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Reply on RC1

Marthe L. K. Wens et al.

Author comment on "Education, financial aid, and awareness can reduce smallholder farmers' vulnerability to drought under climate change" by Marthe L. K. Wens et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-216-AC1, 2022

Dear reviewer

Thank you for your kind words and interest in the manuscript. Since the model is not fully validated, the presented application can be seen as an proof-of-concept decision support tool. We agree that this should be more clear in the introduction and conclusion, and propose to add the following: (Introduction): *"While ADOPT will be subject to additional validation steps in order to more accurately and precisely predict future drought risk, we elaborate the potential of this proof-of-concept model by showcasing the trends in drought risk under risk reduction policies and climate change for a case study in semi-arid Kenya."* (Conclusion): *"While we present a proof-of-concept rather than predictive model, the results improve the understanding of future agricultural drought disaster risk under socio-economic, policy and climate trends."* We would also extend the discussion section to state more clearly the limitations of the model.

To adress your general comment 1, regarding the ODD description, we will fully revise the ODD+D in supplementary (Given that we designed an agent-based model, we followed the ODD+D protocol of Muller et al 2013 – which is an extension of the ODD protocol of Grimm et al. 2010.) so as to incorporate your suggestions on incorporating details on the weights, parameters, formulas used and their justifications. For example, we will give more detail in the submodels section, adding the PMT equations and rationale of the decision model (the model-ification of the Protection Motivation Theory which was explained in Wens et al 2019 will be added to ODD+D II.ii in the section about how agents make decisions). Since the crop-water model AquacropOS is an existing, open-access model, published by Foster et al.2017, only its basic principles (and the used parameters) are explained while the details on its equations should be sought in the article of Foster et al.

To adress your general comment 2, regarding the validity of generalizing the model from current to future scenarios, we would like to remark that indeed, AquacropOS has been parameterized to cover the future climate conditions – other studies have used the model to estimate current and future maize yields (Wang et al. (2015) A review on the research and application of Aquacrop Model; Dissa and Yan (2022) Evaluating climate change impact of rainfed maize production yield in Southern Ethiopia; Hsiao et al. (2009) Aquacrop – The FAO crop model to simulate yield response to water: III. Parameterization

and testing for maize. Irmak et al. (2022) Multi-model projections of trade-offs between irrigated and rainfed maize yields under changing climate and future emission scenarios; Bwambale and Mourad (2022) Modelling the impact of climate change on maize yield in Victoria Nile sub-basin, Uganda; Dale et al (2017) Climate model uncertainty in impact assessment for agriculture: a multi-ensable case study on maize in sub-Saharan Africa).

Regarding the decision making model, the use of a general, existing theory (Protection Motivation Theory (PMT) (Rogers, 1983; Rogers and Prentice-Dunn, 1997)) for the decision module supports us in assuming the processes behind the decision making are universal and the factors of relevance not to change in the near future (Schrieks et al. (2021) Integrating Behavioural Theories in Agent-Based Models for Agricultural Drought Risk Assessments). We agree with your comment in that, in a totally different society, other parameterisations or other theories might better explain the decision behaviour. Therefore, we only evaluated a limited amount of "future" time given the influence of unavoidable black swan effects - and thus we assume the parameters influencing decisions and adaptive behaviour do not change. Withing the model, there is off course a certain transiency in the values of the factors that affect decisions: every year these values are updated, altering the intention to adopt new measures. They also vary between households (which is why we chose or an ABM approach). Since there is a remaining uncertainty regarding the relative weights of the factors influencing decisions, we did an sensitivity analysis altering these weights (similar to the one in Wens et al 2019). This analysis shows that our model is robust: the variation introduced by the policy and climate scenarios is larger than the model uncertainty in the model parameterisation. To address your comment within the manuscript, we propose to add the following to the discussion: *"Lastly, the model application does assume no shifts in the processes underlying weather and human decision making: both the synthetic future weather situation and the decision making processes are based on past observations. To avoid the effect of systemic changes and black swan effect, only 30 "future" years are modelled. "*

To adress your general comment 3, regarding economic relationships in the model, we agree that this was not fully clear in the manuscript nor ODD+D description and propose to clarify this in the manuscript. We would like to highlight that in our analysis, indeed, only an internal market is considered, with fluctuations responding to local changes in demand and supply. In the revised ODD+D, the following will be written: *"The market is influenced by local production and consumption, which results in a variable maize price depending on the balance between supply and demand. In the presented case study, we consider relatively isolated areas, less subjected to globalized market systems: maize price is variable following the total amount of locally produced maize to replicate the observed price volatility (with minimum and maximum prices derived from FEWSnet) during years of reduced production."*

No restrictions on labour availability are given, but households with a larger family size have a higher self-efficacy (as children in this region often help on the land). Labour costs in terms of time are unfortunately not included as no clear conclusions could be drawn from the questionnaire on this topic. The price of the measures is derived from their (perceived) costs based on this questionnaire, and is included in the adaptation-cost factor. (both self efficacy and adaptation cost are part of the coping appraisal factor of the PMT). With respect to the "advantages" we indeed refer to positive effects on yield – but given the protection motivation theory, a certain amount of risk appraisal should be there in order to consider the measures, even if they are cost-effective on the short term and could thus be seen as a general agricultural investment. However, most of them are cost-effective because of the existence of droughts (if we exclude drought years and look only at their impact on crop yield in normal years, they would not be cost effective).

"receiving food aid" as backup is not considered in the agents' risk appraisal (in the model based on their memory of past shocks) but on the field, from the interviews it was clear that many find the current aid insufficient so we feel it is ok to assume this also does not greatly affect their risk perception (although more research on this could be very interesting). In the model, this aid does not increase households wealth (it rather avoids households to have negative wealth or to "die" altogether).

To adress your general comment 4, regarding uncertainty, we agree that it is needed to better describe what was varied through the model runs. We would like to note that indeed, we did account for all three: each run is starts with a random initialisation of the starting synthetic population, and also the random numbers drawn each timestep for each household, is not controlled (they are drawn based on the random-float and random-normal algorithms of Netlogo https://ccl.northwestern.edu/netlogo/bind/primitive/random-float.html#:~:text=If%20you%20want%20to%20generate,4%20%2B%20random%2Dfloat%203%20. The variations in the weights are systematic to allow for a maximum spread. Both are described in Wens et al 2020 "Through a systematic sensitivity analysis on the relative weights of the factors, which also includes variations in model initialisation and in the stochasticity that determines individual adaptation decisions, both the average effect of individual adaptation decisions and its standard deviation are analysed (Wens et al 2020)." Moreover, since we used an agent set of 1000 households (based on the characteristics of a survey among 260 households); different draws did not have large differences. The sensitivity to the assumptions about adaptation behaviour includes some variability, which is clearly represented in Fig. 7 and Fig.6 (not in Fig5 because this would render an unreadable figure). We propose to explain this more clearly in the ODD+D of a revised manuscript and in the figure descriptions.

To adress your general comment 5, regarding the agent sampling, we again agree this was not well described and will detail this in a revision of the ODD+D part on Initialization (III.ii): "*Correlations are present, but the distributions are assumed normally distributed (given the not so large sample of 260 households) except the initial household assets which are based on IFPRI data (table S1).*" If we wanted more empirical households, we could have used the dataset of 260 households to directly initialise 260 agents (which is often done in literature), but this method risks overfitting. We would like to add this as suggestion to the discussion: *"Another improvement to the model could be to, instead of initializing the model from distribution functions based on frequencies in the empirical data, directly sample this empirical household survey data (Wens et al 2020) for the creation of a set of agents (households) with different characteristics. Such one-to-one data-driven approach is similar to microsimulation and gaining popularity among ABMs (Hassan et al 2010).*"

To adress your specific comments, we propose to alter the following in a revised manuscript:

L42: we would specify this in the text: *"Uncertainties in adaptive behaviour are often addressed by using different fixed, exogeneous adaptation scenarios, but…"*

L44: We see bounded rational as context-dependent intuition and reasoning bounded by beliefs, perceptions, and emotion (similar to e.g. (Kahneman & Tversky, 1979)), and therefore would like to stick to this term

L52: We agree that novelty should not be the main focus of the paper so would remove the use of "innovative" in the manuscript.

L58: We agree this is a confusing wording and would change this to: *"The PMT suggests that the intention to protect (in this study, the farmers' intention to adopt a new adaptation measure) is motivated by a persons' risk appraisal (their risk perception, experiences of risk events in their social networks…) and the perceived options to cope with risks."*

L70: We agree this is a bit vague and would remove it. We could assume this model works for similar communities and propose to discuss this in the discussion section.

L82: We agree this was not clear and would rephrase it to make clear that here were refer to the subsistence farming in the region (growing crops sufficient only for one's own use, without any surplus for trade)

L104: We agree this should be clarified as this concerns indeed AquacropOS and would adjust it in the text

L106: We thank you for remarking this misleading prasing, and would change it to:*" The adaptive behaviour of the farm households (agents) is modelled based on the Protection Motivation theory (PMT, Rogers 1975), which was derived as promising in an earlier study (Wens et al, 2020) and includes multiple relevant factors that drive the observed behaviour of farm households (Wens et al 2021)".*

L109: We agree this requires more detail and propose to elaborate this in chapter 3.1 and in the ODD+D III.ii

L130: Farm income is seen indeed in a limited way as only income from excess harvest sold on the market. The poverty line is fixed indeed; but food prices react to supply shortages. this allows comparing past and future poverty in a more simplistic ways. We would like to add these clarifications regarding income and food price variation to the ODD+D.

L138: We see drought risk is a complex phenomenon that includes multiple direct and indirect impacts. While the three factors can be seen as indicators, we feel they rather approximate some aspects of risk, hence the word choice.

L151-160: We agree this is too vague. It is indeed the former (the daily values of the past are altered by adding / substracting the 10%/15%), and we know it is quite biased (as this ignores the increase in variability) - it is a very simplistic way to create synthetic data). However, we wanted to keep the day-to-day and dekad-to-dekad correlation and variability as this is important for AquacropOS while avoiding gridded data that has includes an averaging effect (important for rainfed agriculture). We do not claim that these scenarios have a certain known probability of occurring; it is rather a "what if the same happened under warmer / wetter /… conditions". We will rephrase it as follows: *"These trends were added to time series of 30 years of observed data. While this approach does not account for an increased variability, it allows to simulate the observed day-to-day, week-to-week and month-to-month correlations and variability under changing mean temperature and precipitation values."*

Fig5: We propose to improve the graph by providing the shorthands of the interventions in the text above, and the resulting variability in the text describing the graph. We agree the sum of measures does not tell all. Nor would the number of households with at least one measure, or the number of households with the most effective setup – rather it is a combination of these that is interesting. Since It is impossible to visualise all, this graph

only shows the sum, but the table in appendix B gives the breakdown per type of adaptation measures. Data on the amount of households with x amount of measures exists as well, but we found that even harder to interpret / draw clear conclusions from so decided not to add this to the manuscript.

L212: We would argue it is not really an interpretation as this is really the core of what is in the model (and one cannot draw conclusions about something that is internalised in the model itself. However, we will phrase it a bit better hoping it reads more like results now. In the discussion (5.1), we would like to repeat this observation while adding a more detailed interpretation.

L223: We thank you for this suggestion, and would use top-down intervention or policy intervention in the revised manuscript.

L235: We agree with your observation. It was a result that was not expected by the authors, but indeed clearly explainable.

L266: Yes, there is a limited form of market working (see previous reonse regarding this topic)

L356: You are absolutely right. We would like to rephrase this to: *"Because the model setup could not be fully validated, and scenarios do not provide a complete overview of all possibilities, this study does not claims to provide a prediction of the future for south-eastern Kenya".*

L359: Thank you for this suggestion. In the discussion, we would like to extend the section on uncertainties and limitations to include information of what the model does and what it cannot do (and why).

L376: It could be that some interventions alleviated a barrier that was not the main barrier and thus show no effect – which would mismatch the conclusions of (Wens et al. 2021). We will highlight this more in the discussion section.

L377: We think the relative effect of the measures because of their diversified effect on the heterogeneous households is an interesting confirmation of what we would indeed expect. We would like to desribe this more precisely by adding numbers to this part of the text.

L386: We agree with your suggested change

L388: We agree with your suggested change

With respect to your comments on the ODD+D in appendix, we would like to propose to greatly revise this section to account for all your comments and suggestions.

I.i: Thank you for your suggestion. we will include this in the rewritten ODD

I.ii: We will rephrase this and remove the use of "stock of assets". We indeed see weather as an exogenous factor while climate change si reflected through different scenarios altering this external weather factor. Prices on the other hand are endogenous, based on supply and demand within the model. We propose to create an UML diagram to clarify this.

I.iii: We will extend this section and add a flowchart to support the description

II.ii: We will extensively alter this section to include more details on how decisions are coded in the model

II.viii: We will rephrase this to focus on the model implementation

III.ii. We will provide means and sds of these initial variables and add information on dependencies.

III.iii: Thank you for this clarification, we will change shis section accordingly

III.iv: We feel it is not needed to explain an existing, open-access standalone model that is published (AquacropOS) so we just refer to the academic literature regarding this model. The other 'submodel' is the decision module but this one is already explained in quite detail in the part above (it is not really a submodel, rather the core of ADOPT) so we feel also here, not much additional info would be needed.

We greatly appreciate your extensive comments, which we adresssed above, and are sure implementing them will improve the manuscript significantly. We would also like to thank you for your detailed orthographic and language review, which we will take into account in a revised manuscript.

Respectfully,
Marthe Wens