Note: References to specific lines in this document start with R (e.g. R100-106) and to lines in manuscript with M (e.g. M40-43).

Major changes

- We resimulated maize WFs as we switched from MIRCA2000 (see lines R392-396) to a new harvested area dataset SPAM2010 (Yu et al., 2010). Also, our previous groundwater map was created with a wrong upscaling function in QGIS. We also allowed the growing season to be up to 15 % longer if the accumulated GDDs on the original harvest date are not sufficient for the crop to reach maturity (see Sect. 2.2, lines M202-203). The final results are barely affected by these changes. The main differences are in a smaller number of simulated grid cells and smaller contributions of irrigation and shallow groundwater to WFs.
    - Yu, Q., You, L., Wood-Sichra, U., Ru, Y., Joglekar, A. K. B., Fritz, S., Xiong, W., Lu, M., Wu, W., and Yang, P.: A cultivated planet in 2010 – Part 2: The global gridded agricultural-production maps, Earth Syst. Sci. Data, 12, 3545–3572, https://doi.org/10.5194/essd-12-3545-2020, 2020
- We switched from the MATLAB version of AquaCrop to a recently published Python one in order to have all ACEA's code in one language. These versions are identical, so the results are not affected. However, the Python version is much faster.
- We added an elaborate description of AquaCrop and green-blue accounting in Sect. 2.1.2 as it was requested by the reviewers.
- A new section on the sustainability of maize production (Sect. 4.3) was added as we did not pay enough attention to it in the previous version of our manuscript.

Other changes and the responses to reviews are provided on the next pages.

Below you can find our collective responses to each point raised by reviewers.

Blue for our comments and *purple for our changes to the manuscript.*

Response to RC1

Summary:

The paper introduces AquaCrop-Earth@lternatives (ACEA), a global gridded crop model that was used to estimate maize WF over a longer period, splitting the WF into green, blue-irrigation, and blue-capillary rise. It's well-written and will make a valuable contribution to the literature. However, before the work is accepted, I have a few concerns that must be addressed:

Major comments:

- The ability to model the role of capillary rise to meeting crop water demand at a worldwide level with high spatial resolution is one of the current study's key achievements. That is something I applaud the authors for. However, I have reservations about using a simplified strategy and depending on a single static groundwater table data set that does not account for interannual change or variation during the irrigation season. Fan et al. (2013) have stated clearly that their data is based on a simple WTD that ignores local geology and is presented in its natural state, without groundwater pumping and drainage. You extrapolated this modeled data, which represents a natural condition during the modeling period, over decades. You're presuming that the WTD remains constant across decades and season to season. This assumption is not supported by scientific evidence. I have a few reservations about this strategy: 1) due to natural fluctuations in precipitation, the groundwater table varies within a year and over time; 2) depending on the intensity of groundwater pumping during the irrigation season, the groundwater level declines by as much as 20 meters or more. As a result, the blue WF from the capillary rise is only 'potential' and not actual. I recommend that you address this limitation by using data that includes interannual and seasonal groundwater level fluctuations. Alternatively, you may refer to your estimate as the potential blue WF assuming everything remains the same.

  Since we use the same monthly water table depths (WTD) from Fan et al (2013) as input for every year, there are indeed neither interannual changes in WTDs nor dynamic coupling with our crop model. This is a limitation of our approach, which unfortunately cannot be avoided due to the lack of global historical data on WTDs. At the same time, we would like to note that the contribution of shallow groundwater levels to maize WF is very small (1.2 % globally with the current set-up), and thus this should not affect our main conclusions. In a revised version of the manuscript, we elaborate on this limitation in more detail in Sect. 2.2 (lines M217-223) and 4.2.2 (lines M425-426).

- The yield scaling factor is another concerning simplification. The argument is that others have done it before us, so it's fine if we do it the same way. Because of improved maize types with higher HI, yield has increased over time. The crop structure has altered from a larger plant with leaves that fall laterally to a more compact plant with leaves that grow vertically, allowing for closer planting and increased grain output. How do you explain a +56 percent increase in yield factor (S) with no influence on evapotranspiration? Please support your claim with evidence from the literature that new maize varieties have the same ET as older varieties but yield more.

  This is indeed one of the major assumptions of our and several previous studies that we referred to. First, we would like to point out that according to Duvick (2005) and Lorenz et al. (2010), the harvest index (HI) is not responsible for the historical increase in maize yields as it stayed relatively constant during the last decades. Instead, the main drivers for yield increase are higher plant density (as you already mentioned), improved biotic and abiotic stress resistance, and better field management (fertilizers, chemical control of weeds and insects etc.). In ACEA, we attempt to represent the combined effect of these drivers by scaling the simulated yields to the national statistics from FAOSTAT. We only scale the yields but not the seasonal ET of maize (i.e. CWU) due to several reasons:

1. There is no strong evidence that maize CWU increased along with improvements in **maize varieties**. For example, Nagore et al. (2014) found that two new maize varieties have similar CWU as an old one in Argentina, and Xu et al. (2018) showed the only minor difference in CWU between large and small canopy maize varieties in China. One of the main conclusions from these studies is that differences in crop varieties may change the ratio of transpiration (T) to soil evaporation (E) but overall CWU would be only minorly affected as a decrease/increase in T is compensated by an increase/decrease in E. Both E and T consume green and blue water so we do not expect significant changes to overall green and blue CWU either.

2. To address the effect of increased **plant density**, we carried out sensitivity analysis and found that it does not lead to significant changes in CWU. We illustrate this by simulating maize production in one of the cells in northern Italy during 1986-2016 with three plant density values (see table below):

Plant density [plants ha^{-1}]	Average yield [t ha^{-1} y^{-1}]		Average CWU green [mm y^{-1}]		Average CWU blue [mm y^{-1}]	Average WF [m^3 t^{-1} y^{-1}]	
	rainfed	irrigated	rainfed	irrigated	irrigated	rainfed	irrigated
50000	7.2	13.5	343.5	309	187.6	477.1	367.9
75000	7.2	14	341.7	308.3	194.1	474.6	358.9
100000	7.4	14.3	341.4	310.6	195.7	461.4	354.1

   As you can see from the table, CWU values barely change with an increase in plant density. Thus, the difference in maize WFs comes from an increase in yields rather than in CWUs. Also, Barbieri et al. (2012) concluded that a change in maize row spacing does not affect CWU. Hence, having the same plant density value for the whole simulation period worldwide seems to be a reasonable assumption (in our paper we assumed 75 000 plants ha^{-1} as it is the most common value in literature).

3. As for the **fertilizer application** rates, Rudnick et al. (2017) showed that nitrogen inputs might increase maize CWU up to 13 % compared to no nitrogen input. Also, the authors demonstrated that CWU does not show the consistent direction of changes with different nitrogen application rates (from 84 to 252 kg ha^{-1}) but they were always in a range of -10 % to +10 %. In our study, we have to assume no nutrient stress (i.e. optimal nutrient supply) as AquaCrop cannot simulate the nutrient cycle. This might lead to the overestimation of CWU in the grid cells with no fertilizer application. However, we expect no significant impact on global maize WFs. First, because the potential overestimation of CWU by even 13 % is still minor compared to the effect of yield scaling. Second, because fertilizer application is a common practice among the big maize producers, and thus we think it is safe to assume that the (production-weighted) global average maize WFs would be hardly affected.

To sum up, the literature seems to indicate that historical changes in crop varieties and field management only minorly affect green and blue maize CWU compared to crop yields. Therefore, we conclude that using the yield scaling factors is sufficient to represent historical dynamics in maize WFs at the global level. We substantiated our assumptions in Sect. 2.1.4 according to the mentioned above reasoning. Also, we added a comparison of our maize CWU estimates to literature in Sect. 4.1.2, which shows that our historical estimates align well with considered studies.

References:

- D. N. Duvick, 'The Contribution of Breeding to Yield Advances in maize (Zea mays L.)', in Advances in Agronomy, vol. 86, Elsevier, 2005, pp. 83–145. doi: 10.1016/S0065-2113(05)86002-X
- A. J. Lorenz, T. J. Gustafson, J. G. Coors, and N. de Leon, 'Breeding Maize for a Bioeconomy: A Literature Survey Examining Harvest Index and Stover Yield and Their Relationship to Grain Yield', Crop Sci., vol. 50, no. 1, pp. 1–12, Jan. 2010, doi: 10.2135/cropsci2009.02.0086.
- M. L. Nagore, L. Echarte, F. H. Andrade, and A. Della Maggiora, 'Crop evapotranspiration in Argentinean maize hybrids released in different decades', Field Crops Research, vol. 155, pp. 23–29, Jan. 2014, doi: 10.1016/j.fcr.2013.09.026.
- G. Xu et al., 'A lysimeter study for the effects of different canopy sizes on evapotranspiration and crop coefficient of summer maize', Agricultural Water Management, vol. 208, pp. 1–6, Sep. 2018, doi: 10.1016/j.agwat.2018.04.
- P. Barbieri, L. Echarte, A. Della Maggiora, V. O. Sadras, H. Echeverria, and F. H. Andrade, 'Maize Evapotranspiration and Water-Use Efficiency in Response to Row Spacing', Agronomy Journal, vol. 104, no. 4, pp. 939–944, Jul. 2012, doi: 10.2134/agronj2012.0014.
- D. R. Rudnick, S. Irmak, K. Djaman, and V. Sharma, 'Impact of irrigation and nitrogen fertilizer rate on soil water trends and maize evapotranspiration during the vegetative and reproductive periods', Agricultural Water Management, vol. 191, pp. 77–84, Sep. 2017, doi: 10.1016/j.agwat.2017.06.007.

Specific comments:

Introduction: Some of the notes have mischaracterized the earlier studies:

- Line 44-45- point 1: True, the cited studies did not take into account thermal stress, but they did take into account water stress. Have you looked at the impact of thermal and water stress on crop output separately? Please clearly show the influence of thermal and water stress on crop production as well as the WF (m3/t) as you stated this is one of your additions to the worldwide WF study. You can include a map in the SI.

  The cited papers indeed consider the effect of soil water deficit by reducing the amount of actual daily ET and/or adjusting final crop yield. However, this approach neglects the dynamic feedbacks between crop development and water deficit. In previous studies, crop water requirements (CWR) during each day of the season are based on reference ET and a crop factor, which is a pre-described input not affected by the model. This means that when water stress occurs there is no feedback that actually affects crop phenology and CWR in the remainder of the growing season.

  In ACEA, water stress has multiple negative impacts on the crop (to name a few: canopy cover reduction, pollination failure, stomatal closure, restricted root development, harvest index reduction). The impacts not only affect the specific date when the water stress occurs but also subsequent days. For instance, water stress reduces canopy expansion, which directly affects the crop biomass and transpiration on subsequent days. Therefore, the approach in the cited papers can be considered rather simplistic compared to AquaCrop. We acknowledge that our statement in the paper is not entirely correct, so we removed the part about water stress to avoid confusion and kept only the thermal stress in Sect.1 (line M40).

  As for the separation of water and heat stresses, we cannot look at them separately in the current simulation setup of ACEA. The only way to do this is to perform separate simulations of maize production with and without heat and water stresses, which can provide valuable insights to model sensitivity and to the impacts of those stresses on crop WFs. Unfortunately, this is not the focus of our paper, but we agree that this topic should be addressed in future research.

- Line 54: " To our knowledge, global crop WFs have never been studied with GGCMs". This is not They may have not used the term WF but there are some global studies - look EPIC (Liu et al. 2007, Liu and Yang 2010, Liu et al. 2009, Liu et al. 2016) and LPJmL (Fader et al. 2010, Rost et al. 2008).
  We removed this statement and listed instead several articles (including some mentioned by the reviewer) that analyzed crop-water productivity with GGCMs in Sect. 1 (lines M50-51).

- Line 61-63: true AquaCrop requires different input files for each site, which adds to the processing time and effort. However, I disagree with your assertion that it increases model complexity and computational load without providing evidence. One effort to make input and output processing easier is the AquaCrop-GIS created by Lorite et al. (2013). Others have written their own script to handle a huge number of simulations' input and output.
  The new maize run in ACEA took approximately 12 hours for 57 000 combinations of grid cells and scenarios (34-year long each, see Sect. 2.2) on a working station with 12 CPUs. This corresponds to 160000 simulated years per computational hour. Compared to the performance of AquaCrop-GIS (Lorite et al., 2013) with only 6480 simulated years per computational hour, ACEA is up to 25 times faster. So our statement still holds, we also added ACEA's performance information into Sect. 4.2.1.

Method section:

- Line 131-134: Please define the scenarios s1 to s6
  They are already defined in Sect. 2.1.1 (lines M80-83).

Result section:

- You talked about the different regions' relative reductions in WF, yet you didn't even mention the vast red shaded areas in Figure 6. How can you account for the rise in WF in those dark red areas? You listed some countries where the WF has increased on lines 281-283, but you didn't explain why. Please discuss your findings and try to explain why some locations have seen an increase in WF.
  We added the explanation to Sect. 3.2 (lines M299-302). In short, this is due to overall decreasing trend in maize yields and high interannual variability. Note that maize production from these areas is negligible, and hence the impact on global WFs as well.

References

Fader, M., Rost, S., Müller, C., Bondeau, A. and Gerten, D. (2010) Virtual water content of temperate cereals and maize: Present and potential future patterns. Journal of Hydrology In Press, Corrected Proof.

Liu, J., Williams, J.R., Zehnder, A.J.B. and Yang, H. (2007) GEPIC – modelling wheat yield and crop water productivity with high resolution on a global scale. Agricultural Systems 94(2), 478-493.

Liu, J. and Yang, H. (2010) Spatially explicit assessment of global consumptive water uses in cropland: Green and blue water. Journal of Hydrology 384(3-4), 187-197.

Liu, J., Zehnder, A.J.B. and Yang, H. (2009) Global consumptive water use for crop production: The importance of green water and virtual water. Water Resources Research 45(5), n/a-n/a.

Liu, W., Yang, H., Folberth, C., Wang, X., Luo, Q. and Schulin, R. (2016) Global investigation of impacts of PET methods on simulating crop-water relations for maize. Agricultural and Forest Meteorology 221, 164-175.

Rost, S., Gerten, D., Bondeau, A., Lucht, W., Rohwer, J. and Schaphoff, S. (2008) Agricultural green and blue water consumption and its influence on the global water system. Water Resources Research 44(9).

Lorite, I. J., García-Vila, M., Santos, C., Ruiz-Ramos, M., and Fereres, E.: AquaData and AquaGIS: Two computer utilities for temporal and spatial simulations of water-limited yield with AquaCrop, Computers and Electronics in Agriculture, 96, 227–237, https://doi.org/10.1016/j.compag.2013.05.010, 2013.

Response to RC2

Summary

Basically, the authors describe important new model developments for the ACEA model and show novel findings that are of interest for different scientific communities. The results are well described and well-illustrated. In my opinion, paper readability should be increased before publication. Therefore, redundancies should be removed from the text. Units are sometimes separated with a blank and sometimes not. Please be consistent throughout the text.

Thanks for pointing this out. We tried to shorten the text and checked the units.

Different declarations e.g. about spatial resolution are confusing and the downscaling approach that has been applied is not described in the manuscript.

Since we use different resolutions for modelling (30 x 30 arc minutes) and post-processing (5 x 5 arc minutes), the applied procedures might be confusing for a reader. We briefly explain the reasons and methods for this in Sect. 2.2 (simulation setup). For example: "*The 30 x 30 arc minute modelling outputs are distributed among corresponded 5 x 5 arc minute grid cells …*" – meaning that all rainfed/irrigated 5 x 5 arc minute cells within a respective 30 x 30 arc minute cell are assigned the same values. This allows us to have a more precise distribution of maize WFs around the world. We reformulated the description of the methods and procedures in the revised version to make it clearer. Also, we removed the words "upscaling" and "downscaling".

Important model assumptions and approaches are not described and referred to literature instead. Please summarize the most important model assumptions and approaches, so that this paper stands for its own and the reader doesn't have to read multiple other papers to understand the ACEA model.

Our initial rationale was to exclude detailed explanations of the AquaCrop model as our work was mostly devoted to the development of a simulation framework and data processing. Instead, we referred to sources that already include the elaborated and well-illustrated explanation of the AquaCrop model. However, in doing so we might have rushed over some important assumptions that the reader would like to see reflected in our manuscript too. We therefore revised our description of AquaCrop in Sect. 2.1.2 and included a new graph to explain the key processes included in our model.

Generally, I am not sure if one can say that ACEA is a biophysically based mechanistic process model, such as LPJmL or DSSAT. The crop yield calculation is based on AquaCrop-OS, which - to my understanding - is not a very physically based model, since it works with Penman-Monteith and some rough scaling factors.

Indeed, AquaCrop does not have the same level of model complexity in terms of the number and detail of biophysical processes incorporated in the model compared to some other crop models. As AquaCrop's developers aimed to balance simplicity, accuracy, and robustness (Steduto et al., 2009), the model simplifies some processes. Yet AquaCrop does capture the main biophysical processes that are relevant to accurately simulate crop yield response to water (see a short description below). The model is therefore probably best labelled as *a water-driven process-based crop growth model*. We extended our description of AquaCrop in Sect. 2.1.2 to provide the reader with the general overview of key processes involved.

As for the scaling factors, they are only used in the post-processing of AquaCrop outputs (see Sect. 2.1.4). We need them to account for technological developments and external disruptions (e.g. political unrest, floods etc) that cannot be modelled.

Short description of AquaCrop

AquaCrop is a "*water-driven process-based crop growth model*" (Vanuytrecht et al., 2014). As words "*water-driven*" indicate, the model works around the assumption that crop development is mainly determined by water availability. The model's core processes are daily soil-water balance and canopy cover (CC) development which is a subject to agronomic management (e.g. irrigation, mulching, plant characteristics) and various abiotic stresses (see the figure below). On a daily basis, CC is used to convert ET_0 (calculated with Penman-Monteith) into crop transpiration. The latter is then converted into crop biomass via the CO2-adjusted water productivity factor (WP). The final yield is calculated by multiplying accumulated biomass with a stress-adjusted harvest index. As AquaCrop aims to balance simplicity, accuracy, and robustness (Steduto et al., 2009), the model considers only water-related biophysical processes. The other processes such as nutrient cycle or carbon dynamics (as in LPJmL and DSSAT) are not considered.

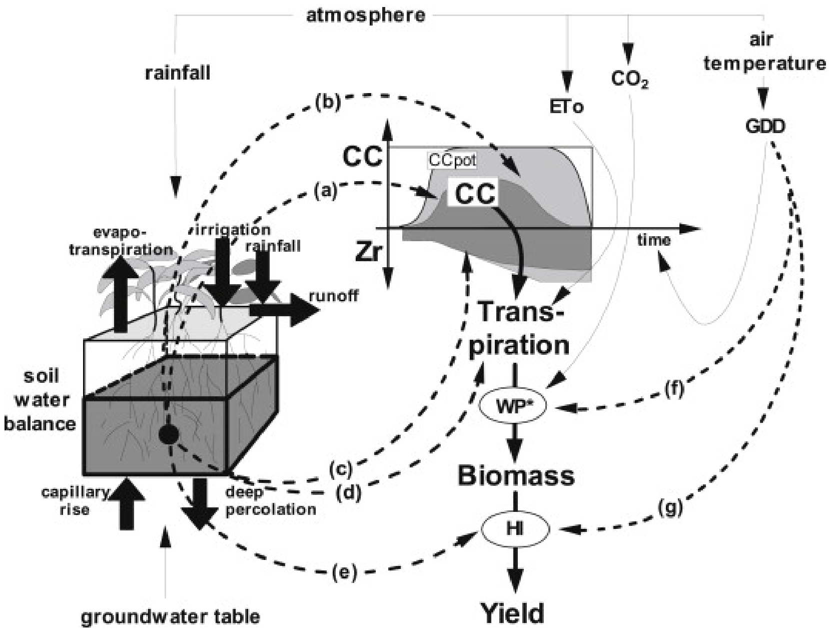


Figure: AquaCrop simulation scheme. Dotted arrows indicate processes affected by water stresses: (a) slows canopy expansion, (b) accelerates canopy senescence, (c) decreases root deepening but only if severe, (d) reduces stomatal opening and transpiration, and (e) affects harvest index; and temperature stresses: cold temperature reduces biomass productivity (f), hot or cold temperature inhibits pollination and reduces HI (g). Source: Vanuytrecht et al. (2014).

Literature:

- Vanuytrecht, E., Raes, D., Steduto, P., Hsiao, T.C., Fereres, E., Heng, L.K., Garcia Vila, M., Mejias Moreno, P., 2014. AquaCrop: FAO's crop water productivity and yield response model. Environmental Modelling & Software 62, 351–360. https://doi.org/10.1016/j.envsoft.2014.08.005
- Steduto, P., Hsiao, T.C., Raes, D. and Fereres, E. (2009), AquaCrop—The FAO Crop Model to Simulate Yield Response to Water: I. Concepts and Underlying Principles. Agron. J., 101: 426-437. https://doi.org/10.2134/agronj2008.0139s

The authors claim several time and in different context that they are the first, which is incorrect every time. I therefore suggest to be a little more modest and careful in the statements, because it could give the impression that you may not be familiar with existing data or literature.

We corrected our statements.

Model results are compared against other models, but the study lacks in a model validation (e.g. of ET), as e.g. done in Kimball et al. (2019): https://doi.org/10.1016/j.agrformet.2019.02.037.

We added to Sect. 4.1.2 the comparison of our maize CWU estimates to other global gridded crop models and several local studies with field measurements.

Overall, I would suggest major revisions for the submitted paper.

Specific comments:

- Line 14: The term 'high agricultural development' same as 'low agricultural development' (Line 16) could be misleading, since it is not clear what 'high development' exactly means. I'd suggest using instead 'highly intensive'.

  We rephrased our wording to high- and low-input agriculture.

- Line 16: Abbreviation CV (coefficient of variation) could be written out at first appearance to make it easier for the reader.

  We removed it from the abstract.

- Line 17: has reduced by 34.6% until which year? 2016?

  We added the year to avoid confusion.

- Line 25: I would be careful with this statement, because the increasing demand is certainly a driver but not the reason for environmental degradation.

  We changed the statement.

- Line 25: I would keep the term 'planetary boundaries' from the reference instead of using 'environmental limits', since this concept is commonly known under the term 'planetary boundaries'.

  We rephrased our wording to planetary boundaries.

- Line 26: There are also large uncertainties and different values exist for the global water consumption of 'crop production'. It would be interesting for the reader and also nice for the introduction (also with respect to your new approach) to describe the range from different approaches (maybe between 70 and 90%). Another question in this context: Do you mean agriculture or exclusively 'crop production' here?

  We only refer here to crop production and, unfortunately, majority of related papers do not report it separately. Therefore, we kept only this number and reference as it is sufficient for supporting the statement.

- Line 51: The coupling of grid cells is only required if it is necessary to consider lateral water flows, what you don't do?

  We do not model the lateral flows as it is not possible in AquaCrop.

- Line 51: Since there is a lot of new literature available for GGCMs, I'd suggest to additionally cite the following publications here to give a broad overview of existing models and latest approaches:
  - Zabel F, Mùller C, Elliott J, et al. Large potential for crop production adaptation depends on available future varieties. Glob Change Biol. 2021;00:1–13. https://doi.org/10.1111/gcb.15649
  - Minoli, S., Müller, C., Elliott, J., Ruane, A. C., Jägermeyr, J., Zabel, F., Dury, M., Folberth, C., François, L., Hank, T., Jacquemin, I., Liu, W., Olin, S., Pugh, T. A. M. (2019): Global response patterns of major rainfed crops to adaptation by maintaining current growing periods and irrigation. - Earth's Future, 7, 12, 1464-1480. https://doi.org/10.1029/2018EF001130

  - Müller, C., Franke, J., Jägermeyr, J., Ruane, A. C., Elliott, J., Moyer, E., Heinke, J., Falloon, P., Folberth, C., Francois, L., Hank, T., Izaurralde, R. C., Jacquemin, I., Liu, W., Olin, S., Pugh, T., Williams, K. E., Zabel, F. (2021): Exploring uncertainties in global crop yield projections in a large ensemble of crop models and CMIP5 and CMIP6 climate scenarios. - Environmental Research Letters, 16, 3, 034040. https://doi.org/10.1088/1748-9326/abd8fc

  We added some references to Sect. 1.

- Line 52: I'd suggest to mention the Global Gridded Crop Model Initiative (GGCMI) within the Agricultural Model Intercomparison and Improvement Project (AgMIP). In the context of climate impact assessments, it would be great to include Jägermeyr et al. (2021), in which the new CMIP6 scenarios are applied to a large ensemble of global gridded crop models. The publication is currently still under review in Nature Food but could be accepted soon.

  We added Jägermeyr et al. (2021) and referred to GGCMI & AgMIP in Sect. 1 (lines M46-48).

- Line 50-54: I disagree that GGCMs have never been used so far to estimate WFs. Maybe that depends on the definition of a GGCM and also of what you mean with WFs. Since a general definition of a GGCM does not exist, this is difficult. In general, a GGCM must not necessarily be physically based. There are a lot of studies that look e.g. at Evapotranspiration (ET) for crop models, e.g. Lui, W. et al. (2016): Global investigation of impacts of PET methods on simulating crop-water relations for maize. Agricultural and Forest Meteorology, 221, 164-175. https://doi.org/10.1016/j.agrformet.2016.02.017.

  We revised our statement to acknowledge that several studies looked into spatial patterns of crop water productivity (hence WFs) but not into historical dynamics.

- In GGCMI, models have simulated ET in phase 1, phase 2 and the latest phase 3. There has been approaches, e.g. by Jägermeyr et al. to investigate water flows, irrigation demands, and crop water productivity with crop models. Deryng et al. e.g. investigated crop water productivity in context with increasing CO2 concentrations:
  - Jaegermeyr, J. et al. Reconciling irrigated food production with environmental flows for Sustainable Development Goals implementation. Commun. 8, 15900 doi: 10.1038/ncomms15900 (2017).
  - Jaegermeyr,J. et al. (2015): Water savings potentials of irrigation systems: global simulation of processes and linkages. HESS, 19, 3073–3091, 2015 . doi:10.5194/hess-19-3073-2015
  - Deryng D. et al. (2016): Regional disparities in the beneficial effects of rising CO2 concentrations on crop water productivity. Nature Climate Change. DOI: 10.1038/NCLIMATE2995

  We added some references to Sect. 1.

- Line 78: I am not sure if one can say that ACEA is a process-based model, such as LPJmL or DSSAT. Crop yield calculations are based on AquaCrop-OS, which - to my understanding - is not a biophysical process based mechanistic crop model.

  Already responded in lines R217-223.

- Line 78: Redundant. The model abbreviation ACEA has already been introduced, so not necessary to do it again. Same with the abbreviation GGCM. Please only write out the complete name at first use and use the abbreviation in the following (without the abstact, in which abbreviations should generally be avoided).

  We switched to abbreviations and made sure they are used consistently.

- Line 79: What means high temporal resolution? In line 92, you say daily. Most GGCMs use daily temporal resolution, but some global gridded crop models run at hourly resolution. Accordingly, what means 'high' in your sentence? I would suggest to delete high and write daily instead. Finally, to reduce redundancy, this should be deleted in line 92.

  We meant daily indeed. We revised the statements.

- Line 86 and Figure 1: Since the 'scenarios' only refer to different water supply systems, I'd suggest to call them 'water supply' or 'water supply scenarios' or 'water supply assumptions' instead of 'scenarios'.

  We changed them to water availability scenarios.

- Line 89: Is fertilizer (N,P,K) a possible input for management in ACEA?

  No, as nutrient cycles are not considered in the current version of AquaCrop. It is mentioned in Sect. 2.1.2.

- Line 91: Why are grid cells iterated, when lateral flows are not considered? You could parallelize the grid cells (as you actually say in line 65).

  There are both options available. The first one is to run grid cells consecutively in a *for* loop. The second one is to run the same loop parallelized so the iterations (or tasks) are distributed among a user-defined number of CPUs. The first one is generally enough for small applications but for large scales, as in this paper, the parallelized option is preferred.

- Line 92: How is crop growth simulated? What approaches are used? I think it is required to describe the main approaches and processes of the model (e.g. how is atmospheric CO2 concentration considered?). The reader has to understand the most important underlying approaches without having to read the other Aquacrop publications!

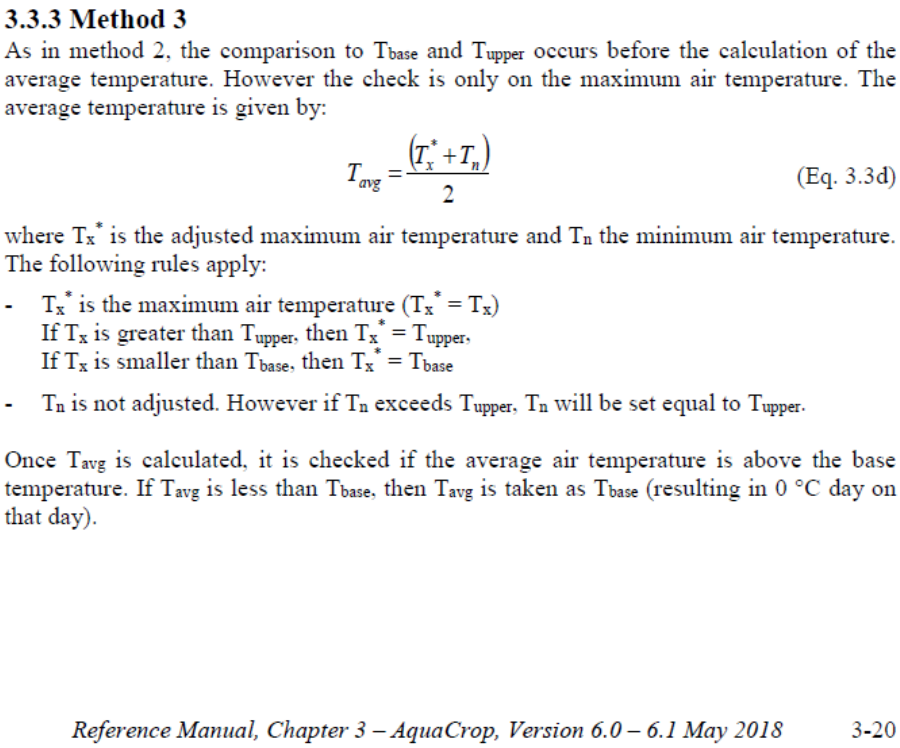
  We added a more detailed description of AquaCrop in Sect. 2.1.2.

- Line 94: Redundant, parallelization is already mentioned in line 65.

  We changed this statement.

- Line 107: Since there are many different GDD approaches available, which one has been implemented to the model?

  AquaCrop provides three methods to calculate GDDs, so it is user-defined in ACEA as well. For our simulation, we chose the default AquaCrop method - Method 3 (see the excerpt from the AquaCrop manual below):



3.3.3 Method 3

As in method 2, the comparison to T_{base} and T_{upper} occurs before the calculation of the average temperature. However the check is only on the maximum air temperature. The average temperature is given by:

$$T_{avg} = \frac{(T_x^* + T_n)}{2} \qquad \text{(Eq. 3.3d)}$$

where T_x^* is the adjusted maximum air temperature and T_n the minimum air temperature. The following rules apply:

- T_x^* is the maximum air temperature ($T_x^* = T_x$)
  If T_x is greater than T_{upper}, then $T_x^* = T_{upper}$,
  If T_x is smaller than T_{base}, then $T_x^* = T_{base}$
- T_n is not adjusted. However if T_n exceeds T_{upper}, T_n will be set equal to T_{upper}.

Once T_{avg} is calculated, it is checked if the average air temperature is above the base temperature. If T_{avg} is less than T_{base}, then T_{avg} is taken as T_{base} (resulting in 0 °C day on that day).

Reference Manual, Chapter 3 – AquaCrop, Version 6.0 – 6.1 May 2018 3-20

Figure: Excerpt from D. Raes, P. Steduto, T. C. Hsiao, and E. Fereres, 'AquaCrop Version 6.0 - 6.1: Reference manual (Annexes)', Rome, 2018.

We added a sentence on it in Sect. 2.2 (line M206).

- Line 150: Is this correction factor, derived by a bias correction of yields, used also to scale evapotranspiration? This is not explained here. But if yes, is a linear relationship between yield and ET realistic?
  Already responded in lines R60-120.
- Line 159: The assumption that maize harvested areas experienced the same dynamics as croplands seems arbitrary to me and must lead to large regional errors. Is there any evidence that maize areas behave similar than total cropland areas? The term 'extrapolation' in this context seems wrong as it seems to be a scaling. To me, the procedure is not yet clear. Does the irrigation fraction in each pixel remain constant in your scaling approach? If not, can you explain where the change in irrigation fraction comes from? If all values are scaled with FAOSTAT in the end, why not directly scaling MIRCA2000 with FAOSTAT trends for maize for each country?
  First, we would like to note that FAOSTAT does not differentiate between rainfed and irrigated production systems. Therefore, if we scale MIRCA2000 (**now SPAM2010 instead**) directly to national FAOSTAT data, the irrigated areas would have the same % change as rainfed ones in each grid cell within a country. However, the literature suggests that many countries substantially changed the fractions of irrigated areas within their territories since the 1980s (e.g. in India, former USSR countries, China, Brazil) (Ambika et al., 2016; Nagaraj et al., 2021; Siebert et al., 2015).

  To address this issue, we add one more step before scaling MIRCA2000 to FAOSTAT: we project MIRCA2000 data – which reports gridded maize irrigated and rainfed harvested area separately – into the past (till 1986) and future (till 2016) by assuming that harvested areas generally followed the same trends as irrigated and rainfed croplands did (see Sect. S1.8). This allows to account for historical change in the fraction of irrigated maize in each grid cell, but of course, it may lead to regional errors as maize is only one of the crops considered in cropland dynamics. However, regional errors would be also present if we just scale MIRCA2000 to FAOSTAT, and thus both approaches would lead to inevitable uncertainties.

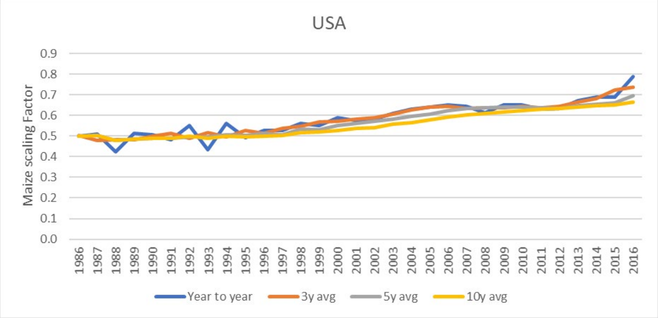
This extrapolation approach was encouraged by one of the creators of MIRCA2000 dataset with whom we had a preliminary online discussion before switching to SPAM2010 (Yu et al., 2020). The latter was favoured over MIRCA2000 as it provides harvested areas around 2010 which is closer to the main period of interest in our study (2012-2016). Thus, the uncertainties in the revised harvested area values (hence WFs) around 2010 should be lower.

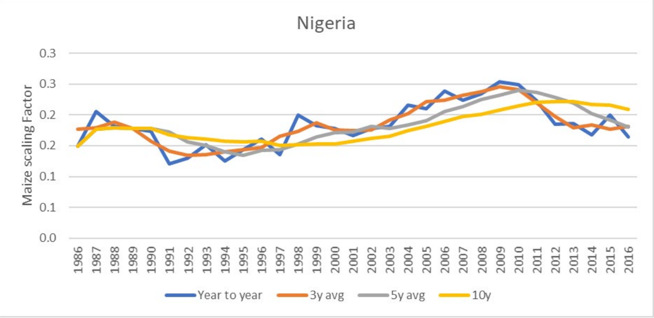
Literature:

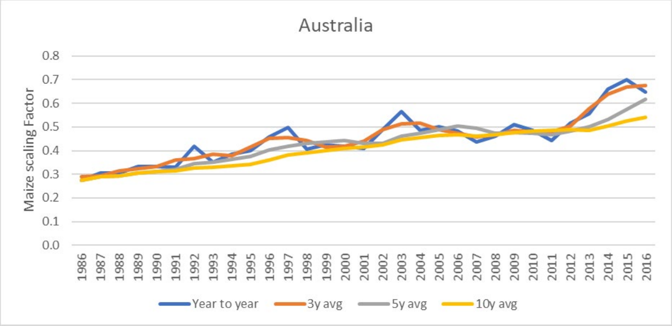
- Ambika, A. K., Wardlow, B., and Mishra, V.: Remotely sensed high resolution irrigated area mapping in India for 2000 to 2015, Sci Data, 3, 160118, https://doi.org/10.1038/sdata.2016.118, 2016.
- Nagaraj, D., Proust, E., Todeschini, A., Rulli, M. C., and D'Odorico, P.: A new dataset of global irrigation areas from 2001 to 2015, Advances in Water Resources, 152, 103910, https://doi.org/10.1016/j.advwatres.2021.103910, 2021.
- Siebert, S., Kummu, M., Porkka, M., Döll, P., Ramankutty, N., and Scanlon, B. R.: A global data set of the extent of irrigated land from 1900 to 2005, Hydrol. Earth Syst. Sci., 19, 1521–1545, https://doi.org/10.5194/hess-19-1521-2015, 2015.
- Yu, Q., You, L., Wood-Sichra, U., Ru, Y., Joglekar, A. K. B., Fritz, S., Xiong, W., Lu, M., Wu, W., and Yang, P.: A cultivated planet in 2010 – Part 2: The global gridded agricultural-production maps, Earth Syst. Sci. Data, 12, 3545–3572, https://doi.org/10.5194/essd-12-3545-2020, 2020.

- Line 165: Why do you use a three year moving average and not 5 or 10-year? Have you made a sensitivity analysis or is there any assumption that gives arguments for taking a 3-year average?

  Yes, we carried out a sensitivity analysis with 3-, 5-, and 10-year moving averages for four countries that represent a range of annual maize production quantities and environmental conditions: USA, Nigeria, Australia, and Algeria (see the graphs below). Among those three moving average options, the 3-year one resulted in the most favourable representation of interannual variability and trends.







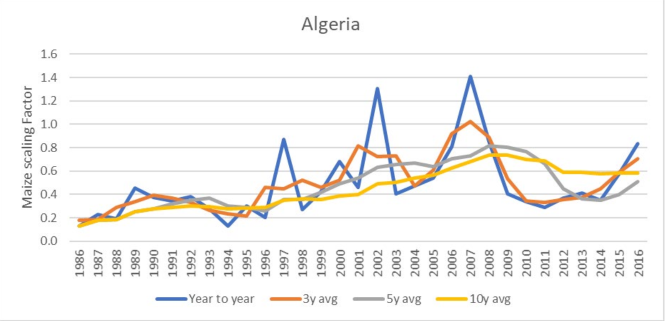


Figure: Sensitivity analysis of moving averages of yield scaling factors for maize in USA, Nigeria, Australia, and Algeria (blue - no moving average, orange – 3-year, grey – 5-year, and yellow – 10-year moving average).

- Line 178: Now I am confused. In line 13 (abstract) and in line 78, you said that you allied the model at 5 x 5 arc minutes. Now you say, you run ACEA at 30x30 arc minutes. What is correct?

  Most of input data for crop modelling are obtained at 30 x 30 arc minute resolution. Therefore, running ACEA at 5 x 5 arc minutes instead of 30 x 30 arc minutes would make almost no difference to the final results (except for the cells with shallow groundwater levels, see lines 200-204). However, the time spent for running the simulation would differ significantly. Therefore, we decided to run ACEA at a coarser resolution and then distribute the outputs to respective 5 x 5 arc minute grid cells from SPAM2010. The following output post-processing (WF calculation, yield scaling etc.) is then performed at 5 x 5 arc minute grid cells as well. We added a similar explanation to Sect. 2.2 (lines M192-195).

- Line 179: Please be aware that MIRCA2000 harvested areas for the maize class includes maize (corn), maize for forage and silage, and pop corn. How do you deal with the different maize usages that also go along with different plant characteristics and harvest characteristics (e.g. for silage, the complete overground biomass is harvested)?

  We considered all maize types (under the maize class in MIRCA2000) as one. The reason for this is a lack of input data to simulate them separately. We added a similar explanation to Sect. 2.2 (line M189-190).

- Line 180: When you consider harvested areas according to MIRCA2000, you implicitly consider multiple growing seasons that are included in the harvested area (if a physical area of 1 ha is harvested twice per year, the harvested area is 2 ha).

  We indeed simulate only the main (one) growing season if the subsequent growing seasons (i.e. sub-crops in MIRCA2000) are minor. In the case of maize, sub-crop 2 area / sub-crop 1 area = 0.53%, so the simulation of the second growing season is not relevant to represent the global trends. We added a similar explanation to Sect. 2.2 (line M191).

- Line 185: Is the GSWP3-W5E5 data is based on bias-corrected reanalysis data? If yes, that would be important to mention here.

  We added this information to Sect. 2.2 (line M198).

- Line 190: The same methodology is also used within GGCMI, you could refer to Minoli et al. (2019):
  - Minoli, S., Müller, C., Elliott, J., Ruane, A. C., Jägermeyr, J., Zabel, F., Dury, M., Folberth, C., François, L., Hank, T., Jacquemin, I., Liu, W., Olin, S., Pugh, T. A. M. (2019): Global response patterns of major rainfed crops to adaptation by maintaining current growing periods and irrigation. - Earth's Future, 7, 12, 1464-1480. https://doi.org/10.1029/2018EF001130

  We added this reference.

- Line 205: I know studies that assume irrigation to be triggered below 70% of field capacity. There seems to be some a range of values in the literature that could be discussed as another source of uncertainty.

  We also saw a 70 % threshold in some studies, but 50 % is the most common value in literature. This is indeed a source of uncertainty as we mention in line 403.

- Line 207: To be clear: You always assume full irrigation and don’t consider e.g. deficit irrigation, right?

Yes, correct, we only consider full irrigation since it is the most common approach and there is a lack of global data on irrigation strategies.

- Line 210: How is the downscaling applied? If you downscale the results to 5x5 arc minutes, you cannot say that the model is applied at 5x5 arc minutes (see e.g. abstract).
  Already responded in lines R424-430. We changed our wording in the abstract.

- Figure 3: For the right panel of Fig. 3, I would suggest to use an area weighted mean to consider the different maize areas (e.g. the US corn belt should weigh more than small areas), instead of using the median of all data points along the latitude. Is the color bar logarithmically scaled? Please explain in figure caption. Additionally, I think the 10thpercentile is the correct formulation (10% percentile would be doubled).
  The colour bar was scaled to optimise the colour distribution, so it is not logarithmic. We show median to represent the influence of climatic conditions on WFs and related variables. If we take the weighted mean, the climatic signal will be attenuated and showing percentile line over it would be misleading. We changed the 10 % percentile to 10th percentile.

- The Table 2 is good to have and helpful.
  We are glad that you like it.

- Line 293: There seems to be high uncertainties about global maize areas and expansion.
  We discuss this in Sect. 4.2.2.

- Figure 7 and 10: For improving this figure, one could set the dot size relative to the maize area or maize production in the country to visualize the importance of the respective country.
  The main point of those graphs is to show the number of countries around -30 % and + 30% dotted lines, so scaling the dots to production will look too messy.

- Line 343: Please be aware that the applied crop calendar also includes high uncertainties, and regions have been identified that do not well represent local phenological data from observations. A new updated crop calendar is currently being processed in ISIMIP.
  We added this limitation to Sect. 4.2.2.

- Line 346: Can you explain why it is less accurate to calculate green and blue CWU in the post-processing? Isn't that a question on how it is implemented?
  As explained by Hoekstra (2019), there are two main approaches to calculate green and blue CWU in post-processing. The first one is to assume that blue CWU is equal to the difference between irrigated and rainfed CWU. This approach is problematic since the rooting depths and soil moisture dynamics are different if irrigation is applied. The second one is to estimate blue CWU based on the relative addition of water to the soil via irrigation and rainfall. This approach is better, but the problem here is the lack of tracing of green and blue water within the soil profile, which leads to the same composition of soil moisture at different soil depths. Both approaches result in a less precise estimation of green and blue water losses through evaporation and transpiration, which leads to a less accurate estimation of CWU. Moreover, it is not known how much green and blue water is entered or lost the soil profile during the fallow period. Hence, the composition of water in the soil profile at the start of the next growing season is not known either. This may increase the uncertainty in green and blue CWU estimates of each subsequent growing season.

  - Hoekstra, A. Y.: Green-blue water accounting in a soil water balance, Advances in Water Resources, 129, 112–117, https://doi.org/10.1016/j.advwatres.2019.05.012, 2019.

- Line 382: I would be careful with this statement. The fact that you don't know any study that has shown this doesn't mean that no other GGCM can do this, as most existing GGCMs have the ability to simulate that on a daily base.

  We revised this statement.

- Line 394: Again, it is not explained, which downscaling methodology is applied. If this is just interpolated, I wouldn't call it a 'downscaling' approach.

  Already responded in lines R424-430. We changed our wording to avoid the word "downscale" .

- Line 396: Actually all data except the crop calendar are available at 5x5 arc minutes. For climate input, one could use e.g. WFDE5, HWSD soil data is also available at 0.00833° spatial resolution.

  We are not aware of the 5 x 5 arc minute version of WFDE5, we are only able to find the 30 x 30 arc minute version online.

- Line 398: Indeed, this is a strong limitation, since cultivar variations and improvements over time play a big role, especially for maize.

  As we already explained before (see lines 56-116), the historical changes in maize cultivars mainly lead to higher crop yields while CWU stays approximately the same. We account for the increase in maize yields by applying the scaling factors, so the improvement in maize varieties is indirectly considered. Consequently, this limitation has only a minor impact on global WF trends. However, the impact at regional scales might be more pronounced.

- Line 410: Please delete this statement. An extrapolation of historical maize areas based on FAO trends has been performed e.g. by Iizumi, T., Sakai, T. The global dataset of historical yields for major crops 1981–2016. Sci Data 7, 97 (2020). https://doi.org/10.1038/s41597-020-0433-7.

  We removed this statement.

- Line 423: What means high spatial and temporal resolution? Please avoid subjective statements such as 'good', 'big' or 'high' in scientific articles.

  We revised this statement.

- Line 423: To me, the conclusion mainly reads like a summary and can be shortened.

  We rewrote and shortened the conclusion (Sect 5).

Response to RC3

Summary:

This study introduces a new GGCM the ACEA which enables long-term global crop water footprint simulations with a case for maize over 1986-2016. The innovative aspect is shown in the separation between blue WF from irrigation and from shallow groundwater, rather than the historical trends simulation, given that there are already global studies available in recent two years (e.g. Chiarelli et al., 2020, https://doi.org/10.1038/s41597-020-00612-0).

Time-explicit separation of green and blue crop WFs is indeed one of the main novel aspects of our study. However, we would like to emphasise the importance of historical coverage in our paper as we provide continuous timeseries of maize WFs. On the contrary, most of the available literature covers only some specific years (e.g. Chiarelli et al. (2020) only cover the period 1998-2002 and 2016) and/or provides less sophisticated estimations of green/blue CWU that exclude some crucial aspects such as the contribution of shallow groundwater, regional differences in crop phenology, and trends in harvested areas and crop yields. Our study tries to overcome these limitations, and thus provides a more comprehensive overview of global maize WFs.

Besides, there are some certain improvements can be made in the revision:

1. The authors mentioned many times the "accurate estimation". But there is not enough calibration or validation processes, especially for the ET simulations. It can be easily done by comparing the global remote sensing images. At least for some selected regions, to show the accuracy of the ET results (Gao et al., 2021, https://doi.org/10.1016/j.agwat.2021.107014).
   We added to Sect. 4.1.2 the comparison of our maize CWU estimates to other global gridded crop models and several local studies with field measurements. In short, ACEA's estimates align well with other crop models. When compared to local studies, the average differences vary between -9.4 % to +14.8 %, which is minor considering the uncertainties involved in global simulations (Sect. 4.2.2). Therefore, we consider our maize CWU values to be fairly reasonable.

2. Fig. 1, the CO2 concentration should belong to the Environmental inputs, right?
   We moved CO_2 to the "environmental inputs" block.

3. In the section 2.1.3, it is highly recommended to add the details on how to separate the two components in blue WF, given it is the key innovative point.
   We rephrased our description of the green-blue tracing in Sect. 2.1.2 and added a new graph.

4. Maybe I was wrong, I am very confused on the second equation in Eq. (4) and (6), how you can just use weight of area to multiply the unit WF to get the so-called average unit WF? Is it the right way of weighted average? Please carefully check.
   Let us first clarify the purpose of the mentioned equations. Eq. 6 is used to estimate the national scaling factors (see Eq. 5) that are used to adjust the simulated crop yields (see Eq. 3). Eq.4 is used to estimate rainfed and irrigated crop WFs in each grid cell. None of those equations has weighting by area in it.
   Now regarding the weighting. When estimating average values such as unit WF, CWU, or yields over multiple grid cells (e.g. country, region), we give more value to the cells with a larger production or larger harvested areas. We acknowledge that it might be difficult for a reader to understand the data processing procedures in ACEA, but we want to assure that the weighting is done properly and in accordance with the previous studies such as Mekonnen and Hoekstra (2011):

- Mekonnen, M. M. and Hoekstra, A. Y.: The green, blue and grey water footprint of crops and derived crop products, Hydrol. Earth Syst. Sci., 15, 1577–1600, https://doi.org/10.5194/hess-15-1577-2011, 2011.

5. Line 283. What is the reason of the increased WF?
   Already responded in lines R169-171.