Review of: "An Optimal Control for Ebola Virus Disease with a Convex Incidence Rate: Imputing from the Outbreak in Uganda"

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Potential competing interests: No potential competing interests to declare.

Referee report of 'An Optimal Control for Ebola Virus Disease with a Convex Incidence Rate: Imputing from the Outbreak in Uganda' by Mbabazi et al.

The paper cope with a very interesting problem: applying the results of control theory to the evolution of a compartmental models for epidemic spread in order to minimize the 'epidemic cost' to the Ebola epidemic in Africa. According to the authors claim, the main result of the paper is to show 'that the most expensive strategy involved imposing lock-down and contact tracing of the infected, while the cheapest alternative was lock-down and treatment of the infected' and it sounds quite interesting. However the model and the results presentation should be improved. In the abstract the authors assume 'that a scientific model would predict EVD disease outbreak control': what is a scientific model? It is not clear to me. In the introduction there is a short presentation of the recent EVD outbreak, but using a very qualitative point of view it is not clear at all if a compartmental models was able to simulate the observed Ebola epidemic in a quantitative way. For example, the author claim that previous models with bilinear incident rates are 'not useful as the disease progresses in the population in the long run' but without any real justification for this claim to introduce a convex incident rate in the model: they only observe 'the convex incidence rate gives a better understanding of the disease progression in communities especially where individuals are doubly exposed' but why this is relevant for the EVD it is not explained. As a consequence, the model (2.2) (I do not understand why the equations is repeated two times) is not really justified and if all the 20 parameters are really necessary for a quantitative description of the epidemic evolution: it would be necessary to show to the reader the comparison of a simulation with the data from the observed outbreak to justify the model, otherwise the work is purely formal. Indeed the compartmental models are suitable to describe an epidemic evolution when an average field theory can be applied to model the social activities (i.e. we are in a homogeneous urban space) otherwise one has to consider the structure of a human mobility network to understand the social acitivities. In which urban context the observed EVD outbreaks happened? The sections 2.1.2 and 2.2.3 present a standard computation of the R_0 parameter for the model and of the equilibrium condition, it is not clear to me if it is relevant for the main goal of the paper (i.e. the control theory applied to compartmental model) . If not. I suggest to move the sections in an appendix. The section 3 is the main section of the paper where the control problem is formulated, but there is a poor justification of the objective function (3.3): how the costs are estimated? Do the authors consider the feasibility of the control policies (i.e. the individual tracing and the treatment can be applied to the whole population)? How the k_i values are estimated and

what is the meaning of the L^2 norm for the control parameters c_i?

This is a key point to understand the validity of the paper results: the author must discuss the 'physical and economic' meaning of the cost function. After the definition of the cost function the application of the Pontryagin maximum principle is a standard procedure. But the results of the different strategies are not well presented in the figures of section 4. No information is given on the population used in the simulations and reported in the y-axis (do the values have a meaning? Are they derived from the empirical observations?) A quantitative comparison among the different strategies is not easy from the figures (the numerical data are quite similar). But the simulations show that without control strategy the epidemic spread very fast. So again there is the question: what actually happened? Which strategy has been applied? Finally the conclusions are difficult to be justified without a discussion on how the cost function is defined (no unit on the cost is given and the compared numbers are really small, so I wonder on the statistical relevance of the results). Moreover it is not clear how to consider the impact of the local economy if the measure is applied for a long time (i.e. six months) to the whole population.

For the previous reasons I suggest the authors to consider the previous remarks before publication.

P.S. there are still English misprints in the text.