

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

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Summary comments:

This is a noble effort to develop and demonstrate a more substantial conceptual linkage between atmospheric water cycle research (based in the "precipitationshed" concept of the lead author) and the social and economic factors in the regions linked by these hydrologic connections. However, I feel there is quite a bit of room for improvement, even in this first attempt, in terms of clarity, consistency, and organization in the presentation. While the choice of the case studies is fine to illustrate the range of archetypes defined in the MRSES structure, the presentation is uneven, as I describe below. I think it may be an issue of completeness and communication of the ideas. The authors take

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on the difficult task of weaving together elements of climate, economics and social science, but are not always clear from sentence to sentence which they are talking about. It appears to be the case that the authors have become quite familiar with their own topic and forgotten how convoluted it can appear to newcomers. As a result, the paper rushes through a lot of material too quickly. More "handholding" would be appreciated! Several of the figures need improvement as well.

General comments:

A. For example, it took several readings before I really understood (I hope this is the point) that the important /input/ is how much evaporation in a precipitationshed is from managed land, inside or outside the country, and if that land is undergoing (or liable to undergo) land use change. Connectivity, the ranges described in the 3 archetypes, stems from this (right?). A plot of managed evaporation, or a table, would do wonders for clarity. One possibility would be modification to Fig 3 with a more stark color key designed along the axis of the degree of human management/impact.

B. The subsections in section 3.2 are only single paragraphs for each case. This, and the jumping around between cases later in the paper was quite jarring to this reader. I think it would be better to organize the results by presenting each case separately in its entirety, from the quantitative hydrologic and socioeconomic analysis to the social dynamics cases. Then Sec 3.5 can be the point where they are knitted back together in the framework of MRSES.

C. It would be helpful for the authors to draw the distinctions between "Market Influence" and the economic links between the case study countries and their neighbors, or even "global markets" as invoked in Sec 3.6. Without defining "Market Influence", which is really very local having only a vague implication that large cities link to global markets, there is a tendency to associate the two when they are not very related. Or do the authors, via Fig 4, try to assert that they are? This needs to be clarified.

D. An element of the social connectivity analysis eludes me. What is a more favorable

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archetype to be in; isolated, regional or tele-coupled? Or are there a range of implications for each (I presume this is true)? There is a natural tendency to try to view these archetypes on a scale from bad to good. If this is not intended, the authors should proactively disabuse the readers from looking at MRSES in such a light.

E. Another detail that the authors need to spell out for unaware readers is that recycling rate (within nations) depends strongly on national area (cf. Dirmeyer and Brubaker (2007 <http://dx.doi.org/10.1175/JHM557.1>); the "scaled RR" in Dirmeyer et al. 2009 accounts for this). So comparing recycling among the study countries (or more generally any countries or regions) should acknowledge the strong effect of total area. Mongolia, Niger and Bolivia are in order of decreasing size and thus expected decreasing recycling rates given other factors like precipitation regime are controlled for.

Specific comments:

1. Fig 1: Please explain the red arrow at the bottom of panel C - what does this connote?
2. P5 L10-11: It is not clear to me how this notion of using spatial sampling as a proxy for temporal sampling in social data has been exploited in this study. Can you point out, perhaps retrospectively in the conclusions or /in situ/ if there is a good example, where this has been applied?
3. P5 L16: Please define Market Influence as used here. I did go to Verburg et al. (2011) to learn this, but it is a simple enough metric that it could be described here in one sentence.
4. P5 L18: Please give the specifics of the "various resolutions" of these data sets, including the time periods they each cover.
5. Table 1: Naturally I compared these values to Dirmeyer et al. (2009) [by the way, the wrong paper is listed in the References; see: <http://dx.doi.org/10.1016/j.jhydrol.2008.11.016>] and found the recycling and near-field

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percentages in Table 1 to be generally lower. Perhaps WAM-2layers and QIBT have systematic differences in moisture advection rates?

6. P6 L6: Fig 5 is cited before Fig 4.
7. P8 L23: What is "malnourished"? As written, the rangeland systems are. I think you mean the people in those systems. Likewise in L26-27, "areas" are not hungry, the people in them are.
8. Fig 3: I think this must be mislabeled. The open circles must mark the in-country sinks and the colored dots are the various evaporation sources, no? Also, please expand or define the abbreviations "Pop." and "Resi." (actually it would be good to point out the threshold population density of 10 people/km² between the categories). Finally, what does the size of the circles indicate and what is the scale for that?
9. Secs 3.2.1 - 3.3.3 relative to Fig 3: It is not always evident by eye the assertions made regarding the relationships between elements in Fig 3. I think it would help to plot in each panel the first two moments (mean and standard deviation) as two crossed whiskers (along X and Y axes): one for all the source areas (weighted by contribution - is that the size of the circles?) and one for the in-country sink. Then their differences and the overlap of the ranges of standard deviations can be easily seen, and statements like P10 L3-4 and L9-10 would have a better basis.
10. Fig 4: The color bar is very unclear. Log scale? The numbers are linear, and appear to be multiplied by 10⁴; clearly not what the authors intend and not commensurate with the ranges in Verburg et al. (2011).
11. Also Fig 4: In fact, I cannot see how Bolivia has such a high Market Influence index (so blue) based on the data of Verburg et al. (2011); their Fig 3 shows this to be very low for Bolivia. The colors seem to have more to do with the "qualitative" descriptions in Sec 3.4 than the quantitative data.
12. Also Fig 4: It would be good to note somewhere that the X-axis naturally corre-

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lates with the size of the country (small=low) and its aridity (dry=low), while the Y-axis correlates with continentality of the climate.

13. P11 L29: "detectable changes in vegetation and associated changes in near-surface meteorology" - please provide a reference for this statement.

14. P12 L33-34: Not a sentence; appears to be missing a clause.

15. Sec 3.4.2: The Niger case is missing a discussion of the economic links among neighboring countries like exists for the other cases. This makes the final paragraph much more "hand-wavy" than the discussions of the other two cases, in my opinion.

16. Sec 3.4.3: Much is made about the strength of national land use regulations, but (1) in the particular case of Brazil they are highly variable in time, depending on which party is in power; (2) enforcement lags behind (this is discussed somewhat) and (3) the spatial and population scale of the problem makes such statements about regulation almost meaningless. The Acre region of Brazil, noted as a main external moisture source for the Bolivian precipitationshed, has experienced significant deforestation over the last 40 years, albeit not as widespread as Rondonia, which also borders Bolivia. The problem seems to be soft-peddled a bit here.

17. P14 L11-12: Likewise, the notion that the "region's land use is relatively well-governed with many controls in place to avoid large-scale change" seems untrue, and in contrast to the very next sentence. It is clear even from Google Maps that there is a very clear demarkation following the Bolivian border where deforestation is rampant in neighboring Brazil right up to the border.

18. Sec 3.5: In addition to explaining the archetypes and how they fit the previously presented data and social dynamics review, it should be frankly stated where they are unclear, or at odds.

19. Fig 5: I also find this diagram somewhat unsatisfying, but perhaps I am not understanding it. Are the boxes meant to be static, or is it the fluctuations (changes)

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in the boxes that precipitate (pardon the pun) effects in other boxes by the arrows? For instance, Mongolia has a huge non-local (regional) evaporation source, small (9%) local source, and strong export connections to its large neighbors, yet is defined as "isolated" based on other factors. The synthesis and the weights given to the various factors seems either subject to interpretation, or not clearly enough defined. If the single driving /hydrologic/ factor is land use change in the precipitationshed, it could be demonstrated much more clearly and succinctly than has been done here (see General Comment above). Then it comes to the economic consequences to fill out the MRSES archetypes - am I seeing this correctly?

20. P15 L6-7: "...large contributions..." of what? Be clear and complete. "...social processes driving the evaporation..." - this statement may be endemic of the communication problem. Presumably this is shorthand for "policy and economics drive land use change that affects evaporation potentially affecting downstream precipitation" but I think the authors forget how much better they understand their own material than their readers will.

21. P16 L5-6: This needs to be stated earlier, to clarify much of what goes before.

22. P16 L33-34: It is sentences like this that lead to confusion; the "regional factors that can influence...." means land use changes affecting moisture sources, right? If so, just say that. I think the hydrologic underpinnings get lost at times in this manuscript.

23. P17 L8: I think this is a typo: "and Brazil" should be "from Brazil"

24. Sec 3.5.3: Aren't the actual drivers of land use change (deforestation in the Amazon) much more from developed nations than is the case in Niger? Doesn't this also have implications for "tele-coupling", or not as defined?

25. P17 L28-30: This sentence would benefit from a concrete example or pointer to the specific data presented earlier.

26. P18 L1-2: Similarly, this sentence would benefit from an actual example of rein-

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forcement/surprise, and not merely describing the situation.

27. Sec 3.8: This section I found to be more clearly presented. I am reminded of the interesting case evident in Wei et al. (2013; <http://dx.doi.org/10.1175/JHM-D-12-079.1>) where evaporation from irrigation in Northeast China appears to supply a significant amount of rainfall to North Korea!

28. P19 L9: "...gives Brazil power over Bolivia in potentially significant ways." I would phrase it that it gives Brazil "responsibility to Bolivia" - this is the other side of the coin from air pollution (including nuclear fallout), where it is pretty easy to track sources to those affected downstream. We are not used to thinking of water vapor in that way, but "responsibility" gives a more overarching concept to such linkages.

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