

"Integration of biophysical and agro-economic models to assess the economic effects of climate change on agriculture: A review of global and EU regional approaches."

Reply to Referee 2

First of all, we would like to thank the referee for his/her valuable comments about our review. We assign the following replies in order, according to the referee's report:

- 1) While the study is not very innovative, it is solid work. It manages to compare studies with very different design and assumptions and is therefore eligible to give readers an overview over the topic. A more distanced and critical view on the research field would have benefitted the study; nevertheless I recommend to publish the study after revisions.*

We thank to referee for his/her positive remarks.

- 2) The research question, "to review the evolution and use of this methodology" is very broad. A more concrete, narrow and innovative question would have provided more insights to the informed reader. Instead, the work has to remain very descriptive which makes it difficult to exceed the knowledge assembled by assessments like the IPCC report. It is now too late to change the general focus of the paper; however it would be good if the research question of the given paper could be concretized a bit more and the paper could be streamlined according to it.*

We thank the referee for his/her suggestion. We think that this review not only responds to the broad research objective posed but also answers to a specific and concrete question. Taking this into account, without changing the general focus of this review, we will change our current research question by one or more concrete objectives that provide more insights to the reader. In addition, we want to add that the next suggestions made by the referee, especially those related to section 2 and 5, will improve the capacity of this review to respond both concrete and innovative questions. We will develop this suggestion in a revised version of our review.

- 3) Section two of the paper has an important role. However, the real difference between structural and spatial-analogue approach does not become clear despite the long description. This section should be better structured, as it mixes several aspects. For example, the distinction between global and regional coverage; should it be by definition only within the structural approach? That land prices do not feed back to agricultural prices is not only valid for spatial-analogue approaches, but also for farm-models, right? Also, the link of a bio-physical model as input to the economic model is not the distinction between those two model-types, is it? What is then the key difference between structural and spatial-analogue approaches? What are examples of spatial-analogue approaches? Are there further approaches?*

We thank the referee for this suggestion. We will improve the structure of this section dividing it into two subsections. We will clearly specify the structural and spatial-analogue approaches highlighting their differences. These changes will have a twofold objective. First, clarifying the key differences between both approaches, answering the central question of this comment. Second, avoiding confusion generated by the mix of several aspects such as geographical coverage or weaknesses observed in both approaches.

Specifically, we will clarify the distinctive feature between both approaches. On the one hand, the 'spatial-analogue' approach estimates the effects of climate change based on observed differences in agricultural production and climate between regions. While the methods are 'structural' in the sense that the economic structural relations (suggested by theory) are specified, rather than being estimated (Adams et al., 1999). Based on this, crop and farmer responses are simulated.

Regarding the second objective, we will structure each subsection considering: 1) their distinguishing feature; 2) their geographical coverage; 3) their weaknesses and strengths; and 4) several examples.

- 4) *Section five sums up the results, but is also a bit repetitive with the last section. This is not necessary. I would suggest to remove where possible the double statements from either part 3, 4 or 5.*

Thanks for this suggestion. In a revised version, we will remove the double statements in section 5.

- 5) *The last two pages of section five provides some outlook on what are current shortcomings in the community that should be overcome by future research. This part should be extended, maybe even as an own section. So far, these recommendations are in my opinion one-dimensional: They mainly criticize the lack of detail; future studies should have more detail, more commodities (crops and livestock categories), more spatial resolution, more climate impacts (pests and diseases). However, a bigger model with more degrees of freedom is not necessarily a better one. Other points of modelling shortcomings are not mentioned at all:*

First of all, we would like to thank the referee for this valuable suggestion. This comment not only calls us to improve the structure of the last section, but also give us specific keys (shortcomings) which will improve the quality of this review. Certainly we will mention each one of them in a revised version. Also, briefly we would like to comment each point mentioned by the referee:

- a) Validation: we agree that the community largely fails to validate their results. Several studies have indicated that more work is needed for the validation of both, model components (Nelson et al., 2014; Willenbockel and Robinson 2014) and their outputs (Bronsch et al., 2013). It is certainly not an easy task. The model validation that aims to make long-term projections poses several problems, and a detail discussion of these problems likely would need a separate review on its own (e.g. Schwanitz, 2013). Several questions arise in the

development of this discussion: what must be validated? The outputs? The internal structure? Both?; What kind of validation exercises are used? What are their weaknesses? What validation exercises has been used by the studies reviewed in our manuscript?. We will try to encompass all these questions in a revised version of our review.

- b) Working on input parameters: We also agree with the referee about the need for more work regarding the elementary economic estimates. Nelson et al. (2014) explicitly point out that although all the models used in their study rely on plausible parameters, many of these "have limited econometric and validation studies to back them up with significant confidence". We will mention this in a revised version.
- c) Model Structure: We agree with the Referee that trade as a buffer of climate change impact can be considered tautological considering the structure of the economic models. Despite this, we think that the response of trade in the context of climate change is interesting and must be mentioned in this kind of review. Considering this suggestion we will mention the response of trade to climate change impact as a finding but noting that these responses must be interpreted with caution. We will also mention the evolution that economic models must have considering externalities, other market imperfections or new functions.
- d) Food Security: We agree with the referee about the importance of food security as the main issue in climate change impact assessments. However is important to take into account the complexity of it. Food security encompasses four dimensions: availability, access, utilization, and stability. These components are interlinked by dynamic interactions between and within biogeophysical and human environments (Gregory et al., 2005). On the other hand, climate change could have a range of direct and indirect effects on all four dimensions of food security. Current knowledge of food security impacts of climate change dramatically lacks in coverage across all dimensions of food security. Food security, its complexity and its relation to climate change impacts will be mention in this section in a revised version.
- e) Policies: In our revised version, we will comment the lack of assessments considering policies that differ from trade liberalization or adaptation policies. We will comment that an interesting future research could be the effects of policies that the referee mentioned, and we will search for other policies insufficiently investigated up to date.
- 6) *Unfortunately, the appendix and the supplementary material was not available to the reviewer. It is important that the reviewer in the next phase of review receives this material.*

Regarding this comment we would like to mention that the manuscript was submitted together with the appendix. We will attach it together with this report in our reply.

The list below represents what we interpret as a minor changes suggested by the referee, from orthographic to content issues. We reply each one of these points in which the sentences in italic are the referee's suggestion and in bold are our replies:

We would like to mention that once we finish the revised version, we will send it to a researcher support network, which account with English speakers that are familiarized with the subject.

In general: Name (economic) models if possible

We will do this in our revised version

Title: Could be shorter.

Modelling economic impacts of climate change on Global and European agriculture. Review of economic structural approaches.

Abstract: "Furthermore, due to the effects of crop prices over yield" do not understand

We will replace the above sentence with "in addition, due to changes in crop yields are a function of both exogenous and endogenous elements (e.g. prices)..."

p.2. " European papers reviewed their modelling" comma missing
- "European papers reviewed, their modelling..."

p.2. last paragraph: reference to non-existing tables 1+2, please include them!
We will attach these as an appendix, together with this report.

p.3. second paragraph is written very fuzzy and incomprehensible.
The section 2 will be restructured in a revised version. In this, a better and more comprehensible description of both approaches will be added.

p.4. "In terms of the economic dimension, the main distinction" here it should be made clear that this refers to the distinction within the structural approaches.
With the new structure, this will be clarified

p.4. last paragraph: It should be made clear that the taxonomy is only for the analysed studies. One could well imagine a BLS model with regional coverage.
With the new structure, this will be clarified

p.4 It is not clear, why only the limitations of the BLS trade models are highlighted. All model types have certainly their strengths and weaknesses. It seems that BLS model is a hybrid between a GE and a PE, and certainly has advantages (high detail in agricultural sector, while remaining consistent with the budget constraint). I would either write strengths and limitations of all approaches (e.g. in a table) or leave the critique of only the BLS away.
In a revised version, we will add a table containing a brief description of both strengths and weaknesses of GE, PE and BLS model

p.5. it should be stated clearly in the introduction that the literature review concentrates on the structural approaches.
We will add in the introduction of a revised version, these specifications.

p.6 "two main components" – two major aspects?
We use the term "component" referring to the estimations of 1) potential changes in crop yields and 2) world food trade responses

p.6. "decision support system" – what is this?

We refer here to the Decision Support System for Agrotechnology Transfer (DSSAT). It is a software application program that comprises crop simulation models. The crop simulation models in DSSAT simulate growth, development and yield as a function of the soil-plant-atmosphere dynamics.

p.7. "these three seminal works were one.."

We will replace the above sentence with “These, were some of the few studies which...”

p.7 "contrary to other studies, this predicts real price increases..." what did the other studies predict? No price increase? Price decrease?

Darwin et al. (1995) and Adams et al. (1998) predicted that, agricultural production and prices are likely to continue to follow the downward path observed in the 20th century. Darwin et al. (1995) predicted that agricultural prices of wheat will decrease -10 to -3% while other grains will decrease between -6 to -4%. Adams et al. (1998) predicted agricultural price changes of -19 to +15%. Parry et al. (1999) was the only study that projected that even a global mean temperature increase of ~1 °C causes prices to rise. Additionally, projected that output prices will raise without climate change. That is why the complete sentence is “... this predicts real price increases with modest amounts of climate change”.

p.7 "the greater negative impacts... derives on" s, "derives on a price increase about 45% by the 2080" sounds strange, comma mistakes, and "price increase of about", and "by the 2080". Please also name it "crop price increase" to not confuse it with "food price increase" (consumer food prices are quite different from crop prices due to value added).

We will fix this in our revised version.

p.8 "However,..." Also several orthographic mistakes

We will fix this in a revised version

p.8 what is a agro-ecological zone model? Agro-ecological zones are the resolution of models, not a type of, right?

Fisher et al. (2002; 2005) employ the FAO/IIASA agro-climatic database and modelling framework known as the agro-ecological zone or AEZ model. This modelling framework uses detail agronomic-based knowledge to simulate land resources availability and use, farm level management options and crop production potentials.

p.9 Table 3: How comes in table 3 that when the production change is lower, the price increase is higher? (GADCM3-B1_B2 with/without CO2)

We will fix this in a revised version; the value of price changes in HADCEM3 – B1_B2 without CO2 is 98.3, not 9.83

p.9. "than scenarios where co2 fertilization" "than in scenarios"

We will fix this in a revised version

p.9. "for a detail description" detailed?

We will replace the above sentence with “for a detailed description” in our revised version

p.9 "Fischer et al (2005)," without comma

It will be deleted

p.9 CSIRO or CSIROC?

We will replace with CSIRO

p.10. "Caused a searching"

We will replace the above sentence with "...the limitations of the BLS model were important factors in the search for..."

p.10. Second paragraph starts with numbering items and continues with "moreover" statements

We will fix this in a revised version

p.11. The sentence "They found that there is potential for much greater changes" is a bit misleading. It should become more clear that their central case also has only modest price changes, and that these high price effects only occur if only the tails (95%) of the distribution are used; a very unlikely outcome, as I assume that a dataset containing only the tails of regional climate distributions would hardly meet the global average-temperature and therefore not be consistent.

We will clarify this in our revised version.

P11,12. What are perfect mitigation scenarios? Why suddenly mitigation? Do you mean perfect adaptation?

The term "perfect mitigation" is used only for the work of Nelson et al., (2010). They called "perfect mitigation scenario" the scenario that climate of the early 2000s was to continue through 2050. This scenario is used to be compared with other four plausible climate futures.

P14 due mainly to --> mainly due to

We will replace the above sentence with "mainly due to"

p14 detail impact --> detailed impact

We will replace the above sentence with "detailed impact"

p14 integrated all into --> all integrated into

We will replace the above sentence with "all integrated into"

p 14 "whilst at the same time model"

We will fix this in a revised version

p15 last sentence, delete "additionally"

We will delete this in a revised version.

p16 "global grid crop models" global gridded crop models?

We will fix this in a revised version. (global gridded crop models)

P16 The AgMIP and ISIMIP projects should be mentioned by name

In a revised version, we will mention this projects by name. (e.g. Agricultural Model Intercomparison Project (AgMIP)).

P16 I think IMAGE is no GGCM but only uses one, e.g. LPJ

The model used by Rosenzweig et al. (2013) is The Global Agro-Ecological Zone Model in the Integrated Model to Assess the Global Environment (GAEZ-IMAGE).

We will clarify this in a revised version.

P16/17 the economic models of AGMIP are not named, but should be. It is not even stated that several models were used (only in a side-sentence "that all economic models...")

We will mention all the economic models used within the framework of AgMIP. Although, we would like to clarify that these are mentioned in the appendix.

p17 "important finding ... is the fact that " better write "is that"

We will replace the above sentence with “important finding... is that”

p19 "Baseline" baseline

We will fix this in a revised version

p 20. delete "with this in mind"

We will delete this in a revised version.

p. 20: first bullet point: This point refers only to production, right? Due to trade, prices changes should also be on a regional level rather modest, while production in specific regions may change strongly

This comment is true. In fact, production changes are stronger in specific regions while prices changes are modest on a regional level. However, it is important to consider that other endogenous responses such as land use changes or income could present moderate aggregate impacts, but greater effects at a more disaggregated level.

p. 20: "most reliable" --> "more reliable"

We will replace the above sentence with “more reliable”

p 21 third bullet point: the authors concentrate on the biophysical side when stating that developing countries are harded hit by climate impacts. There are however also economic reasons for this: Food expenditure shares are higher in developing countries, and consumption is more price-elastic (see e.g. Valin et al. (2014)).

Valin, Hugo, Ronald D. Sands, Dominique van der Mensbrugge, Gerald C. Nelson, Helal Ahammad, Elodie Blanc, Benjamin Bodirsky, et al.. 2014. “The Future of Food Demand: Understanding Differences in Global Economic Models.” *Agricultural Economics* 45 (1): 51–67. doi:10.1111/agec.12089.

We will add this in a revised version. Mentioning the work made by Valin et al. (2014) and indicating the economic reasons why developing countries are harded hited by climate impacts

p.21 "regional disparities are also observed" why also?

We will delete “also”

p.21 "most of the studies reviewed," without comma

We will change this in a revised version

p. 21 "major data availability" improved? Higher? Better?

We will replace the above sentence with better data availability

p.22 what do you mean by "these are richer"

We refer to the broader scope to serve different users compared with the SRES scenarios. In this case, the new scenarios recognize that more than one socioeconomic pathway can lead to the same concentration of GHGs (Kriegler et al., 2012). In addition, 3 of the RCPs include explicit stabilization strategies, which are not considered in the SRES scenarios.

p.22 "Recently, two new scientific papers" Which ones?

Blanco et al. (2014b) and Frank et al.. (2014). We will explicitly mention these studies in a revised version.

p.22 "initials"

We will change this in a revised version

p22 "are still remain."

We will replace the above sentence with "still remain..."

p.22+23 I do not understand why you structure these proposals for improvement into global and EU items. They overlap in most cases, and why should a feature for a global item not be valid for a regional one?

In a revised version, the proposals in common will be treated within one paragraph.

We will avoid this overlapping

P23 I strongly disagree that parameters like price or income elasticities should be harmonized between models. These parameters are very uncertain, and instead of harmonizing, modelling teams should spend time on better econometric estimations of these parameters. Any further harmonization does not reduce the uncertainty, as the authors write, but simply hide the uncertainty.

We will mention this in a revised version. The reply to this comment is strongly related to the reply 5.b of this report

P23 please give examples for adaptation options

Sowing dates was one example mentioned. In a revised version, we will mention other adaptation measures used in this kind of studies (e.g. application of additional fertilization or irrigation)

p23 "there is lack"

We will change this in a revised version

p23 "a need for add" "regarding to narrowing" "EU account on"

We will change this in a revised version

p23 those approaches do not narrow down uncertainties, they just discuss them.

We will change this in a revised version

P24 "other crops."

We will change this in a revised version

Summary:

We would like to thank to referee 2 for the time and feedback given to evaluating this paper. We believe that the modifications, based on the referee's comments, will improve our review considerably. Finally, we would like to highlight that once we finish the revised version, considering each one of the suggestion made by referees 1 and 2, we will send it to a researcher support network, which account with English speakers that are familiarized with the subject. After this proofreading process, we will upload the new revised version of our review.