Author's response

We would like to express our sincere gratitude to the Editor and the Reviewers for their careful assessment of the manuscript and for their constructive and insightful comments. Their observations have been extremely valuable and have allowed us to improve the manuscript substantially, making it clearer, more robust, and scientifically stronger. Among the most relevant revisions, we would like to highlight the clarifications added regarding the applicability of the proposed model to other regions and to different environmental and hydrological contexts. Significant changes were also made to the empirical modelling choices, including a revision of the calibration approach and a more detailed explanation of the rationale underlying the selection of the adopted parameters. In addition, the discussion of the physical processes controlling the described hydrological behaviour has been expanded and made more exhaustive. Further clarifications were also provided on IPI, which represents a key innovative metric of this study. Moreover, additional details on the climatic data used in the analysis have been included in the dedicated section. All these revisions originated directly from the comments raised by the Editor and the Reviewers. Accordingly, in each Reply provided below after the corresponding Comment, we describe in detail the modifications introduced, the modelling choices adopted, and the related changes made in the revised version of the manuscript, with explicit reference to the corresponding sections of the text.

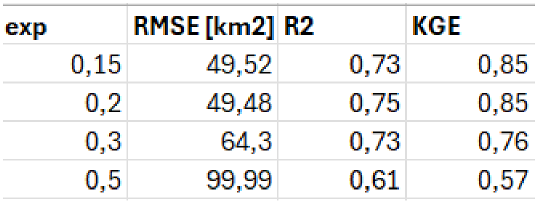
Reviewer #1

Comment 1) Since Section 1 mentions that WetMAT can be applied to other regions, please present actual external applications and their outcomes. Specify which metrics (e.g., MFA, hydroperiod) were used for calibration/validation, how they were evaluated, and the achieved performance levels.

Reply We thank the reviewer for this insightful comment. At present, the only fully developed and documented application of WetMAT is the one presented in this manuscript for the Doñana National Park. Our intention in Section 1 was not to claim that external applications already exist, but rather to emphasize that the model was designed to be transferable, thanks to its simple conceptual structure and limited data requirements. Although section 3.3 of the manuscript mainly refers to MFA and hydroperiod, variables that are used in calibration, validation or in other processes crucial to the work, such as the calculation of IPI, this paper shows that WetMAT allows the calculation of various output variables (shown in the flowchart in Figure 2) relating to soil moisture, flooding depth and volume in channels and marshland. WetMAT is currently being tested also in an additional Mediterranean wetland case study, where calibration and validation rely on different variables tailored to the specific characteristics of the system (permanent coastal wetland characterised by peaks in salinity increase due to various factors); preliminary results are encouraging, but these applications are still ongoing and thus beyond the scope of the present manuscript. In this case, the model outputs still concern soil moisture and the flooding characteristics of the wetland, such as its extent or depth, but these are supplemented by a qualitative output, also used in the validation process, namely salinity, as the available observed data series also allowed this aspect to be modelled. Further details on the implementation and application of WetMAT in other contexts, as well as the outputs produced by the model, can be found at https://hdl.handle.net/11589/295624, a doctoral thesis based on the implementation of the model in different contexts. To avoid any misunderstanding and to clarify how calibration and validation metrics may vary in other case studies, we have revised the text in the Introduction (Section 1, lines 62–64). We agree that demonstrating model performance in additional case studies in a fully documented way would be valuable, and this will be the focus of future work.

Comment 2) In Section 2.2.1, Eq. (6) directly computes MFA, but the basis for the exponent 0.2 is not clear. Please provide supporting prior studies and the empirical fitting procedure, along with a sensitivity analysis over alternative ranges (e.g., 0.15–0.30).

Reply We thank the reviewer for raising this point. In Eq. (6) we represent the relationship between mean Marshland Flooded Depth (MFD) and Marshland Flooded Area (MFA) by means of a simple power law. This choice of functional form is consistent with previous studies on shallow wetlands and low-relief basins, where compact power function depth–area or depth–storage relations have been shown to provide an adequate approximation of the underlying bathymetry (e.g. Hayashi and van der Kamp, 2000, subsequently adopted and extended in later works such as in Minke et al., 2010 or in Chandler et al., 2024). In this sense, the exponent in Eq. (6) is not arbitrary, but reflects the adoption of a standard, parsimonious representation of the MFA–depth relationship for shallow wetland systems. In the case of the Doñana marshland, we did not have access to direct, co-located measurements of flooded areas and water depth that could be used to derive from ourselves an empirical MFA–depth curve. Therefore, we constrained the exponent using published estimates of flooded area and mean water depth for the Doñana marshes reported by Leiva-Piedra et al. (2024). Rather than adopting their more complex formulation, which is not directly suitable for inclusion in our simple daily water-balance model, we used their results as independent constraints on the order of magnitude of depth and MFA. By fitting a power law curve to reproduce these orders of magnitude over the mean depth range of the marshland, we found that exponents in the vicinity of 0.2 provided a satisfactory match. For the sake of parsimony and to avoid over-parameterisation and identifiability issues, we then fixed the exponent at 0.2, while calibrating only the other model parameters. As requested by the reviewer, we performed a local sensitivity analysis in which the exponent in Eq. (6) was varied within the range 0.15–0.50. When comparing the simulated time series obtained for each tested exponent with the observed data, the exponent 0.2 provides the best overall performance, yielding the most favourable values for all three metrics considered (RMSE, R^2 and KGE). The results of the sensitivity analysis are shown in the table below. In the revised text of the manuscript, Eq (6) is better described in lines 199-201.



exp	RMSE [km2]	R2	KGE
0,15	49,52	0,73	0,85
0,2	49,48	0,75	0,85
0,3	64,3	0,73	0,76
0,5	99,99	0,61	0,57

Comment 3) In Section 2.2.2, when applying WetMAT to the Doñana marshland, what are the specific reasons for neglecting river inflow (Iw) and groundwater inflow (Gw) in Eq. (1)? Also explain why, in the simplified balance (Eq. 7), evapotranspiration over water (ETw) becomes the dominant term.

Reply We thank the reviewer for this comment. In Section 2.2.2, Eq. (1) is simplified to Eq. (7) by neglecting the terms Iw and Gw . This step, which represents a major simplification in both the conceptual scheme and the implementation of WetMAT for this case study, is justified by several lines of evidence from the literature. In '*Science to Save Doñana. Evidence of its Ecological Degradation in 2024'*, Gil Gil (2024) reports that, between 1950s and 1960s, the course of the Guadiamar River, historically the main source of surface water inflow to the marshland, was diverted to allow a faster connection to the sea, thereby favouring the drying of the area and its conversion to agriculture. The presence of infrastructures built in the 20th century that altered the hydrological functioning of the region is also mentioned by Morris et al. (2013), who explicitly refer not only to the Guadiamar River, but also to the Guadalquivir River, from which the marshland can now be considered effectively isolated. Minor watercourses such as the El Partido and La Rocina streams have also experienced a drastic reduction in flow (greater than 60%), and are now frequently dry, because they are sustained by groundwater that has itself undergone marked declines probably due to agricultural abstractions (Custodio et al., 2008). These findings support the decision not to include surface water inflows in the water balance, other than direct rainfall over the marshland. Regarding the term Gw, Acreman et al. (2022) report marked reductions in groundwater discharge to the marshland over the last three decades, attributing these to the large number of wells drilled in the area. Green et al. (2024) describe piezometric declines of up to 20 m in some locations and a reversal of the vertical hydraulic gradient in natural discharge areas along the coastal margins of the marshland, i.e. the ecotones. Regarding the central parts of the marshland they are reported as hydraulically isolated for geological reasons as the marsh bed consists of clay deposits. The above considerations support the assumption that the terms Iw and Gw can reasonably be omitted from Eq. (7) in this application. Hints of these simplifications were already present in the original version of the manuscript, however, in the revised manuscript we now discuss this issue more explicitly in Section 2.2.2 (lines 223–230). In addition, lines 172-174 and 234-235 clarify our previous statement that evapotranspiration (ETw) is the "dominant term" in the balance. First, it should be clarified that ETw does not refer exclusively to evaporation from open water. Rather, ETw represents a composite term that includes evapotranspiration from soil and vegetation, which acts over the entire study area throughout the year, and direct open-water evaporation from the temporarily inundated portions of the marshland, which is therefore dynamic because it depends on the flooded extent at any given time. To clarify the notation, the term 'ETw', which originally referred to 'wetland evapotranspiration', has been replaced in the text simply by the term 'ET', an explanation of the concepts described above. In the context of the simplified Eq. (7), ET represents the main outflow from the wetland, while precipitation is the only inflow term considered in this case study. We would also like to emphasise that the WetMAT model explicitly includes both surface-water and groundwater inflow components. We tested the groundwater discharge module in the wetland in another Mediterranean wetland case study, the results of which will soon be shared in a coming paper. The simplifications adopted for the Doñana case study were therefore made solely because the hydrological characteristics of the system allowed it, and do not reflect an intrinsic limitation of the model.

Comment 4) In Section 2.2.3, describing parameter upper/lower bounds as merely "reasonable" is insufficient. Please cite the basis for each value (field observation ranges, literature values, or data statistics) and explain how these sources informed the final choices.

Reply We thank the reviewer for this insightful observation. We have revised Section 2.2.3 to provide a rigorous justification for all parameter bounds, replacing general assumptions with citations from site-specific literature and established hydrological standards. Regarding soil moisture thresholds (ϑ_{WP}, ϑ_{FC}), in the absence of direct on-site measurements, we identified the soil as predominantly clayey based on *García Novo & Marín Cabrera (2006)* already mentioned in the manuscript and detailed studies of the contiguous Lebrija marshland (*Moreno Marín et al., 2003* R1), which is geomorphologically representative of the Doñana area. *Moreno Marín et al. (2003)* report a clayey texture composed of "<1% sand, 30–33% silt, and 67–70% clay". Based on these properties, literature values for field capacity and wilting point were derived from FAO-56 (Chapter 8, Example 36; *Allen et al., 1998*) and further refined using the SPAW (Soil-Plant-Air-Water) software (Soil Water Characteristics tool). This additional analysis led us to adjust the wilting point range to 150–350 mm/m to better reflect the physical characteristics of such soils. Consequently, the sensitivity analysis and calibration process was re-run using this updated range; as noted in the revised text, no substantial changes in results has anyway observed compared to the previous settings. For the lateral drainage factor (*Df)* and hydraulic conductivity (K), we referred to the mathematical groundwater model developed for the Doñana aquifer by *Serrano Hidalgo (2023),* in addition to the source already cited in the text. These values served as the baseline to establish broader, conservative intervals for both sensitivity analysis and subsequent calibration. Regarding other parameters, wetland area (Am) has been derived from established literature and cross-validated through satellite imagery, as already stated in the manuscript. The root depth (Dr) has been set within a 0.5 -1.5 m range, consistent with standard values for herbaceous and wetland vegetation (*Allen et al., 1998; Feddes et al., 2001*) and coherent with the shallow groundwater conditions and seasonal regime of Doñana (*Serrano et al., 2006*). Other geometric parameters relate to the simplified conceptual form of the model, although intrinsically linked to the physical features of the wetland through parameters as Am and drainage density parameter, are the result of a hydrographic map survey. The manuscript has been updated to reflect these references and the rationale behind each choice in lines 242-254, as well as in Table 2. The results of the updated sensitivity analysis in Figure 4 have also been replaced.

R1: Moreno Marìn, A., Vaz Pardal, R., Gutiérrez Cotro, A. (2003), *Effectos del riego sobre la salinidad y composiciòn del complejo de cambio de un suelo recuperado de las marismas de Lebrija,* Estudios de la Zona No Saturada del Suelo Vol VI, Alvarez-Benedì, J., Marinero, P.

Comment 5) In Section 3.1, both θ_WP and θ_FC are highly sensitive for maximum MFA, yet the trends differ: MFA increases as θ_WP increases, while MFA decreases as θ_FC increases. Please provide the physical explanation for these opposite tendencies.

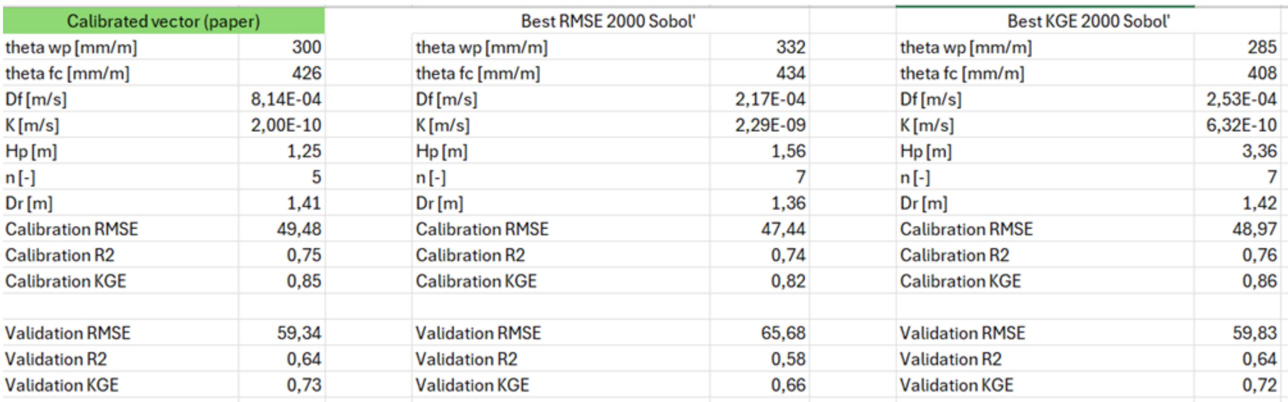
Reply We thank the reviewer for the comment, which allowed us to improve the manuscript by adding deeper physical clarity regarding the phenomena involved, at lines 318-323. A more in-depth explanation of the phenomena governing these opposite trends is proposed below. The behaviour observed for θ_WP and θ_FC is physically consistent with soil hydrologic response, especially clayey ones (Seneviratne et al., 2010). As θ_WP, the wilting point, indicates the hydrological threshold that limits soil water availability for plants and evaporation, it means higher θ_WP correspond to a condition enhanced flooding condition. Conversely, increasing the θ_FC value indicates a greater amount of water required to reach soil saturation and, consequently, a lower response to the flooding phenomenon, as the soil (to be imagined as a 'sponge') tends to absorb it within its pores. The behaviour observed in the WetMAT sensitivity analysis also indicates how the soil water balance is crucial in the development of the model itself.

Comment 6) For nine WetMAT parameters, 100 calibration runs appear insufficient to identify the optimum robustly. Please justify why limiting calibration to 100 runs is adequate for global exploration.

Comment 7) In Section 3.2, state that "As the only datasets available for the maximum MFA and hydroperiod variables are those used in this study, with no other relevant literature providing comparable datasets." but relying on a single metric is limiting. Why was KGE used alone? Please add auxiliary metrics (e.g., R2, NSE) and specify threshold/interpretation criteria from the literature.

Reply We thank the reviewer for this comment. Since this point and the following **Comment 7)** are connected, and both proved useful in prompting a deeper assessment and substantial revisions to the core model calibration and validation procedures, we address the two questions jointly here to provide a more coherent response.

In reality, the parameters involved in the model calibration are seven, not nine, as already indicated in the considerations accompanying the sensitivity analysis (see, e.g., Table 3 and in reply to Comment 4)). This is because the *marshland area* parameter is prescribed based on the literature and on direct measurements of the site area, whereas the *slope of channel banks* is assumed to be 45°, and therefore it is not a parameter subject to calibration. In any case, consistently with recent discussions and guidance in the hydrological modelling literature (Mai, 2023, R2), we acknowledge that, even for a parsimonious hydrological model, a global exploration requires a substantially larger number of model evaluations than a small random sample. Consequently, 100 purely random Monte Carlo runs are not sufficient to claim a robust identification of a global optimum for the model. For this reason, we designed a new workflow that enabled a more thorough calibration and a subsequent comparison with the results previously obtained. We decided to run 2000 simulations using a Sobol low-discrepancy sequence (Sobol', 1967), also referred to as a quasi-random or Quasi–Monte Carlo (QMC) approach, with Owen scrambling randomization (Owen, 1998, R2). The Sobol' sequence generates points that fill the unit hypercube more uniformly than purely random sampling, thereby reducing clustering and yielding a more homogeneous coverage of each parameter domain, with fewer gaps across the admissible space. For each parameter, we set its physical range, consistent with the ranges adopted in the sensitivity analysis. Accordingly, as described in our response to Comment 4), we updated the physical range of the wilting point parameter to “150–350 mm/m” interval. We also used a fixed seed to ensure reproducibility. All simulations were performed in R (Hofert and Lemieux, 2026, R2). At this stage, we evaluated the new results, together with the parameter combination adopted in the previous version of the manuscript, using three different metrics, rather than relying solely on the Kling–Gupta Efficiency as done earlier. As the reviewers suggested, even though the scarcity of comparable datasets in the literature limits an exhaustive benchmarking, focusing on a single metric is restrictive. The selected metrics, in addition to KGE, were R2 and RMSE. In particular, R2 and KGE are now adopted as additional performance metrics to assess the results from a complementary perspective (Althoff and Neiva Rodrigues, 2021). RMSE was selected as it facilitates comparisons among calibration alternatives at the same scale. While R2, was chosen as the simplest and most intuitive metric to indicate the degree of scatter around the regression line. Below, we provide a summary of the results obtained in both calibration and validation for all three metrics described above, considering both the parameter vector used in the manuscript and the best-performing vectors obtained from the Sobol’ simulations. In the table below, we report not only the parameter values defining each vector, but also the top-ranked Sobol’ vectors identified by the lowest RMSE and the highest KGE. Given the substantially worse R2 and KGE values, especially during validation (together with a worse RMSE in validation as well), it was straightforward to exclude the “Best RMSE Sobol’” vector from further consideration. By contrast, the “Best KGE Sobol’” vector yields very similar results, with negligible improvements in calibration and equally negligible deteriorations in validation; therefore, we decided not to modify the parameter vector already reported in the manuscript. It is also worth noting that, beyond the individual parameter values, for the soil-moisture thresholds it is important to consider the difference between the upper and lower thresholds, as this difference controls the soil water balance. For the two vectors under comparison, this value is practically the same.



Calibrated vector (paper)		Best RMSE 2000 Sobol'		Best KGE 2000 Sobol'	
theta wp [mm/m]	300	theta wp [mm/m]	332	theta wp [mm/m]	285
theta fc [mm/m]	426	theta fc [mm/m]	434	theta fc [mm/m]	408
Df [m/s]	8,14E-04	Df [m/s]	2,17E-04	Df [m/s]	2,53E-04
K [m/s]	2,00E-10	K [m/s]	2,29E-09	K [m/s]	6,32E-10
Hp [m]	1,25	Hp [m]	1,56	Hp [m]	3,36
n [-]	5	n [-]	7	n [-]	7
Dr [m]	1,41	Dr [m]	1,36	Dr [m]	1,42
Calibration RMSE	49,48	Calibration RMSE	47,44	Calibration RMSE	48,97
Calibration R2	0,75	Calibration R2	0,74	Calibration R2	0,76
Calibration KGE	0,85	Calibration KGE	0,82	Calibration KGE	0,86
Validation RMSE	59,34	Validation RMSE	65,68	Validation RMSE	59,83
Validation R2	0,64	Validation R2	0,58	Validation R2	0,64
Validation KGE	0,73	Validation KGE	0,66	Validation KGE	0,72

To relate our results to model fitting standards reported in the literature, we referred to general to the threshold values used in hydrological modelling, as no universal standards for these metrics are currently established specifically for wetland inundation. In particular, the R2 values can be considered “satisfactory" in validation and “good” in calibration according to Moriasi et al. (2015), whereas the KGE values are “good” in both calibration and validation based on the criteria reported by Thiemig et al. (2014). We also decided to remove the hydroperiod-based comparison that was included in the previous version of the manuscript, for the reasons already stated in the text regarding the difficulty of computing hydroperiod in a manner that is fully consistent with the literature. Accordingly, Section 3.2 of the manuscript has been completely revised: we added a new schematic layout of the calibration and validation procedures (Fig. 5) and introduced the additional evaluation metrics together with their interpretation.

R2: Hofert, M., Lemieux, C. (2026): *qrng: (Randomized) Quasi-Random Number Generators (R package version 0.0-11). CRAN,* 10.32614/CRAN.package.qrng.

Mai, J. (2023), *Ten strategies towards successful calibration of environmental models,* Journal of Hydrology, 620 (A), https://doi.org/10.1016/j.jhydrol.2023.129414.

Owen, A.B. (1998), *Scrambling Sobol' and Niederreiter-Xing Points,* Journal of Complexity, 14 (4) 466-489, https://doi.org/10.1006/jcom.1998.0487.

Comment 8) For maximum MFA, please explain the hydrological rationale for why achieving KGE = 0.85 required setting θ_WP at the upper bound and K at the lower bound. In addition, please provide literature that supports the final chosen parameter values.

Reply As described at the end of Section 3.2, θWP and K were moved towards their boundary values during a post-processing step applied to the best-performing parameter vectors obtained from the purely random sampling: after parameter normalization, the ten top-ranked vectors (based on KGE for "maximum MFA" output) showed θWP consistently near the upper bound and K near the lower bound; we therefore tested the same vectors by setting these two parameters to their extreme values to further improve MFA performances, with better results. Hydrologically, this behaviour is consistent with the role of the two parameters in WetMAT. A very low K implies quasi-null subsurface transmissivity/drainage, which is actually to be expected from predominantly clayey subsoil. In practice, the chosen lower bound already represents an almost impermeable behaviour in the model, consistent with the limits found in the literature for this type of geology, as discussed in reply to comment 4) so exploring even lower values would be of limited meaning because the drainage term is already negligible. Following the reviewer's remark, we also subsequently expanded the explored range for θWP (see our response to comment 4). Therefore, the adopted θWP value no longer corresponds to the upper-bound but falls within the central portion of the updated interval. The change made to the text of the manuscript, which explains the choice of the lower limit for the K parameter, can be found in lines 379-382.

Comment 9) The Inundation Persistence Index (IPI), which combines maximum MFA and hydroperiod, appears to be the key metric of this study. However, the background and definition of IPI are not sufficiently explained in the manuscript. Please supplement this in the Introduction or Section 3.3.

Reply We thank the Reviewer for this helpful suggestion. We agree that, in the previous version, the background and definition of the Inundation Persistence Index (IPI) were not sufficiently detailed. In our study, IPI was primarily conceived as a practical and physically interpretable way to translate hydrological outputs into a quantitative measure of seasonal inundation phenomenon that can support wetland management and scenario-based assessments. To address the Reviewer's concern, we have revised the manuscript as follows. First, we explicitly introduce IPI in the Introduction (lines 68–71), clarifying its role as a synthetic indicator of inundation dynamics. Second, we substantially expanded Section 3.3 (lines 430–462), where IPI is now presented as a key metric of the study, with a clear definition and interpretation, and with an explicit link to its two components (maximum MFA and hydroperiod). In addition to the changes requested, we also added a clarification in the Discussions section (lines 455–565) noting that, while the current formulation is suitable for inter-annual comparisons within the study site and reflects site-specific characteristics, a natural future development is a dimensionless, normalized version of IPI to enable comparisons across Mediterranean wetlands of different extent.

Comment 10) The statements in Section 3.3 that "The annual precipitation goes above the 400 mm there is a good correlation between the two variables." and that "There is no direct correlation between the 50th percentile of both series." appear contradictory. To substantiate the findings, please present quantile-based correlations to verify whether the relationship holds only beyond the threshold. Also provide a clear rationale for setting the threshold at 400 mm.

Reply We thank the Reviewer for this comment. We agree that the two sentences highlighted in Section 3.3 were ambiguous and could be perceived as contradictory. The key point is that the precipitation–IPI relationship is better described as a non-linear threshold-type behaviour across all years. An important clarification is that the median value of annual precipitation refers to the full historical record, whereas "typical inundation conditions" are meaningful only when inundation occurs and are therefore better described using conditional statistics (e.g., conditioned on P>400 mm yr^{-1} or IPI > 0). Consequently, the precipitation level required to achieve typical inundation conditions can be higher than the unconditional median precipitation, which explains why the two "central" values do not coincide. To substantiate this threshold behaviour, we added rank-based (non-parametric) measures of monotonic association (Spearman's rho and Kendall's tau), which are appropriate for non-linear responses and non-Gaussian variables. Specifically, for dry years ($P \leq 400$ mm yr^{-1}) the precipitation–IPI regression is not significant (Spearman's rho=0.19 ,p=0.59 ,n=10 ; Kendall's tau=0.17 , p=0.52), whereas for wetter years (P>400 mm yr^{-1}) it becomes strong and highly significant (Spearman's rho= 0.93, p=1.9 * 10^-14, n=32 ; Kendall's tau=0.79 ,p= 2.8*10^-10). These regime-specific statistics confirm that the relationship holds primarily beyond the threshold, under wetter conditions. Regarding the choice of 400 mm yr^{-1}, we clarify that this threshold is data-driven and conservative. In the dataset, all years with IPI= 0 occur below P= 373,5 mm yr^{-1}, and for P<=400 mm yr^{-1} IPI remains very low (median =452). We therefore adopted 400 mm yr^{-1} as a rounded transition value separating a "no (or negligible) inundation" regime from a precipitation-controlled regime. The manuscript has been revised accordingly to remove ambiguity in section 3.3 specifically in lines 443-456.

Comment 11) Section 3.4 presents a climate-change analysis but lacks discussion. Please clarify which factor—changes in precipitation, temperature/PET, or model structure—drives the reductions in MFA and hydroperiod. If framed as future work, outline a brief quantitative plan to assess future IPI changes so the purpose of the section is clear.

Reply We thank the Reviewer for this comment. Section 3.4 is mainly descriptive and requires a clearer discussion of the mechanisms driving the projected changes in maximum MFA and hydroperiod. It is worth clarifying also that the climate-projection adopted as forcing in Section 3.4 were derived within the PRIMA LENSES project framework (see "Financial support" section at the end of the manuscript) and are used here as external forcing into WetMAT. Further details about the model used and its characteristics can be found in the text of the manuscript, revised following Comment 5) by Reviewer 2, as well as in the response to that comment. Based on Fig. 10, the most consistent climate signal across scenarios is the increase in mean daily temperature (Fig. 10b), which translates into a systematic increase in potential evapotranspiration (PET; Fig. 10c). In WetMAT, the representation of inundation dynamics is controlled by the balance between precipitation inputs and atmospheric demand. Therefore, the projected PET increase reduces effective water availability and provides a physically consistent explanation for the simulated reductions in both hydroperiod and maximum flooded area (Fig. 10d–e), with stronger impacts under RCP8.5. By contrast, precipitation shows high variability and comparatively smaller changes in statistical tendency (Fig. 10a), which mainly contributes to the spread/uncertainty of future projections rather than providing a single dominant directional driver. We also clarify the purpose of Section 3.4 and the role of IPI in the overall workflow. In Section 3.3, IPI is introduced as an operational composite indicator to translate hydrological outputs into a management-oriented variable, which is then used to support the subsequent step of estimating the additional water volumes required to achieve target inundation conditions (beyond precipitation-driven inputs). In other words, IPI in our framework is primarily a decision-support proxy that links inundation dynamics to potential management actions under present/climatological conditions. By contrast, Section 3.4 is intentionally framed as a climate-forcing sensitivity and scenario comparison of the primary outputs produced by WetMAT (maximum MFA and hydroperiod) relative to the baseline. The aim here is to highlight the direction and magnitude of changes in the fundamental inundation descriptors under RCP4.5 and RCP8.5 (and to discuss their climatic drivers), rather than to derive future management adaptations or quantify future additional water requirements. Extending the analysis from projected changes in hydroperiod and MFA to future water-demand estimates would require additional assumptions (future availability, allocation constraints, management targets, and feasibility of interventions) and a dedicated scenario-design framework (e.g., defining a future BAU pathway and alternative mitigation/adaptation options). This is beyond the scope of the present climate-change overview section, focused solely on showing how WetMAT can reproduce different flood projections under different initial climatic conditions. We revised the manuscript specifying the origin of the climate data used as input for the model (lines 477-489) as well as extending the discussion of climate scenarios to the effects on wetland dynamics modelled using WetMAT (lines 507-512).

Reviewer #2

Comment 1) The manuscript notes that the observed hydroperiod is derived from pixel-based remote sensing products, while the model—lacking explicit spatial representation—computes hydroperiod as the number of days with MFA greater than zero within a single “pond” conceptualization. This approach is acceptable considering the complexity of the processes and data limitations (low KGE, 0,54); however, I think this situation can be more explicitly discussed by adding two-three sentences in results and performance discussion section.

Reply We thank the reviewer for this comment. The different nature of the observed and modelled hydroperiod deserves clearer and more explicit discussion. The substantial difference between the hydroperiod definition commonly adopted in literature, typically derived from direct observations (when available) or pixel-based remote sensing, and the hydroperiod computed by WetMAT, which represents the marshland as a single lumped ‘pond’, implies that the two quantities cannot be directly compared. As a result, the corresponding KGE value is mainly affected by a structural mismatch rather than by model performance alone. Although the comparison was initially included as an extreme-case approximation in the absence of other data, it has been removed in the revised version of the manuscript in which calibration and validation (section 3.2) followed a different process not considering the hydroperiods for comparison, but only the annual MFA values were used. At the same time, hydroperiod remains an important WetMAT output because it constitutes one of the two components of the Inundation Persistence Index (IPI) introduced later in the manuscript. In any case, the section describing the model outputs (section 3.3), lines 409-412, illustrates the difference between the hydroperiod defined in the literature and the calculation of adopted in this study. The issue is revisited in the ‘Discussion’ section, which also addresses this limitation in data availability (lines 562-565).

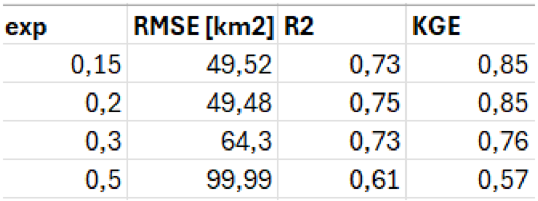
Comment 2) The calibration procedure is reasonable, but the workflow would benefit from a brief, consolidated description. Please summarize the calibration sequence by a stepwise procedure for the sake of transparency and reader’s comprehension.

Reply We thank the Reviewer for this comment. We agree that the previous description of the calibration process was not sufficiently clear and would benefit from a more transparent, consolidated workflow description. Following the suggestions provided in comments 6) and 7) by Reviewer 1 and acknowledging that the original calibration relied on a limited number of runs relative to the number of parameters to be calibrated, we revised the calibration strategy. Specifically, we increased the number of simulations from 100 to 2100 and adopted multiple performance metrics to support model evaluation and selection of the best vector of parameters (KGE, R2, and RMSE). The revised Section 3.2 now provides a clearer stepwise calibration/validation sequence, and Fig. 5 has been updated accordingly to schematically summarize the workflow for the reader.

Comment 3) Regarding the computation of MFA, the manuscript uses an empirical relationship linking marshland area and maximum flood depth (e.g., MFA=A_m·MFD^0.2). Please provide more explicit clarification why the number 0.2 has been chosen.

Reply We thank the Reviewer for this helpful comment. We note that Reviewer 1 raised a closely related point regarding the empirical relationship between MFA and MFD. Accordingly, we revised the manuscript to better justify the use of this relationship within the modelling framework and clarify the rationale for selecting the exponent 0.2, which is now supported by a dedicated sensitivity analysis exploring alternative exponent values. For completeness and to avoid repetition, we report below the detailed response provided to Reviewer 1 on this issue.

‘We thank the reviewer for raising this point. In Eq. (6) we represent the relationship between mean Marshland Flooded Depth (MFD) and Marshland Flooded Area (MFA) by means of a simple power law. This choice of functional form is consistent with previous studies on shallow wetlands and low-relief basins, where compact power function depth–area or depth–storage relations have been shown to provide an adequate approximation of the underlying bathymetry (e.g. Hayashi and van der Kamp, 2000, subsequently adopted and extended in later works such as in Minke et al., 2010 or in Chandler et al., 2024). In this sense, the exponent in Eq. (6) is not arbitrary, but reflects the adoption of a standard, parsimonious representation of the MFA–depth relationship for shallow wetland systems. In the case of the Doñana marshland, we did not have access to direct, co-located measurements of flooded areas and water depth that could be used to derive from ourselves an empirical MFA–depth curve. Therefore, we constrained the exponent using published estimates of flooded area and mean water depth for the Doñana marshes reported by Leiva-Piedra et al. (2024). Rather than adopting their more complex formulation, which is not directly suitable for inclusion in our simple daily water-balance model, we used their results as independent constraints on the order of magnitude of depth and MFA. By fitting a power law curve to reproduce these orders of magnitude over the mean depth range of the marshland, we found that exponents in the vicinity of 0.2 provided a satisfactory match. For the sake of parsimony and to avoid over-parameterisation and identifiability issues, we then fixed the exponent at 0.2, while calibrating only the other model parameters. As requested by the reviewer, we performed a local sensitivity analysis in which the exponent in Eq. (6) was varied within the range 0.15–0.50. When comparing the simulated time series obtained for each tested exponent with the observed data, the exponent 0.2 provides the best overall performance, yielding the most favourable values for all three metrics considered (RMSE, R^2 and KGE). The results of the sensitivity analysis are shown in the table below. In the revised text of the manuscript, Eq (6) is better described in lines 199-201.’



exp	RMSE [km2]	R2	KGE
0,15	49,52	0,73	0,85
0,2	49,48	0,75	0,85
0,3	64,3	0,73	0,76
0,5	99,99	0,61	0,57

Comment 4) Another issue raised in my mind is the discussion of management options. For the Doñana application, the manuscript states that river inflow and direct groundwater recharge can be considered negligible, and that the system is largely disconnected from such inputs due to hydrological modifications. Later, groundwater reallocation is mentioned as a possible option in drought years. While this may be intended as a broader management consideration, the current phrasing could be read as inconsistent with the modelling assumptions. Please clarify this statement by the remedies considered, for example a transfer from another basin or limitation to agricultural withdrawal of groundwater etc.

Reply We thank the reviewer for highlighting this point. We agree that the original phrasing could be read as inconsistent with the modelling assumptions. In the application to the Doñana case, river inflow and direct groundwater recharge are considered negligible within the simulated vertical water balance, consistently with the current hydrological disconnection of the system from the surrounding water bodies. The sentence on "groundwater reallocation" was intended as a broader management consideration, i.e., an external intervention not represented in the present model configuration. We have revised the manuscript to make this explicit and to clarify the type of remedies that could be considered during drought years, as suggested from reviewer, such as limiting agricultural groundwater withdrawals and reallocating part of the saved volume to controlled wetland inundation, and/or supplying water from external sources (for example transfers from nearby basins or releases from adjacent artificial reservoirs). These options are presented as potential management measures that would require additional assumptions and a dedicated scenario design which is beyond the current model scope. The manuscript has been integrated according to the reviewer comment in lines 457-462.

Comment 5) For the climate-change scenario analysis, to strengthen the confidence in the scenario results, I suggest adding a single sentence specifying the source of the climate data (e.g., product or model ensemble), its spatial support (station vs grid), and whether any bias correction/downscaling was applied.

Reply We thank the reviewer for the comment, which prompted an addition to the manuscript. The climate data used in the paper, relating to future projections, were derived from the European LENSES project, which is cited in the "Financial support" section of the paper. In particular, these data are available in the deliverable "D7.2 – Climate projections and risk assessment", which can be found in public access at the following link: https://www.lenses-prima.eu/outcomes/list-of-deliverables/wp7-nexus-operationalization-for-sdg-delivery/. The information requested by the Reviewer has been added to the text at the beginning of Section 3.4 (lines 477–489) and here reported for completeness:

"The climate projections used as forcing in this section were obtained from PRIMA LENSES project datasets and here adopted as external inputs for the model. Daily precipitation and 2-m air temperature data were taken from the Copernicus Climate Change Service (C3S) and, specifically, from the CORDEX regional climate projections database (C3S, 2019), which provides dynamically downscaled simulations from Global Climate Models (GCMs) to Regional Climate Models (RCMs) under the EURO-CORDEX framework. Within the LENSES project, an ensemble approach was adopted in which multiple RCMs driven by different CMIP5 GCMs (Coupled Model Intercomparison Project Phase 5) were used to represent climate-model uncertainty. For the Doñana domain (with coordinates 37.8–36.8°N and 6.9–6.0°W used), the dataset has daily temporal resolution and 5-km spatial resolution for both precipitation and mean temperature. Importantly, for these two variables a bias-adjusted EURO-CORDEX dataset was adopted: the daily precipitation and temperature ensemble was bias-adjusted using EFAS-Meteo as reference and a bias-adjustment method developed by the Swedish Meteorological and Hydrological Institute (SMHI)."

Comment 6) Please correct the word "legenda" as "legend" in figure 3

Reply We thank the reviewer for the suggestion. The word ‘legend’ has been corrected in Figure 3 as well as in Figure 2 in the manuscript.