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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Recent evolution of China's virtual water trade: analysis of selected crops and considerations for policy" *by* J. Shi et al.

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General comments:

This work brings some new insights on China's international virtual water trade, but the major part of the analysis is not new. Indeed, the authors are analyzing the international virtual water trade network from 1986 -2009, which has already been built and analyzed by Dalin et al (2012), for the 1986-2007 period. The authors are not analyzing a new network (the " virtual water trade network of China") but are actually looking at a specific node (China) of the global virtual water trade network, a previously studied network (as

in references cited by the authors, e.g. Chapagain et al 2005, Dalin et al 2012, etc.). Moreover, Dalin et al. had focused some of their analysis on China. Thus, previous findings are repeated by the authors in this manuscript (e.g. l.11-15 in the abstract is a previously drawn conclusion).

We agree with the review that there are previous studies exploring the global virtual water network. But a virtual water trade network centered at China has never been solely analyzed. Since the share of virtual water trade linked with China is high, it's of importance to have the perspective from the standpoint of this country. More important, we have several unique findings from this country-level analysis, including dominance of grain crops in China's virtual water trade, the correlation between geographic location and net virtual water import/export, the reemerging of scale-free property in the China-centered network (comparing to the scale-free property in the whole global virtual water network), the high heterogeneity of China's network etc. We believe a country-specific network analysis on big players in the virtual water trade is equally important with the analysis on a global scale. Also, country-specific analysis is of special importance to assist national policy-making process. We appreciate the efforts of trying innovative approaches like network analysis to provide new insights into virtual water research and we hope to contribute to this exploration and spur more and deeper research along the direction.

A few new results can have some significance: in section 3.1, the dominance of grain crops (Figure 2), in section 3.2, the location of countries importing from China (Figure3b) and in section 3.3. the "super fat tail" node strength distribution. However, some new results, such as structure of China's connections (section 3.3 - network properties), are not analyzed to show their potential impact or significance.

We agree with the review that the implication of the network property is under discussed and we have added more texts (as follows).

"Scale-free property shows the existence of trade partners with a degree that greatly

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exceeds the average ("hubs"). This confirms the high heterogeneity of the network, in line with other network analyses and implies the robustness and weakness of the network, i.e., if failures occur at random and the vast majority of nodes are those with small degree, the likelihood that a hub would be affected is almost negligible; even if a hub-failure occurs, the network will generally not lose its connectedness. However, on the other hand, if the very few trade partners with heavy degree are removed, then the pattern of the network would be significantly changed, which is not at the favor of a steady food supply chain."

Section 4.2 presents an interesting review of policy implications, but the base for this discussion seems to have been brought up by other publications rather than by this work. The paper could be presented as a discussion rather than a new piece of evidence for discussion.

We have largely revised the discussion section, especially 4.2. We mainly base the discussion on our findings. We realized it's not a synthetic paper to address general issues of China's water policy so we organized the discussions in a way that draws implications directly from our results into policy sphere.

Finally, the methods used for calculation of VWC need to be clarified.

We have clarified the methods in the response to specific comments below.

Specific comments:

Abstract and section 4.1 (last paragraph): Water savings are not formally analyzedhere, and thus should not be presented as a new finding in the abstract.

We agree with the reviewer and have removed the sentence from the abstract.

Section 2.3 Please explain methods in more details. Liu et al (2007) refer to CWR calculations in Chen 1995. But this last reference seems to be unavailable online (using the English reference as cited in Liu et al. 2007). It is of high importance to describe and specify the authors' methods and data sources for CWR calculations;more specif-

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ically: please include equation 1 from Liu et al 2007, cite data sources for CWR, A and TA. The authors should also specify the temporal and spatial resolutions of each variable used to estimate VWC in this manuscript. Crop cultivated areas used in Liu et al. 2007 are from several China Statistical Yearbook (NBSC). However, more spatially detailed datasets are now available for the globe (e.g. Monfreda et al 2008, Ramankutty, etc). Thus, the authors should justify their choice of using these specific datasets from NBSC.

We thank the reviewer for raising the possible unclear points in methods. And here we hope to make a clarification. The reference to crop water requirement (CWR) calculation in Chen et al. (1995), contained in Liu et al. (2007), is a published book only available in Chinese. It is the most authoritative literature for CWR in China. It provides CWR dataset of China at a provincial level, but as Liu et al. (1995) mentioned, "yearly crop water requirement data are not available, we assume the same water requirement for a certain crop in different years in the same producing region". And NBSC provides specific yields dataset at a provincial level, which is sufficient to calculate national average CWR, combined with provincial-level CWR dataset in Chen et al.(1995). In this manuscript, we consider Liu et al. (2007) a recent and qualified relevant work and we cite its national average CWR data for the calculation of VWC. As all the calculation details of national average CWR has been clearly described in Liu et al. (2007), we think it's unnecessary to include this part in the main body of this manuscript. The national and provincial data are sufficient to analyze the virtual water trade patterns in China in this paper. Although more spatially explicit datasets are available (e.g. Monfreda et al 2008, Ramankutty, etc), we do not need grid-based data for the virtual water trade analysis (and it is also difficult to do such analysis because trade data are not available at a grid cell level). For the national and provincial data, NBSC is the most commonly used data source. We have clarify this in the revised manuscript.

p.11621, l.6 Please justify why the average of reports from China and from the other partner is not used, instead of ignoring trade partners' reports.

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There are various reasons including differences in statistical methods, reporting time, reporting systems, etc., which, might cause differences in reported data between trade partners. Firstly, we assume such data divergence is negligible if any; and secondly, we also assume the possible error induced by such divergence is no greater than the confidence interval of the reported data themselves; thirdly, it is not convincing to assume the possible error satisfy normal distribution, thus, it is not proper to use the average value of reported data between trade partners as seen in some literature, which, could not effectively reduce non-normally distributed error. Lastly, as we already assume that no direct trade was taking place if no data were reported between trade partners, which could be viewed as one special example of data divergence, it is consistent with the way that we solely used the data reported by China.

Sections 3.1 and 4.1: The way partners are counted is misleading in comparing the importance of connections between China and different world regions (e.g. since there are only 3 countries in North America, versus 25 or more in Europe). The authors should look at the percentage of countries in a region that trade with China rather than at absolute number of trade partners per region.

We are hoping to give a geographic illustration of how China's virtual water trade partners are located so the focus is not to compare the "importance" of connections between China and different world regions by the "number" of trade partners. The countries are divided by continents according to (http://www.worldatlas.com/cntycont.htm). Under such categorization, there are 23 countries in North America, 12 in South America, 14 in Oceania, 44 in Asia, 47 in Europe, and 54 in Africa. Admittedly there are more countries in some continents than others do, but the building of a trade partnership is influenced by many factors, including geographic distance, economic comparative advantages, international relationship, etc. And what matters most to the whole trade pattern is the "weight" (i.e. the virtual water traded) of the connections. So it cannot be assumed that the number of trade partners in a continent with more countries must grow faster than the one with fewer countries. Therefore, we think the number of coun-

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tries in a continent is an irrelevant factor in this case. We thank the reviewer to raise this issue and we have adjusted the sentences not to leave readers the impression that the growth of trade partners in a certain continent is related to the“capacity of countries” of that continent.

p. 11623 l.17 What does this scale-free property means for the network? Please justify here the analysis of the s-k relationship, and interpret findings.

We agree with the reviewer that the interpretation of k-s relationship is under discussed and we have added more discussions in the manuscript. We present the analysisof k-s relationship is to explore the intrinsic characteristic of China’s virtual water trade network as follows.

“Scale-free property shows the existence of trade partners with a degree that greatly exceeds the average (“hubs”). This confirms the high heterogeneity of the network, in line with other network analysis approaches and implies the robustness and weakness of the network, i.e., if failures occur at random and the vast majority of nodes are those with small degree, the likelihood that a hub would be affected is almost negligible;even if a hub-failure occurs, the network will generally not lose its connectedness. However, on the other hand, if the very few trade partners with heavy degree are removed, then the pattern of the network would be significantly changed, which is not at the favor of a steady food supply chain.Also, the identification of the network property is helpful to contribute to building models for projecting the future evolvement of the network”

Section 3.4 This entire section can be found in a previous study, cited in other sections of this paper (Dalin et al. 2012), in which Chinese imports of soybean and VWT are discussed in Fig 4A and in text of page 4. On the same note, Figure 7 of this manuscript is practically identical to Figure 4A in Dalin et al. 2012.

Soybeans are of particular importance in China’s crop-related virtual water trade as its prominent role. It’s reasonable to present such an example in focus following general analysis in China’s virtual water trade. Figure 7 in this manuscript and Figure 4A in

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Dalin et al. (2012) shows similar trend of soybeans associated virtual water trade, which demonstrates the validity of the datasets used in both research. But there are also obvious differences in the pattern of virtual water trade especially before 2000. Please note that the range of traded virtual water is also different because we used different datasets.

Table 1: Discuss assumptions made to obtain CWR of rice as in Liu et al. 2007. The significant difference between percolation and no percolation (8000 vs. 4550) may change the results importantly. Please discuss as necessary.

This has been discussed in Liu et al. (2007) as follows,

"For other crops, percolation is considered as a water loss". For rice, however, percolation does not mean a pure loss. It is necessary for achieving high rice yield in an anaerobic soil environment. Recent results from northern China have recorded 'anaerobic' rice yields of 8.0-8.8 ton/ha in flooded lowland systems, which are much higher than the recorded 'anaerobic' rice yields of 4.7-6.6 ton/ha (Bouman et al., 2002). Many experiments in China have shown that sufficient percolation is needed for higher rice yields (Chen et al., 1995)."

Technical corrections:

p.11615 l. 22 : contributes

We have changed "contributing" to "contributes"

p. 11616l. 21 A study l. 29 China, which

We have added "A" and "," accordingly.

p. 11617 l.1 fluctuations, has l.12 because there are more l. 17 Is the "wastewater" in Guan and Hubacek 2007 coming from agricultural sector only? This study also includes industrial commodities. l. 18 impacts water quality

We have added ","and removed the reference for it's out of the scope of this study.

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p. 11619 l.5 it is calculated l.7 such as in. . .(references) l.14 based on Equations 1 and 2: add an index for country in VWC.

We have changed “it can be calculated” into “it is calculated”. We have added “such as in”. We have taken Review 1’s recommendation to use descriptive language to explain the calculation process, therefore, we eliminated all the equations to make the paragraph more readable.

p.11622 l.13 46% of all VWE (typo?) l.28 even

It’s not a typo. VWE refer to virtual water export and we have changed all the acronyms of VWE into VW export to avoid confusion. We have changed “evenly” into “even”.

p. 11624 I think the authors meant to refer to the "fat tail" characteristic of the distribution, usually mentioned in the literature (not "flat tail").

We thank the reviewer and have corrected this typo.

p. 11626 l.13 heterogeneity

We have changed “homogeneity” into “heterogeneity”.

p. 11627 l.20 cite reference

This statement has been removed.

p.11628 l.25 need to be adjusted

We have corrected the grammar.

p. 11631 l.11 and l.13 Rodriguez-Iturbe

We have corrected the spelling.

Fig 1: panel a: Again, show percentage of countries per region rather than absolute numbers.

Please see the previous explanation.

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Fig 4: cite Software used (i.e. CIRCOS)

We thank the reviewer to point out this omission. We have added the proper citation both in the figure and in the reference section.

Fig 5: Usually shown in log-log scale rather than semi-log like here.

Dalin et al. (2012) also used a semi-log scale, so we followed this approach to better compare the results.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/10/C6747/2013/hessd-10-C6747-2013-supplement.pdf

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