Dear Editor,

Thank you very much for taking the time to consider our paper entitled "A seawater desalination scheme for global hydrological models." We are grateful to all of the reviewers for providing detailed and insightful comments. We have responded to all of the suggestions and concerns as shown below.

Reviewer 1

Although the authors have tried to address the concerns that were raised during the first round of review, I am still not satisfied with the quality and the scientific contribution of this paper to warrant its publication in HESS.

> We are grateful to Reviewer 1 for the insightful comments, which we took into serious consideration to improve our paper. This study presents the first model that quantitatively estimates the geographical distribution and volume of seawater desalination production. We firmly believe that this paper provides an important first step for this challenging and emerging research field, in noting that the performance of our model is in certain circumstances below the standard of traditional hydrological models dealing with the behaviors of well monitored natural catchments. We emphasize that the first development of all models (e.g. global climate models, land surface models) begins with a simple version, typically limited in function and performance compared with later iterations. The shortcomings of our model are discussed, which we believe is a critical first step towards improving it in future studies, by ourselves and by the greater scientific community.

In a nutshell, the paper relies on a large dataset of desalination plants (DesalData) to identify grid cells that rely on desalinated water through a set of predetermined conditions (GDPC>14k, aridity<7.5%, distance<~165km), and then through a set of assumptions, the model establishes the amount of desalinated water that would need to be produced. The established thresholds are kept the same globally and over time and as such they could yield strong implications about the future dependence on desalinated water. For example, the per capita income threshold of 14k does not account for the depreciation value of money over time, and since GDPC is expected to increase under all the 5 SSPs, this assumption will lead to wide spread of desal under all the SSP scenarios even the defragmented (and relatively poor) world of SSP3.

We are grateful for these comments and the opportunity to clarify some technical issues. GDP per capita is in 2005 USD Purchasing Power Parity units. Both monetary depreciation over time and difference in prices among nations/regions have been excluded from GDP projections, as noted in lines 146-148. In addition, the increased prevalence of desalination under all SSP scenarios is primarily attributed to a marked increase in GDP for all of the nations, even after removing the effects of depreciation. Please note that the world of SSP3 is "poorer" than the other four scenarios, but it is apparently "richer" under future scenarios than today. In general, we agree that the world economic view of SSP is quite optimistic, but that discussing the validity of the SSP scenario is beyond the scope of this paper. We noted this point in lines 400-404.

As ground water resources start to deplete, there is no guarantee that the 165km threshold will hold and this is already seen in the case of the city of Riyadh, which is about 400km from the nearest coast. The same can be said about the aridity index threshold.

We agree that future model thresholds may change, due to alterations in water availability, technological change, water use, and other factors. Such factors are hard to quantify and include in our model. What is possible is to conduct sensitivity tests, and add the obtained results to the discussion. As such, a sensitivity test on aridity index (Figure 7) has already been conducted, and we added a new test concerning distance from the nearest coast (lines 441-443). We have also expanded the discussion of potential limitations arising from future changes in thresholds in lines 488-492 and 527-530.

Also the fact that conditions/threshold/assumptions were based on the same data that the authors are using to validate the model defeats that purpose. One would expect there is an independent dataset for calibration and another one for validation, which was not the case for this study. Even when basing the validation on the actual year (2005) to derive the grid areas depending on desalinated water and their production estimates, there are many exceptions as discussed by the authors (e.g., Spain and Israel).

First, it is quite difficult to take a systematic and standard approach towards model calibration and validation due to the patchy availability of data concerning desalination and water use. As such, we carried out simple tests to confirm the robustness of the parameters. We randomly split the data and estimated the three most important thresholds (aridity and distance from coastlines and cities) for each split. These results are close to the original estimates of 0.082, 15, and 170.

Second, we developed a new model (SDM2) and reported key results in the revised paper. The SDM2 estimates the regions in which desalination water is used in supplementary way. This model substantially improved the distribution of Area Utilizing Seawater Desalination (AUSD) in the Mediterranean region, including in Spain and Israel (compare Figure 1 and Figure 4b). Model description and results are shown in lines 272-284 and lines 310-323 respectively. Please also see our response to Reviewer 3 which includes more detailed discussion on SDM2.

Finally, the paper can still improve from proofreading to clarify the writing. I have tried to highlight some examples below.

Thank you for this comment. The revised version has undergone proofreading by two professional editors, whose first language is English.

Line 18: change ‘period’ to ‘year’

Thank you for this comment. This is corrected in the revised version.

Lines 25-26: I would suggest omitting the second to last sentence. If you keep it, I would reduce the number of significant figures used or use billion $ unit instead of millions (1.1-10.5 & 1.5-22.1 Billion USD). Although I am not familiar with how much desal plants typically cost, but the numbers do look small. Is it only the capital investment cost that you have accounted for? or also the levelized cost over the lifetime of the plant including operation and maintenance, and the energy cost often associated with such activities, plus piping structures, pumping and distribution costs?

Thank you for these points of clarification. These costs are now reported in billion USD, with one decimal place included. Secondly, the costs now include both capital investment and operational costs. We then rounded the total cost by assuming a plant life of 20 years and a discount rate of 8%, as per Lamei et al. (2008). A brief method summary is also now included in the Supplemental Text lines S62–S72.

Line 32: ‘there is a growing concern over the sustainability of’

Thank you. This has now been corrected in the revised draft.

Line 45: Add a more recent citation beside (Bremere et al., 2001).

Following a search of the literature, we have now included a citation of a report by Fichtner GmbH, a private consultancy firm, published in 2011. This includes a summary of the report (lines 65-69), its projections (Table 4), and a discussion of its results (lines 392-398).

Line 47: the introduction lacks any information about the current capacity & production of desal and even the historical evolution beyond citing figure S1. For example, when citing that Kim et al came up with the estimate of 250km3/yr by 2100, it is hard for the reader to give sense to how big or small that number is in comparison to current use.

Thank you. We added the total production of desalinated water in 2005 (5.2 km^3 yr^{-1}).

Line 91: give the numbers for the rest of categories, 'followed by brackish water, saline inland and river water, and others.' If I am understanding this correctly, the 13.3 is the largest portion of the 21.9 total, and the other categories add up to the remaining 8.6km3, right?

Yes, this is correct.

Line 93: "First, we selected the desalination plants (hereafter Major Plants) to be included in our analyses." Are not you missing the word 'major' in that sentence?

Thank you for catching this; the word 'major' is now included.

Lines 93-94: "The detailed selection criteria and the rationale is shown in Table S1 and Supplemental Text." Table S1 lists the criteria but does not say anything about the rationale. For example, it is still not clear what was the rationale for using only 613 large of 17000+ plants available? The SM does not really address this point as claimed by the authors. The only piece that relates to this aspect is the following "Finally, we selected plants of larger than 10000m3d-1 of capacity. It corresponds to approximately 1.5mm yr-1 of water resources assuming the area of a grid cell is 2500km2(0.5°×0.5°), which is same order of the magnitude of other water resources components simulated in H08."

As the Supplemental Text of the original version includes a rationale for using a threshold of 10,000 m^3 d^{-1}, we request clarification about the additional information requested. Given that questions regarding the number of plants were included within the comments, we responded by adding Table S3 in the Supplemental Material, which shows the number of plants and total desalination capacity under various conditions. We used 559 plants in our analysis (excluding 54 additional records, including erroneous longitude and latitude). This covers a considerable portion of the total desalination capacity globally (81% of the total capacity of online plants of all sizes using seawater). Thus, we argue that the exclusion of small plants from our analysis would not have major effects, given their relatively small contribution. Text regarding this issue was added to the discussion (lines 117-121).

Lines 95-96: “For example, DesalData contains some records indicating that plants in mountainous inland regions use seawater as the source, which is erroneous. The plants excluded from this study are listed in Table S2with reasons.” The mountainous example is not listed as a reason for any of the 11 excluded plants in table S2. Does that mean that criterion was never used to exclude any plant? If so, then replace with the criteria mentioned in table S2.

Thank you for pointing this out. We removed “mountainous” from text. During the previous revision, we re-examined records to be excluded from our analysis, and noted that elevation should not be used as an evaluation criterion. The sentence now reads “For example, DesalData contains some records indicating that plants in inland regions use seawater as the source, which is erroneous” (lines 116-117). Please note that we needed to exclude another 43 plants because erroneous longitude/latitude information were reported. Altogether 54 plants were excluded in this study.

Lines 103-107: it is not clear yet in the flow of the paper why there is a need to collect data on the distance of desal plants and major cities. Briefly explain

Thank you for this comment. We estimated grid cells that utilize seawater desalination as a primary permanent water resource within the SDM. In many cases, seawater is transferred to desalination plants located near the shore, and freshwater is then further transferred to the cities. We considered that the maximum distance that seawater is transported through desalination is the sum of the distance from the seashore to plants, and from plants to cities. An explanation of this is added in lines 199-200.

Line 149: By inputting ..." rephrase the sentence. replace 'inputting' with 'using', 'extracts' with 'identifies', and 'that' with 'where'. also specify what gridded global maps are used as inputs. Also what conditions? The sentence needs to be rephrased.

Thank you for this comment. This is now rephrased as “SDM is a set of climatological, geographical, and socio-economic conditions to estimate the spatial extent where seawater desalination is likely used. SDM identifies the grid cells where all of the conditions shown below are met. We call these grid cells the Area Utilizing Seawater Desalination (AUSD)." (lines 224-226)

Line 153: delete 'latest'

Thank you. This has been deleted.

Lines 155-157: replace "but it is not necessarily true that desalination plants are located" with 'but desalination plants are not necessarily located'

Thank you. We rephrased this accordingly.

Lines 162-164: the three conditions stand out as subjective at best. It seems that it would make more sense to discuss the data analysis first to justify the selection of these criteria

The three conditions were derived through extensive data analyses, as shown in the text of the draft article. We initially adopted the standard IMRAD (Introduction, Methods, Results, and Discussion) style, such that the model description (Section 2.4 in Methods) comes before the data analyses (Section 3.1 in Results and Discussion). As you mentioned repeatedly, this is a bit confusing. We accordingly moved Section 3.1 to between Sections 2.3 and 2.4.

Line 167: delete "Conditions B, C, A correspond to each."

Thank you. This has been deleted.

Lines 174-176: again these seem random assumptions without sufficient justification to their logic. Where is the 30-80% coming from? The tendency to promise info to come later in the manuscript makes the manuscript harder to follow.

Assumption C (30–80%) was derived from the data analysis explained in Section 3.1. As stated above, we moved Section 3.1 to before Section 2.4 (model description) to allow for an explanation of the model prior to presenting results, which we hope improves the clarity.

Line 168: "data used and spatial resolution of interest, hence modified if SDM is applied in a different simulation settings." – not sure what this really means. Rephrase.

> We had intended to communicate that thresholds are determined by data analyses, and are dependent on both the dataset used and the spatial resolution. This part now reads: "The thresholds are dependent on the climatological data used and spatial resolution of interest; hence, is expected to change if SDM is applied with different data at a different resolution." (lines 250-252). We hope this improves the clarity.

Line 206: replace 'was installed' with 'were installed'

> Thank you. This has been corrected.

Line 245: "We approximated it to 30-80% and set as Assumption C." why not use 28-77% as shown by the data?

> We rounded up 28% and 77% to 30% and 80% as the numbers were derived from limited records of data (only 5 records, as shown in Table 3). We therefore assumed that such rounding best reflected the limited nature of the dataset.

Figure S3: Are you accounting for the diminishing purchasing power of money over time (discount rate) when accounting for the 14000$/person threshold? The SSP3 should be a 'poor' world with high population and difficulties to both mitigate and adapt.

> All GDP projections used in this study are in units of 2005 USD PPP. The rate of inflation (including discount rate) was excluded from the projections. We agree that SSP3 results partially contradict scenarios deriving from its narrative (low income and difficult to adapt to and mitigate climate change). We do note that country-based GDPPC grows steadily and considerably in all SSP scenarios. Thus, SSP3 is poorer and more fragmented than the other SSPs, but is still richer than the present day. This explanation has been added to the discussion in lines 146-148 and 400-404.

Table S2: what does it mean that 'lat/long data collapsed'? this does not make sense to me. Are you saying these points (lat/long) fall outside of the country boundary (Spain in this case)? Or something else.

> Thank you for picking up this point. This sentence has now been phrased as "apparent

inconsistency in location records." For example, longitude and latitude for the Minera Escondida Plant in Chile are actually the location of a hotel in Santiago. We hope this change improves the clarity.

Reviewer 2

I have reviewed the previous version of the manuscript (Reviewer #2). After reading though the revised version of the manuscript, I find the paper much improved and that the authors responded mostly well to my previous concerns. I appreciate that the authors have implemented all of my comments.

We thank you again for your time and valuable suggestions towards improving our study.

As I mentioned in the previous review, this study is novel developing a new desalinated water use scheme, which can be integrated with a large scale water model. This is very beneficial for large-scale hydrological community.

As before, in principal, I would recommend the paper to be considered for publication in HESS. I have a few outstanding comments that the authors may consider to incorporate before submitting a final version.

1. In Abstract, "…(0.002–0.019% of the global total GDP)…", is used, also in in supplement Table S5, all is relative to global total GDP. I don't really find these numbers very useful. I think they could be more relevant if you could specify per country (% of the country total GDP) for both cases (Abstract, Table S5). Costs and GDP are very region-specific so in many cases global cost and global GDP doesn't correspond each other well. Cost is always attributed to a region and associated GDP (not global).

Thank you for this comment. We reported both global and regional numbers in the revised text. Additionally, Table S6 (former Table S5) reports the fraction of desalination cost to GDP for eleven regions and globally. Only global numbers are shown in the Abstract, due to space limitations. We also note that the unit cost of desalination is reported globally in our study (i.e. as stated in lines 427-428).

2. Figure 6. Thank you for your efforts. But I don't see almost any differences for SSP1, 2, and 3.

Can you zoom in a little bit? Or maybe put subpanels?

We again studied some alternative graphic expressions, but could not come up with a new effective one. The differences among SSP1, SSP2, and SSP3 are less than 200 grid cells out of 67,000 total land grid cells. In any graphic expression, it is difficult to see the difference very clearly and efficiently. Therefore, we described the key differences of SSPs in lines 374-376.

Reviewer 3

The authors of the revised manuscript present a model for areas and volumes of use of desalinated seawater (SDM) that is proposed to be suitable for incorporation into global hydrological models (GHMs) to estimate current seawater use (2005) but in particular to derive scenarios of future use of desalinated water. They also generated three scenarios for the time periods 2011-2040 and 2041-2070 based on the global Shared Socioeconomic Pathways (SSP) scenarios of future population and GDP.

In my opinion, the capacity of the developed SDM for simulating current/historical use of desalinated seawater is so poor such that it is not meaningful or sensible to include the model in GHMs. SDM predicts the existence of desalinated seawater use only as a function of aridity, GPD and distance to ocean. Table 2, a comparison of Fig. 1 and Fig. 4 as well as Fig. 6 show that the assumption that desalinated seawater is used only in very arid grid cell with an aridity index of 0.075 or less precludes the simulation of existing seawater use in Spain and Israel as well as in coastal areas with desalination plants in Libya, Australia, the USA, India and China even in the future (see comp. Fig. 6e and f to 6a-d). While with this aridity criterion, the current bulk of desalinated seawater that is produced on the hyperarid Arabian peninsula is captured, this does not make the model suitable for simulating the extension of seawater desalination to other less arid coastal areas that may be expected (and has been observed) with increasing water stress and decreasing cost of desalination.

First of all, we are grateful for your time spent reviewing our manuscript, as well as your insightful comments and constructive criticisms. We have further improved our model and interpretation of the results. In the revised paper, we added an auxiliary model that largely improves the reproducibility in desalination plant distribution. We also revised the interpretation of the original and auxiliary models, which is summarized below.

First, our model simulation is markedly improved. For example, the total desalination water production in nine major countries is 3.0 $km^3\ yr^{-1}$, where our grid-based simulation using multi-disciplinary and independent datasets gives 2.4 $km^3 yr^{-1}$ (Table 2). Our SDM is both intentionally simple and robust. It could be improved by adding local details, but such specificity could lose applicability across temporal and geographic scales. We also note that detail is lacking from even the most recent global scenarios on which the model is based.

Second, we developed an auxiliary model (SDM2) with a different set of conditions from the original model (lines 237-241 and 272-284). One of the most important conditions of the original model (SDM) is that the Aridity Index must be less than 8% (7.5% in the previous manuscript; the threshold was updated after an intensive data quality check), to account for the fact that major desalination plants are concentrated in hyper-arid climates where municipal and industrial activities are largely dependent on desalinated water. We believe this is a valid and useful predictor, but it belies the fact that a considerable number of desalination plants are beginning to be constructed in modestly arid regions. In the revised paper, we proposed SDM2 with a relaxed Aridity Index threshold and added a new water scarcity threshold (discussed in detail below). SDM2 enabled us to reproduce the geographical distribution of desalination plants much better than the original model, particularly for the Mediterranean coasts. This model now represents existing seawater desalination in Spain and Israel, as well as in coastal areas of Libya, Australia, USA, and China (compare Figure 1 and Figure 4b).

Third, we revised the interpretation of the model and the simulation results (lines 237-241 and 310-323). AUSD estimated using the SDM shows the geographical extent of regions that are chronically dependent on desalination. This is the case for a number of cities on the Arabian Peninsula. Owing to socio-economic changes, this dependence will expand beyond the Arabian Peninsula, as shown in our future projections. We find that the estimated AUSD by SDM2 (shown above) is the geographical extent of regions in which desalination water is used in a supplementary way. By combining the results of SDM and SDM2, we successfully reproduced both the geographical extent and total production of seawater desalination in the world.

Unfortunately, the authors do not describe well how they derived their model and how the results using alternative predictors would be. I think that predictors with a higher predictive capacity should be tested. If it is not possible to derive a more "predictive" model than it is better to refrain from simulating future changes in the use of desalinated seawater. This is the main reason why I think the manuscript should be rejected.

An explanation of how SDM conditions and assumptions were obtained through data analyses is included within the text. Because we adopted an IMRAD (Introduction, Methods, Results, and Discussion) style in our original submission, the model description (Section 2.4 in Methods) appeared before the data analyses (Section 3.1 in Results and Discussion). This gives the impression that the conditions and assumptions are arbitrary. Thus, we put Section 3.1 before Section 2.4 in the revised text. We hope this makes the format of our experiment clearer.

Second, we developed and tested the SDM2 model that includes an alternative predictor of water resources index (WWR; Raskin et al., 1997) and a water stress threshold of 0.4. Both this index and this threshold are widely accepted, and have been used extensively to assess global water scarcity in the last two decades (e.g. Vörösmarty et al., 2000; Oki and Kanae, 2006). We then relaxed the Aridity Index threshold by as much as 50% (originally 8%); other conditions and assumptions were kept intact. Using the WWR enabled us to incorporate water requirements, or a 'human' aspect into SDM2. This SDM2 expands AUSD to a greater spatial domain including coastal Spain, Israel, northwestern Libya, and islands in Italy, where desalination plants are actually located. Future AUSD spatial extents are shown in Figure S5. Although SDM2 is promising, we could not substitute SDM with this model. Again, the primary objective of this research is to quantify desalination water production. The AUSD estimated by SDM2 includes regions where seawater desalination is used for supplementary water, necessitating information on the availability of other water sources (e.g. river water, groundwater, and others). This demonstrates the need to couple SDM2 and H08 in future research. This model and results are all discussed in lines 237-241, 272-284, and 310-323.

Third, we are aware of other potential predictors, particularly the importance of economic factors. Whether seawater desalination becomes a practical option or not is determined by its production cost relative to that of other water sources. To estimate the cost of water of various sources is challenging because we need to consider seasonal/inter-annual variations in water availability, water quality, regulations, and many other aspects. Including such aspects in global hydrological models is highly challenging, if not infeasible; taking steps to do so in future work will be possible given the progress and improvements in global hydrological models (lines 518-519 and 524-527).

In addition, the three derived scenarios are driven by climate (change), GDP change and domestic and industrial water use change only, with future new grid cells with desalinated seawater use mainly being determined by the different GDP developments in the three scenarios. In the spirit of scenario development, it would have been interesting to represent the different storylines of the SSPs (sustainability vs. middle of the road vs. fragmentation) by e.g. favoring waste water treatment over desalination in the sustainability scenarios SSP1.

Thank you for this comment. Altering water sources and technology by SSP is quite interesting. We added the points you made: we only used GDP information of SSPs in this study to project future expansion in desalination use. Utilizing qualitative (narrative) scenarios would enhance scenario consistency (lines 527-530). We believe this study is a first important step toward such advanced studies. Please note that future projections in industrial and municipal water use fully incorporated both the quantitative and the qualitative SSP scenarios (Hanasaki et al. 2013a,b).

Finally, the presentation of the research even in the revised version is still only fair. For example, neither in the abstract nor in the introduction is it mentioned that they only simulate desalinated sea water use for domestic and industrial uses. The relationship between aridity index and the existence of desalination plants which is central to the model is not shown while a global map of the aridity index is (lines 214-217). It is not discussed what can be learned from then new panels Fig. 6e and 6f. There are many typos, and the manuscript would require language editing.

Thank you for your valuable comments. We have gone through the text and revised it thoroughly to improve the presentation. First, we mentioned the focus on domestic and industrial water in the Abstract (lines 20-21) and Introduction (lines 88-89). Second, we elaborated what we can learn from Figure 2 and Figures 6e-f in lines 184-193 and lines 380-389, respectively. Finally, two professional editors proofread the text. We thus strongly believe that the quality of the paper and presentation of results have been significantly improved through these actions.

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