

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

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Received and published: 7 March 2018

Reviewer Comment = RC Author Comment = AC

RC 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

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below. I think it may be an issue of completeness and communication of the ideas. The authors take 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.

AC. We are pleased that the Reviewer has taken a considerable amount of time to both interpret our paper, and to provide detailed feedback on how to improve the work. We hope that our responses below will both answer the lingering questions and address the problems identified by the Reviewer.

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General comments:

RC-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.

AC-A. Thank you for the comment, and we appreciate that the Reviewer spent so much time attempting to glean this insight. Ultimately, 'managed evaporation' (as the reviewer puts it) or evaporation that can be or is actively changed is the implicit focal point of the biophysical system. If the Reviewer implies that 'managed' includes all the policies, cultural pressures, economic incentives, legal regimes, treaties, etc. then yes, 'managed evaporation' is the key input. While we think there could be merit

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in a table of managed evaporation, estimating this for all the systems, is an academic task in its own right.

Our goal in this paper is to explore potential ways for characterizing the social connections that link the recipient of precipitation back to the sources of evaporation: i.e., (a) describing conceptual and actual social linkages among sinks of precipitation and sources of evaporation, and (b) developing a method for linking existing methods of quantitative moisture recycling analysis (e.g. precipitationshed calculation), with new methods of quantification (e.g. relating well-being indicators to moisture recycling sinks and sources), and qualitative analysis of economic and social policies related to land-use change.

We recognize the value of a table that explores managed evaporation, so we include the following text in the new section "4.6 Limitations":

"Evaporation can be or is actively changed through e.g., policies, cultural pressures, economic incentives, legal regimes, and treaties in the social systems, and limited by e.g., water availability, edaphic suitability, and energy limitation in the biophysical system. The type and nature of this manageable or managed evaporation is important for understanding the management space. Thus, future work could undoubtedly extend and perhaps substantiate social linkages by first identifying and quantifying managed evaporation within different administrative zones. Likewise, specific policies could be linked to these administrative zones, which could explicitly link legal, policy, and on-the-ground management efforts with particular flows of evaporation, and subsequently moisture recycling."

RC-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

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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.

AC-B. Thank you for the comment, and we have used your suggested changes. We now present each case in its entirety, including (a) precipitationshed analysis, (b) land use distributions among the sources and sink, (c) social linkages between sources and sinks, and (d) literature review of policy and management.

RC-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.

AC-C. We agree that Market Influence needs to be better defined. Market Influence is calculated by multiplying normalized travel time to cities and ports, with national level per capita GDP in terms of purchasing power parity. Given that Verburg et al. (2011) goes into the nuanced relationship between market influence, wealth, and connection to global markets, it seems a bit redundant to do that again. We do cite Verburg to ensure it is clear where to explore those additional arguments.

RC-D. An element of the social connectivity analysis eludes me. What is a more favorable archetype to be in; isolated, regional or tele-coupled? Or are there a range

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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.

AC-D. Thank you and we agree with the Reviewer. It would be wrong to conclude that one archetype is preferred to another and that there are (as the Reviewer suggests) a range of implications for each. We explore this and other aspects of the MRSES in section 4.1 "MRSES archetypes are idealized".

RC-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.

AC-E. We appreciate this detail, and for mentioning "scaled RR". We do not scale the recycling ratios in this paper, as the biophysical reason for low or high recycling ratios have limited relevance for the social implications for individual nations. In fact, smaller nations already tend to be more reliant (or be subject to) outside influence beyond atmospheric moisture connections: e.g., in terms of global markets, national security, and climate change impacts.

Specific comments:

RC1. Fig 1: Please explain the red arrow at the bottom of panel C - what does this

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connote?

AC1. Thanks for the comment, this arrow is simply highlighting the social connection that is often missing from depictions of sources and sinks in moisture recycling research. The arrow is red to match the red boxes containing the "Social" node, and points in both directions indicating that social links can connect in multiple directions. We have clarified this in the updated caption to Fig 1.

RC2. 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?

AC2. We agree that this is unclear. In the revised version of the paper the sentence is no longer relevant, so it has been removed.

RC3. 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.

AC3. We agree with the Reviewer, and as indicated in previous comments, we have expanded the definition of all variables in section 2.4 and Table 2.

RC4. P5 L18: Please give the specifics of the "various resolutions" of these data sets, including the time periods they each cover.

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AC4. Thanks for the comment, and this feedback was echoed by other Reviewers. We have included this information in Table 2, which summarizes the different datasets, including variable name, description of variable, source resolution, units, time period of analysis, and source reference. This table is found in the Methods section.

RC5. 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 percentages in Table 1 to be generally lower. Perhaps WAM-2layers and QIBT have systematic differences in moisture advection rates?

AC5. Thank you for comparing the values, and for noticing this error in the citation. Many apologies! It could be that we misinterpreted the comment, but we ought to be comparing our results to Dirmeyer et al. (2009) Table 6 (i.e. "Top three external contributors of ES, expressed as a percentage of total precipitation over each nation.")

If this comparison is correct, the key countries, as well as corresponding percentages of contribution, match quite well, excepting for some differences in percent contributions to Bolivia. We will refer to this previous work given that it generally supports the results we find, while illustrating the disparity for volumes coming from Peru and Brazil to Bolivia.

RC6. P6 L6: Fig 5 is cited before Fig 4.

AC6. Thanks for the comment, and we have carefully checked all references to Figures and Tables to ensure that the references are (a) in the correct order, and (b)

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that the Figures and Tables do not precede their first reference.

RC7. 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.

AC7. Thank you, and indeed we did not write this correctly. We have corrected these, and carefully read the text for similar errors.

RC8. 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?

AC8. Thank you for the suggestions here. Based on your feedback Fig 3 has been completely remade (per your suggestion in Comment 9 below), and we no longer include the Anthrome data, nor the Anthrome colorbar. The land-use data is now presented as a histogram in a separate panel. A brief explanation of the Anthrome data, including that it incorporates a population density component, is found at the explanation of variables in section 2.4.

RC9. 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

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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.

AC9. Thanks for the comment. This is an excellent suggestion, and indeed adds to both the clarity of the results, as well as provides more robust information about differences among sources and sinks. We have made this change to the figure, and can be seen in each of the case studies.

RC10. Fig 4: The color bar is very unclear. Log scale? The numbers are linear, and appear to be multiplied by 10^4 ; clearly not what the authors intend and not commensurate with the ranges in Verburg et al. (2011).

AC10. Thank you for the comment, and we agree that the colorbar was incorrect. In the interest of streamlining the text, we have since deleted this Fig 4 from the text.

RC11. 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.

AC11. Thank you for the comment, and we agree that the colorbar was incorrect. In the interest of streamlining the text, we have since deleted Fig 4 from the text.

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RC12. Also Fig 4: It would be good to note somewhere that the X-axis naturally correlates with the size of the country (small=low) and its aridity (dry=low), while the Y-axis correlates with continentality of the climate.

AC12. Thanks for the comment, as stated, we have deleted this figure.

RC13. P11 L29: "detectable changes in vegetation and associated changes in nearsurface meteorology" - please provide a reference for this statement.

AC13. Thank you, and the relevant citation was mistakenly included in the sentence immediately prior. This has been fixed now.

RC14. P12 L33-34: Not a sentence; appears to be missing a clause.

AC14. Thanks for the comment, and we have edited the sentence for clarity. It now reads:

"This is relevant primarily because significant areas of land acquired for agriculture (estimated at 360,000 hectares in GRAIN (2012)), could lead to extensive potential modification of the land surface, with associated impacts on moisture recycling."

RC15. 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.

AC15. Thanks for the comment, and we have corrected this in the updated text.

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Please see the new section on the economic interlinkages among the region and beyond.

RC16. 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 precipitation shed, 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.

AC16. Thanks for the detailed attention to this section. We agree with the Reviewer here, and have modified this text considerably. We particularly draw attention to the fact that there is in the cases of the interior of the Amazon considerable discrepancy between the stated aims of government policy and the actual impact on the ground.

RC17. P14 L11-12: Likewise, the notion that the "region's land use is relatively wellgoverned 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.

AC17. The satellite imagery you point to on Google Maps is indeed compelling, and a literature review of relevant deforestation trends in this region bears out your observation. We have modified this section considerably to reflect this updated

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information. We hope the updated text on deforestation policy, enforcement, and reality is now more consistent with the Reviewer's understanding of these systems.

RC18. 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.

AC18. Excellent point. We have included new Discussion section 4.1 "MRSES archetypes are idealized" where we explore these issues.

RC19. 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) 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

AC19. Thanks for the comment, and for the clearly deep consideration of this figure. The archetype classification is subject to interpretation, which is what we did based on the synthesis of the different aspects of our analysis. Other reviewers echoed this feedback about the issue of subjectivity, and we have added several sentences at throughout the text that emphasize the blending of methods, and the explicit use of an interpretive and subjective set of methods.

Furthermore, we include a new discussion section titled "MRSES archetypes are idealized" to discuss (a) where why the MRSES might be unclear or at odds with one another, (b) emphasizing that there are benefits and disadvantages to the different MRSES (per this Reviewer's suggestion), (c) emphasize that understanding MRSES requires subjective interpretation of results, given the blending of analytical approaches and the presence of value-based judgements (e.g. higher child malnutrition

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is subjectively unfavorable).

RC20. 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.

AC20. We appreciate the feedback, and recognize that this ambiguous language can lead to confusion and frustration. We have modified the text in this specific location, and have considered this feedback throughout the entire paper.

RC21. P16 L5-6: This needs to be stated earlier, to clarify much of what goes before.

AC21. We agree, and have moved this to the beginning of the explanation of the archetypes indicating that since it is contained within all the archetypes it ought to be stated clearly at the beginning.

RC22. 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.

AC22. Thank you again, and we have fixed this language.

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RC23. P17 L8: I think this is a typo: "and Brazil" should be "from Brazil"

AC23. Thanks, fixed.

RC24. 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?

AC24. Yes, and this was intended to be part of our point. Evidently we did not make the point well, so we have clarified the text.

RC25. P17 L28-30: This sentence would benefit from a concrete example or pointer to the specific data presented earlier.

AC25. Thank you for the comment, and we have provided a concrete example referring back to the isolated archetype Mongolia, in the form of Mongolian land-use policy.

RC26. P18 L1-2: Similarly, this sentence would benefit from an actual example of reinforcement/surprise, and not merely describing the situation.

AC26. Agreed. We have added an example of this in the context of Amazonian policy within Bolivia

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RC27. 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!

AC27. Thanks for the comment, and this is an excellent example. We have added this in the second paragraph of this section.

RC28. 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.

AC28. Thank you for the suggestion. We have thought about this phrasing, and have switched it to the following: "For example, demonstrating that Brazil is very important for Bolivia's rainfall potentially adds a matter for negotiation between the two countries, with all that entails, especially in terms of responsibility and power."

Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2017-120>, 2017.