

The article "Intergation of Biophysical and Agro-economic Models to Assess the Economic Effects of Climate Change on Agriculture: A review of Global and EU Regional Approaches" summarizes the state of research in the modelling of economic impacts of climate change on European and global agriculture. It provides a broad literature review on the history of past model-assessments and summarizes their results.

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.

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.

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?

Section three and four of the paper is the main part of the article. Comparing the main findings between different studies is a very difficult task, as these studies differ largely in regard to study design and assumptions in regard to climate scenario, co2 fertilization, climate model, crop model, baseyear calibration, assumptions on adaptation, price-elasticities, economic model and more. As the authors can not harmonize these assumptions ex-ante, as model comparison studies often do, these numbers are not easily comparable. Within these limitations, the authors do a good job in categorizing and embedding the results of the different studies in their summary.

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.

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:

a) Validation: The community largely fails to validate their results. This is a serious issue. Validation within the community is in my opinion mostly done by comparing different model outputs, resulting

often in harmonizations when the spread is too large, which only provides the appearance of higher certainty. There has been extremely little validation with past data (backcasting exercises, calibrating the model to one region and simulating a different region with the same parameters, analyzing whether the model manages to simulate past crisis events). This type of validation is probably also not done, because the results were little encouraging. Nevertheless, a review article should mention this shortcoming. It would be interesting to describe and count the number of the studies reviewed in your work which provide such types of validation exercises with historical and independent data.

One could also mention the risk, that the large number of model-intercomparisons and the connected harmonization processes did not actually highlight the uncertainty connected to climate impacts, but provided incentives for outlying models to change their results, and hides the actual uncertainty. Here it would also be interesting to compare results: Did the spread of results diminish, or increase over time? Did the average move?

b) Working on input parameters: Data issues are still a big constraint for the modelling community. There are few consistent time-series for parameters like prices; therefore, also parameters like the elasticities which are estimated on this basis lack good quality. Also, the diversity of available data is low, such that despite the uncertainty, models use the same set of parameters. More effort should be put into the elementary economic estimations, and data should be shared within the community.

c) The model structure, whether PE or CGE or BLS all follow the same basic neoclassical theory. Certain findings are therefore tautologic. The common finding that trade decreases impacts is such a finding. Within equilibrium models, where welfare is aggregated out of consumer and producer rent, no other result is possible. When welfare is optimized, and trade barriers are a constraint, the removal of this constraint improves welfare. However, this is not necessarily true in reality: There are examples where sudden trade liberalization lead to a shock of the economy leading to the complete loss of unprepared domestic industries. Also, if we assume a social welfare function which is not simply adding up consumer and producer rents unweightedly, it is well possible that trade liberalization goes at the cost of certain poor producers in uncompetitive areas, who lose their livelihoods at the favor of well-off consumers. Finally, trade restrictions often have a function, e.g. internalizing environmental externalities. Abolishing them without considering the effect on externalities gives a wrong picture of welfare. New models should evolve, including market imperfections or testing out different welfare functions.

d) Similarly, it is questionable whether the outputs created by the models are indeed the most interesting ones. If we look at the impacts of climate change on agriculture, food security is probably the most important issues. Food prices are thereby just one indicator, and at the producer level they tell little about the burden for consumers. Looking at food security would require socially heterogeneous consumers, consumer prices, food expenditure shares, nutrition value of food baskets, food access, food utilization etc.

e) The selection of adaptation policies is not very innovative. A lot of work has been done on the effect of trade liberalization on climate impacts. This may stem less from the outstanding importance of this process, but from the historical focus of the community to calculate trade policies. It would be interesting to analyze different policies, e.g. increased public spending on research & technology (driving the currently model-exogenous yield shifters in the model). Storage policies have also not been analyzed, even though they largely influence food prices. There are multiple other policies which could be implemented, that are insufficiently investigated.

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.

Finally, I listed below some things that I noticed (orthographic and content issues).

I recommend to send the paper to a native english speaker for proof-reading; there is probably more to

find.

In general: Name (economic) models if possible

Title: Could be shorter.

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

p.2. "researches" no plural

p.2. " Europen papers reviewed their modelling" comma missing

p.2. last paragraph: reference to non-exisiting tables 1+2, please include them!

p.3. second paragraph is written very fuzzy and incomprehenisble.

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.

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.

p.4. "is important to highlight". "It" missing

p.4 It is not clear, why only the limitations of the BLS trade models are highlighted. All model types have certainly their strenghts 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 strenghts and limitations of all approaches (e.g. in a table) or leave the critique of only the BLS away.

p.5. it should be stated clearly in the introduction that the literature review concentrates on the structural approaches.

p.6 "two main components" – two major aspects?

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

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

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

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).

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

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

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)

p.9. "than scenarios where co2 fertilization" "than in scenarios"  
p.9. "for a detail description" detailed?  
p.9 "Fischer et al (2005)," without comma  
p.9 CSIRO or CSIROC?  
p.10. "Caused a searching"  
p.10. Second paragraph starts with numbering items and continues with "moreover" statements  
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.  
P11,12. What are perfect mitigation scenarios? Why suddenly mitigation? Do you mean perfect adaptation?  
P14 due mainly to --> mainly due to  
p14 detail impact --> detailed impact  
p14 integrated all into --> all integrated into  
p 14 "whilst at the same time model"  
p15 last sentence, delete "additionally"  
p16 "global grid crop models" global gridded crop models?  
P16 The AgMIP and ISIMIP projects should be mentioned by name  
P16 I think IMAGE is no GGCM but only uses one, e.g. LPJ  
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...")  
p17 "important finding ... is the fact that " better write "is that"  
p19 "Baseline" baseline  
p 20. delete "with this in mind"  
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  
p. 20: "most reliable" --> "more reliable"  
p 21 third bullet point: the authors concentrate on the biophysical side when stating that developing countries are hardly 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.  
p.21 "regional disparities are also observed" why also?  
p.21 "most of the studies reviewed," without comma  
p. 21 "major data availability" improved? Higher? Better?  
p.22 what do you mean by "these are richer"  
p.22 "Recently, two new scientific papers" Which ones?  
p.22 "initials"  
p22 "are 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?  
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.

P23 please give examples for adaptation options

p23 "there is lack"

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

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

P24 "other crops"