Interactive comment on "Towards decision-based global land use models for improved understanding of the Earth system" by M. D. A. Rounsevell et al.

Anonymous Referee #1

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This paper provides an interesting, coherent, well-structured and relevant summary of issues surrounding decision-based global land use models, the current barriers that exist to their development and outlines ways forward in addressing these barriers and further developing global scale land use models. This paper provides a useful contribution to land use modelling research, and although reiterates a number of points made in previous publications, it presents a clear and accessible current review and offers approaches towards greater inclusion of LULCC in ESMs.

Response: thanks

The paper highlights the economic-centric viewpoint of most global models which do not factor in non-economic rationale in human decision-making. Indeed, my own research has found that non-economic factors are important considerations and, while the influence of such factors could be small at an individual decision maker level, the accumulative effects through up-scaling as the system expands to the global level could be significant.

Response: yes, one of the key messages of the paper. A related important point is that the strict assumptions of economic rationality are impossible to implement once even a moderate degree of system complexity and agent uncertainty about information is incorporated. This point is touched on in Nolan et al and discussed in great detail in Parker (in press), and will be added to the manuscript.

The paper recognises the key challenges of data availability, which is a particular issue at the sub-national level, for use in data-intensive models, such as ABMs. This remains an issue and is reflected in a number of recent papers. However, there was no mention of GEOSHARE, which I believe is a new project for collecting and synthesising spatial data at the global level.

Response: A data portal under development, called GEOSHARE (Hertel and Villoria 2013; geoshareproject.org), seeks to provide an access point for data on agriculture, environment, and poverty in the developing world, and global patterns of land and water use. It is not clear, at this point whether the data will be at a level of detail sufficient for informing agent-based models. Individual-level data, however, are currently available in the form of integrated public use microdata series (iPUMS), which have been compiled from available census and survey sources globally and are served at a common portal (Minnesota Population Center 2013). Indeed, there is now a wide variety of projects for data sharing at a variety of different scales. NASA's Socioeconomic Data Center (SEDAC), Terrapopulus, University of Wisconsin's SAGE, DataONE, and the GLOBE project (http://globe.umbc.edu) are other examples. GEOSHARE is different in some ways, but not necessarily of any more significance than these other initiatives. There is also the Open ABM archive, an activity of the CoMSES network, which provides a venue to share model code, and includes code for many land-use change models. (http://www.openabm.org/). We refer to these various data and code sharing projects in the revised manuscript.

The paper articulates that current knowledge is fragmented regarding up-scaling data intensive models and no current means exist to facilitate in the coordination of data synthesis and assimilation. The paper mentions CCAFS and I'm aware they were developing what they called an online resource or 'knowledge hub' in the context of agricultural models as a means of sharing and synthesising data through facilitating the availability of raw data from research teams internationally. This will allow greater linkage between current agricultural models and create an active network of research teams. Perhaps a similar 'knowledge hub' approach is needed for decision-based global land use models to facilitate the availability of data, ideas and research exchange with other teams and improve future cooperation? Researchers could upload data and it could be centrally administered. The paper only touches on this in Section 3.8 and it does seem an area for future discussion. For example, the literature suggests that to be able to compare LULCC research, modellers need to report how they implemented land cover change in considerably more detail.

Response: yes, agreed a knowledge hub would provide a useful source of data to support this type of research in terms of underpinning both model development and evaluation. The most challenging element of creating and sharing repositories of LULCC knowledge is likely to be the need for sustained support for these efforts that is adequate in enabling their capabilities to grow beyond ad hoc efforts and experiments. Nevertheless, the creation and use of online collaboration infrastructures does tend to drive enhanced data sharing, harmonization and use to answer broader questions. These comments are raised in the revised manuscript.

The paper mentions that at the global scale it is perhaps better 'to look at effects of decision making rather than the process of decision making per se'. I think this is an important distinction to make. Models are designed to simulate various scenarios and then test the outcomes from a range of inputs, such as the effects of policy initiatives, rather than to predict the future by representing 'reality'.

Response: yes, agreed that this is an important distinction to make, and will be emphasised more strongly in the revised manuscript. This includes discussion of an approach to up-scaling that creates meta-models that link parameters at a finer scale to decisions (but without running the process based models), and then using these as inputs to coarser-scale models.

There is always a trade-off to any modelling approach and as a model is up-scaled to the global level then detail and accuracy at an agent level will be lost as more assumptions are made. The use of typologies, whilst useful as a conceptual approach to model design, can risk ignoring the heterogeneous approach, a key ability offered by ABMs.

Response: yes agreed this is one of the main challenges in up-scaling ABM approaches to the global scale level, and is yet to be achieved. We have attempted to highlight these types of challenges in the paper.

The paper does seem to reflect a number of other papers, conference summaries and so forth that suggest what the current issues are and highlight key areas in which to make progress. There is possibly the need to move away from discussions about different modelling approaches and look more at ways to integrate the most promising aspects of each modelling approach and treat them as components of global land use models rather than alternative ways to model global land use, such as the principles behind IAMs.

Response: yes, agreed, it is certainly useful to build on the best current approaches and integrate these to progress understanding. Such an approach is discussed in section 3.3, which explores the relationships between CGE models and individual-based models. However, we would also argue for the exploration of novel approaches that start with a clean slate, as an alternative approach that merits consideration in parallel with the continued development of existing methods. This would provide much needed diversity in method develop from which the next generation of LULLC modelling approaches are more likely to benefit. This point was made in a recent NRC report on land change modelling, which highlights the need for cross-scale integration, and also the need for further development of process-based approaches (NRC 2013). We make this point in the revised paper.

With many possible modelling approaches and methodologies researchers can employ, the paper does not consider the possible future development of a standardised format for describing decision-based global land use models to aid in understanding, information exchange and duplication of work. This has been mentioned in the literature, using terms such as a 'standardised framework', so results are transferable. I also mention this because I'm aware of one attempt to do this for ABMs called ODD (Overview, Design concepts and Details) protocol (Grimm et al. 2006) (1). The authors describe ODD as a first step in establishing a more detailed common format for describing IBMs and ABMs, with the hope of this protocol evolving as it becomes used by a sufficiently large proportion of modellers. Other papers have called for a framework to be established with a focus on coupling models (e.g. Heistermann et al. 2006)(2).

Response: yes agreed common frameworks such as the ODD can provide a useful means of standardisation. The revised manuscript will make reference to ODD as an example of how standardisation through such frameworks can benefit model development and sharing (e.g. Grimm et al (2010)). The recent NRC report (mentioned above) discusses a number of approaches to improving model sharing, such as: "Furthermore, options for improving the interoperability and comparability of land-change models include, better documentation (including further developing the ODD framework; Grimm et al. 2006), use of common modelling frameworks and software environments (e.g., Parker et al. 2008), sharing of models on knowledge hubs, and, even, development of community governance structures of model development and integration (NRC 2013)."

At the end of Section 3.1 the paper states an 'innovative approach is needed' but offers no suggestions of what this could be or provides any ideas or proposals. One could also simply argue that to harness energy from nuclear fusion an 'innovative approach is needed'!

Response: yes agreed, this is somewhat of a loose statement and is modified in the revised manuscript.

Although highlighted in Section 3.1, perhaps the conclusion could be a little more forceful in emphasising what the outcomes are of not including LULCC and representing human decision-making within global scale models e.g. ESMs. For example, Pielke et al. (2011)(3) state that 'unless we undertake a thorough assessment of the role of LULCC on climate, an incomplete understanding of the role of humans in the climate system will persist' and failure to factor in LULCC will have profound consequences, leading to adaptation measures being 'founded on incomplete and potentially misleading information'.

Response: good point and the discussion will be revised to place stronger emphasis on the consequences of not including LULCC in ESMs. N.B. we believe the reviewer refers to section 2.1 (rather 3.1), which will be revised along the lines of:

"As a consequence, linking process-based models of LULCC and the Earth system seems indispensable. The realism with which land cover, and the relevant biophysical and biogeochemical processes, are represented in climate and Earth system must be improved. We are still at the stage of needing to undertake a thorough assessment of the role of LULCC on climate from a range of different perspectives (global, regional, adaptation, mitigation, biophysical, biochemical). This relates both to historical simulations, as in some regions the largest rates of land-use change have already happened in the past. But for other regions, rates of land conversion are still rather large – and this includes both deforestation as well as afforestation. Both affect substantially global water use and runoff, and the exchanges of climate-relevant compounds beyond CO2 (i.e. N2O, CH4, NOx). A true understanding of the role of humans in the climate system must include an in-depth analysis of the LULCC-climate interplay across space and time scales. This is obligatory for the development of effective land-based mitigation options, as well as appropriate adaptation measures (Pielke et al., 2011)."

The paper should also be consistent, such as in the use of abbreviations e.g. it is either an ABM or agent-based model but both are used, and either land-use or land use etc. Page 892, line 5 'gas' is missing after the word 'greenhouse'.

Response: the appropriate corrections have been made.

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Interactive comment on "Towards decision-based global land use models for improved understanding of the Earth system" by M. D. A. Rounsevell et al.

Anonymous Referee #2

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[The reviewer has chosen to remain anonymous simply to avoid institutional internal bureaucracy for publicly attributable text, which would have necessitated breaking the obligation to keep the manuscript sent for review confidential.]

This article makes important points and outlines a research agenda, which merit publication in an international journal such as Earth System Dynamics – particularly since there seems considerable scope for debate on the topics covered. As it stands, however, the manuscript does not present an integrated, or even internally consistent view of the way forward, and in my opinion, fails to reflect critically on the implications of what is said.

Response. We thank the reviewer for the encouraging opening sentence. We also appreciate identification of the parts of the paper that require attention, and we will revise the paper in the relevant places (see response to the individual comments below). Concerning the point about not providing a consistent view of the way forward we would like to highlight that this review is -to our knowledge- the first that brings together the views of the natural, social and economic communities in dealing with challenges of land use change at the global scale level. Naturally, views differ in some aspects between these different communities regarding ways forward, and there will be trade-offs and pros and cons to be dealt with. Moreover, our intention was not to identify "the one" way forward, but to identify the short-comings of current approaches and the necessary progress that needs to be made to overcome these with some suggestion as to how this might be achieved. The revised version of the paper will take account of the need to provide clarity around these issues.

The authors rightly point out that there are two-way interactions between the climate and land use systems that are inadequately addressed by current climate modelling approaches. The authors then argue in section 3.1 that the land use system is the result of decision making at the local scale, which is affected by interactions among heterogeneous individuals, and hence for agent-based modelling to be used to represent land use decisions in global coupled land-climate modelling. Here, the points are more debatable: (i) Whilst many researchers will agree that simplistic assumptions about human behaviour, such as profit-maximisation, inadequately reflect real (land-based) decision making, not all do. Indeed many agent-based models of land use/cover change assume profit maximisation of individual agents! To support their arguments, the authors will need to explicitly make statements citing work demonstrating that: (a) profit maximisation does not represent real decision-making in individual land management actors; (b) profit maximisation does not represent land-based decision-making at an(y useful) aggregate scale; (c) profit maximisation is not even a reasonable approximation to (a) or (b).

Response. The point here is not to say that economics (and assumptions on profit-maximisation) are not important in contributing to land use decision making, but that economics is not the only factor that has an influence. It is correct to say that many ABMs (quite rightly) also take account of the contribution of economic factors to decision making. This is more likely to be implemented through the concept of utility maximisation (and sometime risk aversion) that is more commonly used by economists than profit maximisation, but ABMs are quite diverse. Some use profit maximisation, some utility maximisation, some non-economic decision processes. The framework itself does not specific the decision model. ABMs are also consistent, however, in seeking to incorporate representation of non-monetary behavioural factors. In developing this argument, section 3.3 gives a long discussion about the merits of building on existing economics approaches within global scale models (CGEs) in combination with individual-based models such as ABMs. The paper will be revised with a view to providing clarity on this issue.

Moreover, any profit maximization approach that has undertaken a formal model evaluation will have reported the goodness-of-fit of the model against observational data. For example Rounsevell et al. (2003) reported statistics for a profit maximising model as representing between 15% and 58% of the variance within observed land use data depending on the particular land use in question. Another classic example is Berger (2001), where he demonstrates that imitation effects in terms of network/neighbour technology adoptions are required to completely explain lags in adoption of irrigation technologies. This is a good example since it is based on a traditional mathematical programming model. And even if constrained profit maximization is the appropriate model, assumptions of full rationality might simply not work; e.g. Filatova et al. (2009) demonstrate the failings of the representative agent assumption. Also, profit maximization often fails in environments where risk and resource constraints (labour, etc.) are key, and where market integration is low (i.e., subsistence agricultural contexts).

Taken together, these studies demonstrate that a considerable part of the variance is not accounted for in this profit maximising model, which leaves considerable room for improving models to deal with the non-economic components of land use decision making. Appropriate examples and references are included in the revised manuscript to support the arguments for exploring the effects of non-economic factors on behaviour and decision making.

(ii) The leap from the inadequacy of representation of human decision-making to the automatic recommendation of agent-based models as the solution is one that will carry rather fewer readers with the authors than point (i). Though the authors dodge the issue somewhat by giving ABM as an example of a suitable approach “to better represent variation in human : : : decision-making” (p. 887), this is somewhat disingenuous: much of the rest of the paper focuses on ABM, and indeed later in the same page, the authors state that “the representation of land-use decision making in global scale models does not yet take sufficient stock of the progress made in regional scale ABM”.

Response. The point here is to explore what alternative methods exist in the literature to represent land use decision processes. The argument is made in the text that ABMs are commonly used in regional scale land use studies, but not yet at the global scale. Indeed profit maximising models were at one time more commonly used in regional LULCC studies, but there has been a move away from these approaches toward ABM for the many reasons that are argued in the paper, i.e. that ABMs are better able to deal with a wider range of decision processes, in addition to accounting for economic factors. Hence it is reasonable to explore whether the development of global scale LULCC models might not follow the same logical evolution to an ABM strategy found in the development regional scale LULCC models. Moreover, and as discussed elsewhere, profit maximization and ABM are not necessarily exclusive categories, since as the reviewer points out, some ABM have (boundedly rational) profit maximizers. This debate is rather about equilibrium vs. non-equilibrium models. Further discussion of these points is included in the paper to clarify the logic of the thinking.

To argue the case more convincingly, particularly with respect to comparison with techniques such as microsimulation, the authors will need to argue or cite literature demonstrating that interactions among actors are important (it being generally accepted that agent based modelling emphasises the significance both of heterogeneity, which the authors already provide ample support for, and interactions among individuals, in determining macro-level outcomes). Earlier work by the lead author (Schmit & Rounsevell, 2006), by contrast, found little evidence of imitation among farmers in Belgium. Though this is only one form of interaction, it is one argued to generate complex dynamics that cause rational agent model predictions to fail (e.g. Taleb, 2007). (The introduction does mention interactions among land use choices, but they are not sufficiently social that ABM is necessarily stipulated.)

Response. It is correct to say that Schmit and Rounsevell (2006) found no evidence for imitation behaviour in an empirical analysis of a small study region in Belgium. However, this appears in the literature more as an exception than the rule, and there is a wealth of literature to support the importance of agent interactions in underpinning LULCC decision making. This includes the seminal works of Hagerstrand in the 1960s to more recent studies of spatial diffusion supported by both empiricism and modelling (Alexander et al., 2013) that is cited in the manuscript with examples. Recent work on land use in residential (e.g., Nassauer et al. 2009; Hunter and Brown 2012) and agricultural landscapes (Isham 2002; Walters et al. 2005) provides ample evidence of spatial and social interaction effects on the way land is managed. Another important spatial interaction process is that of land markets. Recent papers have examined this topic (also focusing on the complementary effects of heterogeneity) (e.g.,: Sun et al, Huang et al., Schreinemachers et al.) In the revised version of the manuscript we will draw on these to support the arguments put forward.

In addition to providing evidence supporting the influence of interactions among land managers on regional land dynamics, specific consideration of alternatives to ABM such as microsimulation and system dynamics modelling (and, presumably, their relative inferiority) would be more persuasive. I am confident the authors will be able to find references supporting the arguments in section 3.1 that I have highlighted above that are not, in my opinion, already adequately thus supported.

Response. In addition to the comments above, we recognize that there are alternatives to ABMs, but have focused on these in contrast to the two approaches mentioned for the following reasons. SD models suffer from the same aggregation effects, which limit representation of heterogeneity and interactions, as CGE models. They can represent feedbacks among processes connecting various stocks, but are not well suited to representing the content of those stocks as dynamic and heterogeneous entities. We have included additional discussion that describes systems dynamics approaches and compares these with ABM (with further references).

The distinctions between ABMs and micro-simulation are not as clear, with both focusing on the individual entities. Micro-simulation models can be useful in dealing with some of what is in agent-based modelling. However, micro-simulation models for land use are predominantly applied in urban environments (e.g.: Waddell, P. et al., 2010) and Huang et al (in press, EPB), which is less appropriate to global scale modelling. Our view also is that ABMs are distinguished from micro-simulation models by their emphasis on and more elaborate options for decision making strategies of the units (agents). A comment to this effect is included in the revised manuscript. We have tried to limit the extent of these additions since the purpose of the paper is not to review all available land use modelling approaches, but to put forward with argumentation approaches that the authors feel are appropriate to the demands of large-scale land use change modelling and the interactions of LULCC with other global scale models of the climate and Earth system.

Section 3.2 of the paper onwards then covers the challenges associated with empirical agent-based modelling of the global land-climate system. It is this that brought to mind the writings of a popular science fiction author (Adams, 1980), which I see from Wikipedia borrowed the apposite phrase from Caroll's 'Through the Looking Glass'. I didn't find six impossible things in the article, but here are four:

Response: Some centuries ago, people thought it to be impossible that the earth was round; once that had been proven to be the case, it was deemed impossible that the earth moves around the sun. And so forth. While we have no intention, whatsoever, to place a review such as ours on the same level as these seminal breakthroughs, we do believe that one should use "impossible" with caution when discussing scientific progress.

Impossible thing #1: Agree globally applicable typologies of actors (this being suggested by the authors on p. 889 to address the acknowledged issue with lack of data – which might itself be impossible thing #0). Segmentation of farmers (just one of the classes of actor relevant to the land-climate system) is the bread and butter of the rural sociology literature. Sutherland (2010), for example, says that "there is no single accepted typology of farming styles", citing van der Ploeg (1994) as asserting that typologies are region-specific, and Howden and Vanclay (2000) as claiming that typologies are artefacts of the researcher's methodology.

Response: Yes agreed that developing globally applicable typologies is a major challenge if typologies are indeed used to up-scale individuals to the global scale levels. This does not mean, however, that it is impossible only that, as with any model, it will involve simplification in ways that (we argue) are better than simplifications that ignore agent heterogeneity all together. This has simply never been attempted before for land use actors, and there is much interesting research that could be done on this topic. The lack of appropriate data is a problem (as raised in the article), and something that should be addressed in order to make progress. Note also that we are not looking for a single accepted typology, but rather a group of them. Current CGE models make some assumptions implicitly about supply functions, and thus we would argue about typologies. These points are included in the revised paper.

Impossible thing #2: Describe evidence-based decision-making algorithms for aggregated types. This is the second of two approaches suggested for addressing issues with data. Whilst ABMs commonly use aggregated agents (e.g. households, businesses), these are usually associated with identifiable types for which behavioural data could, at least theoretically, be obtained. But for the large-scale aggregates considered by the authors, such as ‘communities’ (itself an amorphous term), are there any reasonable methods for gathering data on decision-making? Can you interview a community? Can you send it a questionnaire? Indeed, later (p. 894), the authors seem to suggest that aggregation is impossible: “we cannot describe the rules and typologies of these large scale emergent social behaviours”. (This is not the only place where the article contradicts itself.)

Response: The article frequently refers to things that are not currently possible with current knowledge and understanding. That does not mean that they might not be possible at some future point. Indeed imagining what is important in terms of LULCC model development is an important step in realising such advances. The role of this article has been in imagining what might be possible and to explore alternative possibilities to progress LULCC methods.

The principles of complex systems (and their representation in models such as ABMs) is that aggregate properties (such as communities, societies, …) emerge from the interactions of individuals, locally. ABMs are capable of representing the individual actors that form the community; hence there is no need to ‘interview’ a community. Moreover, this does not contradict an approach that might use a typology since agents types are often used to simplify the parameterisation of numerous agents, but this does not preclude multiple interactions between many individuals. Indeed, it is the effect of these multiple interactions that leads to aggregate outcomes.

All that said, providing empirical evidence for decision-making strategies is among the most challenging aspects of empirical agent-based modelling. One of our references (Robinson et al. 2005) provides a review of empirical strategies, including those that can support inference about decision-making strategies. We will elaborate on these strategies in the context of representing agents.

Impossible thing #3: Unify equilibrium-based methods and out-of-equilibrium dynamics. In section 3.3, the authors speculate on the integration of ABM and CGE models, presumably because the latter are currently applied when modelling global land use/cover change (p. 891). Yet one of the often-stated advantages of agent-based modelling approaches to the simulation of markets is their ability to represent out-of equilibrium dynamics. Although they cite Schreinemachers et al. (2010) as an example of how the integration can be done at the farm scale, the cited work uses utility maximisation agents, which the authors are critical of earlier in the paper. It seems strange to argue in section 3.1 that such simplifying assumptions are wrong, to propose ABM as a more realistic alternative, and then in section 3.3 to seek an integration of the flawed technique with the proposed alternative. It is also not clear why this integration is a necessary pre-requisite for modelling the land-climate system with more realistic human decision-making.

Response: The citations we use are often illustrative of one of the points we wish to make. They are not meant to be endorsements of the cited research as illustrative of all aspects of model development that we advocate in the article. Our paper is essentially pointing to several innovations in the literature that could be brought together to make an important advance. If any single paper had already done everything we advocate, there would be no need to advocate it. That said, the reviewer seems to be under the impression that economic optimising methods are being stated here as having no value, which is not the case. The article argues for improving current approaches, which is a reasonable aspiration unless one believes that the current methods could not be improved upon. As stated earlier (see above) many ABMs use utility maximising approaches as a component of their implementation, so it is perfectly reasonable to explore how this might be done better, at the global scale level. Hence this section explores the potential for integrating across methodological paradigms in ways that could advance models of global LULCC. For example, ABMs could be designed to replicate short and long run economic equilibria as discussed in detail in Parker, D. C. (in Press) or Nolan, J., D. et al. 2009. These points (and references) are included in the revised manuscript with an additional paragraph comparing CGE and ABM approaches at the end of Section 3.3.

Impossible thing #4: Semantic integration of global data. This is covered by the authors in section 3.8, but deserves emphasis. Comber and colleagues (2005; 2008) have, for example, discussed issues with semantics associated with land use and land cover, and the potential for confusion over types (even seemingly simple types such as ‘forest’). The “variation [of] : : : class, ethnicity, gender, : : : cultural-historical backgrounds and governance regimes” among individuals and regions, to which the authors refer on pp. 886-887, poses a significant obstacle to a unified representation of not only meaningful land use and land cover types, but also types of people, institutions and networks embedded in the global land-climate system.

Response: This is a reasonable point, but certainly not impossible, just extremely challenging and requiring sustained and persistent efforts. As noted in the response to reviewer 1, the key to resolving this challenge is in sharing and harmonizing data in sustained support of these efforts, and collaborative data sharing and utilization systems that incentivize efforts to harmonize and share data. Global data on land cover, economics, demography, and agriculture do now exist. Indeed, the creation of these datasets has required significant consideration of these semantic issues, and they will be ongoing in any global scientific enterprise. Our paper argues for a focus, in addition, on the processes by which land changes, which is also an important part of this enterprise and one that we feel is under-attended.

The point is that in outlining a series of research challenges to developing an empirical global agent-based model that could contribute to climate predictions through contributions to land decision-making, the reader is not left with an impression of a feasible agenda that, if followed, will lead to greater accuracy and realism in climate modelling.

Response: As we have stated at the beginning of our response, the intention within this article is not to set out an agenda for how this might be achieved, but more to explore the issues and challenges that the Earth System community faces in pushing forward with better LULCC-climate system models. There are –depending on individual research questions- many different ways forward.

The conclusion to the article misses, in my opinion, an opportunity for the authors to reflect critically on the implications of their analysis for climate change models to adequately reflect the role of social systems. It is ironic, isn’t it, that although the discourse of climate change has always been about the question of human impact (even though the IPCC have only recently declared they are 95% certain of it), climate models have, as the authors observe in the introduction of global scale models of human systems, failed to “account for the diversity in the types of human behaviour processes, decision making strategies and governance structures” (p. 879)? Increasing attention has rather been paid to reducing uncertainty and more detailed modelling of the physical climate system.

Response: yes an important point to be emphasised in the conclusions section, which will be done in the revised paper. It is important to make the distinction between climate models (GCMs), which aim to predict the way the biophysical climate system will respond to given trajectories of GHG and land use forcing, and IAMs, which aim to couple the human processes that determine GHG emissions and LULCC with their climate consequences and feedbacks. To criticise GCMs for not including human feedbacks misunderstands their purpose.

This in part, as the authors acknowledge in the conclusion, is due to the fact that representation of social systems in formal models is an open question. It is a feedback loop too far for precise quantitative modelling: the representation of social systems in models requires ontological commitments and assumptions that are the subject of controversy and debate in the social sciences, and are likely to remain so for the foreseeable future. If this (as seems likely) leads to even greater uncertainty in emissions predictions, won't that prove politically counterproductive in the climate change debate beyond the scientific community?

Response: there is a point here about raising the awareness of the importance of social processes (and their representation in models) that is important for the climate modelling community to be confronted with. Even if this does point to the difficulties in achieving such an ambition. This is fundamentally the aspiration of this article in raising these issues. The reviewer argues that highlighting shortcomings of existing approaches will be counterproductive in the climate change policy context. We believe this to be a dangerous argument; if the scientific community was to take this on board it would seriously hinder scientific progress. Yes, there are many short-comings in natural and social aspects of climate change science. Not to debate these openly, in the scientific community (and our paper is targeted at scientists) because methodological short-comings are an easy target for climate sceptics does not help the cause.

Another way of thinking about this is that the greatest uncertainty in future climate predictions lies in the range of possible emission trajectories, not in the inherent unpredictability of the biophysical climate system given a predetermined forcing. This was apparent in the IPCC AR4 results and equally so in AR5. One might say that, if we were to make an unbiased appraisal of the balance of effort going into climate studies, it would seem that the thing we are most certain of - biophysical climate dynamics - attracts at least 95% of available research funding while that we are least certain of - the human drivers and responses - attracts the remainder.

By making the case for agent-based modelling of the global land-climate system, aren't the authors really making the case for the infeasibility of accurately including social systems in global models, and, since humans are embedded in that system, for the infeasibility of climate modelling as a whole?

Response. We do not fully understand this comment - there is no clear logic here as to why making the case for ABM means that including social systems in global models is infeasible. On the contrary, we argue that a global LU ABM would be one way to improve coupling of global natural and social system models in a way that would allow feedbacks to be explored in a process-based manner.

There are some good working examples of 'massive' ABMs that work well for specific purposes. For example the Epicast model (Germann et al., 2006) is a synthetic population model of America with 360m agents. It was designed to model avian flu spread. An Australian version with 22m agents has been used to model the spread of dengue fever under climate change (Newth and Gunasekara, 2010). These models illustrate the point that discrete models (ABMs) of populations exhibit emergent properties that are not seen in aggregated models and which do not depend directly on the agent properties.

Yet, since the trajectories of carbon emissions have continued to follow the worst-case scenarios of the IPCC (Peters et al. 2013), isn't there an argument that, at least for the short to medium term, we can approximate the impact of human decision-making on the climate simply by assuming that societies will do whatever leads to the greatest emission of carbon? That is to say, do the intricacies and complexities of the social system really matter as much as the authors claim? (Though, on p. 894, the authors cite Finnegan et al. 2012 as asserting that local contingencies in decision-making emerge to form regularities at the larger scale.) Will modelling it in more detailed or realistic ways really have a radical effect on climate predictions if the actual trajectory of emissions has pretty much consistently been as bad or worse than the worst cases the IPCC have been able to imagine from the 1990s onwards?

Response. The main point here is that we do not know (nor does anyone else) whether better representation of social processes in global models would make their predictive capacity better, which is simply because such models do not exist. Until such models do exist it is not possible to undertake the model experiments and sensitivity analyses to support or refute the benefits of such an approach. Moreover, it is important to highlight here that modelling can have very different objectives. Models are sometimes used to make 'predictions' or at least 'projections', as indicated by the reviewer, and in these cases process-based models are not always the best way forward. However, this approach does not lead us further towards systems understanding. Hence, and importantly, modelling can also be used to advance understanding of processes by constructing experiments that explore different representations of those processes. Understanding rather than prediction is the main point of this article, and this point will be emphasised (and clarified) in the revised paper.

There will always be room for simple models as well as very complex ones – their application domain, and their strengths and weaknesses depend on the question to be addressed (Fulton et al., 2012). However, if one were to reduce, for example, the climate change question to CO2 emissions alone then indeed the views expressed by the reviewer might be taken as plausible. However, there is a clear need to go beyond thinking of climate change as being solely about CO2 emissions. Hence, we argue for models that will help system understanding (rather than models that provide only improved greenhouse gas emissions) – with the trade-off that along the way there might be enhanced rather than reduced confusion. This is the fundamental message that this article seeks to convey and the paper will be revised to better reflect this point.

Other points:
On p. 886, the authors assert that "simplification of human decision-making leads to uncertainty in assessment results". However, agent-based modelling of the social system will, if anything, increase the uncertainty of assessment results. From a strict interpretation of uncertainty, adding agents will increase the number of parameters and variables to initialise the model with, and sensitivity analysis of these unknowns will increase uncertainty.

Response. Agreed, 'uncertainty' is the wrong way of communicating this point. In this sentence, 'uncertainty' will be replaced by 'a lack of confidence in'.

On p. 893, the authors also speak of the "complex dynamics, : : : threshold effects, multiple equilibria and path dependency" that agent learning and evolution would introduce. Even if the parameters and variables were known with a reasonable degree of certainty (which of course, earlier points in the paper make clear will not be the case), such dynamics can create radically different outcomes, as per the butterfly effect. On p. 903, the uncertainty associated with more realistic representations of the social system is further increased: "There is rarely a single, unambiguous way to conceptualise and represent these system effects in models, and yet model results can be profoundly sensitive to that choice of representation."

Response. Yes agreed, this is a characteristic of complex systems. However, that does not mean that the causes of complexity should be ignored because of an assumed loss of 'predictive' power (see point above about prediction versus understanding in modelling). Indeed the point supports the contention that complex systems thinking (as for example used in ABMs) is an appropriate way forward, which is argued in the text.

Also on p. 886: "simplification of human decision-making : : : hampers the ability to assess how people respond to environmental change as a system feedback". Even utility-maximising decision-making, where it incorporates environmental variables, will create a feedback loop between the environment and decision-making. (Note also the contradiction with p. 889, where the authors say "Aggregation and simplification of the variation in agent decision making is inevitable.").

Response. This is strictly speaking describing an impact of utility maximising decision making on the environment rather than a feedback. For it to be a feedback the decision would affect the environment, which in turn would affect the basis for the decision making. In utility based models there is no capacity to modify the decision structure (and the decision making entity) as would be possible in an ABM as agents change their decision making strategies in response to environmental change, i.e. adaptive learning.

The apparent contradiction about 'simplification' has been removed from the revised manuscript.

On p. 890, it is suggested that "more detailed agent-based models of individual decision making [could be used] to understand how aggregate decision making emerges". This seems to act in opposition to the proposal of aggregation as a solution to upscaling ABMs on p. 889: If we've aggregated to avoid issues with data availability, it seems contradictory to then disaggregate to cope with the problem that "aggregate agent types do not represent real-world entities" (p. 890) for which data can be obtained.

Response. The manuscript discusses and explores different ways in which ABM type models might be applied at different scale levels without drawing conclusions about which is the most appropriate method. It is reasonable, and indeed necessary, therefore, to discuss alternatives in the manuscript. As highlighted above, it is not our intention to pave the way for a single solution to these problems with respect to land use modelling, but rather to highlight various options, that come with their own strengths and short-comings.

On p. 894, the authors discuss more complex representations of adaptation and learning. They focus on evolutionary algorithms, and although there is work (which they cite) using this approach, evolutionary algorithms have a conceptual link with memetics, which has been discredited (Edmonds, 2005). Better would have been to look at learning and adaptive architectures with an Artificial Intelligence heredity, some of which have a sound basis in psychology. There are several, some of which have been used in agent-based models, including such models applied to land use/cover change.

We argue that evolutionary algorithms have their uses in the context of adaptive learning for land use decision making. The manuscript provides several recent examples within the land use domain where this approach has been applied successfully. It is correct to say that other approaches are available to model adaptive learning, so we will amend the text to discuss and reference the use of such approaches. This includes Bayesian belief networks and artificial intelligence approaches.

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