

SWA: Multiagent simulations to explore rules for rural credit in a highland farming community of Northern Thailand

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Research Question

This paper seeks to answer the following questions:

- What are the potential and limits of bottom-up models such as MAS to explore the functioning of agricultural systems, in particular farm differentiation and rural credit systems?
- What are the potential and limits of MAS models to support a bottom-up modelling approach, i.e. a participatory modelling process in which models are conceived and used with and for local stakeholders?

These are questions but they are not research questions. They not make falsifiable propositions. They do not engage with a research debate (e.g., something along the lines of *we hypothesize that credit patterns follow behavior X* to set up a finding of *our participatory model showed that they actually follow Y*).

Thankfully, the authors bypass their ostensible questions to explore some interesting points along these lines. For example, on page 624, they show how the linkage between formal and informal credit in their model confounds expectations of conventional theory. The authors would have been well-served to ditch the MAS-as-a-whole questions and focus more on the substance of their model.

Model Effectiveness

I consider the research questions ill-posed and so focus my assessment of the model on its potential utility under a better-formed research regime.

This model is highly effective. This judgment is best understood in the context of a discussion of the relative merits of empirical vs. modeling studies. With

empirical studies—e.g., if the authors had obtained credit statistics in rural Thailand and run econometric analyses—one cannot dispute the realism of behavior (the data is a record of actual life) but observational data is riddled with selection effects, confounding variables, and the like, such that proving cause and effect is difficult. With models, it is much easier to assess causality because the researcher is in total control of all relevant variables, but models are often unsatisfactory simplifications of real life. At their best, participatory ABMs have the advantages of both: the inclusion of expert opinion enhances model credibility while the formal model structure allows easier causal inference.

This model’s object of study—rural economics in the developing world—is very difficult to do empirical work on because of the lack of good data. Purely theoretical approaches often fail to capture key behaviors. This sort of participatory ABM thus can rapidly generate valuable insight and in fact did: their findings regarding credit patterns were interesting and of theoretical interest. One would not have gotten these findings without the realism of participatory play and the structure of a formal model, and so their model should be considered effective. It is a shame that this good work was saddled to poorly-formed research questions.

Course Themes

Alternative theoretical models of decision-making: actors in these model are not economically rational (by the strict definition of the term). For example, land-usage depends not on a straightforward calculation of costs vs. benefits, but rather on socioeconomic status and cultural preferences, among other things. Similarly, agents do not hesitate to go into debt to afford non-productive expenses (e.g. host weddings).

Model verification and validation: the authors’ use of participatory roleplaying allowed validation of a sort. Participants agreed that the game corresponded closely to their real lives. However, I find their validation incomplete since first-hand perception is not necessarily accurate: e.g., the fact that I am a participant in the housing market does not necessarily endow me with real understanding of the system as a whole. To be clear, it is good that participants found the game believable, but I don’t think is enough.