from non-coupled Integrated Assessment Models (IAMs;)" Question: are these parameters location- specific? This is what I am understanding.

2. the proposed here POPEM model being integrated into a fully coupled model. This is similar to RCPs but: uses a coupled model; uses a dynamic model for the prediction of population and emissions.

- *c) Is my understanding correct???*

Reply: Our apologies. We did not make the point clear. It is the other way around: RCPs are used as a surrogate for point-wise estimates. We have clarified that in the revision of the paper [see next comments for more details]

Referee #3:

. *d) If so, I would suggest two possible modifications:*

1. Use some wording or structure as what I've described above

2. Space-permitting, create either a small table or a flow diagram that shows these different levels of sophistication

Reply: We have rewritten the two paragraphs to clarify the differences between RCPs and POPEM approaches. Thanks.

The amended paragraphs now read:

> *One of the fields most in need of development is the inclusion in global models of co-evolutionary dynamical interactions of the socioeconomic dimension into global models with other Earth system components (Nobre et al., 2010; Robinson et al., 2017; Sarofim and Reilly, 2011). Human activity was a major driver of change in the Earth System in the recent past (Alter et al., 2017; Barnett et al., 2008; Crutzen, 2002), and it now dominates the natural system (Ruth, et al. 2011). However, most global models use basic socioeconomic assumptions about the behavior of societies and are only unidirectionally linked to the biogeophysical part of the Earth system (Müller-Hansen et al., 2017; Smith et al., 2014). The standard way of introducing anthropogenic climate change into ESMs is through Representative Concentration Pathways (RCPs). These are consistent sets of projections involving only radiative forcing components (van Vuuren et al., 2011), but which represent a step forward from the scenario approach of the last decade (Moss et al., 2010; van Vuuren et al., 2014; van Vuuren and Carter, 2014). However, RCPs are not fully-integrated socioeconomic parameterizations but rather estimates for describing plausible trajectories of human climate change drivers (Moss et al., 2010; Vuuren et al., 2012). They provide simplified accounts of human activities and processes from one-way coupled Integrated Assessment Models (IAMs, Müller-Hansen et al., 2017).*
>
> *The use of RCPs is advantageous because they provide a set of pathways that serve to initialize climate models. However, two major problems remain within this approach. Firstly, human activities are not intrinsically embedded into the ESM, impeding sensitivity studies. Secondly, because of the weak coupling of IAMs, they cannot capture the sometimes counterintuitive bidirectional feedback and nonlinearity between the socioeconomic and natural subsystems (Motesharrei et al. 2016; Ruth et al. 2011). Good examples that illustrate the importance of including such bidirectional feedbacks feature in the HANDY model (Motesharrei et al. 2014) which has been used to analyze the key mechanisms behind societal collapses using the predator-prey model.*
>
> *The RCP approach has been used in climate models because of its low computational cost. However, advances in computational resources now allow to*

parameterize human-Earth processes in a more detailed way, including the inclusion of population dynamics into the modeling, as in the POPEM (POpulation Parameterization for Earth Models) module (Navarro et al., 2017).

Referee #3: *P. 2, lines 25-30 – It says: "Given the highly non-linear character of the processes involved, it is not unreasonable to assume that location is significant, and the spatial and time distribution of these emissions may affect global climate" – a bit unclear. Might be better to say ", it is not unreasonable to assume that specifying (or accounting for) geographical variability is significant"*

Reply: We modified the expression following your suggestion. The text now reads:

> *Given the highly non-linear character of the processes involved, it is not unreasonable to assume that accounting for geographical variability is significant, and the spatial and time distribution of these emissions may affect global climate (Alter et al., 2017; Grandey et al., 2016; Guo et al., 2013).*

Referee #3: *P. 3, lines 2-4: "The aim of this paper is to show that this grid point scale modeling of anthropogenic CO2 emissions (and other pollutants) represents an improvement, and that two important variables, namely global precipitation distribution and surface temperature, are not negatively affected by this more-detailed approach." While this is true I believe this is a rather weak statement regarding the benefits of using POPEM-type parameterization of emissions forecasting. I believe the authors are in a position to make a stronger statement,*
namely: including the POPEM dynamical forecasting approach that accounts for the spatial and temporal variability of the emission sources, leads to better representation of the geographical variability of the precipitation.

Reply: We rewrote the las part of the paragraph to include your suggestion.

The text now reads:

> *The aim of this paper is to show that this grid point scale modeling of anthropogenic CO_2 emissions (and other pollutants) represents an improvement over simpler approaches, and leads to better representation of the geographical variability of precipitation.*

Referee #3: *Space-permitting, I would suggest that the* ***Introduction*** *ends with a short description of the outline for the following presentation. Something like: " the following sections outline: the unique features of POPEM; the validation of the POPEM stand-alone performance; the framework for evaluating the impact of POPEM – incorporation into CESM and framework for testing; the comparison between a control run and a POPEM-specific one: evaluating the differences between the two; evaluating how each compares*

to observations; discussions; summary and conclusions;" This would give the reader a clear structure of the paper to follow and will make it easier to highlight the contributions of the paper.

Reply: Thanks for the suggestion. We added a new paragraph with a short description of the outline.

The new paragraph reads:

> *The paper is organized as follows: in section 2, we present the validation of the POPEM standalone mode and set the framework for evaluating the impact of POPEM parameterization –its incorporation into the CESM and the testing framework; in section 3, we compare the outputs of CONTROL and POPEM runs and see how they compare with observations. In the conclusion and future work section, we highlight the importance of the dynamical modeling of anthropogenic emissions at grid point scale to better represent the socioeconomic parameters in the CESM model and improve precipitation estimates.*

Referee #3:
Section 2.2
currently there are sections 2.2 and 2.2.1 but not 2.2.2 or more. It seems that there is no need for 2.2.1. If there is no 2.2.2. I would suggest the following: "2.2 POPEM specifics and validation", followed by "2.2.1 POPEM parameterization model overview: Unique features" and "2.2.2 POPEM trend verification". Of course, this is just a suggestion.

Reply: Thanks for the suggestion. We rewrite subsection titles and numbers to have a clearer structure.

Now, subsections titles are:

2.2 POPEM specifics and standalone validation
2.2.1 POPEM parameterization model overview
2.2.2 POPEM trend verification

Referee #3: *P. 6, lines 8-9 – "Our control case used global CO2 concentration parameters (standard procedure in ESMs), while the POPEM case used geographically-distributed CO2 emissions data" - is the control using homogeneous CO2 concentrations? I am pretty sure this is the case but it might be better to say it this way.*

Reply: [already discussed above] We have replaced the word '**global'** with the word '**homogeneous'** to make it clearer.

Text now reads:

*Our control case used **homogeneous** CO_2 concentration parameters (standard procedure in ESMs), while the POPEM case used geographically-distributed CO_2 emissions data.*

Referee #3:
Section 3.1
P.7, line 23 – it appears that figures 6C, 6D, 8C and 8D are referenced before figures 4 and 5 (and the figure 8 is referenced before Fig.7). This should not be the case. The figures should be referenced in order. However, it seems that this is because the current order of the discussions here might need to be modified. Below is what I mean.

a) Maybe the order should be: 1. Test for "no harm" – figures 6C-6D and 8C-8D show that. 2. Compare the CONTROL to the POPEM simulations to see where exactly they differ. 3. Compare both the CONTROL and the POPEM CESM simulations to the observations, looking at regional distributions. The comparison in steps 2 and 3 brings up the impact of the POPEM geographically-aware CO2 emissions on the geographical distribution of the precipitation, highlighting the positive impact POPEM has (especially in step3).

b) Steps 2 and 3 could be switched – depending on what the authors think.

c) I want to point out that the proposed change in the order of the presentation is just a suggestion for the authors to consider.

Reply: Thanks for the suggestion. We have restructured the order of the figures to make it clear.

***Referee #3:** P.8, lines 2-3: "It is clear from the figure that POPEM does alter the spatial pattern of precipitation and exerts a definite effect on the climate pattern, as the module reduces the otherwise exaggerated ITCZ precipitation in the Southern Hemisphere (South East Asia and Australia)." Do you have a reference that it was exaggerated?? If so, then this is a very strong point that needs to be emphasized. Also, do you mean Fig. 4 or Fig. 5? Please, specify.*

Reply: The double ITCZ bias is a persistent problem in most climate models. It has been reported by several authors (Mechoso, 1995; Terray, 1997; Lin 2007) and the causes of this bias are still unclear (Li and Xie, 2014). In the Southern Hemisphere, climate models produce an excess of precipitation in the band 10S-15S when compared with satellite observations (Hwang and Frierson, 2012). We have added a few citations to highlight the importance of this issue.

Additionally, we made a new figure (Figure 9) to clarify the improvements of POPEM in the double ITCZ bias [see the next reply].

The paragraph now reads:

It is clear from Figures 5A and 6A that POPEM does alter the spatial pattern of precipitation and exerts a definite effect on the climate pattern, as the module

reduces the otherwise exaggerated ITCZ precipitation in the Southern Hemisphere reported by several authors (Hwang and Frierson, 2013; Lin and Xie 2014).

Referee #3: *P. 8, lines 7-8: "There are also important differences in precipitation in the 30N-30S band. Here POPEM reduces model bias, especially in the Southern Hemisphere and on the Tibetan Plateau." How do we know that the model bias is reduced?*

Reply: We have now explained this point in full in the section 3.2 and also made a new figure to clarify the point (Figure 9).

Figure 9A shows monthly precipitation for the area affected by the double ITCZ bias in the Southern Hemisphere (20S-0, 80E-100W). It is clear from this figure that POPEM yields more realistic representation of precipitation especially in the driest months (June-October). Figures 9B and 9C show the annual cycle of rainfall over the Australia Top End region and over the Tibetan Plateau, respectively. In both instances there is a usual bias in the original CESM. We have noted that despite POPEM obtaining slightly better results, both CONTROL and POPEM still have difficulties to estimate the precipitation of the rainiest months.

The paragraph now reads:

Another important benefit of POPEM is the reduction of the double ITCZ bias in the Southern Hemisphere. Although a small change can be inferred from Figure 7A-B, the improvement is buried in the annual mean precipitation maps. Figure 9A shows that the POPEM results are closer to observations of the intra-annual variability of precipitation, especially for the driest months (June-October).

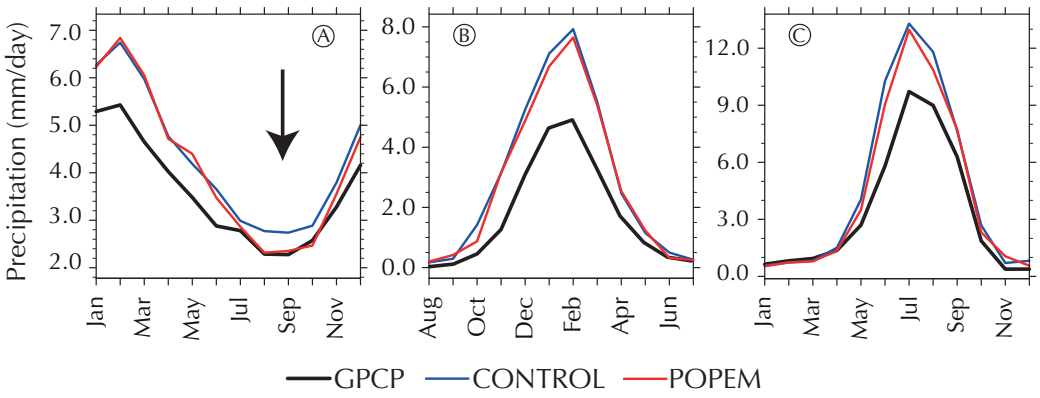

Figure 9: Monthly precipitation (1980-1999) based on GPCP, CTRL and POPEM for three of the regions with important biases in CESM. (A) shows precipitation for the area affected by the double-ITCZ bias in the Southern Hemisphere (20S-0, 80E-100W); (B) for Australia Top End (30S-10S, 128E-140E); and (C) for the Tibetan Plateau (22N-32N, 78W-92W). The black line represents observations (GPCP), the blue line is the CONTROL case, and the red line is the POPEM case. Units are in mm/day. The arrow indicates the improvement of the POPEM model.

The figure also shows slight improvements for another two typical biases seen in CESM, namely the excess precipitation in the Tibetan Plateau (Chen and Frauenfeld, 2014; Su et al., 2013; Figure 9C) and the bias in some areas affected by the Asian-Australian monsoon (AAM), such as the Australia Top End (Meehl and Arblaster, 1998; Meehl et al. 2012; Figure 9B).

Referee #3: *P. 8, line 9-10: "On the other hand, POPEM departs from the control simulation in the Asia-Pacific region between 10N-10S." Is that good or bad? How do we know?*

Reply: If we zoom-in on figure 6A (map: CONTROL minus POPEM) it can be seen that POPEM produces more precipitation than CONTROL. That means that the model reinforces the double ITCZ bias in this area, which is not good. We have noted that in the paper.

The text reads now:

On the other hand, POPEM departs from the control simulation in the Asia-Pacific region between 10N-10S. This result reinforces the double ITCZ bias in this area.

Referee #3: *P. 8, line 31 – "(Q1 and Q3 remain between ± 0.4 mm/day)." Please, define Q1 and Q3.*

Reply: Q1 and Q3 mean Quartile 1 and Quartile 3. We now write down the word in full to avoid possible confusion.

The line now reads:

(The first and the third quartiles of the distribution remain between ±0.4 mm/day)

References

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