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Interactive comment on "Climate Change impacts the *Water Highway* project in Morocco" *by* Nabil El Moçayd et al.

Anonymous Referee #2

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Overall, this paper presents a simple but useful analysis of the sensitivity of runoff in Morocco to changes in precipitation and PET. The approach is simple due to significant data limitations within the region. While the methods used in the paper are not new, their application to a data-limited environment is new and has potential utility in many other regions. My general comments are as follows: 1. While the simplicity of the approach is warranted in this region, I think the simplicity also necessitates examination of the uncertainties in the approach and results. Where possible the uncertainty should be directly quantified. Where that is not possible, it should be discussed. For example, the authors conclude that the elasticity coefficient is near 1.6 due to the similarity of the three regression slopes in Figure 10. However, each regression slope has significant uncertainty in its value due to the limited number of observations in each plot. The regression uncertainty is easy to quantify because the authors are using standard regression analysis. It would also be interesting to compare the implied range of elasticity from the historical data to the range inferred from the empirical relationships. Similarly, summarizing the results with single lines in Figure 12 seems to overstate the certainty of the analysis. Could ranges be presented instead? 2. The analysis is largely based on the assumption that dry years in the past produced runoff in the same manner as a dry climate will. An analogous assumption is made for PET changes. I think the paper would be strengthened by a short discussion about factors that may limit the applicability of this approach/assumption (or why this is a good assumption). For example, this approach seems to imply that the within-year variations of rainfall in the dry climate will be similar to the within-year variations of rainfall during dry years. I think it would be helpful to discuss such implicit assumptions (and to evaluate them if possible). It seems like sufficient data are available to investigate the seasonality assumption. It also seems to imply that the vegetation cover changes the same way in response to a climate change as to a single year drought. Can that assumption be justified? Furthermore, the analysis seems to assume independence between the effects of PET and precipitation variables. Such independence would be violated if there is a transition from snowfall to rainfall. Would that confound the analysis? Does the historical record suggest such independence? Is such a transition expected? 3. The analysis makes an implicit steady-state assumption when evaluating the water highway project. Specifically, forecasts of 31 years into the future are used to analyze the project's viability, but those conditions may not be representative of the project design life. Imagine if the design life is 30 years. If built today, the project would experience a range of conditions between the current climate and the predicted climate that might make it more viable. The authors should mention this issue in their paper and provide an argument about why 31 years is appropriate for this assessment. 4. The analysis relies on dynamically downscaled GCM results. However, the paper does not discuss how that downscaling was accomplished and only cites another paper that is in preparation. I find this problematic since the validity of that work has not been substantiated by a peer-reviewed publication.

Specific Comments 1. The paper needs some additions that help relate the analysis methods to the datasets that are used. Please add explanations about how the variables in the various equations are calculated using the data (e.g., the evaluation of the derivative, the time step of the data, etc.). 2. Some of the variables are not clearly defined, such as phi and F and F'. 3. Can you make a quick analysis to justify why replacing missing data with the mean is appropriate? 4. Table 4. Fonctionnal needs to be translated, I believe. 5. In the first half of the paper, the first sentence or two in several sections is redundant with previous sections. Please remove redundancies.

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