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CHAPTER 3

Who Follows the Elephant Will Have Problems: Thought on Modelling Roman Responses to Climate (Changes)

Maurits Ertsen

Introduction

Societal responses to and dealings with climatic change have become one of the major issues within archaeology. Recent attention to this theme relates clearly to other important discussions in the field, like how controlled water provision through irrigation would have created relatively stable conditions in ancient Mesopotamia, Egypt, India, China, Mexico and Peru—making societies less directly dependent of climatic conditions. Water systems—especially irrigation—did indeed provide more stable conditions to produce the food to support larger populations. When water would make itself available in the wrong amount at inconvenient times, however, would also threat that same food production.

M. Ertsen (✉)
Department of Water Management, Delft University of Technology, Delft, The Netherlands
e-mail: m.w.ertsen@tudelft.nl

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A narrow environmental threshold between benevolent and threatening water may separate stable societies from unstable ones.

Indeed, the archaeological record has its fair share of water-related disasters, with many of them being associated with climatic change (Costanza et al. 2011; McAnany and Yoffee 2010; Schwartz and Nichols 2006). In the reconstructions of those cases, climate is typically positioned outside places or people—as an external input acting independently from what it affects. Far from being an "external driver", however, climate is not independent from places and people. People—societal agents—recognize and respond to climatic changes—which must mean that climate change itself becomes something different, even when the physical properties would not change. These physical properties of climate would change, however, as landscape—places—will have potentially affected climate as much as the other way around (Gilgen et al. 2019). An important further aspect to consider is that places did not necessarily stay the same over time. Like other animals, humans are niche constructors: they modify their habitats (Kendal et al. 2011; Scott-Phillips et al. 2014).

Within archaeology's long-term perspective on larger spatial scales and longer time scales, it remains challenging how social complexity could emerge from continuous interactions between humans and landscapes—let alone how those interactions would have been influenced by climate change. We see the same challenge in recent works on climate change in the Roman world. Dermody et al. (2012) discuss precipitation changes between 3000 and 1000 yr BP. They conclude that changes in atmospheric properties would have changed precipitation patterns, rather than deforestation activities in the period. Gilgen et al (2019) specifically discuss anthropogenic effects on the Roman climate, to conclude, however, that finding these effects is still rather difficult. One element that proved a challenge in the modelling efforts was human activity itself. The authors express a desire for both more detailed simulations and better coverage of (rhythms) of agricultural activities. Finally, Dermody et al. (2014) present their model-based analysis of grain-trade in the Roman world. Their finding is that grain production and trade could reduce the effects of variability of the Roman climate. Rome's grain network was wide enough to compensate problems in one region with opportunities in another one.

This last modelling effort comes closest to the combined models that I try to develop. My own work aims to unravel the dynamics between irrigation systems, the larger environmental setting (including climate and landscape) and societal organization. Most of my work within archaeological settings involves some kind of modelling. What I try to do in this chapter is propose how to study (Roman) responses to climate (changes) using models—especially so-called agent-based models (ABM) (see Romanowska et al., 2019; Davies et al. 2019; Crabtree et al. 2019 for a useful overview)—reflecting on work that I have published with my students and colleagues in recent years.

As my work on the Roman Empire is extremely limited—as in amounting to zero papers—I will provide brief overviews of modelling efforts of three other case studies, to tease out some of my considerations. In the Hohokam case (500–1500 AD, current Arizona), we will discover that it is not that easy to define what would count as "climate impacts". In the Hohokam case, there might be other factors that are at least as relevant as climate to explain societal change. We will also find out that once we have a reasonable accuracy in representing the water systems of relevance to actors, we are able to narrow down possible strategies available to societies, communities or individuals when answering climatic or other environmental changes.

This focus on strategies will be further discussed regarding the Zerqa Triangle—now in Jordan—during the Late Bronze Age (c. 1300–1100 BC) would have been a period in crisis, upheaval and unrest, especially in the eastern Mediterranean, related to climate. However, we will see that the ability to recognize change by ancient actors would have depended rather heavily on the hydrological conditions—and as such on our representation of them.

Finally, in the details of managing water in Tikal (250 AD–900 AD, current Guatemala), we will see that ancient Maya may have had less reasons and options to respond to climatic changes as they might have presented themselves to the Maya and the specific shape of their water systems. After summarizing the three model-based studies, I will discuss the over-arching modelling setup these studies use—including an overview of relevant information on the models in terms of model setup and relevant parameters. Finally, I will discuss how (not) to conceptualize in models any Roman societal response to climate, using some recent publications on (methodological) issues related to modelling of/in the Roman world. In general, I will suggest that larger-scale and longer-term correlations—as, for example, between drought and elite (dis)appearance—have to be explained in terms of causalities between short-term agencies.

The Hohokam

Situated in the larger Phoenix basin (at least that is its name in modern Arizona), Hohokam culture (1–1450 AD) is one of those irrigation-based societies that did not become a complex society like Mesopotamia, Egypt or the Indus. The Hohokam are also—like the Maya—popular symbols of people overstressing a single source of food production. Drought is directly related to Hohokam periodization into Pioneer, Colonial, Sedentary and Classic periods. The Classic period would have been the result of a drought in the late 1000s–early 1100s, with abandonment of settlements outside the river valleys and concentration of settlements closer to the riverine habitats Abbott 2003. A second drought period, between 1275 and 1350, would have resulted in the collapse of Hohokam agriculture, although exactly how is unclear. Our recent work on the Hohokam (Ertsen et al. 2014; Zhu et al. 2015, 2018) suggest two related issues: climate impacts may not be easy to determine and climate may not be the most important factor to consider for societal change. Furthermore, the Hohokam case illustrates how we can use modelling to narrow down possible strategies available to societies when answering climatic or other environmental changes (see also Murphy 2012). Where the Zerqa and Maya may have suggested that models make issues only more complex, our work on the Hohokam actually suggests that models can suggest certain survival strategies.

In a first paper (Zhu et al. 2015, with material taken from Ertsen et al. 2014), we studied the variation in irrigation requirements of Hohokam crops over time. We computed water demands for crop scenarios for each of the typical Hohokam phases. Each phase included similar fluctuations within a year, resulting in different water demands in the first and second growing seasons. With precipitation largely taking place in July, August, December and January, and cropping seasons set in March–July and July–November, later crops receive more moisture compared to those in the first sub-season. Basically, net irrigation demands match the so-called anomaly index, which identifies extreme climatic events at decadal scales. There seem to be more droughts and floods during the sedentary period.

We used these basic numbers to compute total water requirements using the potential irrigated area—which would show the potential stress on the irrigation system in different periods. As we have used four phases of Hohokam chronology, our results are continuous within a phase, but cannot be so between phases—especially as data on population and area size are not continuous. Therefore, we produced two leaps in water demands in Hohokam chronology, between Early Pioneer and Late Pioneer and between Late Pioneer and Early Colonial periods, respectively. Even though, our results for potential maize yields over the span of one thousand years do not suggest that maize water requirements would have become a serious limiting factor. Nevertheless, we can infer from archaeological evidence that stress would have been present at times.

In a follow-up study (Zhu et al. 2018), we therefore focused in more detail on the shifts in water demand between Early Pioneer and Late Pioneer and between Late Pioneer and Early Colonial. In this study, we used a similar short-term approach as already suggested for Maya and Zerqa. Taking representative reconstructions of Hohokam irrigation systems over time, we modelled how these systems could have been used to bring water to crops. We used the amount of hours needed to complete the water distribution for the entire system to measure management complexity. In other words, we used a time-related variable to study the influence of spatial expansion of irrigation—as Hohokam systems grew in size over time. Based on these models, we suggest that irrigation systems of different sizes would have faced different issues and would have required different sets of solutions.

Small irrigation system could have delivered water to farmlands in sufficient quantities in any water control and use scenario. Whether canals were used together or separate in time, all fields would have received plenty of water. Medium irrigation system could still spread water over farmlands, but only reach a basic irrigation demand for all fields. However, when water control was intensified by building weirs on the main canals, the irrigation time was drastically reduced, thus creating the option to increase water delivery to fields. In larger irrigation systems, intensified water control would have been required to keep water distribution at a basic level at all. Using intensive water control would also have changed the priority of water allocation to branches, as our model suggests that certain a certain order in terms which canal uses water first influences the total irrigation time of the system. In other words, users among all branches were required to cooperate to ensure equal and useful water distribution. In our final irrigation model, the largest, we find that even water control structures like weirs could not solve water shortages completely anymore. Whatever the users would have done—at least within our scenarios—water distribution could not be made equal between fields and (presumably) farmers. We suggest that solutions to deal with such water shortage could go beyond “water”. The Hohokam communities that encountered water problems may have intensified or developed specialized pottery production to trade to overcome its water limitation.

This modelling effort suggests not only that water is not everything in irrigation, but also that the Hohokam may not have had problems with a changing climate per se. We think we can conclude that maize production would not have felt huge effects of changes in rainfall and temperature. We do suggest, however, that a limiting factor for crop production would have been the ability to bring water from source to fields. It is obvious that any stress on water availability—for example, because river flows were lower—would only have increased the stress within irrigated agriculture, but we strongly suggest that water distribution within growing irrigation systems could only have been solved by the Hohokam up to a point, even in situations that water availability was not an issue.

Useful as these modelling efforts have been for our understanding of climatic shifts and the options for the Hohokam to respond through/with their irrigation systems, we still aspire to develop a next-generation model. Recent archaeological data provide much more detail about the potential field layout of Hohokam agriculture. In Ertsen et al. (2014), we already provide some model-based analysis on the scale of such fields, but our later larger-scale model study did not yet include fields on the available level of detail from the data. Adding these would have allowed to include more details on how to operate the irrigation system as well. However, I can show some examples where such focus is included. Let us now turn to Jordan, later we will encounter how we developed a model-based analysis for the Maya water systems in Guatemala.

The Zerqa Triangle

The material from Jordan (Kaptijn and Ertsen 2019) focuses on the question how water users, in this case farmers, would be able to recognize changes in climate in terms of water availability, yields, or other signals. Our work focused on the Zerqa Triangle, which has seen many changes in terms of land use, water systems and climatic regimes. A period that would have seen considerable climatic fluctuation is the end of the Late Bronze Age (c. 1300–1100 BC), a period that is also discussed in terms of crisis, upheaval and unrest, especially in the eastern Mediterranean. In our recent paper, Eva Kaptijn and myself used basic modelling methods to capture important dilemma's for farmers that would have been confronted (without knowing it beforehand) with changes in climate—in our case modelled as increasing temperatures and decreasing rainfall. When does a farmer decide that the lower yields are not "just" a bad year in an unchanged sequence of years, but an average year in a new sequence? In our approach, we encountered the key question how current researchers should study this question as well.

In general, it is not unreasonable to expect that when temperature becomes higher and rainfall becomes less available, average crop yields become lower—assuming no other water source can be mobilized. Indeed, we tried to quantify which cropping strategy would have been good options to cope with the change from wetter to drier conditions. In our "original climate", rain-fed farming produced pretty acceptable yields (in terms of predictability, not absolute numbers, as we could not model the ancient crops). It was also clear, however, that even then irrigating crops (if only in drier years) in our baseline climate would have sustained higher yields. Obviously, in case, artificially watering fields were recognized practise before climatic change, farmers may have simply continued to use it—although perhaps water availability would have been less. If irrigation was not practised yet, drier conditions may have suggested to farmers to divert water from rivers and wadi's to fields. There is actual evidence for floodwater farming during the Late Chalcolithic and Early Bronze Ages I-II (c. 4600–3600/3600–2700) (Kaptijn 2015).

An important aspect, however, turned out to be the environmental conditions one would assume to determine the baseline situation when the climate started to change. Groundwater—and generally moisture availability—are rather crucial factors to consider. Our modelling suggested that with high groundwater tables (2 m below the surface), the soil would have contained sufficient moisture to cope with increasing climatic aridity. Groundwater may have been depleted on a much longer term, but would have sustained crop yields for quite some time. Unfortunately, for the farmers of the time, however, groundwater levels would not have been so high. The current groundwater level is 30–100 m below the surface (Van der Steen 2004: 32). It may not have been that deep in the Late Bronze and Iron Age, but archaeology does suggest that groundwater would have been deeper than 2 m. At ed-Dayyāt, located on the northern bank of the Zerqa, an Iron Age I silo was defined with a depth of just under 2 m (Kaptijn et al. 2011: 152). No traces of water were identified either, which suggests deeper water levels. A well outside Tell es-Sa'idiyyeh, north of the immediate Zerqa area, but still close, had a suggested depth of 8 m below surface (Tubb 1998: 85). All this archaeological research strongly suggests that groundwater in the Late Bronze Age/Iron Age Zerqa Triangle was too low so support crop yields in a drying climate.

All in all, we concluded that inhabitants of the Zerqa Triangle mist have felt the impact of more arid conditions at the end of the Late Bronze Age. Temperatures would not have to rise, actually, as increasing aridity alone would have produced quite some crop stress already. With drier years becoming the "new normal", assuring food security through storing surplus yields from a good year to a bad year became less feasible—as surpluses became scarce. This process may have made the drying Zerqa from a rain-fed agricultural region with occasional irrigation in dry years to an irrigation-based farming area. This would have created its own issues—with labour and coordination coming to mind—but given the archaeological evidence for large-scale use of irrigation in the later Iron Age II (c. 1000 BC) (Kaptijn 2015) this seems to have been a feasible option. When moister conditions returned—possibly during the same Iron Age II—irrigated agriculture may have continued allowing cultivating more water dependent crops like flax. As such, climatologically less pleasant conditions for crop cultivation at the end of the Late Bronze Age may have created ideas and/or incentives to shift to crop strategies that at first secured food, but that created very productive cash crop agriculture in later, climatologically more advantageous, periods.

The Maya

The Maya have been a popular case to stress the proximity of human societies and water, but also a popular case to stress that societies can either overshoot their natural resources—or simply end with climatic stress (Braswell et al. 2004; French and Duffy 2014; French, Duffy and Bhatt 2013; Kuil et al. 2016; Lentz et al. 2015; Scarborough and Lucero 2010; Scarborough 2003). Given that there are still many Maya people around, the former idea that the Maya have collapsed is a little strange, but the current claims that droughts have profoundly reshaped Maya society are still strong. In our recent paper (Ertsen and Wouters 2018), we focused on the water system of Tikal during the Classic period (250 AD–900 AD). We wanted to study how the Maya could have controlled and organized their water systems given specific loads, stresses and requirements. We basically concluded that at least in Tikal, the specific shape and loads on the Maya water systems may have created unclear signals of change of and as such less reasons to respond to climatic changes for the people of the time. In other words, although climate change may have led to changes in Maya society on the long run, it would not have created changes on the short term. We encounter the classic dilemma in archaeological and climate studies: how can long-term change be created by the short-term stabilities that we encounter most of the time?

I will first detail a little why we think we could conclude that the Tikal water system may have been less vulnerable for change. Obviously, our Tikal model was rather basic, but the hydraulic model was as accurate as could be given the detailed archaeological data available for Tikal. We based our model on existing data from secondary literature, which gave us the dimensions of reservoirs, possible connections between them and shape and sizes of agricultural land. We included the Maya themselves through their water demands, which we translated into flows taken from certain parts of the total system. We did not use detailed climate scenarios, but defined representative events that would have brought more or less rain to the water system. Even this first, almost conceptual approach, brought some interesting results.

One general idea of Tikal's water system is that the higher (elite) reservoirs would have supported the lower (common) reservoirs. We argue that regular conditions, but also increasing droughts, would not have created any need to convey water from central reservoirs towards lower reservoirs. Even in our driest scenario, with most users, all reservoirs can handle local demands. Actually, the Tikal water system may have had more problems with water surpluses, as large rainstorms bring large spill-overs. Much water needed to be drained from central Tikal into lower lying areas, even during rain events that cannot have been very uncommon. In other words, wet conditions may have been a larger problem than dry conditions. Continuing on the issue of control and response, we suggest as well that the Maya of Tikal would have had problems with quick responses to extreme events: imagine the difficulty to respond within one hour to rainfall. We tentatively concluded that links between reservoirs in the epicentre and reservoirs in lower areas were created to keep the epicentre of Tikal dry. A conceptual shift from central reservoirs burdening lower reservoirs in wet conditions rather than supporting them in dry periods, would still entail top-down distribution of water and thus power. Power was simply based on distributing floods, not drought support. Climatic change—increasing drought—may have lowered water stress on lower reservoirs!

As I already mentioned, even though droughts could have seen elite reservoirs supporting reservoirs downstream, our results in general do not offer much reason to do so. Even when we increase populations fourfold, we see no strong need for external inflows. Sudden increases of water demands could still be met by inflow based on local rainwater flowing into the reservoirs. The local reservoirs would not have sustained enough irrigated agriculture to sustain the population, but could have been used to bridge droughts for rain-fed crops. Again, reservoirs may have been used as a buffer to prevent flooding of valuable agricultural areas. In short, our model suggests that water-related problems in Tikal may have been difficult to appear on the short term. We speculate that the Maya did not have that many reasons to anticipate longer-term drought. We do not suggest at all that the Maya did not have a longer-term perspective as such—the famous Maya calendar is clear proof of such perspective. We simply argue that finding direct links between climatic changes and water system operation appear to have been far from straightforward for the Maya.

The Elephant in the Room

My short comparison of published work by my team and myself, allowed me to explain how a model-based, action-oriented methodology can offer insights into changing water landscapes (see Ertsen 2016a, 2018 for more considerations on modelling). I could not discuss full models of growing complexity, as I have not have them available yet. The Maya case would be an excellent test for what emerging social complexity means in tropical conditions, with other water resource and control issues and different rhythms of non-human agents—including vegetation and crop growth, soils and properties of organic processes. A multitude of local agencies will need to be taken into account—as we did in the Maya and Hohokam cases with reservoirs, rain, weirs, management actions, etcetera.

What I have presented is obviously just a first step towards more encompassing modelling efforts, but they do suggest some ways forward. Let me try to relate my modelling approach and philosophy to some recent work on modelling in general and for the Roman world in particular. I will relate my claim that larger-scale and longer-term correlations need to be understood as being produced by causal relations between short-term agencies to recent work on modelling Roman history. Before moving to the Romans, I will first discuss my reasoning a little more. To do so, I call upon the elephants.[1]

Many will know the metaphor of the blind men and the elephant. We encounter a group of blind men, who have never come across an elephant before. Each blind man engages with a different part of the elephant's body. They then make claims of what they discover: the person studying the leg suggests it is a tree, the person studying the trunk thinks about a snake, etcetera. We, as observers or listeners, already know the object the persons study is an elephant. As such, the story offers an imagination of the difficulty we as scholars face when studying societal change. Recognizing change in the many different records one could mobilize—let alone relating these records—remains a challenge. Climate, hydrology, archaeology, texts, theoretical notions, the amount of data and ideas, but also their coverage in terms of time and space are enormous. How does all of this become a comprehensible whole? How to reconstruct the elephant?

I would argue that this desire to reconstruct the elephant is exactly our problem. After all, we do not know whether we are correct in our assumption that we are looking for an elephant. The metaphor of the blind men and the elephant only holds when the observer already knows it is an elephant—or can see the larger entity. As soon as the observer does not see the elephant, the observations of the blind men become rather more logical. Even if the observer does see the elephant, but does not know it is an elephant—or does not define it as one—the metaphor's simple narrative becomes rather more complex. Elsewhere, I have discussed this as an issue with at least two dimensions (Ertsen 2016a). We are interested (1) how ancient practices were shaped, but need to acknowledge that (2) how we in the present chose to study (ancient) practice, including how we conceptualize the material, is a key. I claim that most modelling studies do not offer the same "possibility of holding society together as a durable whole" (Latour 1991: 103) as the constructions did for the original actors themselves.

One could argue that in our modelling efforts, we do simply build our own elephant when constructing water systems. Indeed, we built

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[1] The title of this text is a modification of an African proverb "Who follows the elephant will have no problems".

reconstructions of the water systems of the Hohokam, Zerqa and Maya using the data sets that archaeologists have made available. Obviously, we did not have all the details available that one would like to have when studying a water system with an hydraulic model. This is why we have made assumptions on certain properties and/or shapes of the respective water systems. Water availability—either by rain, rivers or groundwater—is not known either exactly, we had to use reconstructions there as well. Does this mean we built elephants and assumed that the users back then perceived the same large, grey animals? I do think we did not. In our own modelling, my team and myself try to avoid the elephant in the room by not assuming what societies looked like. Instead, we focus on what the human actors of the time could have done with their water systems—or what the water systems would have made possible.

What we want to find out through hydraulic models is what options for actions there might have been—without suggesting that those actions would have been performed. Indeed, we are building water systems in the models because we want to know how ancient practices were shaped. What we do not want to do, however, is predefine what these practices looked like. We did, however, study to what extent human actions were possible, with actions depending on the system. I would argue that such information does not necessarily provide the evidence how systems were used to support societies, but—as we have seen in all three cases—we can suggest that certain problems had to be dealt with (in the Hohokam case), how climate change could have been noticed (Zerqa), or that excess water may have been a problem for the Maya. Table 3.1 provides an overview of the model features that we mobilized and human actions that we aimed to study to find options for human agents of the time to keep their water system “together as a durable whole’ (Latour 1991: 103).

I think that my elephant stories are more than just a funny interaction: it is fair to assume that the agents within the societies that we study did not have full comprehension of what happened—similar to today’s agents in societies. As such, they would have acted in certain ways based on that comprehension and their options. However, quite often, as soon as modern scholars decide to study older societies, these societies become something very close to an elephant as observed by the all-knowing outsider. Cases are studied with the knowledge of hindsight, and the actions and options of ancient (model) agents are defined within the predefined space that our elephant-society offers. Let me turn to some

Table 3.1 Details of the three model studies discussed in this chapter

Case study	*Archaeological data*	*Artificial data*	*Key parameters*	*Human actions*
Hohokam	General canal layout Selected cross sections	Size and shape of smaller canals Length of irrigation cycle Stylized crop water demands Water level at the upstream end	Irrigation time Fulfilment of predefined water demands	Options to control flows on different points in the system Alternatives to irrigated agriculture
Zerqa Triangle	Crop data Groundwater levels Irrigation features in a later period	Temperature and rainfall patterns, including changes Groundwater levels Crop water demand	Harvests	Options to sense (climatic) change in terms of water availability or harvests
Maya	Reservoirs' dimensions in the higher areas Most routes for water to flow Drainage surfaces in the lower areas	Types of overflow	Reservoir levels and volumes	Options to control high flows Options to store water for the dry season Options to sense climatic changes in terms of inflow

recent work on modelling the Roman Empire to suggest how this shows up in the modelling itself and why this would be a problem.

Modelling the Right Romans?

A few years ago, a brief discussion developed around a paper that used computer modelling to explain what the Roman economy looked like (Brughmans and Poblome 2016a). In a response, Van Oyen (2017) suggested that the model framework of the original study would not be able to deal with all complexities related to pottery and the different roles that human agents could have and/or develop in engagement with the pottery and other human agents. Defining model agents would go against "fluidity, ambivalence or messiness" (Van Oyen 2017: 1357). In their response, Brughmans and Poblome (2017) argue that defining agents does not necessarily mean that they should be defined in a certain way. Specific definitions may be popular in the modelling world (like profit-maximizing agents), but do not need to be the only ones. A quick look at the agents' definition in their original MERCURY model (Brughmans and Poblome 2016b) does suggest that the rational profit-seeking agent was used indeed by them, but I would in general agree with their statement that other roles could have been selected.

However, Brughmans and Poblome (2017) have decided not to respond to another, much more crucial issue that Van Oyen (2017) raises: "people and things co-evolve" (p. 1360). Relations between people, between people and things and between things have a tendency to change over time. If one argues that these changes are driven by laws of nature, one can include these laws in models. However, most scholars would agree with the idea that human agency is not to be captured in "laws of nature"—although I have argued that some sociological thinking does a pretty decent job in exactly that (Ertsen 2016b). This would mean that our models should allow for change in model agents' judgement and actions. Currently, however, most models (including the one of Brughmans and Poblome) do allow for choices in agents' model responses—in relation to model outcomes—but do not allow for changes in agents' reasons to perform model responses. If one is a rational trader at the start, this behaviour (a term used by modellers, see also Romanowska et al. 2019) will not change during the modelling sequence. For me, that is a very fundamental problem of current Agent-Based Models: agents are defined in terms that do not allow change over time of how they perceive the model world. Their behaviour may be guided by modelling outcomes, but cannot change by those outcomes.

Decision making by agents is complex. Assuming I go on holiday once every year: I could model this decision as a single one. However, selecting my next holiday destination used to be a process that mimics a series of discussions and decisions—not anymore, as I do not go on holiday anymore. Clustering all these every-day discussions into one decision to be modelled suggests that the final decision is independent enough of the smaller-scale discussions—which remains to be seen: perhaps the arguments we had in a year have influenced other activities and discussions. A similar point can be made for clustering individual agents into organizations or societies with decision power (like Dermody et al. 2014 do for cities in the Roman world). Apart from the somewhat difficult image of a society that decides (how, when, what?), we have to acknowledge that articulated entities result from all kinds of different agencies, which will be lost when clustering only the outcome of their interactions as a decision by a larger entity. Furthermore, as I will argue in a little more detail below, clustering typically promotes certain predefined features of larger-scale entities, based on patterns that the (archaeological) record suggests or are constructed by scholars.

I would argue that there is a more interesting way of representing agencies. Even with the uncertainty to what extent our modelled realities come close enough to the experienced realities of the time, we can be sure that all actions of the time were both enacted and valued at specific places at specific times. I will return to this point below, after some remarks about modelling language.

Languages of Change and Structure

I am intrigued by the use of certain terms related to mathematical modelling. Apparently, the term "formal modelling" is used for those modelling types by Brughmans and Poblome (and others, see, for example, Romanowska et al. 2019), contrasting it with "conceptual modelling" that would be based on narratives and qualitative analysis. This use of terms is interesting in itself, as the term "formal" seems to provide additional status to the model analysis. After all, "formal" carries more power than "conceptual": "modelling carries a veneer of objectivity and lack of bias – after all, the computer processes the outcome" (Van Oyen 2017: 1358). I consider such use of words as rather interesting. Coming from a community of water scholars, I cannot recall any use of the difference between "formal" and "conceptual" in the meaning entertained in the papers on Roman modelling. For me, the term "formal" connected to "modelling" is rather strange. In my own world, conceptual models are also coded in mathematical language. A distinction is made between process-based models (trying to mimic the physics of water) and conceptual models (simplifying the physics to study the results), but not in terms of language that is used to construct them. Both are valued, although debates are strong on what would be better. However, the terminology does not necessarily suggest a hierarchy. There are models that are problematic, some are not clear at all, but all hydrological models must be seen as formal models in the meaning that seems to be shared in Roman studies.

I do recognize the claims that using mathematical language forces the scholar to be specific about meaning and relations (as made by Brughmans and Poblome 2017 and Brughmans et al. 2019). Although the idea that mathematical formulation "ensures that there is only one possible reading of the propositions put forward by the researchers" (Brughmans et al. 2019) is a little naïve—mathematics is not necessarily the neutral arbiter in scholarly work, as the enormous discussions in many mathematical fields keep showing us—thinking in modelling terms when doing history does help me to rethink how I want to conceptualize the relations I study. However, historians and other typical non-quantitative scholars (should) try to reach the same relative clarity in their work, by explaining concepts and narratives. Let me turn to two other recent papers that discuss the issue of complexity science in relation to Roman studies to clarify this second point.

The term "emergence" is an important item for complexity modellers. After all, a major reason to claim that complex systems exist is the observation that "the behaviours of the multiple individuals and their context collectively gave rise to properties that cannot be understood as merely the sum of individual practices" (Brughmans et al. 2019). This may sound like a totally reasonable phrase, but it is actually rather problematic. It is clear for sure: apparently, we can think of a bunch of individuals that act and then produce something new. But how should we conceptualize those individuals in the first place? They cannot be isolated, because they could then not be able to act together. Furthermore, how could individuals be individual, given that they are related in all kinds of ways to others? This is again not a trivial point, but stresses the issue that what is labelled as "individual" or "collective" is not given, but defined by the scholar in question.

I would argue that the idea of a "sum" is also rather a problem. We cannot add up all kind of small-scale actions into larger unit, because there are no independent entities on different levels. I may not be "the state", but I am not independent from it in my actions—if only because I take trains ort drive on motorways. A conceptualization of individuals on the one hand and society (or context) on the other is at odds with the observation that agents exist per definition in relation to other agents (human and non-human). There is no independent context, there is a shaping of connections between all kinds of agents, again human and non-human. This is even valid for the concept of complex systems: these are shaped by defining them, they become an active part of the “context”.

The claim of emergence as leading to unexpected outcomes is an interesting one as well. One of the main aims of complexity science is to show that complex systems behave according to laws: complex scholars (want to) know the outcomes! A very good example of this tendency is the recent paper by Haldon and Rosen (2018), actually an introduction to a special issue that discusses resilience, adaptation and transformation in the Eastern Mediterranean. We read about adaptive cycles that behave according to certain laws as they move through phases. An attempt is made to make the idea of one overarching system less monolithic by claiming that there are sub-systems that “operate at different scales” (p. 277), but the paper clearly shows how difficult it is to combine essentialist phrasing—including “fundamental features” and the “dialectic between environment and social action”—with a language of nuanced, even messy societal concepts. The authors do certainly recognize that societies are not bounded entities, but maintain the language of “moving through adaptive cycles” nonetheless. They also define a set of sub-systems in the east Roman empire of the sixth century, including food production, political-institutional arrangements and the symbolic universe. Elsewhere, I have argued how close these three must be seen, or how these are enacted and enacting together (Ertsen 2016a, b, 2018).

In complexity studies, laws of complex systems become laws of nature—even when authors provide the usual remarks that these laws only hold because of observations, that they are not driving all societal change, etcetera. However, I would argue that thinking about laws of nature and/or complex systems leaves one only with a selection of one of two options: either laws of complexity/nature are actual laws—which allows no agency of anyone—or laws or nature are as constructed by the observer as laws of complexity. This second position is in line with Actor-Network Theory: laws of nature are defined in interactions between agents, and are not given by nature. This means that the outside world—the complex system, or the elephant—is a claimed reality, not a given reality. When we use the laws we constructed ourselves into models to study how these laws were produced, we encounter the important issue of circularity in complex systems’ reasoning: the same systemic features that are observed (claimed) are used as input in models that claim to clarify how those features emerge in the first place. I think that should be avoided.

By Way of Conclusion

Exploring how interactions of humans and non-humans result in systems and landscapes is a major strength of agent-based modelling (ABM). My model-based, action oriented methodology proposes to replace the bird's eye view on social and physical processes at a lumped scale with an approach that takes the many different perspectives of agents as the basis for modelling. Whatever their shape and size, landscapes are social practices: routinized sets of behaviours with interconnected elements—like physical or mental activities, artefacts, knowledge, emotions, etcetera. In social practices, human agents continuously link themselves with other human and non-human agents. Material objects are shaped through human agencies, but also shape human agencies. Without them being fully predictable, actions of all kinds of agents have a time–space setting that can be described—with the remark that descriptions may need different concepts than the traditional Cartesian logic. However, even using other conceptualizations of time, the observer can determine to what extent certain uses could be supported or what effects could have been.

My ABM approach-in-the-making (as explained in Ertsen 2016a, 2018) builds on model agents being offered a spectrum of possible actions (which will be limited for sure). From these possibilities, an agent selects certain actions based on his/her current perception of the results of actions that went before. Agents could accept what happened before (cooperate) or not accept (defect). This option to judge (perceived or real) results will allow emergence in model outcomes. I would offer all agents—human and non-human—such a spectrum of possible actions and the ability to decide what to do next. In modelling terms: all agents are allowed to update their status and actions in each modelling sequence. Obviously, non-human agents "decide" in other ways than human agents would do—for example by not changing. However, in modelling terms, the actions are of the same type. Building ABMs based on agencies for all allows model structures to emerge from model agent interactions.

I cannot show how to solve all these model issues in this chapter. What I tried to do, is simply argue that the observation, that we are not certain that what we study as modellers is an elephant, matters. More accurately, despite our claims that what we are studying might be very close to what we have defined as an elephant, our model analysis should allow for outcomes to (be) shape(d) into something else. This is crucial, as only such allowance can ensure our models show emergence—even when we use data/observations drafted by someone with the image of the elephant already in mind. If we want to take the implications of the observation that what we study as society and/or reality is the result of continuous enactments of different agents seriously, I suggest that the modelling approach I propose is one of the more interesting ways forward. Obviously, modelling continuous enactment based on actions by many agents is far from trivial—and will still not include all agents—but at least is consistent in terms of theoretical and methodological concepts. My approach has the added benefit that it offers the same type of agency to Roman agents as we allow ourselves in our own time: not too much perhaps, but certainly not just predefined by our successors.

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