Geoscientific Model Development Discussions

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Comment on gmd-2021-115

Anonymous Referee #2

Referee comment on "ANEMI_Yangtze v1.0: a coupled human–natural systems model for the Yangtze Economic Belt – model description" by Haiyan Jiang et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-115-RC2, 2021

I really appreciated the insights provided on this fascinating and important part of the world, and thank the authors for the informative perspective on the challenges faced by the region. I feel the model as presented here could be a useful step towards developing stronger insights on the systemic controls relevant for future development and policy decisions. That said, I unfortunately do not feel the paper is ready for publication in GMD, as it does not sufficiently document either the underlying assumptions nor the mathematical construction of the model and presents some poorly-supported conclusions. I would encourage the authors to undertake a major revision that more rigorously describes the model, and which provides robust insights - which I suspect may have to do more with the identification of important questions, rather than answers.

= Shortcomings in the model description =

I can certainly empathize with the authors regarding the challenge of appropriately documenting a model with such an ambitious scope. The model addresses many sectors, including many mechanistic linkages, and thoroughly documenting all of these is a very large task. But it still feel it is necessary, in a model-description journal such as GMD, to include the most important equations and parameter values. There should also be some discussion of the theoretical basis underlying the equations, as well as documentation of the source and/or justification of parameter values. I recognize that many of this may be present in the ANEMI documentation, but given the number of changes that appear to have been made from the global ANEMI model to this regional version, I think it’s preferable to repeat the information rather than risk ambiguity.

The paper currently relies on a non-peer reviewed report (Jiang and Simonovic, U of Western Ontario, 2021) as the source of all model equations and parameter values, but even there - although the equations are given - the conceptual reasoning is often cursory or absent. Table 5 of the model description report (Jiang and Simonovic, 2021) provides an illustrative example. The table lists 27 parameter values used in the Energy sector. There is no explanation of where these values came from, what degree of uncertainty is associated with them, or how the results might be altered across reasonable ranges in their values. There is a parameter sensitivity section included in the report, which is helpful, but only some parameters are tested (with no justification for how they were selected) and they are all varied by only +/-10% with no explanation of why this would be a reasonable range.

= More robust conclusions =

It is not clear to me that the model has much predictive skill. The 'validation' of the model simply compares a few observed and simulated variables, all of which follow what is close to a linear trend. Given the very large number of degrees of freedom provided by all of the uncertain parameters, I find it very unsurprising that the model fits these variables - which I presume were used in the training dataset in order to tune the parameters. A true test of predictive capacity would require a more complex pattern against which to test (straight lines contain very little information) that was not used for parameter selection. I also don't see much of interest in the future projections - either they are linear projections of historical trends, or they follow the population trend, which is determined almost entirely by the effect of the 1-child policy. These may be reasonable, but what is learned about the system dynamics from this approach?

In addition, I fail to see sufficient report for three points identified as conclusions:

1. the identification of the cross-sectoral interactions and feedbacks involved in shaping Yangtze Economic Belt's system behaviour over time

-> I did not see any rigorous test of cross-sectoral interactions and feedbacks with data. Rather, the model construction proposes a set of interactions and feedbacks that reflect the ideas of the model builders: the results follow directly from the modelers'

assumptions.

2. The identification of the feedbacks within each sector that drive the state variables in that sector

-> Again, these are proposed by the model builders, rather than objectively identified.

3. the explanation of the theoretical and mathematical basis for those feedbacks.

->The mathematical treatment of the feedbacks is only provided in the UWO report, as mentioned above.

I think that the manuscript could potentially be publishable if the authors provide a more useful description of the model that follows the GMD guidelines. The conclusions should also be reframed to reflect the actual results of the modeling study, rather than restating the assumptions.

Additional comments

Section 2.1 provides an enthusiastic description of the qualities of the region, but I do not feel these are necessary, or that they add to the insights of the paper and would therefore suggest that this section be removed.

What evidence is there that riverine N and P levels have a significant impact on human mortality?

The paper would also benefit from a careful proofreading by a fluent english speaker, as there are a number of missing articles and awkward phrasings.