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Discussions

Interactive comment on "Coevolution of water security in a developing city" *by* V. Srinivasan

Anonymous Referee #1

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Review of "Coevolution of water security in a developing city"

Summary This paper uses the case of Chennai water management to develop a descriptive model of the evolution of water resources that is applicable to the context of urban water supply in less-developed countries. 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. Still, many assumptions that are made require additional justification, and the claims made in the front sections and discussion are too broad and sweeping, and therefore inconsistent with the paper's scope. My main concerns are summarized below.

General comments 1. Please review and revised claims to be consistent with the scope of the paper. My general impression is that these are largely overstated. (More specific comments follow). 2. 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. Where does water quality (entirely absent from the introduction) fit into this definition? The definition eventually comes at the end of section 2, but it is exceedingly narrow and likely to be unsatisfactory to most readers. Then, another even narrower definition, based purely on cost, emerges in the modeling 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. 3. Section 3: The case study description is surprisingly static given that this is a dynamics paper. The reader wants to understand more of the history of water supply and demand in and around Chennai, rather than simply being presented with the current status. This is particularly so given that the paper purports to be backward-looking and descriptive. On the other hand, the questions about future water supply and changes, which get some attention, are less critical given the paper's putpose. 4. Section 4: In the model, how can it be justified that "household dependence on private wells" is taken as constant if the water supply becomes unreliable. It might be the case that no new well construction occurs over a short time horizon, but even this is somewhat questionable. 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. In fact, a variety of studies from less developed countries (as well as economic theory related to changing supply costs and substitution effects) with unreliable systems document extensive and nimble changes in water sourcing by households. 5. I can understand making economic and population growth exogenous to the water situation (although this should perhaps be justified based on existing literature which finds little evidence of direct relationships between water and growth). However, I have serious concerns about modeling user demand as exogenous (p. 13273), since water policies are likely to affect both supply and demand. This is an issue that has great implications for thinking about water market equilibria. More justification is required if exogenous demand is to be maintained. In addition, though this issue is simpler to resolve, assuming water demand to be fixed over time (as income is rising) is not consistent with economic theory or empirical evidence. 6. I have a number of specific concerns with the modelling assumptions (see specific comments below). 7. I have some questions about the realism of the counterfactual scenarios. Simply assuming that Chennai would start with double the reservoir storage that is did in 1965 (the good engineering scenario) would seem to be wishful thinking – how would these investments be financed? Note that this is linked to my modeling concern above wherein infrastructure costs are not included in the cost recovery problem. 8. Again related to the definition of water security as equivalent to cost, the analysis leads out some important considerations. First, how should one think about higher relative cost (in scenario 2) compared with higher variability in cost (scenarios 1 and 3)? This is a classic insurance type of question. Second, is this variable continuous or is there a threshold? As you can see, I am very much puzzled by this concept.

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. 2. In the abstract, the third major insight is stated as: "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 ways that have not been conventionally understood. I am not sure that is the case, and so I am not sure if this is a new insight. But perhaps this can be tied in a better way to the second insight on initial conditions, path dependencies, and evolving water security. 3. 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. 4. The author suggests that socio-hydrology is "backward looking" but then points to challenges related to: "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. If description is the goal as stated in the subsequent paragraph, why even bring this up? 5. Modeling: First, why is it necessary to assume that non-tap coverage drops from half to 33% by 2005. Shouldn't this be based on data? Second, do tankers and local wells supply directly to households, and if so, why would there be leakage loss in these terms? 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? In the results, it becomes apparent that perhaps tariffs are considered to allow supply expansion, but the mechanism for this is totally unclear.

Suggested technical corrections 1. Abstract, line 5: Should be "water studies attempt". 2. Abstract, line 15:The author of a case study can argue, or the case study can provide evidence to suggest that, but it cannot itself "argue". 3. Abstract, lines 17-18: "When consumers are forced to purchase expensive tanker water, they are water insecure." Is this necessarily the case? What if they are rich? Is water security so simply defined? 4. Introduction p.13268, lines 30- : This sentence does not make sense as written; it appears some connecting words are missing. Also, is it necessary to understand dynamics over large scales, or is there also a place for smaller scale studies. I have to admit that I do not accept that large scale modeling is essential here. Comparative studies can still be focused at fine scales. 5. Page 13269, line 7: "bi – feedbacks" does not make sense. Maybe you mean bi-directional feedbacks. 6. Page 13270, line 8: This sentence is awkward as models is repeated. Could be streamlined. 7. Page

13270, line 19: Revise to "the modeler needs to make a choice. . ." 8. Page 13271, lines 25-26: Something is missing in the sentence, which is not grammatically correct as written. 9. Page 13273: Do illegal connections dynamics fit into the model somewhere, as a response to pricing policies? 10. Page 13281, line 16 should read: "starts in 1965 with double the water. . ." 11. I suggest shifting the description of scenarios to the case study section, rather than results. The scenario definition is not a result. 12. Section 5.1: "In this scenario, Chennai's population rises exponentially from just 400 000 households." It was my understanding that population growth was exogenous, so why is this only in scenario 1? Similarly with the driving assumptions on rainfall, etc. I thought all this was taken as given. 13. Page 13282, lines 16-17: Something (a connecting word?) is missing in this sentence. 14. Page 13282, line 22: Grammatical problem: "The cost of water is much high. . ."

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