Referee Report on Bodin et al.:
'Optimizing cropland cover for stable food production in Sub-Saharan Africa using simulated yield and Modern Portfolio Theory'

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 Sub-Saharan 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?

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

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?

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.

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. 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.

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.