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Earth System Dynamics Discussions

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Interactive Comment

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.

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).

(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". 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

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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.) 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.

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:

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.

Impossible thing #2: Describe evidence-based decision-making algorithms for aggre-

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gated 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.)

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.

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.

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.

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. 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? 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?

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?

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. 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."

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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.”)

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 up-scaling 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.

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.

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