Dear editor,

Please find in the below the review comments, responses and changes made in the manuscript. We appreciate the comments which have contributed to a significant improvement of our manuscript. The reviewer's comments are in italics and the author's response in bold.

With kind regards, and on behalf of all co-authors,

Ileen Streefkerk

Reviewer #1

General comments

Kenya is increasingly growing agricultural products for export. The water needed for commercial export farms competes directly with the water needed for agropastoral communities in Kenya. The authors did study this water competition. They developed a model that integrates a hydrological model with a decision model that simulates adaptation decisions by agropastoralists. After model calibration, they used the model to study the effect of upstream abstraction by commercial export farms on drought risk and its impact on agropastoral communities. The results show that these effects are relatively small compared to the effect of drought periods themselves.

The developed tool in this study is novel due to integrating a hydrological model with a human decision-making model. The upstream-downstream interactions and competition occur in many parts of the world and are therefore very relevant for the EGUsphere readers. The current increase in commercial export farms in different African countries makes the study very topical.

The setup of the study and the main results seem sound. However, the current manuscript raises a large number of questions and comments, which are listed below.

Thank you for your review and your kind words on the relevance of the paper. Your suggestions and remarks are very much appreciated. We have addressed your comments below.

Major comments

Eq. 2: The water demand plays an important role in your study. I suppose that DRYP calculates the volumetric water content θ. If so, mention that here. It is not clear how extensive the irrigation is. Probably most agricultural fields are rainfed. Clarify how you determine the area that is irrigated.

Thank you for your remark. Yes, the volumetric content is calculated by the DRYP model. From the household surveys, we obtained the distribution of agricultural field sizes and their irrigation status. This statistical input is used to generate farms across the model domain. We have added this explanation to the revised version of the paper. For your reference, around 8% of households in semi-arid zones, and 17% for semi-humid areas have irrigated lands, respectively. We sentences read as follows (Lines 217 to 221 in the tracked-changed document):

"where D_{root} is the rooting depth (mm), θ is the volumetric water content (m^3/m^3) and $θ_{fc}$ is the volumetric water content at field capacity (m^3/m^3). Irrigation demand is derived by multiplying the SMD by the sum of the irrigated land area for each household in a cell. The agricultural field size and whether a household has adopted irrigation techniques is initialised based on the initialisation procedures of household characteristics (see section 3.2). If an agent adopts an irrigation measure during the simulation period, land will be converted from rainfed to irrigated land."

Line 246: The sentence "Greenhouses are modelled as 'closed systems' and irrigation water is not added to the model while there is evaporation." is unclear. Do you assume evaporation at the greenhouse locations?

We understand this is not clear and we have revised the text accordingly. We assume that the flowers evaporate/transpire the full water demand of 40 m3/hectare/day. This means that in the model irrigation and evaporation cancel each other out and we model the greenhouse as a closed system. So, irrigation is not added to the model and there is no evaporation at the greenhouses. We realise that this is not the case for the entire grid cell because the greenhouses do not occupy the whole grid cell, so we updated the evaporation (crop factor) of the model based on the percentage of greenhouses in a grid cell (assuming crop factor of 0 at the greenhouse and 1 at the rest of the cell). The sentence is re-formulated as:

Line 264 to 266:

"Greenhouses are modelled as 'closed systems'. We assume all water added as irrigation is transpired by the flowers, so irrigation and transpiration balance each other out. However, as the greenhouses do not cover the entire grid cells, we take into account the percentage of coverage of a grid cell and calculate evaporation and transpiration only for the part of the grid cell which is not occupied by greenhouses."

Line 273: You mention that you list only the relative factors of the hydrology-related parameters in Table 2. Therefore all the factors are dimensionless. However, in this way the reader does not have any information on the actual values of the hydrology-related parameters. Therefore include in Table 2 a column in which you specify the reference values, such as Table S2 in the Supplement.

Thank you for your suggestion. We have incorporated the actual values as an extra column in the table in the revised version of the manuscript. Please see Table 2.

Line 295: In general de term "model validation" is reserved for the application of a model to different circumstances: a region or period for which the model was not calibrated. Am I right that the simulation results which you show in Figure 4 were derived after model calibration? In that case, you cannot call this "validation of the model".

Yes indeed, the results were derived after model calibration. We agree with you that we should not call this section validation, but model performance instead. We have removed the validation header and replaced it with 'model performance' throughout the manuscript (e.g. line 281, 285, 295,331).

Figure 4: Milk production is one of the criteria you use to quantify the impact of water extraction. The simulated milk production does, despite the calibration, strongly deviate from the measured milk production. Especially in the dry periods, which are the focus of your study, the milk production is grossly underestimated by the model. Discuss how this may impact your results and conclusions.

Thank you for these valid points. The model indeed underestimated the milk production compared to observed data. This might indicate that milk production is not just influenced by environmental stressors, but that responses of organisations have decreased the impact of drought on milk production (e.g. people receiving cash transfers through humanitarian aid). However, we should also note that the observed data might not be accurate. Milk production does not go down as much as one would expect from the severity of drought impact reported on food security (Reliefweb, 2023) (assuming high milk production is strongly correlated with low food insecurity; Jodlowski et al., 2016). We have added this to the discussion of the paper.

Lines 543 - 549:

"However, the model underestimated the milk production compared to observed data in dry periods. This might indicate that milk production is not just influenced by environmental stressors (Busker et al., 2023), but that responses of (non-)governmental organisations have decreased the impact of drought on milk production (e.g. people receiving cash transfers through humanitarian aid allowing people to buy water and hay). However, we should also note that the observed data might not be accurate. For example, some literature shows that milk production does not reduce as much as one would expect from the severity of drought impact reported on food security (Reliefweb, 2023) - assuming milk production is strongly correlated with food insecurity (Jodlowski et al., 2016)."

Figure 6: What is the unit of soil moisture in the top graph? Earlier you used volumetric water content (-), which can never exceed 1.0.

Please note that 1e*-5 is on top of the x-axis. However, we understand that this is difficult to read and we have changed the plot accordingly. Please see the revised Figure 6.

Figure 6: This figure shows only the differences of scenarios 2 and 3 with the baseline scenario. In order to put these changes in perspective, I recommend to show also the time series in time, as depicted in Figure S6 of the Supplement.

We indeed thought about doing this, but as the changes are relatively small it is really hard to see the differences compared to the variation in time.

Line 364: You discuss here where soil moisture is decreased. However, looking at the values in Figure 7, this decrease is always < 0.01. You should mention this here.

Thank you for your suggestion, we have incorporate the values here.

Lines 404-406:

"Figure 7 shows these effects spatially over the whole drought period of 2020-2023. The left panels show that the decrease in soil moisture (up to ~0.01) is largely at the location of the commercial farms in the *forest (2)*- and *communities (3)* scenarios during the whole drought."

Lines 415-417: Here you discuss serious reductions (22 and 36%) of stream flows as a result of your study. However, you did not show these reductions in your paper. In Figure 7 you only mention absolute reductions (m^3/d). You might add these % reductions in the Supplement as a function of time and refer to this information here.

That is a good suggestion. We have revised lines 411-413 and added Figure S6 to the Supplement.

"Maximum absolute increases of streamflow during the 2020-23 drought are at the outlet of the catchment, up to 6 m^3/d. Increases up to 20% can be observed over OND seasons with an average of 5.8% (between 2002 to 2012) in the Naru Moru river (see Figure S6), located in sub-catchment 2. Over the period of 2013 to 2023 the maximum increases over the OND season are 30%, with an average of 7.3%. Average streamflow differences in the Naru Moru river is an increase of 4.4% over the period of 2002-2023. Note that during the dry seasons differences are often zero, as most of the time there is zero-flow in both scenarios during the dry seasons. "

Lines 482-483: "it should be noted that not all factors are included in this study and more factors may influence the adoption of drought measures". Can you mention some of these factors?

Other factors include for example the risk and time preferences, included in economic theories (Expected Utility Theory and Rank Depended Utility theory) (Schrieks et al., 2023). We have specified this a bit more in the revised version.

Lines 562-563

'.. such as variables which are included in economic theories (e.g. risk and time preference) (Schrieks et al., 2023).'

Line 485: In line 59 you write: "The main goal of this paper is, therefore, to develop a coupled hydrological and agent-based model (ADOPT-AP) to investigate the influence of upstream large scale commercial export farms on downstream drought risk and adaptation by agropastoralists." In this Conclusion section you discuss the influence of commercial export farms. However, how do you evaluate the performance of ADOPT-AP? Which parts of the model framework perform well and which parts need further development?

Thank you for your reflection, we should indeed incorporate some results of the model performance in conclusion as well. We included that the model underestimates milk production during dry periods. The model framework as a whole is tested with performance metrics (BR, KGE). However, the human-decision part of the model needs more validation, but that is currently not possible due to lack of longitudinal survey data (as mentioned in the discussion).

Lines 566-568:

"The model was calibrated using the NSGA-II technique and shows a performance of a KGE score of 0.54 for discharge, and a BR score of 0.63 and 0.62 for crop and milk production, respectively. Compared to the observed data, milk production is underestimated by the model during dry periods."

Lines 21-22 (abstract) + 489-490: You state: "The analysis shows that in the scenarios where these farms are replaced by forests or communities, drought conditions are alleviated by increasing soil moisture, streamflow, and groundwater tables." Your results show that the simulated increases in soil moisture, stream flow and groundwater depth are very small and have a minor effect on crop production, milk production and distance to water. In my view, the current statements are too firm and should be put more in perspective.

We agree the wording is too firm and have put this sentence in perspective.

Lines 24-25:

"However, compared with the impact of drought hazard itself these changes are very small."

Lines 586-587

"Changes in drought impacts among the different scenarios are, however, low in comparison to the effect of drought itself."

<u>*Minor comments*</u>

Line 55: The phrase "minimum water availability" is unclear. Replace by "minimum river flows" (based on Lanari et al. (2018)).

Line 203: Equation 4 should be equation 2.

Line 206: Change "θ is the water content (–) and θ_{fc} is the water content at field capacity (–)" to "θ is the volumetric water content (–) and θ_{fc} is the volumetric water content at field capacity (–)"

Line 268: Table 3 should be Table 2.

Line 272: Do you mean Table 2 with "Table 5"?

Lines 366, 381 and 387: Check figure references.

Line 388: produciton should be production

Line 397: resuling should be resulting

Thanks for providing minor comments on spelling and wording, they are incorporated in the revised version.

Reviewer #2

This is an interesting paper, which presents both a tricky setting with loads of water-related interactions and a fascinating methodology to study the setting. Having said that, I am afraid that to me the paper is less convincing than it should be on both aspects. My main reason for this assessment is that the paper focuses rather strongly on the "what" (model choices, data inputs) and tends to ignore the "why" (are certain choices allowed, what are consequences of choices?). I cannot determine whether model results are not a direct result from the modelling setup. Let me rephrase that: I am convinced that the model results are the direct result of the modelling setup, but cannot easily determine to what extent the agency of model agents is "free" enough to escape modelling setups. As far as I can judge, the ABM setup is quite deterministic. I do not have problems as such, as some of the ABMs I have worked on have deterministic procedures too. I do have problems with this text because the choices made are not clearly explained. I will not present detailed comments on all parts of the text, as what needs to be done first in my opinion is a clearer discussion on the model setup. Below, I share some feedback on selected parts of the text to illustrate my assessment.

Both in the abstract and the introduction, the lack of studies on the topic is connected to the ABM setup with "therefore". Why "therefore"? I can imagine very useful studies to be developed on the topic without ABM. In fact, the strong hydrological base of the ABM setup suggests that agency may be less required to study flow-related feedbacks. What does agency add?

We agree with you that the 'why' and the agency of the model can be better addressed in the paper. Thank you for pointing this out. While our explanation referred to the previous paper, we understand that some of the assumptions made in the model should be made clearer such that this paper can also be understood as a standalone paper. Therefore, we have incorporated this feedback into the manuscript.

For example lines 206-207 and 257-229 in the tracked-changed document:

"Fulfilling domestic water demand is prioritized in the model, followed by livestock water demand, and lastly irrigation water demand."

"The decision on where to migrate to is solely based on the maximum grass availability in the neighbourhood of the agent. Grass is consumed on a first-come first-serve basis. Due to the random activation order of the agents, the agent who is activated first has most grass available."

We understand that the model might come across as deterministic and therefore raises the question of agency. We are bringing in agency in the human decision-making part of the model as the communities are affected differently by drought impacts. Communities also respond differently to these impacts due their varying vulnerabilities, social network, water/grass availability, household characteristics etc. This responsive behavior is stochastic due to the agents' heterogenous characteristics (household size, off-farm income etc.) and randomly created elements, such as, cost perceptions, social network, groundwater points, etc. To see what the effect is of commercial farming activities on (different groups of) communities we use the agent-based setup. The commercial farms follow deterministic water allocation rules to fulfil their water demand requirements. We have clarified this further in section 3.3.4 of the manuscript. However, in the way we present the paper it

might indeed come across deterministic as we needed to set the model 'fixed' to be able to compare results among the three scenarios. For this reason, we have run the model multiple times (with different initialisation/randomness), so one can better see the results of the model acroos different runs. Please see the revised Figures 5, 6 and 8.

The text moves between "drivers", "behavior" and "actions" when describing aspects that I think are very close if not the same. As soon as "actions" are "driven", where is the choice? To me, "behavior" reflects a longer-term pattern, whereas "actions" are shorter term. As soon as hydrological effects are produced by "actions", can they be an external "driver"?

About the use of "drivers", "behavior" and "actions". We looked at the text for inconsistencies, but we do not use the word 'driver' in the text. We see actions as the options people can take to adapt to drought, which is a result of behavior – although they can be used interchangeably indeed. Behavior might indicate that actions are taken because of a high risk or coping appraisal for example. We have revised the text, especially the second paragraph of the introduction. In these revisions, we made sure the terms 'actions' and 'behavior' are used in a more consistent way and replaced the word 'driving'. Please see the changes made in lines 39 to 59.

The ODD protocol and other materials in the supplement are obviously more detailed compared to the text on model setup. This is to be expected, but I think some elements from the supplements need to be clearer in the main text.

Thank you for your specific questions. Please see a response to your points below. We have made this clearer in the main text.

- *Household characteristics: does the framing on the best fit mean all households start the same?*

It means that a statistical fit is used to generate a random distribution of the (heterogeneous) characteristics of the agents – with a unique set of characteristics. The distribution is based on the household survey. We have emphasized that the agents all start with a unique set of characteristics.

Line 121-122:

"The agents have a heterogeneous and unique set of characteristics (e.g. location, income, age, etc.) at a spatial resolution of 1x1 km^2."

- *The same for self-efficacy and how household properties like gender influence decisions.*

A regression analysis is used to define the relationship between household properties and self-efficacy. We use those relations – but the self-efficacy is unique for every household as a consequence of the unique set of household characteristics. Self-efficacy is also dependent on knowledge, which is dependent on the social network. We parameterized the relationships between self-efficacy and household characteristics based on regression analysis of a recent household survey study (Schrieks et al. 2024).

- *The PMT theory is from 1983. I would like to read more about why such an old one can still be used.*

The protection motivation theory (PMT) is indeed originally from 1983 but is still state of the art theory in psychology and decision making under risk. Many recent studies on drought risk adaptation behavior use protection motivation theory and recent household survey studies with farmers and pastoralists in East-Africa show that PMT is a suitable theory to explain adaptation behavior under drought risk conditions (Wens et al. 2021; Schrieks et al. 2024; Gebrehiwot and Van der Veen, 2021). PMT is used in many ABM studies on natural hazard management (Heliegiorgis et al. 2018; Wens et al. 2020; Michaelis et al. 2020; Moradzadeh and Ahmadi, 2024). Advantages of using such an

established psychological theory are that the theory is supported by a lot of empirical evidence and that it increases comparability with other studies and replicability of the study (Schrieks et al. 2021)

- *How can a grid cell represent multiple household as one agropast agent when apparently the density can differ?*

The size of the representative agent can vary through differences in household size and number of households in a cell. The total "size" of the representative agents depends on a density map (CIESIN, 2022). We have clarified this in the next iteration.

Lines 122-123:

"This means each grid cell has one agropastoralist agent, representing multiple households (depending on population density and household size)."

- *My main concern is the time step. Decisions are annual according to the text. The supplement mentions that annual decisions on crops and livestock are at other moments, right? This would mean that decisions are not annual, or should not be assuming that decisions on crops and livestock should be related. After all, there is only so much one can do in the time given to an agent. With the hydromodel running on shorter time steps, why can agents not act more often? or does the hydromodel actually represent many decisions of human agents, but keeps them hidden and unrelated to the "real" decisions?*

Thank you for your question about the timestep. Yes, it is indeed possible to include multiple decisions or decisions that are made multiple times a year. In principle the model runs on a daily timestep in which people use water for domestic, livestock, and irrigation purposes (based on water availability and demand). We have made this clearer in Section 3.3 'Socio-hydrology' in the revised version. The adaptation decisions are made at the point in the season people also make these decisions; the crop-related decisions are made before the rainy season, and the livestock-related decisions are made before the dry season. These the adaptation decisions are based on the household survey and the decision-making process is based on the PMT. We have elaborated on the timesteps of the model in the general overview of the model in the 'Data and Methods' section. We have added more information on the timing of the adaptation decisions in Section 3.2 'Human-decision making'.

Lines 205-207 in section 3.3

"Water can be abstracted directly from a river or through groundwater abstraction points for all three purposes on a daily basis. Fulfilling domestic water demand is prioritized in the model, followed by livestock water demand, and lastly irrigation water demand."

Lines 116-120 in Data and Methods

"The coupled model has a daily timestep (see Section 3.1), and agropastoralists exhibit daily to yearly decision making. Water is abstracted at the nearest available location, either at the river or groundwater well, for domestic, livestock and irrigation purposes. Irrigation decisions, following simple rule-based, are made every day during the growing season depending on soil moisture and available water. Major decisions are based on the psychological theories (see Section 3.2) and are made once a year (See Table 1)."

Lines 144-145 in 3.2

"Depending on the adaptation measure, agents decide whether to adopt a specific measure at different moments throughout the year (Error! Reference source not found.**)."**

Lines 147-148 in 3.2

"Agropastoralists can take six drought adaptation measures at different moments in the year (numbered 1-6 in Error! Reference source not found. **and Table 1)."**

I find figure 4 not easy to understand, as the middle and lower panel do not seem to have a legend. This remark stand symbol for almost all of the figures, which i find not easy to understand. I do know how hard making useful figures can be...

Thank you for pointing out your remark on the readability of the figure. We added the legend also to the other panels for all figures. Figure 6 is revised as well.

The Discussion is extremely general and is not going into much detail on model results in relation to the setup or in relation to other literature. Paragraph 5.1 does not seem to need ABM anyway to make its points. 5.2 is a nice overview of some literature, but does not reflect back on the ABM proposed in the text beyond claims that this model is "valuable" in "capturing" and "understanding" things. I would agree on the "capturing", even when I would still need to know more about why model choices make sense. I take issue with the "understanding", as copying processes is not necessarily the same as finding out why these processes happen.

About the discussion, we do agree with the points raised, especially concerning the impact on communities. Our assumptions should be made more clearly in the Methods section and elaborated on in the discussion. We also agree that, in the way section 5.1 is currently written, the results generated might not need an ABM - as we only discuss the hydrological implications. However, according to our opinion, this does not mean we cannot discuss these results as our quantitative results are new and have practical implications for water management.

Furthermore, ABMs are relevant for discussing about community impacts and policies, and we have elaborated more on the heterogeneity of the results in the discussion. About section 5.2, we believe modelling processes close to reality is valuable to generate alternative scenarios and to assess policy options. In this case, we see value in quantifying the impact of commercial farming on hazard and impact of communities. ABM's in general provide greater understanding of the system as a whole (including the human-water interactions therein) by quantifying these processes and assessing the implications of various scenarios. We have reformulated this in the discussion. Please see the revised version of the discussion, e.g. lines 483-487, 500-508, 546-457 and 527-529:

"In this study we observed minimal changes in the dry seasons, but observed large differences in streamflow during the cropping seasons of drought events (2010/11, 2015/16 and 2020/23). The average changes during OND season are 5.8% in the period of 2013-23. In the current modelling setup it is assumed that the 'crop' commercial farms only grow in the OND season (similar to the agents), which may not be the reality and underestimate water use, explaining the limited effect of the commercial farming activities on dry season flows."

"As illustrated in this research, average distances to water are increasing over time during the dry seasons (up to 2.5%) and crop production decreased during drought seasons (up to 0.5 %) in the *commercial farms* scenarios. In non-drought periods, crop production for agents located near a river is lower in both the *forest* and *communities* scenario, although the streamflow is often higher during the cropping season (especially in the community scenario). A possible reason can be the competition of water for different purposes and the way the model prioritizes water allocation. Increases in streamflow in the *communities* and *forest* scenario can lead to changes in agent's water sources from groundwater to river, resulting in an increase in water demand for domestic and

livestock purposes at river locations. This increase in demand can result in less streamflow remaining for irrigation purposes (leading to a decrease in crop production).”

“… , in particularly by quantifying these processes and accessing the implications of various scenarios. ABM’s can also inform policy development by providing evidence-based insights into how different stakeholder behaviours and interactions influence system outcomes under various management scenarios.”

“The results show, however, a wide range in the uptake of adaptation measure among the various model runs, highlighting the importance of heterogeneity and individual characteristics of agents in adaptation behaviour.”

I will be totally clear: when reading the conclusion, my first response was a simple "wow". It was not an enthusiastic "wow, though, as I find the conclusion frustratingly brief and shallow. Apart from the numbers mentioned (why only those?), I could have written the same conclusion without any deeper analysis of the setting. Again, to be totally clear: this conclusion was almost enough reason for me to suggest a "reject" for this text. If this is all one should take from the study, it can be easily missed in the academic world. There must be more to share. As already said, I propose that sharing much more on reasons for model choices and on implications of these choices for results would be required. Only with these aspects clarified could one think about consequences of both for understanding and follow-up actions (research and policy).

After carefully re-reading the conclusions, we agree the conclusions should be elaborated on and the discussion should include more arguments for the modeling choices we made and on the implications of these choices on the results. Please see the changes made in the revised manuscript, in both the discussion and the conclusion.

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