

Interactive
Comment

Interactive comment on “Gender and climate change in the Indian Hindu-Kush Himalayas: global threats, local vulnerabilities” by M. V. Ogra and R. Badola

Anonymous Referee #2

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This paper is potentially a useful addition to the growing literature on gender dimensions of climate change adaptation. It adds some useful insights from the Himalayas, based on case study material from India. It includes an extensive discussion on overall debates centering on climate change, with a focus on the problematic exclusion of gender issues. Primary data is based on a case study focusing on ecotourism as a form of livelihood diversification, which provides useful insights to the way local communities are changing and adapting their livelihoods in the face of rapid change.

It will be important to include this paper on gender issue in the special issue. However, conceptual, methodological, case study, flow and structural issues in the paper need to be addressed. Overall, the paper, as currently written, is disjointed. The concep-

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tual framework does not support the primary case study, and the conclusions jump to new issues without adequate framing. The paper requires tightening. Specific scientific issues include: i) many “grand” questions are raised and unnecessarily drawn out, but not adequately answered (technical interventions, holistic approach, mitigation, north/south power imbalances, gender power imbalances, etc.). The paper needs to get the point about gender issues faster, shorten the conceptual discussion considerably, and use freed-up space to develop and analyze the case study more deeply. ii) the case study focuses on eco-tourism as a diversification strategy, but the links to adaptation are weak (with some mentioning in the table); the analysis therefore needs to be deepened considerably, using a tightened/focused conceptual framework. iii) the question of attribution needs to be addressed; is livelihood diversification solely a response to climate change, as the paper suggests, or are other forces at play as well? iv) Some conceptual issues require attention. The term gender is used as a way to analyze the homogenous category “women” and “men”, “women’s needs”, etc. The paper criticizes homogenizing tendencies (i.e. Mohanty) and out-dated WID approaches but falls prey to the same uncritical tendencies without analyzing gender power relations or differentiating along class, age, marital status, etc. This needs to be centrally engaged in the analysis of the case study material, and not as suggested in the paper, to have this “more sophisticated analyses” (page 22) picked up by future research. Its uncritical focus on “social systems” (assumed to be bounded entities) is problematic; it is suggested that the authors consider political-ecology concepts to analyze gender power relations, inter and intra-household gendered negotiations, etc. (as authors they cite engage). v) The paper needs to be more specific regarding methodology (i.e. how many interviews, how many women, how many men, how many times each were interviewed, overall profiles of the participants, etc.). vi) In the conclusion, issues of climate modeling and climate justice appear without prior context. Although the issue of climate justice and equity are important, they need to be framed beforehand (i.e. what evidence in the case study tying to climate justice). vii) In several places, the authors assume or assert (i.e. “assuming”, result “may be caused by...”, “may well”, “may re-

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sult in”, etc.) rather than focusing on the evidence in hand. viii) In many places, the paper remains general, without evidence to back up/frame particular arguments (i.e. direct impacts, indirect impacts, gendered impacts, gender-differentiated outcomes, etc.). ix) While the authors argue that women’s resilience on natural resources is reduced, one wonders about newly added pressures of additional people (i.e. tourists) in their demand for food, fuelwood, water, etc.? Is this factored into account? If so, do they still lead to positive adaptation? x) We are told the area is agro-pastoral, but this not adequately discussed or elaborated.

Technical corrections requiring attention and clarification include: - conceptual clarity and consistency in the use of the categories “male/female” and “men/women” which are inter-mixed throughout the paper (often in the same sentence) and in the tables/figures (it is suggested adding that a short explanation on how the word gender and sex differences are understood and used in the paper); - the use of the term Hindu-Kush Himalayas and Indian Himalayas (the paper is more focused on the latter, given there the one case study, and therefore it is advisable to drop the word “Hindu-Kush” and simply use India, or Indian Himalayas); - critically read the statement on page 6 referencing Neumayer and Plumper (2007), go back to the original paper, as the statement does not reflect what they are suggesting; - list specific countries where gender differences in death rates disappear in societies where women and men enjoy equal rights (reference by Aguilar, 2010); which countries are these? And what do death rates in disasters have to do with the primary data from the case study? - the title (global threats, local vulnerabilities; gender) does not reflect the content of the paper (diversification; women); - the abstract could be tighter, as it raises some issues that are not adequately developed in the paper (i.e. season pastoral migration, discursive gender/climate change nexus, women’s lack of political and economic authority in decision-making processes – at what levels? Which institutions?, etc.); - correction of grammatical errors and typos are required.

Interactive comment on Earth Syst. Dynam. Discuss., 5, 1491, 2014.

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