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Earth System Dynamics Discussions

Interactive comment on "Impacts of climate mitigation strategies in the energy sector on global land use and carbon balance" *by* K. Engström et al.

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—Summary—

The authors have pulled together a lot of material to address the research questions, which are quite complex and required the development of several new modeling tools that haven't had any prior publications. I would want to see changes to the modeling processes prior to recommending publication, and some more references to the literature on applying climate damages in an IA modeling context.

—Several points—

* My first comment pertains to the energy/economy model used here: I don't see any

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value to publishing a new set of SSP-ish scenarios from this very simple model that appears to be parameterized inconsistently with the corresponding publicly available SSP scenarios. The sophistication of the energy/climate model in this study is similar to, or less than, the IA models in the 1980s. This wouldn't be a problem as long as the simple model were parameterized so as to replicate the results of the larger energy-economy models used to produce the SSPs (in similar fashion to the simple climate models that replicate the results of the GCMs). Most of the parameters that this model takes as exogenous inputs are the product of complicated and generally non-linear dynamics, and instead of just being guessed (e.g., technology efficiency improves at 2%/yr from 2010 to 2100), they should be calculated from those more detailed models' outputs. Much more effort should be focused on validating that the outputs from the energy/economy and land model here are in fact consistent with the published scenarios. That comparison should be done for all key variables assessed here in order to ensure consistency; the discussion includes mostly anecdotal observations that two of the ten scenarios here have similar cropland quantities and total primary energy demands as two of the scenarios in the SSP database.

* I also have a problem with the basic design of the study, but this is really a decision for a journal editor and not a reviewer, and there's not really anything that could be done to change it. The study uses a detailed crop and vegetation model to represent climate impacts at the 0.5 x 0.5 degree scale, but then uses an extremely simple multiplier on a nation's GDP (or the world's GWP) to calculate the climate damages. I am aware that others in this field do that, and so perhaps there is no issue here. But in my opinion, climate impact-related damages simply do not lend themselves well to that sort of simplistic representation.

Climate impacts, by their nature, are non-linear with respect to global temperature, variable over time, region-specific, and context-dependent. In the form of droughts and extreme events, they are also relevant at sub-annual time scales, below the resolution of the timesteps being represented in the global energy/economy/land models. To estimate the costs of climate impacts in any region and time period, one would first need to know what the physical climate impacts are; second what the direct damages are; and third what the adaptive capacity of the system is, along with the costs of adaptation. At this point, the scientific community has produced scenarios of climate at the appropriate temporal and spatial scales, and is currently working on how to model the impacts of the climate on the relevant activities in the economic, energy, and agricultural sectors. This study doesn't address the complexities of climate impacts in estimating GWP losses; it uses a simple "marginal damage" function that relates economic productivity loss to the CO2 concentration. I know they cited another study that used/developed that function, but in my opinion there is no reason to believe that this relationship has any validity, applied to a future economy that is likely very different from today's, and with climate impacts that include much more than temperature change. Given the current state of the art in the impacts, adaptation, and vulnerability (IAV) community, I doubt this relationship was demonstrated to hold for a variety of nations with different climate impacts and different economic structures.

* The authors should specify what the downscaled gross world product (GWP), to the country level, is used for. The method is documented in the text and appendix, but I never saw what subsequent calculations it was used for; it may be used to modify a country's GDP and therefore energy demand, but I'm not sure. I don't particularly like the method, as it doesn't consider the inter-national differences in climate impacts; for instance, temperature increase could be good for economic productivity in some countries (e.g., Sweden) while bad in others (e.g., India). Also it doesn't consider that climate impacts will affect different sectors of the economy in different fashion (e.g., agriculture vs manufacturing vs services vs household), so that the climate impacts on GDP will be different for countries with different economic structures (all else equal).

* More documentation of how the climate impacts were applied to the agricultural sectors should be provided. In this sort of vegetation and agro-economic model link-up, many countries typically see unrealistic and positive yield impacts, particularly places

with a harsh climate and low yields in the historical years, where small increases in precipitation can lead to large modeled yield increases. In my work with similar data I've had the most trouble with the Middle East, North Africa, Russia, and Canada. But to some extent this depends on the mathematical formulations for applying aggregated crop model output to the baseline nation-level yield trajectories.

* Next, I'll address a few of the simplifications and representations that struck me as particularly problematic in the modeling exercise; unfortunately, without the raw data inputs and outputs to/from the model, I can really only guess as to the relative importance of each.

1) shareBEcr: this parameter, exogenous in all periods and scenarios, represents the combustible energy content of all ethanol and biodiesel feedstocks divided by total global bioenergy demands. The denominator includes all remaining uses of bioenergy, which the authors note account for some 97% of the base-year bioenergy demands. The basic problem is that these bioenergy commodities (in the denominator) have fundamentally different future demand drivers from ethanol and biodiesel (the numerator), so there isn't really any way to know a priori how this will evolve over time, in the various narratives of the SSPs.

In the current study design, the authors are attempting to set the "shareBEcr" such that the quantity of agricultural crops used as bioenergy feedstocks does not grow by more than 30-50% from its base year value, according to the estimates of a study (Haberl et al. 2010). However, in the model, this is applied as a share constraint rather than a quantity constraint, so the target quantity (from Haberl et al. 2010) appears to be greatly exceeded in some if not all of the scenarios. On the other side, the bioenergy commodities that do grow a lot (up to 450 EJ/yr) are the unspecified ones, which in the study methods are not tied to cropland or the land/carbon models, even though it is stated that this commodity class includes ligno-cellulosic (i.e., "second-generation") bioenergy sources. These bioenergy crops are a very important component of future land use change in the SSP scenarios, and probably account for the vast majority of

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the growth of bioenergy here. This is because traditional uses of firewood and charcoal, and industrial recycling of bio-derived byproduct fuels, are simply not energy demands that are likely to scale up in any significant way in response to an emissions mitigation policy. So, by bunding second-generation bioenergy crops with waste and traditional biomass commodities whose production is not tied to land use, the scenarios are getting up to 450 EJ/yr of bioenergy, almost as high as total global primary energy consumption of all fuels today, without causing land use change or any other consquences relevant for emissions and carbon stocks.

2) conversionEff: this parameter describes the relationship between the combustible energy content of harvested bioenergy and the biofuels produced, in the form of ethanol and biodiesels. The authors estimate this efficiency at about 65% in the base year, with a maximum value (year 2100, with efficiencyBEcrEJ set to 50%) of 95%. The end-of-century levels are simply not realistic; that would entail conversion processes wherein the vast majority of the combustible energy content of the by-products (dried distillers grains and oil crop feedcakes) are somehow transferred into the fuel. I don't know what the theoretical limits on that conversion are, but I suspect it's closer to 65% than 95%.

3) A2 and A3: the annual improvement rate in the efficiency of producing coal and carbon-free energy, respectively. It is possible that this description is inaccurate in several ways; I'm hoping that what is intended is the improvement in the whole-economy energy intensity of the use of these fuels, or the ratio of primary (usable) energy to economic output. Improving the energy efficiency of producing these energy commodities (e.g., less fuel-intensive coal mining or farming practices) wouldn't make much difference to energy consumption at the global level, and in any case these practices are likely to become more energy-intensive over time, not less, due to resource depletion, mechanization of farming, and others. There are also problems if this were interpreted as the efficiency of using energy. An efficiency that grows at 2% per year from 2010 to 2100 ends up 6 times higher than it started, and for the maximum improvement rate

used, 2.5%/yr, it ends up nearly 10 times higher. There are no uses of coal in the energy system, at a global level, with thermal efficiency levels low enough to permit this sort of improvement.

And, like many parameters here, I would suggest calculating them from the model outputs in the publicly available SSP scenarios, and using some simplification from that calculation, rather than arbitrarily guessing. The SSP suggested parameterizations (guidelines) were written for IA models with a much higher level of detail of the physical systems than the tools used here.

4) Yield: the yield growth rates I would also suggest taking from the SSP database, using area-weighted and indexed cereal yields in each region. The current method assigns baseline productivity growth on the basis of the yield gap, from the Mueller et al gridded yield gap study. There are two issues with this approach. For one, as the authors note, the rate at which countries close the yield gaps is tied to "each scenario's technological growth, economic development and technology transfer." However, these attributes are more granular than the inputs to the model used, and it isn't specified how those yield trajectories were developed. Second, convergence with base-year yield gaps is only one component of future agricultural productivity improvements; the distribution itself should also shift upwards due to technological change. In regions with no or little yield gap (e.g., Europe, the USA), yield improvements to 2100 are effectively frozen in this method, which likely isn't what is intended.

5) p: the rate at which future welfare is discounted. Part of the problem with the research goals of this study is that the impacts of climate change from emissions today play out over hundreds of years, due to the long lifetime of CO2, not even taking into account issues like sea level rise or thermohaline cycle disruption. How the net present value of damages can be applied to an economy over such a long time span and across generations is a topic without consensus in the modern economic literature. Some review is warranted (e.g., Stern versus Nordhaus). Still, one point with good agreement is that the discount rate is very important for the balance between near-term emissions

mitigation and long-term reduction in climate damages. I couldn't find where the discount rate was stated, but did find a statement that the discount rate was not varied in any sensitivity analysis, so I'd suggest clarifying what is used, stating the justification, and running a couple of sensitivity scenarios.

—Specific items—

p2 line 10 - mitigation isn't solely for the purpose of decreasing negative impacts on human society. also for terrestrial biosphere (e.g., biodiversity, ecosystem function).

p4 lines 6-8: climate impacts isn't the only factor driving yield changes over time (also yield gap convergence) p4 line ∼20: how are energy supplies modeled, in order to get supplies and demands to balance? Are there exogenous supply curves used? p4 lines 20-21: all IA models represent energy markets explicitly, and have since the first-generation IA models back in the 1980's (e.g., Edmonds-Reilly-Barnes was first documented in 1986). p4 lines 23-25: given the complexities involved, I don't see how one can reasonably state that the % GDP loss is a linear function of the global average temperature, but given that it is another study that is being cited, please provide a 1-2 sentence description of how this was estimated in that study–over what time scale, geographic scale, temperature change, and was is an empirical estimate from historical data, or a model-derived estimate? It is crucially important for the results in this study, but strikes me as very questionable.

p5 line 2: the emissions pathways from this model should be compared with the published ssp's, and harmonized to the extent possible.

p5, lines 25-30: from my understanding of the methods later on, trade is set a priori and cropland expansion is used to modify the supply, so that it is equal to demand plus or minus net trade. this is a bit unusual in this field; in most models, trade is price-sensitive, and can be an important determinant of the equlibrium between agricultural production and demand. it woud be a good idea to make sure the results from this approach are reasonable in India, which has already very high cropland shares, and a population that is growing fast and becoming more wealthy, both of which put significant upward pressure on agricultural product demands.

p6, line 29: it is stated that bioenergy is only produced on abandoned cropland; what is used to estimate abandoned cropland? I'm not aware of any inventories that disaggregate this quantity specifically, but there are vast quantities of land in the former Soviet Union (Central Asia), the Middle East, and the forests of the eastern United States that were cropland at some point in human history. it is hard to see how these lands would be the preferred sites for bioenergy production, particularly in light of the locations where cropland expansion is currently taking place (e.g., tropical rainforests).

p7 line 30: the hurtt et al (2011) dataset distinguished pasture on the basis of land use, not land cover class. it classified as pasture vast tracts of land area that are not grassland, including most of Tibet, Australia, Central Asia, and the western USA. it's probably not correct to assume this is all grass, but it might also not be important for the study; I can't tell. p8 line 10: irrigation, N application, and tillage intensity are held at base year levels while yield gaps are assumed to close. However, in Mueller et al. (2012), these were the main factors that account for present-day yield gaps.

p11 - for any grid cell, the yield impact is not a simple linear function of the radiative forcing. I'm not sure what is gained by using this probability-weighted approach as opposed to just simply assigning a single RCP scenario that is most similar to the emissions outcome of the given scenario.

Figure 4 - Please clarify whether global cropland (4d) includes global cropland for bioenergy (4c). It did in the SSP reporting database and in Schmitz et al. (2014), so hopefully it does here too!

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