

Interactive comment on “On the social dynamics of moisture recycling” by Patrick W. Keys and Lan Wang-Erlandsson

Anonymous Referee #3

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Overview: The paper by Keys et al presents three case studies of links between the social and terrestrial moisture recycling system. This study combines quantitative modeling of terrestrial moisture recycling with metrics and a literature review of social factors. In this way, the study estimates the major sources of precipitation (i.e. precipitation-sheds) for three case study countries. Gridded social variables are then evaluated for the source and sink nodes in each case study. Finally, a literature review is performed to reveal additional context for each case study and enable the development of moisture recycling social ecological systems archetypes. Overall, I think this is an innovative, well-executed, and (reasonably) well-written paper that would make a unique contribution to the literature. I recommend publication after consideration of my comments below.

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Major comments:

1. Human well-being/welfare has a precise definition in the social sciences literature.

The term(s) “well-being” and “welfare” are used several times in the paper. I don’t think these is the best term to use, since they mean something precise in the economics literature that is distinct to the meaning here. I think it would be better to refer to “social” aspects/variables/indicators of source and sink nodes. Then, the precise metric references should be specified whenever possible.

2. Why not perform a global scale analysis?

The literature review would be too difficult to perform for all countries in the world. However, a global scale analysis of precipitation-sheds and receiving countries would be relatively straightforward to perform. It appears the authors have all the information they need for this. They have WAM-2 pixels, social variables at the pixel scale. So, couldn’t this be a global scale analysis for most aspects? Then, the 3 case study countries could be used for the literature review portion of the paper.

If a global scale analysis is performed, then the authors will have more data to run some interesting regressions. For example, they can calculate “precipitation-sheds” and “sink” nodes for all countries. Then, they can obtain average values of social variables in each source/sink. In this way, they will have enough statistical power to run multivariate regressions of the driving factors of the terrestrial moisture recycling system.

3. Fig 3 is confusing and could be simplified.

There is a lot of information in Fig 3. I don’t think most of it is necessary. For example, does the biome information convey anything interesting? There does not appear to be any trend between malnourished children (y-axis) and GDP/capita (x-axis), so this information could be made easier to read. I think this figure would be better if it presented the average value of malnourishment and GDP/capita explicitly for the source and sink

region of each country. This might be able to be accomplished with a simple bar graph or box-whisker plot for each variable for each source/sink node. A table might even best illustrate upstream/downstream differences. This simplicity would better illustrate the main points made in sections 3.2.1-3.2.3.

4. The section on power dynamics could be improved.

There seem to be many similarities between upstream/downstream power dynamics in precipitation-sheds and watersheds. I think this section would benefit from drawing from the power dynamics concepts in the transboundary watershed literature. A lot of work has been done on power/politics in international river basins that section 3.8 would benefit from referencing.

Generally, section 3.8 could use a bit of a rewrite for clarity. Have any papers quantified the impact of upstream precipitation-sheds on downstream droughts? This seems like it would be the most clear example of upstream-downstream conflict/power issues. Also, can you expand on the Daw et al (2011) reference? Does this paper specifically focus on power dynamics in teleconnected systems?

5. A bit more connection with the SES and socio-hydrology literature would be helpful.

How does this work relate to socio-ecological systems (SES) work? Have similar archetypes (Fig 5) been presented in SES literature? Or socio-hydrology? What outcome variables are primarily of interest in the SES literature?

Minor comments:

1. The term “average market influence” is not clear and confusing. Please just call it what it is, i.e. GDP per capita. 2. Figure 4 doesn’t seem to show much. What happens if you just plot national international moisture recycling (y-axis) against GDP per capita (x-axis)? 3. P 18 line 14: “Though the analysis of environmental justice flows has been simplified (Fig 3). . .”. Environmental justice flows are not quantified or presented in Fig 3. This statement is not warranted. 4. P 19 line 5: This sentence is a bit ironic. It

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seems to be a call for interdisciplinary scientists to engage and communicate with one another. However, this sentence is laced with jargon that is not widely understood (i.e. “positivism”, “normative terminology”).

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