# Editor comments

Editor comments:

The comments from both reviewers are highly important and substantial. Please address all of their comments except the second reviewer's concern about lack of calibration and validation of model against experimental data. I will accept the manuscript if you can address all other comments and as long as you can strongly justify that further calibration and validation is not necessary.

Response: Thank you for considering our paper and for guiding us on what we should focus on. We have made all edits suggested by reviewer 1. We have also addressed most of the comments from reviewer 2. On the concern of calibration and validation, we have provided the necessary citations.

# Reviewer 1 comments

Reviewer #1:

Title: Risk-based evaluations of competing agronomic climate adaptation strategies: The case of rice planting strategies in the Indo Gangetic Plains  
  
This study employs a crop modeling approach to evaluate diverse climatic adaptation strategies for rice and wheat farmers in the Indo-Gangetic Plains (IGP). The Methodology and Results sections are presented clearly. The authors also demonstrate considerable effort in this study. However, some points need to be addressed to enhance the comprehensibility and applicability. Below are specific comments.  
  
1.The uploaded includes the draft of the manuscript. Please submit the latest version for review.

Response: We uploaded the latest version as revised from comments from the reviewers.

2. A few suggestions on citations are suggested.

1)When citing multiple sources simultaneously, it is better to place them chronologically (L64-65, L248).

Response: Thanks for the suggestions. We have edited accordingly.

2)Using "and" to connect authors' names is more appropriate than using a semicolon (;) when citing multiple sources (L73).

Response: We have edited accordingly.

3)When the number of authors exceeds two, "et al." can be used for simplicity (L49-50, L70-71, L72-73, L90-91).

Response: We have edited accordingly.

4)The citation at the end of sentence should be enclosed in brackets such as (IPCC, 2022) in L42.

Response: We have edited accordingly.

5)In the APA style, in-text citations consist of the author's name followed by the year in brackets, such as Meyer (1977) (L158, L255, L455).

Response: We have edited accordingly.

6)A comma is suggested between the author's name and published year (L403, L410, L445).

Response: We have edited accordingly

3.Regarding the study site, it is suggested to provide the rationale.

Response: In the introduction (L50 on-wards), we explained that the IGP is one of the most severe hit regions with climate change and its dominant cropping system is under threat which requires better risk-based decision making.

4.Please spell out the abbreviations for the first-mentioned terms, such as IGP for Indo-Gangetic Plains, to enhance the readability. (L22, L108).

Response: We have edited accordingly for IGP and APSIM abbreviations.

5.It is suggested to refrain from using first-person pronouns (i.e., our, we) to maintain a neutral and objective tone (L143, L216, L225).

Response: We thank the reviewer for the comment. We have edited accordingly throughout the manuscript.

6.Several wordings can be revised to improve the clarity and comprehension.  
1)The phrase "so far" can be replaced with a more formal term, such as "until the present" or "up to date" (L89).

Response: We thank the reviewer for the suggestion. We have edited accordingly with the phrase, “Up to date, ”

2)The phrase "more" seems irrelevant and can be removed (L144).

Response: Thank you. We have deleted the word “more.”

3)A hyphen is suggested to add for the phrases "risk averse" (L13, L18), "Indo Gangetic", and "long term" (L102).

Response: We thank the reviewer for the suggestion. We have added hyphen for the suggested words throughout the manuscript.

7.There are several ambiguous statements that need to be clarified.  
1)This statement "…whether they are worse, better or worse, and better than…"? is unclear (L302-303).

Response: We have edited accordingly to “worse than, better than, and not different”.

2)It is suggested to add a simple equation such as "Return/profit=Yield× Cost" (L138-142)

Response: We have edited accordingly.

3)The parameters G, F, and Q need to be clarified. Please briefly define these terms (L173).

Response: We discussed that these are hypothetical cumulative distribution functions.

4)The abbreviations for the Scenario Numbers or Columns (S0-S6) should be provided for clarity (L264-275). Here is an example: "…long duration rice varieties at the monsoon onset (S3) …"; in L266, "For farmers practice (S0), …"; in L268, "Table 3: … with fixed long as baselines (S1)"; for L273-275: "Among the Scenario number S3-S1, planting…."

Response: We thank the reviewer for these excellent suggestions to help readers in connecting the tables and the text. We have edited all parts of the paper where the scenarios are referred to.

5)The method for deriving the value of 78% from Table 5 needs to be explained in detail (L324).

Response: We thank the reviewer for spotting the error. We have edited to refer to 89% which is the right number in table 5.

6)Please check the figure number (Figure 6 or Figure 7) in p. 65.

Response: We thank the reviewer for catching this error. We have edited to refer to Figure 7.

# Reviewer 2 comments

Reviewer #2:

Review report  
I have analyzed the "Risk-based evaluations of competing agronomic climate adaptation strategies: The case of rice planting strategies in the Indo Gangetic Plains" manuscript. The manuscript used computational spatial ex-ante and second order stochastic dominance approaches for risk-based evaluations. The manuscript requires revision to meet the publication standards.

Response: We thank the reviewer for taking the time to read and carefully provide substantive suggestions. We hope we have managed to address most of the comments that are within the scope of the paper while acknowledging the many limitations of our work which we have outlined in the limitations section.

The major shortcoming are:

\* This manuscript lacks validation where the proposed results are compared against measured

Response: We thank the reviewer for the comment. But as we explained in the last review, the main reason of using crop simulation results in this context is that there are no rice-wheat system long term trials at many locations spanning the IGP that test performance at different dates over 30 years to analyse the risk aspects.

\* Calibration and validation of APSIM is not sufficient, heavily dependent on the previous paper

Response: We thank the reviewer for the comment. We have also indicated in the paper that we are not doing new APSIM simulation, we are simply using the results from already calibrated and validated model to demonstrate the value of conducting the risk assessment as an additional analysis instead of using means, inter-temporal variability or statistical significance tests as is conventionally done and as done in the previous paper.

We want to emphasize as well that this comment was well addressed in the previous review in which we stated that calibration and validation of APSIM were reported in the previous paper and that for this paper the focus is on the risk-based evaluation which can be conducted for any crop model results. The APSIM data is just used as case study of modelled results anyone can produce and we don’t think its helpful for the readers to rehash the calibration details in the previous paper. If the reviewer has another crop model in mind for the planting date scenarios, we could have used it as well though that is beyond the scope of the current paper.

\* The mapping work is coarse and lacks 'uncertainty' mapping

Response: We thank the reviewer for the comment. The computational model is about using intertemporal uncertainty through second order stochastic dominance to produce robust estimates as such there is no uncertainty mapping needed. Uncertainty analysis is already part of the model and we reported quantiles in the tables, final results in the figures and in figure 6, we reported the willingness to pay bounds which are also reflective of uncertainty. In locations where we can’t give an exact recommendation are the ones where the uncertainty in the estimates are too high to make a robust recommendation.

\* The results are presented in a straightforward way but lack in-depth statistical analysis and critical interpretations of the data.

Response: We thank the reviewer for the comment. We did not attempt to do any inferential statistical analysis in this paper. This paper uses an optimization model which is an approach different from the statistical approach. Nonetheless, if one considers stochastic dominance as a statistical concept related to cumulative distribution functions, then our analysis is much more in-depth than most statistical analyses in that it focuses on distributional comparisons (not just mean comparisons of the strategies) and is linked to the theories of decision making under risk.

\* There is a risk of circular reasoning as the paper relies too heavily on datasets or methods from the same research groups published already

Response: While we thank the reviewer for raising the worry of circular reasoning, we don’t understand what the reviewer means by circular reasoning in the context of this paper. The main contribution of the paper is on the method of risk evaluation regardless of the data source or crop model used. We use the data and application we are familiar with to demonstrate the risk-based evaluation approach. Any datasets or crop models can then be used. The crop model choice is trivial to the contribution of the main paper.   
  
**Other comments:**  
  
**Abstract:**  
\* The abstract lacks specific results, based on revised version (line 24-30), see what the results are??? Quantitative data or specific percentages is required to enhance clarity. The authors did not address this comment as was identified in earlier review also. This abstract promises a framework but does not clarify what sets this framework apart from existing methods in a substantial way?

Response: We thank the reviewer for the comment. We have added percent of area for which the scenarios explained in the abstract are preferable based on the computational analysis. This is 38% using the state recommended planting dates and 22% with long duration rice varieties at monsoon onset with or without supplemental irrigation. The other scenarios are discussed in the appropriate results section (3.2.3).

\* Rewrite Line 12-14: "However, choosing recommendations amongst competing levels of yield and yield stability is not straightforward and need to cater to farmers that are risk averse - especially financially." This sentence is not straightforward.

Response: We thank the reviewer for pointing the ambiguous sentence. We have deleted the sentence to avoid this confusion among the readers.

\* Line 17-18: "This framework allows development of climatic risk proof recommendations such that even risk averse farmers would find it profitable to adopt that strategy." The phrase "climatic risk proof" is misleading, as no strategy can be entirely risk-proof. This could overpromise the paper's deliverables.

Response: We thank the reviewer for the comment. We have added “partially”” climatic risk proof recommendations to reflect that we may not capture some extremes in the APSIM model. In some parts, we have replaced with “robust” recommendations.

**Introduction:**  
\* The introduction section is not sufficient to describe the gap in the existing literature. It describes the problem and general approaches but fails to highlight the precise deficiencies that this study aims to address.

Response: We have reorganized the introduction to address these concerns. We believe we have pin-pointed the precise deficiencies in the existing tools which do not consider the risk aversion of the farmers when interpreting crop model results. This has been explained clearly in the introduction.

*“This paper contributes to two strands of literature. The first strand of literature is on stability analyses of agricultural technology benefits based on ex-ante cropping system assessments (Urfels et al., 2022). Montes et al. (2023) used inter-annual standard deviation to analyse the stability of the planting date scenarios. Urfels et al. (2022) used deviation from the mean caloric yield for each of the years when a shock occurred as a measure of yield instability. These measures of yield stability, while a step better than mean comparisons, they do not consider robustness of the optimal decision to risk aversion of the farmers. In addition, these measures do not consider higher order moments beyond mean and variability that may matter for distributional comparisons. In addition, we argue that mean comparisons do not consider the trade-offs for achieving highest returns and reducing uncertainty. Up to date, most studies address uncertainty by, for example, using model ensembles or Monte Carlo simulations (Iizumi et al 2009; Rosenzweig et al., 2013). But these approaches only allow for establishing confidence in the mean and variation around it and do not adequately take into account the implied risks to farmers.*

*These limitations are addressed in the second strand of literature which focuses on the spatial risk assessment of economic benefits of agricultural innovations (Nalley & Barkley, 2010). This literature attempts to optimize on the trade-offs of achieving the highest return and lowest uncertainty therefore allows one to choose strategies that are more robust. Using modern portfolio theory (Markowitz, 1959) which suggests that a strategy to maximize average returns may be a suboptimal strategy, (Nalley and Barkley, 2010) used a mean-variance analysis to optimally select wheat varieties that achieve highest return and lowest risk. This strategy still suffers from the limitation of using a subset of moments (mean and variance) of the distribution. The stochastic dominance approach was developed to resolve these concerns in selecting robust strategies (Levy, 2016).”*

\* Provide more detailed justification of the chosen methodology, comparing it to other potential methods and explaining why it is preferred over other simple and easier methods?

Response: We thank the reviewer for the comment but as shown in the except of the introduction above, we have compared our approach to the mean-variance risk approaches. We have also compared them to the conventional methods used in assessing intertemporal yield stability. Our approach adds the risk aversion consideration and distributional assessments which are not captured well with the simple methods.

\* The manuscript briefly described computational optimization and stochastic models and lacks depth description of how these models were calibrated and validated against actual measurements.

Response: This is trivial and as we have belaboured this point in the paper and in the revisions that the APSIM model was from another paper and that we could have used any other crop model in any other country to demonstrate the methodological contributions to the field. In the methods section, we provided a hypothetical example to demonstrate how by using the link between second order stochastic dominance and risk aversion theories, the computational results provide a clear cut assessment of what farmers would find preferable.

\* Line 41-43: The connection between 'climate change's impact on agriculture in low and middle-income countries and the specific focus of this paper is articulated'. Then, it jumped too quickly into the specifics without setting up a clear connection.

Response: We thank the reviewer for the comment. We have now reorganized and edited the paragraph to show the clear connection to the methodological contributions of the paper.

\* Line 41-45: The introduction fails to contextualize the research within the broader field adequately. Actually, this should have been the 'methodology comparison' paper. The specifics of how the study addresses the research gaps mentioned are missing.

Response: We thank the reviewer for pointing this out, but we have discussed the comparison with prior methods in the latter part of the introduction as shown in the introduction except above.

\* Line 52-54: "The main aim of this paper is to develop a climatic risk proofing framework for making recommendations on rice sowing strategies in the IGP using evidence from crop growth models." The terminology "climatic risk proofing" again overstates the capabilities of the framework, provide more accurate framework 'name' to mitigate or manage climatic risks.

Response: We thank the reviewer for the suggestion. We have edited the text to focus on developing a framework for managing climatic risk through robust recommendations (or partially risk proof recommendations).   
  
**Materials and methods:**  
\* The description of the "golden section search algorithm" and "second order stochastic dominance" is overly technical without sufficient explanation for readers unfamiliar with these terms. Mentioned "Golden section search algorithm" without explaining why this method was chosen over others. A comparative analysis with other potential methods offers more credibility to the choice. \* Clear, precise language is critical in scientific writing to avoid misinterpretation. Throughout the manuscript, there is excessive use of jargon that should be simplified.

Response: We thank the reviewer for the comment. We have attempted to add explanations for every complex word we have used as suggested including golden section search. This algorithm is the simplest and though computationally intensive it is accurate at identifying the quantiles and value at which one strategy’s area below the cumulative distribution function is lower or higher than the other (defining second order stochastic dominance).

\* The study lacks validation where the proposed results are compared against measured.

Response: We thank the reviewer for the comment. We have also indicated in the paper that we are not doing new APSIM simulation, we are simply using the results from already calibrated and validated model to demonstrate the value of conducting the risk assessment as an additional analysis instead of using means, inter-temporal variability or statistical significance tests as is conventionally done and as done in the previous paper.

\* The manuscript does not clearly state the assumptions in the models, such as the independence of climatic events or the homogeneity of soil types across the study area, which are crucial for interpreting the results accurately.

Response: We cited the original paper that reported all these things. For example, the appendices to Urfels et al (2022) already provides a detailed characterization of the study area. In the paper, we have also discussed that the APSIM model took as its inputs the soil and weather parameters for the corresponding years and referred the reader to this prior paper for details. This allows us to focus on the contribution we are making to the field on risk evaluations. We have also provided links to the code for the APSIM model and computational model in section 2.3 so that those interested can understand the procedures followed.

\* How is the uncertainty of the mapping work? The authors are unable to present the uncertainty such as presented by 'Poggio, L., De Sousa, L.M., Batjes, N.H., Heuvelink, G.B.M., Kempen, B., Ribeiro, E., Rossiter, D., 2021. SoilGrids 2.0: Producing soil information for the globe with quantified spatial uncertainty. SOIL 7, 217-240. <https://doi.org/10.5194/soil-7-217-2021>' Present uncertainty map including robust statistical analysis including uncertainty measurements.

Response: Our approach is about understanding uncertainty and we show the bounds of the WTPs. Stochastic dominance approach is about understanding uncertainty anyways therefore all our maps are essentially expressing strategies for which risk aversion to the level of uncertainty in the strategy doesn’t matter.

\* Line 131-133: "In this section, we showcase and explain our risk-assessment framework." This line could be more specific about what aspects of the risk-assessment framework are novel or distinct from existing frameworks? What makes this framework different or better than existing ones? More detail is needed here.

Response: We thank the reviewer for the comment. We have added text to extend this sentence in explaining the approach we use in relation to the competing method of risk assessment.   
  
**Results and discussion:**  
\* The results are presented in a straightforward way but lack in-depth statistical analysis (e.g., confidence intervals, sensitivity analyses).

Response: We thank the reviewer for the comment. Our interest is in a distributional comparison. The statistical assessments were already done using the conventional methods in Urfels et al (2022) and Montes et al (2023). In this paper, we focus on the distributional comparisons. In the tables, we presented the results using quantiles and using the second order stochastic dominance interpretation to decide which pixel gets which strategy based on a computational model not a statistical model.

\* This manuscript lacks critical interpretations of the data and presents a one-sided view of the findings. Also, some claims are not fully supported by the data presented.

Response: We thank the reviewer for the comment. This is a very generic statement. We wished the reviewer was pinpointing what the other view is and how he/she thinks our results are wrong.

\* The figures and tables are under-explained in the text, making it difficult for readers to understand how they are related with the findings?

Response: We thank the reviewer for the comment. We have attempted to add edits where appropriate.

\* Unlike Newport et al. (2020), which utilizes detailed maps and regression models to demonstrate the impact of different sowing dates on wheat yields, the current manuscript lacks comprehensive visualization that could enhance understanding of the spatial and temporal dimensions of the data.

Response: While we thank the reviewer for expressing this opinion, we believe with 7 figures that are all spatially represented and with sub-plots of about 6 in each, we have provided enough dimensions (yield, revenue and profits for both and rice) to the computational results. As compared to Newport et al (2020), our paper uses a computational or optimization model not a statistical model.

\* There is a lack of critical discussion comparing the proposed strategies with existing practices beyond the theoretical simulation and coding models. Insight into how these strategies could be implemented practically, considering local socio-economic conditions, is missing.

Response: We thank the reviewer for pointing this out. We have edited parts of the limitations and future research to explain that this indeed a challenge of using results from any crop growth model.

\* Line 243-247: The results are presented without adequate support from graphs or charts. The authors should compare their results critically with others considering the result of this and other studies.

Response: We have compared the results with Wang et al (2022, 2023) who used EPIC crop growth model.

\* Line 249: "Our results are in line with previous analyses..." lacks specificity. Which previous specific analyses and results and how? specify.

Response: We thank the reviewer for the comment. We have deleted the statement as it will confusing the readers to put specific results when introducing the results the discussion.

\* Line 243-247: The manuscript broadly mentions "quantitative evidence" without presenting specific statistical analyses that could substantiate the claims made.

Response: We thank the reviewer for the comment. We have added percentages to explain but we believe the numbers in all the tables and graphs are quantitative evidence. As explained already as well, this is an optimization model not a statistical model though the uncertainty evaluations are similar.

\* Line 255-256: "While the results generally corroborate the findings from the previous crop simulations reported in (Urfels et al., 2022)..." Again, the authors did not respond the earlier comment adequately. Please specify what are the differences and novelties of this paper than Urfel et al., 2022??? There is a risk of circular reasoning as the paper relies too heavily on datasets or methods from the same research groups published already without external validation.

Response: We thank the reviewer for expressing this concern of circular reasoning. We have presented evidence from Wang et al (2022) and Wang et al (2023) who used EPIC crop growth model and are not in our close or distant network. If we can make advances in other methods using the same set of data, we don’t think this has risk of circular reasoning. The similarity is because the data is the same and though using different methods there are similarities in addition to the new results from the new method.

\* Line 416-418: "However, in the Northern and Southern parts of the Western and Middle IGP, we get produce substantially different outcomes - but also indicating that in these areas multiple rice planting strategies perform equally well..." This sentence is confusing and needs rephrasing for clarity. It should also clarify what "substantially different outcomes" means quantitatively and qualitatively???

Response: We thank the reviewer for the comment. We have deleted the sentence as it was indeed confusing but not adding much to the discussion of the results.   
  
**Conclusions:**  
\* Some claims have not been fully supported by the data presented, causing the conclusion overstated. As the findings are context-specific, needs data well interpreted for adequately supporting the finding.

Response: We thank the reviewer for the comment. We have addressed the concerns by providing specific estimates of the areas that are recommended to adopt each of the scenarios.

\* Throughout the manuscript, the terms "climatic risk proof" need reevaluation to ensure they do not overstate the findings.

Response: We have edited accordingly to reflect partial risk proof or simply robust recommendations.   
  
**Figures tables:**  
\* As commented in earlier version, APSIM model calibration process has yet to be clarified.

Response: APSIM calibration is the appropriate papers that we have substantially cited. If you feel those were inadequate then write a letter to the editors or authors of those published models not for this paper because the focus is on methods for assessing risk.

\* Pixel size of the figure is very coarse. What does it tell and what can be understood providing mapping result over the cities, forest and non-rice areas?

Response: As explained in the last review, mapping choices depend on the paper’s focus. There are thousands of GIS studies (e.g., soil grids) that map results of yields or soil assessments by state or district or pixel. This does not in any way mean everyone in that jurisdiction will get those yields or that there are not buildings. This comment doesn’t apply to our paper. Our product is a generic product so anyone can use their own appropriate mask but its not the focus of the paper. We are sharing the data and code in the links in the methods section so anyone can use it for their purpose.