Revision letter

Full Title: "Climate change and drought effects on rural income distribution in the Mediterranean: a case study for Spain."

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Response to the comments provided by reviewer 3:

The comments are numbered 1 to 34. The response of the authors is following each comment within a box.

Reviewer 3 comments

1.The introduction should be better structured by being more concise, simplifying, and avoiding repetitions.

We have revised the introduction as suggested.

2.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).

We have revised all this concepts along the text to avoid inconsistencies.

3.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).

Figure 1 summarizes the steps on the methodology, as suggested we have changed this description to the methods section.

4.Better stating the main objective (too many objectives are stated).

We have included the main goal in the introduction: "Our main goal in the paper is to analyze the drought induced changes in the distribution of incomes that are based on production."

5.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").

We have tried to reorganize some parts of the manuscript as suggested.

6.Proofreading English in the abstract and introduction.

We did a revision with a professional service before sending the second round review as was suggested by other reviewer.

7.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.

We have rephrased the abstract as suggested.

8.The review offered by the authors could be more exhaustive including a more comprehensive overview of existing methodologies and studies and eliminating the paragraphN15-20 (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.

We included some general context on climate change-driven inequalities in related fields of social sciences (environmental justice, climate-driven migration) responding to a previous reviewer suggestion. However, we have dropped the following sentence: "Since the environmental justice concept proposes everyone (independently of their income, race, gender, etc) enjoying the same degree of protection from climate hazards, more knowledge related to empirical effects of climate change on income distribution is essential."

9.In paragraph N5 (p. 4355) the authors stress the relevance of considering market issues in dealing with adaptation and mention models performing sectorial 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.

We have added the following paragraph into the text: "In general, individuals and firms are modeled as representative agents within, respectively, one region and one market sector, what implies assuming the same socio-economic types of preferences across the world and across economies (Michetti and Zampieri 2014). As an alternative we consider market issues – what is crucial when dealing with adaptation –directly through incomes at the exploitation level what may reveal another part of the picture also interesting to understand the expected impacts on people."

10.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.

As suggested we have referred to efficiency in the use of water. We have change the sentence as follows: "Climate change will probably increase water conflicts among sectors, and the improvement of efficiency in water use will be essential to maintain environmental flows and therefore ecosystem sustainability"

11.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.

See answer to Reviewer 1, comment 1 for more details on this issue.

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

We have added the following paragraph into the text: "Extraordinarily bad governance practice related with water and irrigators had been reported in Spain, especially in relation to water rights in Spain. In the Tagus river basin, especially in the case of Western Mancha, the lack of clear definition of water rights currently produces important conflicts with estimated thousands of illegal abstractors. To achieve more effective water governance in the area, what seems to be a priority, it is necessary to create an enabling environment, which facilitates efficient private and public sector initiatives and stakeholder involvement in articulating needs. (De Stefano et al., 2013; Rogers et al., 2006)."

De Stefano, L., Hernández-Mora, N. López Gunn, E., Willaarts, B. Zorrilla-Miras, P. (2013), Public Participation and transparency in water management, in De Stefano, L., Llamas, R. (eds.) Water, Agriculture and the Environment in Spain: can we square the circle?, Taylor and Francis, London pp.217-226.

Rogers P.P., Llamas R., Martínez-Cortina L., (2006) Water Crisis: Myth or Reality?.Ed. Taylor & Francis plc., London, UK.

13.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.

We have included this idea into the text: "Although this paper focuses on the priorities for adaptation, we want to place these priorities in terms of mitigation efforts. Adaptation and mitigation are dependent one from the other. (IPCC, 2015). 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, some unavoidable effects remain independently of mitigation efforts)."

14.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.

We have included the following clarification into the text: "Land is defined as the real value in monetary terms (deflacted) for the planting area for every farm, so this is not constant along the considered period. Every year the farms declare the value of their properties (which can be sold or bought), and this value in real terms to avoid the inflaction effects is what we consider as land input. Although we do not have information about real land use, at least we capture somehow land evolution."

15.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 referencesand stressing pros and cons in their specific case.

We have added the following paragraph to the text: "Most of the studies in the literature using Olley-Pakes methodology assume a Cobb-Douglas production function (see Rizov et al, (2013) and Kazukauskas et al. (2010) as recent examples focused on EU farm data). Since this is the functional form more commonly accepted, we assumed it in our study. However, we tested with a more flexible form translog function, and no significant impacts were observed in the results of our analysis. The robustness of this method has been proved previously in Petrick and Kloss (2013)."

Kazukauskas, A., Newman, C., Thorne, F., 2010.Analysing the Effect of Decoupling on Agricultural Production: Evidence from Irish Dairy Farms using the Olley and Pakes Approach.German Journal of Agricultural Economics, 59, 144-157.

Rizov, M., Pokrivcak, J., Ciaian, P., 2013. CAP subsidies and productivity of the EU farms. Journal Agricultural Econmics 64, 537-557.

16. 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;VonThünen 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.

Petrick and Kloss (2013) present the following typology of production factors to explain how the Olley-Pakes model consider some of the mentioned factors:

	Highly variable	**Subject to adjustment costs**	**Fixed**
Observed by the econometrician & farmer	**Type I** Seed, fertiliser, chemicals, concentrate, livestock numbers	**Type II** Land, labour, machinery, buildings	**Type III** Geographical location
Typically unobserved by the econometrician but known to the farmer	**Type IV** Farmer's effort, reaction to environmental shocks	**Type V** Management abilities, human capital of the labour force, availability of a farm successor	**Type VI** Soil quality, climatic conditions
Unobserved by the econometrician & unanticipated by the farmer	**Type VII** Weather events, rainfall, diseases, legal requirements	–	–

Olley and Pakes (1996)attempts to proxyΩ_{it} (as a compound type IV-to-VI production factor) by a non-parametric control function, which itself contains only observed farm character (see appendix A for the methodology). Olley and Pakes were the first to suggest log investment (i_{it}) as an observed characteristic driven by $\Omega_{it.}$

$i_{it} = I(\Omega_{it}, K_{it}, A_{it})$. The inverse function for the unobserved shock Ω_{it} can be written as:

Ω_{it}= h(i_{it}, K_{it},A_{it})

Estimation proceeds in two stages. The basic idea is to jointly control for the influence of K and Ω in the first stage and to recover the true coefficient of K as well as Ω in the second. All observed factors except capital are assumed to be fully variable type I factors.

Since the focus of this journal is not the econometric model, as has been indicated by the Editor, and so most of the methodological aspects are in Annex A, we have not included this extended explanation into the text.

We find this discussion too specific to place the details into the manuscript. We have just mentioned the following in the Appendix A: "(see Petrick and Kloss, 2013, for an extended typology of farm production factors)."

17.SPI index: 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.

We have mention the limitation into the test: "We have used SPI to characterize drought since it is widely used and more comparable across regions with different climates than other more complex indexes as the Palmer Severity Drought Index (PDSI). SPI does not consider temperature, which is responsible to affect evapotranspiration, but we have consider temperature effects among the explanatory variables of the model. However, other limitations include that SPI does not consider the intensity of precipitation and its potential impacts on runoff, streamflow, and water availability within the system of interest (Keyantash et al., 2015)."

Keyantash, John & National Center for Atmospheric Research Staff (Eds).Last modified 20 Oct 2015. "The Climate Data Guide: Standardized Precipitation Index (SPI)." Retrieved from https://climatedataguide.ucar.edu/climate-data/standardized-precipitation-index-spi.

18.SPI index: 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)?

We have introduced the SPI index in a dummy form since we are interested also in the direct effect of temperature and precipitations and we wanted to avoid collinearity problems with the SPI index since it is constructed from precipitation data. This approach has been used before in some previous analysis in Spain (Iglesias et al., 2007; Quiroga and Iglesias, 2009; Iglesias et al., 2010; Garrote et al., 2007).

Iglesias A, Quiroga S., Schlickenrieder J. (2010). “Climate change and agricultural adaptation: assessing management uncertainty for four crop types in Spain”. Climatic Research, 44, p. 83-94.

19.SPI index: Have the authors controlled for collinearity problems between precipitation variables and SPI?

Multicollinearity is a matter of degree. There is no irrefutable test that it is or is not a problem. But, there are several warning signals and they are not present in our study. The matrix of correlations show not highly correlated predictor variables in regression models:

	Ptson	Ptdef	ptmam	ptjja	dspi
ptson	1.0000				
ptdef	0.2097	1.0000			
ptmam	0.4421	0.2899	1.0000		
ptjja	0.2329	0.0893	0.2488	1.0000	
dspi	-0.2879	-0.2203	-0.1516	-0.1230	1.0000

Also, we calculate the VIF(Variance inflation factors) which show the degree to which a regression coefficient will be affected because of the variable's redundancy with other independent variables.VIF greater than 10 roughly indicates significantmulticollinearity, in our variables:

Variable	VIF
Ptson	1.35
Ptdef	1.13
Ptmam	1.34
Ptjja	1.09
Dspi	1.13
Mean VIF	1.21

We have added the following paragraph to the text: “Also, we have tested collinearity problems between precipitation variables and SPI index and considering the matrix of correlations and the variance inflation factors, we conclude that there is no problem among factors to be concerned about.”

20.SPI index: 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.

Our model also considers the effects of Temperatures and Precipitation in addition to drought. See answer to Reviewer 1, comment 3 for more details.

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

We have introduced the following paragraph into the text: "The more difficult issue with an unbalanced panel is determining why the panel is unbalanced: (1) If the reason a farm leaves the sample is not correlated with the idiosyncratic error (those unobserved factors that change over time and affect profits), then the unbalanced panel causes no problems. (2) If the reason a farm leaves the sample is correlated with the idiosyncratic error then the resulting sample can cause biased estimators. One advantage of the mechanics of Olley and Pakes (1996) (as it is explained in the Appendix A section) is that takes into account the selection bias resulting from the exit of inefficient farms."

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

To avoid this problem, standard errors have been calculated in this paper through bootstrap techniques as explained in the methodology.

23.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.

See answer to Reviewer 1, comment 13 for details on the selected scenarios.

24.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.

See answer to Reviewer 1, comment 17.

25.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?

We have changed the sentence and also have homogenize different concepts on productivity and production as suggested also in comment 2.

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

This table shows the estimates for the nature state drivers and management factors elasticities and semi-elasticities for climate variables.

27.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 of1990 euro. In what sense do the authors refer to the "size" of land given the way they have constructed this variable?

Land factor is measured in thousand Euros and deflated to 1990 prices. For more details see the answer to comment 14.

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

In the long term, since productivity is being reduced, farms are less competitive through the international markets. This is expected to have important implications, especially since EU CAP policy is becoming more oriented to market mechanisms.

29.In the discussion, the authors speculate on market mechanisms to explain theirresults. However, their model is not able to analyze these market mechanisms anddynamics. While interpreting results, they should avoid mentioning about it or at leaststate more clearly that it's just a speculation not derived from the model results.

We have stated this in the text. See answer to Reviewer 2, comment 3.

30.What do the authors mean by "crop income functions"?

See the answer to comment 2

31.Is the smaller unit of observation the farmer or the farm (it seems that the two words are used indistinctly)?

The smaller unit of observation is the farm. We have homogenized the concept along the text.

32.What are the "estate variables"? Can the authors explain or use more clear expression?

The state variables are those that are not decision variables but affect the production output, such as climate variables.

33.Either choose to write Sect. or Section. (see paragraph N4, p.4357)

We have written Section through all the text.

34.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.

We have made an effort to reduce the different terminology to avoid confusion as suggested.