

We would like to thank the Editor for his hard work finding a reviewer for this manuscript and also for taking his time to act as a reviewer, as no second reviewer could be identified. As with the comments from reviewer 1 we believe that some of the critical aspect might have arisen from a misunderstanding of the objectives of the manuscript. We address these in detail below and hope we can clarify the important points raised by the editor. These points will also be addressed when revising the manuscript, since it is important to clarify this to the reader. The editor's comments are presented in *italics* with the authors' comments in unformatted text.

- 1) *This report has been written by the editor as it proved extremely difficult to find a second referee. In their ms 'Optimizing cropland cover for stable food production in SubSaharan Africa using simulated yield and Modern Portfolio Theory', Bodin et al. test three hypotheses (among those: yield maximization, variance minimization) how farmers in Sub-Saharan Africa (SSA) would take management decisions on crop functional types (CFTs). Overall they find that yield maximization, variance minimization and observation are in good agreement. In a second step, when assuming the validity of either yield maximization or variance minimization, they identify regions where management could be improved. Technically they base their analysis on the LPJ-GUESS dynamic vegetation model.*

While in terms of food production the ms tackles an important topic of earth system dynamics and delivers some interesting results, overall the ms displays problematic aspects. As a general comment, the ms is rather unclear about its scope. Is the abovementioned interpretation correct? Is it really the authors' aim to establish yield maximization or variance minimization as the dominant management criteria of subsistence farmers?

Evidently we have not been clear enough about the purpose of the paper. The purpose was never to try to simulate actual farmer's behaviour. Neither was the rationale behind the ms that the dominant management criterion for subsistence farmers is to minimize variance in yield or to increase yield. The rationale was simply to move away from the rather simplistic approach currently employed in large-scale crop modelling studies (Rosenzweig et al., 2013; Schlenker and Lobell, 2010; Müller et al., 2010) to solely look at changes in decadal mean yields (see also response to reviewer 1 regarding the scope of the paper). This ms. should not be seen as an attempt to simulate the real agricultural system as the editor and the reviewer correctly states include many other aspects that are far beyond the scope of this study. Rather it should be seen as a step towards accounting for risk when simulating future changes in yield. Also, the aim with the comparison with current portfolios of crops was not initially intended as a validation. Rather the intention was to see how far off these current crop distributions were from the optimized ones.

- 2) *Then firstly, the choice of those criteria appears rather ad-hoc, and the hint of Modern Portfolio Theory is not helpful in that regard. When referring to general economic principles, the canonic criterion for an individual's decision-analytic framework would be expected utility optimization according to the work by von Neumann & Morgenstern. Aversion against stochastic losses would then be represented much more convincingly by a rather concave utility function rather than a penalty on variance (why should one penalize stochastic gains?). The trade-off between maximizing expected harvest and insurance against extreme losses would then be parameterized by the concavity of the utility function.*

In the ms we wanted to include criteria for risk that could be applied for the entire sub-continent. The rationale was not to base this criteria on an individual's decision making framework, but rather to find a measure of risk and to explore the effect on implementing this criteria on average crop yield. There are several ways to implement a risk criterion into this optimization. At a local scale farmers would, for example, likely take into account the avoidance of crop failures. This could be implemented by selecting a minimum accepted number of calories per grid cell and year. This could either be done in absolute terms as a minimum amount of calories produced per area or in relative terms by using, for example, the 10th lower percentile of current yields as a measure of crop failure (Piontek et al., 2014). The first option would require the selection of a value that would be valid across countries and climatic zones which then also could be seen as ad-hoc. The second option would then be limited to the number of crop failures per time period and would not take into account the compensating effect of high yielding crops. Instead we decided to use the criteria of variance in yield. This as it was included in the already existing MPT framework that had already been applied in relation to making suggestions for increasing yield stability for wheat in Mexico (Nalley and Barkley, 2010). Regardless of the selected method, as the simulations are made at a larger spatial scale than individual farms or farming communities any selected criteria for risk aversion will not be explanatory but rather of explorative nature.

3) Furthermore, why focus on harvest at all and not net harvest, being a function of investments as well? How about inter-annual storage?

If the study was conducted at a local scale, including actual farmer's decision this would indeed be of importance. But as described above: to include the investment costs of individual farmers for a range of crops for entire Sub Saharan Africa under current and future climate would be far beyond the scope of this study, or even current data availability at the spatial and temporal scale of the study. Inter-annual storage would likely be an insurance option for some farmers, but certainly not all farmers and crops. This would then need to take into account storage costs as well as differences between crops and regions in post-harvest losses caused by storage (Affognon et al., 2015). Again, this would be beyond the scope of this study.

4) Also, the third management strategy appears rather ad hoc and is then dismissed in the light of observational data. I would have expected that the authors had started by introducing the set of hypothesis on management criteria currently debated in the literature, and then motivating their choices from that background, rather than introducing three ad hoc criteria.

This assumption has previously been used to study the effect of selecting the highest yielding crop in order to maximize food production globally (Koh et al., 2013; Franck et al., 2011) and was therefore included in the study as a comparison.

5) Secondly, within the paper it is not clear what parts are on validation of a hypothesis, and what parts are on assuming one of the hypotheses and then identifying sub-optimal management practices. So again, the scope of the whole paper remains rather opaque. In the same vein, the systemic aspect of their findings should have been explicated better: why is the ms of interest for ESD?

The part in which we compare current and optimized cropland fractions is not to be seen as a validation. As it turns out (even in the updated analysis) many of the optimized crop fractions

actually do follow the same latitudinal pattern as the observed. Perhaps we did over emphasize the importance of these findings, but we find it to be a rather interesting and slightly unexpected result. Even so there are still regions where there is a large mismatch and where there is a potential to increase yield or decrease variance in yield solely by switching crops (with no change made to other management strategies included in the model).

6) *Thirdly, the discrete numerical approach for optimization (by permutation) appears not state-of-the-art. In particular for multi-dimensional optimization problems, any kind of sampling strategy without any sort of local optimization delivers poorly in terms of approximating the optimum. In that regard the numerical results displayed in the ms might be mere artifacts induced by an ill-posed optimization scheme.*

Please refer to the response to reviewer one's first comment.

7) *Finally, variables and procedures are introduced rather poorly. In particular I found the definitions around Eq.1 extremely confusing. First we learn that the subscript 'p' stands for 'potential', as against 'n' for 'actual'. Then 'current,p,c' is supposed to be a derivative of 'n,c' which is contradicting the above 'p' as distinct from 'n'. 'y' is never used. While Eq.4 seems correct when having read the subsequent text it is not helpful when various sigmas represent different units (standard deviations vs. (co-)variances). On p.8, first §, I could not find out how the baseline is defined. Only later in the ms we learn, that either the mean or the variance are fixed. Still questions remain how the baseline was derived. And why would an optimization 'fail' (l.24) if the input were found to having already been optimal? Further aspects of the results & discussion sections should have been mentioned in the introduction such as p.13,11, p.15,8, p.16,4. While the ms tackles a very important issue of potentially systemic nature, it displays several shortcomings on a very fundamental level.*

These detailed comments will be addressed in an updated version of the ms.

8) *The ms should not be published in its present form. The changes necessary might be so fundamental that a new version might in fact be a new paper.*

We hope that the above explanations of the intended scope of this manuscript, assuage the concerns that led to this conclusion.

9) *Minor aspects: 1. In many ways the ms does not address the interdisciplinary audience of ESD. The concept of actual and potential yield should be explained. 2. P7,14: superscript in unit messed. 3. For ESD, subscripts that denote variables, should be in italic font, any other (such as acronyms or un-abbreviated real world terms such as 'current') in roman font.*

These detailed comments will be addressed in an updated version of the ms

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