Reply on RC3
Jens A. de Bruijn et al.

Author comment on "GEB v0.1: A large-scale agent-based socio-hydrological model – simulating 10 million individual farming households in a fully distributed hydrological model" by Jens A. de Bruijn et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-664-AC3, 2022

Dear reviewer, we thank you for the kind words. Below we suggest improvements to the manuscript based on the reviewer’s comments in italic.

This paper is quite ambitious in attempting to deepen and enrich the current state of the science around agent-based socio-hydrology modeling by reducing hydrological response units to individual farmholdings through the usage of a higher-resolution spatial scale. At the same time, it promises a fine-grain resolution on the large scale, but then explains that the agent behavior is simplified and that a large-scale model will be forthcoming in the future. For the most part, it is a valuable contribution that I enjoyed reading. However, I have a few questions and thoughts before publication.

1. It is unclear why the author has rendered a single farm into multiple HRUs when its holding expands beyond single grid cells. Does the inability to maintain one HRU per farm not detract from the representation goals of the study? Perhaps it does not, but an explanation of this . (Lines 85-86)

Dear reviewer, thank you for noting that this is unclear and this could indeed be explained better. In this study the main aim is to simulate farms as independently operated environments, ensuring that management options which are chosen by a farmer do not affect other farms. However, due to the incompatibility of a farm map and a gridded hydrological model due to the much higher resolution of the former, it is necessary to split some of these HRUs. However, these splitted HRUs are still owned by a single farmer and thus management decisions (e.g., switching crop type) by a farmer affect all HRUs and thus the entire farm they own.

We hope this explanation is clear and will include an explanation along these lines in an updated version of the manuscript.

2. It is cautioned that the study is merely to showcase the model, but this feels dissatisfying somehow. A model is only interesting insofar as it is useful, and the reader needs more support beyond the vague notion that the authors enjoy hypothetical scenarios (Lines 365-367). Why is it not realistic? Is it stylized or semi-stylized? Does this effect its generalizability?

Based on the comment of the reviewer (as well as other reviewers) we suggest to replace
the current scenario with a more realistic scenario along the following lines:

1. Use the Indian Development Human Development Survey (IHDS), which presents highly detailed information on 41,554 households, including crop types for the different growing seasons, household size, household income, expenditure, irrigation techniques, farm size etc etc.
2. From the Indian agricultural survey, we will collect farm characteristics (i.e., marginal distributions), including farm sizes and crop types at tehsil level (comparative to counties in the US)
3. Use an adapted version of the iterative proportional fitting to create a synthetic farmer population using the micro-level data from the IHDS yet fits the marginal distributions of the tehsil level agricultural survey

Then based on this data, land use data derived from satellite imagery, and the distribution of farm sizes, we will distribute those heterogeneous farmers spatially, which now include heterogeneous characteristics (according to the IHDS), such as crop types for the different growing seasons, household size, household income, expenditure, irrigation techniques, farm size etc.

Then, using historic crop prices, as obtained from the Indian Agricultural Marketing Information System, in combination with simulated yield based on farm size, potential yield and the simulated ratio of actual to potential evapotranspiration, we will simulate individual farmer income. Combined with inflation-adjusted household expenditures (from IHDS data) and crop expenditures (based on data obtained from the Ministry of Agriculture and Farmers Welfare) we can calculate disposable income.

Next, we can include farmer adaptation behaviour, specifically the construction of irrigation wells, in the model. To simulate this, farmers without an irrigation well, look to (farm-size adjusted) income of neighbouring farmers with similar crop types but with an irrigation well. If the income difference of the agent’s farm compared to those surrounding farms is higher than the implementation and upkeep costs of an irrigation well, the farmer will implement an irrigation well.

This also allows us to derive more in-depth conclusions, based on the heterogeneity of these agents (in addition to the presentation of the model framework and potential future applications).

3. The elaboration of the findings and the conclusion are both insufficient. The paper's findings end strongly with the description of Figure 10, but there's little elaboration on what it means. There is in other words, scanty "discussion" of the model results. What do they tell us, the reader, in socio-hydrological terms (an expansion on socio-hydrology in the literature review could help with this)? Conclusions could also show further generalizability and the future potential for studies like this.

Saying that a large-scale model will happen in the future both undermines the initial claims of the article and fails to examine the purpose of the present one. In short, I think the paper ought to be revised in order to promise less at the beginning and offer more at the end.

We thank the reviewer for their comment, and in a future version of the manuscript we will include the scenario as suggested above, which can also provide the basis for a more elaborate discussion and conclusion on socio-hydrological components. We also will include an additional paragraph in the introduction on socio-hydrological/ agent-based models.