"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 1

First of all, we would like to thank to the referee for his/her valuable comments about our review.

Before reply each of the weakness found by the referee, we would like to refer to a specific issue that encompasses most of them. Section 2 of this review, play an essential role, framing the focus of our review and explaining some important concepts regarding the economic assessment of climate change impacts on agriculture. The aim of this section was to frame the specific area of our review and support the reason why we make a review of such a specific approach (i.e the integration of biophysical and agro-economic models to assess the economic effects of climate change on agriculture). This, despite the low number of studies that rely on this approach, compared with others that assess the impact of climate change on agriculture. However, after the referees' comments, we understand that this section must be better structured and needs more work to determine the focus of our review, in order to facilitate the comprehension of future readers. Given this, we will lengthen section 2 in order to further clarify the structure and the objectives of this review. Moreover, we will balance the description between early integrated biophysical and economic models and current developments in section 3 and 4. Finally we will divide the current section 5 into two subsections, where we will extend the review about the shortcomings of this approach.

Considering the above mentioned, from now on, we will focus on the weaknesses mentioned by the referee. We reply each one of these, with a view to improving the next draft of the paper. The following replies are assigned in order, according to the referee's report:

1) *Yet, while there are some merits of the review as mentioned above, it its current form it is incomplete and not well balanced (superficial in places; lengthy in others): most importantly, it is not fully up-to-date; comprising 32 pages, the review is quite long and would benefit from substantially cutting parts, e.g. those describing early developments – while some additions on developments during recent 5 years would be needed; a revised structure making it easier for readers to navigate through different methods, stages of development and scales of analysis would also be beneficial (for example, when in section 5, a summary is made, the six categories of methods presented (also illustrated in Fig 1, page 5) do not match with the structure/sections followed in the review text)*

Regarding this comment, we propose a reviewed version, providing a shorter description of early developments, and more detailed descriptions of developments during recent 5 years, with the aim to provide a well-balanced review. Regarding the structure of the article, we understand that it is necessary improve section 2, especially where six categories of methods were presented, focussing our review only on five. In this particular case, we decide to exclude those studies at farm level, mainly because their methodologies (e.g. exogenous prices) and spatial features (e.g. farm vs regional or

global level) are quite different than the other 5 categories of methods reviewed. These differences make it very difficult to compare its outputs with the results of the other 5 categories. In a new revised version, we will add at the end of section 2 an extended explanation about the differences between the last category and the others. Giving examples about their different outputs and explaining the difficulties to make a comparison between them.

2) *The qualification "not fully up-to-date" especially applies to EU regional assessments (see also other bullet, below); this becomes obvious if one goes through other reviews that look at developments in CC impacts assessments for agriculture during last 20 years – be it for agricultural impacts for certain EU countries or certain crops at EU level (e.g. Wolf et al., 2012; Höhn and Rötter. 2014), or, how crop modelling has served integrated impact assessment of agriculture over time from farm to global levels (Ewert et al., 2014)*

Regarding this comment, we checked the reviews mentioned by the referee (except for Höhn and Rötter., 2014 that was not available in internet). Wolf et al. (2012), within the framework of SEAMLESS indeed was an example of a study which used integrated biophysical (ACE-FAST) and agro-economic models (CAPRI) to assess the economic impact of climate change on EU agriculture. However, we understand that this kind of modelling framework, links different models mainly looking for consistency of their results at different scales (see also the examples of EuRuralis or Scenar2020). In this particular case, this makes that the economic effects of climate change at regional or global level are not the focus of the study. Wolf et al., (2012) indicate that the main level of analysis is the farm level, taking the results on product prices at EU level as a context for further research. On the other hand, we agree with the referee that there are several studies that assess the CC impacts for agriculture during last 20 years that are not considered in this review, however, these studies are not within the focus that we try to define. This review is framed by 3 main features (section 2 page 5), and other studies outside this framework are beyond the scope of this review. The main focus of this review could be defined as those studies that Porter et al. (2014) (page 27, chapter 7; AR5 of WGII) identify as : "studies that use projected yield impacts as inputs to general or partial equilibrium models of commodity trade". Within this framework, we focus only on structural approaches, at global and UE level, between 1990 and the present. Furthermore, we look for studies that: 1) provide economic outputs (e.g. the impacts on food prices); and 2) use an integrated approach, where the economic component is suited to capture market feedback.

Considering the above mentioned, and as we state at the beginning, we propose a reviewed version, with an improved section 2. In this case, specifying the focus of our review; mentioning several relevant applications of integrated biophysical and economic models that were not included in this review; and explaining why these were not considered.

3) *Authors claim that in assessing economic effects of climate change on agriculture, many studies (during last two decades) have relied on the integration of biophysical and agro-economic models; yet, authors fail to explain how they define "model integration". If one goes through the agricultural impact literature since IPCC WGII, SAR (Second Assessment Report - published in 1996), one finds that in most cases results from biophysical modelling have been integrated with different kinds of economic analysis (not always economic modelling); only in a small fraction of studies have crop models and economic models actually been (physically) integrated. And this situation only is gradually changing in recent years: when looking at agricultural impact studies quoted in the recent IPCC assessment report (AR5) of working group II (see, Porter et al., 2014) most still entirely rely on biophysical modelling – for underlying reasons (i.e. CC effects that can easily be investigated /lack of appropriate methodologies (Wheeler & von Braun, 2013)), see also the recent review by Ewert et al. (2014) on the contribution of crop modelling to integrated assessment and modelling (IAM) of risk to food production from climate change. That review especially examines to what extent crop models comply with IAM demands. This situation also holds true for the farm level (see, van Wijk et al 2014).*

Regarding this comment we are agree with the referee that there is a lack of definition for what we understand by "model integration". A common taxonomy of modelling integration is that mentioned by Reilly and Willenbockel (2010) and Böhringer and Löschel (2006), which make a difference between hard-linking models and soft-linking models. While hard-linking implies integration of several models into a single modelling tool, soft-linking refers to the use of outcomes of one model as inputs in the other. Hard-linking implies additional computing effort and limits the flexibility of the linked models to adapt to new situations. Based on this, our review was based on soft-linking models. However, it was not specified in the article and we understand that this might cause some confusion. We proposed to add this explanation to the section 2, in a reviewed version of it. Furthermore, we are also agree with the referee that the most agricultural impact studies, rely most on biophysical modelling than in integrated biophysical and economic models. A clear example of this is the review made by White et al., (2011), where 221 peer-reviewed papers that used crop simulation models to assess the effects of climate change on agriculture were reviewed. This number is not possible for the studies that we focus on this review. Considering this, we understand that the lack of definition of the concept "model integration" plus the use of the word "many" in this case, might be interpreted as if this approach has been used much more than others (e.g. biophysical models). This will be indicated clearly in a reviewed version.

4) *Furthermore, I have my doubts whether in the context of climate change impact modelling, it is useful to speak of a "bio-economic approach" that has gone through different stages (see, also comments by van Wijk et al 2012 on this issue). To my knowledge, "bio-economic modelling" has been a rather ad hoc formulation invented by development economists and agro-ecologists from Wageningen when applying the multiple goal linear programming (MGLP) approach at farm and (sub-)regional levels to explore options for sustainable land use, i.e. achievement of different agricultural development goals and their*

trade-offs (see, e.g. de Wit et al. 1988. Agricultural Systems 26, 211-230; Kruseman & Bade, 1998. Agricultural Systems 58, 465-481); the mean-time the label "bio-economic" has got quite different meanings

The term "bio-economic approach" was used with the intention to identify those studies that rely only on biophysical models from those that integrate both biophysical and socio-economic models. Wijk et al. (2012) define this term as "models that integrate biophysical and economic components". Within this definition, the concept used as "bio-economic approach" has the intention to frame our review. However, we are conscious of the referee's doubts regarding the usefulness to speak of "bio-economic approach". First, within the literature, the concept of "bio-economic modelling" approach is mainly associated with integrated models focused on lower scales than global or regional assessments (Ruben and Van Ruijven 2001; Janssen and van Ittersum, 2010; Louhichi et al. 2010; Wolf et al., 2012). Moreover, we are agree with Wijk et al. (2012) when he states that the term "bio-economic model" can be used for such a diverse set of models that is it no longer distinctive. Considering this, we propose within a reviewed version clarify to the lector that the term "bio-economic model" here used must be taken with caution, noting that in this review it was used for the sole purpose to identify and differentiating a specific approach.

5) *Another claim of the authors is that the review analyses the evolution of the "bio-economic approach", both at global and EU level; I disagree – unfortunately, the review is quite selective and biased; for example, for the EU level just a few studies are discussed, while a multitude of studies from large European research consortia or networks (e.g. SEAMLESS, SENSOR) (see, van Ittersum et al 2008) have not been mentioned that have developed fully integrated modelling approaches – subsequently applied to CC impact assessment (e.g. Wolf et al 2012).*

It is true that several modelling frameworks were not included in our review (see also, EuRuralis (Rienks, 2008); Scenar2020 (Nowicki et al., 2006)). These modelling frameworks link different models in order to answer complex policy questions and to deliver results that are consistent at global, national and regional levels. The work of Wolf et al. (2012) is a good example of how SEAMLESS (Van Itersum et al., 2008) was subsequently applied to assess climate change impacts on agriculture. However, the same study is a good example of why we did not include it in our review. As we mentioned before, one of the main objectives of these fully integrated modelling approaches are the consistency of their results, in all levels. As such, the economic impacts of climate change (at EU level in this case) are a means not an end. For instance, Wolf et al. (2012) explicitly indicated that the high-scale EU level analysis is required as a context, within which a regional level analysis can next be done (Wolf et al., 2012; page 12). On the other hand, in the same study, several economic endogenous responses were not shown in the result section (at EU level). To avoid the impression of a biased review, within section 2 (as we mentioned in our second reply of this report), we will mention those works which have relied on these modelling frameworks that also have been applied to assess the economic effects of climate change on agriculture (e.g. Wolf et al. 2012).

6) *Even though the review is restricted to the global and EU regional level, it would have been worthwhile to mention an important recent publication that applies so-called "bio- economic" modelling approaches with focus on climate change adaptation and mitigation at farm household level (van Wijk et al. 2012; 2014).*

Thank for this suggestion. In section 2 of a revised version we will mention this publication.

7) *Surprisingly, recent progress in terms of IAM in research networks like AgMIP, MACSUR and CCAFS are not or not explicitly mentioned*

Several references cited in our review are part of the AgMIP project (e.g. Nelson et al., 2013, 2014; von Lampe et al., 2014; Witzke et al., 2014). Indeed, the AgMIP project is explicitly mentioned in page 19 of this review. However, the other projects were not mentioned neither MACSUR nor CCAFS. In accordance with the referee's comments, we will mention these projects in a reviewed version, highlighting their contribution to the assessments of the economic effects of climate change on agriculture at global and EU level.

8) *Then, throughout the text there are claims that are not true – e.g., to name a few, that the two EU studies presented (Ciscar 2009; Shresta et al. 2013) (pp. 12-14) are the first integrated assessment studies on EU agriculture, or, that over last two years most impact assessments base their results on new scenarios and are focused on quantification of uncertainty – actually, only few studies already used the "new scenarios (RCPs, SSPs, etc) and while uncertainty finally has received (more) attention, it is not the main focus of recent studies.*

Regarding this comment we will divide our reply on two phrases that we think are the problem. First, (page 14) was written: "These studies are the first regionally-focused, quantitative, integrated assessment of the effects of climate change on vulnerable aspects of the European economy and its overall welfare". These lines are after the description of the studies of Ciscar, (2009) and Ciscar et al. (2011). Nowhere in this section, have we argued that Shrestha et al. (2013) is the first integrated assessment study on EU agriculture. On the other hand, regarding the works of Ciscar, this claim is supported by the same authors (e.g. Ciscar, 2009; page 7). They indicate that, for the first time in Europe, an integrated approach that combines high resolution climate models (GCMs-RCMs), sectoral impact models and comprehensive economic models, was able to estimate the impacts of climate change.

The second claim reads: "Over the last two years, most of the impact assessments that based their results on the new scenarios have been focused towards the quantification of the uncertainty that underlie their approaches". Considering this, the above claim was based on those studies that used the "new scenarios" (as was established in the introduction and the section 2). Nowhere in this review, it has been argued, that *"over the last two years.....most impact assessments base their results on new scenarios"* as the referee indicates. Second, as we mentioned above, we are exclusively referring to those studies that used "the new scenarios". Within this context, it must be noticed the close relationship of the studies cited in this review and the AgMIP project, which

among their cross cutting themes, try to address uncertainty implied in analysis of individual models.

After this comment, we propose a revised version of section 4, with a better wording and improving the structure of this. Moreover, this section will be supported for a better structured section 2.

9) *Some of conclusions /common findings (section 5) are trivial or not supported by this review.*

We will make a review of this section. According to the referees' comments, we will revise our current conclusions, and the structure of section 5 will be improved. First, we will divide it in two new sections, where the future research directions (current shortcomings) will be treated alone. In this last sub-section we will include several topics mentioned by Referee 2, taking into account other points of modelling shortcomings, in addition to those already treated.

Summary

We would like to thank to referee 1 for the time and feedback given to evaluating this paper. The weaknesses detected with respect to structure, key concepts, and the lack of mention of important projects or modelling frameworks are extremely constructive. We really believe that the modifications, based on the referee's comments, will improve our review considerably.