Response to Reviewer 1

I want to thank the reviewer for these very detailed comments. I am really very grateful for the close read and all of the comments are valid and well taken.

Comment	Response
The model necessarily simplifies in order to simulate dynamics under historical conditions as well as two counterfactual scenarios. The analysis is interesting and thought-provoking.	Thanks for the positive view of the general approach.
Please review and revised claims to be consistent with the scope of the paper.	Agreed. Please see responses to specific comments below.
It would seem that a definition of water security is warranted in the introduction before going too far, so that the reader can understand what the author is referring to.	I agree. Water security in generally is poorly defined and some definition (albeit not a universal one) is needed.
Then, another even narrower definition, based purely on cost, emerges in the modelling section on p.13281. I would not recommend calling this water security, since cost itself means little unless compared with income and more, generally, with livelihoods.	This narrower definition was picked after looking at the detailed Chennai study where cost of water was found to be the single biggest determinant of how much water people eventually used. It is true that cost means nothing if everyone can afford the higher cost. However, in the specific case study, dependence on tankers makes the cost of water rise by a factor of 10 – which makes it unaffordable to most income groups. Of course, the very wealthy 1% are not very affected by ten-fold increase in but since the equity implications are explored elsewhere, I felt that in this paper using cost as a proxy for well-being is justified.
The case study description is surprisingly static given that this is a dynamics paper.	This is a very fair comment. I will expand on this section starting with a longer history of Chennai's water supply.

Household dependence on private wells (and incidentally also on taps) would also include reliance on others' (e.g. neighbors) existing wells (and taps), which will clearly be subject to change.	Our household survey (panel data over 2 years a wet and a dry one) did not really point to evidence of people's buying water from neighbour's wells/taps. This will be clarified in the paper.
However, I have serious concerns about modelling user demand as exogenous (p. 13273), since water policies are likely to affect both supply and demand.	I am not sure I agree here. Urban water policies do affect demand but mainly through pricing. Unlike agricultural or industrial water use, urban water demand is much less elastic. Even if the cost of water is reduced to zero and infinite supply is available, there is only so much water people can use for domestic purposes. Therefore, in the model the demand is influenced by supply and price.
Specific Comments	
1. As motivation, the abstract puts forth that few studies attempt to "why some regions develop sustainable, secure well-functioning water systems while others do not." The problem with this statement as a motivator for the study is that the paper only discusses one city, so by construction it also cannot address this question. I suggest the abstract be revised to more clearly state the contributions of this particular paper, which are mainly descriptive rather than comparative or allowing causal attribution of any type.	Very true. Although the original motivation of the paper was to do a comparative analysis – I never got around to doing this. I will scale down expectation significantly here if I am unable to add sensitivity analyses on this.
"Third, the effects of mismanagement do not manifest right away." But this is not a new insight. It has long been argued in the economics and sustainability literatures that the social phenomenon of discounting plays out in this way. What would be more interesting is to consider if this phenomenon interacts with technical or physical realities in	Very true also. This is hard to show with a single case study but I think what is needed here (which may still be possible) is to play around with aquifer characteristics – asking what if Chennai has a much deeper or much shallower aquifer and then see if this affects how quickly the impacts of mismanagement manifest.

ways that have not been conventionally understood.	
The approach taken in the paper is framed as: “simply justifying the choice of outcome variables by referencing contemporary debates over water security and acknowledging the limitations of the choice made.” This sounds very ad-hoc and appears to be precisely the approach that is being criticized as too case-dependent. I would urge the author to reconsider this position or the criticism. “If socio-hydrologic models are intended to feed into the policy process, the researchers cannot truly remain an external observer of the system. The very process of deciding what to model, which model variables are static and which ones may be changed in the model could inadvertently influence which futures are possible making the model a self-fulfilling prophecy. I am not sure I follow, since this sounds like prediction, not description. Some clarification would help here.	I agree this needs clarification. As I see it there are currently two schools of socio-hydrology which caused the confusion in this sentence. One school defines socio-hydrology as backward looking, descriptive focused on modelling bi-directional feedbacks (e.g. Kandasamy et al, diBaldasarre et al in special issue). The other school (e.g. Gober and wheater, Lane papers in the special issue) views socio-hydrology as bringing transparency into how hydrology is implemented in practice and policy. So carefully sifting through which components of models involve value judgements and which involve objective scientific fact and coevolving models through stakeholder interactions. E.g. In her review comment Gober writes *“If socio-hydrology is to move from what Srinivasan calls backward-looking to forward-looking models, its members will need to shed their unease with the policy process and play a more active, enduring, collaborative, and transparent role in the decision support and decision making process. Indeed, the great strength of exploratory modelling is that it uses stakeholder feedback to identify socially relevant trade-offs, builds trust in the modelling process, and integrates the qualitative knowledge of stakeholders with the more quantitative knowledge of model developers.”* The Chennai model tries to find some middle ground. Ideally, I would like to find a way to better build on the strengths of both schools of thought. I do think that even backward looking models should pay some attention to model conceptual framing. However, I will try to rewrite this or failing that

	drop the criticism in this paper.
Second, do tankers and local wells supply directly to households, and if so, why would there be leakage loss in these terms?	Correct. The leakage should be zero in these terms. I will check the model but more likely the error is in the paper equation.
Third, the operation and maintenance model appears to ignore the fact that capital investments in expanding the network may also reduce the funds available from tariffs, depending on how this financing is achieved. Can this assumption be justified?	I agree that this needs clarification and will do this in the paper. At present, the model uses a very simplified feedback equation in linking the tariffs (and % of costs recovered) to the rate at which capital investments are fulfilled. I feel a simplistic model like this cannot have a detailed model of the finances of the utility but I am open to other suggestions here.