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Hydrology and Earth System Sciences Discussions

Interactive comment on "Contributions to uncertainty in projections of future drought under climate change scenarios" *by* I. H. Taylor et al.

I. H. Taylor et al.

pete.falloon@metoffice.gov.uk

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Response to reviewers' comments

Taylor et al, HESS-D

The authors would like to thank the reviewers for their constructive and helpful comments. We address each of these in a point-by-point fashion below, in **bold** font..

J. Sheffield (Referee)

justin@princeton.edu

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This study calculates the uncertainty in future projections of drought based on a perturbed parameter ensemble of the HadCM3C ESM. Uncertainty is calculated across three drought indices, five drought thresholds, 57 ensemble members and two emission scenarios. The uncertainties in significant changes in time spent in drought are greatest among the indices and least among the thresholds. Despite this, there are robust signals regionally that are driven by robust signals of precipitation changes,

whereas robust signals that are associated with increasing temperature depend on the index examined.

Overall, this is a well-written paper with some interesting results and good insights.

There are many points of clarification that need to be addressed and some further

discussion required on the interpretation of the results. These are detailed below.

One issue is the conclusion that the drought index contributes the largest uncertainty.

I would think that this is expected, as different indices reflect different types of drought and different parts of the hydrological cycle. For example, if you quantify the uncertainty in future climate by looking at an index of precipitation versus an index of temperature they will show quite different changes and you will conclude that there is large uncertainty in future climate projections. Similarly for drought, if you look at the SPI (dependent on precipitation only) versus the PDSI (mainly dependent on temperature according to the results here) you will get very different answers (because they represent different things) and therefore large uncertainty. So, if one of the goals of the study is to help inform drought management and planning, then the message should be to choose the correct index for the impact rather than taking into account the uncertainties across a range of indices, which assumes that they are all plausible for the particular management problem. The large number of available drought indices reflects this, because there is a large range of impacts that are of interest. You state this on page 12616, line 1-3. Another way of thinking about this is by asking ourselves whether it really matters whether we use a set of different indices or not, as the choice of index should be dictated by the impact we are interested in and is therefore under our control.

The real uncertainties arise from things that are out of our control and not known – i.e.

Uncertainty in future emissions scenarios; uncertainty in model physics; uncertainty in model structure or natural variability (not included here). Therefore it may be more informative to look at the uncertainties derived from different indices of the same drought type and based on the same variable(s).

Thank you for these constructive comments.

We agree with the comments in the two paragraphs above. To some extent, this also depends on whether the indices are viewed as separate models of drought, then this would be another source of uncertainty (for instance, Haddeland et al. 2011 present water model simulations from different types of water model, concluding that assessments need to incorporate impact modelling uncertainty). In the present study however, clearly the indices are aiming to do quite different things. In light of this we will:

1. Revise the introduction and conclusions along these lines.

2. Assess whether the different drought metrics are influenced to a greater or lesser extent by the uncertainties or whether they are all influenced in a similar fashion.

The revised paper will also include SRI and SPI on different timescales. As per the comments of JP Vidal, we will replace the discussion of uncertainties with discussion of mitigation aspects, and also highlight mitigation implications in the abstract and conclusions.

Specific comments

Page 2614, line 13: Why the A1B and RCP2.6? It would have been better to have

scenarios from the same family. I see you discuss this later.

This is what was available from the modelling setup and is discussed later in the paper. We can add clarification on this in the text.

Page 2614, line 16: “representing the 2080s” – I’m not sure what this means here?

Simply that the 2080s was that period. We will modify the text to make this clearer (e.g. remove “representing the”).

Page 2614, line 17: statistically significant?

We will modify the text to add statistically significant.

Page 12616, line 11. Why choose these three indices? Do they reflect the three types of drought type (met, agric, hydro)? See later comment.

These could be readily applied to direct climate model outputs and had been applied in former studies, so could be compared with them. We will modify the text accordingly.

Page 12616, line 13-17. Is this paragraph needed? It seems a little out of place and

doesn’t add much.

OK, we can remove this paragraph.

Page 12616, line 20-24. This has been revisited since the 4AR (see IPCC SREX, 2012; Sheffield et al., 2012).

OK, we will update this section to add those references.

Page 12616, line 24-25. Why? Because the observed changes are related to anthropogenic climate change?

Because of the already increasing drought trends – we will modify the text accordingly.

Page 12616, line 27 “Temperature is projected to increase everywhere” and page 12617, line 3 “Evaporative demand is also likely to increase everywhere”. These are hanging sentences that need some context, such as a few words that link them to changes in drought.

OK, we will revise this section to highlight that T/ET are strong drivers of drought and increase everywhere.

Page 12617, line 7. So, do you account for these?

No, and we will clarify this in the text.

Page 12617. I think you need a statement somewhere around line 9 giving the overall objective of the paper, in the context of the previous discussion. And also mention what is the difference between this and the previous paper by Burke and Brown (2008) or how it builds on that study?

OK, we will add this.

Page 12617, lines 19-22. But do you discuss the importance of these in relation to your results? For example, the uncertainty due to different models (structural uncertainty) and due to internal variability, which may be comparable to the other types of uncertainties? Okay – I see that you do this later in the discussion.

No action needed.

Page 12617, lines 23-27. Again, some discussion is needed of why the impact assessment (i.e. choice of index) is an uncertainty.

If we view the indices as models of drought, then this would be another source of uncertainty (for instance, Haddeland et al. 2011 present water model simulations from different types of water model, concluding that assessments need to incorporate impact modelling uncertainty); but we agree with your assertion that

in this case the choice of drought index should reflect the application and will modify the text accordingly. The revised paper will also include SRI and SPI on different timescales.

Page 12617, line 28 to end of paragraph. Why choose the SMA and PDSI which both represent the same type of droughts? It may be better to use a hydrological drought index such as the SRI to cover all three types of drought.

These were chosen since they could be readily applied to climate model outputs available, and were comparable with our earlier studies. SMA and PDSI represent different kinds of drought, being soil moisture and precipitation based, respectively. We will also add SRI to the revised manuscript.

Page 12618, lines 20-23. Less uncertainty means a more robust projection, but if the model/index is wrong in the first place then less uncertainty places more confidence on an incorrect conclusion. What do you mean by “messages”?

OK, we will modify the text accordingly.

Page 12620, lines 8-11. The acronyms have already been defined.

OK, we will modify the text accordingly.

Page 12620, lines 19-20. Bit awkward. Perhaps rewrite as: “This represents the variation of the impacts of reduced precipitation with event duration (Sivakumar et al., 2010)”

OK, we will modify the text accordingly.

Page 12620, lines 21. How does this time scale relate to those from the other indices? If they are different, then you are introducing a difference in what they represent.

Approximately comparable, but highly regionally dependent. We will add this information for SMA and PDSI

Page 12621, lines 3-4. I know you define the time periods later, but it may be better to do that at the front of section 3.

OK, we will modify the text accordingly.

Page 12621, line 11. Soil moisture measurements are routinely collected in many regions – it is the lack of spatial coverage of routine measurements that is the greater problem. Perhaps better to say "not collected over large areas" rather than "not routinely collected".

OK, we will modify the text accordingly.

Page 12621, line 12-15. So does the model include these effects or not?

Yes the model does and we will modify the text accordingly.

Page 12621, line 16-17. Can you provide a brief description of how the SMA is calculated, because it is important for comparison with the other indices, such as the timestep it is calculated over and any scaling that is done. Similarly for the PDSI – are the monthly values used as is?

OK, we will add more detailed description of these.

Page 12622, line 18. "simplification of soil moisture across regions". I'm not sure what this means?

I presume this means lack of considering soil moisture heterogeneity (e.g. within grid cells), so we will verify this and alter the text accordingly.

Page 12622, lines 24-28. Perhaps rewrite as: "and potential evaporation. Potential evaporation is calculated with the Penman–Monteith equation (using temperature, relative humidity, pressure, wind and short and long wave radiation) following the methodology of Burke et al. (2006) as this is more suitable for application to climate change scenarios"

OK, we will modify the text accordingly.

Page 12624, line 4 onwards. I understand the need for the exemplar member, but its selection based on country-based ranks (which will bias it towards larger countries and regional specific biases) seems a little counterintuitive for a global study.

OK, we will alter the text to note this limitation. There are other impact studies being published using this exemplar system, so for comparability it needs to be kept.

Page 12625, line 10. Can you give a reason here why you exclude cold regions? Also, is there an argument for excluding arid regions also?

Yes, because the PDSI does not include frozen processes; thus we have excluded them also from all regions. We will modify the text to include this.

Page 12626, lines 3-4. This is not quite clear to me. I understand that you calculate the variance of each uncertainty source, but what do you mean by the "mean of each resultant variance"? You have a variance value for each uncertainty source at each grid cell, but what are you calculating the mean of? How do you calculate the variance of the sources of uncertainty, which only have 2 (emission scenarios) or 3 (drought indices)?

This is no longer relevant since we will replace this section with discussion of mitigation.

Page 12626, paragraph starting on line 23. Can you give any (brief) explanations of the differences from the two scenarios? In particular it would be good to have some insight into why you see large spread for the A1B than the RCP2.6 (for both temp and precip) – is this expected?

Yes, this is expected since the larger forcing (and hence climate changes) in A1B will lead to a larger range of responses. We will add this to the text.

Page 12628, line 21. Is this because the SPI is a 12-month index and so will tend to smooth out any seasonal changes? If you calculated the 1- or 3-month SPI, would you get more similar results to the other two indices?

SPI shows more of the globe with no significant change. This issue is probably because SPI is soley based on changes in precipitation and not changes in other components, so as a result we would probably not get more comparable results to the other two indices by changing the SPI timescale. Information will be added to the manuscript on SPI on different timescales.

Page 12628, line 9 onwards. Are the differences between the SMA and PDSI in higher latitudes related to snow? Presumably the Hadley model has a representation of snow/ice that influences the soil moisture, whereas the PDSI has no such representation.

We will assess changes in snow cover, and comment appropriately. This is most likely due to temperature amplification in high latitudes, driving the difference between SPI (which does not include temperature) and SMA/PDSI, which do.

Page 12628, line 24-25. Presumably this is also the case for the other two indices?

Yes, this is the case also, we will edit the text accordingly.

Page 12628, line 26 onwards. For the PDSI, the time spent in drought is > 80% in many regions. This seems very high.

The probable reason for this is that the ESE tends to have greater warming compared to the studies of Burke et al. 2006 and Burke and Brown 2008 – we will modify the text to say this.

Page 12630, line 7 onwards. You start talking about the 2050s now, without any mention of this in the introduction. Perhaps a sentence in section 3 would help.

OK, we will add this at the beginning of that section.

Page 12632, line 20-24. Do you know why the PDSI is so strongly influenced by temperature, given that it is based on the whole water balance? Temperature should only come into play in the PM formulation for PE, which implies that trends in net radiation and VPD are well correlated with temperature, and that the PDSI is highly influenced by PE, relative to precip and runoff.

We will add a comment and references on this aspect since it has been mentioned before in the literature (e.g. Sheffield & Wood 2011; van der Schrier et al 2011).

Page 12634, lines 19-22. It is interesting that even under the mitigation scenario, there are still large increases in drought in many regions.

OK, we will add a note on this.

Page 12635, line 9-10. How do you know that temperature changes have a larger influence on the PDSI than precipitation? Can you quantify "mainly"?

As above, we will add a comment and references on this aspect since it has been mentioned before in the literature (e.g. Sheffield & Wood 2011; van der Schrier et al 2011).

Page 12635, lines 11-15. This is an interesting point, that different scenarios give different signs of change. If you are basing this on the changes from the exemplar member (rather than the mean across members) is there any influence here of decadal (natural) variability coming into play that gives an increase in one scenario and a decrease in the other for a particular time period, even though the overall trend may be increasing (or decreasing) in both?

The discussion is based on the exemplar; we will add a comment on this.

Page 12635, line 26 onwards. This paragraph repeats a lot of the results section,

rather than discussing the results and providing insights, perhaps by referring to figure 1 (or the actual distribution changes for a representative grid cell). For example, can you explain why the decreases in time spent in drought are sensitive to the threshold whereas increases are not?

OK, we will edit the text accordingly.

Page 12636, line 20. Do you have a sense of whether the model structural uncertainty (i.e. CMIP5 ensemble) would have similar uncertainty to the ESE?

OK, we will add information on this (from the Lambert et al. paper).

Page 12638, line 14-16. This is just repeated from the previous section. Suggest

deleting it.

OK

Page 12638, lines 20-23. This is also essentially a repetition from the previous section. Suggest deleting it.

OK

Page 12638, lines 24 onwards. Again, this repeats much from the previous section.

Why not merge everything on page 12637 with the conclusions? Figure and table captions. Suggest replacing “HADCM3C Earth System Ensemble” with “ESE” as you have this defined in the text. No sense in repeating the full name for every caption.

OK, we will edit the labels accordingly.

Table 1. Does “land points” exclude the cold regions? Suggest including units in the

table itself, e.g. “Land temperature change (oC)”

We will clarify the land points issue in the table title and add units in the table as suggested.

Figure 3. “avereaged”

OK, we will correct the text accordingly.

Figure 4 and 5 and others. “exluded”

OK, we will correct the text accordingly.

Figure 7. Can you increase the label font size?

OK, we will increase the font size.

Figure 8. No need to explain the color scheme in the caption as you have a legend.

OK, we will remove this part of the caption.

J.-P. Vidal (Referee)

jean-philippe.vidal@irstea.fr

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General comments

This is a generally well-written paper on a relevant topic, based on innovative Earth

system modelling simulations. I have however three general comments detailed below that need to be addressed.

Thank you for these constructive comments.

Physical and socio-economic droughts

There is a recurrent ambiguity/confusion between physical droughts – some of which are actually studied in this paper – and socio-economic droughts, mainly in the introduction (P12616, L11-12, P12616, L24-25, P12617, L3-6) and discussion (P12633, L7, P12636 L4-6, P12637, L13-14). See all specific comments below. This distinction is crucial, as physical droughts are hazards, and socio-economic droughts are the results of hazards and vulnerability, which is specific to each anthropogenic hydrosystem (be it a hydropower plant or a crop field). As clearly defined by Wilhite and Glantz (1985), socio-economic droughts "incorporate features of meteorological, agricultural and hydrological droughts". It should therefore be made clear that this study is only about (some types of) physical droughts. Additionally, any conclusion about socioeconomic droughts cannot really be reached at the global scale unless anthropogenic hydrosystems are explicitly modelled into the Earth System.

We agree and will modify the text throughout following your suggestion.

Definition of uncertainties

Although you appropriately distinguish between actual uncertainties (ensemble members and emissions/concentration scenario) and choices (drought index and drought threshold) P12618 L9-14, you consider in this study the above choices as uncertainties. This is closely linked to the issue raised by Justin Sheffield. First, different drought indices represent different things, and the choice of a specific index is dictated by its vulnerability to a specific type of drought. For example, a high-altitude hydropower reservoir or a rainfed rice crop is mainly dependent on meteorological drought, a wheat crop on agricultural (edaphic) drought (which combines effects of precipitation and evaporation), and a run-of-river hydropower plant or a drinking water scheme on hydrological drought. Then, the choice of a drought threshold also depends on the hydrosystem studied, and more specifically on its degree of vulnerability against water deficits. For example, the cost effectiveness of a hydropower scheme may be positive when streamflow is above a x Consequently, I would strongly recommend not to include drought index and drought threshold as actual uncertainties. This has no implication on the calculations performed in the paper except for the analysis of contribution from

the uncertainty sources (P12625 L25 onwards and P12631 L7 onwards), which should be removed from the manuscript as they are not relevant. This has however large implications on the way to present the objectives of the paper and the results obtained. For example, the current title (and abstract) would no longer holds when drought index and drought threshold are not considered as uncertainties. I would suggest to present this paper in line with something like "What are the impacts of mitigation on future droughts?". The objective of such a paper would then be to compare drought changes between A1B and RCP2.6, across a range of drought indices and drought thresholds. This would be in my opinion much more policy relevant.

As mentioned in the response to Justin Sheffield, we agree that while if we view the indices as models of drought, then this would be another source of uncertainty, in this case the different drought indices may not be usefully compared in this way. We will replace the discussion of drought index uncertainties with discussion of mitigation, and will also note in the paper that the choice of index should reflect the application. We will also add text on mitigation aspects to the abstract and conclusions.

Choice of drought indices

You chose here to compute three different indices, one meteorological drought index (the standard WMO-adopted SPI) and two agricultural drought indices. I could not clearly see what the objective is here behind this choice (except only by reusing indices computed by Burke and Brown, 2008). A possible objective could be to compare different agricultural drought indices, like SMA and PDSI. Another possible (and much more attractive) objective could be to characterize droughts along the land surface hydrological cycle, by using indices of all three types of physical droughts. One potentially highly interesting addition to this study would be to compute a runoff-based index in order to see how changes in climate variables impact propagate along the land surface hydrological cycle (see Vidal et al., 2010). If runoff data is available from the ESE, I would strongly suggest to compute a Standardised Runoff Index, as suggested also by Justin Sheffield. Moreover, you consider only the 12-month time scale in this study (at least for SPI and presumably SMA), although the response of drought to climate changes has been proved to be highly sensitive to the time scale considered (see e.g. Vidal et al., 2012). If feasible (it would not take that much time now the calculation procedures are implemented),

I would recommend to compute SPI and SMA at other time scales (for example 3 months, in order to assess the differences between short and long droughts). If not, I would strongly recommend to clearly acknowledge this limitation of this study.

The indices were chosen for compatibility and comparison with earlier global studies (e.g. Burke et al 2006; Burke & Brown 2008). We can note this in the text.

As per the comments from Justin Sheffield, we will add the SRI (and SPI on different timescales) to the revised manuscript.

Specific comments

P12616, L11-12: This sentence is a bit ambiguous, given that (1) you consider only physical droughts in your study and (2) the analysis is restricted to indices of meteorological and agricultural droughts.

OK, we will edit the text accordingly.

P12616, L20-23: Please give a more precise reference than the IPCC Summary for Policymakers.

OK, we will edit the text accordingly.

P12616, L24-25: "This suggests a particular vulnerability in these regions to any projected increases in drought occurrence". You're writing here about vulnerability, so to socio-economic droughts, whereas the previous and following statements about the occurrence of intense and multi-annual droughts, and the extent of drought-affected

areas, refer presumably only to physical droughts. Please make it clear, by also giving some more precise references than an IPCC chapter.

OK, we will edit the text accordingly.

P12617, L3-6: "Drought is not solely affected by climatic drivers, non-climatic drivers such as population changes, land use and water management have a large influence on water availability and hence drought (Kundzewicz et al., 2007)" Once again, you refer here to socio-economic droughts, whereas surrounding general statements are about physical droughts.

OK, we will edit the text accordingly.

P12617, L14-19: This not all entirely true. Indeed, as shown by Hawkins and Sutton (2009, 2011), these results depend hugely on two things: (1) the variable considered, temperature or precipitation, and you should therefore refer here also to the 2011 paper on precipitation (I see you mention it later in the discussion), and (2) the region considered, and more precisely its size and location on the earth. For example, internal variability is not the predominant source of uncertainty when looking at the global mean surface temperature (Hawkins and Sutton, 2009). Conversely, emissions scenarios account for virtually nothing even in the second part of the 21st century when looking at the European mean precipitation (Hawkins and Sutton, 2011). All these differences should be made explicit in this paragraph, and conclusions should support the choice of the specific uncertainties considered in this study.

OK, we will edit the text accordingly.

P12617, L28 onwards: You consider one meteorological drought index and two agricultural drought indices. Please justify it (see my general comment 3).

As per earlier comments, we will edit the text accordingly and justify why (see

comment above), plus adding the SRI, and SPI on different timescales.

P12618, L9-14: You appropriately mention here that any drought assessment depend on (1) uncertainties and (2) choices. Drought index and drought threshold are choices indeed, but they cannot be considered as uncertainties. See my general comment 2.

OK, no action needed

P12619, L14 onwards: I don't understand why you consider two scenarios from different families, given that ESE simulations have presumably been done also with other RCPs. Could you justify it?

These were the data available from the ESE at the time of writing

P12619, L23 onwards: You describe here the ESE as incorporating many parameter uncertainties (with interactions). Why not studying the impact of these uncertainties on drought results? I see in the Lambert et al. (2012) paper that you have metaparameters (carbon cycle and atmosphere). You could for example perform an analysis of variance (maybe after a variance reconstruction, see e.g. Vidal et al., 2008) with these effects, similarly to what you have done in Sect. 4.3. This way, you could assess the relative importance of carbon and atmosphere parameters on drought changes. I do not suggest to do it necessarily for the present study, but I see it as a nice follow-up.

OK, we will add this to the discussion.

P12620, L15: The appropriate reference for the adoption of SPI by WMO is Hayes et al. (2011)

OK, we will edit the text accordingly.

P12620, L21: Please justify the use of SPI12 (12 month accumulation).

OK, we will edit the text accordingly and add SPI on different timescales

P12621, L3-4: unclear. Please also give some details about your SPI calculation method (parametric or not parametric fitting, extrapolation, etc.)

OK, we will edit the text accordingly

P12621, L16 onwards: Please give some more details about the calculation of the SMA. More specifically, what is the time scale used? 12 months as in Burke and Brown (2008)?. Is a monthly climatology used?

OK, we will edit the text accordingly

P12622, L26-28: I agree on using Penman-Monteith instead of Thornwaite. But please give some more details about the calculation of potential evaporation, as this variable is hugely sensitive to formulation choices (time step, etc.) and to the quality of input variables (see e.g. Kay et al., 2008).

OK, we will edit the text accordingly

P12622, L27-28: "this is more suitable for application to climate change scenarios". Could you develop further this statement? (see previous comment)

OK, we will edit the text accordingly

P12624 onwards: The idea of using an exemplar member is quite interesting. However, basing its selection on an average of responses over different countries make it simply non informative at the regional scale where drought assessments are useful (and potentially used). This limitation should definitely be emphasized in the text. Moreover, in order for you not to redo this selection, I would strongly recommend to show the distributions of responses for each variable and country in order to see how the exemplar member behaves regionally compared to the overall ensemble.

OK, we will alter the text to note this limitation. There are other impact studies being published using this exemplar system, so for comparability it needs to be

kept.

P12626, L1-4: These are not all actual uncertainties. See my previous comments.

OK, see our response to those comments. We will edit the text accordingly.

P12624, L22 onwards: It should be emphasized here that the time spent in drought (that you choose to study here) is the most basic drought characteristics. However, the impact of a drought is hugely dependent on its spatio-temporal characteristics, i.e. how it develops in time and space and its seasonality (see Vidal et al., 2010).

OK, we will edit the text accordingly

Fig.3: It would be nice to be able to identify the exemplar member in the final 30-yr average values at the right of each plot.

OK, we will edit the plots following your suggestion.

P12627, L15 onwards: You should specify here (and recall it in the captions) the chosen threshold (10)

OK, we will edit the plots and text following your suggestion.

P12631 L7 onwards: This analysis is in my opinion not relevant. See my general comment.

This section will be removed.

P12632 L5-8: Presented like this, it is obvious, isn't it?

We will edit the text here. The intention was to simply highlight the key drivers/sensitivities of the models in terms of driving variables.

P12632, L12-14: It is by definition strictly equal (apart from SPI fitting errors) to consider SPI or precipitation percentile thresholds.

OK, we will edit the text accordingly to note this.

P12633, L7 onwards: Any particular drought impact assessment will make use of a specific index and threshold. So the recommendation would be here to adequately choose them to represent the vulnerability of the studied hydrosystem.

OK, we will edit the text accordingly

P12633 L25: Please give some references for the MOSES2 land surface scheme

OK, we will add references.

P12635 L15: "It would be useful to understand the climatic processes leading to these projected changes to understand more fully how mitigation actions may influence drought occurrence regionally". Well, yes! Any physical insight would be welcome here.

OK, we will add any insight from related published studies to this section (e.g. Lambert et al. 2012 cited in our discussion paper).

P12636 L4-6: Once again, the choice of drought threshold should be conditioned by the specific hydrosystem under study. Moreover, there are inevitably "threshold effects" when considering socio-economic droughts that are related to thresholds in physical droughts.

OK, we will edit the text accordingly

Sec. 5.5: This section is in my opinion not relevant, as the analysis mixes actual uncertainties and choices.

OK – this section will be replaced with a discussion of mitigation, as per the earlier comments.

P12637, L13-14: "This has important implications for drought management planning in the future." Well, not necessarily, it really depends on which areas are affected. Non vulnerable areas (inhabitated areas for example, but not only) may have large increases in drought without any socio-economic effects. Conversely, an increase in drought in areas with important crop production would have dramatic consequences.

OK, we will edit the text accordingly

P12637, L23-24: already written above

OK, we will remove this.

P12637, L26-29: unclear

OK, we will edit this statement to make the intention clearer.

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