Dear Editor,

Thank you very much for taking into account our paper.
In the following table, we answer in detail to every comment given by you and all the reviewers. Thanks to your observations, we have corrected our mistakes, modified some sections and improved the quality of the paper.

Kind regards
The authors

Comments by the editor	Comments by authors
On many issues raised by the referees you already state in the replies that you will add the modifications/details that are asked for. In reference to other comments you explain or detail the issue in the reply, but do not state that you will modify the revised manuscript accordingly (one of such examples is the addition of the steps from Eq 1 to Eq. 2): I suggest you to always do so (if the referees, who are experts in this research area, cannot understand the meaning of a paragraph or of a figure, the 'average' reader will have even more difficulties…).	We modified and added in the paper the answers previously explained to the referees, as you suggested.
One of the main issue is that of the data employed for calibration and validation of the hydrological model: the use of the observations and meteo forecasts of the 3 years should be clarified, as highlighted by all referees. Reformulate ll. 13-21 of page 15815 to clarify how the hydrological is initialized (see Ref2's comment).	In Section 2.2 (Pag. 5) we describe the coupling strategy for the forecasting chain between meteorological and hydrological model, and the available dataset where we explain which parameters were calibrated, which hydro-meteorological data were measured and which are the required information to set up the hydrological model.
Concerning data availability and the use of field measures (often not available in real-world applications), you should also better clarify which model parameters are derived from measures and which have been calibrated, see Ref1's comment (in fact nor the caption nor the text referring to Table 1 state that such values refers to parameters that were optimised; and, if a calibration was performed, some details on the optimization procedure must be added).	a) In order to set up the hydrological model, it is necessary (Pag. 5, section 2.2): - Land use - Water retention properties for the soil texture (Table 1) - Soil depth - Hydraulic conductivity (Ks) - Soil moisture - Type of vegetation (date of sowing and harvest) - DEM - Aquifer parameters - Scheduled irrigations - Observed weather data (at least temperature and precipitation) Soil texture, Ks and soil moisture values were also measured in-situ. Soil properties (shown in Table 1) for the Livraga silt loam soil were calibrated as well as the soil depth which

	was modelled as a single layer. As mentioned in the text (L.12, Pag. 6), eddy-covariance measures to control actual evapotranspiration (ET) fluxes are not necessary for the PREGI target. b) In order to couple the FEST-WB model with the WRF meteorological model, weather forecasts (temperature and precipitation fields) are required.
And it is absolutely fundamental the addition, that you now intend to carry out as specified in your replies, to add a comparison between the simulations obtained with or without the use of the PREGI platform.	As it is shown in the text (Pag. 13), this issue is the main addition we did (Section 3.3). We re-ran two simulations: one assuming that the landowner follows the advice provided by the PREGI platform on when to irrigate, and the other assuming that he follows the currently planned decision criteria. The results show that one out of three irrigations could have been saved!
2) The second important issue is that of skill assessment and, related to that, the better definition and meaning of the thresholds, as raised by Ref2. Ref2 suggests to use Brier Skill Score for assessing the improvement of the proposed approach in respect to an unskilled standard forecast: if you do not have climatological information on the pilot case study, you may use persistence as a standard for reference.	As explained in the previous answers, we cannot use the climatological data, since weather data on our experimental test-site (Livraga) were available for the 3 project years (2010, 2011 and 2012) only, hence this period is not sufficient to be analysed from a climatological point of view. In regard to the persistence score, since a persistence forecast is defined as "*a forecast that the current weather condition will persist and that future weather will be the same as the present (e.g., if it is raining today, a forecast predicting rain tonight)*" (NOAA), this skill index is usually applied with daily precipitation values and not with cumulated values, such as we carried out for cumulated precipitation forecasts over a period of 1, 2, 3,…, 30 days (Pag. 7).
The meaning of the threshold is not well-defined, too: rephrase ll. 1-5 p. 15824, clarifying the period of cumulated rainfall and the meaning of such thresholds; the phrase ' the last two values are quite equivalent' is indeed not clear as highlighted by Ref2.	As better explained in the text (L. 8-19, P10): In regard to the BS score, suppose that the forecast probability to exceed a threshold of cumulated rainfall is 70% and then this event occurs, the BS score is equal to 0.09; vice versa if it does not occur the BS score is 0.49; therefore, best scores are close to 0. In this analysis, three thresholds were chosen: 20, 50, 100 mm; these last two values are reasonably similar to half and full irrigation in the Livraga maize field, while the 20 mm threshold corresponds to typical precipitation amounts in that area, which is not usually affected by heavy rainfall in the summer season, as occurred in 2012. It is important to bear in mind that this computation, performed with the entire forecast dataset, is not referred to daily precipitation values, but cumulated precipitation values over a period of 1, 2, 3, 30 days. For instance, the BS score at 7th day as lead time considers the occurrence probability of a cumulated precipitation forecast over a period of 7 days to exceed the threshold of 20, 50 or 100 millimeters (occurred over the same time period of 7 days).
Meaning and interpretation of Fig. 7 must be clarified too (mening of BS in reference to the thresholds).	a) Fig. 7 (now changed in Figure 5 (Pag. 26)) shows the REPS-WRF model performance with forecasted precipitation, using the Brier Score index for a forecast horizon from 1 to 30 days during the 2012 growing season. b) L.8-15, Pag. 13: Our decision, to show the weather model performance over a period of 1-30 days as lead time, is the result of a preliminary investigation carried out with the landowner of the Livraga field who is the real

	decision-maker: from his point of view he was more interested in knowing the reliability of a cumulated precipitation forecast over 7 days or 10 days and not whether it is going to rain exactly on the 7th or 10th day from the forecast initialization date. c) L. 7-8, Pag. 13: For the 2012 growing season it is found a good level of the forecast reliability (BS values lower than 0.15) within the first 10 days even for an occurrence probability forecast to exceed the threshold of 20 mm (cumulated in 1, 2, 3,…,10 days). Therefore, the Livraga landowner can rely on cumulated precipitation forecasts at least for one week (which the available irrigation time allotment for his field). We are aware, we cannot draw general conclusions with one-year analysis only, in fact, one of the future developments is to extend the study over different sites with other case studies during future growing seasons. However, taking into account the cumulated precipitation forecast over 7 days or 10 days and not whether it is going to rain exactly on the 7th or 10th day, the performance shows a good starting point for a real-time drought forecasting system for irrigation management and answers to landowner's expectations.
I believe, too, that some of the doubts/concerns of the Referees are due to the English syntax: a final revision of the language is now done for every article in HESS by the Editorial office, but of course if the English is improved and made clearer in the revised manuscript it would greatly help the second revision process (and also the following English editing). I warmly suggest to ask a colleague to revise the manuscript.	A revision of the English was done.

Comments by Reviewer 1	Comments by authors
My main concerns regard the impact of the paper. In its current form, the manuscript provides an application of existing coupled meteorological and hydrological models for real time drought forecasting in one location in Northern Italy, with two-year calibration and one year validation. The impact of the paper would be greatly enhances should the author choose i) to discuss the applicability of the tool beyond the specific case study; ii) to objectively present strengths and weaknesses of the proposed modeling framework when applied for irrigation management; and iii) to quantify the advantage of employing such a tool. The first two points are crucial in defining the applicability of the proposed framework in routine, 'real world' problems – which, as far as I understand, is the final goal of the project. This discussion should include also clearer information on data requirements for model running, as well as information of the ability to the model to provide reasonable results upon calibration with a more limited (but more common) data availability.	As written in the text: a) L. 28-33, Pag. 4: The experimental test-site for the PREGI Project is a field located in the middle of the MBL basin at Cascina Nuova farm in the town of Livraga, where meteorological, eddy-covariance stations and TDR probes for evapotranspiration fluxes and soil moisture profile have been respectively installed to measure hydrological processes Since no measures in other consortium fields were available to calibrate and validate the hydrological model, it was not possible to verify the PREGI forecasting system outside the Livraga experimental site. Notwithstanding this, such a system can be replicated in any geographical area and vegetated field, on condition that soil features, weather, hydrological data and irrigation time allotments are available. b) L. 12-18, Pag. 6: In addition to these soil analyses, eddy covariance measures were used to control actual evapotranspiration (ET) fluxes and to make a comparison with the ET simulated by the FEST-WB model (see Sect. 3.1 for further details). In case eddy covariance measures are not available, the system target would not in any case be affected, since the main hydrological variable is the soil moisture, and TDR probes are sufficient for monitoring and forecasting purposes. On the contrary, the limits of such a system, in order to be replicated in other areas, are the availability of real time data (weather and soil moisture values), amounts and scheduled irrigation allotments. c) Section 3.3 (Pags. 13-14) quantifies the advantages that the Livraga landowner could have obtained if he had followed the PREGI system, saving one irrigation in the 2012 growing season.
The last point, the quantification of benefits, aims at investigating whether such tool can really make a difference in water management. The first step in this direction is clarifying what role the model suggestions played in the investigated case: this point is currently not very clear, with an irrigation application the day before a major rainfall event, but also a hint to the farmer employing PREGI in his/her water management choices (also, if the forecast was used for water management, how could that be done before model calibration?)	As written in the text (L.8-9, Pag. 11): "Unfortunately, in 2010 the PREGI tool with hydro-meteorological forecasts was not yet in service and it was only available for the 2012 vegetation season."
A more in-depth exploration of the advantages of such a toolbox – which I strongly suggest – would require run two season-long simulations, one	As above-mentioned, section 3.3 (Pags. 13-14) quantifies the advantages that the Livraga landowner could have obtained if he had followed the PREGI system, saving one irrigation in the 2012 growing season.

assuming the farmer follows the PREGI platform suggestions for when to irrigate, the other assuming that the farmer follows the currently employed decision criteria (which could even be as simple as irrigation applications whenever possible). The comparison of total applied water between the two runs will make it possible to assess the benefits of such a system in terms of water savings, the difference in total transpiration (or occurrence of periods with low soil moisture) can be used as a (rough) proxy of yield.	
A similar analysis could be extended beyond the three-year timeframe, to fully assess the advantages of such a system under a variety of climatic conditions.	Meteorological forecasts provided by the REPS-WRF were available in the 2012 season only. As written in the conclusions (L.13-16, Pag. 15), one of the future developments is to extend these analyses over different sites with other case studies during future growing seasons.
The model undergoes a calibration based on the data available at the case study site. Nevertheless, no mention is made of which parameters need calibration. This is an important information when considering the applicability of the model beyond the very specific (and data rich) case study (see above).	Please, see the above comment to the editor.
The measures of model performance ought to be defined within section 2 (the scope of which should be broadened to 'Methods'), discussing what specific aspect(s) of model performance they allow assessing. In this way, the result section can be focused on just presenting the model performances. The description of data availability (now at the beginning of the result section) should be moved earlier, either by widening the scope of current section 2 or within a new sub- section in section 3, which then should be broadened to 'Methods', as also suggested above).	Results and discussion are described in Section 3, while the measures of model performance were moved in Section 2.6, and the description of data availability in Section 2.2.
The presentation of the PRE.G.I. platform, including Fig. 8 and the description of the website, is unnecessary within the general economy of the paper and could be omitted/moved online as supplementary material.	The presentation of the PRE.G.I. platform was moved in the "Appendix" (Pags. 15-16), while some parts in regard to the website description were omitted.
I suggest broadening the introduction and discussion with reference to other related works (also broadening the reference list – current references mostly refer to works focusing on the	References were broadened with other related works focused on the optimization of irrigation management. L. 8-13, Pag. 3: In particular, we highlighted how scientific literature proposes different methods, more related to statistical approach, for optimizing irrigation scheduling and planning, while the application suggested

same region in Italy, which is relevant but not unique in the international arena).	in this paper takes into account observed soil moisture, weather data and updated forecasts to provide landowners with a suitable product for real-world farm profit optimization.

Comments by Reviewer 2	Comments by authors
One of my main concerns is how the model was validated.	The validation of the model is referred to the FEST-WB hydrological model as described in Section 3.1 ("Calibration and validation of the FEST-WB model"). While in Section 3.2 we describe the PREGI performance with three statistical indexes for the 2012 growing season: a) The MAE and MRE for soil moisture forecasts; b) The NS for cumulated precipitation forecasts including the irrigation contribution over a period of 1-30 days as lead time; c) The BS for the RESP-WRF weather forecasts over a period of 1-30 days as lead time;
In figure 7 the authors states that the rainfall forecasts shows better skills for more extreme precipitation thresholds (100 mm), however this is not completely true if not misleading. As the Brier score is defined, the rarer an event it is easier to get a better BS without having any real improvement in the forecast skill.	Since the BS is calculated over a period of 1, 2, 3,..,30 cumulated days, 100 mm can be considered as extreme event only if they occur in a few days, but not in 7 or more days. However, as written in the text (L.2 -15, Pag. 13): "In the way in which the BS is defined, the rarer an event, the easier to get a better BS. This is true if we consider the frequency of events, which exceed the threshold of 100 mm cumulated in 1, 2, 3,…, 30 days, occurred during March-August 2012, and more in general in the summer season in the Po Valley area, in comparison with the cumulated precipitation values (observed/forecasted) of 20 mm which are much more typical from a climatological point of view for this area; however, there is a good level of reliability (BS values lower than 0.15) within the first 10 days even for a threshold of 20 mm cumulated in 10 days. Notwithstanding this, our decision to show the performance over a cumulated period of 1, 2, 3,…, 30 days is the result of a preliminary investigation carried out with the landowner of the Livraga field who is the real decision-maker: as mentioned above, from his point of view he was more interested in knowing the reliability of a cumulated precipitation forecast over 7 days or 10 days and not whether it is going to rain exactly on the 7th or 10th day from the forecast initialization date."
In this respect I would recommend the authors to benchmark the model with different metrics that take into account a reference forecast as the climatology or the persistence. Just to name one, this is the case for the Brier Skill Score (BSS, see Mason 2004). In this way, some of the authors' statements need additional justification.	As explained to the editor and referee 1 (see the above comments), we cannot use the climatology as reference, since weather data over our experimental test-site (Livraga) were available for 3 years (2010, 2011 and 2012) only, hence this period is not sufficient to be analysed. Neither the persistence score could have been used, since a persistence forecast is defined as "*a forecast that the current weather condition will persist and that future weather will be the same as the present (e.g., if it is raining today, a forecast predicting rain tonight)*" (NOAA). In this study, we calculate the BS not with daily values (as it is usually performed), but with cumulated precipitation forecast values over a period of 1-30 days.
Page 15815, lines 16-18: Here is not clear the source of the temperature and precipitation data. Are an output from the WRF or is observed data? Why at every 2 days? And not 1 or 10 days?	As written in the text (L. 22-29, Pag. 6 and L. 3-4, Pag.12): Probabilistic forecasts (temperature and precipitation) were provided by the REPS, based on the WRF-ARW model, implemented and developed by the EMC. The forecast has a lead time of 30 days while the temporal resolution is 12 hours. The REPS-WRF is carried out

	every two days, since this is the computational time to run the combined system and, in fact, the data set includes 90 forecast instances out of about 180 days between 27 February and 31 August 2012.
Then in the next sentence the authors state that the hydro model is initialized with observed data. Are referring to the same data from the previous statement? I do think that the entire paragraph need to be rephrased, please try to be more specific here.	As written in the text (Section 2.2, Pag. 5), the FEST-WB hydrological model can be fed with observed weather data (used for hydrological simulations and for creating the initial soil moisture conditions), and with forecasted data by the REPS-WRF model (to generate soil moisture forecasts).
Page 15815, lines 22-24: "In addition to observed and forecasted data, the knowledge of scheduled irrigation dates are fundamental to calculate the irrigation water input over the experimental field of Livraga." This is a general statement or the authors want to refer to the information used in the analysis? This sentence seems to be disconnected, please rephrase.	As written in the text (L.30-34, Pag. 5): "In particular, amounts and methods of water allotments are fundamental to keep updated soil moisture initial conditions. In fact, since irrigation allotments are planned by the MBL consortium, landowners cannot irrigate their fields on days other than the scheduled ones; therefore, this information becomes mandatory in this hydro-meteorological forecasting chain."
Page 15819 eq (2): For me it is not clear how eq(1) becomes eq(2) and how the stress threshold is defined. A clearer link between the two equations is necessary. Please explain in more detail the meaning of RAW and TAW and their link with the stress and water surplus threshold. Page 15819 lines 12-13: the values of 0.23 and 0.33 are intended to be incorporated in eq(2)? I can't follow the construction of this thresholds.	This was better detailed in the text (L.25-34, Pag. 8 and L. 1-4, Pag. 9).
Page 15820 line 4: It's hard to see the contribution of the precipitation and irrigation separately. I suggest to use a stacked bar with two colors (one for each contribution) in the figures 2, 3 and 4 and enlarge the axis fonts -specially the horizontal axis- as it is difficult to read them in the printed version.	This suggestion was accepted and this figure was modified (Pag.23).
Page 15820 paragraph between lines 9-13: Is this paragraph referring to Figure 3? If yes, I would recommend to swap this paragraph with the next in order to present the Figure first.	This paragraph was changed (L.10-13, Pag. 11).
Section 4.2. I feel that this section could be reorganized and addressed in a better way. For instance, some results of the performance metrics are presented first than the metric is defined. This is the case of MAE and MRE that are already depicted in the previous section 4.1. Also MAE values are presented but this metric is not	Statistical indexes, which are used in this analysis, are moved in Section 2.6 (Pags. 9-10) separately, while the PREGI performance is described in Section 3.1

defined at all in the text. The Nash-Sutcliffe index is used (lines 16-20 page 15822) before the equation is defined (eq-4). Also the acronym related with this index should be homogenized (NS or ENS, or are different things?). I suggest to present first the performance metrics used. Sections 2 and 3 can be merged as a section named as data and methodology where all the metrics can be defined. Or preferably this metrics can be defined separately in an appendix.	
Page 15824 line 2: It's not clear to me why the thresholds of 50 and 100 mm are equivalent? Please explain or rephrase.	As we explained in Section 2.4 (L.26-31, Pag. 7), the estimated irrigation input implemented in the FEST-WB model was assumed to be equal to 108 mm, hence 100 mm as threshold is reasonably similar to full irrigation water allotment, while 50 mm (as threshold) can be thought as half irrigation water allotment in the Livraga maize field.
Figure 7. In my opinion this is one of the weakest points of the paper. The authors states that the greater skill is observed for the forecasts of the extreme events. These results obtained in such a short period are only an artifact of the methodology used to assess the skill.	Although this index was performed in the 2012 growing season only, it takes into account 90 forecast instances from 27 February to 31 August. In regard to "*the greater skill for the threshold of 100 mm*", see the above comments.
I would recommend the authors to assess the skill of these forecasts by using other metrics that take into account reference forecasts like the climatology as a benchmark.	Please, refer to the above-comments to the editor and referee 1.
Page 15824 lines 19-21: This sentence is a little bit cryptic. Please consider rephrasing it.	This paragraph was re-written (L.31-34, Pag. 15 and 1-6, Pag. 16).
Page 15824 lines 22-26. After reading this sentence, I'm not 100% sure if I understand how the probabilities were computed. The number of ensemble members exceeding the threshold is a daily value or it is accumulated over 30 days? Please explain. Figure 8: What are the meaning of the yellow circle and the 60% value? Please add a clarification in the caption.	As shown in Figure 9, the picture shows 60% probability (i.e. 12 ensembles out of 20) of exceeding the surplus threshold in at least one of the subsequent 30 days with the forecast simulation started on 31 August 2012. Therefore, the value displayed on the colored dot means the higher daily probability value over a period of 30 days. (L.31-34, Pag. 15 and 1-6, Pag. 16).
Figure 10: As far I understood the extra irrigation water is not affecting the rainfall forecasts but is a deterministic value that is systematically added to both observed and forecasted information. I think that adding here the water added for irrigation is not necessary and can hide the real magnitude of the	Since this picture is unnecessary within the general economy of the paper, it was omitted.

differences between the forecast and observations. Also it can be helpful to see in the plots the 25 and 75th percentile as in figure 9. Page 15825 lines 15-18: The authors state that "The comparison between the REPS-WRF model forecast and the observed value at Livraga rain gauge (leaving out the two scheduled contributions coming from irrigation which are known a priori) shows a good agreement during the central phase of the maize growing season." How the authors drawn this conclusion? Is hard to see it from figure 10 as the magnitude of the irrigation is too high. Please consider to redraw Fig 10 with only the accumulated rainfall. Moreover, how the authors determine the good agreement? Is this measured somehow or is only a graphical estimation? Please provide further elements that sustain this conclusion, as this is one of the key questions.	
Page 15826 lines 10-16: This paragraph is a quite general statement that is not supported in the paper. Moreover, I can't agree that the system presented in this paper "has a higher reliability in comparison with flood forecasting systems", at least I can't found any evidence of that in the paper. Please consider deleting or rephrasing this paragraph as in the present form is not completely accurate.	Parts of the conclusions were re-written (L. 28-34, Pag. 14 and L.1-5, Pag. 15) and that statement omitted, as you suggested.

Comments by Reviewer 3	Comments by authors
The topic of the paper is interesting and challenging, but I think a proper validation of the procedure is still missing. Only one growing season (2012) was considered to evaluate the reliability and the benefits of the forecasting chain, but the reliability assessment would definitely need more than a year of experiment and the benefits should be more clearly investigated by comparing two situations, one supported by the forecasting system and one without this system. Results are not well documented and not clearly explained.	Following your suggestions, we added the Section 3.3, where we quantify the advantages of the PREGI system in the 2012 growing season. Unfortunately, meteorological forecasts provided by the REPS-WRF were available in the 2012 season only, and it was not possible to test in other seasons. As written in the conclusions (L. 13-16, Pag. 15), one of the future developments is to extend these analyses over different sites with other case studies during future growing seasons.
The potentials of the forecasting system for other case studies is not discussed, nor are its limits.	As answered to referee 1 and written in the text: a) (L.28-31 Pag. 4 and L.1-2 Pag. 5) The experimental test-site for the PREGI Project is a field located in the middle of the MBL basin at Cascina Nuova farm in the town of Livraga, where meteorological, eddy-covariance stations and TDR probes for evapotranspiration fluxes and soil moisture profile have been respectively installed to measure hydrological processes Since no measures in other consortium fields were available to calibrate and validate the hydrological model, it was not possible to verify the PREGI forecasting system outside the Livraga experimental site. Notwithstanding this, such a system can be replicated in any geographical area and vegetated field, on condition that soil features, weather, hydrological data and irrigation time allotments are available. b) (L.12-18, Pag. 6) In addition to these soil analyses, eddy covariance measures were used to control actual evapotranspiration (ET) fluxes and to make a comparison with the ET simulated by the FEST-WB model (see Sect. 3.1 for further details). In case eddy covariance measures are not available, the system target would not in any case be affected, since the main hydrological variable is the soil moisture, and TDR probes are sufficient for monitoring and forecasting purposes. On the contrary, the limits of such a system, in order to be replicated in other areas, are the availability of real time data (weather and soil moisture values), amounts and scheduled irrigation allotments.
In chapter 2 a clear explanation of data used in this work and for model validation purposes is missing.	Please, see our comments to the editor and referee 1.
Part of it is included in chapter 3 but should be moved in my opinion to chapter 2.	This suggestion is accepted and Section 2.2 (Pags. 5-6) describes also the available dataset in order to set up hydrological simulations.

Page 4 – line 6-7 - meteorological fields are available every two days? or every 12 hours (twice a day)?	Please, see our comments to referee 2.
Page 5 line 5-6 – 200 m spatial resolution and daily time scale, you should discuss the suitability of this space and time scale for the goal of your analysis	As written in the text (L.13-17, Pag. 7): "The spatial domain is discretized with a mesh of regular square cells (200 m in this application), while the temporal resolution of soil moisture simulations and forecasts calculated on a daily time scale; since the Livraga maize field is about 8 ha wide and the landowner schedules his activities on daily/weekly planning, both the spatial and time scale turned out to be appropriate from a computational time point of view."
Page 6 line 33 – deduction of eq. 2 is not clear.	The deduction of equations used in Section 2.5 was clarified (L.25-34, Pag. 8 and L. 1-4, Pag. 9).
Results and discussion- Figures and numbers provided only refer to the Livraga site, while it would be interesting to see how the hydrological model performs on the whole simulated domain (Livraga experimental filed?)	As written in the text (L.31-33, Pag. 4): "Since no measures in other consortium fields were available to calibrate and validate the hydrological model, it was not possible to verify the PREGI forecasting system outside the Livraga experimental site."
References - Two papers by Ravazzani et al. (2011) are actually listed, they shoud be probably cited as 2011a and 2011b. Wilks (2006) is not listed, nor is Joliffe (2003) which should probably be substituted in the text by Joliffe and Stephenson (2003).	References were corrected.

Comments by T. Caloiero	Comments by authors
Pag. 15812 Line 8 The fourth IPCC Report has been cited, but the fifth IPCC Report has been published even though only as "Summary for Policymakers". I suggest to cite the fifth IPCC Report (2013) and insert the following reference in the reference list: IPCC, 2013: Summary for Policymakers. In: Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, S. K. Allen, J. Boschung, A. Nauels, Y. Xia, V. Bex and P.M. Midgley (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA.	This reference was changed, as you suggested.
Pag. 15814 Lines 10-20 In the introduction the aims of the paper are not clearly stated, so, I recommend rewriting the paragraph from lines 10 to 20.	Parts of the introduction were re-written to better clarify the aims of the paper (L.8-25, Pag.3).
Pag. 15817 Lines 3-5 In these lines the authors refer to some precipitation and temperature gauges, avoiding details about their location. I suggest to localize these gauges in Fig. 1a. Figure 1 I suggest to localize the precipitation and temperature gauges in Fig. 1a and to insert a bar scale in Fig. 1b.	Figure 1 (Pag. 22) was changed as you suggested.
Formulae Results of some indices are described before the equation are defined, I suggest to define the formulae and then to describe the results of the application of these formulae (e.g. Nash-Sutcliffe)	Statistical indexes were moved in section 2.6 (Pags.9-10).