

Interactive comment on “Relationships between environmental governance and water quality in growing metropolitan areas: a synthetic view through the coupled natural and human system lens” by H. Chang et al.

Anonymous Referee #1

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(1) This paper claims to use "coupled human and natural system (CHANS)" and/or "social ecological systems" (SES) approach for modeling the "feedback" effects between environmental governance and water quality. However a close reading of the paper reveals that this paper does not accomplish or develop either a CHANS or an SES model of the case study areas. Instead, as shown in figures 1 and 2, a "conceptual" model is presented. It is not clear how the "feedbacks" are modeled in this "conceptual" model. The introductory section sets the reader to anticipate a new CHANS or SES model, but instead a bunch of regression equations and t-tests are presented in the results

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section without even elaborating about the nature of "positive" or "negative" feedbacks in the coupled system.

(2) It is not clear why the following three questions are even included in the manuscript (as they are not addressed later in the manuscript):

"1. How do differences in local and state levels of governance and policy affect the resilience of both social and ecological landscapes? 2. How do alternative land use planning strategies affect provision of ecosystem services in response to different disturbance factors? 3. How effectively do the processes and outcomes of monitoring ecosystem services provide a usable feedback loop in urban socio-ecological systems?"

While there is a growing and, often contested literature on "resilience" in SES modeling, this study does not provide any clear idea about what do the authors imply about "resilience", i.e. how is "resilience" measured and operationalized in the case study context.

Further, the notion of "governance" is never defined and rather reduced to a proxy variable (monitoring) that might not even reflect "governance" in a social and/or political system. What is the nature of intergovernmental system in place? How are civil society actors included in "governing" the water quality in the case study regions and so forth are perhaps more important "governance" questions than the easily measurable proxy of "monitoring".

Discussion about alternative land use planning strategies that are available to the case study policy makers never takes place in the manuscript. The NLCD database is used to re-construct the "baseline" land use patterns in the case study areas, but the discussion about "alternate land use planning strategies is missing in the paper (both theoretically and methodologically).

Similarly, the notions of "ecosystem services" and "useable feedback loop" are also not

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clear.

(3) After a rather ambitious section that lays out large claims about CHANS and SES modeling, authors reduce the research paper's goals to addressing the following specific research questions:

"1. Does monitoring effort differ as a function of governance? 2. Do riparian conditions differ between the two watersheds and do they correlate with indicators of water quality? 3. Do land development patterns differ between the two watersheds and do they correlate with water quality? 4. Is there a relationship between water quality and the sale price of properties and, if so, does that relationship vary between the two watersheds?"

None of these four specific questions inform the discussion about "usable feedback loops" for SES modeling. In fact, some of these questions are even redundant. For example, governance is defined as "monitoring" and then "monitoring" is used to (mis)characterize the governance in the case study regions. The feedback effect of water quality on governance (as claimed earlier in the paper) is not even addressed in the first specific question.

The other three specific questions 2, 3 and 4 are reduced to estimating "correlations". Methodologically, "correlations" can NOT be used to measure "feedback loops" in SES modeling. More importantly, none of the correlation questions 2, 3 and 4 contribute any new knowledge in the hydrological sciences. For example, it is already well known that different riparian conditions "correlate" with different water indicators. It is also well known that different land development patterns affect water quality differentially. The hedonic models have also established the relationship between water quality and property prices. There is no surprising finding either in addressing any of these four specific questions for the two case study areas.

(4) There are host of specific technical and methodological issues that also require serious reconsideration: Why is water temperature used as the key indicator for wa-

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ter quality? Why other indicators are not used (except in the case of property price sub-study!!)? Worse, water temperature is a function of a large number of parameters (in addition to the canopy cover), however none of these other parameters, e.g. climatology, that can potentially affect water temperature are even controlled for. A t-test cannot be used to control for other parameters that also affect the water temperature in the streams. Similar issues are evident for the hedonic pricing model. Prices of the properties are not mere simple functions of water quality, or distance, rather other factors such as distance from highways, schools, hospitals or even macroeconomic cycles and so forth can also affect the housing prices. The time series pricing data has typical problems of temporal and spatial autocorrelation; however none of these issues are even identified in the manuscript, let alone addressed.

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