Response to Comments

Referee #1

The authors introduce and describe a new version of the ANEMI integrated assessment model that simulates the regional dynamics of the Yangtze River basin, China. This paper is the first of two; it sets out the rationale for the work and describes the model. The stated aim of the overall study is "to improve the understanding of the complex interactions among human and natural systems in the Yangtze Economic Belt to provide [a] foundation for science-based policies…". Application of ANEMI_Yangtze is to provide this greater insight.

Model downscaling is an active area in integrated assessment modeling, and so the topic may be of interest to GMD readers. In this case, the downscaling is from the global version of ANEMI to the river basin scale. The resulting "ANEMI_Yangtze" model contains a large number of sectors – climate, carbon, population, land use, food production, sea level rise, hydrologic cycle, water demand, energy-economy, water supply development, nutrient cycles, and persistent pollution – that are dynamically linked through feedbacks in system dynamics software. The model has been updated continuously from its first publication in 2010 (Davies and Simonovic) to a most recent edition in 2020 (Breach and Simonovic). Regionalization of the model for application to the Yangtze has required changes to ANEMI's global structure that have replaced dynamic linkages with exogenous inputs in several cases, and the authors have added a "fish" sector, since fisheries are important for the regional economy and diet.

The work is topical and interesting overall, but the paper suffers from several deficiencies. These include 1) **insufficient presentation of the data available for model development and validation**, 2) **insufficient model detail for understanding, and for replicability of the work**, and 3) **a rather lengthy and vague presentation style**. In terms of the first point, the paper is not clear on sources of information for model development and validation. Can the authors provide quantitative values in the case study description, for the equations and parameter settings, and for model validation? Can they compare model projections with values from other studies? For the second and third points, the model is described through a high-level overview only. Clearly, space does not permit a detailed description of each sector, and the authors reasonably refer the reader to previous papers for further information. However, some aspects of the work are new and should be described in greater detail, ideally also with relevant model structures, equations and data sources. The major problem here is that the authors have developed a quantitative simulation model, but have presented their work entirely qualitatively. This high-level framing means that the study cannot meet its aims of improving "understanding of the complex interactions among human and natural systems…", since it is not clear exactly how these systems are connected and the

effects of these connections are not analyzed in the paper. Additionally, in terms of study context, the work should also be presented in the context of river basin models and perhaps integrated water resources management (IWRM), as well as its current focus on IAM.

I believe GMD to be an appropriate journal for publication, and so recommend major revisions rather than rejection; however, making the necessary changes will take significant time – perhaps longer than the journal permits for revisions.

- The authors appreciate your comments and have made significant changes based on your comments. These include: 1) addition of the ANEMI_Yangtze data system (Line 1104-1118) and comprehensive model validation and sensitivity analysis (Line 1301-1358), 2) detailed description of the new aspects of the model, including the stock and flow diagrams, the equations and their underlying theoretical basis, parameters and their associated sources (see Line 1103-1300), and 3) improvement of paper presentation.

The following are specific comments:

- Line 71: Include also models like MESSAGE, AIM, POLES, REMIND, TIMES and others? The authors should also review recent publications on integrated assessment modeling, such as Fisher-Vanden and Weyant (2020, Annu. Review Resour. Econ.), Calvin et al. (2019, GMD), Krey et al. (2019, Energy), Gambhir et al. (2019, Energies), and others
  - The authors appreciate your comments and have updated related references.

- Line 78: Downscaling of IAMs is an ongoing effort. There have even been recent calls to downscale them to the city level (Dermody et al., 2018, Earth Syst. Dynam.). The GCAM model currently has several sub-national versions, including GCAM-USA, GCAM-China, GCAM-Latin America and others in development. The references here should be revised
  - The authors appreciate this comment and have updated related references.

- Lines 81-106: This section contains neither numerical values nor references to literature/government reports. Please revise
  - The authors appreciate this comment and have made significant changes in this section. Please see Line 135-483.

- Section 2 of the paper is also very light on details. For example, line 146 states "[The Yangtze Economic Belt] is home to many advanced manufacturing industries, modern service industries, major national infrastructure projects…". Given that the paper is intended to introduce and discuss a simulation model of the river basin, more information must be provided. A model is

neither understandable nor reproducible from this level of description. Can data tables be provided here? Alternatively, a thoughtful discussion of data limitations and necessary assumptions could be provided instead. System dynamics can prove valuable in such contexts, but this point is not explored in the article.

- The authors appreciate your comments and have rewritten this section. Please see Line 135-483. The background of Sytem dynamcis is also added, please see Line 543-560.

- The purposes of sections 3 and 4 could be explained. It was not clear to me why two different sections were required, when both described the model in general terms
  - The authors appreciate this comment and have merged these two sections into one section.

- Line 222: ANEMI represents "a different approach" to which other options?
  - The authors have deleted this description.

- Line 227: Other IAMs also capture feedbacks and nonlinearities. What is it about ANEMI that is unique?
  - The authors have deleted this sentence.

- Line 245: ANEMI_Yangtze is "downscaled" to the river basin scale. The literature review should therefore also reference basin-scale models and their capabilities. How does ANEMI-Yangtze compare?
  - The authors appreicate your comment and have referenced a few basin-scale models. Please see Line 129-134. Comparison will be a bit "too much" considering that models at that scale should and are reflecting specific characteristics of the basins.

- Line 268: Please demonstrate the point that interactions matter through analysis of model results
  - The authors appreciate this comment. We have added a new S_energy scenario to explore the impacts of shifting energy consumption patterns on the Belt's system performance. In this section, we have thoroughly analyzed the nonlinearity and feedbacks in determining the system behavioural of the Belt. Please see Line 1359-1465.

- Line 302: Please define terms like "water stress" with reference to relevant literature. Which equation is used?
  - The authors have defined the term "water stress" in the **4.6 Water** subsection. Please see Line 1265-1282.

- Line 307: Increased N and P concentrations are important. However, other studies use measures like dissolved oxygen. Can the authors please discuss modeling choices like this in more detail, or refer to alternative sources?

  - The ANEMI_Yangtze's representation of the N and P nutrient cycle is adapted from ANEMI (Breach and Simonovic 2018; 2020; 2021), which has its basis on Mackenzie et al. (1993).

  Breach, P. A., and Simonovic, S. P.: Wastewater treatment energy recovery potential for adaptation to global change: an integrated assessment. Environmental Management, 61, 624-636, https://doi.org/10.1007/s00267-018-0997-6, 2018.

  Breach, P. A., and Simonovic, S. P.: ANEMI 3: Tool for investigating impacts of global change, Water Resources Research Report no. 108, Facility for Intelligent Decision Support, Department of Civil and Environmental Engineering, London, Ontario, Canada, 133 pages. ISBN: (print) 978-0-7714-3145-6; (online) 978-0-7714-3146-3, https://www.eng.uwo.ca/research/iclr/fids/publications/products/108.pdf., 2020.

  Breach, P. A., and Simonovic, S. P.: ANEMI: A tool for global change analysis, Plos One, 16, 0251489, https://doi.org/10.1371/journal.pone.0251489, 2021.

  Mackenzie, F. T., Ver, L. M., Sabine, C., and Lane, M.: C, N, P, S Global Biogeochemical Cycles and Modeling of Global Change. Interactions of C, N, P and S Biogeochemical Cycles and Global Change, Springer, Verlag, pp 1-61, 1993.

- Section 4 explains some of the connections shown in its CLDs, but not the model structure, parameters, equations, and reasoning behind modeling assumptions. Describing a new model in a journal paper is difficult – so much to explain and so little space. However, a more quantitative presentation would help, and GMD is a journal that is well-suited to such a description. Since much of the model structure has been explained in earlier papers, the authors could focus these detailed descriptions on novel components. The CLDs themselves are very nicely presented.

  - The authors appreciate your comments and have made revisions accordingly. In the original manuscript, Section 3 describes the background of the model and the cross-sectoral interactions and feedbacks, and Section 4 mainly describes the interactions and feedbacks within each of the nine sectors. These two sections are all about the interactions and the causal loop diagrams (qualitative in nature). Therefore, in this revision, we have merged these two sections into one section. In addition, we have added a new section describing the novel components of the model, including the stock and flow diagrams, the equations

and their underlying theoretical basis, parameters and their associated sources. Please see Line 1103-1300.

- Line 389: The number provided here illustrates a key problem with the model description. What is the source of the number 0.95? It is uncited. It is also the only number in the entire subsection.
  - The authors appreciate your comments and have referenced the source.

- Figure 8: What is "water demand"? Is it "water withdrawal" or "water use"? Or is it an economic term, with the possibility that water demand > water supply? Please define terms
  - Water demand here is an economic term defined as "as the volume of water requested by users to satisfy their needs - in a simplified way it is often considered equal to water withdrawal, although conceptually the two terms do not have the same meaning". In addition, there is a possibility that water demand > water supply.

- Line 487: How does water pollution affect life expectancy in the model?
  - The effect of water pollution acts as a multiplier ($Pollution_{multi}$) on life expectancy and takes the following form: $Pollution_{multi} = a \cdot PI^2 + b \cdot PI + c$, where PI is pollution index, a, b, and c are calibrated parameters.

- Line 491: Does "weathering" also include releases of N and P to surface waters through fertilizer use, domestic wastewater, and so on?
  - The "weathering" here refers to the natural process. The releases of N and P to surface waters through fertilizer use, domestic wastewater are anthropogenic processes.

- Line 515: Please rephrase "to verify the feasibility…". The model is validated, rather than verified, and "feasibility" is not the correct word
  - The authors appreciate your comment and have rephrased this sentence.

- Section 5: What are the data sources used to validate the model? Please tabulate or discuss
  - We have added a new subsection 4.1 The ANEMI_Yangtze data system. In this subsection we discuss both historical data that are used to initialize and validate the model and future parameters that govern changes in the future. Please see Line 1104-1118.

- Line 523: Can the authors provide examples of the types of policies ANEMI omits?
  - The authors have provided an example at the Line 13167-1320.

"For example, overcapacity in coal production gradually appeared after the mid-1990s, and this situation worsened after the outbreak of the 1997 Asian financial crisis. To alleviate the overcapacity crisis, the government issued series of documents to reduce production, and this is responsible for the production drop around 2000 seen in Figure 16(d)."

- Figure 13: Please describe the reference scenario used here
  - We do describe the reference scenario in the original manuscript. Please see Line 1360-1370.
    "Under the S_base scenario, all the policies remain at their 2015 values during the simulation. Specifically, the one-child policy remains unchanged for the Population Sector. The intensity of water withdrawals/consumptions in industry and agriculture for the Water Sector, the energy shares among different energy sources for the Energy Sector, and the fishing mortality for the Fish Sector shall all remain their 2015 values, respectively. The N/P removal efficiency in the Nutrient Sector is 0. The exogenous inputs of precipitation and temperature take their historical average annual values."

- Line 578: "We focus on analyzing the nonlinearity, delays and feedbacks…". The paper did not really analyze the results. Rather, it presented simulation results, compared them against historical values, and identified causes briefly on pages 26-27. The analysis should be expanded
  - The authors appreciate this comment. In this revision, we have added a new S_energy scenario to explore the impacts of shifting energy consumption patterns on the Belt's system performance. In this new section, we have thoroughly analyzed the nonlinearity and feedbacks. Please see Line 1359-1465.

- Lines 580-598: The results presented in the paper do not really support these conclusions. Indeed, these points are not explicitly presented in the results section. For example, please show results that illustrate the effects of an increasing population on demands for resources and the resulting pollution effects (line 582), or that a growing economy drives energy production and consumption (line 585).
  - The authors value your comments and have rewritten the conclusion and discussion sections. Please see Line 1466-1583. Also, we have added S_energy scenario in the model application section. In this scenario, the impacts of shifting energy consumption patterns on the Belt's system dynamics was analyzed thoroughly. Please see Line 1359-1465.

- Line 595: Why was this connection not modeled?

- The reseason why we didn't include this connection in our model is because the data necessary to describe this relationship are currently not available. So, we envisage including this in our future work when more data are available.

Referee #2

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 authors appreciate your comments. we have added a new section describing the new aspects of the model in detail (Population, Food, Energy, Water, and Fish Sectors). Please see Line 1103-1300.
  In this new section, the stock and flow diagrams are presented, the equations and their underlying theoretical basis are discussed, and the parameters and their associated sources are referenced.

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.

- The authors appreciate this comment. In the new section which describes the new aspects of the model, we have discussed the model equations and their underlying theoretical basis, and referenced model parameters and their associated sources. Please see Line 1103-1300.
- In this new revision, we have included a parameter sensitivity discussion section (Please see Line 1323-1358). Justifications for the selected parameters are provided. We did not vary each parameter separately, for simplicity we vary all the selected parameters by a fixed variation range and include two scenarios: mild variation scenario (parameters are varied by -10% ~ +10%) and extreme variation scenario (parameters are varied by -50% ~ +50%).
- The aim of sensitivity analysis is not to validate the parameters used in the sense of how well they represent the real world. It only provides confidence in the model (whether the main state variables will exhibit alternative behaviours).

= 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.

- It is worthwhile to mention that ANEMI_Yangtze model is not meant to predict the future for the development of the Yangtze Economic Belt. Instead, it aims to help understand the behavioural consequences of various policy options. The absolute values **are** important, however, the emphasis here is on the model behaviour so that we can analyze the feedback mechanisms that are driving the model to future states.

- As for the validation, the system dynamics simulation modelling approach is often used to analyze complex and interlinked systems that could include social elements or decision-making processes that can be more abstract or where a high degree of uncertainty exists in measurements. That is why in the field of system dynamics simulation, true validation and verification are deemed impossible.
- Unlike the global scale research, there are many available projection results to compare. For the Yangtze Economic Belt, research is rare, projection comparison is not available right now. So we only compared modelled variables to historically observed data. To build more confidence in our model structure, we also conducted the sensitivity analysis. Please see Line 1323-1358.

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?

- As the focus of this paper is the model description, a **comprehensive policies application** is beyond the scope of this paper.
- In this revision, we have added a new S_energy scenario in which the impacts of shifting energy consumption patterns on the Belt's system dynamics was analyzed thoroughly. Please see Line 1351-1457.

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.

- We value your comments and have rewritten the conclusion and discussion sections. Please see Line 1466-1583.

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.
  - The authors appreciate this comment and have removed the description of the qualities of the region.

- What evidence is there that riverine N and P levels have a significant impact on human mortality?
  - There is a lot of research published on water pollution's impact on life expectancy. However, there are no references that are directly describing riverine N and P levels' impacts on human mortality. In our work the N and P levels are used just as indicators of water pollution.

- 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.
  - The authors did their best to improve the language use.