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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Climate change and drought effects on rural income distribution in the Mediterranean: a case study for Spain" *by* S. Quiroga and C. Suárez

Anonymous Referee #3

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1- Overall quality of the paper

This paper intends to present econometric/statistical models to evaluate the variation in agricultural income (not clear if at farm or farmer level) resulting from drought.

The paper focuses on a relevant field of research, which is gaining increasing attention in time, and which is far from being scientifically saturated (more quantitative research is needed to shed light on the income distribution – not only in the agricultural sector - as a function of climate change). However, several changes should be brought about and the overall paper presentation must be improved (some parts should be presented

for proofreading). Finally, too many economical or technical concepts are managed with approximation and are not appropriately defined (coob-douglas, gini coefficient, and estimation methods).

More specific comments and suggestions for improvement follow below.

2- Section addressing individual scientific questions/issues ("specific comments")

ON THE INTRODUCTION:

1. The introduction should be better structured. . .by:

a) Being more concise, simplifying, and avoiding repetitions; b) Being more precise in expressing concepts (many times the authors mention "productivity" without saying of what; many times they refer to a production function without specifying for what; many times they mention crop income functions and production functions as if they were the same. . .). Also, it could be better that the same concept is always expressed with the same words, rather than using continuously different expressions). c) Being more precise in describing (very shortly in the introduction given the existence of a section on "methods") the steps undertaken and the methodology/ies used (by reading abstract and introduction a reader is still confused about objectives and methodology). d) Better stating the main objective (too many objectives are stated). e) Eliminating in the introduction parts related to "methods"; eliminating from the section "methods" parts that should be included in the introduction (like objectives, and motivation: e.g. see paragraph N10-15 in p. 4357 in the section "methods"). f) Proofreading English in the abstract and introduction;

2. The abstract is confusing and should be rephrased (see comments for the introduction) stating more simply and clearly i) objectives, ii) methodologies, iii) main results.

3. The review offered by the authors could be more exhaustive including a more comprehensive overview of existing methodologies and studies and eliminating the paragraph N15-20 (p. 4355) that is of poor use in the context of this investigation. For

example, what is the link between climate driven migration, justice inequality, and the objective of this analysis? I would better focus on literature much more related to income distribution and agricultural productivity.

4. In paragraph N5 (p. 4355) the authors stress the relevance of considering market issues in dealing with adaptation and mention models performing sectoral studies or not dealing with market dynamics (in their opinion...). However, market issues are accounted for in the model types mentioned by the authors (e.g., general equilibrium models, sector models, agent based models, etc.). For example, in the case of the general (and even partial) equilibrium model, its ability to account for economic feedback mechanisms in time, space, agents, and sectors is precisely one of its major strength (see Michetti and Zampieri 2014 on the differences in treating economic and environmental/climatic variables by different models). The authors should consider the revision of that paragraph not to state the opposite.

5. In paragraph N5 (p. 4356) the authors refer to the need of reducing the amount of water in the future. However, the concept of efficiency in the use of natural resources involves two components: the input and the output quantity. A better use of this resource translates not necessarily in a reduction of the denominator (water quantity/input side); a better performance could also result from an increase in the denominator (portion of irrigated land with the same amount of water). For this reason, it could be more appropriate no to talk in absolute levels but in relative ones. They could refer to efficiency in the use of water or similar periphrasis.

6. In the same page (paragraph N5-10) the authors state "We have selected those crops representing Mediterranean crop systems. Cereals grapes and olives ...representing a higher proportion of harvested area..." I believe the authors should add some figures to prove it (or a reference) at Mediterranean level and for the Spanish case.

7. In the paragraph N20-25 (p. 4356) the authors refer to the existence of big socio-

economic conflicts in Spain related to the use and management of water without specifying which ones. Doing it could support further the motivation of their investigation.

8. The authors claim the relevance of their exercise in the context of adaptation. However, they also mention mitigation in more than one occasion. The discussion on adaptation and/or mitigation could be more interesting and clear better framing the relationship the two in the context of their analysis. Adaptation and mitigation are dependent one from the other. The greater the effort in mitigation (i.e. more stringent target is foreseen in CO2 concentration reduction in 2100), the lower will be the cost of adaptation to climate change (although, of course, there are effects already in place that cannot be avoided anymore, and for which adaptation is required independently of the future effort in mitigation). Framing these issues in this sense would also help and guide the discussion on results, where the authors mention again mitigation in addition to adaptation.

ON ASSUMPTIONS, FUNCTIONS AND INDICES CHOSEN:

1. In their analysis, land use is maintained constant over a long period. While this can be a valid assumption for the short run, it surely represents a problem for the long run. I am aware of the limitations of such an approach used in the context of this exercise with respect to representing land use change; however, I believe that the authors should at least discuss the possible implications of not considering land use as a time variant variable.

2. Why do the authors use a Coob-Douglas function, just because it is the simpler way of dealing with production? Have the authors tested the use of different functions? They should motivate the use of this functional form, at least by mentioning references and stressing pros and cons in their specific case.

3. Some relevant variables affecting land use decisions could have been omitted in the designed model. Among factors influencing land decisions and agricultural productivity traditional literature always includes the land quality and characteristics (Ricardo, 1817;

Von Thünen 1826 and Wartenberg, 1966 for the English version) proxied with, e.g., rent differentials across uses and space. Space autocorrelation has been acknowledged as a very relevant aspect (Bockstael, 1996; Smith and LeSage, 2004; Brady and Irwin, 2011), not to talk about the relevance of crop prices, land prices, and other economic variables that have been omitted as well. If the authors choose not to account for them (perhaps they intend to include everything in the error term), at least they should mention their relation with productivity.

4. SPI index. a. Compared to more complex and complete indexes to assess drought (such as The Palmer Drought Severity Index), the SPI index, does not involve any consideration on temperature, which is responsible to affect evapotranspiration. The authors should discuss the implications of this lack of information. b. I wonder why they use the SPI index in a dummy form, losing the relevant information that could be captured if considering the accumulated amount of precipitation. What is the gain in using a 0-1 variable for drought (which is defined at annual level, if I understand correctly,) rather than constructing an ad hoc variable based on the seasonal information on precipitation (since the authors have this information at their disposal)? c. Have the authors controlled for collinearity problems between precipitation variables and SPI? d. The authors claim that they analyze the effect of climate extremes (drought) but consider drought as a dummy variable. Is their framework sufficient to say that they have analyzed their impact on income distributional effects? Can't they consider threshold or range variables? Can the authors comment on this? Also, rather than stating that they assess climate change effects they should focus on what they really analyze – drought - which is just a very small subsample of climate change effects.

5. Given that the authors use an unbalanced panel data, how do they treat and face the problem of missing data?

6. If I am not wrong, uncertainty (resulting standard errors) should not to be considered as true when dealing with marginal effects, unless corrections terms are considered. The authors should therefore use appropriate statistical measures to assess uncer-

tainty, for all the cases where semi-elasticities are in place.

7. Why did the authors refer their analysis to SRES rather than the more recent RCPs and SSPs scenarios? Anyway, on the choice of the specific scenarios considered one wonders why did they chose precisely the E1 and A1B. Was this choice led by some socio-economic Spanish context or where these scenarios chosen randomly? The authors should better justify their choice.

8. In p. 4365, paragraph 20-25 the authors say "…olives are the one with the highest probability of having more risk and also of generating more inequalities in rural areas". It would be interesting including some consideration on how many farmers are today working on this specific sector and trying to quantify the possible impact in terms of economic loss and land area allocated to it.

9. In page 4357, paragraph 20-25 (section 2.1), the authors say "So we first need to define and estimate a productivity measure". However, it is not clear how they define productivity and why, given the dependent variable they chose. What kind of productivity do they refer? Can the authors be more precise in explaining and defining?

10. In table 3, what are the figures presented? Marginal effects? It is not clear from the text and the title of the table

11. The authors state, "The effect of size (land) is not relevant in determining crop productivity…" (p. 43-64 paragraph 20-25). Nevertheless, if I am not wrong, the variable Land is defined as the value of the planting area and is expressed in thousands of 1990 euro. In what sense do the authors refer to the "size" of land given the way they have constructed this variable?

12. It is not clear what could be the long-term implications on competitiveness.

13. In the discussion, the authors speculate on market mechanisms to explain their results. However, their model is not able to analyze these market mechanisms and dynamics. While interpreting results, they should avoid mentioning about it or at least

state more clearly that it's just a speculation not derived from the model results.

3- Compact listing of technical corrections (typing errors, wordiness, etc.).

1. What do the authors mean by "crop income functions"? 2. Is the smaller unit of observation the farmer or the farm (it seems that the two words are used indistinctly)? 3. What are the "estate variables"? Can the authors explain or use more clear expression? 4. Either choose to write Sect. or Section. (see paragraph N4, p.4357) 5. Income losses, crop income production, productivity, social distribution, distributional effects. . .too many concepts and words, sometimes used in an improper manner. Their definition and the relation amongst those concepts is not completely clear in the text. The authors should be more precise when using these words. . .even an economist can get confused.

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