Dear Editor and Reviewers,

The authors are grateful to you for taking time to handle and review our manuscript. We have received comments from two anonymous referees and Dr. Yaling Liu which were quite helpful to improve our paper. We have addressed all the comments and concerns as shown in the response to each, and thoroughly revised the manuscript accordingly. We believe our paper now comes up to the standard of possible publication in Hydrology and Earth System Sciences. Please note that the responses below are identical to the Author Comment posted on the web site (http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-119/#discussion).

Kind regards,
Naota Hanasaki
(on behalf of authors)

Response to Reviewer 1

The manuscript describes a new routine for incorporating seawater desalination dynamics in a global hydrologic model in this case the H08 model. The work is certainly an advancement over the current of the start of the art and how GHMs currently handle seawater desalination. So the work is of interest to the readership and would likely help advance the representations of desalination in GHMs. However, there are several major concerns especially with the proposed Seawater Desalination Model structure and the appropriateness of the validation exercise. I recommend major revisions.

> Thank you very much for taking time to review and providing detailed and insightful comments to our work.
> Since we have received your comments, in order to fully respond to your questions and recommendations, we have collected additional data and evidences, improved models and algorithms, revisit the thresholds, and others. Owing to your comments, the model and paper have been largely improved. Although improved, the Seawater Desalination Model (SDM) is still not a perfect model. In this paper, we fully disclosed not only the strengths and detailed methods but also the limitations and shortcomings of SDM for possible future improvements by readers. We firmly believe that this study will be a first step to formulate desalination in global hydrological model.

Major concerns:
- The criteria for selecting desal plants (e.g, 613 from the 17,000 available in Desal-Data), the amendment of records as necessary such as in mountainous areas, and the selection of thresholds for the proposed conditions (e.g., 0.1 AI, 165km) seem ad hoc and subjective, and irreproducible by others in case of the adjustments to the data.

> Thank you. First, we added detailed rationale for selection of desalination plants in lines 15-36 of Supplemental Material. Second, about the amendments or records, now we added a list of desalination plants excluded from our analyses in Table S2 with reasons. Next, about thresholds, we agree with you that the thresholds in our model are dependent on the dataset used (e.g. Desal Data or others) and spatial resolution (e.g. 0.5°×0.5°grid cells or finer). We afraid, however, such dependency is common with all hydrological modeling (i.e. hydrological parameters are dependent on the selection of input meteorological data and spatial resolution). The readers who wish to reproduce our work would first compute AUSD with the threshold shown in our report, then adjust thresholds if necessary by comparing it with the actual distribution of desalination plants and volume of water production. These

procedures are now added lines 167-168.

Also why was the model calibrated/tested for two countries and not all the countries for which desal exists?

First, we validated our model for nine countries (see Table 1 and lines 204-209), not two countries. These nine countries produces 85% of seawater desalination in the world in 2005. We further elaborated these two countries in text because detailed municipality-specific statistics were obtained (Table 2). We tried to find similar statistics for other countries, but it was not successful. Note that collecting data for water production and use is quite difficult partly due to language: we have been searching documents written in English, not in local languages. These limitations are now added in Discussion (lines 435-439).

Also why not use procedures such as decision trees or regression decision trees, or the like to establish those thresholds based on the actual data as opposed to the guessing of thresholds?

Thank you for this suggestion. Following your advice, first we estimated the thresholds in SDM by the decision tree method. The thresholds obtained didn't reproduced the reality because they were erroneously influenced by data outliers. To avoid this problem, we also tested the random forest method. The results were improved, but due to considerable difference among the iteration runs, we could not determine a single value for each threshold. Finally, we adopted the segmented regression method to estimate the threshold of aridity index (AI) which was determined by visual inspection in the discussion paper. We plotted the relationship between the AI of plants located and their capacity. It shows remarkable difference in the distribution of plots by AI. We applied the segmented regression method and statistically estimated the break point at 0.075 and set as the new threshold (original number shown in the discussion paper was 0.1). We have re-calculated all the numbers using this new threshold and revised throughout the text. The revised methods are described in lines 214-217.

- The validation of the SDM is inadequate. First, capturing the spatial distribution over a snapshot in time is an important element, but equally important is the ability of the model to simulate the evolution over time especially since the study considers desalwater withdrawal over time as a central focus of the study.

Thank you for this comment. It was not possible to conduct a retrospective simulation because of lack of key input data (i.e. GDP per capita and water withdrawal for industrial and municipal purposes). Therefore, we added a new figure (Figure 5) and text (lines 281-294) showing the country-base historical variations in desalination capacity and water withdrawal. The results showed that the temporal change in domestic and industrial water withdrawal in AUSD corresponds fairly well with that of the total capacity of seawater desalination, which promises the temporal evolution in simulation.

Second, the modeling approach is employing 3 conditions and assumptions as discussed in the paper to generate a gridded dependency map of areas dependent on desal plants globally with the goal to compare them to an observational dataset (DesalData) in this case. However, the only serious validation was a visual comparison between the results of Figure 4 and the desal plants location in two countries, Saudi Arabia and UAE. This visual comparison of the results (not even on the same figure and limited to two countries when the intent is to apply globally) is not sufficient to justify the appropriateness of the model.

Thank you very much for this comment. Actually, SDM was validated from three perspectives: geographical extent of AUSD (Figure 4, lines 248-268), total desalination water production in 2005 (Table 1 and Figure S2, lines 269-280), and temporal change in production (Figure 5; newly added in revisied manuscript; lines 281-294). In order to make this structure clearer, now we added a summary of validation in lines 295-300.

Also in figure 4, for example, the simulated areas/grids of area utilizing seawater desalination are widespread over much larger areal extent than the distribution of where the cities are located. For example, many of the inland grids between some of the major cities in the west of Saudi Arabia hardly have any population presence and a population based estimate might be more appropriate. Obviously many of these grids exhibit minimal water demands and where the major cities lie is what matters, but again this highlights the inadequacy of using figure 4 as the validation of the model.

Thank you for this comment and sorry if our results confused you. As you pointed out, the areal extent of AUSD and the locations of individual desalination plants are not directly comparable. AUSD is expected to include major desalination plants, but it is not necessarily true that all of the AUSD grid cells contain major desalination plants. The definition and interpretation of AUSD is thoroughly revised and shown in lines 148-157. Additionally, we revised the whole text not to confuse readers.

Also the comparison of country scale results is not sufficient to justify how the desal water withdrawals are distributed within a country. I would recommend expanding the validation exercise to global scale. For example, a scatter plot of total desal water use at the country scale for multiple time periods over history would help fill in this gap. Extending Figure 1 to show the global map and comparing that to Figure 5a would also be useful to show if the areas popping out as desal dependent over history are consistent with where the desal plants are located around the globe.

> Thank you for an excellent idea. Now we added two new panels to Figure 6 (Figure 5 in the original discussion paper), showing the location of major desalination plants globally in 2005 and 2014 (latest data accessible). It shows, for instance, AUSD in 2055 expands into coastal Chile and southwestern Africa, where some major desalination plants actually exist . In terms of temporal dynamics, we newly added Figure 5 showing the scatter plot for multiple time periods over history.

- The first key question in the paper of looking at figuring out the common climatic and socioeconomic conditions associated with usage of seawater desalination around the global is a very interesting and important question, but as discussed in the previous two comments, the authors did not approach this in a systematic way and barely scratched off the surface in addressing this question. The paper is really mainly focused on the second question outlined by the authors.

> Thank you. As you pointed our research question was a bit too broad in contrast to what we obtained so far and data availability. Now we have rephrased the first question to make consistent with what we have actually done, which is not discovering “the common condition”, but investigating some characteristics in present desalination plants (lines 69-72).

- Why limit the analysis to large and seawater plants only? Also in many regions the use reverse osmosis and brackish water is more prevalent and in some cases many of the plants are actually inlands like in the US. Expanding to take a more holistic look at all of desal water use would make the paper a lot more useful to the scientific community.

> Thank you. To develop a general model on desalination plant would be very useful for larger community, but highly challenging. Particularly, inland desalination is of a matter of not only available water quantity but required water quality which is difficult to simulate with global models. As we stated in Introduction (lines 67-75), in this study, we focused on the role of seawater desalination that sustains local water usage.

Minor concerns

Line 15: change ‘likely’ to ‘projected’

Line 16: I would omit the term ‘modern’ throughout the manuscript when describing GHMs

Line 18: specify the historical period

Line 26: I would suggest to add a sentence or two at the end of the abstract to state the implications of those estimates (or provide a range of the fraction of local GDPs in countries that depend on desal or maybe translate the estimates to total energy instead of cost and compare to total energy consumption)

Thank you. We corrected all.

Line 80: 46.9 km3 sounds extremely large. Are you using the correct unit? It is confusing that units are km3/yr in the text and million m3/day in the tables. I would use consistently.

Thank you. As of June 2014, DesalData included records for 17423 desalination plants globally. The number 46.9km^3 was the simple summation of capacity of all the plants recorded. We noticed that the number includes plants in “offline” and “planned” status. In the revised text, we calculated the total capacity of online plants only which amounts 21.9 km^3.

Line 84: it is not clear to why only Major Plants.

Thank you. We added detailed rationale of plant selection criteria in Supplemental Text (lines 15-36).

Line 85: ’29.3’ is the production or capacity? Also it would make the sentence more informative to include the values for the other categories as well.

Thank you for pointing out this. For the same reason stated above, now we only summed up the plants in “online” status. Now it reads 13.3 km^3 yr^{-1}. Since general seawater desalination information is available elsewhere (e.g. Voutchkov, 2012), in order to keep our text concise, we are afraid but we didn’t include numbers for other categories.

Line 91: ‘erroneous’ how were these circumstances adjusted. Clearly explain (maybe in SM) to ensure reproducibility of the work also who many data points were adjusted?

Thank you. We listed eleven plants excluded with reasons in Table S2.

Line 105: add ‘for each grid’ before globally.

Thank you, we added.

Line 110; I suspect there were both downscaled and bias-corrected. I would add a sentence to state so with a reference for completeness.

Thank you. We mentioned that the methods of downscaling and bias correction are identical to those introduced in Hanasaki et al. (2013a,b)

Line 127: to avoid having to send the readers to previous publications, I would suggest to consider reproducing these figures/table in the supplemental materials. Even better would be to tabulate the results at the country scale for the historical and future scenarios and placing them in an Excel file as a supplementary document.

Thank you for this suggestion. Perhaps most important information for the readers is not figures but the assumptions of future scenarios, which is only available in the original references. Because the references are both published in an open access journal, it doesn’t take much time to view them, we believe.

Line 182: what does it mean to have 110

Thank you. As shown in the next sentence, it is due to inconsistency in data. (line 209).

Line 213: why use 40-80 when the data says 28-77? How was that approximation done?

Thank you. We agree with you. The lower boundary of 40% is not justified from the data. Based on the minimum value appeared in Table 2, we modified the threshold as 30-80%.

Line 269: I would show these numbers as fraction of regional or global GDPs. I would suggest doing this at the country scale at the least and giving a range.

Thank you. We added regional numbers as fraction of regional GDP in Table S4. Taking into account the number of assumptions and reproducibility of the past, we believe that the model is not in the stage for country-base assessment.

Line 299: less than triple

Thank you. We corrected it and now it reads “more than double”.

Line 302: the increase of population from 14.5 to 184.3 million sounds really large! Is this due to the assumption of static population distribution within a country?

Thank you. This is mainly due to expansion of AUSD in North Africa (for all scenarios) and South Asia (only for SSP1; see Figure 6).Particularly, when densely populated Egypt and Pakistan becomes AUSD, the population living in AUSD showed a rapid increase (line 327-332).

Table 1: what does other mean? I think adding a footnote to explain this sector and the value of 35 in Spain would be useful

Thank you for this comment. We added a footnote and noticed that this is used for irrigation.

Table 3: why 2055 in particular?

Thank you. We have added numbers for 2005 and 2025, because we intensively analyzed these periods as well.

Table 4: several comments

- Replace ‘historical’ with ‘historical (2005)’

Because some earlier studies didn’t report the year of 2005, we kept as is.

- I would suggest using consistent units with those in the text (km3/yr).

Thank you. We used the unit of 10^6 m^3 yr^{-1}

- I would suggest adding or even replacing the last column on the right because it is hard to draw any conclusion from the total production cost. One option is to compute the range of the percent of GDP at the country scale to highlight that this maybe a substantial value in countries like Qatar or UAE for example. Another option is to look at the total energy associated with the production of that much desal and then compare to the total primary consumption in that country (in a way this would isolate the GDP portion not related to the energy sector). But the latter will like take some additional work so it may suffice to do the first option.

> Thank you very much for this suggestion. We added percent GDP of seawater desalination cost of the world in Table 4. The same indicator are now reported in Table S4 for eleven regions. Again, since too many assumptions and uncertainties were included in the model and settings, country-base results were not shown in the revised text.

- Also consider adding the estimates from the study of Kim et al (2016). Balancing global water availability and use at basin scale in an integrated assessment model. Climatic Change, DOI 10.1007/s10584-016-1604-6

> Thank you for letting us know the work of Kim et al. (2016). We mentioned this interesting work in Introduction. Since quite limited quantitative results were provided (only the total desalination production in 2100), we couldn't include their results in Table 4.

Figure 1: I would suggest adding 3rd figure at the bottom showing the whole globe and maybe showing all the plants excluded from the analysis in gray color. Also is color scale for capacity of production?

> We displayed the present location of major desalination plants in Figure 6. The color scale of Figure 1 shows the capacity of production.

Figure 2: this is solely based on two countries and I would imagine that it should vary based on the size of the country and the distance from saline water bodies should matter in defining this threshold.

> About the first point, actually Figure 3 is based on the data for nine Major Countries (please see Figure 1, red cells extends in many countries). Hence we believe that the results are not dependent on the national characteristics of Saudi Arabia and UAE. We added this point in line 218-219.

Figure 6: I would suggest omitting this figure and adding the results to table 4.

Thank you for suggestion. We have tried this, but it made Table 4 very complex. We kept

Figure 6 (Figure 7 in revised manuscript) as is.

Response to Reviewer 2

The paper presented by Hanasaki et al. developed a new modeling scheme for seawater desalination water use that can be applicable to existing global hydrological models. The newly developed desalinated water use scheme was then applied to project future desalinated water use under different socioeconomic (SSPs) and climate change (RCPs) scenario. It was found that future desalinated water use is expected to increase substantially (about 2-15 times), however, the future estimates vary significantly depending on different socioeconomic pathways. To my knowledge, this study provides most comprehensive results for historical and future desalinated water use estimates and projections across the globe. The author used the latest and very comprehensive data source, DesalData (http://desaldata.com/), and incorporated them into a global hydrological model. Existing global or large-scale studies on desalinated water usetypically use the data from FAO AQUASTAT, WRI EarthTrend, EuroStat, and available country statistics, which often have very limited global coverage. The authors combined the desalination data with other socioeconomic data (GDP, population, production cost, etc) to construct the Seawater Desalination Model (SDM). The paper is topical and presents interesting and useful findings, and it is concise and mostly wellwritten. The newly developed SDM is useful for large-scale modeling framework, and appears to be quite applicable to other GHMs. However, I do have some comments regarding the methodologies that were applied in SDM as detailed in the following.

Thank you very much for taking time to review and providing insightful comments. All of the concerns you raised have now addressed as below.

1. The methodologies described in Section 2.4 is the key part of this study. The section is concise, however, it currently lacks rationales for those conditions and assumptions (A-C) made in the SDM. The methodologies appear to be quite arbitrary at its present form. Since this study focuses on developing a new desalinated water use model, these key parameters need to be described more thoroughly. I urge to expand Section 2.4 and provided further explanations of each key parameter, condition, and assumptions that have been incorporated in the SDM. These information are very useful for other large-scale hydrological modelers. Without further explanations, the novelty of this study is very limited. In addition, I suggest to combine Section 3.1 into Section 2.4. Section 3.1 is basically the method and background information that derived the key parameters.

Thank you for this comment. Now Section 2.4 is further elaborated. First, taking the comments and questions of Reviewers 1 and 2, we have elaborated the concept of SDM and parameters (lines 148-157). In the original discussion paper, the parameters (thresholds in Conditions A-C and Assumptions A-C) are suddenly appeared without sufficient explanation. Now it is clearly described how the parameters were derived (lines 165-171 and 177-186). Because many reviewers and readers like to distinguish methods and the results of data analyses, we kept the subsections 2.4 and 3.1 as is, but we further edited for readability.

2. Some assumptions made for the SDM are not entirely reasonable. For example, the cost of desalinated water use is decreasing and the efficiency of desalinated water use is improving in recent years. For future projections (towards 100 years later), further technological improvement is expected to reduce the cost drastically. The assumption made based on a historical trend may not be applicable for the future, e.g. desalinated water use for irrigation. In addition, fossil groundwater reserves are actively used in the Middle East and Northern Africa (MENA) despite the near zero natural recharge. The authors may include fossil groundwater use estimates to isolate the impact on desalinated water use. I would suggest to at least discuss these uncertainties further.

Thank you. We completely agree with you that technology advances and subsequent cost reductions will enhance the introduction of desalination plants. We noted that this mechanism is not included in SDM in lines 428-432. Note that importance of this mechanism and possible distortion of simulation results have been already discussed elsewhere (lines 386-390). Also, this mechanism have been partly analyzed in the sensitivity test of changing the threshold of Condition A (minimum per capita GDP; Figure 7): the decrease in the threshold corresponds to diminish in the relative cost of desalination. We also fully notice the importance of alternative water sources. As you pointed out, the production of desalination is largely determined by the availability of renewable/fossil groundwater. This could be partly achievable if we combine SDM with the global hydrological model that explicitly simulate renewable/fossil groundwater. We added discussion in lines 432-434.

3. The uncertainty inherent in future water use estimates needs further discussion with some quantitative information (e.g., Wada et al., 2016).

Wada, Y., Flörke, M., Hanasaki, N., Eisner, S., Fischer, G., Tramberend, S., Satoh, Y., van Vliet, M. T. H., Yillia, P., Ringler, C., Burek, P., and Wiberg, D.: Modeling global water use for the 21st century: the Water Futures and Solutions (WFaS) initiative and its approaches, Geosci. Model Dev., 9, 175-222, doi:10.5194/gmd-9-175-2016, 2016.

Thank you. Wada et al. (2016) is particularly relevant to Section 3.5 "Key uncertainties and implications". We cited this paper and mentioned that the water use projections substantially differ among models and systematic model inter-comparison is under way (lines 411-412).

4. I find the results presented are very interesting, and in particular, the substantial ranges in future desalinated water use estimates among different SSPs are intriguing. However, currently the paper focuses primarily on a global scale estimate but I think it is more beneficial to focus on regions and highlight the change in desalinated water use per country in MENA or other parts of the world. For example, the information like "As shown in Table 4, the volume of seawater desalination was estimated at 3.7 $km^3yr^{-1}$with a cost of 1.5–14.0 109 USD, equivalent to 0.0025%–0.024% of total global GDP." is no so informative, in my opinion. This type of information should be provided at a country basis, since the regional heterogeneity of desalinated water use is extremely large.

In the revised manuscript, we showed the change in the volume of seawater desalination for 11 regions in the world in terms of desalinated water production (Table 5) and cost (Table S3 and S4). Discussion is shown in lines 317-337.

5. Global figure like Figure 5 is not so informative, and I suggest to zoom in to some regions like Figure 4. This is a global scale study, but it should highlight more the regions of interest. The majority of the map is blank in Figure 5. The information density is very low.

Thank you for your suggestions. We tried your idea but it was not very successful. Due to income growth by the mid of 21st century, regions including AUSD increases substantially in all continents which makes the figure highly complex.

6. Additional information on surface water availability per country (in some tables) for the future period would be useful to highlight the importance of desalinated water use. Water availability is generally projected to decrease over e.g., the Mediterranean.

Thank you. This is an important point. Now we have added discussion in lines 379-381.

7. I suggest the authors to make a similar table like Table 4 but focusing on some regions (Middle East, Mediterranean, etc). Table 4 is useful for a global comparison, but additional information on regional desalinated water use is also very useful including historical estimates and future projections like Table

1.

Thank you for this suggestion. As stated above, regional projections are now shown in Tables 5, S3, and S4. Discussion is shown in lines 317-337.

8. When you describe "14,000 USD", please be careful that this indicates per capita GDP only. Please check this throughout the manuscript.

Thank you for noticing this. We have checked the unit of per capita GDP throughout text.

9. The term "modern GHMs" is not clearly defined and not commonly used. This is rather confusing, what it is exactly indicating, e.g. model representations, framework, human components, etc. This should be corrected.

Thank you. We've also received similar comment from another reviewer. Now we removed adjective "modern" from text. The original intention was global hydrological models including latest treatment of human activities, but we agree that the adjective was simply confusing.

10. I think this study is important highlighting the significance of desalinated water use for coming decades. The paper can be a bit restructured with more regional focus. In principal, I would recommend this paper to be considered for publication after some revisions.

Thank you. Now we have revised our paper trying to include your suggestions as much as possible. We believe now the paper has been substantially improved including extended discussion on regional results.

Dear Dr. Yaling Liu,

While this work has merit of enhancing the understanding of driving forces for desalination expansion, it needs substantial improvement to provide solid contribution to the community.

Thank you for taking time to read our discussion paper and provide valuable comments. Our response to your questions and comments are shown below.

1. The methodology part is very vague, no model representations or equations are documented, and it is not clearly explicit so that the study can be reproduced.

Thank you. Although we still believe all the necessary information is described in the discussion paper to reproduce our results, we further elaborated the details of methodology in the Method Section for broad readership. In short, our model overlays several gridded global maps of hydrological and socio-economic factors, and extract the grid cells that meets the conditions. Such fundamental concept has been now clearly shown in line 148-157 of the revised manuscript.

2. The assumption of "All the municipal and industrial water withdrawal in Area Utilizing Seawater Desalination (AUSD) is supplied by seawater desalination" is unrealistic. Take the current biggest desalination country Saudi Arabia as an example, the surface freshwater withdrawal, groundwater withdrawal and desalinated water in 2005 is 1.1,21.5 and 1.0 km3, respectively (AQUASTAT).

Thank you for giving us an opportunity to clarify some important points. The large volume of groundwater you pointed out is used for agriculture which is excluded from this study (we are focusing on only industrial and municipal water). In fact, AQUASTAT reports that as much as 88% (20.83 km^3 yr^{-1}) of total water withdrawal was used for agriculture in Saudi Arabia. Siebert et al. (2010) showed that 88.4% of irrigation water is supplied from groundwater in Arabian Peninsula. Hence the results shown in our discussion paper and the data provided by AQUASTAT are consistent.

3. Only global total desalination amount for 2025 and 2055 is presented, the part that would be valuable to the community – the spatial and temporal changes of desalination amount – is missing.

Thank you for this comment. In the revised manuscript, we have provided spatially and temporary detailed results. Spatially detailed results are now shown in newly added Table 5 (production of seawater desalination) and Tables S3-S4 (cost and its GDP percentage) and discussed in lines 310-330. Temporally (Historically) detailed results are shown in newly added Figure 5 and lines 281-294.

References

Siebert, S., Burke, J., Faures, J. M., Frenken, K., Hoogeveen, J., Döll, P., and Portmann, F. T.: Groundwater use for irrigation – a global inventory, Hydrol. Earth Syst. Sci., 14, 1863-1880, 10.5194hess-14-1863-2010, 2010.