Earth System
Dynamics

Discussions

EGU

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Interactive comment on "Differential climate impacts for policy-relevant limits to global warming: the case of 1.5 C and 2 C" *by* C.-F. Schleussner et al.

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Reviewer 1

We thank the reviewer for the in-depth review of our manuscript and the very detailed comments that substantially helped to improve our manuscript.

General Comment 1

One general comment is that I think there needs to be a bit more detail about the temperature limits. As much of the paper is phrased, 2°C (or 1.5°C) is seen as the upper limit of global mean temperature rise. However, those numbers are fundamentally heuristic, not hard limits. It could be that 1.9°C is already dangerous, and 2°C is even more dangerous (the authors find that something along these lines is indeed the case). I would appreciate it if the authors would go through their arguments (particularly the introduction and conclusions) and ensure that their presentations of the global mean temperature limits of 1.5°C and 2°C are presented appropriately, as useful heuristics instead of hard limits.

Response

We thank the Reviewer for her comment and we fully agree that 1.5°C and 2°C should not be characterized as " scientifically determined" thresholds of dangerous anthropogenic interference with the climate system, but rather "focal points" determined by policy makers based on value judgments and world views. We modified our manuscript accordingly to take full account of this remark.

General Comment 2

Another general comment refers to Section 5. It would be useful to see some context. For example, what does a 6% reduction in local yield mean? Is this catastrophic for nobody, a farmer, a region, a nation, etc.? Can it be compensated for? It's hard to say " that's really bad" or " that's not so bad " (or somewhere in between) if only the result is reported.

Response

We very much agree with the reviewer that only a placing these results in context can truly inform about the importance of these changes. This has been highlighted also in the recent IPCC AR5 that clearly distinguishes between climate hazards, vulnerability and exposure that together constitute the severity of the climate impact. We currently do not account for the latter (e.g. this would become possible once the the recent shared socio-economic pathways are available in their full extent), and hence cannot provide a thorough assessment of what these projections actually "mean". Clearly, we could use present day "climate analogues" for this purpose, but such analogues have to be chosen very carefully without being misleading. On P. 2474, 23ff we qualitatively discuss our findings in the light of countries vulnerabilities also specifically with regard to yield changes P. 2474, 23-29:

> The risks posed by extreme heat and potential crop yield reductions in tropical regions in Africa and South East Asia under a 2 ° warming are particularly critical given the projected trends in population growth and urbanization in these regions (O'Neill et al., 2013). In conjunction with other development challenges, the impacts of climate change represent a fundamental challenge for regional food security (Lobell and Tebaldi, 2014) and may trigger new poverty traps for several countries or populations within countries (Olsson et al., 2014).

From our perspective, a more quantitative assessment of what certain projections imply is beyond the scope of what can be provided in this analysis and would require a separate assessment directly involving trajectories and vulnerabilities.

<center>C1236</center>

Specific Comment 1

Page 2452, first paragraph: I understand why the authors chose two different reference periods, but it makes the presentation a bit confusing and raises some questions. How much do the deviations from past climate affect your results? Could you provide some quantitative evidence that indeed it's not a good idea to make all of your comparisons relative to preindustrial?

Response

We understand the reviewer's concern about the apparent use of two different base periods, although we would like to highlight that in fact we do not. We derive all model projections (including GMT increase) from the 1986-2005 reference period, but since the policy targets are derived with respect to pre-industrial warming levels, the impacts analysed for $0.9°C$ and $1.4°C$ GMT increase above 1986-2005 are expressed in their absolute warming above pre-industrial ($1.5°C$ and $2°C$, respectively). We agree with the reviewer, that the current manuscript is not sufficiently clear in this regard and modified P. 2452, 10 accordingly to:

> All our results are given with respect to this common reference period, although for consistency with the respective policy targets we express the GMT differences of $0.9°C$ and $1.4°C$ by the implied pre-industrial warming of $1.5°C$ and $2°C$.

Specific Comment 2

Page 2452, line 27 to Page 2453, line 10: I'm a bit dissatisfied with this paragraph, in that I don't think there is any reason one would have confidence in individual grid

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box results in the first place. In addition to natural variability, there could be numerical errors on such small spatial scales. I take it as a foregone conclusion that aggregation or some other kind of filtering is necessary to obtain robustness.

Response

We agree with the reviewer's comment and modified the respective paragraph accordingly to:

> In addition to the anthropogenic forcing, natural variability is a dominant driver of the climate signal on multi-annual time scales for time-averaged quantities such as mean temperature and precipitation change (Knutti and Sedlácek, 2012; Marotzke and Forster, 2014) and in particular for extreme events (Kendon et al., 2008; Tebaldi et al., 2011). This finding has been further consolidated by experiments with perturbed-initial condition ensemble simulations (Fischer et al., 2013). Thus, natural variability may mask an already present climate change signal and consequently lead to a delayed detection of the imprints of climate change (Tebaldi and Friedlingstein, 2013). To overcome this limitation, Fischer et al. (2013) proposed a spatial aggregation approach that allows for a robust detection of an anthropogenic footprint in climatic extremes despite natural variability – an approach that has also been successfully applied to the observational record (Fischer and Knutti, 2014). Here, we adopt and extend this spatial-aggregation approach.

Specific Comment 3

Page 2453, line 20: Did you check the robustness for more stringent significance levels? It could be that you get similar results for (say) 99% significance, which reduces

the chances of obtaining false positives or negatives in your test.

Response

Clearly, higher significance levels would increase the test's performance in reducing Type 1 errors (*false positives*). At the same time, this increased rate, however, comes at an increased rate of Type 2 errors (*false negatives*). Therefore, a trade off between the two error levels has to be considered when determining the significance level. In our case, we do not focus on singular model output, but rather an ensemble result when speaking about the robustness of our findings (e.g. more than 66 % of the models reject the null hypothesis of the KS test at the 95 % significance level). Given the minimum number of 11 models, this translates to 7 models and the probability of all the individual KS-tests being false positives is negligible. Therefore, we think that by choosing a 95% significance threshold, our overall test-scheme is already very robust and an increased significance level on the individual model basis will only lead to less discriminatory power of the test.

Specific Comment 4

Page 2455, lines 12-13: Why did you only choose 11 and 14 models, respectively? Why did you choose the models that you did? Are the models that you chose significantly different from each other? A bit of transparency would be helpful.

Response

We agree with the reviewer's suggestion that the choice of the model ensemble should be fully transparent to the reader. In our case, there is really not much to it. The choice of the model ensemble was based on data availability and we decided to always

include the maximum number of models available for each respective analysis. We did not have access to the required combination of RCP8.5 and historical runs for the respective variables for more than 11 models for extreme temperature and more than 14 for precipitation related changes. We modified the respective paragraph to clarify this point.

Specific Comment 4

Page 2457, lines 9-10: I know these are cited, but I would say that the point itself is arguable. I would like to see something less strongly phrased.

Response

We understand the reviewer's reservations against the expression used and have revised it accordingly:

> It is the regional natural climate variability that arguably determines a "climate normal" to which human systems as well as ecosystems might be adapted to.

Specific Comment 5

Page 2459, line 14: It would be nice to have more description so that the reader doesn't have to read Schewe et al. (2013) to understand what you did.

Response

We thank the reviewer for this comment and updated the respective paragraph to be more explicit with regard to the input data used and the intercomparability to the CMIP5 results presented above:

> Projections are based on 11 global hydrological models (GHM) that participated in the ISI-MIP intercomparison project. These are forced with bias–corrected climate simulations from five CMIP5 GCMs (HadGEM2-ES, IPSL-CM5A-LR, MIROC-ESM-CHEM, GFDL-ESM2M, and NorESM1-M, see Hempel et al. (2013) for further details on the bias–correction methodology). Each of GCM-GHM combinations is treated as an individual ensemble member resulting in a N=55 ensemble as a basis for the KS tests described above.

Specific Comment 6

I don't think a separate Section 4.1 is necessary if you only have one subsection. Just put everything in Section 4.

Response

We deleted this subsection as suggested by the reviewer.

Specific Comment 7

Either in Section 4 or Section 5, it would probably be useful to talk about sea level rise and consequent saltwater intrusions. This will certainly exacerbate water availability

for coastal cities/regions.

Response

We thank the reviewer for this helpful suggestion and included the following paragraph:

> In addition to changes in fresh water availability as a consequence of changes in the hydrological cycle, saltwater intrusion resulting from rising sea-levels or extreme coastal flooding has to be considered (Werner et al., 2013). Although strongly dependent on local circumstances including regional water management and coastal protection, saltwater intrusion might present a substantial challenge, in particular for low-lying coastal areas and small island states (Cisneros et al. 2014).

Specific Comment 8

Page 2461, line 29 to Page 2462, line 3: Choosing to plot relative changes makes sense, but it might also be helpful to mask out regions with small absolute change, thus reducing this amplification problem.

Response

We thank the reviewer for this suggestion and would like to highlight that we already apply such a masking on the regional level for Alaska, East Canada and Northern Europe for different crop types to avoid the amplification problem the reviewer mentions. However, in particular the North Asian region is a major crop producer for all crop types except rice (compare Fig. S5, Northern Europe is relevant for wheat) that should not be masked out of our analysis. We are furthermore of the view that applying a masking

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on the individual grid cell level will not help to make our results more accessible, but quite to the contrary make our analysis less transparent. If we take North Asia again as an example: While the individual grid cell productivity might be comparably low in this region, it will however in total amount to a change relevant on the global level that should not be neglected. Therefore, we refrained from applying filters on the grid cell level beyond the regional filters we have already in place.

Specific Comment 9

Section 5.2.4: I don't understand why there isn't any difference between the two different warming levels in the CO2 ensemble. Some insight would be useful.

Response

We are not sure, if we understand the reviewer's comment correctly, but we assume that she refers to the statement 2465, L9:

> While differences between warming levels are apparent for some regions and the CO2-ensemble, these display comparably low confidence levels.

What we actually refer to here is the minor difference between the percentage change assessed under 1.5°C (6.8% [-16.6,24.5]) and 2°C (6.8% [-14.3,26.8]), in particular displaying the very same median. We are not equipped to look into greater detail on this, but as this is a unique phenomenon for the rice global median value and does not occur for any other crop type, we can only assume that this is indeed by chance. However, we cannot rule out that our applied kernel density function affects the overall shape of our fitted pdf that underlies this. However, what we find remarkably consistent over all crop types is that although the full-CO2 ensemble shows median gains

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for some crop types under 1.5°C, no further increase (or even a sign reversal) is projected between 1.5°C and 2°C, indicating that climatological factors are substantially increasing between 1.5°C and 2°C thereby overcoming the benefits of increased CO2-concentrations.

Specific Comment 10

Page 2466, lines 9-10: I don't think it's very helpful to specifically call out 2030. This comes across as predicting the future.

Response

Although we of course do not intend to predict 2030 warming levels, a warming of around 1.5°C is inevitable reached around 2030s under all RCPs and also scenarios implied by the Paris Agreement. Thereby, we think that our statement

> Given that a 1.5°C warming might be reached already around 2030, our findings underscore the risks of global crop yield reductions due to climate impacts outlined by Lobell and Tebaldi (2014)

is justified, in particular as we do not predict 2030 temperature levels.

Specific Comment 11

Page 2466, lines 25-26: Say more about how this is consistent with the assessment of climate sensitivity. Does it span the same range? Does it have the same mean? Are you talking about median warming?

Response

The energy-balance carbon-cycle climate model MAGICC6 (Meinshausen et al 2011a, 2011b) is constrained to historical forcing estimates, and observations of hemispheric temperatures and ocean heat uptake, while sampling the parameter space in a way such that the posterior distribution of Equilibrium Climate Sensitivity (ECS) reflects the ECS estimates from IPCC AR5 WG1. This model is well-established and documented in the literature (e.g. underlying the temperature estimates from emission pathways in the AR5 WG3 report), and we thus kept this explanation rather brief. However, we see the reviewers point and updated the respective paragraphs to:

> For both scenarios, temperature projections are derived with the reduced complexity carbon- cycle and climate model MAGICC (Meinshausen et al., 2011) in a probabilistic setup (Meinshausenet al., 2009), which has been calibrated to be in line with the uncertainty assessment of equilibrium climate sensitivity of the IPCC AR5 (Rogelj et al., 2012, 2014). Each probabilistic setup ensemble consists of 600 individual scenario runs.

Specific Comment 12

Section 6.1: How do your generated scenarios compare with the CMIP models? Do they replicate any other scenarios?

Response

As described in section one, the scenarios used here in this study are specifically designed for the purpose to study SLR and coral reef estimates for scenarios that exhibit a median warming of 1.5°C and 2°C. However, given uncertainties in the climate

response to anthropogenic perturbations, there's some uncertainty in the GMT projections connected to these scenarios, which propagates through the impact assessments (compare Fig. 13 and 14). Neither the CMIP3 nor the CMIP5 model ensemble (based on the SRES or the RCP framework) included scenarios directly targeted at such levels, whereas the RCP2.6 scenario exhibits a median warming of about 1.6°C. In section 6.2, some discussion of our results in the context of the RCP framework is given.

Specific Comment 13

Section 8: It would be helpful if you summarized the first few paragraphs in a table so that the reader can easily see the whole picture.

Response

We very much appreciate the reviewer's suggestion and have included an overview figure (Fig. 15), which highlights key findings of our study.

Specific Comment 14

Can " not unlikely" be a number?

Response

We have corrected the wording.

Specific Comment 15

Page 2474, lines 4-15: This paragraph feels a bit hand-wavy. Is it possible in Section 6 to assess the contribution to SLR of the collapse of the Greenland ice sheets in your two simulations?

Response

As our simulations only address sea-level rise over the 21st, we do not assess any non-linearities connected to ice-sheet disintegration that operate on much longer time scales. However, we agree that this paragraph is a bit repetitive as it is not directly related to the findings presented in that manuscript. Therefore, we shortened it considerably.

Specific Comment 16

Page 2475: Mentioning Paris might not be a good idea, as the results from Paris will be clear well before this paper is published.

Response

In the light of the Paris Agreement and the explicit reference to 1.5°C there, parts of the introduction and the discussion have been rewritten substantially.

Specific Comment 17

Figures 2, 3, 5, 6, 8-12: It's really hard to discern much useful information from these figures. They're very crowded, and the individual panels are small. I'm not quite sure how to improve these, but something really doesn't work here.

Response

We agree that these figures are crowded and might not be straight-forward to read an assess as CDFs are not widely used in such a context. However, we see some merit in them as they display a wealth of information related to the exposure of land-area to changes in climate and climate impact signals beyond what can easily be displayed in a table or in any kind of other map. In addition, they provide a common framework to address very different impacts and to visualize key differences between a 1.5°C and a 2°C warming on a global and regional basis. As the individual panels are small, they are provided in a high resolution so that assessing all the information on a regional basis is possible. In addition, we now added regional overview figures that display all relevant impact panels for the respective regions in the supplementary material. These fill a single page each and thus allow to assess the regionally relevant information much more directly than the overview figures in the main body of the manuscript.

Specific Comment 18

I don't understand the top row of Figure 13. If warming caps at 1.5°C, how can there be any results above this value?

Response

We hope that our additional explanation given above at Comment 11 does help to clarify this point. As we use probabilistic projections that reflect the IPCC AR5 WG1 climate sensitivity assessment with a 600-member ensemble based on emission scenarios that show a median (50th percentile) warming of 1.5°C and 2°C, half of the 600 ensemble members will thus exhibit a warming above 1.5°C or 2°C.

Specific Comment 19

Figure S5: I assume this is percent?

Response

Indeed. We thank the Reviewer for spotting that.

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Reviewer 2

We thank the reviewer for her positive perception of our manuscript and her detailed comments in particular regarding our methods section.

General Comment 1

The methods described in Section 2 are very similar to those used by the impacts community in pattern scaling, particularly in regards to the relationship between GMT and climate variables. This type of scaling was mentioned in section 6, but not explicitly. There is a wealth of information (and studies) that use pattern scaling to look at regional impacts through impact assessment models (IAMs). Tebaldi and Arblaster, 2014, give a thorough critique of such methods.

Response

We thank the Reviewer for that helpful comment and pointing us to that reference. Referencing to the broad literature on pattern scaling is so far missing from our manuscript and while our approach differs in many regards from pattern scaling approaches, there are also some key similarities that would be worth pointing out. Firstly, as in pattern scaling approaches we assume that most impacts scale with the magnitude of warming and that "changes in the climate and climate impact signals studied here are dominantly driven by changes in GMT ". Clearly, this is limited to continuously increasing warming signals as are pattern scaling approaches. Tebaldi and Arblaster discuss in greater detail the limitations of such an approach for stabilizing scenarios as oceanic processes and large-scale circulation changes continue long after temperatures stabilize. However, our time-slice approach differs from classical pattern scaling approaches as we don't assume a continuous scaling of impacts with temperature. This is in particular

appropriate as we're looking into climate extremes and the hydrological cycle as well as into climate impacts like water availability and crop yields depending on those. Quoting from Tebaldi and Arblaster:

> Pattern scaling is likely to be more limited for extreme events (Lustenberger et al. 2013), or in cases where certain feedbacks (e.g. the drying of the Mediterranean) lead to an amplification of some types of events...

Similar limitations of the pattern scaling approach have been discussed e.g. in Lopez et at. (2013) or Chadwick Good (2013). Our time-slice approach is not based on such an assumption of linear scaling and capable of including non-linear increases. And in our results we find clear evidence for such non-linear increases in extreme event indices and climate impacts e.g. for South Asian extreme precipitation or Mediterranean water availability. To outline similarity and differences between our time slice approach and pattern scaling we introduced the following paragraph:

> Traditional approaches that analyze impacts over a given time period for all models in a model ensemble and relate this to a median GMT increase across the model ensemble do not account for this ensemble-intrinsic spread of global warming levels and will consequently overestimate the ensemble uncertainty of the GMT-dependent indices studied. Such a time-slice centered approach has been shown to provide better accuracy then traditional pattern scaled approaches (Herger et al., 2015). Although relying on the debatable assumption of scenario-independence of the projected signals that does not fully hold in climate stabilization scenarios (Tebaldi and Arblaster, 2014), time-slicing avoids known short-comings of classical pattern scaling analysis. In particular, it allows to capture non-linearities in extreme event and precipitation related signals that relate to non-linear local feedbacks (Lopez et al., 2013) or large-scale circulation changes (Chadwick and Good, 2013; Hawkins et al.,2014).

Specific Comment 1

Introduction, page 2450, lines 15-20: The argument that global temperature scales with local impacts should be made clearer in the introduction. Reference should be made to Held and Soden, 2006. Briefly describing the thermodynamic relationship between temperature and the hydrological cycle would add value to the method section(s). This is briefly discussed on page 2452, lines 13-20, but the physical mechanism is not mentioned.

Response

We agree that outlining the relevance of the scaling of local impacts with GMT increase would be helpful in the introduction of our manuscript. Therefore, we added the following statement:

> The assessment of such differences would greatly profit from a regional and impact - centered approach that allows for a more differentiated picture than globally aggregated metrics (Seneviratne et al., 2016). In particular, changes in the hydrological cycle as a result of temperature increase will be regionally dependent (Held and Soden, 2006).

Specific Comment 2

Section 2, page 2452, lines3-7: How do the models used compare against observations? I understand that a pre- industrial baseline from observations is not possible, but I didn't think there was a clear surface temperature trend in the observations. Also, was the preindustrial scenario used or was this a period in the historical scenario? Is the pre-industrial period mentioned here the same as in section 6 (1850-1875)?

Response

Clearly, the reference to the reference pre-industrial period should be made which is 1850–1900 as in the IPCC AR5. We have added this reference accordingly. The warming between 1850-1900 and the reference period 1986–2005 was 0.6°C. By deriving all changes relative to this reference period (which translates to a 0.9°C and 1.4°C warming above 1986-2005) we correct from any possible deviations of the GCMs over the historical period.

Specific Comment 3

Section 2, page 2452, line 8: I am unclear as to what the " X " means in Table S1. The dates listed in Table S1 are the centered dates around which a 20-year running average GMT reaches a specific threshold? I am not sure this information is needed.

Response

One characteristic of the time-slice approach is that GCM-specific slices centered around certain warming targets (in our case 1.5°C and 2°C) are chosen. As these slices can differ considerably (up to nearly 20 years for 2°C) this information is given in the supplementary material. In addition, not all model data has been available for all assessments. The availability for the Temperature, Precipitation and ISIMIP analysis is indicate by an 'x' in Table S1, which we explain in the table caption.

Specific Comment 4

Section 2, page 2453, line 20-27: Because there is the assumption of stationarity, you could do a Priestly-Subba-Rao test of stationarity to support the null hypothesis.

Response

We are not fully clear what stationarity assumption the reviewer is referring to here. The underlying data is already time-averaged on a grid-cell basis and then aggregated regionally. Thereby, from our understanding no test for stationarity of our two KS distributions would be required here.

Specific Comment 5

Section 3, page 2454, line 16: The assumption is that climate variables and extremes have a relationship with GMT has been examined in many papers. The relationship of GMT and precipitation should be referenced with the Held and Soden, 2006, and/or Liu and Allen, 2013. Also, you could reference the Sillmann et al, 2013, paper to show that the models show good agreement with reanalysis for the ETCCDI variables.

Response

We fully agree with the reviewer that the relation between climate (extreme) variables and their change with increased radiative forcing is a subject of intense research. Our statement is thereby referencing the most recent IPCC AR5 WG1 report that from our perspective represents the most comprehensive review of the scientific literature on the matter. We also thank the reviewer for pointing out that a reference to the Sillmann et al. (2013) paper highlighting good model agreement with observational data is relevant here and we have included this reference accordingly.

Specific Comment 6

Section 3, page 2455: Why was a land mask applied for the ETCCDIs? I would have liked to see the results (i.e. maps) over the oceans as well.

Response

We agree with the reviewer that for model intercomparisons our analysis of changing patterns in large-scale circulations, analysis of oceanic signals is of great relevance. However, in the approach we pursue here, we focus on a regional analysis of the SREX-regions of specific land-areas. Thereby, we applied a land mask to our analysis and also to the figures presented as this is the main purpose of our analysis.

Specific Comment 7

Section 3, page 2458, lines 10-14: As with the King et al, 2015, paper, regions of complex topography show little significance in changes in extreme precipitation. Aggregating to large regions is likely to mask significant changes in precipitation extremes.

Response

We take this as a general comment on the work presented here, as we cannot identify, to what specific statement in Section 3, page 2458, lines 10-14 this comment refers. In particular, we checked the King et al. 2015 reference for corresponding statements on that and were not able to identify the findings the reviewer is referring to here.

Specific Comment 8

Section 7, page 2471, line 9: The reference period (1980-2000) is different from reference period used in prior sections. Why?

Response

The methodology for the coral reef analysis is based on a paper by Frieler et al. (2012) that chose this reference period for their analysis. Thereby, several aspects might differ from the newly developed approaches for the CMIP5 and ISIMIP analysis presented above. This include the reference period, but also the AOGCM ensemble underlying this analysis, which in this case is CMIP3 as outlined in section 7.1

Specific Comment 9

Section 8, page 2475, lines 10-14: Will this sentence be revised due to the outcomes of the Paris 2015 meeting?

Response

Clearly, this section is outdated now after the Paris Agreement and has been fully rewritten together with parts of the introduction to fully reference the Paris Agreement and the long-term global temperature goals of 1.5°C and 2°C included therein.

Specific Comment 10

Figure 2: Is this for TXx? It doesn't say this in the figure caption.

Response

We thank the reviewer for pointing this out and indeed, this figure displays TXx.