Anonymous Referee #3
Received and published: 6 May 2014

This paper analyzes the hydrological drought using tree CMIP3 GCMs that provided driving data for the EU WATCH FP6 GHMs. The manuscript is well written and methods are technically sound. However the data used for the analysis are somewhat obsolete.

The manuscript can be considered for publication after a major revision. The major comments follow:

Major Comments
1) The analysis is based on just the three GCM projections from CMIP3 which needs to be extended to more GCMs (~20) from CMIP5 projections to evaluate uncertainty coming from CMIP5 projections. The statistical bias correction methodology that was used to correct the 3 WATCH GCMs for biases can be redone using WFDEI (see next comment).
We kindly thank the reviewer for the suggestion to use more GCMs from the CMIP5 projections. However, at the time we started this research (January 2013, submitted September 2013) the mini-ensemble from the CMIP3 seems to be a good choice to use for our research. The advantages of this mini-ensemble is that the bias correction was performed by experts in the field both for the control period (Piani et al., 2010a; 2010b, Haerter et al., 2011) using the the WFD dataset (Weedon et al., 2011) to correct the models and for the future (Hagemann et al., 2011; Chen et al., 2011). This resulted in CMIP3 simulations from 1963-2100. The period 1963-1970 could be used to initialize the hydrological model and make sure that the groundwater and discharge simulations where no longer influenced by the initial conditions. The CMIP5 simulations corrected by WFDEI only cover the period from 1979 onward, which would result in discharge and groundwater simulations that are influenced by the initial conditions. Since in this study we focus on hydrological drought this could significantly impact the obtained results. Additionally, we are focused on the drought generating processes and how changes in future hydro-climate will impact the feedbacks between climatology and drought generating processes. Therefore a large ensemble is less essential than for example in a global impact study.
At the start of this research these were the main reasons to use a mini-ensemble of CMIP3 projections, that are bias corrected by well-known data with appropriate techniques. Furthermore, bias-corrected forcing at the 0.5^{o} scale (downscaled) for all major climates across the globe was not available at the start of our research. These became available, when the first results of the ISI-MIP were published (on line publication PNAS, December 2013), see item 2 under Other Comments.

2) The WFDEI observational dataset that is base to ERA interim reanalysis project should replace the WFD dataset.
The downscaled forcing of the three WATCH GCMs for the 21st century is bias-corrected using WFD. Although WFDEI is a good alternative for WFD for upcoming research on hydrological extremes (drought and floods), at the start of our research downscaled, bias-corrected GCM forcing using WFDEI was not available. This became available not earlier than the publication of the first ISI-MIP results (see item 2, above). The forcing for the future (GCMs) should be consistent with the control period.

3) GCMs used in the analysis were interpolated to 50 km to match the spatial resolution the WATCH forcing data. Simple interpolation may result in inconsistencies in daily precipitation statistics (like number of rainy days), therefore, statistical downscaling should be performed.
The downscaling was not performed by the authors and done by the partners involved in the EU-WATCH project. The downscaling is extensively reported by Weedon et al. (2010; 2011), following procedures , which are well accepted to downscale GCM data to the fine 0.5° resolution of the hydrological model simulations. These donwscaled and bias-corrected forcing data are widely used in recent literature, e.g. Harding et al. (2011); Haddeland et al. (2011); Prudhomme et al. (2011); Corzo-Perez et al. (2011); Stahl et al. (2012); Van Vliet et al. (2012); Gudmundsson et al. (2012); Van Huijgevoort et al. (2013); Van

Loon et al. (2014).

Other comments:
1) The presentation of the results is lean. The global results can be provided in maps. Moreover there is no map to show the spatial distribution of the “major climates”.
The authors agree that more results should be shown. A map will be included showing the changes in hydrological drought in the future. Additionally, a map with the climate zones as derived from the WFD will be included. The authors will explore other ways to better graphically display some of the results, to give the reader more information on the results.

2) Finally I would like to bring to authors’ attention the ISI-MIP community modelling effort from where you can find CMIP5 driven GHM runs (http://www.isi-mip.org/).
The authors were aware of this community, but had no access to the data until the end of 2013 (later than submission of this manuscript. The ISI-MIP community modelling effort is highly appreciated and should be a way forward to global hydrological modelling with a special focus on the uncertainties in future projections.

Anonymous Referee #4
Received and published: 20 May 2014
Summary
Authors evaluate changes in future droughts on a global scale using a mini-ensemble of three GCMs. WATCH forcing data and GCM outcomes are used to force a conceptual hydrological model. Impacts on drought characteristics are reported for five types of climate. Authors conclude that drought events will be less frequent but more intense.

General

The manuscript reads quite well with only a few minor errors. I am missing a figure showing the locations used for characterising the analysed climates. Also a map showing the relative changes on the globe would make the message of the article stronger.
We kindly thank the reviewer for the suggestions and will provide these maps. Additionally, we will see if more results could be displayed with maps or other graphs (see also reply to reviewer#3, item 1 under Other Comments).

Major comments
Seasonality effect on hydrological droughts: During winter, snow accumulation limits river flows thus triggering low-flow conditions, during summer we could expect low flows due to lack of rainfall. On a global scale covering different hydro-climatologic systems, where both these processes are observed, I would expect an analysis covering this seasonality. Authors should provide some analysis and discussion on this issue.
In our study we investigated effects of climate change (temperature, precipitation and evapotranspiratrion) on hydrological drought. The interplay is complex and outcome indeed depends on hydro-climatic systems. For instance, due to climate changes lack of rainfall is not the only trigger to generate summer droughts in snow dominated regions. As a results of the projected temperature rise the snow will melt earlier in the season leading to decreased water availability in the summer months even without a precipitation change . The authors will included more information on the seasonality in the manuscript and link it to the drought typologies proposed by Van Loon and Van Lanen (2012) and the seasonality effects on drought (Van Loon et al., 2014)..

Impacts must be analysed in the context of the outcomes. For example, larger and uncertain drought changes are projected for arid and polar climates, however, how relevant are those in terms of potential impacts? This was not discussed by the authors. In addition, authors must discuss the strength of these changes by reporting statistical significance.
The drought as a natural hazard occurs in all climates, incl. the very cold climates (E-climate) and very hot climates (B-climate), but the authors agree that the impact in polar regions will not be large by the changes in future hydrological drought. In these sparsely populated areas the affected population will be minimal, hence risk is low due to low vulnerability. For the (semi-)arid regions the impact will be much larger since a large number of people live in the transition zone between arid and Savannah climates. When these people are affected by changes in the hydrological regime and in particular hydrological extremes this will significantly impact the local population (higher vulnerability than colder climates). These changes may lead to forced emigration putting pressure on adjacent regions usually also scarce in water already. Uncertain projections for these regions will challenge policy makers and stakeholders to take appropriate decisions for drought adaptation measures in these regions. These reasons will be included in the revised manuscript in the discussion section.

After correcting for bias GCM-driven hydrological runs, is surprising to see some striking differences between them and the reference run for the cold-type climates (considering that WFD is used as target for calibration and to force the reference run). This may suggest that either the bias correction or the hydrological simulation is failing during the control period to replicate the reference run. This issue should be properly addressed and discussed by the authors.
We trust that the reviewer is referring to Tables 2 and 3, and Figure 2. The Similarity Index (SI)

when we take all climates together is above 90% (last column, Table 2). This implies that in the control period (1971-2000) the bivariate probability fields of the duration and the standardized deficit volume obtained with the hydrological model forced with the GCMs largely coincide the one that we got with the model forced with WFD (Figure 2). We see that only the cold climates (E-climate) have rather low SIs (<75%) for all three GCMs (and to some extent the B-climate). This illustrates a weakness of the bias-correction approach as proposed by Piani et al. (2010a; 2010b). This conclusion is supported by the comparison of the drought duration (upper part, Table 3) for ECHAM and CNRM. IPSL deviates. The agreement (percentage) for the standardized deficit volume between numbers obtained with GCM forcing against WFD is indeed lower, but it is definitively affected by the low magnitude of the standardized deficit volume leading to rather large % errors. We will address these aspects more thoroughly in the Results and Discussion.

Authors must acknowledge that a mini-ensemble of three GCMs will most likely undersamples the climate model uncertainty. In addition, a discussion on how well the selected GCMs could span the range of climate predictions could be provided.
The authors will motivate in the manuscript why these GCMs have been selected and also refer to other studies using a similar number of GCMs. In the Results and Discussion we will show where the difference and compatibilities between the models are. The authors acknowledge that this mini-ensemble will very likely undersample the total uncertainty and will include information on this issue in the Discussion of the revised manuscript.

Specific comments
P7704 l17, delete “and”
The authors will follow the suggestion made by the reviewer

P7704 l21, define GHGs
This will be corrected to GHMs (typo)

P7705 l28, which
The authors will follow the suggestion made by the reviewer

P7706 l3-14, as written it gives the impression that different bias correction methods were applied on different variables, and using different observation datasets as targets. Please rephrase for clarity
The manuscript will be rephrased, since only one bias correction method was applied

P7706 l20, define SRES A2 scenario. A short description of the A2 scenario should be given
The authors will follow the suggestion made by the reviewer and will include a brief description of the A2 scenario.

P7707 l25, I would suggest to use a consistent nomenclature in the article. So, Fig1 and eqs 1-6 should refer to the same variable names
The authors will follow the suggestion made by the reviewer

P7709 l20-22, I would suggest to briefly report on the performance of the synthetic model just for completeness
The authors will follow the suggestion made by the reviewer. We will briefly describe model performance and we will refer to literature (e.g. Van Lanen et al., 2013; Van Loon et al. 2014).

P7710, What about persistence of droughts? Is the smoothed time series accounting for frequent short-duration events? Please clarify
This approach includes short-duration events, since only the threshold is smoothed (daily-smoothed monthly threshold), but the original input time series is not. Therefore, the short (<5 day) droughts are still present in the drought characteristics.

P7711, What about frequency of droughts? Will Q80 for control be different than for future time windows? Please clarify

We used the same threshold (i.e. Q80), both for the control period and for the two future time windows (2021-2050 and 2071-2100). The frequency of future drought events has changed and is a function of the PDY (Percentage Drought per Year) divided by the average duration of a drought event. This will result in the average drought frequency per year. This description will be added to the manuscript.

P7711 l20-22, How many of these locations were excluded under this criterion?
A total of 21 locations were excluded from the analysis, either because they were located in a desert area or in a region with constant freezing conditions (glaciers). This will be added to the manuscript.

P7712 l6, I would suggest replacing the term “scenarios” by “time windows/periods/..” to avoid confusion
The authors will follow the suggestion made by the reviewer

P7712 l20, definition of “m” and “n” is not clear
The authors will clarify the meaning of “m” and “n”, which are the standardized deficit volume and the duration in the kernel plots.

P7713 l3-4, how many locations were selected? 1495?
The total number is stated in line 9.

P7713 l10-14, having defined a minimum threshold of 30 locations per climate region for reliable drought characterisation, what is the explanation to have nearly half (47%) selected locations under snow and polar climates? It would be interesting to see whether projected drought changes in these two climates have a significant impact
These climate regions cover 47% of the world and accordingly also represent 47% of the sampled locations. We can evaluate the impact of these regions on the global climate change in an appendix and mention the major findings in the Discussion. This will provide the reader with more insights on the importance of these regions with respect to global climate changes. We kindly thanks the reviewer for this interesting suggestion

P7714 l20-25, P7715 l1-14, it is not surprising that reference and GCM-driven hydrological runs are similar since the latter were bias corrected using WFD as target over the same period. What is surprising is the fact that results show some striking differences for cold-type climates (D/E) even after bias correction, thus suggesting that either the bias correction or the hydrological simulation is failing during the control period to replicate the reference run. This must be addressed and properly explained
This is probably an issue that has to do with the fact that the downscaling of the GCMs is done based on number of rain days and similar statistics, while it could be more important to see up to what extent the frequency, co-occurance and severity of these events impact drought events. (see also reply to reviewer#4, item 3 under Major Comments).

P7713 l22, “Sect. 3.2”
The authors will follow the suggestion made by the reviewer

P7715 Fig2, I am missing an explanation for the odd behaviour of CNRM around 30 and 120 d duration
We did not investigate these details, but more the generic patterns. It seems to be linked to the snow climates (D-climate). Likely, the model structure of CNRM, in particular the snow accumulation and snow melt, causes the small, but noticeable deviations from the general shape of the bivariate distribution. A thorough analysis of simulated discharge and comparison against the threshold will give more insight. We will include this in the Results.

P7715 l5-8, most likely both desert and polar climates are related to extreme events, does this mean that the conceptual hydrological model is rather weak for extreme climate conditions?
The extreme events probably suffer more from the combination and co-occurrence of

precipitation events than other climate types. In these climate types a single precipitation can have an higher impact than, for example, in a generally wet moderate or tropical climate. The model has been validated in a snow dominated region and did not show significant problems. Especially since the snow module of the model is well tested and it is similar to the well-known HBV model (Bergström, 1976; 1995; Seibert, 2000). The authors believe that the occurrence of precipitation events in the GCM simulations is the driving force between the differences between observations and simulations. For the more extreme cold climates the daily temperatures and related snow-melt will result in a similar impact.

P7715-7716, section 4.2, reporting percentage changes respect to the control period is acceptable; however, readers deserve to know how significant these changes are from a statistical point of view. I would suggest testing the significance of these changes. Can these changes be related to a decrease of precipitation in equatorial/warm template climates or D climate becoming colder in the future, thus storing more precipitation as snow?
More details on the significance of the obtained results will be added to the manuscript. Significance (where possible) will be added and additionally relations between obtained results and changes in driving meteorological observations will be added to the manuscript. When positive of negative significant relationships exist they will be reported in the revised manuscript.

P7718 l2-3, climate-type D keeps coming back with peculiar results. Authors should aim at explaining these results more in depth
The authors will follow the suggestion made by the reviewer. We will add descriptions in the Results and Discussion.

P7718 l26, “main reason”?
We will revise, i.e. “mean reason” will be replaced with “main reason”.

P7718 l26-27, what about defining this as “climate model uncertainty”
The authors will follow the suggestion made by the reviewer

P7721 l13-14, it wouldn’t hurt trying to hypothesise on possible causes for this mismatch
The authors will follow the suggestion made by the reviewer and will add some more information about the B-climate and E-climate.

P7721 l25-27, I did not see any seasonal analysis throughout the manuscript so, this sentence may need rephrasing
More details on the seasonal analysis will be added to the manuscript and the sentence will be rephrased according to the changes in the manuscript (see reply, 1st item Major Comments).

P7736, fig1 define t and j
The authors will follow the suggestion made by the reviewer

P7738, fig3 it might be better to show the relative change/difference with respect to the control period and enlarge the scale of the panels a bit
The authors will follow the suggestion made by the reviewer

Recommendation

Based on the above review I would suggest publishing the article after major and minor revisions have been provided.

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