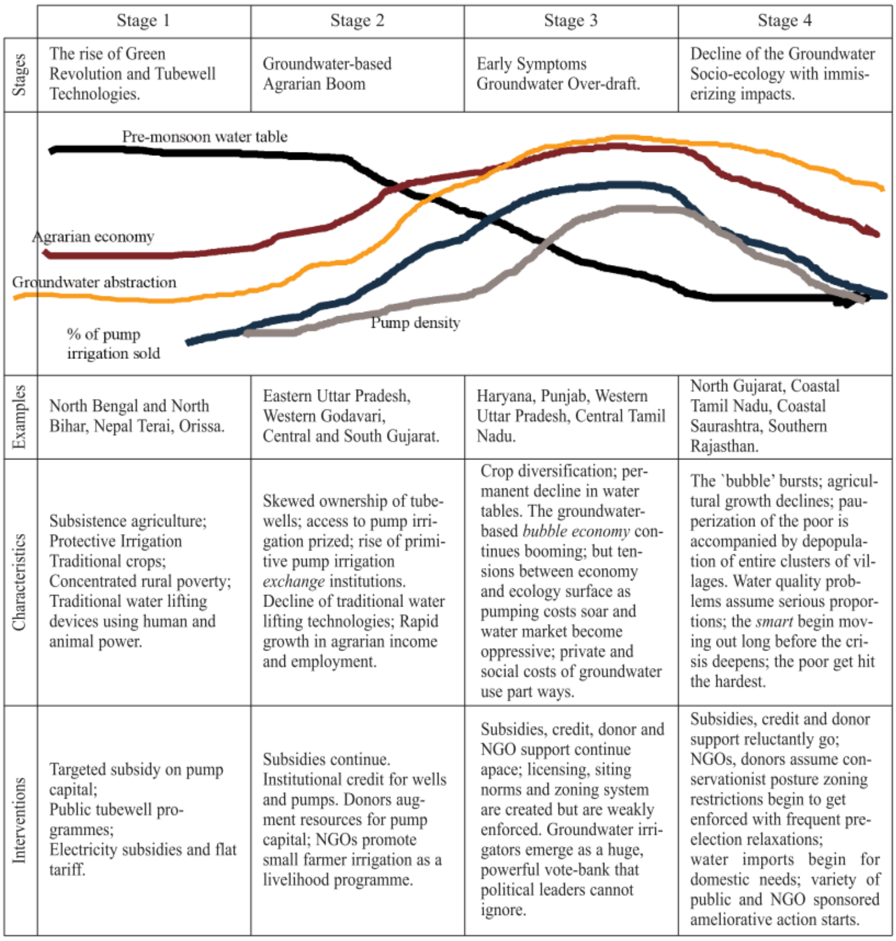
Thank you for the invitation to review this manuscript. In this work, the authors extend the GEB, a coupled agent-based hydrological model, with the Subjective Expected Utility Theory and apply the model for analysis of the Bhima River basin in India under consecutive droughts. The manuscript is impressive for the complexity of model integration and the breadth of analysis conducted. I especially commend the authors for the extensive sensitivity analysis that is conducted using the model, which is often a critical gap of coupled human-water systems analyses. However, the extensiveness of the manuscript is a double-edged sword, with the manuscript very challenging to wade through given the sheer amount of material (as reviewer #1 also noted). In this sense, I reiterate reviewer #1's comments in regards focusing the analysis. I have additional comments in regards to the manuscript:

Thank you for your positive words and overall constructive feedback! The extensiveness has indeed also been referred to by reviewer #1. Therefore, we have reduced the number of panels in figures 5 and 7, removed several paragraphs that did not contribute as much to the discussion points, and trimmed the remaining paragraphs to have a more focused narrative.

However, reiterating the response to reviewer #1, our main finding is that the combination of hydrological and socio-economic factors steer the area and especially certain groups of farmers into a more vulnerable state. For this, combinations of the results of yield, crop choices, income, groundwater and wells are needed, as the interplay between those leads to this result. We also find that being able to model the interplay of these factors is a major strength of this type of model.

1. My first and foremost comment is that the authors should demonstrate the validity and reasonability of the model in relation to real-world observation / understanding. While I understand that a full-scale, spatiotemporal validation of the model isn't likely possible given the sparsity of real-world observations and the complexity of the model, one can still ask the question: does the model better capture real-world patterns of the complex system in comparison to alternative approaches (e.g., the no adaptation alternative). For example, model results indicate that there is a very significant uptake in groundwater wells for large farms (growing from 30 percent of farms to 65 percent of farms) over the course of the model run. Is there any real-world quantitative or qualitative data that supports these model results? The onus in this case would be demonstrating that the adaptive model outperforms the non-adaptive model in replicating these large-scale patterns observed in reality. Similarly, do we in reality see the significant increases in groundwater depletion associated with the adaptive behavior (~10 meters in relation to the non-adaptive version); I would imagine that even apart from point groundwater level measurements, such a stark difference in depletion could be corroborated by GRACE, or even other qualitative sources. Cropping patterns are another example, the adaptive model shows large-scale crop switching that could likely be corroborated, in a broad *scale sense, via agricultural census information or remote sensing data. While the modeling integration and advances are impressive, there are so many choices that are made in regards to theory and implementation (as is the case with nearly all coupled human- natural models), that it becomes nearly impossible to assess the value of these model improvements in the absence of such evaluation.*

This is indeed a valuable suggestion and in the revised version of our paper, we have added a new paragraph to the discussion section where we explicitly verify the modeled trends (e.g., uptake of wells) with literature (lines 564-584). This is mainly centered around the findings of Roy & Shah (2002), which describe multiple stages in a process of well expansion and decline in many locations in India (Figure 1). Additionally, we refer to observed well uptake percentages and observed groundwater decline rates. Regarding crop choices, we discuss how our choice of behavioral theory without sufficient negative feedback effects led to too homogeneous cultivation and propose methods to simulate this more realistically.



	Stage 1	Stage 2	Stage 3	Stage 4
Stages	The rise of Green Revolution and Tubewell Technologies.	Groundwater-based Agrarian Boom	Early Symptoms Groundwater Over-draft.	Decline of the Groundwater Socio-ecology with immiserizing impacts.
Examples	North Bengal and North Bihar, Nepal Terai, Orissa.	Eastern Uttar Pradesh, Western Godavari, Central and South Gujarat.	Haryana, Punjab, Western Uttar Pradesh, Central Tamil Nadu.	North Gujarat, Coastal Tamil Nadu, Coastal Saurashtra, Southern Rajasthan.
Characteristics	Subsistence agriculture; Protective Irrigation Traditional crops; Concentrated rural poverty; Traditional water lifting devices using human and animal power.	Skewed ownership of tubewells; access to pump irrigation prized; rise of primitive pump irrigation *exchange* institutions. Decline of traditional water lifting technologies; Rapid growth in agrarian income and employment.	Crop diversification; permanent decline in water tables. The groundwater-based *bubble economy* continues booming; but tensions between economy and ecology surface as pumping costs soar and water market become oppressive; private and social costs of groundwater use part ways.	The `bubble' bursts; agricultural growth declines; pauperization of the poor is accompanied by depopulation of entire clusters of villages. Water quality problems assume serious proportions; the *smart* begin moving out long before the crisis deepens; the poor get hit the hardest.
Interventions	Targeted subsidy on pump capital; Public tubewell programmes; Electricity subsidies and flat tariff.	Subsidies continue. Institutional credit for wells and pumps. Donors augment resources for pump capital; NGOs promote small farmer irrigation as a livelihood programme.	Subsidies, credit, donor and NGO support continue apace; licensing, siting norms and zoning system are created but are weakly enforced. Groundwater irrigators emerge as a huge, powerful vote-bank that political leaders cannot ignore.	Subsidies, credit and donor support reluctantly go; NGOs, donors assume conservationist posture zoning restrictions begin to get enforced with frequent pre-election relaxations; water imports begin for domestic needs; variety of public and NGO sponsored ameliorative action starts.

Figure 20. Rise and fall of groundwater socio-ecology in India.

Figure 1: Stages of groundwater irrigation (Roy and Shah, 2002))

2. As I understand, the region is also heavily managed in regards to the surface water supply system (reservoirs, diversions, manmade canals, etc.), which influences water availability for irrigation and associated demand for groundwater and farm decisions to install a groundwater well. Can the authors speak to the capabilities or limitations of CWatM in effectively representing surface water deliveries for irrigation in this region and how this may be influencing results?

It is indeed true that the area is heavily managed, and we have therefore included several features in the model specifically to address this supply system. Yet, there are limitations and uncertainties.

First of all, reservoir command areas are included in the model. The delineation of the command areas was obtained from the India Water Resources Information System, and manually linked to reservoirs (De Bruijn et al., 2023). In principle, agents can abstract water from these reservoirs if they are in the reservoir command area and have access to the reservoir based on census data.

However, the current reservoir management module follows relative simple decision rules simulating two types of release: (a) the first is release into the river channel, which is based on protocols for reservoirs that are designed for power generation. (b) The second is a daily fixed proportion of total reservoir storage that gets released to farmer agents to abstract from. In the model, there are no physical canals delivering water to agents; instead, agents directly extract water from the reservoir as long as it remains within the daily allocated budget. Upstream agents have priority in water extraction, simulating the way canal water delivery functions in this region (see section 1.3.1. in ODD+D protocol). The volume of these releases are too low, and we, therefore, see relatively little effects of reservoirs in our results. For future research, we want to improve this module and better represent the different types of reservoirs and their effects on farmer adaptation. However, as you and reviewer #1 have both remarked, there are already many elements in the manuscript, thus we have decided to leave it out of the main text, and left the reservoir agents descriptions in the ODD+D protocol.

3. In this discussion, the authors note that groundwater well drilling is potentially maladaptive, as farmers then rely on wells that can go dry during subsequent droughts. These are important findings that seem to be largely glossed over in the results section. For example, there isn't a figure reporting on the drying of these wells during subsequent droughts.

Thank you for noting this statement was not properly linked to the presented results. The drying of wells is represented by the trend of the well percentage (Figure 7; particularly the 2011-2015). These figures refer to wells *with* groundwater access, or "wet" wells. However, to improve the communication of our findings we renamed the figures and changed the descriptions to make this more clear. This downward trend can be due to the effect of wells not being replaced after their maximum lifespan was exceeded, and the drying of wells. However, the drop is too large to be fully explained by the non-replacement and it coincides with the groundwater decline, thus we can attribute the drop in well uptake fraction mainly to drying wells. While we feel adding an additional figure would increase the amount of material again – which both reviewers noted that we should avoid – we have included these notes much more explicitly in the descriptions of the results (section 3.1).

4. It would seem to me that the imitation technique (described in lines 155-156) would very quickly lead to homogenization of crops across farmers using the same irrigation technology. Is this not the case? Could the authors further comment?

This is indeed a very relevant remark, We include imitation together with the SEUT as this is how adaptation has been observed to spread in real life (Baddeley, 2010). Thus imitation in itself would not directly lead to homogenization. However, we agree that in our case, there is indeed too much homogenization. In the revised version of the paper, we discuss several reasons for this feature in the discussion section. First, there is an absence of economic feedbacks. This is especially important since the economic behavior theory we have implemented is mainly based on utility maximization, thus it would require feedbacks in the same domain. Second, there is no accounting of other factors influencing crop choice, such as cultural factors, intention to behavior gaps, unobserved cost factors (similar to Yoon et al. (2024)), and e.g. the prevalence of subsistence farming in the area. While it's true that once a crop rotation option is eliminated it can no longer be chosen, leading to homogenization, we believe that this elimination itself isn't inherently negative. However, the mechanisms driving it should be modeled more realistically.

We have included recommendations to improve methods and future studies can incorporate either additional economic feedbacks, such as a crop market, ensuring that farmer profits go down as more farmers grow a particular crop. However, due to the already complex methodology, we have reserved this for future work. These options are discussed in lines 584 to 599.

Additionally, reducing the number of crops and crop rotations would allow us to let agents compare the different options, without having to rely solely on imitation for computational reasons. Influence of neighbors could then be translated in an adjustment of the intention factor for example. Additionally, instead of letting agents choose between all possible crops, we may explore the decision between crop variety options, allowing agents to select varieties of their main crop that are more resistant to drought/water-efficient/etc, which also fits with literature (Drugova et al., 2021).

5. *I understand that the political economy of sugarcane is particularly influential on water security outcomes in the region (e.g.,*

https://iopscience.iop.org/article/10.1088/1748-9326/ab9925/meta). Could the authors speak at all to how such considerations factor into the analysis? More broadly, crop prices are a significant driving factor of farm behavior, but the subjective expected utilities are only formulated in relation to subjective drought perception. Can the authors comment on whether/how farmer perceptions of economic conditions might influence results (even if outside the scope of this analysis)?

Indeed, we refer to economic shocks coinciding with meteorological shocks, but only simulate behavior change in response to the latter. In the model, following the SEUT theory, the behavior of farmers is strongly dependent on crop prices, meaning that if prices drop, agents will start cultivating different crops that are now more profitable, and vice versa. We observe this effect for droughts in the model results, but similar behavior would be exhibited in the case of price drops or increases due to other external factors.

But on a perceptional or behavioral level you would expect that farmers fall back on crops which give security (especially during uncertain times). These could be subsistence crops for smaller farmers (mentioned in the discussion), or indeed crops such as sugarcane which have a guaranteed sale and price set by the government. This may require a second behavioral factor, which instead of reweighting the probability of future events, would reweight future crop prices based on their probabilities (e.g. 50% chance at higher prices, or 100% price of a slightly lower price) and would be similarly reweighted by risk perception and risk aversion. Perhaps this could be implemented alongside forecasts, where crops are weighted based on how well it would do in the current forecast along with the forecasts' probability weights? We included these recommendations in the revised version of our paper (lines 600 to 603)

6. The above article is conducted as part of the Stanford FUSE project, which was an outgrowth of the Stanford Jordan Water Project (JWP) which also introduced a coupled agent-hydrologic model for similar types of analysis (e.g., https://www.nature.com/articles/s41893-023-01177-7; https://www.pnas.org/doi/abs/10.1073/pnas.2020431118). While much of this work was focused in Jordan rather than India, these are important studies to note as part of the literature context. Can the authors speak more to how the current effort relates to and is distinguished from this line of coupled agent-hydrological model?

Thank you for bringing the attention to these papers. There are indeed **many** similarities, and unfortunately we have only seen this research as of now. After carefully examining this literature, we think the differences lie in four areas.

- First, our research is focused on drought *events* specifically: What happens during drought events, what happens over consecutive events, how does the crop yield change (which required, for example, a more extensive crop module), how does this affect profits, etc. This event focus is also a step towards future studies where agents have to react to alternating droughts AND floods, which is a different path compared to these studies.

- Second, is that the focus in our paper is more on the differences in situations and behavioral aspects of farmers: how do they make investment decisions using past experiences of droughts, how is this affected by a risk perception, risk aversion, time preferences, farm size, difference in climate between upstream or downstream and how do all these factors affect their choices? Some of these factors are present in the linked studies (and other spatial factors, like transportation costs are equally important but not of relevance for our study), but they are implemented slightly more rudimentary and are less the focus of the research.
- Third, we simulate all agents and localized abstractions instead of using representative agents.
- Lastly, of course we have very different local conditions which require a different model set up. For example, in our study area electricity is subsidized to cost nothing or almost nothing, which means that the costs are in the loan for the initial investment, and not in the structural price of water. This makes the differences between access in groups much more strict and leads to other dynamics which are characteristic of the area (like many agents at once losing access during a drought) and is a clear difference between these models. For future studies in the global north we do intend to make it more similar to these papers (and to Yoon et al. (2024)), where it is assumed that if agents could have gotten access to groundwater, they would already have, and now pay a price per volume of water (dependent on the groundwater levels, pumping costs, etc.) they use instead of for getting access to the water. The investment decisions are then focused on different ways of decreasing water use, like switching crops or improving irrigation.

We agree that these are good examples of similar socio-hydrological models and have added references to the socio-hydrological nature of these papers in the introduction.

7. Figure quality throughout could be improved. Resolution is often poor with text difficult to make out and colors often hard to distinguish (e.g., couldn't distinguish crops in the cropping figs). Fig 1 is also difficult to interpret and missing text in boxes.

This was addressed in my comments to the previous reviewer; we now use the OKABEITO color palette (Okabe & Ito, 2002), which should still be colorblind friendly, but better suited to categorical data. All labels have been made larger and figure 1 has been updated. Figure quality decreased when we exported the data to PDFs, but production quality figures (300 dpi) will be made available.

8. Lastly, I agree with reviewer #1's comment regarding the >1 million agents. Even if such # of agents is warranted, headlining the # so prominently throughout the paper (in title, abstract, etc) in my opinion misplaces focus and potentially signals the wrong *message (e.g., model complexity for the sake of model complexity). This ability to model of large # of agents was already heavily featured/highlighted in the original GEB paper, so in this case I'd rather see the spotlight placed on the insights drawn from the modeling improvements and analysis, rather than the # of agents that can be modeled.*

We agree that this was the main focus of the original GEB paper, while this manuscript focusses more on the analysis and results that can be performed with such an approach. We have altered the title to reflect this, and now reads: "Adaptive Behavior of Farmers Under Consecutive Droughts Results In More Vulnerable Farmers: : A Large-Scale Agent-Based Modeling Analysis in the Bhima basin, India". Thank you for the suggestions, we do believe this is a much more fitting title.

Baddeley, M. (2010). Herding, social influence and economic decision-making: Socio-psychological and neuroscientific analyses. *Philosophical Transactions of the Royal Society B: Biological Sciences*, *365*(1538), 281–290. https://doi.org/10.1098/rstb.2009.0169

De Bruijn, J. A., Smilovic, M., Burek, P., Guillaumot, L., Wada, Y., & Aerts, J. C. J. H. (2023). GEB v0. 1: a large-scale agent-based socio-hydrological model–simulating 10 million individual farming households in a fully distributed hydrological model. *Geoscientific Model Development*, *16*(9), 2437–2454.

Drugova, T., Curtis, K. R., & Ward, R. A. (2021). Producer preferences for drought management strategies in the arid west. *Renewable Agriculture and Food Systems*. https://doi.org/10.1017/S1742170521000259

Roy, A. D., & Shah, T. (2002). Socio-ecology of groundwater irrigation in India. *Intensive Use of Groundwater Challenges and Opportunities*, 307–335.

Yoon, J., Voisin, N., Klassert, C., Thurber, T., & Xu, W. (2024). Representing farmer irrigated crop area adaptation in a large-scale hydrological model. *Hydrology and Earth System Sciences*, *28*(4), 899–916. https://doi.org/10.5194/hess-28-899-2024