Humboldt-Universität zu Berlin, Philosophische Fakultät, Institut für Europäische Ethnologie

Masterarbeit

Knowledge Practices in the Modelling of “Socio-Ecological Coevolutions“

On Simplification, Experimentation, Visualization and Alignment

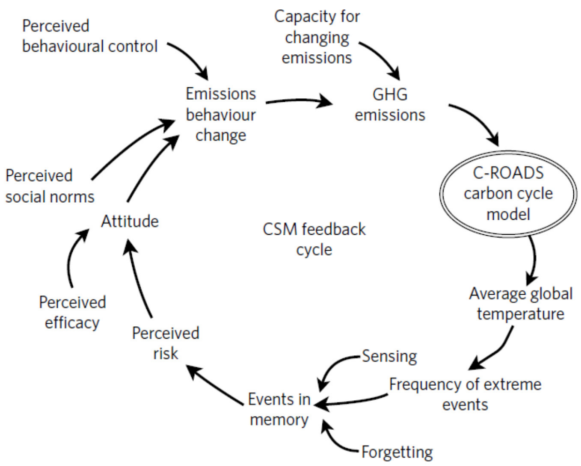

Fig. 1 | Conceptual model. Linkages between temperature, extreme events, perceived risk, social components and GHG emissions in the CSM. Average global temperature is calculated from the GHG concentration using the carbon cycle model of C-ROADS[7].

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For my sisters,

not always nearby, but always close to my heart.

I want to thank all those people without whom this work would not exist:

My parents who always believed in me,

My friends who kept me grounded,

My supervisors who supported the project from the very beginning,

My colleagues and friends from the STS-Lab who proofread and commented on the thesis from its early stages (and who supplied me with coffee),

And, most importantly, "the research group" that was so welcoming and open and answered all my questions.

Content

Figure 1 (on the cover): Conceptual loop diagram of a coupled climate change and social model with accompanying description (cf. Beckage et al 2018: 80)

1. Introduction

This thesis is about knowledge practices in the development of models of socio-ecological transformations as I was able to observe them during the three months of ethnographic research I conducted in an interdisciplinary research group. Especially on a global scale, modeling socio-ecological processes presented an important research boundary for the research group and their academic community. This motivated my research in two ways. On the one hand I want focus on the production of scientific knowledge, as it has been studied in Science and Technology Studies (STS), focusing on how a group of scientists works and thinks together every day as well as concomitant social and technoscientific practices of knowledge production. In this sense, this work stands in line with classic laboratory studies. On the other hand, I was also interested in how 'the social' vis-à-vis the environment, as a central target of inquiry in social anthropology, is conceptualized and re-produced in modeling practices.

Subsequently, my initial research question was concerned with how and based on which epistemic assumptions decisions are made about what to (not) include in a model: Which implicit and explicit theories about the social, society, and human decision-making enter the modeling process? What assumptions exist about the environment, nature and the material? And more specifically: How are assumptions represented, made explicit and productive throughout the process of model construction, e.g. through practices of mapping in causal loop diagrams and the subsequent transformation into mathematical models? These questions were anchored on three different levels: epistemology, social theory, and its operationalization.

However, shortly into my field work I realized that there is a gap between the goal of modeling socio-ecological processes on a global scale and the work done in the research group at that time with several simple, conceptual models of various social, psychological, environmental and/ or economic processes. This is why for this thesis I decided to focus less on model content and assumptions and more on a general *understanding* and description of modeling *practices* in the research group as I was able to gain it on the basis of interviews mainly with master and PhD-students in the research group, as well as accompanying participant observation.

Modeling encompasses model construction as well as model runs. Models in this specific context are mathematical models, sets of equations describing certain processes, relationships, interactions and entities. Corresponding computational models are then used to simulate that system's behavior over time, starting from a particular scenario. Additionally, the models appear in a number of other ways in everyday practices, e.g. as conceptual diagrams or plots.

Thus, I will describe practices in model construction as continuous socio-technical and material-semiotic practices of alignment of model formats. Because each of these formats is a part of different knowledge-practices, their alignment will not and does not have to be perfect in order for the model to work. Rather, it is an ongoing process.

In particular, I will focus on simplification, experimentation and visualization as such practices and how they are related to moments of bifurcations entraining varying degrees of irreversibility in modeling, e.g. where decisions about the model structure are being made, some of which might be harder to reverse than others. These practices are also practices of "social modeling" – e.g. collectively, in a certain institutional context, under pressures of time, or based on underlying ontological assumptions. Ultimately, "Social modeling" and "models of the social" are related in important ways. To argue that point in my last part, I will take up my initial research focus on epistemic assumptions again.

But first, I will describe the research field in more detail, the modelers as well as their models. Then I will briefly spell out the research process and methods before I will frame my project within anthropological STS and sketch a material-semiotic perspective on knowledge practices to ground my focus on bifurcations and irreversibility in model construction. In the main part I will focus on three such practices: simplification, experimentation and visualization. In the last part, I will again return to the perspective framing my research and analyze what means for modeling socio-ecological processes in epistemological and ontological terms.

2. Research Field

My research field was an interdisciplinary research group developing ways of modeling socio-ecological transformations. Their aim was to eventually be able to model these transformations on a global scale, bringing together Earth System Analysis, climate models and new methodological modeling approaches in order to include social processes other than economy. Talking about their research motivation in interviews or writing about it in publications, they often referred to discussions surrounding the Anthropocene and Planetary Boundaries.[1] A central theoretical perspective was Complex Systems Theory. Complex systems are systems that display some sort of emergent behavior on the macro level which is neither centrally organized nor predictable from the knowledge about the rules governing the micro-processes, the agents' interactions (cf. Boccara 2010: 4). Examples for such systems may be ant colonies, agent-based models, the brain or the earth's climate.[2]

The group had around 15 members, most of them physicists, but included also a mathematician, a sociologist and an economist. The group was co-chaired by two senior scientists, and apart from three other post-doctoral researchers consisted mainly of PhD-Students, as well as some Master's students and two undergraduates. Due to the different educational stages and other institutional affiliations, some of them were more loosely connected to the research group than others. They spent less time at the research institute, contributing to a relatively fluid group structure.

The group developed mostly simple and conceptual models. That means that they were not as complicated as Earth System Models or Integrated Assessment Models concerning the number of processes included, even though some of them were also global in scale. Several models focused more strongly on social dynamics than on socio-ecological transformations, like opinion dynamics in social networks or coalition formation. Furthermore, the modelers explored and developed modeling methods and frameworks in various combinations: e.g. agent- based

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[1] *The Anthropocene* is a proposal to name our current geological epoch where humanity has become a major environmental force on all scales (e.g. Crutzen 2002). *Planetary Boundaries* (cf. Rockström et al. 2009, Steffen et al. 2015) express how "Humanity is […] overstretching the planet, not primarily by reaching the limits of resource availability, but rather by approaching or even transgressing the limits of anthropogenic disturbance absorption and ecological resilience – what the planet can absorb" (Palsson et al 2013: 6). A central difficulty is to determine the beginning of the Anthropocene (see Working Group of the Anthropocene 2018 and Bonneuil/ Fressoz 2017 for a critical account of the history of the term and competing imageries, see also Charbonnier 2017 on genealogies). A recognition of the Anthropocene ensues the need for new modes of interdisciplinary knowledge production also in the humanities and social sciences (cf. Palsson et al 2013, Latour 2017a, 2017b, for an assembly of various anthropological perspectives see Howe/ Pandian 2016).

[2] The study of complex systems was institutionalized in 1984 with the foundation of the Santa Fe Institute and it fascinates researchers from very different disciplines, e.g. in anthropology, it (resp. cybernetics) influenced the work of Gregory Bateson (e.g.2014) and Stephen Lansing (e.g. 2006). For an introduction see Mitchell (2011) or Holland (2014). See also part 5.1.1.

models (ABM),[3] co-evolutionary dynamics, approaches from game theory and artificial intelligence focusing on learning processes, simple economic models, multilayer networks or any combination thereof.

In general, they emphasized understanding of the inner workings of a model over comprehensiveness or complexity. While I was there, they had just begun to close that gap between simple and complex models with the development of a modeling framework building on what had been learned through working with the conceptual models. It contained "building blocks" for social, economic and ecological processes and entities as well as methods to link these. It was meant to be used by the wider scientific community to build models with.

While the PhD students often developed their own models, undergraduate and master's students mostly worked with already existing models – changing them and experimenting with model dynamics on various scales. Working on their individual projects, they were linked through supervising relationships, collective papers, shared offices and the weekly team meeting. Most of them additionally invested time into the development and testing of the modeling framework. Spatially, the group was distributed between two buildings and members often shared offices with colleagues from other research groups at their research institute.

3. Research Methods

While earlier studies of scientific knowledge practices often used "the laboratory" as a field site and thus, could rely on the productivity of participant observation (e.g. Latour/ Woolgar 1986, Knorr-Cetina 2011, Traweek 1992), the fluidity of the group, its spatial distribution, the use of the computer as the main site of model development and the fact that the models did not exist as clearly delineated object made another approach necessary.

Ethnography is a specific way of connecting theory and empirical material during the research process but also in the written product (cf. Hirschauer 2008, Ghodsee 2016). As a methodology, it encompasses methods of data gathering and data analysis (cf. Madden 2017: 28). This allows or even calls for an adaption of methods to the research field, research interest and a certain unpredictability of everyday life (cf. Bischoff 2014: 19f). Thus I primarily employed interviews to gather data and contextualized these with participant observations.

[3] ABMs are models of social processes, for example, where heterogeneous agents interact with neighboring agents following simple, local rules ultimately exhibiting some sort of unpredictable emergent phenomenon, e.g. segregation (cf. Schelling 1971): "agent-based models provide computational demonstrations that a given microspecification is in fact sufficient to generate a macrostructure of interest" (Epstein 1999: 42). These agents do not have all the information nor are they perfectly rational. Joshua Epstein was one of the first to develop such models on a larger scale (e.g. cf. Epstein/ Axtell 1996).

Over the course of almost three months I conducted two group discussions, two unstructured and 10 semi-structured interviews (cf. Schlehe 2008, Bernard 2006), each lasting between 45 and 90 minutes and transcribed word-for-word. Conceiving expertise as a relational status, I treated the interviews as expert interviews and prepared my interview guideline accordingly on the basis of initial observations, theory and methodological insights (e.g. kinds of questions to ask etc., cf. Kaiser 2014, Littig 2008, Powis 2017, Meuser/ Nagel 2009). I included two mental maps during the interviews in order to generate text during the interview, but also as visual data (a method adapted from urban studies, cf. Helfferich 2014): one of the subjective position within the research group and one of the model the person worked with. The fact that most of the interviews were with master's or PhD students of course influences my perception of modeling as a learning process and its description in this thesis.[4]

Participant observation (cf. Spradley 2011 [1980]) contextualized these interviews, e.g. in gathering first insights to develop the interview guideline. Most situations I was able to observe were supervisions of students and the team meetings. The meetings focused on organizational matters as well as content: work in progress and interesting papers were presented. Both kinds of situations proved to be very fruitful, but allowed only limited insights into everyday practices since they mainly served to discuss either results or problems of modeling. Nonetheless, especially the supervisions impacted the way I structured this thesis.[5]

A third way of gathering data treated the models as artefacts, taking a closer look at the code (when available), papers published about the models, visualizations of the models in the mental maps, and in plots as well as in conceptual loop diagrams used in presentations/ papers.[6]

I conducted my field work in three short cycles, leaving the field in between to gather some analytical distance and prepare the next phase. I began my research with three days of participant observation including two unstructured interviews, a spontaneous group discussion and gaining additional access to online platforms used by the team which added another unforeseen possibility of data gathering. Based on these initial observations, I finalized my interview guideline and returned to the field two weeks later in order to conduct semi-structured interviews,

[4] All direct quotes from interviews will be cited with the date and the lines in the transcript (e.g. 23/01/2018, 33-35). Quotes from my fieldnotes will be marked as such (e.g. fieldnotes, 22/02/2018). For the sake of readability, I will forgo para- or non-verbal qualifications in the quotes used here, however, should these bear meaning, I will mention it in brackets in the quote concerned (e.g. laughs, lowers voice etc.).

[5] While I already had some experience in conducting interviews, doing participant observation was much more of a learning process for me – from focusing my observations to writing jottings and formulating full field notes (Emerson et al. 1995) and finally, respectfully managing relationships with my informants between distance and proximity. Additionally, the lack of a fixed physical location made participant observation more complicated, so I had to search people out and be very intentional about being involved in everyday meetings and activities.

[6] In order to analyze this, the artefacts (visualizations, maps, etc) had to be turned into text through extensive memo writing, for which I adapted Adele Clarke's (2009) suggestions to my needs.

and participant observation whenever an opportunity presented itself. Afterwards, I left the field for two weeks in order to do a first analysis of my material and identify central themes. These became the basis for a workshop where I presented first tentative hypotheses and discussed them with the research group.[7]

Data analysis consists of a “distillation” and a “fattening” of data (Madden 2017: 149). New ways of theorizing empirical material and bifurcating theoretical concepts through data (cf. Strathern 2011, see part 4.) hopefully lead to abductive moments of inference in that process. Concretely, I approached my data with focused coding (cf. e.g. Charmaz 2006), building on the themes I had identified preparing the workshop. I employed various coding strategies in order to open up and de-familiarize my data (e.g. in-vivo coding, switching between a focus on emotions, descriptions, processes or binaries, coding with analytical concepts, and coding contrasting data, cf. Saldaña 2016).

This more open-ended process of coding was complemented and contrasted by a focus on what Marilyn Strathern has called “ethnographic moments” (Strathern 1999: 3f). These moments connect observation and analysis, data gathering and the writing process – they are “moments that the analysts cannot shake from their mind, and that continue to generate surprise as they are revisited in the light of new materials. They are images that refuse exhaustion.” (Street/ Copeman 2014: 25). These moments inspired the three longer stories introducing each section in the main part of this thesis. For these, I created two personae out of the different people I talked to, a student and their supervisor. First, because creating more than one persona allows me to capture model construction as a collective learning process and the importance of skill and expertise gained in years of practical experience.[8] Secondly, the stories are a means of anonymizing because in them I fused several situations together (cf. Markham 2012). The student could be BA, MA or PhD, the PhD supervisor, Post-Doc researcher or team leader, depending on the situation. The model in these stories is a model of opinion dynamics in social networks, and bears some resemblance to a model in the field.

[7] Both my supervisors actively participated in that workshop and its preparation, which led to very lively discussion.

[8] “The need to study skill comes from the fact that, while technological mediation ensures the global dissemination of standards, professional apprenticeship still constructs knowledge locally by training expert practitioners.” (Grasseni 2007:10). Even though this is not a phenomenologically oriented work, with this point I reference a notion of “educating attention” and “enskillment” involving all senses and personal apprenticeship as they were developed by Tim Ingold (2008), for example (cf. ibid: 8, see also footnote 52 on “skilled vision”). I will neglect an account of experience at this point but recognize its necessity for further research.

Another central aspect of anthropological research I already hinted at with the connectedness of empirical data and theory is a twofold concern with reflexivity. First, doing and writing ethnography confronts my own assumptions and concepts. Thinking through the empirical material always changes anthropological thought, too:

> Ethnography in its most daring undertakings (and as formulated from its very beginnings) has always been about the uncomfortably transformative mediated immediacy of the encounter, an encounter designed – however often it is diverted from that end – to bring us one step closer to an other's ontologized world (or worlded ontology) and one step further from our own –be that other an interpreting human or a sieving machine, a parasite or a meteorite, Maxwell's demon or Bayes' equation. (Kockelman 2013: 58)

Secondly, doing and writing ethnography means to be attentive to the conditions of anthropological research and in recognition of how what we do and write feeds back into and affects the world (cf. Law/ Urry 2011, Mol 2002).

However, one question concerning this encounter with models and a modeler's ontologized world remained. While ethnographic fieldwork usually implies a certain unfamiliarity with the practices, logics, discourses and experiences which provides a productive distance (cf. Hirschauer/ Amann 1997), in a highly specialized field such as this I kept and keep wondering how much I would have to master of the knowledge produced, the mathematics used, and the code written by my informants in order to make sense of what was happening in the everyday. Would a certain "interactional expertise" gained through the encounter be enough (cf. Collins/ Evans 2002)? For this master's thesis I think it sufficed. However, for future extended or collaborative future research, the question remains open.

4. Research Perspective

This work draws on literature from anthropology and Science and Technology Studies, as well as converging literature from the neighboring disciplines of philosophy and history of science. However, at this point I will not give a review of the literature but instead go into more detailed discussions when it is pertinent to my argument in the main part.[9] Here, I want to outline my research perspective and the gap this thesis begins to close.

Whereas a substantial amount of work has been done on physical models, especially climate models (Edwards 2013, Heymann et al. 2017, Hastrup/ Skrydstrup 2013, Guillemot 2007,

[9] Central lines of inquiry that became relevant are: collective aspects of modeling in a research group situating knowledge practices socially and historically (see part 5.1.1.), the value of simplifications for models as mediators (part 5.1.2.), the epistemic novelty of computer simulations (part 5.2.2) and finally the role of materiality when comparing laboratory experiments and computer experiments in relation to their epistemic value (5.2.3.).

2010a, 2010b, Sundberg 2005, 2010, 2016), few studies in social anthropology/ STS focus directly on knowledge practices in the modeling of socio-ecological transformations.[10] This constitutes my research gap in a very straightforward way.

A number of these empirical studies on various models have focused on individual steps in model construction, partly corresponding to the steps I follow in this account. Some of the steps may seem deeply generic. After all, what discipline does not do a literature review in order to narrow down a research question (see part 5.1.)? Which models are not used for experimentation and parameter runs (see part 5.2.)? And are visualizations and images not abundant in many other areas (see part 5.3.)? Of course, the point of this thesis is not to say that modelers perform parameter runs, for example. Rather, the point is to describe and theorize where, when, why and how they do this. In this specific research group, a modeler performs a bigger number of parameter runs with a model of a certain socio-ecological process on the research institute's central computer when the model structure is mostly clear – indicating a moment of closure in model development. Modeling a "complex system", she does this to identify parameter values where bifurcations happen – qualitative changes in model behavior – which she will then discuss with the whole research group.

The example illustrates, how modeling practices are situated – in a place as well as in a collective, materially as well as theoretically. Accounting for the situatedness of scientific practices and the implications for the resulting knowledge has been a central insight from anthropological STS (e.g. through laboratory studies, for a recent summary see Liburkina/ Niewöhner 2017) within which in turn this thesis is situated.

Another insight on which I build upon is a conceptualization of knowledge and knowledge production – through the construction and manipulation of a model – not just as situated, but as a material-semiotic practice. Originating in feminist science studies, material semiotics now covers a broad and diverse range of approaches ranging from Actor Network Theory (for an overview see Law 2011) to posthumanist feminism (cf. Haraway 1991) and the new materialisms (cf. Van der Tuin/ Dolphijn 2012, Barad 1996, 2003, 2012).[11] Material Semiotics attend to the entanglements of matter and meaning in practice and discourse:

[10] The work of Catharina Landström and colleagues with and on a participatory project of flood risk modeling could be read as such (cf. e.g. Landström et al 2011, Landström/ Whatmore 2014).

[11] First, there was a "diagnosis" of a hybrid situation, e.g. with the notion of the cyborg (cf. Haraway 1991) which was, taken up and developed by Latour (2015 [1991]). This was a response to various "turns" leading to a social constructivism unable to deal with materiality, or dealing with materiality only in one way, conceiving it as fully social: "Language has been granted too much power. The linguistic turn, the semiotic turn, the interpretative turn, the cultural turn: it seems that at every turn lately every 'thing' – even materiality – is turned into a matter of language or some other form of cultural representation. The ubiquitous puns on "matter" do not, alas, mark a rethinking of the key concepts (materiality and signification) and the relationship between them. […]. Language

> Discursive practices and material phenomena do not stand in a relationship of externality to one another; rather, the material and the discursive are mutually implicated […]. But nor are they reducible to one another. The relationship between the material and the discursive is one of mutual entailment. Neither is articulated/articulable in the absence of the other; matter and meaning are mutually articulated. Neither discursive practices nor material phenomena are ontologically or epistemologically prior. Neither can be explained in terms of the other. Neither has privileged status in determining the other. (Barad 2003: 821)

Donna Haraway first coined the "unwieldy" term "material-semiotic actor" to "portray the object of knowledge as an active, meaning-generating part" (Haraway 1988: 595) in processes of knowledge production in order to accommodate contingency of knowledge, reflexivity concerning one's own knowledge practices and "a no-nonsense commitment to faithful accounts of a 'real' world" (ibid: 579) .[12] Consequently, agency is distributed between human or non-human material-semiotic actors, constraining or enabling each other e.g. in making decisions about which processes to include in a model.[13] One example of this, to link this back to the field of laboratory studies, are experimental systems in the laboratory and related practices, e.g. translation and inscription (Latour/ Woolgar 1986). They are not just tools or "carriers of meaning", but independent actors in the construction of scientific facts (cf. Liburkina/ Niewöhner 2017: 183).

Models, like other experimental systems and knowledge, as "epistemic tools" (Knuuttila 2011: 263) are thus products of and actors in material-semiotic practices. Accounts of model epistemology need to take the not necessarily human (cf. Humphreys 2009), distributed "interpreting agent" (Kockelmann 2017: 180), into account (see part 5.2.1.).

Barad stresses the openended-ness of these practices whose outcome is "neither a matter of strict determinism nor unconstrained freedom. The future is radically open at every turn" (Barad 2003: 826). [14] Thus, decisions made in model development – be they pragmatic or after thoughtful considerations – bring with them a host of new possibilities, but also exclude others. Alignment is an ongoing achievement.

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matters. Discourse matters. Culture matters. There is an important sense in which the only thing that does not seem to matter anymore is matter" (Barad 2003: 802).

[12] Her case is the body as object of knowledge in biology and in feminist theory. She does not want to "loose the body" in a separation of sex and gender privileging either biological determinism or its social construction and extends this to social studies of science: "The same problem of loss attends the radical 'reduction' of the objects of physics or of any other science to the ephemera of discursive production and social construction" (Haraway 1988: 591f).

[13] The various material-semiotic approaches differ in whether it is symmetrically or asymmetrically distributed.

[14] Thus, several accounts of material-semiotics include ethics, as I have already hinted at in relation to reflexivity in research in part 3. Haraway (1988) appeals to feminists' responsibility for a better world and Mol (1999) calls our attention to ontological politics. Barad develops an "Ethico-epistemo-onto-logy" (Barad 2012: 100) with a stronger processual focus: "The ethical significance of agential realism, therefore, is not just in extending the idea that things 'could have been otherwise' to the ontological realm, but in conceptualizing the precise moments at which things congeal 'as they are' by understanding the processes through which particular material properties emerge and other realities are excluded from being." (Hollin et al. 2017).

I want to use the image of a bifurcation to illustrate this. “Bifurcation” as metaphor bridges modeling and anthropology. In physics, a bifurcation is defined as “a qualitative change in a family of vector fields, which depends on a finite number of parameters” (Boccara 2010: 85) and describes the point of change between two qualitatively different behaviors of the same model:

> A model whose qualitative properties do not change significantly when it is subjected to small perturbations is said to be structurally stable. Since a model is not a precise description of a system, qualitative predictions should not be altered by slight modifications. Satisfactory models should be structurally stable. (ibid: 32)

In the research group, performing a “bifurcation analysis” on a model to pinpoint these moments of change was quite common.

In anthropology, Marilyn Strathern introduced *bifurcation* as a way of working with concepts in order to generate better descriptions instead of more and more meta-theories removed further and further from the object of study:

> I dwell instead on the point of bifurcation, the moment of division, which need not take a binary form but very often does. It is the moment at which a distinction between terms could lead analysis down different routes. [...] [A] distinction between terms also maintains them in relation: they can still be found in one another’s company, to be repeated, at any juncture, later. We can repeatedly bring ourselves back to the point from which we started. In short, distinctions can keep terms from dissipating. (Strathern 2011: 90)[15]

This means that terms acquire meaning in relation and tension to other terms (cf. Ballestero forthcoming: 9), not in and of themselves. Bifurcations are differentiations to avoid working only with pre-existing categories (cf. Street/ Copeman 2014: 12). It means developing concepts out of the gap between language of analysis and object of study, or theory and description, which “establish unintuitive juxtapositions between newly differentiated things. These ‘things’ are neither objects nor theories because theories in this mode of knowledge production remain firmly attached to their objects […].” (ibid: 17). This is precisely what I attempt to do with the concept of “bifurcation” in this work.

Whereas Strathern focuses on the value of bifurcations in anthropological theorizing, Andrea Ballestero asks empirically “how people create bifurcations amidst the intense relationality of word, measurement and matter” (Ballestero forthcoming: 17) and characterizes them as unstable and temporary, even contradictory (cf. ibid: 30).

[15] Bifurcating is not separating, and is not working with opposites or dualisms, “a binary move simply allows an argument to take off in one direction by rendering another (direction of argument) also present” (Strathern 2011: 91).

To sum this up: A metaphorical use of bifurcation in the physics' sense emphasizes how at a bifurcation something becomes something else. On a more reflexive level, Strathern's conceptualization asks me to inquire in what ways bifurcating concepts could further anthropological theorizing and generate better descriptions of modeling. And following Ballestero, it becomes a method and an empirical question: Where are bifurcations produced in the modeling process through practices of alignment? What are the specific material-semiotic practices at such moments? And are some of them more crucial, stable and difficult to reverse than others?[16]

While I will be able to indicate some potential bifurcations, I will not be able to answer these questions in full in this thesis. Writing about and looking for bifurcations is a frame, a methodological focus which allows me to attend to moments of differentiation rather than continuities produced in material-semiotic practices.[17] The following is an account of such practices as practices of alignment.

[16] Relating to this last question, "degrees of irreversibility" (Callon 1990) will be introduced in the following part.

[17] I will return to this as a frame again in part 6.

5. Practices of Alignment in Model Development

My aim for this thesis is to develop a basic understanding of modeling from an anthropological perspective on knowledge practices through the focus on bifurcations in model development and the material-semiotic practices at these moments. This main part is divided into three sections, focusing on one particular modeling practice: simplification (5.1.), experimentation (5.2.) and visualization (5.3.). Each section begins with a short, introductory story of student developing a model, followed by two parts that correspond to the steps in model development as laid out above: Finding a research interest (5.1.1.), identifying basic processes in the system in question (5.1.2.), systematically assembling the model (5.2.1.), letting the model run multiple times (5.2.2.), observing interesting phenomena emerging within the model (5.3.1.), and looking for similarities between the model output and its target system (5.3.2.). With each step, I am going to delve a little bit further into one particular principle or practice of model development as it was made explicit by the modelers and contrast or elaborate it with insights from STS.

My thesis is that model development can be described as a process of iteratively and continuously aligning model formats – mathematical model, computational model, etc. - through practices of simplification, experimentation and visualization. This alignment takes work and the different formats still never align completely – and they do not have to in order for the model to work. Each of these formats sets slightly different accents, is a part of different knowledge practices and needs as well as explains the others. This means that this work is less about the model as a final product, and more about the process of model construction where at different points the model becomes more and more final in the sense that it becomes more difficult to change in its basic structure.

With "model format" I want to capture the different ways in which "the model" is enacted in various practices. Talking about "the model" is, in fact, a simplification on my part. Especially at the beginning of my project I grappled with pinning down "the models". Soon I learned that models exist in a variety of ways and forms: most obviously, as mathematical equations and computer code, but also as visualizations, plots of model output, descriptive text in a paper about the model and as the modelers' mental models, which I elicited with the mental maps in the interviews. I take the notion of "format" from philosopher of science Marion Vorms' elaboration on how formats of model presentation matter for theorizing in scientific practices, specifically for inferences made by particular model users with particular epistemic interests and skills.[18] With this she moves beyond discussions about the representational relationship between model and target system to a relational account of format, model user and inference:

> Therefore, the notion of format is fundamentally dynamical in character; the format of a representation has to be defined (beside the perceptual properties of the representation and a minimal set of construction and interpretation rules) in reference to a particular situation, involving a particular agent, with particular skills, theoretical commitments, preferences, reasoning habits, as well as interests and intentions in the particular inquiry in which he is involved. (Vorms 2012: 267f)

Whereas she focuses on visual and spatial aspects of diagrams, tables and equations, I will also include mental models and computer code. Especially for that last format the material affordances of the technologies employed will become relevant as well, e.g. computing power and programming languages (see part 5.2.).

I want to illustrate alignment with the metaphor of the telescope or binoculars. One of the modelers used it in a mental map to illustrate how each modeler only sees parts of the world they then translate into a model. I use it as metaphor for the model. Like lenses in the telescope, the different model formats have to be attuned to each other in order for the modeler to see through it and to work with it (I will get back to the notion of the model as a tool in part 5.2.1.).

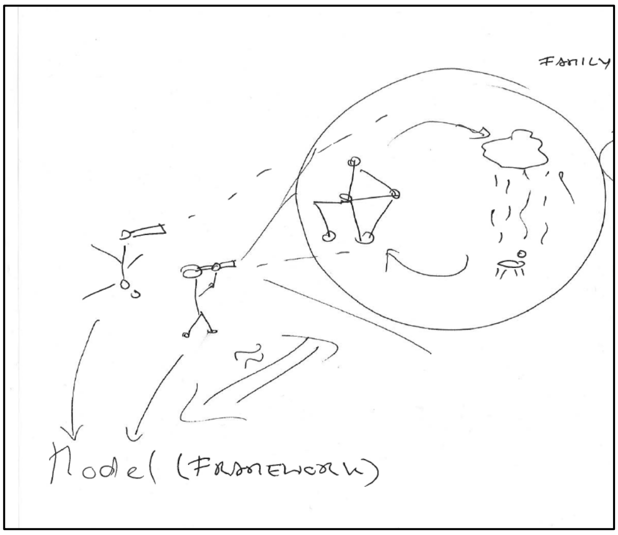

Figure 2: Mental map of the research group (31/01/2018). Reproduced with permission.

Alignment was conceptualized within the framework of Actor Network Theory (ANT). Michel Callon (1990) proposes a set of analytical tools to analyze techno-economic networks in order to understand how these networks evolve – how heterogeneous or even incommensurable elements are made to work together, sometimes irreversibly. Alignment describes the successful outcome of such a process, which

> generates a shared space, equivalence and commensurability. It *aligns* […]. When there is 'perfect translation', A and B speak in exactly the same way about themselves, about one another, and about the intermediary that links them together. There is total equivalence with no ambiguity. (Callon 1990: 145, emphasis in the original)

Concretely, model formats are aligned with each other to become "the model". But contrary to Callon, I stress that model formats never align completely and do work of their own accord because they are bound up in different knowledge practices. In modeling, they act upon each

[18] Her main examples are different equations describing a certain problem in classical mechanics.

other and the modeler, which in turn causes iterations and feedbacks. Thus, alignment is a continuous achievement, it takes work (cf. ibid: 148).

Some translations or alignments are more stable than others, their "degree of irreversibility"[19] depending on "(a) the extent to which it is subsequently impossible to go back to a point where the translation was only one amongst others and (b) the extent to which it shapes and determines subsequent translations" (ibid: 149f).[20] Bifurcations in model development may therefore entrain different degrees of irreversibility, something I will highlight whenever possible.

Despite the iterations mentioned above, the outline of the following parts follows a quote where one of the modelers sets out the main steps of model development in an ideal, linear sequence:

> At first I identify a system I am interested in. And then I consider… or, then you investigate […]: what are the basic processes happening in such a system. […]. And then you think about, ok, how can I approach this systematically? […] And little by little you assemble the model, turn certain effects off, and you let it run again and again and then at first you try to observe interesting emergent phenomena. And in the end, ideally, you are able to observe structures that you can also observe in the system you really want to study. And then you check, whether the model you built describes in any way what you are seeing there. (31/01/2018, 60-73)

Towards the end of the same interview it became clear that an iterative, playful, creative, trial-and-error approach is much more central to model development than this initial quote would lead me to believe. Interestingly, this is introduced as an ideal in itself, a principle of model development at the end of the same interview, after the modeler had demonstrated the implementation of a new idea:

> This is still very improvised. But it's basically a first playful approach.
>
> I: But isn't this playing around important?
>
> Exactly (with emphasis), yes, it is. But of course, this is the way of working we have learned that is central to physics. To approach such questions conceptually and then simply try stuff out and iterate and adapt it until it converges to a point where you can say, ok this is a model that is still generic enough but also possible to interpret without having to build too many bridges. (31/01/2018, 486-595)

In general, the process of model construction oscillates between a linear sequence of steps and an iterative, looping repetition of a few steps at particular moments of misalignment. Thus, iteration can be understood as linking the practices I will focus on in the following.

[19] Differentiating "degrees" implies that reversibility is, in principle, still possible (cf. Callon 1990: 159). Ultimately, unidentified, high degrees of irreversibility could potentially lead to lock-in's in model development as Marisa Beck and Tobias Krueger warn concerning the lack of research into the social-scientific coproduction of Integrated Assessment Models (cf. Beck/ Krueger 2016: 638).

[20] For Callon, alignment is a result of translation. Translation means that "A defines B" (Callon 1990: 143) and in the framework of Actor Network Theory every entity A is the result of previous chains of translations inscribed in different material media, texts, publications, skills, technical objects etc (cf. ibid). Like the practices I explore her, translation could also be understood as a practice of alignment. In ANT it is a far more general concept of relating heterogeneous actors (cf. Schulz-Schaeffer 2017: 276f), which is why Callon (1984) first named ANT a "Sociology of Translation". Translation and inscription are central concepts in ANT, esp. in laboratory studies where they characterize scientific practice (cf. Latour/ Woolgar 1986).

5.1. Simplification

When the student decided to do his next project with this research group, he had already been working with another group at the research institute for other projects. He decided to change groups because this research group was closer to his personal and scientific interests. The idea for the model he would base his thesis on originated from his supervisor. Given current debates, he was interested in digital social networks and opinion dynamics. With this, the type of model was more or less decided. It would be a network, where the nodes, or agents, stood for people linked through "friendships" in a digital social network. Over the course of the following weeks, research question and model structure developed hand-in-hand. That digitalization brought with it deep changes in opinion dynamics seemed almost self-evident to the student. But to pinpoint what concretely had changed turned out to be more difficult than he had anticipated. So, he did extensive literature research and looked at already existing models, trying to figure out what had not been done yet and whether he could adapt one of those models. Because the model subject had little to do with physics and more with the social sciences, he read a lot of social science literature, too, something which required a different way of reading: "reading the social science stuff was super interesting. That is definitely something one could pick up, to learn that it is not about being right or wrong, but that two opinions can be discussed next to each other. And one doesn't necessarily have to be wrong. It's just that I have to pick the one I like better", he explained with a laugh.

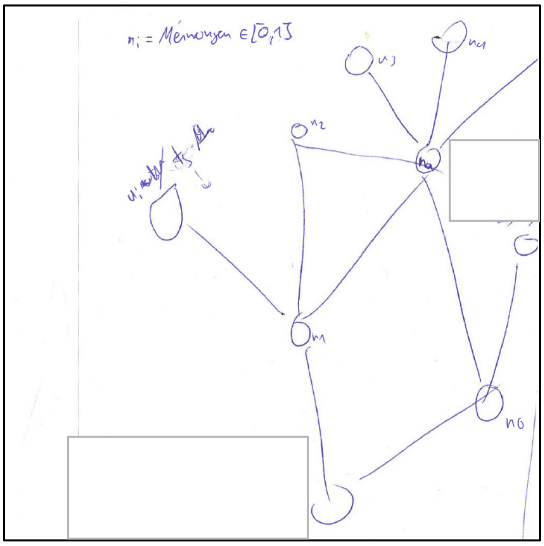

Figure 3: Extract from a mental map of a model (06/02/2018). Reproduced with permission.

Still, it was very difficult to decide what he wanted to focus on, and to identify the relevant processes. Soon he had an idea, a hunch, but at first he dismissed it. After some more reading, he decided to develop his own model based on that original idea: "I felt like doing something that hadn't been done before and none of the existing models really fit – even though they were really beautiful. Then I remembered what I had thought about before which actually seemed to be relevant now."

But even after that, decisions about other processes and the agents' characteristics were not easily made, leading to more research and the decision to follow the "mainstream" in *the social science literature: "I had been researching the issue for a really long time and a lot had already happened in the area. There are different approaches with either continuous or discrete opinions. You know, where you have a set number of possible opinions and not a range with an infinite number of possibilities? It was quite difficult, but after a while I decided to use continuous opinions because I felt that most social scientists worked with that. After all, I as a physicist am working on something from the social sciences. And discrete opinions are often used for mathematical-analytical descriptions."*

5.1.1. "At first I identify a system I am interested in".

In this part, I will situate the "I" that defines a research interest and narrows down a research question within the disciplinary perspective and the collective practice of the research group. Defining a research interest could be motivated by personal interest as the title of this part indicates or by current societal discourse, as in the introductory scene. Another fundamental choice and bifurcation in this stage of modeling was the one between the modification of an existing model and the development of a new model, which I will elaborate on now. I will focus explicitly on simplification in the following part on literature research.

The reasons for working with an existing model vary: Existing models are broadly accepted in the academic community, they help to test a modeling framework, and they have an educational purpose as a more manageable task for undergraduate students. Still, the very first day I spent with the research group, they discussed that using an existing model means to adopt simplifications made by other researchers and contingencies coming out of other development contexts. The models developed in the group that had been used by several generations of researchers were a connecting element, generating, solidifying and transmitting ideas bigger than the individual researcher involved at a specific time. They modified and were modified. When existing models were used for undergraduate students or graduate students to learn through working with it, they shaped that learning process. Simultaneously, the students gained insights that could enhance already established models. While people flowed through the group, as sediments of collective efforts, the models stayed and connected the group across time. Published as open source code and in scientific papers (e.g. Earth System Dynamics, Nature Climate Change, Physical Review), the models represented the research group to a bigger scientific community.

In studies of scientific practices, collectivity has been a central issue from the very beginning. Already in 1935, the microbiologist, immunologist and philosopher Ludwik Fleck developed the idea of a "though collective" sharing a "thought style" on the empirical basis of observing the development of a new test for syphilis. The concepts grasp the interlacing of collectivity and knowledge. He introduces them with the following observation:

> Such historical and stylized relations with knowledge show that an interaction exists between that which is known and the act of cognition. What is already known influences the particular method of cognition; and cognition, in turn, enlarges, renews, and gives fresh meaning to what is already known. Cognition is therefore not an individual process […]. Rather it is the result of a social activity, since the existing stock of knowledge exceeds the range available to any one individual. (Fleck 2008 [1935]: 38)

He then turns this into an empirical research program: “the three factors involved in cognition – the individual, the collective, and objective reality (that which is to be known) – do not signify metaphysical entities; these too can be investigated” (ibid: 40)

This use of already existing models illustrates how the research group can be understood as a thought collective and how modeling is not independent of specific thought styles. Extending the original concept, the research group as a thought collective is made up by people as well as models.[21] Thought styles do not just extend synchronically across a group, but they also relate researchers diachronically (cf. ibid: 39). As such, the thought collective of the research group is situated in a history of thinking about systems from world systems and cybernetics to complex systems. This is entangled with histories of certain types of modeling techniques and technologies (e.g. computer simulations, see part 5.2.1.).[22]

An early example of accounting for collectivity in modeling is Brian Bloomfield’s (1986) work on the beginnings of world system models in the 1970s at the Santa Fe Institute and implicit theories of the behavior of social system. Influenced by the sociology of knowledge at the time (the Strong Programme, e.g. Bloor 1991 [1976]), he focuses on the social construction of system dynamics, dedicating the main part to the cosmology of modeling.

Simon Shackley, a biologist and social scientist working on the sociology of climate change, also focuses on the social embeddedness of modeling. Having worked at three different climate modeling centers, he distinguishes two “ideal-types” of climate modeler's “epistemic lifestyles”[23] that situate modeling strategies, decisions and assumptions affecting model output more or less decisively: climate seers and climate constructors.

[21] See Latour (2015: 11) for an extension of the notion of the collective beyond human actors.

[22] I will not go into detail but see e.g. Weart (2010), Edwards (2013) on the development of Global Circulation Models, Gießmann (2008) on graph theory and network visualization, Kunnttila/ Loettgers (2012) on the Lotka-Volterra Model that served as a computational template (see below) for further modeling efforts esp. for non-linear dynamics, Galison (1996) for the Monte Carlo Method and the works of historian Amy Dahan-Dalmedico (2001, 2010a, 2010b) on the development of climate and earth system modeling.

[23] “By epistemic lifestyle I mean the set of intellectual questions and problems, and the accompanying set of practices, that provide a sense of purpose, achievement, and ambition to a scientist's work life […]. Additionally, an epistemic lifestyle includes the social networks and connections through which scientists organize their individual and collective work. [...]. The factors that give rise to different sorts of epistemic lifestyles include many of those identified in sociology of science: disciplinary concerns and practices; institutional culture, structures, and

The main difference between these two lifestyles consists in their take on model complexity. Climate constructors aim at building a comprehensive, complex model of the climate system irrespective of a specific application for its own sake. Climate seers use models as tools for specific applications and emphasize the importance to understand and predict changing processes in the climate system (cf. Shackley 2001: 114- 116).[24] Concerning model construction, the latter focus on understanding model processes before adding components which make the model more complex:

> Seers tend to be more cautious about changing a model that 'works' and is reliable. An incremental reductionist strategy is adopted in changing the model, with the influence of each model component analyzed separately. Only after the model is well understood is it appropriate (in the climate seers' opinion) to add complexity. Additional elements of complexity are then added one at a time, and their implications for the rest of the model are analyzed. (ibid: 119)

I observed a comparable attitude toward models in the research group. They stressed repeatedly how important it is to understand what is happening inside their models, e.g. how the different parameters and processes influence each other. This was often expressed in opposition to building models that aim to be realistic or predictive but become too complex and complicated in the process, so that it becomes impossible to say which parameter change is responsible for which change in model output.[25] They defended that position within their academic community and it was an important part of the way they defined their work and themselves.[26]

Other studies echoing Knorr-Cetina's comparative analysis of (2011) epistemic cultures in particle physics and molecular biology also illustrate the importance to consider models, modelers and their practices situated within their research group, institution and discipline to understand how this frames modeling decisions and knowledge.[27]

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processes; policy and 'user' relationships and support; funding sources; peer-group concerns; career trajectories; and so on. […]. Such lifestyles are, of course, only 'ideal types', evident to different degrees in any individual modeler or organization" (Shackley 2001: 114f).

[24] For Shackley, the recognition of epistemic lifestyles serves to understand the diversity of scientific practice, especially in dealing with uncertainties in climate modeling. He concludes that transparency about this could preemptively deal with critics of climate change that try to play e.g. "climate constructors" off against "climate seers" (cf. ibid: 130-131)

[25] This ultimately leads to a form of confirmation holism: "We have argued that complex simulation models in general, and climate models in particular, are – due to fuzzy modularity, kludging, and generative entrenchment – the products of their contingent respective histories. [...] As such, climate models are analytically impenetrable in the sense that we […] are likely to be unable to attribute the various sources of their successes and failures to their internal modeling assumptions. Climate models in particular, and complex models in general, exhibit a form of confirmation holism." (Lenhard/ Winsberg 2010: 261)

[26] Dahan-Dalmédico (2001) mentions how the difference between simplified models for understanding and comprehensive models for prediction is at the heart of computational physical modeling since its beginnings.

[27] Heymann, Gramelsberger and Mahony characterize "cultures of prediction" in atmospheric modeling by the "social role of prediction, significance of computational practices, domestication of uncertainty, institutionalization and cultural impact" (Heymann et al. 2017: 18-36). Putting a technological development center stage, Mikaela Sundberg follows Turkle (1997) and contrasts "cultures of calculation" and "cultures of simulation" as collective ways of relating to computer simulations in order to better understand their use in modeling practices. While cul-

When a modeler decided to develop a new model, and the research question was clear beforehand, it could determine the model structure, as one of them remembered: “The process was that I already knew what the model was supposed to do in the end”. Enumerating the four components of his model he mused: “I don’t know whether the rough structure could have been any different. I think that that was predetermined through my expectations and through me wanting to keep it as simple as possible” (01/02/2018, 124, 180-183). His colleague seconded that “If you begin to model something you already have something. You have a certain research question and that already structures a lot of the model, the variables, the processes to include and those you neglect” (16/01/2018a, 234-237).

Alternatively, narrowing down the research question can go hand in hand with developing the basic model structure in a more iterative process as it happened in the introductory example. However, even a new model may involve already existing “building blocks”, or “computational templates” (Humphreys 2004). These “are genuinely cross-disciplinary computational devices, such as functions, sets of equations and computational methods, which can be applied to different problems in various domains.” (Knuuttila/ Loettgers 2012: 3). For example, when writing in the programming language Python, the modelers use so called “Python Packages”.[28] In their recount of the history of the Lotka-Volterra Model[29] becoming such a template e.g. for non-linear-dynamics, Knuuttila and Loettgers emphasize how the use of these templates leads to a productive tension:

> Consequently, there seems to be a tension inherent in the modeling practice that is due to scientists' aim to depict the basic mechanisms underlying some specific phenomena in a certain domain and the general cross-disciplinary templates used in this task. This tension, we suggest, is a central driving force of modeling practice being productive in different ways. (Ibid: 5)

How the mentioned underlying mechanisms and phenomena are identified is the topic of the following part.

5.1.2. “Then you investigate […]: what are the basic processes happening in such a system?”

Taking “then you investigate” as my cue first, I will briefly describe this investigation before I will focus on “basic processes” and elaborate on simplification as a practice of alignment.

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tures of calculation are characterized by a more serious attitude towards modeling, an in-depth focus on mathematical models, write the model code themselves and focus on the simulation of reasonable scenarios, cultures of simulations tend to use existing computer programs, show a more playful attitude in exploring model simulations and focus on extreme, yet interesting model scenarios. Operationalizing these cultures she employs a set of dichotomies – surface/ depth, play/ seriousness, extreme/ reasonable – to analyze typical activities and situations of modeling in meteorology and astrophysics. She shows how these cultures manifest side by side and relative to specific situations rather than as clear cut collectives.

[28] Python Packages are a way of organizing modules of code that serve a specific purpose, e.g. scientific programming, and can be used across different applications in order to limit the amount of code one has to write oneself.

[29] The Lotka-Volterra Model is a model of predator-prey dynamics in biology.

The main "site of investigation" was the existing literature, inside and outside of physics.[30] As a bifurcation, the choice of literature brings with it certain degrees of irreversibility when it is guided by what is already used in a research group and builds on collective experience and skill in modeling. Using tried and tested elements out of the literature is also a matter of feasibility. This investigation happened iteratively together with the development of the research question and the model structure. However, knowledge gaps concerning social processes had to be dealt with through the review of literature outside of physics. Decisions building on that literature were usually oriented towards the standard approach used in the respective discipline. The willingness to engage this body of literature outside of physics is another moment of bifurcation. Here, as indicated in the introductory story, the physicists were confronted with other ways of doing science, of knowing as well as of dealing with uncertainties and ambivalence (see also part 6.2.). This knowledge had to be mobilized, e.g. through simplification, in order to work in their own disciplinary context:

> What I am doing is also a master's thesis in physics, and I want to see how I can embed my problem in the context of network models in physics. Also, when physicists build network models they are usually less complex, because you want to be able to still understand them. And when social scientists build models my impression is that they want to have them as realistic as possible. But then it becomes harder to understand it. And I think the model I am working with is still quite complicated for a model in physics, a lot of processes are relevant. And yes, it would be cool if it would be possible to further simplify it. (16/01/2018a: 124-132)

With this, I come to the "basic processes". For the modelers the basic processes of a target system have to be describable numerically, quantifiable and measurable. And, as I already mentioned, the modeler laying out the linear sequence of model development insisted that the model should still be as simple as possible. In a first, central simplification, the question becomes: What are characteristic *key figures* of a phenomenon?

> You want to approach the issue step by step until you reach a point where a minimum of assumptions results in an optimum of convergence. And then you generated an understanding of processes […]. And the first thing you have to do is to figure out how the system you want to understand can be described. And we as mathematicians and physicists try to describe it numerically. And to quantify. To figure out the key figures characterizing the object of our interest. (31/01/2018: 76-86)

Later in the interview he described this very aptly and pragmatically as cost-benefit-evaluation, but without becoming more concrete concerning simplifications:

> And of course that is always an estimation. You can also do it the other way around. You write down an equation and realize, oh God that is really complicated. And you take a closer look and see 'if this term wouldn't be in there anymore I could solve it' […] and of course, there is a lot of theory of how you simplify such things while at the same time controlling the error, we call it deviation, from the exact. And that is of course a cost-benefit-evaluation. […] a large portion

[30] Very rarely a model builds on statistical data or psychological experiments, for example.

> of research questions I want to answer I can answer well with an approximation. (31/01/2018: 147-155)

Another modeler added that they are not interested in special cases. This and the need for simplification was illustrated by the rich imagery used in the interviews to describe the relationship between model and world: they are skeletons, tools, or metaphors, symbols, binoculars and parallel worlds. Still, models vary in degree of abstractness. And sometimes it may take active work to keep model and world apart:

> Finally, what you usually do, is to take the model and say: this is how things are. You have to be very careful not to do it […]. You always have to remind yourself: 'it is not real', and you simply count the mistakes it contains. Or the areas where it does things differently than nature or contains simplifications. (16/01/2018b, 210-216).

Simplification is a crucial aspect of modeling for at least three reasons: as a matter of aesthetics,[31] to keep models understandable, and easily computable as well as mutable in order to align model formats. For philosophers Mary Morgan and Margaret Morrison, their capacity to simplify is what grants models partial independence from theory and world and therefore enables them to mediate between them:

> As a matter of practice, modeling always involves certain simplifications and approximations which have to be decided independently of the theoretical requirements or data conditions. […]. The crucial feature of partial independence is that models are *not* situated in the middle of a hierarchical structure between theory and world. (Morrison/ Morgan 1999: 16f emphasis in the original)

The relationship between models, theories, reality and experiment has been debated at length in the philosophy of science (summarizing see Frigg/ Hartmann 2012).[32] Conceptualizing "models as mediators" was a central intervention into these discussions (Morgan/ Morrison 1999). This acknowledges models as "autonomous agents" and "instruments of investigation" (Morrison/ Morgan 1999: 10).

This independence arises in the way they are constructed, function, represent and make learning possible. Models derive neither entirely from theory nor data, but from both and more – this mixture of elements grants them independence (cf. ibid: 14). Once they work, models can be used for multiple purposes such as theory construction and application or experiments (cf. ibid:

[31] Discussing a model with his supervisor, one modeler exclaimed "but it is not beautiful to introduce another parameter into the model" (fieldnotes, 29/01/2018). Recognizing something as beautiful or "interesting" (see part 5.3.1.) is a skilled practice situated within a discipline.

[32] Deliberations on modeling in general in the philosophy of science are connected to older questions on the epistemology of experiments and the role of fictions in science (vgl. Contessa 2009, Suárez 2010, Toon 2010, Winsberg 2010). Some focused on practices early, Morgan and Morrison mention Hesse (1966) and Gibbard/ Varian (1978): "Their treatments, emphasizing the physical characteristics of models [...] attempt to address questions concerning the interplay among theories, models, mathematical structures and aspects of creative imagination that has come to constitute the practice we call modeling" (Morgan/ Morrison 1999a: 7). In part 5.2.1. I will discuss the epistemological novelty of computer simulations of models, in part 5.2.2. models, experiments and materiality.

18ff). And because they involve representations of theory and of data, they are investigative instruments within a specific purpose or context of investigation:

> [W]e often use many different kinds of models to represent a single system. [...]. We do not assess each model based on its ability to accurately mirror the system, rather the legitimacy of each different representation is a function of the model's performance in specific contexts. (ibid: 28)[33]

Lastly, learning happens through building and manipulating the model (cf. ibid: 30- 33).
Seeing simplifications and simple conceptual models as an epistemic advantage, not just a practical necessity was a guiding principle of modeling in this group. At least in these first steps of model construction, the model itself was at the center of the inquiry.
Looking at simplifications from anthropology and STS I heed two insights that cautioned me against rushing too quickly into an evaluation or even judgement of simplification as "reductionist" or "essentialist".[34] First, the *centrality* of simplification as a productive epistemic practice for model development needs to be acknowledged, as the anthropologist Anders Munk described it in his account of learning how to model flooding. In fact, the efficacy of modeling rests on simplification. In learning how to model he learned how to make simplifications meaningful:

> Tied in and related through a set of formalisms the composites of the model are made to do something meaningful. [...] I know from the hydrological cycle on the flip chart and from the perceptual sketches of the environment in my field notes that things are missing. Everything is not here. But there are absences for which I have been taught to account – the co-conspiring parts of an arrangement to make the model speak on behalf of a world much wider than itself. As our instructor pointed out, there would be no point in the models if they were replicating in every particular the things modelled. The point in models, in other words, is their transformation of the things they model into simpler forms. It is this specific and purposive process of doing away with things which I have been able to engage in through my apprenticeship. (Munk 2013: 158)

Secondly, *framing* matters for simplifications. In their study of a participatory hydrological modeling project Catharina Landström et al. highlight that even though all models simplify, the decisions about what to leave out depend on the framing of the problem rather than any independent, objective set of criteria. Most model development is done with framings that tend towards the general rather than the particular (cf. Landström et al. 2011: 1631).
Even though that was also the case here, I was not able to delve far enough into the various model contents as to do justice to the simplifications within their frame. It proved very difficult

[33] With this, they move beyond questions of representational adequacy to a more relational perspective, much like Marion Vorms did with her focus on model formats (see part 5.). I will come back to the question of representation and of models as investigative tools when I discuss models and experiments (see part 5.2.2., esp. foot note 49).
[34] But I do want to maintain that simplifications deserve more scrutiny, because they are also normative. "Models of" can become "models for" wittingly or unwittingly (cf. Geertz 1973: 93-94), and have an effect in the world. And the use of models in this research groups to generate "narratives" is right in-between that (see part 5.3.2.).

to get close to concrete simplifications. I already noticed this at my very first day with the research group, when during a discussion at a team meeting I inquired after ways of simplifying. While some of them tried to explain it with a concrete example I was not able to follow at that point, somebody else said “through abstraction and reduction”, which was still quite unspecific, even redundant. This remains an issue for further inquiry, but understanding simplifications was also not the central aim of this work, especially since from a practice theoretical perspective, simplification is more than just following explicit rules of how to simplify process X into value x’. Rather, I wanted to gain a basic understanding of the process of model development as a whole in this particular research group. This is why for now I have to write about simplification as a practice of alignment on a more general level.

To sum up: Simplification is central to model development, it is framed by a research purpose and like the literature review and the formulation of a research interest it is situated in a thought collective. In the following section on experimentation in constructing and manipulating the model, I will show how it is indeed not just an application of certain rules I could potentially uncover with more research. As a part of the whole process of model development, simplification is entwined with other practices. When the model is assembled by aligning the different model formats, e.g. mathematical to computational model, simplifying may again be required – but for more pragmatic reasons.

5.2. Experimentation

> *After the model structure was clear the student tried to include a certain aspect of somebody else's work into their model of opinion dynamics. A paper had already been published on it and since the code is open source, he had access to that as well. However, there was one particular equation he kept stumbling over so he asked one of his supervisors for help: “I don't really understand this equation. I mean, it is supposed to be a conditional probability, but then sometimes it becomes bigger than 1. That actually shouldn't happen with a probability, right?” The supervisor agreed: “No, it shouldn't.” He took a closer look: “Maybe there is a typo somewhere? Do you already have results from trying to integrate that into your model?”*

The student showed him two plots with his laptop. Each of them displayed two graphs, one in green and one in pink. In the first plot, they both ran jaggedly downwards from the top left corner to the bottom right corner. The second plot looked quite different, with strong oscillations in the lower half of the quadrant. This astonished the supervisor. For a moment, he stared at the screen in silence, then shook his head: “That is strange. Crazy. I mean, continue your analysis and if it comes up more often, it might indicate a new feature.” They talked about something else for a while, then the supervisor leaned forward again, still bewildered: “This is really interesting”.

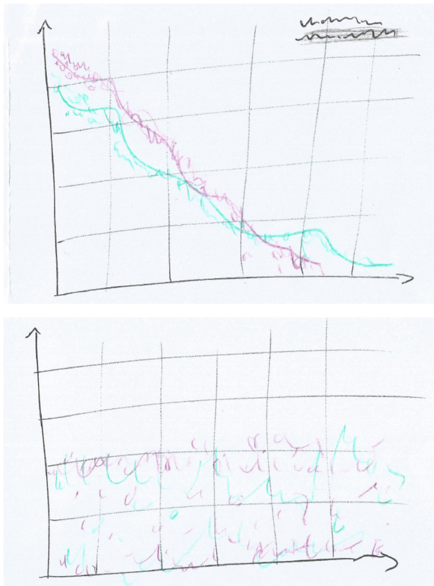

Figure 4: Sketches of the plots (fieldnotes 18/01/2018).

The student then called up a third plot: “This is where the conditional probability actually was bigger than 1”. One graph rose continuously, while the other rose at first and then fell again. The supervisor looked again at the equation in the paper: “I thought I had understood it, but apparently it can actually be bigger than one. So, maybe if we look at it as if it was a ratio, not a probability... you know, simply as a comparison of values?” The student again called up the plot so that now both equation and plot are visible next to each other on the screen of his laptop. The supervisor nods: “Yes, that's it! That makes sense”. It appeared as if the problem had been solved, the knot disentangled. Plot and equation made sense of each other. But the student was not quite satisfied: “Then why is it that in the code there is a (-1) behind the equation?” He opened another window on the screen showing the code. The supervisor leaned so far forward to take a closer look that his glasses threatened to slip off his nose: “Funny. I’ll have to think about this a bit more in depth. We will get back to that later. For now, keep implementing the model and I will try to understand it. Maybe I can get a hold of the person who wrote the code, I think I've met them at a conference last year. Also, sometimes that programming language requires you to add or subtract a one so that it is all correct in the end. But all in all you are doing very good work and qualitatively the model output seems to be more or less correct.” He explained the next practical steps: “Soon you should begin to do ensemble runs. Some of the oscillations and jumps in the plots will even themselves out then. And even if they don't, your work could contribute to an understanding as to *why that is. Sometimes, oscillations, like slow fluctuations, are normal in a state of equilibrium." And he moved his hand in a wavelike motion. Then they kept discussing certain parameters, expected results and external forcings in the model. The supervisor drew possible shapes of graphs in the air. Another form of embodied modeling, I thought, just analogue and based on experience.*

The problem seemed to have been solved, the alignment was successful. But, three weeks later, when the student and the supervisor met again, the student brought up the equation again. He wrote it on the whiteboard pointing to the last factor in the equation: "I have tried to work with the equation, but I really don't understand where this factor comes from and what it is supposed to do." The supervisor took the pen, erased the factor and notated it in more detail as a fraction. Then he wrote Bayes' Theorem underneath it, the basic form of a conditional probability, and tried to translate one into the other, drawing arrows between parts of the two equations. But it did not quite add up, even though a resemblance seemed to be there. After more than an hour of discussion, they reached a provisional, but not satisfying understanding. Then the supervisor reread the part in the paper where the equation was explained: "This is not really helpful. This whole thing is really counterintuitive. I need some more time to dig deeper into this. It is not necessarily wrong, but it is certainly not explained well. Maybe I should ask some colleagues about it as well." He then summed up the problem again for the student to write it down and asked: "Would you actually be able to implement this, just for fun, to see what happens?" The student hesitated, then shook his head: "No, I think that would be too much effort. I'll wait and see what else we'll find out about it".

5.2.1. *"And then you think about, ok, how can I approach this systematically? […] And little by little you assemble the model."*

Assembling the model is not a straightforward or deterministic process. In this section, I will focus on the alignment of mathematical and computational model formats, especially the work of coding, which is of course not completely detached from mental models and visualizations, as the scene above already illustrated.

Aligning equations and code usually happened when processes have been identified and simplified so that the basic model structure is clear, but programming could also serve experimental purposes following a vague idea or a hunch that something might be interesting (an example can be found in part 5.3.2). Alignment at this stage of model development may still influence the assumptions and simplifications of the central processes, both because it tests and disproves some of the assumptions (see part 5.2.2) or because, more pragmatically, not every idea is easily translatable into equations or code (see below).

Experimentation as a practice of alignment is more strongly linked to iteration than the other practices. And through parameter runs understood as experiments with the model (see part 5.2.2.) it also connects modeling with experimental practices in the laboratory. This first part therefore is about model construction and iteration, the following part describes model manipulation more explicitly as an experimentation process. I still chose to frame this as experimentation rather than "iteration" or something comparable, because no clear rules exist for constructing a computational model, neither for an iterative nor a linear sequence of steps.

A few words on iteration: when I asked in the interviews what modeling is like in the everyday, one answer was: "usually it is very profane, I sit in front of my computer and produce some random code or shove equations around." The modeler who said this then continued to explain with more excitement, what modeling is for him on a more abstract, ideal level. I wondered why it then ends up being so profane and he described the concretization of a modeling idea as "fiddling around [rumgefrickel]" (23/01/2018: 22-35). During the interviews I've conducted, the modeling process was described as iteration frequently and explicitly, though often nuanced differently: either as a tedious fiddling around or more positively as playing around. The latter denotes creativity and freedom, where surprises happen and "interesting" things become visible (see section 5.3.). Iteration also has a collective aspect, because the research group and the wider academic community was used as a corrective when work is repeatedly presented and exposed to criticism.

The scene at the beginning of this section illustrates how the model formats work together in order to make aspects of a model understandable and transparent for others. It focuses on a mathematical model, and on the needed collaboration of equation and text, but also shows how code and a plot of model output come into play: The plot suggested that something was not as it should be. In that sense, it illustrated a situation of misalignment, disturbing the smooth process of model construction.

To give some epistemological and theoretical context, in the first part I discuss the question what is new about computer simulations in modeling in order to clarify the specificity of this model format. In the second part I will go into the issue of comparing computer experiments and laboratory experiments and the question whether epistemic privilege of the latter is bound to material continuity between experimental system and target system.

Before I elaborate on the epistemology of computer simulations and writing of a model code, I want to emphasize that advances in mathematics crucially impacted modeling, just as did the more obvious technological changes like the computer, even though it may seem to be a less "flashy" format.[35] I will not go into details of depicting the mathematical model now, a format that remained largely opaque for me during my field work, but I will get back to its entanglement with the code at the end of this section because the interface between mathematical equation and computer code is maybe the most crucial interface for creating bifurcations with higher degrees of irreversibility.

In the introduction to their edited volume on computer simulations, Küppers, Lenhard and Shinn emphasized four novelties of computer simulations: (1) Simulation has become almost omnipresent in many areas of life, (2) the speed and capacity of calculation in computer technology made new kinds of questions and models possible, (3) they also allow for the generation of visualizations and, finally, (4) "[Programming languages] determine to an important extent how programs can be conducted and how the […] social practice of programming operates." (Küppers et al. 2006: 8).[36] I will treat programming languages and related aspects in part 5.2.2., and visualizations in section 5.3. Now I want to demonstrate and discuss what the new capacity of calculation made possible.

Whereas the modelers in the research group talked about models creating "parallel worlds" with their models, Gabriele Gramelsberger calls them "extreme worlds" and distinguishes between the extreme worlds that mathematical and computational models create. Due to their more complex structure, computational models are able to render the worlds created in the mathematical model less extreme:

> Computer-based models and their simulations allow for a consideration of more variables and relevant parameters, not having to eliminate or linearize dependences, the choice of more complex geometric forms and not having to study extreme conditions. In short: the computer enables an indefinite increase in degrees of freedom of a system. (Gramelsberger 2010: 245, translation AK)

Computational models constitute calculation rules for the mathematical models in order to examine them quantitatively:

> A scientific programmer does not have to create patterns of relationships between variables and examine these quantitatively. Rather, he has to fit these patterns into the conditions of solvability

[35] see Gramelsberger 2010: 17-37 for a summary of mathematical developments

[36] Historically, Evelyn Fox-Keller describes how the epistemological novelty of computer simulations emerged gradually: "Provisionally, I suggest three such stages: (a) the use of the computer to extract solutions from prespecified but mathematically intractable sets of equations by means of either conventional or novel methods of numerical analysis; (b) the use of the computer to follow the dynamics of systems of idealized particles ('computer experiments') in order to identify the salient features required for physically realistic approximations (or models); (c) the construction of models (theoretical and/or 'practical') of phenomena for which no general theory exists and for which only rudimentary indications of the underlying dynamics of interaction are available" (Fox-Keller 2003: 202).

> and thus generate practical calculation rules minimizing structural loss vis-à -vis the mathematical model […]. In addition to mathematical conditions, computational conditions have to be considered. (ibid: 241f , Translation AK)

The computer constituted a new "condition of solvability" (ibid), because it enabled the modelers to simulate differential equations – but only through simplification and discretization:

> To mesh them with a digital computer, the continuous equations have to be transformed into discrete objects. Continuous differential equations are replaced by difference equations. Whereas the differential equations represent the functional (global) relationship between the variables, the difference equations determine the local values of the variables in different time steps. (Küppers et al. 2006: 10)

Simulations are not solutions in the strictest sense, even though they made it possible to work with differential equations, "rather, simulations are numerical imitations of the unknown solution of differential equations, or more precisely, the imitation of complex dynamics by a suitable generative mechanism" (ibid: 9). Various manners of discretization are possible, often already available as existing subroutines or "packages". This is a critical moment in modeling, because it is not possible to prove the equivalence of a differential equation and its discretization into a difference equation (cf. Gramelsberger 2010: 242).

The choice of parameterizations (see part 5.2.2.), of initial conditions (the scenario the model simulates) and of boundary conditions may further estrange the computational from the mathematical model (cf. ibid: 246). This lack of equivalence leads philosopher of science Wendy Parker to describe the mathematical model as a target system of the computational model:

> The upshot is that, like the material/physical systems that scientists set out to simulate in the first place, the mathematical systems specified by the preferred model equations and by the programmed equations must be thought of as target systems, and conclusions about them on the basis of computer simulation results cannot be automatic, but rather require justification. Of course, scientists who actually perform simulation studies often recognize this. (Parker 2009: 490)

The point so far is that in practice, programming is not just a question of translating equations into code as exactly as possible. Rather, it is a bit of an epistemological scramble or patchwork, and introducing the computer into the mêlée brought with it a host of new, unforeseeable issues and practices. Paul Humphreys, a pioneer in the philosophical study of simulations, even claims that "[c]omputational science introduces new issues into the philosophy of science because it uses methods that push humans away from the center of the epistemological enterprise" (Humphreys 2009: 617), requiring a non-anthropocentric account of epistemology (cf. ibid: 625).[37]

[37] Karen Barad made a similar claim for knowledge-practices in general and with this questions the separation between ontology and epistemology: "There is an important sense in which practices of knowing cannot be fully claimed as human practices […] because knowing is a matter of part of the world making itself intelligible to another part. Practices of knowing and being are not isolatable, but rather they are mutually implicated […]. The separation of epistemology from ontology is a reverberation of a metaphysics that assumes an inherent difference between human and nonhuman, subject and object, mind and body, matter and discourse. Onto-epistem-ology –

This is provocative, but it echoes the attentiveness paid to the specificities of models when Morgan and Morrison (1999) described them as mediators.

Concerning computational models, they claim that certain structural features of models enable and constrain simulations.[38] In her study of computer experiments in the context of climate modeling, Gabriele Gramelsberger extends their argument on models as mediators in understanding simulations as "autonomous agents". She turns their question on its head, asking whether now simulations determine the structure of models and theories as simulatable, leading to a new class of models, theories and experiments (cf. Gramelsberger 2010: 231). She then problematizes how in the recent literature there is no close description of practices of scientific programming:

> This can only mean that it is generally assumed that mathematical models can be translated into a program without greater problems. Or that the computer is only understood as a theoretical variable in the sense of solvability which justifies talking about certain properties in very general terms. Both marginalizations are incorrect. (ibid: 235)

Even though I will not be able to fill that research gap, I will now describe the programming of the computational model, because coding is a thought-provoking activity, where decisions about the model structure are being made and a processual understanding of the model is generated. Then I will describe some instances where the iterative alignment of mathematical model and computational model proved difficult and thus led to further simplifications within the model.

In a first step I want to define "code" for the purpose of this anthropological study. Here, I build on the work of sociologist and software developer Adrian Mackenzie. His main point is that even though code seems to be an abstract, remarkably context-free grammar, a formalism, or clear rules of operations on items of data, programming is a technical as well as a cultural practice:

> Despite appearing 'merely' technical, technical knowledge-practices overlap and enmesh with imaginings of sociality, individual identity, community, collectivity, organization and enterprise. Technical practices of programming interlace with cultural practices. […]. However, no computer code, not even textbook demonstrations of principle, can maintain this level of abstraction. [...], code itself inevitably slips into tangles of competing idioms, practices, techniques and patterns of circulation. [...] Code can be read as permeated by all the forms of contestation, feeling, identification, intensity, contextualizations and decontextualizations, signification,

the study of practices of knowing in being – is probably a better way to think about the kind of understandings that are needed to come to terms with how specific intra-actions matter" (Barad 2003: 829). I am not able to go fully into the implications of this here, but I will take issues of ontology and epistemology up again in part 6.

[38] "Although simulation and modeling are closely associated it is important to isolate what it is about a model that enables it to 'represent' by producing simulations. This function is, at least in the first instance, due to certain structural features of the model, features that explain and constrain behavior produced in simulations. In the same way that general theoretical principles can constrain the ways in which models are constructed, so too the structure of the model constrains the kinds of behavior that can be simulated." (Morrison/ Morgan 1999: 29)

> power relations, imaginings and embodiments that comprise any cultural object. (Mackenzie 2006: 4-5)

Therefore, he continues, "[c]ode is agency-saturated" (ibid: 9). But this agency is distributed unevenly and in some situations, e.g. a virus attack, the critical agents become hard to identify (cf. ibid: 10). This is one way, in which humans are pushed away "from the center of the epistemological enterprise" of modeling (cf. Humphreys 2009: 617). An important factor determining how agency in coding is distributed is skill growing with experience. In an interview, somebody pondered the possibility that while constructing a model may not become routine, implementing it in code will – with increasing experience. The less experience one has, the less one knows how to interpret or avoid altogether errors and bugs in coding, for example.

Two spontaneous remarks illustrate how in the research group, coding was assumed to either form ideas – "We think in code, actually" – or to merely express them – "coding is only the technical part, not the real science. Or, well, maybe you can't separate it that easily". Both claims were applicable to varying degrees depending on the situation. Coding is a crucial knowledge practice for learning about and with a model. One way this happened was through translation from one programming language into another. It is not always necessary, but a modeler who did it admitted: "of course it is a lot of work to do it myself. But first of all, it's fun and secondly, through writing it myself I have developed a better under-standing of the model" (16/01/2018a: 196-198). That did not mean that the basic structure of the model had to be changed in any way, but simply the fact that Python offers packages for processes that he had to write himself in more detail in C++ made a difference for his understanding.

On a more general note, choosing a programming language presents an important bifurcation in model construction.[39] Somebody else described how the model he used was written in an older language, FORTRAN, and because it was a bigger, more complicated Earth System Model, it would be too much work to translate it into a newer language. In a slightly different case, the research group decided to translate the framework they developed from Python into C++, which took a lot of effort. For some parts and functions of the model this was easier than for others, which shows again that degrees of irreversibility are closely linked to a programming language and the kind of expressions it enables or at least makes easier.

[39] Programming languages play an important part in the emergence of new practices and competencies in relation to computer simulation. This forms new collectivities: "This takes the form of shared techniques (the neural network model, cellular automata, Monte Carlo models, etc.), shared simulation-linked informatics languages (such as Simula or the general purpose, multiparadigm, object-oriented C++ simulation language, […]), shared competencies, shared images, and shared horizons. Taken together, such common resources, reactions, and perceptions constitute a lingua franca, itself akin in some ways to a form of practical universality" (Küppers et al 2006: 20).

A second way of learning by coding was the classic "learning through mistakes", since code is unforgiving and demands clarity in expression: "When I translate the whole thing for the computer […] if I don't tell it clearly what I want, it won't do anything. […]. Writing the code, it has to be unambiguous […], otherwise it will produce a mistake" (18/01/2018a: 101-111).
One could almost say code is resisting too easy an implementation of an idea, forcing a programmer to think it through thoroughly: "So, then you have thought about a more appropriate way for a model component. And then you turn it on and then there is the problem and you spend the rest of the day fixing it" (18/01/2018b: 197-199)
Bug-fixing while trying out new ideas simply made up a big part of the workday.[40] Looking for mistakes became even more difficult (or instructive) when somebody else wrote the code: "in that case, you have to read a lot of the code and try to understand what is actually happening. And you have to jump around a lot between the different files, to look where else the problem is happening. And that always took a lot of time" (18/01/2018b: 204-208). This suggests a certain intimacy between a programmer and their code. However, once the problem was dealt with, the code became controllable and mutable again[41]: "When I want to try something out, I expand the code and in principle I add a switch where I can turn that new part on and off" (06/02/2018: 233-234).
Taking a closer look at code it was easy to see that it is never just pure code. Instead, all kinds of comments were strewn in. The hashtag # (Python) or a double slash // (C++) was used to insert headings, ToDos, instructions and explanations into the code. Some of these would be removed before publication, others expanded. This also structured the code visually:

[40] This can be seen as a form of verification of the correctness of the code "on the go", instead of as a separate step in model development. Eric Winsberg states that at least in the philosophy of science, verification of the model code has been more or less ignored, as opposed to the validation of the whole model. Comparable to my account of iteration in modeling practices he questions the strict separation of these two ways to examine a model: "The equations we choose often reflect a compromise between what we think best describes the phenomena and computational tractability. So the equations that are chosen are rarely well 'validated' on their own. […]. So one point is that verification and validation are not independently-successful and separable activities. But the other point is that there are not two independent entities onto which these activities can be directed: a model chosen to be discretized, and a method for discretizing it. [...]. So success is achieved in simulation with a kind of back-and-forth, trial-and-error, piecemeal adjustment between model and method of calculation" (Winsberg 2015: 18f).
[41] With mutability I refer again to Adrian Mackenzie: "Software embodies a mixture of mutability, contingency, necessity" He continues about coding: "[...] Borrowing a concept of physics, we could say that software undergoes phase transitions or changes of state. It solidifies at some points, but vaporizes at others" (Mackenzie 2006: 1f).

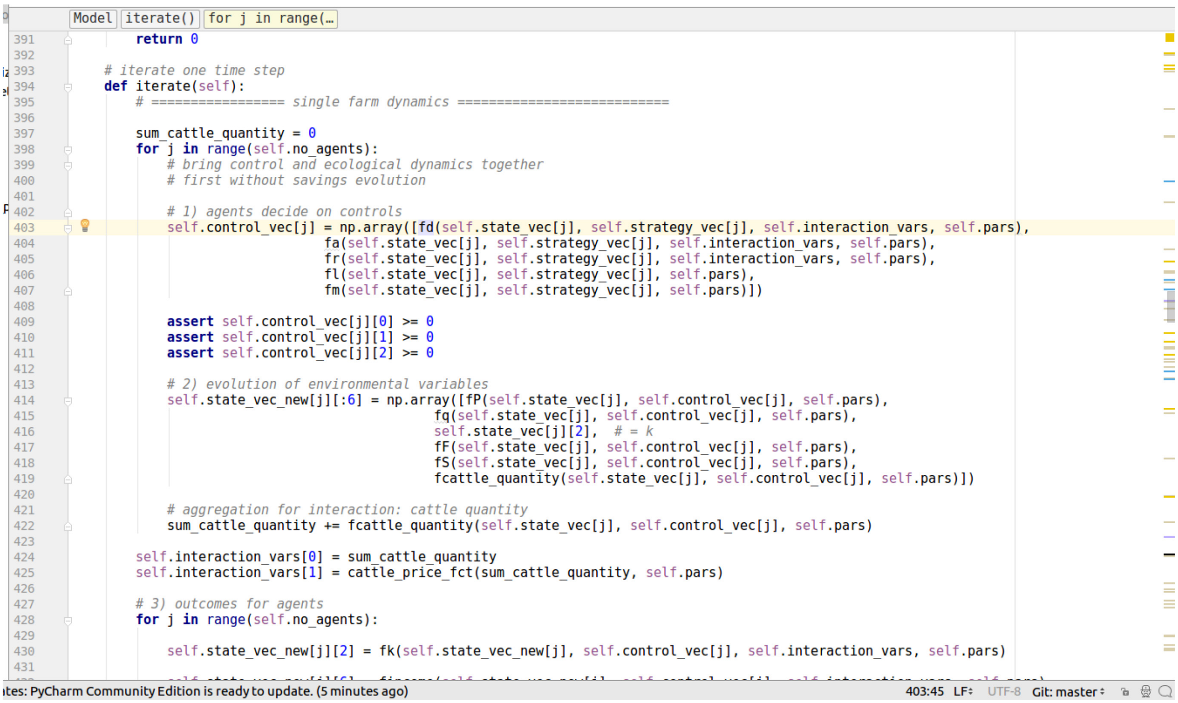

Figure 5: Screenshot of model code (23/01/2018). Comments are grey. Reproduced with permission.

Some comments characterized the code as work in progress and were used to delay decisions, for example. In the code of the model framework, hidden at the end of a line, a # indicated the need for a decision between two ways of calculating countries' gross domestic product:

```
economic_output_flow = \
        Variable("total economic output flow",
                "(in value units)",
                 IAMC="GDP|PPP",  # or GDP|MER?
                 unit = dollars / years,
                lower_bound=0, is_extensive=True, default=0)
```
[42]

These comments are part of the work it takes to separate code and modeler, as the fact that other people's mistakes are much more difficult to fix already suggested. Comments make the code legible, transparent and transferable for others. They redistribute agency and put the modeler closer to the epistemological center of modeling. Some programming languages require this documentation more than others. The modeler who had begun to translate the modeling framework from Python into C++ explained some features of C++ to his colleagues: "This is nasty, if you don't know that, the whole thing is quite cryptic. In C++ you have to be a stickler for

[42] This may not be a trivial decision because the IPCC discussed it in its fourth assessment report. I do not know if that caused this comment, but once the decision is made and not made transparent, the outcome could a) be misinterpreted by people who are aware of that discussion and b) have an impact on model output: "To summarize: available evidence indicates that the differences between projected emissions using MER exchange rates and PPP exchange rates are small in comparison to the uncertainties represented by the range of scenarios and the likely impacts of other parameters and assumptions made in developing scenarios, for example, technological change. However, the debate clearly shows the need for modelers to be more transparent in explaining conversion factors, as well as taking care in determining exogenous factors used for their economic and emission scenarios" (Metz et al. 2007).

documentation, if others are supposed to be able to work with your code. It is almost impossible to debug a macro you haven't developed yourself" (fieldnotes, 10/01/2018).

Another connotation of code, to encode or encrypt, maybe comes closer to what is happening here. Without the comments, the explanations, the alignment of language and programming language, even the code authors would lose their overview. This became more and more of a problem for the growing research group, where modelers changed but models stayed, and where the models were an important part of the output. They were usually published in papers and additionally made accessible as open source code. This issue came up during one of the team meetings, when they spent more time than usual discussing teamwork and atmosphere. They decided to document more of the process of model construction, and to explain the reasons for decisions they made, especially when an option was *not* chosen: "Some of this can be written directly into the code", somebody remarked (fieldnotes, 22/01/2018).[43]

The need for comments illustrates the difference between language and code (even though it is written in a programming *language*), and between the formats of mental model and computational model. This lack of equivalence is also indicated by the fact that the research group wanted to publish example code in the paper introducing a new model, because that would better illustrate its use than only an explanatory text. The practical intimacy between a modeler and their code further suggests that implementation is not as linear as the apparent formalism of programming languages would lead one to believe. It is more idiosyncratic. Anders Munk emphasizes this connectedness with the hybrid figure of the model-modeler:

> If it is true to say that we would be useless as modelers without the aid of the computer programme, it is equally true to say that the programme would be useless without the aid of us, its modelers, for translating the world into formats which conform to its inbuilt hydrodynamic formalizations of nature. Computer Simulation thus implies the becoming of a hybrid – the model-modeler. (Munk 2013: 145)

In this non-anthropocentric account of epistemology, the model-modeler hybrid bridges the misalignment between model formats and allows them to unfold their respective characteristics in the various material-semiotic practices. It therefore plays a crucial part in aligning the model, of making it sensible and getting it to work.

With this I want to return from this detailed zoom into the code to the alignment of mathematical and computational model, which is the main issue of this part. As stated above, it is crucial for

[43] It is interesting when in a group the need for more formality and documentation arises. The group has grown, and members changed all the time. And because there now is a project, the modeling framework that transcends the individual members, the transmission of information had to be secured and made transparent. It was a moment of transformation for the research group, more or less forced also through model- and financing structures. But formalization is a double edged sword as one of the senior members hinted at: "we need more order as an aim, but a certain disorder is also good" (fieldnotes, 22/01/2018).

creating more or less irreversible bifurcations in model construction. The computational model and the mathematical model go hand-in-hand and iteratively shape each other. In the most productive sense, mathematical model and computational model build on and complement each other, such as in this agent-based model:

> At first, you write down the equations, describing for example an agent's dynamic. But then you have to think about how you can numerically simulate this for every agent. That means, that I have to write code, where I don't have to write spell this equation out for each of my many agents. I have to fiddle around, until for each agent I have some sort of representation, or descriptive variable and can apply this equation to all of them. Then I have to write a lot of code around it, so that I don't have to implement this for every single agent. Instead, I write some sort of loop that iterates over all the agents. (23/01/2018, 314-323)

But iterating between mathematical and computational model is not always as seamless as the quote above suggests. Even though computational models made approximate solutions of differential equations possible (see above), not all mathematical problems are solvable, even though in this particular case it is not clear whether it was a problem with the used code-package, the mathematics or a lack of experience:

> I wanted to implement this beautiful study and I did, but then the set of equations wasn't solvable. And I spent a lot of time trying different solvers and it simply didn't work. So I had to change the model again […] which was a pity, because it was the only really new idea I put in my model and now it is lost. (18/01/2018b, 232-239)

He then decided to use an average value, as his supervisor had suggested: "this was a simple possibility [...] I mean, once you get it to run properly like that, then later you can always try again to solve the original problem" (18/01/2018b, 282-286).

This pragmatism was echoed by his colleague: "I think a lot is trial-and-error, external conditions, feasibility, time management. So, when you code you think 'ok, this solution is prettier, but takes more time'. So I'll do it differently, maybe not as pretty, but requiring only a fraction of the time" (16/01/2018a, 304-308).

And of course, not just mathematics and code could limit each other. The modeler's experience and knowledge also plays a part and is shaped by the learning process that is the construction of a model regardless of the modeler's academic status:

> I also have agents who are more or less complicated in the way they make decisions. For the computational models, it worked relatively well with algorithmic heuristics. But for some kind of aggregated decision I had to throw it away again because it couldn't be expressed in aggregated equations, at least with the knowledge I had at that point. […]. That would be a possible future project, to see if I could at least get a partly aggregated understanding to work mathematically. (01/02/2018, 189-195)

Whereas computational and mental model format were aligned in this case, an alignment of the mathematical model proved more difficult which led to a delay in including agents with that particular ability of making decisions.

The next step after experimentation in model construction is experimentation in model manipulation through “letting the model run repeatedly”. One of the modelers emphasized: “You shouldn’t get lost in coding because ultimately it is only a set-up, like an experimental system or something. Or a form of study design in the social sciences” (16/01/2018a, 276-279). In the following part I will interrogate that comparison of experimenting in modeling and laboratory experiments, asking: “Does matter really matter?” (Parker 2009).

5.2.2. “And you let it run again and again”

In the framing of this work, experimentation is a practice of alignment. I already mentioned that experimentation is more iterative in character, especially in the previous part, where I described how “little by little you assemble the model”. I have clarified how computational models are *not* exact solutions of mathematical models that consist of differential equations. Thus, they require new ways of investigation in order to understand their properties and behavior – like parameter runs, as one of the modelers explained in reference to a classical physical model: [44]

> And actually, the models we build, with complex systems and networks, are also models, only that often you can’t describe them analytically anymore, in equations giving you a result. Instead you have to examine the parameters… or let simulations run and then you understand the system through those parameter runs. Generally, physicists, they simply want to understand models and stuff. (06/02/2018, 14-19)

In this part, I want to focus on experimentation more literally. I will now explain what parameters are, how parameter runs are used to explore properties and behavior of a model, being understood as experiments with the model. Then I come to the question of the relation between materiality, “experiments, simulations and epistemic privilege” (Parke 2014) in order to interrogate whether and how experiments in modeling can be seen in continuation with laboratory experiments. As one possible answer I will introduce how philosopher and STS scholar Tarja Knuuttila (2005, 2006, and 2011) conceives of models as epistemic artefacts or tools.

Parameters are certain deterministic or stochastic values that characterize entities in an agent-environment model examining reinforcement learning, for example:

> If all agents are homogenous, that means all the parameters are the same, they don’t change. Because they all learn to react in the same way to the other’s behavior. They learn the same. Their behavior is the same. […]. When I have heterogeneous agents, then I have more parameters to examine. And the different environments have parameters, too. And then I can choose some and then try to understand, how the different agents behave in the respective parameter space. (18/01/2018a, 214-223)

[44] That is one reason why computer simulations could even be seen as a new type of experimentation, or “extension of empirical research” (Gramelsberger 2010: 268 in ref. to Humphreys 2004), such as immediate observation and its technological mediation with microscopes or telescopes, for example. It enables experiments with mathematics in time and space in order to make visible and understand trajectories and processes (cf. ibid: 251).

Even the simplest models contain parameters. In general, they are used to include smaller-scale processes into a model by subsuming them under this one value. This kind of operationalization is called sub-grid parameterization and is another practice of simplification.[45] They can make all the difference between models and can be used for biophysical as well as social processes, from the Albedo effect to forgetfulness and well-being.

In the models used by the research group, parameters could be derived from statistical data, empirical studies or simply assumed to be more or less accurate to explore model dynamics. As "switches" they could be used to turn different model components on, such as different types of agents, which could be included one after the other into a model. As characterizations of (often human) agents in network models and representations also of social processes, they were closely linked to assumptions about "the social", e.g. whether and how agents make rational decision or how quickly agents adopt somebody else's opinion (see part 6.1.). Nevertheless, using too many parameters was not considered "elegant" and made a model uninteresting.

Experimenting through model runs is a crucial aspect of computational modeling in general (cf. Hastrup 2013: 5). Parameter runs are used to explore the influence of certain parameter settings on the model behavior, e.g. in performing a bifurcation analysis. They produce plots that make visible these effects and are hence a model format involved in practices of experimentation and visualization (see part 5.3.1.). Most of the models in this research group were so simple, that a run on a normal computer only took a few minutes up to half an hour. But a model with several parameters needs to run with different combinations of those parameters and of different values of these parameters. That means it may have to run several hundred times which would take too long on a normal computer:

> Well, modeling itself took only the last two months of my thesis research. Letting the model run again and again, look at the results, see what's happening, change the model again, set up runs on the Cluster again, let it run another 100 times. Or you directly set up 10 different parameter combinations and let that run, check the results, and then usually change the model again because something doesn't seem to be right. (18/01/2018b, 216-222)

That is why at a certain point in the process of model construction the modelers began to use the so-called "cluster", the central super computer, for a bigger amount of systematic parameter runs. The model formats were now sufficiently aligned that the model could be used for systematic experimentation, because "once you have the model, the possibilities of what you can

[45] See Sundberg 2016 on parameterizations in climate models as boundary objects between modelers and scientists generating observational data. See also Guillemot 2010a on practices of relating observations and simulations in the validation of climate models.

try out are endless" (06/02/2018, 121-124). It was now worth to go through the effort of preparing and planning runs on the cluster.[46] This indicates a moment where the past bifurcations in model development have reached a higher degree of irreversibility. That does not mean that components might not be changed again at all, but that the basic structure reached a certain stability and now it was a matter of using aforementioned "switches" to turn model components off and on and taking care of details.

The possibility to experiment was considered an advantage of the computational model over the mathematical model or other, more static model formats: "I really like computational models as models because they make it very easy to check your assumptions and to experiment" (01/02/2018, 261-263). In her introduction to "The social life of climate change models" anthropologist Kirsten Hastrup gives a broad definition of experimentation:

> *Experimentation* implies some kind of manipulation with forms, computationally, mentally, or experientially. Once a form has been established that depicts the regularities, experimentation allows for trying out the not-yet-realized, the possible; […]. One could say that experimentation allows the object to 'talk back', or the matter to really matter – at least within the model. (Hastrup 2013: 5, emphasis in the original).

But what does it mean, that "the matter really matters"? Comparing computer experiments and laboratory experiments as material semiotic practices raises the question of where the material is situated in the computer experiment. I want to briefly lay out two opposing ways of answering this question in order to situate a third position from a different angle.

On the one side, matter matters. Philosopher Mary Morgan claims that laboratory experiments enjoy epistemic privilege over computer simulations because of a material continuity given between world and laboratory experiment but not between world and simulation: "ontological equivalence provides epistemological power" (Morgan 2005: 326). A second reason is that the ability to "not only surprise but also confound" (Morgan 2005: 324) only pertains to laboratory experiments. Gramelsberger sharpens these claims as "the corrective of material resistance" that computer experiments cannot hope for (Gramelsberger 2010: 194).

On the other side, Emily Parke refutes both claims made by Mary Morgan. Assuming that a material correspondence between object and target of inquiry automatically bestows an experiment with greater epistemic value and inferential power is unfounded:

> I agree with Parker (2009) that material object-target correspondence does not necessarily mean greater epistemic value. The above considerations establish a further point: It does not even follow from the fact that we have a material system as our object of study (that is, we are doing an experiment) that material correspondence is doing, or is even meant to be doing, the work in validating an inference. (Parke 2014: 9)

[46] Some of the modelers worked on a method to automatize these runs.

For judging these, a distinction between experiment and simulation grounded in the lack of material continuity between model and target system is not as decisive as the context either of them is used in: "For addressing certain kinds of questions, realism and material correspondence seem paramount. For other kinds of questions, they do not" (ibid: 17).

Also the ability to surprise, which Parke interprets as referring to the observation of unexpected behaviors and hidden mechanisms or causal factors, is not pertinent to experiments alone (cf. ibid: 13, 16). She uses ABMs as a case in point for the display of unexpected behavior and cites early studies by Joshua Epstein that showed emergence of complex patterns from simple rules to illustrate surprise by hidden mechanisms.

From my own disciplinary perspective, I agree with Parke's call for context-sensitivity in analyzing methods and the concrete ways inferences are made. Additionally, it is not only a matter of questions that experiments or simulations address because that limits inquiries into modeling again to the adequacy between model and target system. Epistemic value or claims of inference also rest on concrete practices and transformations (like simplification in this case) that make something "experimentable" or "simulatable", like simplification or visualization, for example. These need to be taken into account as well.

I now come to a third way of dealing with materiality in computer simulation modeling that approaches the issue from a different angle. Like Morgan, Knuuttila underlines that "models are typically made out of different stuff and embodied in a different scale than the things modeled" (Knuuttila 2006: 41). Unlike Morgan, she does not take this as a point of departure to judge their epistemic value as inferior to experiments. Rather, she approaches the models themselves as material artefacts or precisely as "things that are variously materialized" (Knuutilla 2005: 1266), and uses this to attend to their epistemic capacities.[47] Ultimately, like experimental systems in the laboratory, models are "epistemic tools" (Knuuttila 2011: 267):

> Approaching models as epistemic artifacts draws attention to both their intentional and material dimensions from the interplay of which their epistemic qualities arise. Models gain their intentionality by being constructed, used, and interpreted in purposeful human activities. On the other hand, there is nothing to use, construct, or interpret unless models are materialized in some medium. This material dimension of models makes them able to mediate and travel between different groups, epistemic activities, and disciplines […]. In fact, it is typical of modeling that the same computational templates travel across sciences (see Humphreys 2002, 2004). As they gain different interpretations and uses, they become part of the embodiment of different models. (Knuuttila 2006: 50)

[47] With epistemic tools she builds on earlier accounts of models as independent entities, like Morgan and Morrison (see part 5.1.2.) and the common interest of these approaches in the construction and use of models (cf. Knuuttila: 2011:266). Her focus on the material, "tangible" dimensions of models is complementing these earlier accounts (cf. ibid: 267): "Without materiality mediation is empty" (Knuuttila 2005: 1266).

They distribute epistemic agency, e.g. in making processes mathematically dressed in differential equations legible for human interpreters.

In several of the interviews the models were explicitly described as tools, each setting a slightly different accent. Comparable to Knuuttila's concept of epistemic tools, seeing models as a tool "to make something understandable" is given as an alternative to seeing them as a direct representation of reality which would be problematic (fieldnotes, 11.12.2017). As tools, models are used to systematize the ruminations over a problem. But, differing from Knuuttila's concern with the materiality of models, seeing models as tools is rooted in their symbolic nature in relation to the world:

> For me it is important, that a model is as abstract as possible while still reflecting those qualities of reality I want to think about. That means, for me a model is a tool generating understanding as well as discourse. […] Ultimately, they are complex symbols for things and relations in the world on the basis of which you can talk about the world. And in that sense they are good or bad, in how far they have something to do with the world and in how they help to communicate about the world. (01/02/2018, 250-258)

In particular, they help communicate about the world because they serve as a basis to develop narratives to think through and talk about certain scenarios (see also part 5.3.2.):

> Models help to call forth associations, which is great, because for one, generate understanding in basic physics, which is important for the scientific community, because as a side effect new methods and model components are developed that other people can use. And at the same time you generate narratives you can use to think through certain scenarios. Models help you think, in a way. As a graphic or visual way to illustrate certain processes. (31/01/2018, 113-119)

In the understanding of the research group, models were tools in a straightforward sense of lending aid or assistance. I will get to the visual aspect mentioned in the previous quote in the next section. However, the concept of epistemic tools grants the models an agential status of their own in the process of knowledge production, especially because they are not "the model" but as Knuuttila called it "variously materialized" as different "representational means".[48] Representational means can be ways of relating model and target system, such as simplifications or approximations, but also ways of visualizing the model:

[48] With her focus on concrete representational means, Knuuttila wants to attend to "the paradoxical nature of modeling" (2006:42), which is constituted through the representational as well as pragmatic or performative aspects of modeling. She takes the difference in earlier philosophical approaches to modeling – that lingered on representation as the source of knowledge through models – as her starting point to describe representation as becoming relevant at certain points of modeling practices: as a practice of building models and as only one among many uses of models (cf. Knuuttila 2005: 1260-1268, 2011): "Thus, being productive things created by representation, simulation models question the distinction between the performative and representational conceptions of science and challenge us to approach representation performatively. From this point of view, representation is less a relation to be aspired to by philosophical analysis than an important object of factual knowledge itself." (Knuuttila 2006: 53). With "performative conceptions" she refers to the work of Ian Hacking and Andrew Pickering, but her argument could also have implications for further research in a material-semiotic stance, potentially enabling a productive connection between questions of representation and questions of practice.

> [O]n the one hand the representational means impose their own constraints on the model design, yet on the other hand they facilitate the results derived from it […]. More generally, it seems that the wide variety of representational means modelers make use of i.e. diagrams, pictures, scale models, symbols, natural language, mathematical notations, 3D images on screen all afford and limit scientific reasoning in their characteristic ways. (Knuuttila 2011: 267f)

I spelled out a similar point with the notion of different model formats which need to be aligned through practices of simplification, experimentation and visualization. Consequently, each model format is a slightly different epistemic tool, and a certain degree of misalignment can be productive. The model formats I have focused on so far were the computational model and the mathematical model, and I briefly touched on the mental model and visual model formats – diagrams and plots of model output. Mathematical models enable the skilled user to grasp model components and their relationship with one glance. They are potentially the most "condensed" model format. Computational models spell the models out in great detail, a practice that can be described as "story telling with code" (Gramelsberger 2010: 170, see also Gramelsberger 2011) and, as I have shown in part 5.2.1., they respond to the modeler with error messages, becoming an active counterpart in model construction. Thus, computational models at the same time encrypt and spell out model processes. These processes and the behavior of the model over time are made visible by model plots. In the next section I will focus on visualization as a practice and a model format.

5.3. Visualization

The student was almost finished with his model. He had identified a research gap and decided on a research question as well as on the processes to include. He had performed a number of experiments through parameter runs on his computer. He had introduced a new parameter to capture certain characteristics of digital social networks. Finally, he was at a point where the model structure was clear and it became a matter of beginning experiments with additional components. He would have to do the necessary parameter runs on the cluster. Yet first, he presented the state of his work to his supervisor and later to the team. We met at the supervisor's office at 10 am, and when everyone had arrived a few minutes later, the student opened his laptop, summarized what he had done since the last meeting and then began to explain the model's structure. "This is still very simplified", he admitted. His supervisor frowned at the defensiveness: "We do take the real world as inspiration, but in the model we can do what is interesting and what we want, independent from that. That is the beauty of physics."

The student nodded and continued his presentation with three plots in black and white that showed how the parameter he had introduced influenced opinion dynamics. The lower it is the more opinions coexist over the course of a simulation. When it is high, consensus happens. He had chosen the plots carefully out of a number of parameter runs and contrasting them with each other illustrated the basic process of the model.

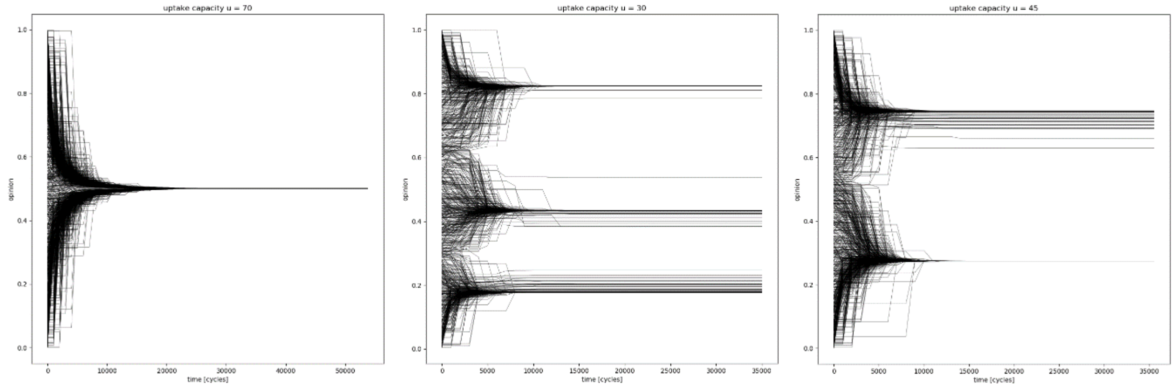

Figure 6: Model plots (fieldnotes 31/01/2018). Reproduced with permission.

They look almost elegant, I thought. Especially when the student showed some plots from an older paper he referenced and said: "those are quite ugly, to be honest". Still, also those plots worked through contrast and comparison.

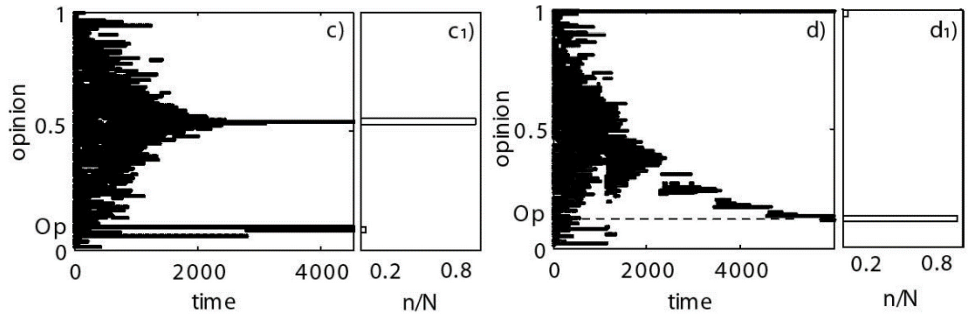

Figure 7: Model plots (cf. Carletti 2006: 226).

The student called our attention to a particular phenomenon that at least for me was almost lost behind the obvious "it boils down to two or three opinion groups": For certain runs, two main groups formed with smaller subgroups of different opinions in the immediate proximity. They continued to discuss the meaning of the plots, tracing the various lines with their fingers on the screen.

Then the student showed us a beautiful, colorful plot. It was a combination of 50 model runs, an ensemble run, and he had added some other plots to it. The plot was dominated by a dark blue morphing into a lighter green. Three additional graphs were in a contrasting orange, differentiated by the quality of the line. A fourth graph was added in *white. Outside the diagram four different measuring scales were indicated. He zooms closer into the plot: "Basically, this sums up my work", he quips.*

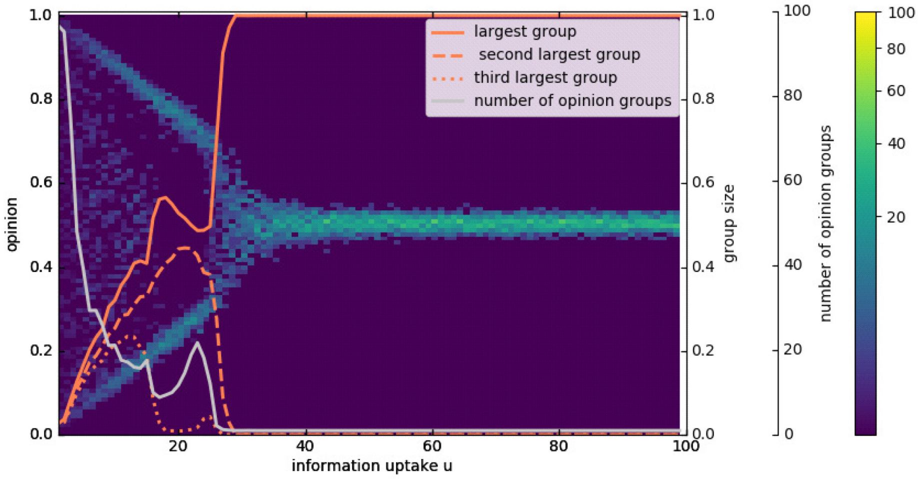

Figure 6: Ensemble run (fieldnotes 31/01/2018). Reproduced with permission.

From what I could gather, the plot showed how when the value of one particular parameter is increased, the number of opinion groups concentrates around a small part of all the possible opinions, almost leading to a consensus. The student explained the plot and that he had subsequently introduced another parameter which "led to a shift in the attractor state". I had only a vague idea what he meant, but I saw the difference when he quickly jumped back and forth between three different versions of the plot: The part where the two thicker lines ran together in the middle shifted further and further to the right. The student summed his presentation up by explaining which other parameters and types of agents he wants to include in the model to experiment with. He ended with: "If I still have time, I also want to try out what happens, if the agents prioritize certain opinions. But, I mean, the qualitative result is already there, so…"

They began to discuss details. The student pointed to a bump in one of the lines he did not know how to interpret. The supervisor suspected that it was due to too few parameter runs: since he had done only 50 runs, the error margin would probably still be rather large. It actually could correspond to that bump. He indicated the possible span of the margin with his fingers on the screen.

Then the supervisor turned his attention to the model equations: "Did you let the model run long enough? Because these suggest that after a while actual consensus happens, not just almost, as it is the case here:" The student agreed: "That is something I want to take care of with the runs on the cluster. On my laptop, as it is, one model run already takes at least half an hour. Do you have any practical suggestions how I could save time *here?" "What language did you use?" "Python." The supervisor looked at the model code: "You can always try and leave out low level statements like 'print', those usually are too slow." The student nodded: "Yes, I could try that. And do I need to worry about using code packages like Numpy? Would it be better if I wrote that part of the code myself?" "That particular package is reliable enough. Everybody uses it, so don't worry about it. You have enough things to do as it is. By the way, there is this empirical study that actually tries to measure the parameter you introduced in the data. You should take a look at that. I'll email it to you. Remind me, if I haven't done that by tomorrow."*

At the end of the meeting, the supervisor cautioned the student not to lose sight of his central research question, even though he could certainly do a lot of interesting things with his model, now that it was working: "In any case, you're on track with what you're doing, to compare and discuss differences that follow the introduction of other model components. Actually, a model only makes sense in relation to other models, because they really don't depict reality, anyways."

The following week, the student gave a presentation of his work in front of the team. He talked freely, supported by presentation slides containing mainly plots, but also illustrative images, numbers and equations, as well as a diagram of model components and an enumeration of the modeling steps that had to be performed with each simulation.

He used the same plots as before. Additionally, he had actually found a storyline and a series of drawings, like a comic strip, to illustrate his model. He used colors to mark correspondences between the images and his plots. The drawing was adapted from an older paper on a similar subject, which in return had been adapted from another source – the agenda setting paper for the matter at hand, actually. Mostly, he put two different kinds of visualizations next to each other on one slide – juxtaposing drawing and equations, equations and plots. And finally, to make sense of what the plot showed, he put plot and drawing next to each other. All the time the colors connected the corresponding elements in two images.

5.3.1. "And then at first you try to observe interesting emergent phenomena."

Even though a model is never really done, at a certain point it is done enough to "observe interesting phenomena" – The verb used in this phrase indicates the importance of model output as visually accessible data, usually as diagrams or plots. This quote from one of the modelers when asked how he would define modeling underlines this: "It is the depiction of processes that happen in nature, for example, or social processes or something. And to depict them in a metric which we are able to perceive somehow, such as numbers” (M4: 23).

In this section, I will focus on visualization – both as practice and model formats. In the first part, I will describe how plots are used and embedded in other model formats before I will come to the “interesting phenomena” they make visible. I will focus on plots here, even though different kinds of visualizations played a role in my research.[49] The second part is about the ways of relating model and world in the research group which can involve visualizations, but do not necessarily have to.

Visualization is an integral practice of alignment because visualizations make visible alignments as well as misalignments: a plot of implausible output makes visible at a glance that mental model, mathematical model and computational model format are misaligned, even though technically, the code is correct. The story illustrated some of the many ways in which visualizations are used in everyday modeling practices and in the communication about the model and its results.

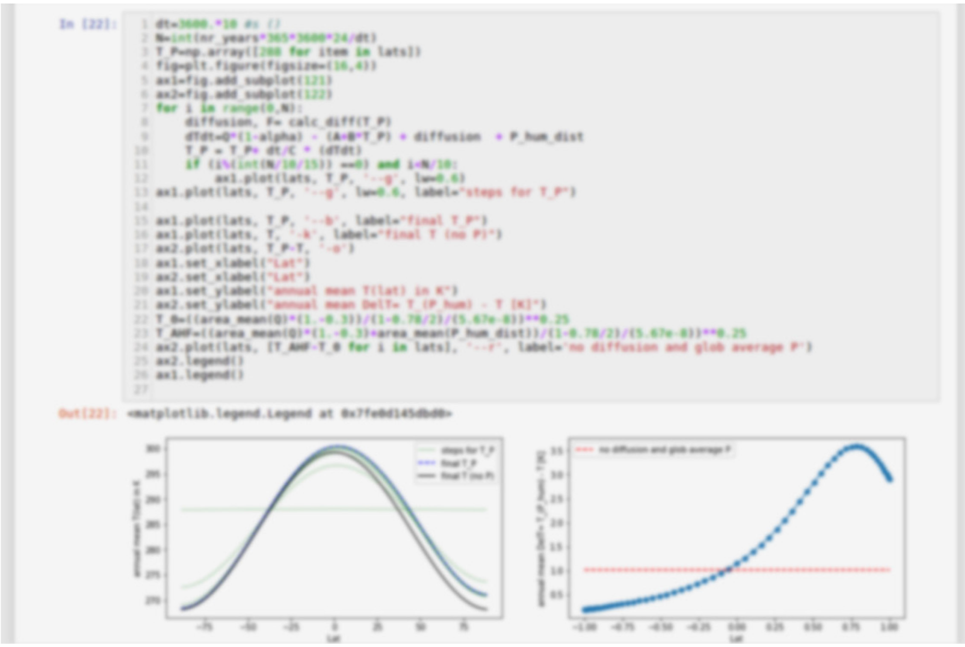

Figure 7: Screenshot of a plotting function (16/01/2018/. Reproduction without permission, thus blurred.

Different kinds of plots, like ensemble runs, state space plots, bifurcation diagrams or simple graphs are the most common way of presenting the results of the model runs. Especially when combined with each other they make model dynamics visible at a glance. Thus, every computational model included a plotting function to create these plots after each run:

[49] Four kinds of visualizations were relevant: the ones produced ad hoc in interviews or supervision situations (though often building on earlier experiences of visualizing a model), the plots produced en masse for parameter runs and the ones then reproduced (or chosen) for a paper, the team or the public as a basis for discussion and evidence in support of an argument, and visualizations of model dynamics in causal loop diagrams.

> And then you have helping functions. One function counts how many opinions still exist in the model, another one counts, how many edges between different opinions exist, how strong plurality is, so to speak. […]. Then you have the so-called main function putting it all together […]. And then there is always something visualizing the whole thing, how it behaves over time for example. […] And you always try to find something two-dimensional, with two axis, because that is easiest to grasp visually. (31/01/2018, 378-387)

These plots seldom appear alone – neither in code nor in papers or presentations. They are at least accompanied by explanatory texts. Often, several plots of the same kind are contrasted, or plots of different kinds are used to explain each other.[50]

Using contrasting colors and clear lines, plots have a certain aesthetic appeal. Hence, they could be analyzed like other images from a variety of theoretical perspectives (cf. e.g. Günzel et al. 2014), focusing on their non-verbal efficacy and "obstinacy" the iconic turn calls us to attend to (cf. Schulz 2009: 11), but also focusing on their specificity as diagrams: "Images are looked at, diagrams are read. Their legibility implies familiarity with its underlying syntactic and semantic rules" (Beck/ Wöpking 2014: 347).

Diagrams and plots as they are used here can be seen as "hybrids" of script and image (cf. Mersch 2015: 104).[51] A broad body of literature on techno-scientific images illustrates how these are bound up in specific practices, fulfil a certain purpose and require a specific kind of "skilled vision" (Grasseni 2007)[52] to appreciate and interpret them (e.g. cf. Bredekamp et al. 2015, Daston/ Galison 2017, Liebsch/ Mößner 2012, Reichle et al. 2008). Early laboratory studies have also concentrated on visual material produced in their field (Knorr-Cetina 1999, Latour/ Woolgar 1986). The abundance and importance of plots makes visualization a distinguishing feature of simulation modeling:

> Whereas telescopes and microscopes render phenomena visible by affecting the scale of 'tangible' entities through optical processes of resolution, simulation renders 'visible' the effects of parameters and forces such as time, dynamic interactions, and so forth that are not dealt with by optics related transformations. Thus, simulation, by constructing images, may translate absolutely non-visual events into a visual media! Often there is no opportunity to compare simulated images with the original - there may be no possible perspective from which to view things like this, or it may even be that the depicted material does not exist in the real world. Hence, simulations may equip virtual worlds with visual and other qualities that do not mirror those of real-world processes. (Küppers et al. 2006: 8)

Because plots either by themselves or combined with each other make visible otherwise non-visible, sometimes surprising processes within a model, they play an important part in aligning model formats and can be seen as one themselves. After the presentation described in the story, I inquired after the meaning of the blue ensemble run plot: "It's not that difficult, actually. Here,

[50] I do not know how much influence the modeler has on the design of a plot that is made by a plotting function.

[51] He uses the difficult to translate term "Schriftbildlichkeit" – "script-image-ness" to capture this.

[52] The concept of skilled vision captures learning to look as an apprenticeship which like other scientific practices happens between standardization and personal experience: "skilled visions are the result of concrete processes of education of attention, within situated practices and ecologies of culture" (Grasseni 2007: 7)

at x you have the values of the parameter U, the opinions. Then I took U=1, let it run 50 times and plotted every single opinion in here." The student continued to explain it and finished with "And then it produces quite an interesting image in the end" (06/02/2018, 24-26).

But what qualifies as "interesting image" or "interesting emergent phenomena"? "Interesting" as an evaluation indicates a disciplinary learning process, and is highly context-dependent. In the research group, it was often used to characterize counter-intuitive, mathematical phenomena. So, when something looks "interesting" it could mark the point where "matter really matters" (Hastrup 2013: 5), or "the world kicks back" (Barad 1996: 188). Or, more precisely, where a certain disciplining teaches to let the world kick back in a certain way, e.g. through parameter runs. At one instance, a supervisor proposed to their student: "you could test this parameter continuously. The results for 1 and 0 will be trivial, but in-between it is going to be interesting. That's how you would do it in physics, test each parameter systematically for itself" (fieldnotes, 10/01/2018).

Because plots and other visualizations cause and support moments of interest, like the ensemble run plot I replicated in the story at the beginning of this section, they were used repeatedly in different contexts – as allies to support an argument, as "immutable mobiles" (cf. Latour 1986: 5f).[53] Choosing the right plot out of the numerous plots produced by the many parameter runs to stand for the model in public is another crucial bifurcation. Thus the student's quip in the story about one plot basically summing up his work hit the nail on the head.

But if it is true that "[o]ften there is no opportunity to compare simulated images with the original - there may be no possible perspective from which to view things like this, or it may even be that the depicted material does not exist in the real world" (Küppers et al 2006: 8), how then do the modelers make the connection between model and world? With this question I come to the last part.

5.3.2. "And in the end, ideally, you are able to observe structures that you can also observe in the system you really want to study."

How to characterize the relationship between model and target was a source of tension in the research group. Working with conceptual models was a concession to the difficulties of modeling social processes (see part 6.2.). Still, the question remained, whether comprehensive models of reality are possible and desirable as a middle- to long-term goal for their collective efforts.

[53] In ANT, immutable mobiles are "objects which have the properties of being *mobile*, but also *immutable, presentable, readable* and *combinable* with one another" (Latour 1986: 7, emphasis in the original). They are powerful allies in making an argument because of the simple mechanism of "I will show you" (ibid: 14).

In the meantime, however, they agreed on relating model and world through "narratives". In this part I will focus on how, when the model formats are more or less aligned, visualizations become the predominant model format: plots, visual illustrations and "narratives" understood as non-visual illustration are made to work together to make sense of model dynamics and to present "the model" to others.

In section 5.1.2. I have already mentioned the imagery used to characterize the relationship between model and world. Conceptual models are not predictive or realistic in the sense of attempting an exact description of the world. Still, they were supposed to render central processes intelligible: "Especially dealing with complex systems, a model is not enough to describe a system, but you learn to understand it. And in the field I am working in I don't see any models to describe a reality completely, at least not yet. Maybe that will be the case in the future" (06/02/2018, 24-26).

With this, the models made an – albeit limited – claim to realism: They were supposed to be "as abstract as possible while still reflecting qualities of reality" (01/02/2018, 250-251), "the relevant aspects" (06/02/2018, 50) the "characteristic, key figures" (31/01/2018, 86). Comprehending dynamics within the model became possible precisely because of their simplicity. Finally because of this claim to limited correspondence, "reality" hopefully became fathomable: "Ultimately, you can build infinitely complex models, but in the end the question is what the added value would be if you then understood the model as little as the reality which you want to describe, yes, but also want to explain" (23/01/2018, 144-146).

Even though the modelers had to quantify within the model and formulated a need for quantifiable, generalizable results in the future, conceptual models allow for qualitative rather than quantitative observations: "In none of our models we claim to predict something […]. But we are able to generate understanding of processes, which are not directly visible, I would say. Certain non-linearities, feedbacks. And you can roughly estimate orders of magnitude" (16/01/2018b, 183-188). This caused ambivalent attitudes towards direct model applicability:

> In my view, I am still able to grasp the essential processes in a model. You have something like a skeleton, quantified, but still describing essential things. And in the end it allows an interpretation, or re-transformation to the real-world system. Even though, in the case of our strongly simplified social-ecological models it is questionable whether this is even possible. (16/01/2018a, 44-50)

The research group bridged this gap with a recourse to so-called "narratives": generalizable kinds of stories the models support. In the story introducing this section the student used different kinds of visualizations – plots and drawings telling a story – to literally illustrate the narrative connecting model and world. These narratives were meant to provoke discussions and reactions inside and outside of the academic community because like the plots, they illustrated model dynamics for people unfamiliar with mathematical or computational models. In them, a connection between world and model was carefully, but also normatively articulated. The following example illustrates this point.

A more experienced modeler explained to me how he had tried out a new idea for network model of opinion dynamics, based on a problem a standard version had: after a while inevitably every agent had the same opinion or a completely different one. Either way, communication between them stopped. Already a very simple, first implementation of this idea, where an agent is a node in different networks instead of just one, produced a surprising result:

> In that case I was really surprised, because I didn't expect this to happen, something comparable, yes, but not this particular kind of state. […] Take this scenario, for example: In the beginning there are eight different opinions in each network. And then it runs for a while and then you have six, then only five, and at some point only two opinions. Until the end of all days you have two, it does not sink to one. That is the punch line. […] The plurality remains, but microscopically a lot changes during the whole amount of time. (31/01/2018, 404-419)

Already at this early stage the phenomenon was made sense of through a narrative, possibly aligning mental model and computational model format: "And this is such a narrative, that you need different levels to secure plurality. If everybody only stays within their own network it might not be good for plurality and exchange of opinions, so to speak" (31/01/2018, 347-350). This result was thrilling and later in the interview he emphasized again: "intuitively, this was not to be expected, at least from the point of view of models. […] Suddenly, something completely new emerged" (31/01/2018, 427-431). He added: "What you observe in systems out there is the existence of a plurality of opinions. It simply is like that. And this is also a qualitative observation, I wouldn't even need to quantify that" (31/01/2018, 469-472).

For the research group, these qualitative statements and narratives present a way of coming to terms with the difficulties of modeling what they deem social processes:

> While natural fields are describable for the larger part with physical equations – or at least in theory, if we would know them all – this is not always the case for social areas. Of course, you can find certain rules and write certain simulations, but that is far less predicative […]. And you can make both areas compatible when you indeed describe them on different levels. You could use the result from the simulation of melting ice shields, for example, as an input variable for the human system. This, you can model with a network. But then you have to keep in mind that it is only telling a narrative. And then, you put this variable 'melting ice' in there, a bit artificially, and then observe the human system for swings of opinion, for example. (19/01/2018, 128-136)

In the last section I will describe assumptions about "the social" and "social processes" as well as the related difficulties in modeling.

6. Problematization: Considering Ontology in Modeling "the Social"

Until now I have not explicitly dealt with model content, other than stating that for the research group developing models and a modeling framework of socio-ecological processes relevant on a global scale constituted the over-arching aim, while at the same time the individual modelers worked with simple, conceptual models. Two of those exclusively captured social processes, while several others included a renewable or finite energy resource.[54]

Due to the diversity of models, I did not delve as deep into their content as I would have liked to during my fieldwork. Instead, I decided to focus on a more general grasp on modeling practices in the group. Still, epistemic assumptions about "the social" and related concepts, their representation, operationalization and efficacy remained an important focal point in my considerations. Therefore I decided to include this final part into my thesis which together with the part on bifurcation as a research perspective frames the main part on modeling practices. Nevertheless, as the title indicates, I want the following to be understood only as a tentative problematization that is still very much a "work in progress".

First, I will try to untangle some assumptions about the social, especially how it is conceived in relation to ecology. Then, I will distinguish between three kinds of difficulties the research group associates with modeling social processes, most prominently feedback loops between a model and the processes it is supposed to describe. Finally, I will problematize assumptions and the feedback problem using Paul Kockelman's (2013, 2017) proposal for a "Bayesian Anthropology".

6.1. Assumptions about "the Social"

In the mental maps of the subjective perception of the research group the modelers usually delineated different thematic or methodical entities from each other, as well as people or models, depending on what was relevant to the individual person. They were drawn either as spheres or as nodes in a network and connected through overlaps or lines. Within those maps, they made no verbal connection like socio-ecology or something comparable.[55] Instead, if themes were differentiated, two to four were made out. Mostly, they separated a social, a natural and an economic sphere from each other. One sphere was always related to "nature", "environment", "biophysical processes", sometimes more specifically the "CO2-cycle" – or more generally the

[54] Two modelers worked on mainly "biophysical" processes, but they also had a more "hybrid" status concerning their involvement in this and other research groups, which is why I will focus on the other models here.

[55] One map stood out because in depicting a coordinate system where different actors were positioned it considered ranges and fluid borders. Still, the fact that its dimensions were "social" "economic" and "environmental" makes it a case in point. Stylistically, they were remarkably similar to the mental maps of the models which indicates how they are rooted in a specific thought style and thought collective (see part 5.1.1.).

"Earth System". The other spheres were often "the social" and sometimes additionally "economics" or socio-cognitive processes like "opinion dynamics" or "learning". In the group discussion at the end somebody pinpointed this as "for us it is also the question of what there is beyond economic behavior". Taken together, the maps make visible a modern nature-culture divide, albeit a bit blurry.

In contrast, in answers to the interview question "what is the goal of the research group?" concepts like the Anthropocene, interactions or coevolutions etc. were explicitly named:

> The point for the research group is that you can't take humanity out of the equation of the natural system. The Anthropocene principle is that humanity is such an important part of our climate system that it doesn't make sense to model the thing without it. And so we try, maybe a bit blurry to approach stuff that has been sidelined in the big models. (18/01/2018b, 41-47)

> In any case, for me it is decisive that this idea of human-environment-interaction is more strongly considered in models. And that means not only to ask how people influence the environment, but also to consider the other direction. To look at that with tools such as networks, ABMs and stuff is the core for me. (23/01/2018, 56-60)

However, on a more basic (ontological?) level, even these concepts were employed in relation to a dichotomy, or rather, several dichotomies: social system – climate system; deciding subjects – determined environment, natural components – social components:

> Our aim is to get a better understanding of how humanity, or the whole social system as the opposite pole to the climate system behaves in interaction with the earth system, the climate system. And how to approach the discussion surrounding the Anthropocene from the natural sciences. And we decided to take the path of co-evolutionary modeling and analysis.(31/01/2018, 161-166)

At the same time it was clear that the implication of the Anthropocene is that not everything fits into these oppositions anymore. This was expressed in terms of social metabolism, for example in reference to the work of Marina Fischer-Kowalski et al. (e.g. 2011):

> At the research institute there is a trend to focus on metabolisms, material and energy flows between society and nature. Also historically, which factors, like sedentism, urbanization, and so on, impacted these flows […] and this is a try to transfer social sciences onto natural science questions. (13/02/2018, 195-203)

But this "diagnosis of hybridity" (see footnote 11), became – again– a problem of delineation:

> The way I perceive it, social metabolism is a thing that is human as well as natural. And then you get into trouble, like, where is the separation now? And isn't everything 'Anthropocene' now? That is the beauty of mathematics, I have my agents, my environments etc. and that is that. (18/01/2018a, 151-155)

Before I go deeper into the issue of clearly separating a social, a natural and a hybrid sphere, I want to bring out (1) what actually was subsumed under the umbrella of "the social" or "social processes" and (2) the difficulties of modeling this that the research group made explicit. In some interviews, being social was explicitly limited to people and their interactions:

> I: Another question: what do you mean with 'social' when you talk about 'social networks'?

Human interaction. Yes. Human micro processes. That you look at people at all, basically.

I: That means humans are through and through social beings? […]

I mean, if you take a closer look, there are processes inside a person that are not social, but physical. But in my case, when it is about opinion dynamics, it doesn't play into it. […] Even though, it is also a social reaction, if somebody next to you smells really bad and you move away from them. Actually that is all social stuff: What leads to an exchange of opinion, and what does it look like? (06/02/2018, 327-330, 337-345)

In practice, in the networks of ABMs a node could be an individual person, like in the example stories introducing the earlier sections of this work. But they could also stand for a household, or a farmer and some land on a geographic network, as this mental map illustrates.[56]

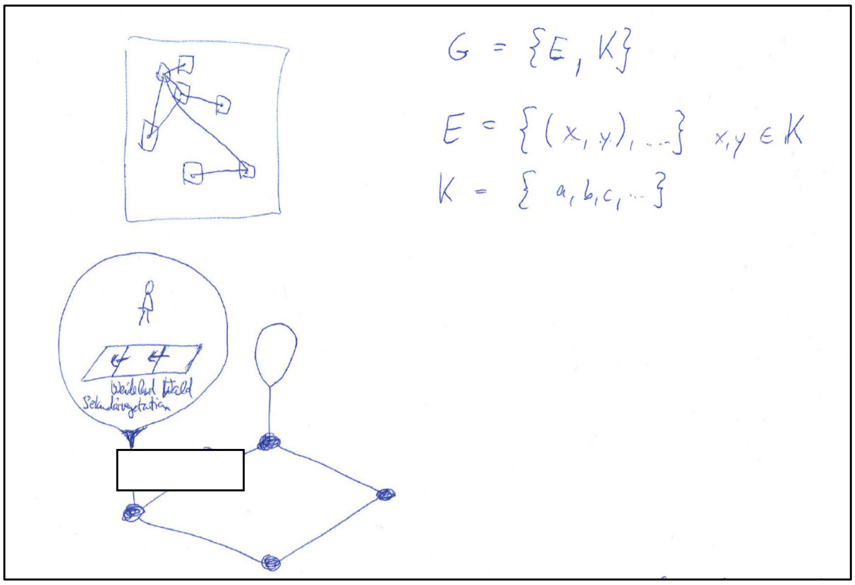

Figure 8 Mental map of a model (23/01/2018). Reproduced with permission.

That particular model seemed to me to be relatively balanced concerning socio-ecological processes. Still, even in this model, decisions were made by the person: "Of course it somehow is a unity, the field and the deciding agent. It belongs together, but the function of the decision describes a person, or a household or something like that, and there I would make a strong separation to a nonliving nature, which reacts instead of acts" (23/01/2018, 227-230) [57]

Another model described society as the sum of individual people aggregated in institutions and political parties:

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[56] Nodes can also be geographical points in the climate system or a network can describe traffic infrastructure, to name but two examples. Mathematically, a network is a very abstract entity described by two sets, as can be seen in the mental map in the top right corner.

[57] Even though the decisions made are subject to a form of "bounded rationality", it is described as socially, not ecologically bounded: "it is not a process, where agents simply say 'I will do this', but it has a strong social component" (23/01/2018, 176-178).

> You break up the social fabric into some sort of pyramid. […]. You have individuals that are affected by something happening in their environment […]. The question is, whether then something 'more tangible' forms, maybe interest groups, organizations, political parties […] that in turn are also connected with each other. (31/01/2018, 268-277)

As a fruit of their interdisciplinary composition, the group also discussed ideas of social stratification – class, agency and class-bound agency, as well as bounded rationality – all within the structure-agency-paradigm (cf. e.g. Ortner 2006 for an extensive discussion of this opposition). Looking at the group's model portfolio and assuming a complex systems perspective, for me the question arose whether for them "the social" emerged from the complex system or whether the micro processes within a system were "social". I posed this question in the group discussion concluding my data collection. After some discussion, one answer was that it was both, but that given enough data and a better comprehension of the processes, "the social" or "society" is ultimately reducible to social micro-processes and then describable through ABMs, for example: "But I think in the end, the emerging social is all reducible to micro-processes of behavior in the models. The only thing is that the way it emerges is unpredictable" (fieldnotes 26/02/2018).

This corresponds to how social processes were thought of as statistical processes, in which case the emergence of social groups became a case of statistical clusters. Modeling these clusters again would then be a matter of available data: "In the end, what I do amounts to a cluster analysis, and we discussed whether it makes sense at all, to open drawers and sort individuals into them like this and then see what happens. That is of course a questionable approach" (13/02/2018, 136-140). Still, these emerging macro phenomena are not "the social", in the sense of social theory building on the Durkheim's oeuvre. The idea that the social is reducible to micro-processes is opposed to his conception of the social as a reality sui generis: [Social facts] are general among, external to, independent of, and constraining upon particular individuals […]. They constitute a distinct level of reality and are not reducible to the meeting of individual intentions" (Lukes 2015: 701).

For modeling this implies that from an empirical, social anthropological perspective, there is no "point zero" where un-socialized, bare agents interact. "The social" is always already acting and enacted, changing and changed by agents and their practices. Phrased differently, one could say the social becomes an agent (in the ABM-sense of the term) in its own right and needs to be included as such.[58]

[58] Interrogating the individualism inherent in ABMs, Brian Epstein also concludes that "in light of the nonlocal and cross-level dependence of social entities I suggest that true micro-foundations – even computational and non-reductive ones – are a pipe dream" (Epstein 2012: 133) and suggests to include macroscopic properties of a system in question already in the model scenario.

6.2. Difficulties in Modeling “the social”

From the point of view of the modelers, several difficulties of modeling social processes became apparent in interviews and observations. All issues were again formulated in reference to and delineation of concepts of “nature”. These difficulties are reducible to three aspects: (1) the challenges of interdisciplinary research, (2) a lack of quantifiable data, and (3) feedback loops. I will only briefly touch upon the first two issues here and take up the third problem again in the following part.

Interdisciplinary research and research into interdisciplinary problems was seen as challenging: “A big challenge are the different groups involved, social scientists and physicists, accompanied by different approaches, and that is a difficulty” (06/02/2018, 47-49). As I already discussed in part 5.1.1, it confronts the physicists in the group with other ways of doing and writing about science. One problem here was dealing with a certain ambiguity: the existence of several valid positions next to each other. Additionally, they noted that social sciences do not attempt to give a general theory of social dynamics, not even hypothetically:

> So, I haven’t found it yet, the generally established theory of social dynamic and in my observation, in the way science is approached this is not what the typical social sciences are after. […] I mean, there is not nothing. But even the range of relevant communities still not covers what we attempt to do. And that certainly is a challenge or source of tension. (18/01/2018a, 19-26)

This leads to a lack of data in a format usable for modeling: quantifiable, aggregated and compatible with data in physics:

> A huge difficulty that I wasn’t aware of in the beginning was quantification. For biophysical processes there is a higher degree of objectivity, I believe, or whatever you want to call it. […] the knowledge has another quality, and independence from the observer. And yes, there are certain biological, physical, chemical processes and of course, a model may not work because you forgot something […]. But concerning the observer, there are no feedback loops, the climate system does not change because we observe it. […] And I think that that is different on a social level. (16/01/2018a, 66-75)

Acknowledging the situatedness of the social cast doubts whether this kind of data is even attainable:

> A lot of individual things and interactions in the social system have been well described, and the question is how, if at all, you can aggregate that sensibly. That is the difficulty, to make this compatible with quantified observations in the climate system, in the level of aggregation as well in the depth of understanding them. And then of course, it depends from a lot of factors, somehow… local factors play into it, and fluctuations in time. (31/01/2018, 181-187)

The previous quote already mentions the third problem the modelers identified: feedback loops. Their idea of feedback is that people are going to react in one way or another to their inclusion in a model. This leads to such a deep change in people’s actions, mental models etc. that the basic modeling assumptions as well as the model output are not adequate anymore:

> If you include people, then they also have mental models, and when you build a model it can lead to a change in that mental model and the people's behavior. So, I think that this is a big challenge that you at least consider that models don't just depict reality but in the case of systems involving people they can also create reality. (23/01/2018: 65-70)

> The biggest difference concerning modeling is […] that nature doesn't care about the assumptions you make about it. And people almost certainly are going to react when you tell them 'you will behave in such and such a way'. (01/02/2018, 18-21)

An attempt by the research group to solve all three of these issues was the development of a modeling framework, containing a number of components and methods to build models with. The framework could be used by other disciplines, addressing the problem of interdisciplinarity; it could be used to generate more quantitative data or building on quantitative data not directly accessible for them, dealing with the problem of a lack thereof; and it could maybe even be flexible and mobile enough to be adaptable to feedbacks within and outside the models. One of the modelers stressed emphatically that the framework was not just a "toolbox" containing model components/ entities, but that they had developed a new fundamental method of linking model components:

> Framework for me means two things. For one, it is this construction kit, with which I mean the concrete components, for another there is more abstractly the way these components can be connected […] concerning the programming, that is not easy […]. And in the framework there is this very elegant way of solving this by adding and subtracting certain terms independent from whether a certain component exists at that moment or not. That sounds very abstract but it is actually widely applicable. (31/01/2018, 208-224)

At the beginning, this way of linking components was grounded in Python as a programming language, which made it easier to express it, which became obvious when translating the framework into C++, another programming language:

> In Python you can tell two different components 'there is a resource' and when you combine those components, there will be only one resource, not two. They refer to the same resource. And in C++ that is not that easy. And that is one of the finesses that is precisely the cool thing about the framework, that in the case of the Python version this is a way of thinking and coding that goes beyond the framework itself. You could write completely different models with it […]. It is such a way of thinking that is totally cool and that could click with the whole physics community. (31/01/2018, 227-236)

That way of linking components, called mixing, had already existed in computer sciences, but its application to modeling was new. The moment of abduction in this innovation prompted me to think differently through the problems explained above, in terms of inference and ontology. To me, it seemed to have a different kind of reach or generalizability than some of the other models I had encountered in my fieldwork.

6.3. Thinking through Ontological Transformativities

So far, modeling the social and the related practices and problems have been discussed on the level of available methods and data. Seen like this, modeling socio-ecological processes is mainly a question of feasibility, and of epistemology: How, if at all, can I know the social through my model? But the feedback issue in modeling the social has ontological aspects because it makes explicit (1) how models may have deep implications for "what there is", and (2) how there may be certain processes and effects models will not be able to grasp.

This is related to my framing of modeling practices in terms of bifurcations as well as degrees of irreversibility and the questions asked in part 4: Why does it become harder at certain points to change a model's structure? Which bifurcations are more crucial than others? Why are some new ideas and processes harder to introduce into a model than others? And when/ for which phenomena does it become necessary to build a wholly new framework?

The main part of this work was able to provide some examples of bifurcations, like the choice of programming language or the kind of literature research performed, as well as the material-semiotic practices they are embedded in. But what about bifurcations already in place before the process of model construction begins, through a person's socialization or a discipline's history? In a way, these bifurcations also socialize the models. This final part rests on the assumption that the feedback problem as an onto-epistemological problem (Barad 2003, see footnote 37) is precisely a case of such bifurcations made before even beginning to construct a single model. It means taking a step further away to try and understand what models in general can and cannot do.

I refer to "ontology" as it is used in recent debates in anthropology and STS, where it is less about clear definitions of "what there is" and more about its contingency. These debates inquire into the pervasiveness, given-ness, mutability and contingency of the very fabric of the world and its intertwinement with our ways of perceiving, experiencing and ordering it. Are we meeting the universe halfway (Barad 1996)? Have we ever been modern (Latour 2015)? And what is there beyond nature and culture (Descola 2013)? Those are just some interventions that sparked these discussions. And even though I will not go into them in detail,[59] the fact that questioning the nature-culture divide (and all the other, smaller divides) in a number of ways is at the heart of these debates, is one reason why I choose to problematize the epistemological issue of "feedback loops in modeling the social" via ontology.

Before I go into more detail, I want to emphasize that the following is not a suggestion for *solving* the feedback problem. On the contrary, as a problematization, it is another way of *framing* the problem.

[59] These debates are often subsumed under the notion of an "ontological turn", see Holbraad/ Pedersen 2016, and Jensen, Ballestero et al 2017 for detailed summaries and discussions.

I want to look at one particular attempt to theorize mathematics, computation, automation, inferences, epistemology, ontology and feedback loops together: A paradigm the linguistic anthropologist Paul Kockelman (2013, 2017) called "Bayesian anthropology". I am employing this particular theoretical framework here for four reasons:

First, some of the modeling efforts I was able to observe or talk about in the interviews fit quite neatly into Kockelman's schematization.[60] Secondly, I think Kockelman's framework helps to understand, why model construction and model alignment can become much more difficult, maybe even impossible for certain processes. Thirdly, the underlying assumptions in modeling the social are ontological and need to be problematized as such. Laying out a contrasting ontology is one way to do this. Fourthly, while still very abstract, "Bayesian Anthropology" is more systematic and concrete than other interventions in relation to ontology and explicitly deals with automation and computation, with which I follow the field's need for simplification and quantification to a certain extent. But it also addresses the limits of computation.

Before I focus on his concept of "ontological transformativity", I want to briefly describe how Kockelman situates his approach in anthropology. He departs from "the tension between relatively reductive and nonreductive approaches to human behavior more generally" (2017: 146), e.g. in anthropology between structuralism and practice theory (cf. ibid: 147). Specifically, he thinks linguistic anthropology and computer science together, but makes a much more far-reaching suggestion concerning the mediation of communication and information processing. He uses the image of the sieve as a very general way of information processing through sorting – something fits through it or it doesn't. His example are the algorithms of spam filters based on Bayes' Theorem of conditional probability. Sieves bring out similarities, patterns and predictability (cf. ibid: 171) and hence do work – much like epistemological tools. Information processing is understood as "the organization of complexity for the sake of predictability" (2013: 38). In that sense, models as epistemic tools are also sieves and process information or data.

His fundamental point is that some information is easier to process automatically than others, because of the depth of the assumptions it potentially transforms. Some ways of processing information may even have fundamental effects in the world, like feedback loops. The central vanishing points from where he then differentiates these ways of processing information are

[60] Even though the range of modeling approaches used in the research group made it difficult for me to go into in-depth assumptions about the social in the last part, it now allows me a to generalize carefully.

"ontology (assumptions that drive interpretations) and inference (interpretations that alter assumptions)" (Kockelman 2017: 173).[61] Ways of processing information are ultimately modes of inference. Each mode of inference is able to grasp a certain kind of ontological transformativity "whereby an interpreting agent's ontology transforms via mediated encounters with an individual." (ibid: 180).[62] Rephrasing his initial point this means that some ontological transformations are more difficult to automate (e.g. compute or describe mathematically) than others.

He distinguishes five kinds of ontological transformativity that are characterized by the mode of inference needed to process them respectively (see Kockelman 2013: 44-48, 2017: 180-182 for the following definitions). With each level of ontological transformativity, the ontological assumptions that are subject to change are deeper, leading to increasing ontological inertia. The difficulty to automate them increases as well:

> Finally, not only do these transformations exhibit different ontological inertias, they may also get progressively more difficult to mathematically formulate and technologically automate, and so the transformations in question seem to turn more and more on human based significance, and less and less on machine-based sieving. (Kockelman 2013: 48)

The easiest kinds to grasp almost intuitively are the first and the fifth kind. The first level of an ontological transformation occurring is a simple causal process, causes may be natural or social: Something happens and turns one thing into another – predictable, stable and reliable, e.g. a chemical reaction, or a marriage ceremony that turns some man into a husband. The fifth level of ontological transformativity is what so far in this thesis has been discussed as "feedback loops": Changes in an agent's assumptions about a world may change the world about which the agent makes assumptions.[63]

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[61] He uses "ontology" very broadly: "Such a set of assumptions might be called a theory (when articulated in relation to a scientific institution, episteme, or disciplinary formation), a ground (in the way this term was used in chapter 5), a stereotype or prejudice (when negatively valenced), a likelihood (when framed mathematically), a heuristic (when framed qualitatively, or as a 'rule of thumb'), an imaginary (when understood in relation to an underlying account or narrative about the prototypic entities involved in the domain being judged), a culture (when more or less intersubjectively shared by a group of people), and even a habitus or 'sense' (when understood as a tacit intuition regarding another's identity via their techniques of the body, styles of speaking, and so forth). The term ontology functions as a cover- all term to capture the ramifications present in each of these framings" (Kockelman 2017: 177).

[62] He borrows his vocabulary from Peirce' Semiotics, in order to remain abstract: index, kind, agent, individual, and ontology (see Kockelman 2017: 175 for definitions). Because I apply his framework to a rather concrete case here, I refrain from using these terms, even though this may limit its sweeping reach. Kockelman also means his categories to be complemented by others and "portable" (Kockelman 2013: 56). The confrontation with a particular empirical case may in turn transform the framework, in the way that ethnography brings theory and empirical data together in order to bifurcate concepts and generate better descriptions and understanding (cf. Strathern 2011, Hirschauer 2008).

[63] This issue has received some attention in STS, foremost by Ian Hacking. He described looping effects, e.g. between people and the development of a diagnosis in the medical sciences, that go beyond a change in mental models as they were described here to ways of how people are constituted in and constitute their very existence. Subsuming various empirical studies under the umbrella of an historical ontology, he writes: "Historical ontology

Once these two are clear the others are easily understood in more classic terms of inference: Deduction, induction and abduction. Kockelman uses Bayes' Theorem of conditional probability to illustrate these (I already mentioned it in the story introducing part 5.2.). This equation basically captures how an a priori-probability of something being the case is transformed into a different a posteriori-probability after the person (or agent) that assesses the a priori-probability has acquired an additional piece of information about that case. With this rather vague way of describing it I wanted only to highlight that Bayes' Theorem is about a moment of transformation, namely ontological transformativity No. 2, or *deductive* inference.

Transformativity no. 3 is the case of *inductive* reasoning, e.g. when data changes hypotheses or, to stay with the example, when the underlying statistical profile is changed and with it the likelihoods that were used to determine a priori and a posteriori probabilities).[64]

In order to grasp transformativity No.4, *abduction* as a mode of inference is needed. Theories and data have to be related in unusual ways in order to deal with a surprising observation. In the case of Bayes' Theorem, this means more fundamentally that it is not appropriate as a method to deal with a certain case anymore and another approach has to be used.

Ontological transformativity	Ontological Inertia	Inference/ effect	Difficulty to automate	Mathematical Example
I.	Strength of assumptions	None (causality, assumptions drive interpretation)	Strength of inference	
II.		Deductive		Bayes Theorem
II.		Inductive		Change in likelihoods
IV.		Abductive		Theorem does not apply
V.		Feedback (interpretations alter assumptions)		

Figure 9: Schematization of the five kinds of ontological transformativities (AK).

is about the ways in which the possibilities for choice, and for being, arise in history. It is not to be practiced in terms of grand abstractions, but in terms of the explicit formations in which we can constitute ourselves, […]. Historical ontology is not so much about the formation of character as about the space of possibilities for character formation that surround a person, and create the potentials for 'individual experience.' (Hacking 2002: 23). Kockelman himself focuses on the middle transformativities: "The first and last kinds of transformativity (1 and 5), in various guises, have received a huge amount of attention in anthropology, and critical theory more generally. In contrast, the middle three transformativities (2– 4) are relatively under- theorized, and so will be the focus in what follows" (Kockelman 2017: 182)

64 In statistics, broadly speaking, probability describes the outcome, and likelihood the values used to calculate it.

In order to apply this to my case, I want to recall the four reasons for going through all of this that I gave in the beginning:

(1) The range of modeling practices, methods and models maps onto Kockelman's schematization to a certain extent.
(2) The schema could also indicate why modeling some phenomena is harder than others or potentially impossible, simply put.
(3) The underlying assumptions in modeling the social need to be problematized as ontological assumptions.
(4) Bayesian Anthropology is a systematic framework addressing automation and computation and their limits.

While most of their models are conceptual and deductively inferred from theory, other models are built on the basis of large quantitative data sets and present a case of inductive inference (or both). The development of a new methodology with the modeling framework can be seen as an instance of abduction.

However, the problem of feedback loops in modeling "the social" is either captured by the fifth kind of ontological transformativity, the kind that is impossible to compute, or by the fourth kind, where automation is a question of adequate methods and methodologies. Thus, the (im)possibility to model something depends on the kind of ontological transformativity and its accompanying type of inference. Bayesian Anthropology is a first step to recognize and systematize this in modeling as well as in anthropological knowledge production. Even though its operationalization is an open question, it sensitizes anthropologists and modelers alike for evaluating the adequacy of methodological approaches or models to certain questions, such as feedback effects, beyond "better" or "worse".

The ontological assumption in modeling the social that I want to address with this framework is its separation from other spheres like "ecology", "environment", "economy" or even "the hybrid". What I want to problematize is this way of answering the Anthropocene – a profound ontological transformation – through further dissociations. The Anthropocene shakes these fundamental distinctions. That does not mean that for grappling with the first ontological transformativities an assumption of separate spheres might not be helpful, or that I do not recognize the need for distinctions and clarity in order to build models. But the assumptions at stake in modeling socio-ecological transformations are fundamental.

Bayesian Anthropology is not just a schematization, as a way of ordering "world" – an ontology – it also illustrates an alternative to interacting yet separate spheres like "social system" or "ecology": instead of thinking in vertical "pillars of an Earth System", where only one or two pillars need to be modeled a bit more, it evokes thinking in horizontal layers of socio-ecological processes of varying ontological inertias and transformativities requiring different kinds of inferences.[65]

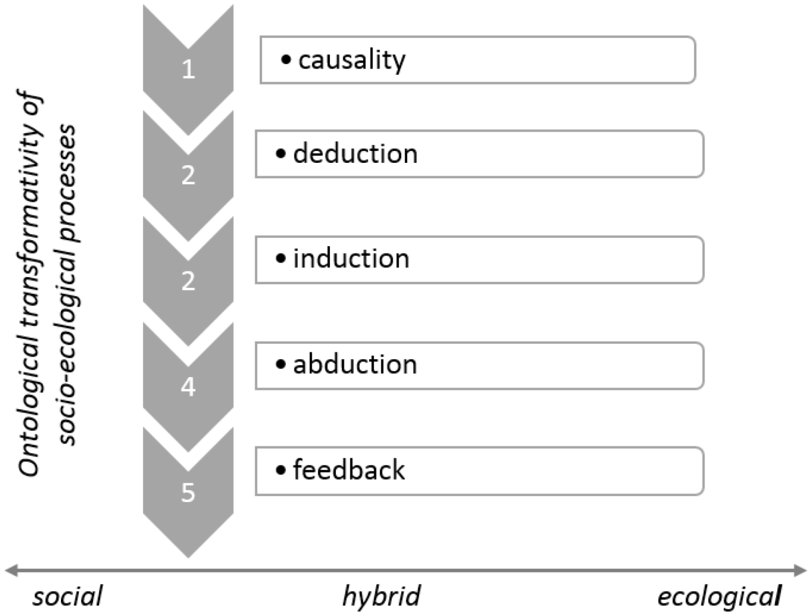

Figure 10: Schematization of socio-ecological processes and their transformativities (AK)

And in order to understand (and model, if you wish) what is happening in the Anthropocene, we need to deal with ontological assumptions of various depths. These depths require new and adapted ways of knowledge production, and modes of inference beyond induction and deduction especially in dealing with the so-called feedback problem or ontological transformativity no. 5. The question is then not if, but what anthropological methods and knowledge can contribute (see also Palsson et al 2013), in a potentially collaborative or *co-laborative* way – "temporary, non-teleological, joint epistemic work aimed at producing disciplinary reflexivities not interdisciplinary shared outcomes" (Niewöhner 2016: 2).

I will end here because as stated in the beginning, this is supposed to be a problematization, a suggestion for another way of framing problems in modeling "the social", not yet a fully developed research program. In practice, work on processes beyond the nature-culture divide is probably already happening in the research group and elsewhere. Further inquiry into modeling practices could hopefully show, how in everyday practices ontologies are already transformed. One would just have to know where and how to look – and maybe this means looking through a different framework.

[65] From this ensues an exploration of socio-ecological processes as practices (as informing my research perspective, see part 4), as phenomena (in the sense of Barad 2012) or indeed as processes in an alternative conceptualization e.g. as proposed by Alfred North Whitehead (1985 [1929], Stengers 2011).

7. Summary and Outlook

I have demonstrated how modeling can be understood as a continuous process of aligning different model formats almost completely, while each is contributing something particular to the model in a productive misalignment. In order to do so I have employed a selective focus on bifurcations and degrees of irreversibility grounded in specific material-semiotic and socio-technical practices: simplification, experimentation and visualization. They each are related to some model formats more than to others and they interlock at specific moments.

Simplification is on the one hand an aesthetic modeling principle and on the other hand a necessary outcome of aligning model formats that do not perfectly correspond to each other in experimental, iterative practices of assembling the computational and the mathematical model. Next to being a way of aligning, experimentation is also a way of learning about the model through its construction and manipulation. It allows the model to serve as an epistemic tool or rather, each slightly different materialized model format as a slightly different epistemic tool. Visualization plays a double role as practice and model format. As a practice, it makes strong misalignments visible, e.g. between mental and computational models through plots of implausible model output, as well as processes within the model, e.g. effects of different parameter settings. As a format, together with illustrative narratives, visualizations are used to stand in for "the model" to others unfamiliar with the model and modeling in general to varying degrees.

In developing that argument I accentuated specific bifurcations: The decision between using an existing model and developing a new one, the choice of literature and the willingness to engage with literature outside of physics, more minute iterative bifurcations in aligning computational and mathematical models, the choice of programming language, and finally the selection of visualizations as stand-in for the model in presentations and publications.

Identifying all possible bifurcations and classifying them after degrees of irreversibility would have been beyond the scope of this project, but I was able to develop a basic understanding of modeling practices, which was my main goal. In giving a detailed and empirically grounded account of modeling practices in a specific setting I operationalized several concepts such as bifurcation, degree of irreversibility and alignment while at the same time situating this thesis in various theoretical approaches and debates in STS, philosophy and anthropology: collectivity in scientific practice, epistemology of computer simulations, materiality in experiments, laboratory studies, assumptions and ontology.

Finally, I turned to the subject matter of the models and developed and problematized uses of "the social" and "ecology" as separate spheres as the ongoing efficacy of the nature-culture divide alongside the need to demarcate additional spheres of "the hybrid". In contrasting this with another ontological sketch (Bayesian Anthropology) I suggested an alternative framing of these ontological assumptions and the resulting problems. In the terminology of this work, ontological assumptions are bifurcations determining the modeling process and "socializing" models before it has even begun. In order to come to terms with hybrid, socio-ecological processes as they become more and more pressing in the Anthropocene, it is necessary to question these assumptions as well.

Over the course of the last 60 pages I have already elaborated on related issues and hypotheses that merit further consideration to some extent. First of all, it is necessary to break down each practice of alignment into further practices, especially simplification. A comparative approach including other research groups might be fruitful in this context. Also, mirroring and completing this work, an account of continuities and reversibility in modeling is needed. An operationalization of Bayesian Anthropology and the next steps of a potentially interdisciplinary or colaborative research program should follow at the point where I ended my final part, with particular reference to the socio-ecological content of models. Additionally, more inquiry is needed into a potential non-anthropocentric account of epistemology in modeling here that brings into being such hybrid figures like the model-modeler, as I was only able to begin it here. Finally, the role of experience and skill in modeling should be considered more extensively as well as how the models as epistemic tools function in other contexts, e.g. policy, and together with other methods and methodologies, e.g. qualitative approaches.

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