

## ***Interactive comment on “Assessing floods and droughts in the Mékrou River Basin (West Africa): A combined household survey and climatic trends analysis approach” by Vasileios Markantonis et al.***

### **Anonymous Referee #2**

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The paper presents data for the Mekrou River Basin in West Africa from two different sources: a survey conducted in the area, covering Benin, Niger and Burkina Faso and climate data for the same region. While the idea of combining data from surveys with climate data is in principle promising, the paper has several problems:

- (1) The abstract describes the datasets and the methodology, but fails to point out clearly what the subsequent study aims to show. Similarly, even after reading through the paper it is not entirely clear, what the basic message of the paper is.
- (2) The literature review in the introduction is far from being exhaustive. Many important and influential studies on the impact of climate change and natural disasters on

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economic development are not mentioned at all. To mention only a few:

Barrios, S., Bertinelli, L., Strobl, E., 2010, Trends in Rainfall and Economic Growth in Africa: A Neglected Cause of the African Growth Tragedy, *Review of Economics and Statistics* 92(2), 350-366.

Berlemann, M., Wenzel, D., 2016, Long-term Growth Effects of Natural Disasters. Empirical Evidence for Droughts, *Economics Bulletin* 36(1), 464-476.

Cavallo, E., Noy, I., 2011, Natural Disasters and the Economy – A Survey, *International Review of Environmental and Resource Economics* 5, 63-102.

Dell, M., Jones, B.F., Olken, B.A., 2014, What Do We Learn from the Weather? The New Climate– Economy Literature, *Journal of Economic Literature* 52, 740–798.

Felbermayr, G.J., Gröschl, J., 2014, Naturally Negative: The Growth Effects of Natural Disasters, *Journal of Development Economics* 111, 92-106.

Hsiang, S.M., 2010, Temperatures and cyclones strongly associated with economic production in the Caribbean and Central America, *PNAS* 107 (35), 15367-15372.

Skidmore, M., Toya, H., 2002, Do natural disasters promote long-run growth? *Economic Inquiry* 40, 664-687.

Skidmore, M., Toya, H., 2007, Economic development and the impacts of natural disasters, *Economic Letters* 94, 20–25.

(3) Whenever it should be a goal of the paper to contribute to the literature on evaluating the costs of climate change and natural disasters, the authors should mention other approaches existing in the literature and explain in how far the results presented in the paper are superior to these methods. As an example, the authors should refer to the Life Satisfaction Approach which has often been used to evaluate the costs of natural disasters. See e.g.

Luechinger, S., Raschky, P.A., 2009, Valuing flood disasters using the life satisfaction

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Approach, *Journal of Public Economics* 93, 620-633.

Welsch, H., 2006, Environment and Happiness: Valuation of Air Pollution Using Life Satisfaction Data, *Ecological Economics* 58, 801–813.

Welsch, H., Kühling, J., 2009, Using Happiness Data for Environmental Valuation: Issues and Applications, *Journal of Economic Surveys* 23, 385–406.

(4) The authors claim that they combine climate data and survey data. However, I did not really understand where they are really combined. In section 3.1.4 the authors report on the population perception on the occurrence of extreme events and climate variability. However, the authors simply report the outcomes of their survey, here, without confronting the perceptions with reality (as measured by climate data). In section 3.4 the authors present some regression analysis where the reported costs of floods and droughts are related to some other variables. However, again the climate variables seem not play any role herein. It seems as the authors only discuss the two sorts of data in the same article without combining them in a meaningful way.

(5) The regression analysis in section 3.4 is conducted and/or reported very poorly. First, it remains completely unclear, why the regression analysis is conducted at all. As the result of the analysis the authors simply report the “average cost of floods per household” and “the estimated cost of droughts per household that experienced droughts”. Apart from the fact that both formulations are very imprecise it is completely unclear why a regression analysis has to be conducted to find out about the costs as they are directly reported in the survey. Maybe the goal is to find out which factors determine the magnitude of the costs of affected households. But then the authors should state this clearly and discuss the hypotheses they want to test. They also make any attempt to present theoretical arguments explaining which variables should enter the regression equation. Even the variables used in the regression are explained poorly. The variable ECONSTAT seems to describe the households’ wealth. However, as the variable is not metric, it makes little sense to include it in a linear regression. When-

ever it shall be used, the categories should enter the regression equation as dummies. I also do not understand why the first regression includes no constant while the second one does. Again, there is no explanation. The authors do neither report a measure of the goodness of fit (such as r-square) nor the results of an F-test, as it is usual in regression analyses. The authors also seem to neglect possible heteroscedasticity, a problem occurring in almost all linear regression models. And finally, the authors' description of the choice of variables which finally enter the model (all variables reaching a P-value of less than 0.05) does not fit to Table 9, which also contains "LivestockLoss" with a P-Value of 0.068. Altogether, the empirical analysis in section 3.4 is completely flawed.

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