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Natural Hazards and Earth System Sciences
Discussions

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.

Vasileios Markantonis et al.

vmarkantonis@gmail.com

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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:

'Response: We thank the Referee for taking time to review our work and for the constructive comments. We hope that a deep revision of the manuscript, including the modifications detailed below, could be effective in addressing all the issues highlighted.'

(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.

'Response: The aim of this study is to provide an assessment of flood and drought by combining remotely sensed climatic data and locally collected household information. On the one hand, we aimed at producing information about a sensitive topic, as the assessment of economic and social impacts of extreme events, in an area of the world that is poorly represented in literature. On the other hand, we wanted to compare the natural hazard perception of the local population with the actual measurements of the climatic data about the extreme events referred in the survey. Additionally, the findings of this analysis are likely to benefit local decision makers toward a more effective disaster risk management in developing countries. In the revised version of the manuscript, we will revise the abstract, as well as the rest of the paper, to better highlight our aim and scope, stressing at the same time the novelty of our contribution.'

(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 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.

'Response: We agree with the Referee that some of the vast literature about natural disasters and their impacts was overlooked in the first version of this manuscript. We will revise the introduction and the discussion sections including also the suggested references. We will make sure to review literature about the impacts of natural disaster and climate change on the economic growth of the least developed countries. '

(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 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.

'Response: We are aware of the many approaches presented in literature aimed at evaluating the costs of natural hazards, such as Life Satisfaction Analysis, Cost of Illness, Contingent Valuation, Input Output Analysis, Hedonic Pricing etc. However, natural hazard impacts cost assessment was only one of the goals of this analysis. Therefore, in the first version of the manuscript, we did not provide an extensive literature review about natural disaster cost assessment. However, we agree with the Referee that, since we analyzed also this aspect, we should include some literature review about this topic. That for, in the revised version of the paper, we will provide a

natural disaster cost assessment literature review summary.'

(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.

'Response: First of all, we would like to apologize for the error in numbering the 3.1 subsections (page 7 and 8). The whole section 3.1 was dedicated to the presentation of the results of the analyses conducted over the trends detected in: precipitation (page 7 lines 9-25); Standard Precipitation Index (page 7, line 28 to page 8, line 11); heat-waves (page 8 lines 15-20); and river flow trends (page 8, line 24 to page 9, line 5). In the same section, as mentioned by the Referee, we presented the results of the survey (page 9 lines 8-32). We agree that the results were firstly presented separately, but in the discussion section (section 4) we combined the results of the survey with the climatic trends observed. In particular we stated: "This paper combines the results of a household survey and climate data analysis to assess floods and droughts as well as climate variability in the Mékrou river basin in West Africa. The opinions and perceptions of household representatives revealed a strong climate variability at a ten years period (2006-2015). It is worth mentioning that 83% of the population, during this period, noticed a delayed onset of the rainy season. In addition, 91% of the population observed also an anticipated end of the wet season. Moreover, 88.5% of the respondents reported a general reduction of the precipitation during the ten years period under consideration, and 75.9% reported an increase in magnitude and frequency of the extreme heat events. This tendency is partially confirmed by the analysis of the climatic variables, mainly based on precipitation and temperature data. The findings of the analysis confirmed the increase of both frequency and magnitude of the heat-waves in the area of study. The climatic variability was also found noticeably high, but the Mann Kendall analysis failed in finding statistically significant trends in the precipitation patterns. It was not possible to identify clearly a shift of the intra-annual temporal distribution of the precipitation, neither indicating a slightly delayed onset nor an anticipated end of the rainy season" (page 13, lines 1-13). In the revised version of the paper, we will further describe the combination of the survey results with the outcome of the analysis of the climate data. Regarding the regression analysis in section 3.4, we have tested how climate variables influenced the costs of floods and droughts, but unfortunately they proved to be statistically non-significant. For this reason, as explained in the methodological section (Page 4, Line 30), they were not included among the variables determining the costs. In the revise version of the study, we will include alternative compositions of the statistical model including also the non-significant variables to fully describe our findings.'

(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.

'Response: We agree with the Referee that the regression analysis could be further elaborated and better explained in both scope and nature. Our main aim was to investigate the relative importance of those factors influencing floods and droughts costs, independently of the overall estimated values. This would represent a very important

analysis for the natural hazards related decision making and planning. Beside linear regression, we tested also other approaches, including logarithmic regression, but the approach presented in the paper appeared to be the best performing for the problem under consideration. In the revised version of the paper we will provide additional statistics proving these aspects.'

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.

'Response: Variable selection was conducted basing on their statistical significance. As mentioned in the response to the previous comment, in the revised manuscript, we will provide alternative compositions of the statistical model in order to discuss the possible use of other variables and to demonstrate their poor significance in this context. A more detailed description of the variable used (mainly presented in table 7 in the current version) will be also provided. Additionally, theoretical considerations explaining which variables should enter the regression equation, also considering existing literature, will be included.'

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.

'Response: Since the dependent variable is continuous, a linear regression model is appropriate to analyze the problem under consideration. The use of categorical predictors in linear models makes perfectly sense. However, we realized that our use of the variable ECONSTAT was typical of a 2-levels dummy, while the variable includes 5 categories. We will revise our statistical model to correct this and include other possible composition of predictors. In the revised manuscript, we will provide statistics including F-test and R-square, and we will discuss the problem of heteroscedasticity. We agree with the Referee that the P-value of the variable "LivestockLoss" is (a little) higher than 0.05, however we decided to include it in the model due to the importance of the animal farming in the area under consideration. Also the significance of this variable will be tested again in the light of the deep revision of the statistical analysis proposed above.'

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