Slightly revised for *Environmental Modelling & Software*, 120, 2019

# Decision-making in model construction: unveiling habits

Lucie Babel* [a], Dominique Vinck [b], Derek Karssenberg [a]

[a] Department of Physical Geography, Faculty of Geosciences, Utrecht University, 3584CB Utrecht, the Netherlands, l.v.babel@uu.nl and d.karssenberg@uu.nl

[b] Institut des Sciences Sociales, Faculty of Social and Political Sciences, University of Lausanne, Quartier UNIL-Mouline, 1015 Lausanne, Switzerland, dominique.vinck@unil.ch

*Corresponding author: l.v.babel@uu.nl (L. Babel)

## Abstract

The first stages of the modeling process remain thus far poorly studied, in particular the modeler's decision-making in regard to the inclusion and representation of environmental phenomena within the model. Anchoring itself in the theoretical and methodological framework of Science and Technology Studies, the present study aims at identifying and retracing the origin of the modeler's choices. Extensive, semi-constructed interviews were conducted with European and North American modelers in a variety of disciplines within Earth and Universe sciences. The outcome of the interviews sheds light on the role of different actors in transferring choices progressively incorporated in the modelers' practice, up to becoming self-evident. We introduce and engage the sociological concept of habits as an additional layer of analysis. Modeling habits can contribute to efficiency and expertise – provided their origin, epistemic load and implications in regard to collectives are assessed.

Keywords:

Model construction, model structure identification, decision-making, sociology, habits

## 1. Introduction

The modeling community aims at sound modeling practices. The ubiquity of numerical models (computer simulation models of processes occurring within a system) in scientific research and their use for decision-making processes, framed in a context of political and public defiance, has led environmental modelers to pay increasing attention towards enhancing the credibility of their models. A vast number of studies have been devoted to the assessment of model uncertainty (Refsgaard et al., 2007), notably through sensitivity analyses, error-propagation calculations or multi-modeling approaches. Climate science has witnessed an almost exponential rise of model intercomparison projects (Eyring et al., 2016), a nowadays spreading practice in various disciplines (e.g. Kim et al., 2018; Krysanova et al., 2017; Rosenzweig et al., 2013; Thielen et al., 2008). In parallel, the evaluation of model performance remains a sensitive topic academically and when collaborating with stakeholders. Not surprisingly, it has triggered extensive discussion in the modelling community. Numerous studies can be found on approaches to model evaluation (see e.g. Bennett et al., 2013; Harmel et al., 2014), its philosophical relevance (Oreskes et al., 1994) and terminological and epistemological alternatives (Rykiel, 1996; Beven and Young, 2013; Augusiak et al., 2014).

The above-mentioned body of literature focuses primarily on the last stages of the modeling process. Uncertainty assessments, model intercomparison and performance evaluations are generally realized after the construction of the model. In comparison, the initial steps of the modeling process – including the selection of processes to be represented, their associated equations, software etc. – are far from having been addressed as closely. In the context of water management and environmental decision-making, some authors have called for more global consideration of the modeling process from start to finish, in the aim to establish good practices and to head towards quality assurance of models (Grimm et al., 2014; Jakeman et al., 2006; Refsgaard et al., 2007). Sound modeling practices, after all, shall not be restricted to a particular stage of modeling.

In this perspective, the present research precisely focuses on the least studied steps of the modeling process: the very initial steps of the model building activity, which are just as likely to impact the "end of the pipe" product as later stages. When reflecting on the practices of modeling, a striking aspect of model construction lies in the number of decisions that have to be made by the modeler or his group. Beck and Krueger (2016) related the existence of these choices to the scientific uncertainty inherent to environmental modeling[1]. Most natural processes and their interactions are far from being theoretically completely understood and hence do not enable scientific determinism. Additionally, a model always amounts to a simplification of reality; while some natural processes and interactions will be chosen to be integrated in the model, others will be disregarded. But even when a particular natural process has been selected, the modeler may still face a wide range of representations to choose from (Guillemot, 2010). Indeed, many different approaches to the inclusion of one and the same process can generally be contemplated. Which process representation is selected significantly influences the model behavior and the modeling outputs. We asked ourselves on which basis

[1] In the case of integrated assessment modeling, Beck and Krueger (2016) distinguish scientific uncertainty from ethical uncertainty, the latest being associated to the different possible approaches to address ethical implications of climate change.

the modeler does, then, select one of the representations of the process under consideration – and not another one.

We can think of several explanations, which are not new to the modeling community. Jakeman (2006) and Radchuk (2016) extensively showed how model objectives and available data do – and should – constrain model construction. Existing paradigms and theoretical accounts also guide the choice of particular representations or suggest relationships between different model variables (on the intricate relationship between models and theories, see for example Morgan and Morrison, 2010 and Winsberg, 2010, 1999). The same applies to technical constraints. The modeler necessarily has to make choices based on the limitations of available software and computational power. While these explanations were handled in the modeling literature as external factors, discussion emerged on their internal (psychological, ethical) counterparts. Beven (2008, 1991) and Westerberg et al. (2017) engaged the concept of perceptual models, encompassing the modeler's personal representation of the hydrological system or of uncertainty. Azadi et al. (2017) investigated the philosophical paradigms underlying land use change models. Van der Sluijs (2002) and Krueger et al. (2012) stressed the subjective and implicit nature of modeling assumptions. Voinov et al. (2014) advocated appropriate assessment (and communication) of values of both modelers and users during the process of modeling. Drawing from operational research, Hämäläinen (2015) shed light on possible behavioral issues related to the modeler, such as cognitive and motivational biases, while Lahtinen et al. (2017) insisted on the path-dependency of decisions in model construction.

We however identify a rarely studied link between the seemingly external (theoretical, data-driven, technical) and internal roots for decision-making in the process of model construction. Indeed, the modeler has seldom been considered as a social actor within networks. A notable exception concerns the interface between the political, decision-making spheres and modeling, which has been approached among others in the context of climate modeling and integrated model assessments by Shackley et al. (1999) and Beck and Krueger (2016). The two studies insisted on the mutual construction of modeling and policy. The former displayed how the modeler's implicit view of the policy process is likely to influence his epistemological stance and his modeling practices, while the latter highlighted the political and ethical implications of each modeling choice. Addressing more broadly the embedding of water science research in its social context, Krueger et al. (2016) presented an account of the coproduction of knowledge by experts and non-certified expert stakeholders and citizens. While these studies have contributed significantly to the understanding of interactions of modeling practices and policy, the modeler also acts within scientific networks – which has been studied less extensively. However, she is part of a group within an institute or a research department, attached to an institution. She was maybe member of other groups before. She has been interacting with other individuals in and outside of her institute for years and became part of an international network. She reads, meets, discusses, presents, submits, defends, reviews. Prior to that, she was educated in a particular University or in several ones, where others have taught and supervised her. How could this not influence her modeling? Within Science and Technology Studies, Krueck and Borchers (1999), Shackley (2001) and Sundberg (2010) approached institutional and disciplinary cultures of modeling in climate science and astrophysics. Modelers, so we think, are not unaware of these aspects playing an important role in model construction. The existence of modeling “schools” peculiar to certain Universities, for instance, or differences in modeling approaches depending on the institute’s country of origin, are commonly – yet informally – discussed.[2] The problem? We fundamentally lack empirical studies that enable us to better understand the social construction of environmental modeling. Amidst the plethora of best practice modeling guidelines, we miss data on how modelers (the individuals themselves) actually make decisions when building a new model. We lack observations and based upon them, analyses and conceptualizations which could help the community to reflect on existing practices. Consequently, Lane (2014) called for a constructivist exploration of the modeling process. Beck and Krueger (2016) regretted the absence of “empirical investigations on how modeling choices are made in practice”. These empirical studies are required to pave the way for increased reflexivity on implicit and explicit mechanisms within model building, which are likely to significantly impact modeling outputs.

The present research is a first step in this direction. Anchoring ourselves in the theoretical framework of Science and Technology Studies, we pursue the objective of identifying the origin of modelers’ decision making in model construction, conceptualizing it and discussing its relevance and implications. To this end, we employ an established methodology from social sciences based on extensive, semi-directed interviews conducted with scientists constructing models in a variety of disciplines within Earth and Universe sciences, many of which with high relevance for current societal issues related to climate change. We consider choices related to representations of natural processes, through time transfer functions and numerical schemes. This methodology is presented in Section 2 along with its epistemological justification. We then explore the obtained material in Section 3 and propose a conceptualization of the observed mechanisms of transfer and incorporation of choices. The sociological concept of habits is then introduced and discussed as an additional, suitable layer

[2] A recent exception consists of the text mining research of Addor and Melsen (2019), who highlight regional preferences for the choice of hydrological models.

of analysis. The relevance and implications of this concept for the modeling community are accordingly discussed. Section 4 summarizes our main findings.

## 2. Methodology

The present section introduces in subsection 2.1 and 2.2 the epistemic stance adopted in the research and its justification. The general design of the research, the selection of the interviewees, the interview grid and considerations on anonymity are addressed in subsections 2.3 to 2.5.

### 2.1. Modeling choices

Decision-making in model construction encompasses a high number of choices made by the modeler, sometimes in an iterative process. It should be noted that while the existence of these choices might appear evident to the modeler, they have long been overlooked by philosophical accounts, which considered models to be fully derived from theory or observations (see the analysis of Morgan and Morison, 1999), suggesting a form of scientific determinism. However, the modeler needs to select the temporal and spatial scale at which to simulate the system at stake, as well as the desired type of output. He or she has to choose the processes which he or she wants to incorporate; their interactions; their representation through physical equations or parametrization; their numerical implementation; the source of data; the programming language in which to code the model; the hardware and software at use. Many other choices are already blackboxed into the existing tools and infrastructure which are used (e.g. programming language, software, data sets, mathematical formalism, etc.). This study does not address decision-making in its globality, but explicitly chooses to focus on one of these choices. The choices considered herein concern the representations of natural processes, through time transfer functions (model equations representing processes over time within the system considered) or numerical schemes (the numerical solutions used for time transfer functions). Table 1 specifies whether time transfer functions or numerical schemes were addressed in the interviews. We concentrate hence not on the choice of processes to incorporate, but on the representation of these processes. Furthermore, the focus lies on decisions made by individuals (the model builders) related to a single model equation or component.

### 2.2. Methodological stance

Identifying the root of decisions made during the modeling process is not a straightforward task. As pointed out by Guillemot (2009) and Sundberg (2010), the model building activity of a researcher can prove particularly opaque to analyze through direct, ethnological observation, one of the dominating methodologies of Science and Technology Studies since the pioneering work of Latour and Wolgaar (1979). Such an undertaking could provide relevant data, as shown by Jaton (2017a, 2017b) for the neighboring domain of algorithm construction. However, addressing the origins of decisions in model building through direct observation would require limiting oneself to decisions that are verbalized (through discussions with colleagues, notes and alike) during the process itself. In order to enlarge one's analysis to also encompass the substantial number of decisions which do not leave traces during the process, one necessarily has to rely on a posteriori verbalization. Words hence, but which words? A rather straightforward approach would consist of basing one's analysis on written words only, studying the literature accompanying the release of a model. Isolating the reasons behind modeling choices could then amount to a text analysis. However, the format and the purpose of the document (most notably peer-reviewed articles) deeply reshape the narration (Rinck, 2010). In his work on the IMRAD (Introduction, Methodology, Results, Analysis and Discussion) format, Pontille (2007) insists on the recomposition of the temporality of research activities that this format implies: "the interweaving and the superposition of research operations, as they appear in the "real" path of empirical investigations, are masked" in favor of a linear narration that necessarily omits, selects, cuts, flattens and impersonalizes (objectivates) the course of actions. Moreover (and partly as a consequence), far from all decisions are being documented in articles, technical documentation, or user manuals. Authors will detail the process representation they used in the model, but not necessarily explain the justification of this choice. We do not believe that this lack of justification represents a conscious omission. Rather, it could indicate that the choice of this representation is a stabilized practice in the community, so common that neither the authors nor the possible reviewers deem any justification necessary.

It is hence not the presence of words in written material, justifying choices, which forms our starting point – but their very absence. On the basis of articles or manuals, we would be uncapable of retracing the origins of the modeler's choice. Another source – another type of verbalization – is required. Oral narration, obtained in face-to-face meetings with the modeler herself, can be just this other source. Of course, in this case too will we not (no more than with written material) pretend to have access to the intangible reality of the highly complex intra- and interindividual mechanism of decision making. Oral words too are chosen; facts can be modified, recomposed or eluded. Yet interviews, through their dynamical and spontaneous nature, enable a constant adaptation to the discourse of the researcher. There where silence reigns, questions can be asked upon; there where words flow, these can be collected with no "maximum number of words" limit which would restrict the level of detail of the information retrieved. Interviews have hence been a privileged methodology of Science and Technology Studies scholars when exploring various facets of modeling (see for example Daniel (2018) on odor dispersion models, Lahsen (2005) and Guillemot (2009) on Global Circulation Models, Sundberg (2010) on models in astrophysics and meteorology, Landström et al. (2011) on modeling in flood risk management, Kouw (2016) on hydraulic models). An abundant literature has been devoted to the methodology of interviews in qualitative research. One can refer to the work of, among many others, Gubrium and Holstein (2001), Seidman (2013), Kvale (1996), Weiss (1995) or Spradley (2016).

### 2.3. General design

We chose to conduct semi-directed interviews, one of the most widely used methods of interviews in social research (Quivy and Van Campenhoudt, 1995). Semi-directed interviews are based on an interview grid (detailed at the end of this section), which is designed prior to the interviews but does not consist of a constraining listing of questions. The “semi” prefix of the method indicates that the guiding of the interview is constantly re-adapted by the interviewer to the course of the interview (see Mason, 2004). Semi-directed interviews predominantly make use of open questions, which call for narrative or argumentative content. Priority is given to the flow of the interviewee’s discourse.

### 2.4. Interviewees

We decided not to limit ourselves to one scientific discipline when selecting interviewees. We assume that numerical modeling practices in Earth and Universe sciences share common issues (or puzzles, in Kuhn’s understanding of the word; Kuhn, 1962), related to the representation of natural phenomena and their temporal and spatial scales. These issues, such as the trade-offs between the simplifications necessarily required and the appropriate level of detail for the modeling projects’ objectives, as well as the discretization of time and space, are embracing the disciplines well beyond their traditional frontiers. While the answers developed to tackle these issues can be of a specific nature, we hypothesize that the process of decision making itself reaches a more generic level. Testing this hypothesis implies to explore the internal and external variability of modeling choices, and led us to interview modelers from different scientific disciplines, many of which with high relevance for current societal issues. Are modelers from various disciplines taking similar aspects into account when selecting representations of processes? Can we identify common threads beyond disciplines and specific models? We consequently conducted interviews with modelers in astrophysics, biogeosciences, ecology, geochemistry, geomorphology, hydrology and hydrogeology.

Our focus on the roots of decision-making led us to select researchers who actually developed the model, from the earliest stages of the modeling project on. Additionally, as the starting point of our reflection was based on the absence of justifications in accompanying written material, we decided to consider models which had already been presented to the community through peer-reviewed articles. An uncomplicated way of choosing interviewees could then have consisted of singling out, in our direct academic accountancies, the modelers who satisfied these criteria. Yet the risk of remaining in one and the same network of researchers would be latent. Not that we aimed for exhaustivity – an irrelevant Grail whenever the complexity of human thoughts, actions and interactions is at stake – but we did want to account for some of the diversity in Earth and Universe sciences. Except for two, none of the interviewees did hence belong to any of our professional or academic networks. The interviewees were selected on the basis of their models, which we identified as suiting the diversity we wished to attain in the types of models considered (see section 2.5).

The results presented in section 3 are predominantly based on the content of seven of the interviews. A total of fourteen extensive interviews were however conducted, encompassing researchers involved in different stages of the modeling process. While these interviews are not explicitly considered in the analysis, they formed an additional background which enabled to test the interview grid as well as to prolongate and investigate the modelers' discourses. The seven interviewees under consideration came from five universities or research institutes, located in four different countries in Europe and North America. Five of the interviewees were now working at a different university than the one where they had obtained their PhD, four of these in a different country. Except for one postdoctoral researcher, the interviewees were senior scientists or professors.

### 2.5. Types of models

An overview of the natural systems simulated by the models of the interviewees, as well as their spatial scale, can be found in Table 1. A diversity in the types of models (from models openly presented as simplistic up to highly complex ones) was sought across disciplines. Some could run on a laptop, while others required access to national supercomputing centers. All interviewees constructed the model during their PhD or as young postdoctoral researchers, except for two researchers whose models were constructed in cooperation with others in later stages of their careers. Yet all interviewees who had started constructing their model more than five years prior to the interview were still actively developing it; the models at stake represented a significant part of their current professional activity. All models are nowadays used by groups outside of their institute and country of origin. Four of the seven models can be considered as highly prominent in their associated field of research, yielding important yearly literature and for two of them, annual or bi-annual international user conferences are held.

Table 1 – Overview of the disciplines of the interviewees and indications on the modelled systems

| Interviewee | Discipline[1] | Simulated systems | Spatial scale | Subject considered during the interview | Alternatives (as perceived by the interviewees) |
|---|---|---|---|---|---|
| A | Geomorphology | Wetland gas emissions | Local | Time transfer function[2] | Other equations with increased level of detail |
| B | Hydrology | Hydrological cycle | Global / local | Time transfer function | Other equations with reduced or increased level of detail |
| C | Geochemistry | Carbon cycle[3] | Global | Time transfer function | Other equations representing different processes with increased detail |
| D | Hydrogeology | Surface and subsurface hydrological cycle | Global | Numerical scheme | Other numerical scheme with reduced physical significance |
| E | Ecology | Vegetation dynamics | Local | Time transfer function | Other equation with increased level of detail |
| F | Astrophysics | Galaxy formation | Universe | Numerical scheme[4] | Alternative scheme[4] |
| G | Biogeosciences | Biosphere[3] | Global | Set of time transfer functions | Own representation |

*[1]The disciplines correspond to the main one of each interviewee; in the case of interviewee A, the genesis of the model (displayed in section 3.2) led him to model a system generally associated to another discipline. [2]Equations representing processes over time within a system. [3] These models are coupled to a global circulation model. [4] Both numerical schemes had strong implications on the representation and physical behavior of hydrogen within the model.*

## 2.6. Interview grid

Prior to the interviews, the interviewees had been contacted via e-mail and been solely informed of the general theme of the interview (the construction of models in their field), its purpose (a sociological research) and the geoscientific background of the interviewer. They did not have access to the interview grid nor to the research objectives. The interviews, all face-to-face, were conducted by the first author at the working place of the interviewee. Four of the interviews were conducted in French (the mother tongue of these interviewees), the remaining ones in English. The interviews were open-ended, but generally lasted from one to two hours. The literature surrounding the models had been carefully studied by the interviewer in forehand, enabling precise questions about specific components of the models. Due to the nature of these semi-directed interviews (open questions giving priority to the interviewee's discourse), this duration almost equated to the actual speaking time of the modeler and warranted in depth narratives.

The interviews started with an introductive question about the general context and the reasons that lead to the construction of the model. This apparently trifling question pursued a double objective: put the interviewee at ease by enabling him to start with a biographic narrative, and shape a general understanding of the base of the construction of the model. Would the modeler engage theoretical, technical (related to data, hardware or software) or societal issues in his narration of the model's origin? How much would the modeler intertwin his own (academic) biography in the initial threads of the model's story? Would he mention colleagues or supervisors, and if so, which role would he grant them? What place would other, pre-existing models take in his explanations? In some cases, this introductive question could lead to up to twenty-five minutes of uninterrupted narration. Depending on the elements that surfaced, additional questions were asked to clarify or obtain information on parts of the interviewee's story. This introductive question granted a common ground of discussion and enabled the interviewer to move on to questions on more detailed aspects of the model.

A second set of questions focused on a particular component of the model, which could either be a time transfer function representing a natural phenomenon or a specific numerical scheme. The time transfer function or numerical scheme had been explicitly chosen by the interviewer because the accompanying literature did not enable one to identify the reasons of its selection by the modeler. Through her questions, the interviewer attempted to obtain enough information enabling her to retrace the origin of the component. We should point out here that we did not strive for the historical origin of the component itself, in the manner of MacKenzie (2003) in economics or Lane (2014) in hydrology; instead, information was sought on the origin of the component in the modeler's experience. By answering the general "why did you use this component?" question, the interviewee was naturally encouraged to cover the content of other, unasked questions. Was it a component that the modeler had already been using in earlier work? If so, why did he use it in the first place? Was he asked or advised by others to implement it? Did he use other alternatives before or afterwards? These aspects were often entangled by the interviewee in an argumentative net, leading him to detail the added value that he perceived in the use of this component.

The interviewee was then asked about the reception of the model by the community. This question would of course reveal more about the modeler's own perception of the reception – and about how willing he was to report on criticism to a stranger. Yet shifting the focus to an external point of view (the point of view of others on his modeling work) could prove useful to assess how the modeler regarded his own community. Would he place himself in opposition to particular criticism, referring to modeling groups inside of his discipline? Or would he justify alternative points of view on his work? The interview was then concluded by a reverse question – focusing this time on his glance on the modeling work of others. The interviewee was asked what he generally deemed necessary in order to assess the quality of a new model, which he would read about in a peer-reviewed article. What type of justifications or explanations would he require? Would the use of particular components be considered unavoidable? Would certain approaches be favored? If so, why? This wide question, the only of its kind in the interview as it did not focus on the interviewee's model, enabled the interviewer to approach the interviewee's perception of the modeling activity from yet another angle. Time was left at the end of the interview for the interviewee to add whatever he felt had been missing during the conversation.

The interviews were recorded and entirely transcribed – the equivalent, for the interviews considered for the content analysis presented in the next section, of an 80'000-words document.

### 2.7. Anonymity

The names and affiliation of the interviewees quoted in section 3 remain undisclosed. The interviewees are referred to through single letters, from A to G. Their field of research can be looked up in table 1. Along with the description of the system simulated by the model and its spatial scale, we deem these indications necessary for the reader to picture himself the types of models at stake. It should be noted that we do not conceive the interviewees as spoke-persons of their disciplines, representing the epistemic stance of a complete group. Neither do we intend to shift the emphasis of the present research on a hazardous comparison among disciplines, for which our methodology was not designed. However, exploring the disciplinary diversity enables to question the origin of influences on modeling choices, as arising from field-specific differences regarding the modeling content or from inter-personal dynamics.

## 3. Results and discussion

This section is divided into six subsections. We shall first assess the implementation of the method (section 3.1), before continuing with the content analysis. The interviewees' accounts of the origins of the construction of their model are displayed in subsection 3.2. Subsection 3.3 presents and discusses the interviewees' explanations of the choice of particular representations in their model. These findings are conceptualized in subsection 3.4 and framed within existing sociological accounts in section 3.5. Section 3.6 explores the implications of the employed conceptualization for existing practices.

### 3.1. Epistemic position

The results of this study - just like any outcome from a field research, whether in the disciplines under consideration or in sociology and anthropology - are highly dependent on the methodological set-up employed. We were aware of the fact that the detail of the answers, the degree of introspection, the readiness to open oneself up to the questions of a stranger would derive from the interviewee's perception of the research. In this respect, we were surprised by the interest the interviewees displayed in the study and the confidence they placed in the interviewer; the discussions proved considerably more open and franker than we would have expected. Part of it can probably be explained through the epistemic position of the interviewer. Even though the interviewees had been informed of her original background in environmental sciences, they regarded the interviewer as a social scientist. She had not carried out extensive research in their fields, was nowadays anchored in the domain of Science and Technology Studies and hence not perceived as a rival. While the risk of facing overly controlled and contained answers vanished, that of hovering at a too general level was looming. In many of the interviews the modelers were tempted to explain their field of study to the interviewer as to a layman. She then had to demonstrate that the discussion could engage on more detailed aspects of the modeling process. The turn was generally reached when the interviewer started asking questions on a particular component of the model, using the modeler's article as reference for the discussion. The most manifest illustration of such a tipping point appeared in an interview conducted with a modeler who, straight after the interviewer quoted the work from a scientist of the same field, expressed his surprise and immediately dropped the formal French "vous" for the colloquial "tu" (you). The interview could then continue on a more detailed and technical basis. The position of the interviewer might hence be considered as two-fold, simultaneously in and outside the domain of study; it guaranteed both a distance and a proximity to the interviewee which could explain some of the density of the collected answers.

### 3.2. Narrating the origins of the modeling process

The methodological choice to use oral narratives as a source stemmed from the assumption that scientific articles exert significant reshaping and cutting out of the course of events. When they were asked about the general context and the reasons that lead to the construction of the model, the interviewees' narratives were indeed convoluted, drifting away from the motivations reported in their academic research article(s) on the model considered. The geomorphologist A, whose article strongly underlined the drawbacks of pre-existing models (high computational demands or predictive uncertainty) as a justification for the model's construction, immediately weakened the importance of the role these drawbacks had actually played in the modeling process:

> "That's more to…that's very common when you write a paper about modeling… you show the strength of the other models (…) but then this is the limitation. And you continue to do that for several models and your model is trying to address those limitations, it's almost like selling the model, so why do we need this model, you know. (…) So that's usually how I phrase all my papers."

The reality – or the interviewee's narrative about the historicity of events – proved more complex. Rather than involving other, preexisting models, the interviewee assigned significant emphasis to the role of individual actors and coincidental encounters. The professor who interviewee A was working with, anchored in fluvial geomorphology, indeed met a "field person" from another research area – wetland sciences – during a meeting. This wetland scientist told him about an erratic phenomenon they had observed in the field, which seemed hard to predict. The professor, "being a modeler", could see the link between that phenomenon, which he had not previously worked on, and a modeling technique based on cellular automata that was "quite well known" and had "a bit of popularity" in his own field - geomorphology. He subsequently told his conversation partner that a model could possibly explain this behavior and instructed interviewee A to build the model.

> "… so he came back to the office and he explained to me this whole dialog he had and he said: Hey, would you like to build it? On your free time? I said, sure, why not? (…) So… that's how that started."

The role of interviewee A then amounted to code the model according to the method identified by his professor – but also, in a later stage, to squeeze the model's intricate, coincidental genesis in the mold of scientific writing. Several actors were indeed intertwined in interviewee's A narrative from the very beginning on, yet entirely replaced within the article in favor of a posteriori rationalization: a particular environmental issue leading to a research question, the drawbacks of existing models to tackle it leading to the necessity of this innovation, etc. The puzzle encountered by the wetland scientist, his fortuitous discussion with the professor in geomorphology, the community within which the modeling technique was "quite well known" and to which the professor belonged, however all played a substantial role in the commencement of the modeling project. These first answers on the general context of model construction – of which, as we will see below, interviewee A's narrative seemed rather representative than outlying – appeared to confirm our methodological stance to go beyond what could have been deduced from scientific articles only. Indeed, before even approaching during the interviews particular choices in the representation of natural phenomena, the very rationale underlying the construction of the models already appeared to unfurl rich and manifold interactions between individuals, collectives, institutions and their sociotechnical networks. The narrative of interviewee B is a further example in this regard. As a hydrologist, interviewee B was hired in his current university to build a global hydrological model. His director had identified the construction of a global model as a “niche”, a decision interviewee B refers to as “strategical”.

“…the only question that we had was, we had to build a model and… yeah, that was it, there was no direct scientific question (…) other than just getting the model going, so… [laughs]. Yeah, so it was a bit of a strange deal…”

Constructing a new model rather than using a pre-existing one could prove profitable in the long term. Interviewee B reported that while no direct application was determined, the model would contribute to establish an innovative orientation of the research group in hydrology: its existence would open up collaborations with other groups and funding opportunities, as well as enable to attract researchers whose expertise would be durably beneficial for the institute. With the construction of the model progressing, investments were made in technical infrastructure (high performance computing devices), which significantly increased over time. A growing number of researchers were involved in the development, maintenance and use of the model, contributing to its positioning on the international scene and to the constitution of a “geocomputational group” within the institute. The model was thus an actant in structuring and stabilizing a network of scientists, equipment and computing machines in the institution (on the structuring role of objects in scientific practice, see for example the work of Latour, 1979, 1993; and Vinck (1999, 2011, 2012). Far from being unique, these motivations were echoed by other interviewees. Hydrogeologist D reported on funding opportunities related to societal issues on waste disposal steering the construction of his model; geochemist C and biogeoscientist G involved strategical orientations of their university or institute in their models’ genesis, which were coupled to a Global Circulation Model, while astrophysicist F underlined competition against other groups (see section 3.4.) as motivating the rapid development of an efficient and innovative alternative for the simulation of galaxy formation. These first answers highlight the impossibility to separate the initial impulses to the construction of the models from their collective and institutional context of emergence, which co-shape them along with their designed scientific objectives. Having now elaborated the motivations of the construction of models, we will delve into the details of the modelers' choices in the representation of natural phenomena in the next sections, aiming to approach this collective co-shaping at closer sight.

### 3.3. Explaining choices in model construction

The interviewees had constructed either all (for the majority of them) or substantial parts of the first version of their model themselves. Yet in the second part of the interview, where the interviewees were invited to explain precise modeling decisions, all modelers were quick to involve many other actors in their narratives. The unfolding of actors in the narratives was of two kinds. The actors were either individualized, identified and named by the interviewees (a particular person), or diffusely mentioned (a collective). These two different cases will be treated hereafter in section 3.3.1 and section 3.3.2. It should be remembered that no question had been asked on these actors. The general question, as presented in section 2.6, remained that of why the interviewees had chosen this particular transfer function or numerical scheme to represent the natural phenomena at stake. We expected answers involving mainly technical constraints, associated to hardware and software limitations; issues related to data availability; results of comparisons between the application of several alternatives and observational data; successful preceding experience with the implementation of the equation or numerical scheme. The spontaneous, immediate involvement of other actors in the answers surprised us.

#### 3.3.1. Involvement of individual actors and epistemic stances

Whenever the interviewees engaged individuals in their narrative, their role was explicitly delineated, often chronologically. Hydrogeologist D, for example, whose model simulated the hydrological cycle of the Earth's surface and subsurface, explained that he attended courses by a professor member of his thesis committee, a person he deemed "really brilliant" because of his knowledge of numerical methods and with a "very, very practical mind" due to his past in industry. This professor had used the method in one of his models and taught it in his course; the interviewee reported to have decided "straight away" to use the method, which became one of the distinctive features of his model[3]. Interestingly, it is only after having mentioned his professor in his narrative that interviewee D started to list technical reasons for the implementation of the method – efficiency, stability and rapidity. These characteristics, which were "advantages" compared to "what most people were using at that time", indubitably played a paramount role. Yet the intellectual and biographic authority of the professor stood at the forefront of the interviewee's justification of the use of the method. We will not engage on a

[3] In this case, precising the type of numerical method employed would be likely to reveal the identity of the modeler. We hence keep it undisclosed.

hierarchization of the actual influence of each aspect (the intellectual and biographic authority of the professor, versus the advantages of the use of the method for computing purposes and for departing oneself from groups competing for the same sources of funding) played in the decision. Oral narratives let room for a posteriori rationalization too: the interviewee might have wanted to emphasize, through highlighting his influence, the gratitude towards a professor he considered as his mentor; or reversely, justify the use of the method by listing its advantages. However, the fact that the interviewee, from the very beginning of his answer on, spontaneously involved another actor in his justification, referred to his own education and opposed himself to other groups, enables us to retrieve first glimpses at sociological aspects of decision-making invisible in the accompanying literature and rarely analyzed in the modeling context. Members of the thesis committee – most notably the thesis director himself – were the individual actors that most frequently appeared in the interviewees' narratives about the choice of model components. Colleagues (though often referred to generically and less precisely identified) were involved in the discourse too as acting on modeling choices. For example, ecologist E explained how his local model of vegetation dynamics was at first "quite complicated" until a colleague of his department "forced [him] to make it as simple as possible", leading him to discard terms in the time transfer function describing plant density, selecting linear functions and removing any differentiation between plant components. He briefly reported on the discussion with his colleague as a series of repeated questions which initiated a change of perspective on his own approach:

> "So he [referring to his colleague] said why do you need that [points at one term of the transfer function], why do you need that [points at a second term of the transfer function], why do you need that…"

Ecologist E consequently parameterized or suppressed the terms of the transfer function the colleague had addressed, while they had been physically represented before. The colleague probably never exerted an actual (external) constraint to parameterize or suppress these terms. However, the use of the verb "force" (in "forcing [him] to make it as simple as possible", stated above) is interesting in this regard because of the externality of decisions it suggests – in the same way that data scarcity or computational limitations are often described in literature as constraining the choice of representations. Here, data and computational power appeared to be available. The suggestion of his colleague, which ecologist E reported about as a decisive bottleneck in the development of the model, seemed to be rooted into considerations of epistemic nature. Interestingly, while ecologist E seemingly easily accepted the input of his colleague, he rejected comments of the reviewers of the first journal he submitted the manuscript on his model to, which deemed it too simplistic – just like the comments of hydrologists who called for a more explicit representation of overland flow within his model.

Conversely, the interviewee modified the (nowadays widely used) model towards even greater simplicity with one of his doctoral students in later stages of model development. Simplicity was repeatedly advocated in the course of the interview as a form of epistemic virtue to which the interviewee was increasingly tending over his career. While he had already built “very simple models” during his PhD, he had started to complexify them after it; the encounter with his colleague appeared to have resonated with his epistemic stance and brought him back to an approach of modelling which he nowadays strongly identified himself with. His colleague’s questions, we could interpret, had steered the representation towards their shared epistemic stance. The reviewers’ and hydrologists’ comments, distant from it, had been disregarded.

Ecologist E was the interviewee who most clearly established a link between his modeling choice and his and his colleague’s epistemic stance; however, five of the remaining six modelers also spontaneously alluded during the interviews to what they frequently defined as their “modeling philosophy”. The consistent and repeated use of the term “simplicity” in ecologist E’s discourse could be linked to the simplicity/complexity dichotomy being often discussed within the discipline (e.g. Peck, 2004, and Merow et al., 2014), simplicity having historically been a well-established epistemic view on modeling in ecology (see the analysis of Evans et al., 2013). The strong discipline-specific past discussions on “process-based” (or “physically-based”) versus “conceptual” modeling (see for example Freeze and Harlan, 1969, Beven, 1989, 1993, Grayson et al., 1992) could also explain hydrologist B’s and hydrogeologist D’s up front framing of their models within one of the two approaches at the very beginning of the interviews. Noteworthily, the sociotechnical implications of these epistemic views on modeling appeared to be very different from one discipline to another. Describing his cosmological model as being of “the discovery type” (as opposed to models designed for forecasting purposes), astrophysicist F appeared very close to ecologist E’s and geomorphologist A’s approach when he stated that “in this discovery mode (…) we have to simplify the equations up to the extremes, you should not get troubled by… a whole bunch of thorny considerations”. Yet the geomorphological and the ecological “simple” models required only a few lines of code and very limited computational time, while the “simple” cosmological model was run on a national supercomputing center and demanded extensive numerical solving techniques. While the dichotomy between competing epistemic views seemed very common across disciplines, the underlying implications appeared discipline-specific. These epistemic dimensions of modeling surfacing in the interviews connect two social dimensions of the origin of modeling choices: the inter-personal one, which we outlined in this section, and the collective one, which will be treated into more details in section 3.3.2.

### 3.3.2. Allusion to collectives

Beside the role of individual actors on modeling choices and epistemic dimensions of modeling being amply mentioned, modelers recurrently referred in their narratives to collectives. This case was most obvious for geochemist C, who incorporated in his answers his institute or the entire community of scientists from his field of study. His model, which simulated the distribution and evolution of greenhouse gases and aerosols within the atmosphere, was coupled to a Global Circulation Model (GCM) and regularly participated in international model intercomparison projects, which he repeatedly made mention of. The interviewee seemed not to perceive his model as a singularity, but as one representative of a set of models which all pursued an identical objective. Accordingly, whenever the interviewee started his sentence with "this model", he automatically corrected himself and replaced the words by "each model". The wall of collectivity could not be broken: his practices were that of an entire group which he very coherently identified himself with. This represented a peculiarity in comparison to the other interviews, in which the modelers all started a case-specific narrative. A possible explanation could lie in geochemist C's research field. The modeler emphasized how in his domain, the ensemble median forecast obtained from model intercomparison projects – and based on forecasts from a set of different models – is considered to yield better results than any of these models alone. What seemed to be sought for was not the one model displaying outcomes in best accordance with observations, but a diversity of models that enabled to represent the scientific uncertainty at play in climatic processes. The fact that his model simulated particular aerosols such as desert dust with increased physical accuracy (while focusing less on marine salts, for example), was hence deemed by the interviewee as serving the community by increasing the diversity of representations. This collective endeavor was particularly striking in geochemist C's narrative and could not be explained by his participation to model intercomparison projects alone. Indeed, biogeoscientist G's model – just as geochemist C's – was also coupled to a GCM and took part in a high number of intercomparison projects too. Yet interviewee G did not adopt a similar, collective mode of narration: while frequently mentioning and comparing his approach to that of other models, he dove into its singularities and its individual genesis. A main difference between these two cases may reside in the strong structural and epistemic divergences between biogeoscientist G's model (which simulated the phenology and carbon cycle of the biosphere) and the alternative ones; geochemist C appeared to find these divergences less prominent in his own discipline, which facilitated his identification with an entire set of models.

While geochemist C's answers represented an outlier through his constant collective mode of narration, collectives were not absent from the other interviews. These collectives were loosely and atemporally called upon: they were utterly generic, in a striking opposition to the explicit mention of individuals reported about in section 3.3.1. Hydrologist B described an equation he used to represent evapotranspiration within his global hydrological model as being “a kind of hydrological practice”, “more or less the standard”, adding later on that “if people have the data, they will then use [it]”. Here, data availability appeared to act as a first filter among alternatives, excluding representations that required data (for example root fraction and canopy resistance) the modeler did not possess; the extent of their use within the community – or, more precisely, within the collective sharing a similar epistemic stance – acted as a second one. While explaining his choice of another transfer function to represent snow melt and snow accumulation, he outlined a general approach to component selection:

> “…one of the first things that you do when starting [to build a model] is that you start looking at (…) what are the typical components of (…) models that exist. And [this component] is I think one of the most well-known…”

This thought was rephrased in a later stage of the interview:

> “…but often of course you look at yeah, what is a kind of consensus within hydrological practice and that, that’s your starting point.”

The description of a representation as “standard”, “typical” or as belonging to a “consensus” or disciplinary “practice” was echoed by geormophologist A, who considered the cellular automaton representation his professor had chosen to have “a bit of popularity” in his field, or by biogeoscientist G, who reported that “everybody, almost all models use [this representation of photosynthesis], which is a bit the one of reference”, even though alternatives existed. Ecologist E described a time transfer function he used for plant growth as “very common”, “typically used for that”, and five other parts of time transfer functions as “textbook”. He seemed surprised to be asked about the origin of the implementation of these representations which he precisely considered to be so common. Yet this apparent triviality was exactly what we aim to scrutinize. The common, typical, popular or well-known nature of the representation suggests the absence of choice: the representation imposes itself through its wide use in the community. In the eyes of the modelers, the choice of that representation is hence well-nigh not perceived as one. A choice was still made, since alternatives existed in each of the cases detailed above; but the interviewees’ narratives seemed to imply that the choice had been made by others, embraced in the above-cited terminology engaging collectives. Not only had these implied collectives rendered the choice trivial through its ubiquity; they had also set it as a “standard” (hydrologist B) or as a “reference” (biogeoscientist G). By using that one representation, the modeler was thus within the norm.

Which implied collectives these norms referred to remained however unclear. Typical, standard, very common or well-known for whom? "Everybody" was a generalization the interviewees were often gradually whittling down in the course of the interview. They first reduced it to their own discipline, as in the case of geomorphologist A and hydrologist B cited above. Ecologist E asserted that hydrologists and meteorologists would defensively "stand up" when confronted to the transfer functions used within his model. Biogeoscientist G criticized the fact that ecologists would seek for simplicity in global models, while he considered that the more generic[4] the representation of photosynthesis was, the easier it could be applied to the globe. But even within the suggested frontiers of their discipline, the interviewees progressively distanced themselves from an all-embracing generality. Except for interviewee C and G, they directly mentioned having faced critics from peers, whose approaches (and sometimes epistemic stances) were different (interviewee A, E), or criticized competing alternatives in their field (B, D, F). The peers they alluded to could slowly be excluded from the picture of the implied and norm-creating collective. Who then were these crowds hiding behind the consensus, the practice, the popularity? The picture remained utterly blurry. Should we maybe consider the interviewees' generalizations to be a sign that they, too, could not pinpoint the exact origin of the choice that had imposed itself? That it was so embodied (Kaufmann, 2001), interiorized, that its origin – in the model as well as in the modeler's practice – had faded away? If so, how to make it resurge in order to better apprehend the processes at hand? The interviewees' involvement of particular individuals in their narratives, which we outlined in section 3.3.1, might offer a grip in order to engage on this reverse journey. In the next section, we dissect these individual accounts in the search of a process which could ultimately have led to the observed generalizations – and to the perceived absence of choice.

### 3.4. Transfer and path dependence

The involvement of (human) actors represented a cornerstone in the interviewees' explanations about the choice of representations in their models. As stated in section 3.3.1, members of the thesis committee belonged to the most cited individuals, regardless of the time (from five up to twenty-six years) that had elapsed since the interviewees who cited them had earned their doctorate. Through the example of hydrogeologist D, we showed how the intellectual and biographic authority of a supervising professor could be intertwined in the narration of the implementation of a particular representation. Interviewee D reported the use of the numerical scheme as a seemingly direct transfer: the choice of the professor (who had used the method for his own model) had been adopted by the interviewee and was still

[4] By "generic", the interviewee meant "close to processes", and hence less dependent on the type of vegetation.

nowadays a distinctive feature of his hydrogeological model. The case of interviewee F, detailed below, depicts a lengthier, less conscious process, which will be used as a starting point for our conceptual model.

Interviewee F started to construct his model as a post-doctoral researcher, twenty years prior to the interview. He still continues to develop this model, which is nowadays used by a wide network of scientists in his research field, astrophysics. The model ought to simulate galaxy formation. In the course of the interview, the modeler spoke about “rivals” who were using a different numerical method to represent fluid dynamics of hydrogen. This numerical method will be named hereafter Zeta. He went on to relate a “quite spectacular, quite famous controversy” which had opposed the supporters of one numerical method against the other: two “schools”, which referred to specific groups across the globe[5] and who competed with each other across different subdisciplines, from galaxy formation to planet formation. The two numerical methods had such strong implications on the representation of fluid dynamics that the interviewee considered them to be “structuring” the entire modeling approach. In some particular modeling cases, the Alpha representation the interviewee used proved complex, “a bit wobbly” to apply – a limitation the Zeta representation could overcome. However, when comparisons were carried out, it was realized that the Zeta representation had “an abominable problem” and was a “total disaster”: hydrogen simulated by Zeta had the viscosity of honey. Even though the Alpha representation also showed drawbacks – “the thing should not be idealized”, the interviewee specified several times – the Zeta representation was (at least momentarily) “crushed”. He conveyed the impression to have stood on the right side of history. The comparisons had temporary revealed winners and losers, which for the latter resulted in declining funding. But why did he, at the very beginning – long before the controversy broke out and established Alpha – choose to use this one rather than Zeta? The full answer of the interviewee to this exact question is transcribed hereunder:

> “ It is an interesting [question]… because indeed, during my PhD, my PhD director was only working with [Alpha]. And so I was educated with it. And so I couldn’t imagine doing something else, because… by lack of knowledge, when we are PhD students, we don’t know much, so we… And so after my thesis, I naturally evolved with this approach because it was what I knew. It was a natural evolution, it is as… yes, when we can speak a language, we evolve with this language. So here it is a bit the same, I knew how to speak [Alpha] and so I naturally kept on evolving with this approach. But it is true that… yes, it is the main reason, I believe. “

[5] According to the interviewee, these groups were not specifically found in particular regions, but could co-exist within a same institution. The financial and institutional consequences of the controversy presented hereafter were however significantly more important in Northern America than in Europe.

This answer contrasted with the vehemence displayed in the prior account of the controversy. The fact that the interviewee stood on the winning side depended on his thesis director, two decades ago; had the latter been working with Zeta, the interviewee suggests, he would himself have been likely to use this alternative numerical method. In addition to this relativistic perspective, the interviewee's answer deserves attention for another aspect it underscores: that of a process, an "evolution", as the repetitive use of the related verb furthermore emphasizes. The interviewee depicts himself as a doctoral student without a priori preference for either of the two approaches; in contact with a user of the Alpha approach, moreover holding the academic responsibility to guide his work (his thesis director), the interviewee is progressively educated to use it. The evolution continues well beyond his doctorate. Having gained particular skills in using Alpha, the interviewee is "naturally" inclined to pursue that direction, drifting away from what originally could be considered as a coincidence. This process appears self-reinforcing: the more the interviewee uses Alpha, the more the probability of him switching to Zeta in the course of his career diminishes. The interviewee's analogy with languages is enlightening in this regard. Implementing a numerical method requires learning, the gain of particular skills – in other words, time and efforts. The re-use of a numerical method mastered by the modeler hence smooths out the friction (Edwards, 2013) of this process: by speaking a language he is already fluent with, he avoids having to invest supplementary time and cognitive resources in starting from scratch anew. The astrophysicist F is not anymore under the academic influence of his PhD director, but both explicit and tacit knowledge accumulated through the repetitive use of Alpha induce a path dependence (David, 1985; Lahtinen et al., 2017). This path dependence is of course reinforced - or rendered less reversible - by the suitability of the Alpha method to the interviewee's objectives, and its success in comparison to Zeta. This aspect of path dependence was also evoked by geomorphologist A and hydrologist B. Geomorphologist A witnessed with skepticism current external endeavors to increase physical descriptions of processes within his model: "it's like, something is working, why do you want to change it?". When being asked by the end of the interview whether he had anything to add, hydrologist B started to outline that for him, a "funny thing" in model building was that "a lot of choices you make are to a certain extent subjective" – and went on to describe what he called the "legacy effect".

> " at a given moment you have the legacy effect in the model, you know... Stuff you put in a long time ago and.. yes, maybe you want to change it, but then at a given moment it becomes difficult to change it, and also there are things in a model that are still relatively… you know, you put them in at initial steps and… well, they function."

The "legacy effect" mentioned by hydrologist B can be understood as a synonym of path dependence. Here, the internal structure of the model is seemingly presented as an additional factor contributing to the modeler keeping the same representation. Changing of representation could require modifying the very structure of the model, while many developments were based on it – implying considerable time investment and technical difficulties. Just as for geomorphologist A, the fact that the model "functions" (functioning being regularly defined by the interviewees as producing results in accordance with available observations) reinforces the path dependence and diminishes the likeliness of the modeler throwing himself into the challenges of the modification of a representation. Additionally, and albeit not directly mentioned by the interviewees, funding opportunities can also contribute to the reinforcement of the path dependence by encouraging the extension of a specialized track the modeler already embarked upon.

### 3.5. Incorporation and anchoring

The evolution astrophysicist F refers to can be split into two phases, displayed in Figure 1. We shall describe the first phase as the transfer of a choice of a particular representation (Alpha) from an external actor (the thesis director) to the modeler (interviewee F). The representation is progressively incorporated by the modeler in his practice up to becoming his own – a second phase defined as incorporation. In the language analogy, the modeler becomes fluent; he gains skills and expertise which render the use of the representation increasingly evident. Note that these two phases are not divided by a sharp line. Rather, the choice for a particular representation is gradually interiorized as the modeler keeps on using the representation.

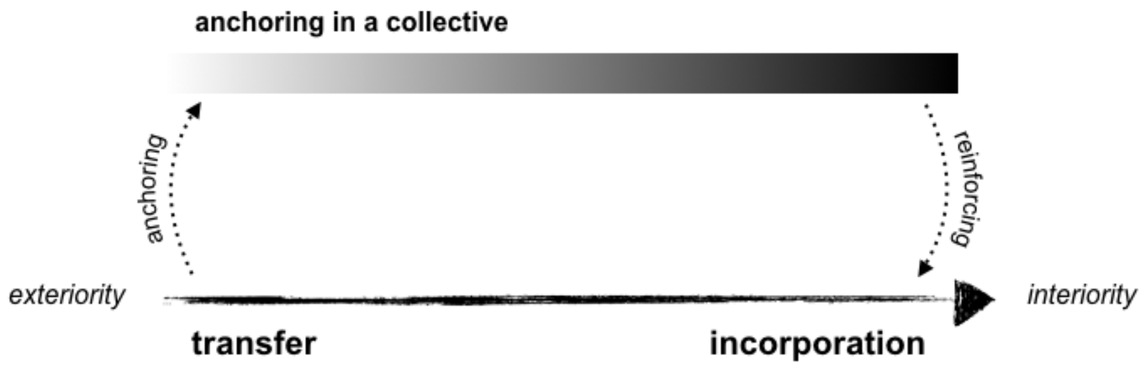


*Figure 1. Conceptualization of the process of incorporation, by a modeler, of a certain representation. In the studied cases, the representation of the phenomenon is transferred from an external source (for instance a thesis director, a professor, a colleague) to the modeler. As the latter uses it and gains associated skills, the choice is progressively incorporated and made his own. The process of incorporation displays a shift from exteriority to interiority. In parallel, the use of a particular representation rather than another anchors (dotted arrow on the left) the modeler in a collective, formed by the network of its users. The shading line depicts the progressive stabilization of the anchoring, happening as the choice is incorporated by the modeler. The stabilization in the collective can reinforce the incorporation (dotted arrow on the right) by increasing the friction required from the modeler to move away from it.*

We believe that another process runs in parallel to the progressive incorporation. In the case of interviewee F, the Alpha representation was not used solely by his thesis director, but by other peers too. Put in other words, the transferred choice was or had been shared by other astrophysicists, in or outside his university or institute. Additionally, and in contrary to the Zeta method, it had been used by engineers too, a collective the interviewee made repeated reference to in the course of the interview and which, in his opinion, seemed to speak for the efficiency of the method. By using the Alpha representation, the interviewee hence implicitly joined a collective – a network – of researchers and engineers sharing a similar approach and epistemic stance. We insist here on the implicit character this "anchoring" may have at the beginning of the process. During his doctorate, interviewee F was indeed most likely unaware of embracing a particular current (distinct of that of the Zeta users), through his use of Alpha; he describes himself as not knowing of alternatives to Alpha at that time. It is only in later stages of his career that the anchoring in a collective became explicit and that the modeler could refer as he did to the two different "schools", one of which was "rival". Through the shading line of Figure 1, we postulate that the more the modeler uses the representation, the more his anchoring in the network of its users solidifies. Collaborations with other researchers sharing a similar approach, participations in conferences and publications in journals within which the representation is well accepted, contribute to stabilizing the modeler's anchoring in the collective. But the anchoring may in turn reinforce the incorporation itself, as the dotted line on the right of the figure displays. In the Alpha versus Zeta rivalry, the outcome of the controversy acted as deepening the furrow already ploughed by the modeler. The controversy confirmed him in the – already incorporated – use of the representation; changing of representation would not only have challenged two decades of work and expertise, but also implied to turn away from a relatively clear-cut collective in which the modeler had established himself.

The initiated conceptualization, represented by the two phased transfer/incorporation-arrow and the progressive anchoring (Figure 1), can prove useful to re-interpret the accounts provided in section 3.3. Along with interviewee F, interviewees A, E and D made explicit reference to the transfer of a choice of a representation. Geormophologist A explained how his professor had the idea of a cellular automaton approach that would allow to reproduce observed patterns in wetlands (see section 3.2) and asked him to use it. The interviewee had to become familiar with it, which he did through "a book that explained the algorithm". In the modeler's own words, the approach was very simple; he rapidly assimilated – incorporated – it. Here, his path however diverged from that of astrophysicist F. While the transfer and the progressive incorporation was still observed, it happened on a shorter, definite time span. Indeed, the project he worked on was limited in time and, arising from wetland sciences, did not correspond to his main discipline. The interviewee moved away from the topic in his new position. Yet he showed particular consciousness of the implications the use of the component conveyed in regard to anchoring in particular groups. In his own words, his professor “came from” reduced complexity modeling, which represented a “whole field” of scientists in which the approach enjoyed “popularity”. In contrast, others would “not really” appreciate this type of modeling: “we break a lot of rules”. In spite of his change of environment, position and task, the interviewee still strongly identified with the general modeling current he had implicitly been anchored in, and defended it several times during the interview. His professor had not only transferred a “simple” algorithm, but a complete epistemic stance. Ecologist E’s citation in section 3.3.1 shows similarities. His colleague’s interference, leading him to adopt a simpler representation of plant growth and parameterize terms of the time transfer function, was an input to a reevaluation of his practice. It ultimately anchored him more firmly in an “approach” which he had identified himself with – simplicity and mini-modeling. The explicit description by hydrogeologist D of the role a member of his thesis committee had played in his choice is also compelling in this regard. By underscoring the practical mind and experience of his professor, the interviewee rooted not only his professor, but also himself (through the use of the technique) in a collective different from that of “mathematicians who have their heads in the clouds”, as he defined them. The technique, which was innovative and required particular skills in numerical methods, also enabled him to depart from the dominant approach of modeling that prevailed at that time, which conveyed advantages in terms of funding opportunities.

The examples described above epitomize the left side of figure 1, characterized by a transfer and the resulting anchoring in a network of users sharing the same choice of representation. Yet not all interviewees alluded explicitly to these initial steps. Other narratives, such as hydrologist B’s, ecologist E’s and biogeoscientist G’s narratives, which predominantly engaged generic collectives (see section 3.3.2), can rather be related to the right-hand extremity of the figure. The use of the representation seemed evident to the modelers, imposing itself and dissipating the fact that it had been preceded by a choice. The choice precisely appeared as a non-choice; it was fully incorporated. It had anchored the modelers in a collective, whose neighboring groups the interviewees did not hesitate to criticize. However, the transfer of the representation could not be isolated in their narratives. We can hypothesize on the reasons for the more explicit mention of the transfer by some of the interviewees. In astrophysicist F’s competitive and controversy-laden context, the use of the representation acted as structuring and amounted to choose one’s side, while the presence of an alternative was prominent. The history of the divisive representation in one own’s practices was hence likely to be more conscious. But the very act of the transfer may also be impressed on a more durable basis because of the personification of the choice, related for example to a professor who profoundly marked either the modeler (hydrogeologist D) or the modeling project itself (geomorphologist A)[6]. Similarly, the intervention of ecologist E's colleague, leading him to discard terms of his time transfer function describing plant density, acted as an epistemic turn in his own history, which rendered the choice conspicuous. Yet in other cases – possibly less controversial, personified or far reaching – the choices of the same interviewee were loosely rooted back as textbook, just as others had described theirs as standard or consensual. All in all, some decisions seem to be deemed structuring enough for their origin to be fixed in consciousness, memory and narratives. The path of others fade, leaving us with nothing less but an apparent self-evidence. We cannot then but say that we use these representations precisely because we use them, used them, and because they are used by others around us. We cannot remember why, in first stance, we started "speaking this language" we became over time so fluent with. Did we get acquainted with it during education, under the impulsion of a supervisor, through literature or by collaborating with peers? In everyday situations, such incorporated schemes of actions would commonly be described as habits. The word choice is less trivial than it appears; the concept of habit has enjoyed repeated attention and analysis from social scientists and philosophers over the last centuries up until today. Habit might offer the required conceptual framework to sum up our findings. We shall briefly define it before exploring its implications.

### 3.6. Habits

There is an inherent risk in using a nowadays colloquial word to refer to a particular concept: that of a turmoil of signifieds eclipsing its actual pertinence and delineations. Daston and Galison (2007) portrayed the tumultuous history of the concept of objectivity, which went on to carry opposite meanings in the course of time. The concept of habit has not much to envy in this regard. First conceptualized by Aristoteles (Camic, 1986), habits were amply theorized before becoming a collateral damage of the endeavor of sociologists of the beginning of the 20th century to depart their field from psychology. With the rise of behaviorism, habits had indeed been restricted to mere reflex actions, hence inherently individual-centered; sociologists progressively abandoned the term. But the concept used to be richer than that. In fact, the very sociologists who discarded it for the sake of their discipline's institutionalization had previously used the concept as a cornerstone of their own theories. Most notably among them, Durkheim had been considering habits as "the real forces that govern us" (Camic, 1986). What was meant by it were not reflex actions. Rather – and here lies its interest for the analysis

[6] Personification of modeling decisions appeared less prominent when the modeling project was not conducted by the modeler individually, but by a team (as in the case of geochemist C and biogeoscientist G). Further intra-disciplinary research could enable to assess the role of collective functioning of a discipline in enabling or encouraging "personal" models, versus consensual building.

of modeler's decision making – habits were understood as patterns of conduct, incorporated schemes of actions which could involve reflexivity. This "middle range" of habits, as opposed to the biological automatisms implied by behaviorists and the "structured structures" of Bourdieu's habitus (Bourdieu, 1990), recently attracted new interest (see for example Latour, 2013, Kaufmann, 2001, Sloterdijk, 2014). In line with Camic, we shall define habits as a "more or less self-actuating disposition or tendency to engage in a previously adopted or acquired form of action" (Camic, 1986).

Camic's definition seems indeed to fit the self-evident depiction the interviewees rendered of some of their choices. As stated in section 3.4, some choices seemed to impose themselves without always implying conscious reasoning. While the colloquial usage of the word habit is often taut with negative perception (a banal act), social scientists have underscored its necessity. If schemes of actions were never to be incorporated, the modeler would at each time be confronted with decisions monopolizing his full attention, similarly to an individual who would re-consider alternatives to her road to work at each intersection, every day of the year. As Latour (2013) posits, habits act as "smoothing discontinuities" in courses of action. They are what enables the individual to move forward, instead of stagnating in ever-recurring doubt and interrogation. They are what enables the individual to gain skills, expertise – in other words (the ones of astrophysicist F), fluency. In biogeoscientist G's and hydrologist B's examples, taking over a set of time transfer functions (describing photosynthesis for the former, evapotranspiration for the latter) deemed as "standard" or "of reference" and that they or their team had used before, made it possible to concentrate more explicitly on other parts of the model where they considered the real innovation to be.

Habits are needed. But while some increase our skillfulness, Latour asserts, others might "degenerate into mechanical gestures and routines". The difference between habits that bring us forward and automatisms that confine us in obtuseness lies in our level of attention. Habits smooth discontinuities - the path of our decisions - but they do not make them disappear completely. Using Latour's terminology, habits merely "veil" them. What we witnessed in the interviews was this very unveiling: a progressive reappearance of crossroads, of intersections, where actors (whether individual or collective) had acted as road signs in the course of action. Geomorphologist A's, hydrogeologist D's, astrophysicist F's professors and Ecologist E's colleague had been these very road signs, impetus to follow a particular trajectory that the interviewees progressively made their own. We do not generally think about these discontinuities; but we are able to retrieve them, to take the control over again when a disruption occurs. Whenever these discontinuities are not just veiled but totally erased, the automatism is looming. Our attention has faded; we apply, but are unable to tell why.

When adopted (rather than self-acquired), habits convey another distinctive feature, summing up much of our findings: that of embedding social knowledge. This property, less investigated by Latour, has been particularly highlighted by Kaufmann (1997, 2001). Following the theoretical contribution of the latter, habits can then be considered as mediating between exteriority and interiority; between the transfer from an external source to the individual incorporation, as figure 1 portrays. The habit – that of using one representation, in our modeling context – cannot be detached from the network of actors which used it earlier. The interviewees' representations all inscribe and record a knowledge shared by individuals, and which is inherently engaged through the use of the representation. The habit incorporates collective practice, sociotechnical and epistemic components; by incorporating the habit, the modelers anchor themselves in the collective too.

### 3.7. Implications

Our study shows that in model construction, choices of representations are not made anew every time; they can be transferred and evolve, through incorporation, to become a modeler's own modeling habit. Whether habits are supporting one's work in increasing expertise and efficiency, or locking one up in automatisms, amounts to examine our level of attention and alertness. Can we retrace the path of initial decisions – the discontinuities – the habit progressively smoothed? Could we revert it, if we wished so? Or, as Latour (2013) portrayed with humor, can we still identify the IP addresses of our (modeling) habits? The interviews we conducted did not aim at asserting whether the interviewees could answer these questions or not. We did not track habits down; the concept of habit emerged from our analysis as a suitable theoretical framework to embrace the incorporation and the self-evidence of decisions the interviewees depicted. The above-mentioned questions ought to be personal ones for all of us. In some cases, which we covered earlier, the path of habits might be profoundly impressed on one's consciousness due to the controversial, far-reaching context, or because of the personification of decisions. In other cases, the path having led to habits might be thinner, requiring deeper self-reflexivity.

Whether conscious or veiled, all modeling decisions have implications in regard to collectives. Choosing one representation rather than another amounts to anchor oneself in a social network rather than another. The anchoring might be the consequence (as for astrophysicist F's Alpha representation), but also one of the reasons of the choice (see hydrogeologist D). A choice is a statement: that of establishing oneself in relationship to others or apart from them; that of situating oneself in keeping with previous work or in dissonance with them. Yet as loaded with epistemic stances as the representations were, the interviewees had mostly perceived them as choices only in later stages of their career. At the moment of their transfer

– usually during the interviewee's doctorate, or shortly after it – these representations were non-choices: the modelers were not necessarily conscious of the existence of alternatives. These findings should question our own awareness of choices we inherently transfer to novices, and which they do not always recognize as such. The issue at stake, after all, is that of asking ourselves *whether we know why we do what we do.* Why are we using the Alpha representation (if we may refer a last time to it) rather than the Zeta one? Which implicit decisions did we or others make? Which networks are we engaging in the choice and, through it, anchoring ourselves in? Answering these questions might not per se lead to better models; but it might enable to raise one's attention – and hence control – on incorporated habits, and lead to even more thought-out, weighed modeling. It might, too, enable one to render decisions more explicit during education, so as to keep alternatives visible for upcoming researchers involved in modeling. Presenting a representation to novices as a choice amounts to unveil a crossroad: an opening to possibilities, rather than the tunnel of an automatism upon which the attention long faded.

While the present study sheds light on habits, we do not believe them to subjugate the other actants (Latour, 1987) commonly identified as impacting decisions during the modeling process, such as data availability, computational power or technical constraints, even though these were less spontaneously appearing in the interviewees' narratives[7]. Rather, we conceive habits as intertwined with them. Habits play a role *among* other actants. This articulation would deserve additional research in order to assess more accurately their conjoined roles. We sketched social networks; we may now need to dive into the materiality of the modeling practice in order to embrace models in their sociotechnical (Mialet, 2012) perspective. Similarly, habits represent only one among several theoretical concepts aiming at revealing practices progressively rendered invisible. The transfer of modeling skills (and not only representations), for example, would gain to be studied through the lens of the neighboring concept of tacit knowledge (Collins, 2010; Polanyi, 1966, 1958). It should also be noted that showing the influence of habits does not imply that modeling decisions are completely determined by them. Studies exploring the conditions of emergence of new process representations could enable to explore the limits of their influence.

Finally, other methodologies – most notably direct, ethnographical observation – could now be deployed to observe the processes framed into words by the interviewees. Diversifying the methods of qualitative data collection might enable to overcome limitations inherent to

[7] We had expected these aspects to appear forefront in the answers of the interviewees. A possible explanation might be that the interviewees considered them to be obvious, self-speaking constraints for the interviewer too; either unworthy of mentioning or less strikingly inscribed into their memory of the course of events.

interviews, such as possible a posteriori rationalizations (induced by the perceived necessity, by the interviewee, to provide a compelling story of his or her own practices) or the omission of particular aspects (either regarded as unworthy of interest or disadvantageous).

### 3.8. Navigating through different disciplines and types of models

The limited number of interviewees does not enable all-encompassing generalizations, for which our exploratory study was not designed. However, we had wished to account for some of the diversity within earth and universe sciences by selecting interviewees from different disciplines and whose models ranged from very simple to highly complex, and from local to global. Our findings did not seem to be restricted to one specific class of models or disciplines. The analysis presented in section 3.2 to 3.4 linked examples from models generally partitioned within distinctive categories and covered by different specific literature: similar results appeared to emerge from varied contexts and contents. Future research could be conducted to explore this diversity by encompassing both an even wider range of disciplines and more modelers from a same discipline in order to explore internal diversity. Conversely, limiting oneself to one case study within one discipline could enable to observe in greater details and "in the making" the processes addressed herein. Such ethnographical case studies could prove profitable to study the conjoint role of other factors in decision-making which the interviewees did not spontaneously allude to, such as institutional and political triggers, as well as technical and economical constraints. Similarly, we call for studies tailoring a specific type of models within one discipline (such as "process-based" models in hydrology, or "simple" models in ecology, see section 3.3.1); unseen correlations between the model type and its context could explain habits not determined by personal or inter-personal influences.

## 4. Conclusion

We presented the outcomes of the first research basing itself on the methodological and theoretical framework of Science and Technology Studies to investigate decision-making in environmental model construction. We hereby aimed at exploring the modelers' own narratives, as opposed to the written accounts in scientific articles. Extensive face-to-face, semi-directed interviews were conducted with European and North-American senior scientists from various disciplines of Earth and universe sciences. The interviews were recorded and entirely transcribed. This methodology was well accepted by the interviewees, who displayed particular openness. When asked to explain choices they had made during the process of model construction, the interviewees engaged a variety of actors in their narratives. The description of personal, inter-individual and collective influence on modeling decisions was not sought for in the interview grid, but spontaneously emerged from the modelers' narratives. We showed how individual actors (predominantly members of the interviewees' thesis committees) were often credited with the choice of a particular representation of a phenomenon, which was transferred to the modeler and progressively incorporated to become his own. This transfer played a paramount role in the interviewees' justifications of using a representation rather than another, even in competitive and controversy-laden contexts. A thesis director's or a colleague's own modeling practices were spontaneously referred to as the origin of a choice which, in several cases, still marked the interviewee's modeling decades later. Next to individual actors, modelers also made frequent mention of generic collectives in which they deemed the representation to be established or even normative. They appeared to be strikingly conscious of their practice being specific to a "school" or "field" in their own discipline. By using a particular representation, the modelers anchored themselves in a collective; this anchoring could either be a mere consequence or a pursued objective. These results emerged from varied contexts and contents across different disciplines and types of models.

We engaged the sociological concept of habit to discuss the incorporation of choices, evolving from a transfer (from a former professor, colleague or alike) to a self-evident practice. In line with Latour (2013) and Kaufmann (2001), habits were presented as i) smoothing discontinuities in the course of actions; ii) embedding social knowledge. The importance of modeling habits in enabling expertise and efficiency was emphasized, yet made conditional on one's awareness of the decisions initially made and their reasons; habits are never far from automatisms. We are not describing modelers as being determined by habits. However, at the level of both the individual and the community, tracking modeling habits in reverse – hereby unveiling and analyzing the reasons for made choices – may lead to a better understanding not only of existing practices, but also of the individual preferences, collective inclinations and epistemic stances which are embedded in choices transferred to novices. This study is hence an important step in exploring the inter-individual and collective influence on the model building process, which necessarily impact the modeling outputs. We see the need for additional ethnographic studies scrutinizing the role of other conjoint triggers of model decision that the interviewees did not spontaneously alluded to, such as the sociotechnical context of model development.

Acknowledgement:
The anonymous interviewees are most gratefully thanked for their time, their welcome and their openness. Helpful insights were received from Emilie Bovet in the initial stages of the research. We further thank Kris Decker, Verena Halsmayer and Christoph Hoffmann for fruitful discussion, inputs and comments on a first version of this article, as well as Tobias Krueger and one anonymous reviewer for their very insightful suggestions to improve the present version. This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

Declarations of interest: none.

Bibliography

Addor, N., Melsen, L.A., 2019. Legacy, Rather Than Adequacy, Drives the Selection of Hydrological Models. Water Resour. Res. 55, 378–390. https://doi.org/10.1029/2018WR022958

Augusiak, J., Van den Brink, P.J., Grimm, V., 2014. Merging validation and evaluation of ecological models to 'evaludation': A review of terminology and a practical approach. Ecol. Model. 280, 117–128. https://doi.org/10.1016/j.ecolmodel.2013.11.009

Azadi, H., Barati, A.A., Rafiaani, P., Taheri, F., Gebrehiwot, K., Witlox, F., Lebailly, P., 2017. Evolution of land use-change modeling: routes of different schools of knowledge. Landsc. Ecol. Eng. 13, 319–332. https://doi.org/10.1007/s11355-016-0311-9

Beck, M., Krueger, T., 2016. The epistemic, ethical, and political dimensions of uncertainty in integrated assessment modeling. Wiley Interdiscip. Rev. Clim. Change 7, 627–645. https://doi.org/10.1002/wcc.415

Bennett, N.D., Croke, B.F.W., Guariso, G., Guillaume, J.H.A., Hamilton, S.H., Jakeman, A.J., Marsili-Libelli, S., Newham, L.T.H., Norton, J., Perrin, C., Pierce, S., Robson, B., Seppelt, R., Voinov, A., Fath, B.D., Andreassian, V., 2013. Characterising performance of environmental models. Environ. Model. Softw. 40, 1–20. https://doi.org/10.1016/j.envsoft.2012.09.011

Beven, K., 2008. Environmental Modelling: An Uncertain Future?, CRC Press. ed. CRC Press.

Beven, K., 1993. Prophecy, reality and uncertainty in distributed hydrological modelling. Adv. Water Resour., Research Perspectives in Hydrology 16, 41–51. https://doi.org/10.1016/0309-1708(93)90028-E

Beven, K., 1991. Spatially Distributed Modeling: Conceptual Approach to Runoff Prediction, in: Bowles, D.S., O'Connell, P.E. (Eds.), Recent Advances in the Modeling of Hydrologic Systems, NATO ASI Series. Springer Netherlands, Dordrecht, pp. 373–387. https://doi.org/10.1007/978-94-011-3480-4_17

Beven, K., 1989. Changing ideas in hydrology — The case of physically-based models. J. Hydrol. 105, 157–172. https://doi.org/10.1016/0022-1694(89)90101-7

Beven, K., Young, P., 2013. A guide to good practice in modeling semantics for authors and referees. Water Resour. Res. 49, 5092–5098. https://doi.org/10.1002/wrcr.20393

Bourdieu, P., 1990. The Logic of Practice. Stanford University Press, Stanford.

Camic, C., 1986. The Matter of Habit. Am. J. Sociol. 91, 1039–1087. https://doi.org/10.1086/228386

Collins, H., 2010. Tacit & Explicit Knowledge. The University of Chicago Press, Chicago and London.

Daniel, F.-J., 2018. Interpréter dans la grosseur du trait: Les usages prédictifs et évaluatifs des modèles de dispersion des odeurs. Rev. Anthropol. Connaiss. 12,1, 1. https://doi.org/10.3917/rac.038.0001

Daston, L., Galison, P., 2007. Objectivity. Zone Books, New York.

David, P.A., 1985. Clio and the Economics of QWERTY. Am. Econ. Rev. 75, 332–337.

Edwards, P., 2013. A Vast Machine: Computer Models, Climate Data, And The Politics Of Global Warming. The MIT Press, Cambridge, Massachusetts.

Eyring, V., Bony, S., Meehl, G.A., Senior, C.A., Stevens, B., Stouffer, R.J., Taylor, K.E., 2016. Overview of the Coupled Model Intercomparison Project Phase 6 (CMIP6) experimental design and organization. Geosci. Model Dev. 9, 1937–1958. https://doi.org/10.5194/gmd-9-1937-2016

Freeze, R.A., Harlan, R.L., 1969. Blueprint for a physically-based, digitally-simulated hydrologic response model. J. Hydrol. 9, 237–258. https://doi.org/10.1016/0022-1694(69)90020-1

Grayson, R.B., Moore, I.D., McMahon, T.A., 1992. Physically based hydrologic modeling: 2. Is the concept realistic? Water Resour. Res. 28, 2659–2666. https://doi.org/10.1029/92WR01259

Grimm, V., Augusiak, J., Focks, A., Frank, B.M., Gabsi, F., Johnston, A.S.A., Liu, C., Martin, B.T., Meli, M., Radchuk, V., Thorbek, P., Railsback, S.F., 2014. Towards better modelling and decision support: Documenting model development, testing, and analysis using TRACE. Ecol. Model., Population Models for Ecological Risk Assessment of Chemicals 280, 129–139. https://doi.org/10.1016/j.ecolmodel.2014.01.018

Gubrium, J., Holstein, J., 2001. Handbook of Interview Research. Sage Publications, Thousand Oaks, California. https://doi.org/10.4135/9781412973588

Guillemot, H., 2010. Connections between simulations and observation in climate computer modeling. Scientist's practices and "bottom-up epistemology" lessons. Stud. Hist. Philos. Sci. Part B Stud. Hist. Philos. Mod. Phys., Special Issue: Modelling and Simulation in the Atmospheric and Climate Sciences 41, 242–252. https://doi.org/10.1016/j.shpsb.2010.07.003

Guillemot, H., 2009. Comment évaluer un modèle numérique de climat ? Rev. Anthropol. Connaiss. 3, n° 2, 273–293. https://doi.org/10.3917/rac.007.0273

Hämäläinen, R., 2015. Behavioural issues in environmental modelling – The missing perspective. Environ. Model. Softw. 73, 244–253. https://doi.org/10.1016/j.envsoft.2015.08.019

Harmel, R.D., Smith, P., Migliaccio, K., Chaubey, I., Douglas-Mankin, K., Benham, B., Shukla, S., Munoz-Carpena, R., Robson, B., 2014. Evaluating, interpreting, and communicating performance of hydrologic/water quality models considering intended use: A review and recommendations. Environ. Model. Softw. 57, 40–51. https://doi.org/10.1016/j.envsoft.2014.02.013

Jakeman, A.J., Letcher, R.A., Norton, J.P., 2006. Ten iterative steps in development and evaluation of environmental models. Environ. Model. Softw. 21, 602–614. https://doi.org/10.1016/j.envsoft.2006.01.004

Jaton, F., 2017a. The Constitution of Algorithms. Ground-Truthing, Programming, Formulating. Université de Lausanne, Lausanne, PhD thesis.

Jaton, F., 2017b. We get the algorithms of our ground truths: Designing referential databases in digital image processing. Soc. Stud. Sci. 47, 811–840. https://doi.org/10.1177/0306312717730428

Kaufmann, J.-C., 2001. Ego. Pour une sociologie de l'individu. Une autre vision de l'homme et de la construction du sujet. Nathan, Paris.

Kaufmann, J.-C., 1997. Le coeur à l'ouvrage: théorie de l'action ménagère. Armand Colin, Paris.

Kim, H., Rosa, I.M.D., Alkemade, R., Leadley, P., Hurtt, G., Popp, A., Vuuren, D.P. van, Anthoni, P., Arneth, A., Baisero, D., Caton, E., Chaplin-Kramer, R., Chini, L., Palma, A.D., Fulvio, F.D., Marco, M.D., Espinoza, F., Ferrier, S., Fujimori, S., Gonzalez, R.E., Gueguen, M., Guerra, C., Harfoot, M., Harwood, T.D., Hasegawa, T., Haverd, V., Havlík, P., Hellweg, S., Hill, S.L.L., Hirata, A., Hoskins, A.J., Janse, J.H., Jetz, W., Johnson, J.A., Krause, A., Leclère, D., Martins, I.S., Matsui, T., Merow, C., Obersteiner, M., Ohashi, H., Poulter, B., Purvis, A., Quesada, B., Rondinini, C., Schipper, A., Sharp, R., Takahashi, K., Thuiller, W., Titeux, N., Visconti, P., Ware, C., Wolf, F., Pereira, H.M., 2018. A protocol for an intercomparison of biodiversity and ecosystem services models using harmonized land-use and climate scenarios. Geosci. Model Dev. Discuss. 1–37. https://doi.org/10.5194/gmd-2018-115

Kouw, M., 2016. Standing on the Shoulders of Giants—And Then Looking the Other Way? Epistemic Opacity, Immersion, and Modeling in Hydraulic Engineering. Perspect. Sci. 24, 206–227. https://doi.org/10.1162/POSC_a_00201

Krueck, C.P., Borchers, J., 1999. Science in Politics: A Comparison of Climate Modelling Centres. Minerva 37, 105–123. https://doi.org/10.1023/A:1004692000447

Krueger, T., Page, T., Hubacek, K., Smith, L., Hiscock, K., 2012. The role of expert opinion in environmental modelling. Environ. Model. Softw. 36, 4–18. https://doi.org/10.1016/j.envsoft.2012.01.011

Krysanova, V., Vetter, T., Eisner, S., Huang, S., Pechlivanidis, I., Michael Strauch, Gelfan, A., Kumar, R., Aich, V., Arheimer, B., Chamorro, A., Griensven, A. van, Kundu, D., Lobanova, A., Mishra, V., Plötner, S., Reinhardt, J., Ousmane Seidou, Wang, X., Wortmann, M., Zeng, X., Hattermann, F.F., 2017. Intercomparison of regional-scale hydrological models and climate change impacts projected for 12 large river basins worldwide—a synthesis. Environ. Res. Lett. 12, 105002. https://doi.org/10.1088/1748-9326/aa8359
Kuhn, T.S., 1962. The Structure of Scientific Revolutions, University of Chicago Press. ed. Chicago.
Kvale, S., 1996. InterViews: An Introduction to Qualitative Research Interviewing. SAGE Publications, Thousand Oaks, California.
Lahsen, M., 2005. Seductive Simulations? Uncertainty Distribution Around Climate Models. Soc. Stud. Sci. 35, 895–922. https://doi.org/10.1177/0306312705053049
Lahtinen, T., Guillaume, J., Hämäläinen, R., 2017. Why pay attention to paths in the practice of environmental modelling? Environ. Model. Softw. 92, 74–81. https://doi.org/10.1016/j.envsoft.2017.02.019
Landström, C., Whatmore, S.J., Lane, S.N., 2011. Virtual Engineering: Computer Simulation Modelling for Flood Risk Management in England. Sci. Technol. Stud.
Lane, S.N., 2014. Acting, predicting and intervening in a socio-hydrological world. Hydrol. Earth Syst. Sci. 18, 927–952. https://doi.org/10.5194/hess-18-927-2014
Latour, B., 2013. An Inquiry into Modes of Existence. An Anthropology of the Moderns. Harvard University Press, Cambridge, Massachusetts.
Latour, B., 1993. We Have Never Been Modern. Harvard University Press, Cambridge, Massachusetts.
Latour, B., 1987. Science in Action, How To Follow Scientists And Engineers Through Society. Harvard University Press, Cambridge, Massachusetts.
Latour, B., Woolgar, S., 1979. Laboratory Life. The Social Construction of Scientific Facts. Sage, Los Angeles and London.
MacKenzie, D., 2003. An Equation and its Worlds: Bricolage, Exemplars, Disunity and Performativity in Financial Economics. Soc. Stud. Sci. 33, 831–868. https://doi.org/10.1177/0306312703336002
Mason, J., 2004. Semistructured Interview, in: The SAGE Encyclopedia of Social Science Research Methods. Sage Publications, Thousand Oaks, California. https://doi.org/10.4135/9781412950589.n909
Merow, C., Smith, M.J., Edwards, T.C., Guisan, A., McMahon, S.M., Normand, S., Thuiller, W., Wüest, R.O., Zimmermann, N.E., Elith, J., 2014. What do we gain from simplicity versus complexity in species distribution models? Ecography 37, 1267–1281. https://doi.org/10.1111/ecog.00845
Mialet, H., 2012. Hawking Incorporated. Stephen Hawking and the Anthropology of the Knowing Subject. The University of Chicago Press, Chicago and London.
Morgan, M.S., Morrison, M., 1999. Models as Mediators: Perspective On Natural and Social Science. Cambridge University Press, Cambridge.
Oreskes, N., Shrader-Frechette, K., Belitz, K., 1994. Verification, Validation, and Confirmation of Numerical Models in the Earth Sciences. Sci. New Ser. 263, 641–646.
Peck, S.L., 2004. Simulation as experiment: a philosophical reassessment for biological modeling. Trends Ecol. Evol. 19, 530–534. https://doi.org/10.1016/j.tree.2004.07.019
Polanyi, M., 1966. The Tacit Dimension. The University of Chicago Press, Chicago and London.
Polanyi, M., 1958. Personal Knowledge. Towards A Post-Critical Philosophy. Routledge & Kegan Paul, London.
Pontille, D., 2007. Matérialité des écrits scientifiques et travail de frontières : le cas du format IMRAD, in: Paul-Cavallier, P.H. et M. (Ed.), Sciences et Frontières. E.M.E, pp. 229–253.

Quivy, R., Van Campenhoudt, L., 1995. Manuel de recherche en sciences sociales, Dunod. ed, Psycho Sup. Dunod, Paris.
Radchuk, V., Oppel, S., Groeneveld, J., Grimm, V., Schtickzelle, N., 2016. Simple or complex: Relative impact of data availability and model purpose on the choice of model types for population viability analyses. Ecol. Model. 323, 87–95. https://doi.org/10.1016/j.ecolmodel.2015.11.022
Refsgaard, J.C., van der Sluijs, J.P., Højberg, A.L., Vanrolleghem, P.A., 2007. Uncertainty in the environmental modelling process – A framework and guidance. Environ. Model. Softw. 22, 1543–1556. https://doi.org/10.1016/j.envsoft.2007.02.004
Rinck, F., 2010. L'analyse linguistique des enjeux de connaissance dans le discours scientifique. Rev. Anthropol. Connaiss. 4, 427–450. https://doi.org/10.3917/rac.011.0427
Rosenzweig, C., Jones, J.W., Hatfield, J.L., Ruane, A.C., Boote, K.J., Thorburn, P., Antle, J.M., Nelson, G.C., Porter, C., Janssen, S., Asseng, S., Basso, B., Ewert, F., Wallach, D., Baigorria, G., Winter, J.M., 2013. The Agricultural Model Intercomparison and Improvement Project (AgMIP): Protocols and pilot studies. Agric. For. Meteorol., Agricultural prediction using climate model ensembles 170, 166–182. https://doi.org/10.1016/j.agrformet.2012.09.011
Rykiel, E.J., 1996. Testing ecological models: the meaning of validation. Ecol. Model. 90, 229–244. https://doi.org/10.1016/0304-3800(95)00152-2
Seidman, I., 2013. Interviewing as Qualitative Research: A Guide for Researchers in Education and the Social Sciences. Teachers College Press, New York.
Sloterdijk, P., 2014. You Must Change Your life. John Wiley & Sons, Chicester.
Spradley, J.P., 2016. The Ethnographic Interview. Waveland Press, Long Grove, Illinois.
Sundberg, M., 2010. Cultures of simulations vs. cultures of calculations? The development of simulation practices in meteorology and astrophysics. Stud. Hist. Philos. Sci. Part B Stud. Hist. Philos. Mod. Phys. 41, 273–281. https://doi.org/10.1016/j.shpsb.2010.07.004
Thielen, J., Schaake, J., Hartman, R., Buizza, R., 2008. Aims, challenges and progress of the Hydrological Ensemble Prediction Experiment (HEPEX) following the third HEPEX workshop held in Stresa 27 to 29 June 2007. Atmospheric Sci. Lett. 9, 29–35. https://doi.org/10.1002/asl.168
van der Sluijs, J.P., 2002. A way out of the credibility crisis of models used in integrated environmental assessment. Futures 34, 133–146. https://doi.org/10.1016/S0016-3287(01)00051-9
Vinck, D., 2012. Accessing Material Culture by Following Intermediary Objects, in: Naidoo, L. (Ed.), An Ethnography of Global Landscapes and Corridors. InTech, Rijeka, Croatia.
Vinck, D., 2011. Taking intermediary objects and equipping work into account in the study of engineering practices. Eng. Stud. 3, 25–44. https://doi.org/10.1080/19378629.2010.547989
Vinck, D., 1999. Les objets intermédiaires dans les réseaux de coopération scientifique. Contribution à la prise en compte des objets dans les dynamiques sociales. Rev. Fr. Sociol. 40, 385–414. https://doi.org/10.2307/3322770
Voinov, A., Seppelt, R., Reis, S., Nabel, J.E.M.S., Shokravi, S., 2014. Values in socio-environmental modelling: Persuasion for action or excuse for inaction. Environ. Model. Softw. 53, 207–212. https://doi.org/10.1016/j.envsoft.2013.12.005
Weiss, R.S., 1995. Learning From Strangers: The Art and Method of Qualitative Interview Studies. Simon and Schuster, New York.
Westerberg, I.K., Di Baldassarre, G., Beven, K.J., Coxon, G., Krueger, T., 2017. Perceptual models of uncertainty for socio-hydrological systems: a flood risk change example. Hydrol. Sci. J. 62, 1705–1713. https://doi.org/10.1080/02626667.2017.1356926
Winsberg, E., 2010. Science In The Age Of Computer Simulation. The University of Chicago Press, Chicago.
Winsberg, E., 1999. Sanctioning Models: The Epistemology of Simulation. Sci. Context 12, 275–292. https://doi.org/10.1017/S0269889700003422