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SOCIALIZATION INTO NUMERICAL SIMULATIONS: THE PERSPECTIVES OF SIMULATIONISTS IN ASTROPHYSICS AND OCEANOGRAPHY

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Socialization into Numerical Simulations: The Perspectives of Simulationists in Astrophysics and Oceanography

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Abstract

With the rise of numerical simulations as a common technique for producing scientific knowledge, the everyday working activities of scientists involved in computer modelling center on computers and the virtual world they create. Like experiments, numerical simulations are capable of generating surprises and unexpected results. This article addresses how simulationists handle unexpected results, how doctoral students learn to do so, and the perspectives that the socialization into the activities of numerical simulations generates. On the basis of ethnographic case studies of astrophysics and oceanography, the analysis draws inspiration from Mead's (1934) discussion on play and game and Shibutani's (1955) development of Mead's thoughts regarding reference groups and perspectives. The development of computer models creates a tension between play and game as different perspectives. While the focus on programming and computer work may impede the chances of achieving an individual scientific career, the possibilities of dealing successfully with uncertain output are greater when there is a familiarity with the "inside" of the numerical model. Comparison with observations is a way to evaluate simulations from the "outside" and the use of observations illustrates how the perspectives of play and game may can co-exist. I also show how unrealistic outcomes are sometimes interesting in themselves, and how the fascination with these virtual features illustrates simulation work as a form of play. The paper concludes with some methodological reflections related to the reconstruction of the perspectives and a discussion of the findings in relation to previous research on simulation modelling.

Introduction[1]

The role and influence of computers in modern society cannot be underestimated, as Sherry Turkle (1984[2004]; 1995), among others, has shown in her extensive studies of computer culture. Turkle explores the relationship between people and their computers, the holding power of computers and how they embody the boundary between the physical and the virtual. Yet it is unfortunate that discussions of virtual worlds tend to focus exclusively on phenomena in popular culture (Cubitt 2001: 130), without exploring the parallels within scientific culture. With the rise of numerical simulations as a widespread method for producing scientific knowledge, the everyday working activities of scientists involved in computer modelling center on computers and the virtual world they create.[2]

While the activities involved in computer simulations can be partly understood as play (cf. Dowling 1999), it is more common to discuss computer simulations as a new form of experiment (e.g. Merz 2007). [3] Like experimental work, numerical simulations result in large quantities of numbers (output) which require analysis and there is a constant concern with

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[2] Numerical simulations are based on computer models, which are transformations of mathematical models into algorithms which are then translated into computer code (Winsberg 1999). This enables researchers to execute (run) the program and thereby produce the simulation. Computer simulations in the physics based sciences tend to reduce the full complexity of the phenomena under study to a small number of physical laws; the related equations define the dynamics of the system. If the differential equations are non-linear, they cannot be solved analytically since it is impossible to write down closed form equations that represent a unique solution. The continuous equations are therefore discretized and transformed into difference equations for which solutions can be approximated. Because of their finite form, the simulation models often divide space into a large number of points resulting in a grid, and the numerical solutions for the equations are calculated for each grid point. The grid leads to a one-, two- or three-dimensional model domain. In dynamic models, there is also a time dimension. The smaller the distance between the grid points and the time step, the higher the resolution of the simulation model. In this article, I use the terms *numerical model*, *computer model*, and *simulation model* interchangeably and also switch among *simulationist*, *computer modeller* and *simulation modeller* to refer to the scientists working with this technique.

[3] Modellers often refer to simulations as (numerical) “experiments”. I do not explore in-depth the meaning of this term in their usage and do not implicate any connotation of the term *experiment* regarding the logic of scientific discovery (cf. Delamont and Atkinson 2001: 105), even if Collins (1985) approaches that discussion. See Winsberg (2003) for a discussion of the relation between simulation and experiment from a philosophy of science perspective.

uncertainty and error (Winsberg 2003). In his seminal work on the practice of experimentation, Collins (1985) claims that the sole defining criterion for a successful experiment is that it produce the expected result. Expectations are based on previous conceptions developed from the scientific literature, background and training. It is against this background that the scientist interprets new results as more or less plausible. Notwithstanding, outcomes that appear as implausible from this vantage point may obviously be important vehicles for the development of scientific knowledge. Unexpected output is not necessarily considered as incorrect in the end. However, the point here is that unexpected outcomes are *initially problematic.* They require that scientists *work out* how to judge them: as worthy of further analysis (potentially leading to exciting new findings), or as resulting from errors that must be fixed. How do simulationists work with computer models in order to deal with the unexpected results they may produce?

This is something students learn during scientific training. An interesting topic in relation to unexpected results is how doctoral students learn to analyze output, and, more generally, how they socialize into the practice of numerical simulation. Previous research on the enculturation of doctoral students describes how they are rarely prepared for the craft work of science (Delamont and Atkinson 2001). They have learned about science from text books that present successful and choreographed experiments that are far from the messy reality of experimental work (Kuhn 1970; Traweek 1988), and they face the challenge of mastering new working techniques (Becker and Carper 1956: 296f.). Last but not least, they have to learn to evaluate and interpret data (Roth and Bowen 2001: 551; cf. Traweek 1988: 82).[4]

The primary purpose of this article is to address the perspectives that the socialization into the activities of numerical simulations generates. It has previously been suggested that the distinction between simulations and the real becomes blurred among scientists working with simulations (see e.g. Helmreich 1998; Lahsen 2005). In what ways can the working activities and especially the socialization into them evoke this tendency?

Using case studies of astrophysics and physical oceanography, this article answers the above questions by taking its point of departure in the activities that familiarize doctoral students with numerical simulations. In particular, it focuses on the mundane work that simulationists

[4] For a more thorough review of social studies of science research on how new members become part of science, see Campbell (2003: 899ff).

must address when dealing with problematic output. In this sense, unexpected outcomes can be seen as everyday situations to be analyzed to illustrate further interesting aspects of numerical modelling practice and its perspectives.

Exploration, Socialization and Perspectives in Simulation Modelling

Simulation models are material rather than abstract objects; they effectively work like, and in fact are, particular forms of computer programs (cf. Knuuttila 2006; Sundberg 2008) that are often referred to as *codes.* In this article, I approach them as epistemic objects: objects with the potential to surprise and outstrip expectations and imagination produced by the current way of thinking and doing (Rheinberger 2000; see also Knorr Cetina 2001). The concept focuses on the relation between scientists and the objects they explore, as an attempt to change the division between scientific objects and technology (cf. Merz 1999; see also Knorr Cetina 1999; 2001).[5] Scientists explore epistemic objects through *unfolding* and *framing*. Unfolding refers to the continuous unravelling of features, details, composition, and behavioral implications through which properties can be extracted and displayed (cf. Knorr Cetina 1999: 71f.; 197f.). Framing means to consider objects or pieces of information in the light of other such components which serve to check, control, extend, or compensate the former (Knorr Cetina 1999: 72 ff). I use these concepts to describe how simulationists investigate the unexpected and uncertain output that simulations produce.

Two other interesting questions that arise are: What modes of reasoning do these activities generate and maintain? How do simulations remind of play? In this article, I see play as a perspective from which to conduct simulations. A *perspective* is an ordered view of one's world, an organized conception of what is taken for granted, plausible and possible, but also a coordinated set of actions (Becker et al. 1961: 34; cf. Shibutani 1955: 564). It is through socialization that a person learns how to approach the world from the perspective of one's reference group, which may vary according to the situation (Shibutani 1955). "The socialized person … sets the same standards of conduct for himself as he sets for others, and he judges himself in the same terms …. [H]is perspective always takes into account the expectations of others." (Shibutani 1955: 564) According to Mead (1934), socialization takes place through

[5] Merz (1999) suggests that computer models oscillate between being epistemic objects and technical artifacts and that they act as the former when they are opened up in problematic situations. Learning how a simulation model works and handling unexpected, uncertain results can be seen as constituting such a situation.

play and game. *Play* is marked by the ability to assume the attitude of only one particular individual at a time and a capricious attitude towards role playing. *Game* refers to the ability to assume the roles of many others simultaneously and to control actions on the basis of rules of the game. While play and game are originally a way of describing the early development stages of the self among children, the concepts have also been used to analyze socialization in general (see e.g. Persson 2007).

In this article, I use *play* and *game* loosely, close to the everyday meaning of play as about playfulness and game as related to rules. For example, to take the role of the other is an essential part of Mead's (1934) notion of play. My usage does not take this into account. In simulation practice, we can compare play to a narrow focus on the simulation model and its properties (a focus on you and your computer model so to speak) as opposed to focusing on the whole "game" of science (astrophysics or physical oceanography). The game includes the work with the computer model, but also the views and expectations of the other actors (supervisors, other scientists, journals) and the structure and organization of academia in general – what can be called "the rules of the game".

More specifically, I refer to play and game as embodied perspectives, evoked in different situations. Perspectives develop over time and people can handle different and contradictory perspectives and switch between them depending on the situation (cf. Becker et al. 1961). It is because of the symbolic interactionist focus on how ordered, but sometimes contradictory, views develop within groups that I use this theoretical stance (not the theoretical viewpoint of game theory or the cultural studies of computer games, for example) when analyzing the accounts on numerical simulation practice. As perspectives or modes (rather than as stages of a socialization process), the socialized person may switch between play and game, whereas the newcomer is unable to act in complete accordance with the game (cf. Persson 2007: 161). Although my focus retains the modes, not the process, doctoral students become important as they are likely to be the principal holders of the play perspective. [6] However, from a strictly Meadian viewpoint of socialization, a group perspective could only be part of a developed self and therefore not part of play stage. In this looser use of the concept of play, one could therefore suggest that a persistent play perspective is a sign that one is not yet fully capable of acting in accordance with the role of the scientist.

[6] See Campbell (2003) for a symbolic interactionist analysis on the socialization of doctoral students.

Methodological Considerations

The empirical basis for this analysis is two ethnographic case studies of numerical simulations in astrophysics and physical oceanography, both part of a larger study that I am completing.[7] Therefore, I did not specifically choose the cases to maximize their utility for a comparative analysis regarding the themes of this article, which primarily focuses on what is common rather than specific.[8] Astrophysics and oceanography share the inability to carry out controlled experiments in the traditional sense. Processes like development and formation of stars and galaxies and patterns and changes of large-scale ocean dynamics cannot be brought into a laboratory setting and simulations – "numerical experiments" – have become common ways of performing research in these fields.

I gathered the material through open-ended interviews with twelve oceanographers and eleven astrophysicists. They were, or had all been, working to some extent with numerical simulations.[9] Of these 23 interviewees, there were six doctoral students, two post-doctoral students, ten research scientists, and five professors.[10] I had the opportunity to listen in on

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[7] The study as a whole is based on three comparative case studies of astrophysics, meteorology, and oceanography. The general aim is to reconstruct the perspectives that develop in relation to simulation modelling and develop the sociological understanding of the role of simulation models in scientific practice, for individual scientists, research fields, and science more generally.

[8] Dowling (1999) highlighted similar *attitudes* among a variety of computer modellers. The current article focuses more on what the scientists say they are *doing* and the collective *perspectives* that evolve from these activities.

[9] Regarding astrophysics, the material covers a broad variety of research work on the processes and forces (e.g. turbulence, convection, magnetic fields) involved in planet formation, accretion disks (flattened astronomical objects made of rapidly rotating gas which slowly spirals into a central gravitating body), mass loss and formation of stellar (star) content in galaxies, the birth of stars, and modelling of stellar atmospheres. Regarding oceanography, the material covers modelling of small and large scale dynamics in the oceans, the Baltic Seas, the Nordic Seas, and the Arctic, and of processes such as sea-ice dynamics, air-sea interaction, and coupled physical-chemical-biological reactions. Nowadays, ocean modelling is often a part of coupled climate modelling, in which ocean models are connected to atmospheric models. Three of the ocean modellers worked with the ocean part of a coupled climate model and two with an ocean climate model.

[10] Four ocean modellers worked in Norway and three astrophysicists in Denmark. The remaining interviewees worked in Sweden, but several of the research scientists did their doctoral work elsewhere (e.g. in Germany, Finland, the Netherlands, France). Eight of the interviews were conducted in English, the rest in Swedish. Citations from these interviews have been translated to English. To protect the anonymity of the informants, I do no inform about translation.

both formal and informal discussions by attending seminars (seven in astrophysics, seven in oceanography), two workshops in astrophysics (lasting three and four days, respectively), and an interdisciplinary doctoral student course on multiscale modelling and simulation (which I attended six out of ten days). I have also discussed some issues by email with a few of the astrophysicists I have met. I base the analysis on the simulationists' accounts of the work with simulation models and analyse the narratives as descriptions of practice or as expressions of perspective related to practice (cf. Gubrium & Holstein 1997).

The analysis consists of three parts. First, I discuss the time-consuming work of developing computer models, the focus on programming and computer work, and how this relates to the individual's career and the game. The second part presents more on the use of computer models. I describe how simulationists in general and doctoral students in particular diagnose problems and unrealistic output. At the same time, the fascination of these virtual features illustrates simulation work as a form of play. The final part of the analysis relates the role of observations in simulations to the game. It illustrates how the perspectives of play and game exist side by side, but tied to different situations.

Development and Falling in Love with Computers

Doctoral studies based on simulations often start with the task of modifying a computer model, by developing and implementing a new component, or developing completely new code. One of the major purposes of this is to gain a deeper understanding of the technique. Doctoral students in astrophysics and oceanography have university training in the physical sciences, but tend to have little initial knowledge of programming. One oceanographer emphasized that "you have to program as a modeller … which students sometimes don't understand". This lack of understanding may obviously be a consequence of lack of preparedness. At the same time, using and especially developing computer models requires a great deal of programming work.

It takes a long time to learn how a computer model works if one is going to implement a new part of it, develop it, and make it work. After the discretization of equations and writing of code – frequently using "numerical recipes" – and often implementation into an existing program, it is common to test parts of the numerical model against an analytical model (cf. Kennefick 2000: 26). This is a way of framing the former by checking them in relation to the latter. In both oceanography and astrophysics, analytical work refers to the writing of formulas and pen and paper calculations. During the development of new computer models, analytical test problems with known solutions are used as "prospective tests" (cf. Pinch 1993) for comparison of output. Whenever possible, these tests provide the lowest benchmark of computer model performance, but it sometimes takes a very long time before they can be performed at all.[11] Debugging – finding errors in the computer code and fixing them – is a constant preoccupation during all stages of development and it is very time-consuming (cf. Kennefick 2000: 26). One astrophysicist who was just about to complete his doctorate told about the difficulties in working with simulations.

> Debugging and programming, developing code, it is hard, I think. Because you forget about, when you start focusing too much on programming, then you forget about physics. I don't have the same, I have a good intuition around physics but when you start concentrating on variables and discretization and that, I feel you don't have as good enough grip in the physics anymore.

Along a similar line, another doctoral student in astrophysics said that when he started to develop and implement a piece of code to include the representation of particles in an existing computer model, it was about "learning how the code handles the particles, rather than learning new physics." These accounts imply that developing a computer model requires the understanding of "code physics". This task draws a lot of attention in itself, especially for someone with limited experience with computer models. One senior oceanographer defended the focus on understanding numerical models per se and suggested that it is a part of becoming a scientist: "Becoming a scientist is a development so to speak, at a certain stage the young researcher or student perhaps has a kind of interest where you want to understand how a model is working and forgets what it has to do with the rest. That is not necessarily something negative. It is simply a way of focusing." This quote illustrates play in the sense of focusing exclusively on the model (and forgetting about "the rest"). It also implies that this focus can be regarded as negative, but that it is only part of a stage. There is, however, a fear of retaining this focus and remaining forever in that "stage" (cf. Turkle ([1984] 2004: 20;

[11] Analytical solutions only exist for very simple problems. This is part of the reason why numerical rather than analytical models are used to approach the more complex problems in the first place (cf. Küppers et al. 2006: 11). In oceanography, this analytical technique to evaluate performance of simulations is hardly ever used because the non-linear differential equations that serve as the mathematical basis for the simulation models cannot be tackled analytically. Comparison to analytical solutions is therefore impossible.

1995: 30f.). For example, during an informal discussion with some astrophysicists at a workshop, a recent PhD said that there is a danger in computational astrophysics of "getting stuck" in model development because you "fall in love with the computers" and "stop doing science". He knew several people who have become "system administrators", referring to those who continued to focus on computer model development.

The status difference is also manifested in the fact that astrophysicists distinguish between talking about "code", including the simulation model and its characteristics, as opposed to "science", i.e. the scientific problem. Ocean modellers refer to everything that has to do with the construction of simulation models as "technical". Due to this value-laden distinction between science and technology, pure computer model development papers count as second-class articles ("method papers"). They are commonly published as working papers or user manuals, as opposed to as articles in more prestigious journals. Because these accounts reflect the lack of prestige regarding code development as opposed to pursuing a scientific career, it might seem like a better career investment to focus on use (cf. Shibutani 1955: 567). In an email, one astrophysicist described his doctoral work consisting of the implementation of a new algorithm and concluded: "It took a few years of code development, which was *costly* because time spent writing code was time lost from performing numerical simulations and publishing papers" (emphasis added). This computer code was originally developed by the doctoral student's supervisor. Later development has been conducted by his doctoral students. This is a typical scenario. Successful original developers (those who have reached a high position) tend to leave subsequent development work to their doctoral students. More generally, people in high positions tend to work more with interpretation of output than with development.

This section has shown how programming is not only time-consuming, but may even appear to be a waste of time. In the "worst" case scenario, the student loses himself in programming, as opposed to in important physical problems.

The next section shifts emphasis to the use of computer models and the activities involved in understanding unexpected output. It shows how the computer model is unfolded and framed in different ways, in part depending on the simulationist's knowledge of the computer model at hand. I also discuss the blurring between model physics and real world physics and the interest in the model as a virtual world per se.

Using (a Ready-Made World) and Understanding Output

Doctoral students who receive or search for a ready-made off-the-shelf computer model only take responsibility for the set-up of the simulation, without developing the simulation model used to perform it. They therefore do not examine the computer code closely. Sometimes, they do not even have to get their "hands dirty" – a way of categorizing the work inside the computer code. During a workshop, a doctoral student in astrophysics told me how he applied a well-known, widely distributed astrophysics computer code to his problem. He thought it was very easy to use and said that if you wrote the type of problem you wanted to do, e.g. hydrodynamics, the computer code could help to choose the required "solvers", even if you did not know exactly which parts of code were necessary for a particular simulation. "So you can get results," he said, without a deep knowledge of the innards of the simulation.[12] A doctoral student in oceanography described her work with a ready-made "world" in the following way.

> They have created a functioning world and then I can decide what happens in that world…. So I can design the experiment on the basis of this, but I don't have to go in and look very much at how they formulated the physical equations, because this is something static. The world behaves according to the laws of physics… nicely [laughs]. Those I don't need to change.

In other words, the doctoral student comments that it is not often necessary to check the code and look closely at the formulation of the equations. They are "static" and remain opaque. Yet the quote leaves ambiguous *which* world is governed by these laws of physics; is it the model world, the real world or both? This account neglects the fact that physical equations can be transformed and adapted to numerical simulations in different ways (this is one of the reasons why different computer models generate different outcomes). It also forgets that there is a difference between the physical laws that are believed to govern nature and the selection of laws and process descriptions that a particular computer model uses (cf. Lahsen 2005: 912). "A representation of a ball, unlike a real one, never need obey the laws of gravity unless

[12] In some well-developed, modular codes the user can "switch" parts on and off depending on what is necessary for a particular simulation.

its programmer wants it to," writes Turkle ([1984] 2004: 69). The student's quote above represents a far less distinct view. The consequences of this are accentuated when the understanding of how to deal with output is not straightforward.[13] In the next section, I show how modellers diagnose and investigate uncertain output, how this relates to their insights into the computer model, and how the unexpected can also be interesting.

Exploring Simulation Models

Simulationists within oceanography and astrophysics frequently find themselves in uncertain situations where they are *unsure* how to interpret the output of simulations. For simulationists who develop numerical models and create simulations, questionable output is an everyday occurrence. They use expressions like "all the time" or "many, many times" (cf. Kennefick 2001: 26) and tell how they are often uncertain whether to take such output as a departure point for analysis or to recalculate. Doctoral students, lacking experience with scientific work, struggle the most with the interpretation of output in general and uncertain output in particular. As one astrophysicist said, "[Doctoral students] are often very frustrated, and complain, 'numerical calculations, you don't know if they are *right or wrong*'."

Just like traditional experimenters, simulation modellers need to develop an appreciation of the types of errors likely to emerge under different circumstances and of how to diagnose problems (cf. Collins 1985). One doctoral student in oceanography complained about a simulation and offered a suggestion for the reason behind it.

> I got lots of strange results. For example, the temperature was below zero in large parts of the water and so on. And then it is often something numerical that isn't working and for my part I suspect that it was the time step that was too large. You know you calculate forward in time and if you take too big steps, too much can have happened along the way that is not resolved and then it gets off track.

In a similar vein, a doctoral student in astrophysics talked about a simulation he was not sure he understood, "It's a weird thing that this vortex here shows up in the boundary of the buffer zone right. I think that's a hint that it is something numerical." These two quotes show how

[13] Rather than focus on what happens when particular cases of unexpected results are published and cause heated controversies in the scientific community (see e.g. Collins 1985; 1999), I describe the different working methods computer modellers use to investigate uncertain results and handle such situations.

young scientists diagnose so-called numerical effects. A numerical effect is a particular type of artifact which is due to the numerical method that has been used to solve the set of equations, for example, the way they have been discretized. Blaming numerical effects for strange results is somewhat routine, but how do scientists determine if such results are numerical errors or reasonable output with which to work further?

The first step is to look at output, in the form of visualizations, plots or diagrams. Another way to check if numerical effects are present when the outcome is uncertain (but not evidently wrong) is to run the computer simulation once again, change some parameter, or, if possible, the resolution. We can refer to these methods as ways of unfolding through investigation of the behavioral implications of the simulation. One way of framing simulations includes comparison to observations. This is a common way to evaluate how reasonable output is. One oceanographer stated, "We don't know if it is really going on this way or not. Is it just a model artifact? That it stabilizes around half the cycle of the tides. So this, we don't really know. A colleague … is looking to see if she has some data for this." We discuss the role of observations further below. At this point, it is more important to note that these methods of investigation are the only possible ones for users who treat computer models as black-boxes and who cannot analyze the computer code *themselves*. [14] Those simulationists who are more familiar with the internal features of the computer code can also look inside it. This they accomplish by stripping the simulation model of everything except its most essential mechanism and then checking this mathematically, as a way to unfold the simulation model by exploring its composition. This is possible when working with astrophysical as well as ocean simulations. One astrophysicist said: "You do something you call toy models so you take the equations and reduce them, throw away almost everything except the part that you suspect causes the effect, so to speak. And then you go inside of them, on a sheet of paper, to see what it can be." This approach to uncertain outcomes aims at understanding what happens

[14] However, they may ask the developers of the code for help with interpretation. I will analyze the relations between users, developers and computer models in a subsequent article. At this point, suffice it to say that although simulationists emphasize the importance of choosing the right code for the problem under consideration, most simulationists tend to stick with the same code no matter what scientific problem they address (in fact, the capacities of the code are likely to influence what problem one addresses). They therefore learn and become accustomed to how the particular code behaves, including the particular weaknesses it might have. Some odd things may therefore be expected and to some extent accepted (cf. Lahsen 2005; Turkle [1984] 2004: 81).

inside the computer model when it calculates to determine in what sense the output derives from the underlying model.[15]

The importance of understanding the inside of a computer model in relation to understanding unexpected output is often emphasized by modellers who have worked with numerical model development. One of the senior oceanographers (typical in the sense that he started his career with developing a model, although nowadays he only focuses on the interpretation of models) emphasized that a lack of awareness of the internal features of computer models may lead to interpretational problems.

> If you have a good apprehension of what physics and numerics there are in a circulation model, or in a model on the whole, what it tries to model, resemble, if you know the weaknesses of the model and the advantages and what it does and why it does, then you can look at it from the perspective of *what is missing* and *what exists in reality*. I often note when I show model results and someone points and says, "can it be this and this?" and then I jump, "but it is not even in the model" …. Tides, for example, it's enough if you know there is or there isn't but it is a quite good and clear example of that you cannot point, I mean if you have a circulation model that runs without tides, that this and this mixing is a result of the tide. No of course not, because it's not included. There are other things which are not as obvious as tide, how you parameterize and model some things in the model, scales which are there … etc. If you don't know this, then *you can't understand the model*. (emphasis added)

The excerpt suggests that in order to interpret its outcome, it is important to really know the inside of a computer model. One cannot necessarily use the knowledge of real world processes to understand what is going on in a model. One has to start from, and relate to, the underlying model and construction of the computer model as whole. What is plausible in one computer model may actually be completely "unrealistic" in another. The following excerpt from an ocean modeller clearly illustrates how a result *could* have been reasonable, if another process description had been included: "We have maybe suddenly one hundred degrees at the ocean floor and you know for sure that this is wrong because you didn't include volcanism or

[15] Computer models are constructed to deal with equations that are analytically intractable. Computer simulations are therefore not numerical solutions of theoretical models but employ a generative mechanism to imitate the dynamic behavior of the underlying process (Küppers et al. 2007: 11).

anything in the model." Yet the analysis of output from the standpoint of the underlying model may also be viewed as an analysis that is "isolated from reality":

> When you run a model, results are coming out. And then you can analyze why you got those results? And then you can understand this strictly dynamically from the model. And it is actually a science in itself to understand what the model spits out and why and what, then it gets kind of its own life, isolated from reality. This maybe sounds horrible but there is actually nothing wrong about thinking like that. It is important, to a certain extent, but you always have to be conscious about that; if you don't relate to reality … then some use the word *hydrodynamical playschool*. It has no [laughs] it has no value. The taxpayers have no reason to pay for something like that, scientists are just *playing* in that case, but, but *the interesting thing is to know if our models have something to do with reality*. (emphasis added)

This oceanographer implies that value of play is disputable and questionable as motivation for funding – play has per definition no social utility (Asplund 1987: 55). Understanding the *model* is distinguished from trying to understand *reality*. From the game perspective, the "interesting thing" is the latter. But if this would be the only perspective, why coin an expression like "hydrodynamical playschool"?

Playing with Simulation Models

Interestingly, a perceived unrealistic outcome is not necessarily *only* considered as problematic. Outcome that is thought to be evidently incredible can also be "interesting". For example, one doctoral student in astrophysics said: "But you can also find yourself in situations when you get a result that doesn't have any physical meaning. You get some very interesting output but it has no physical meaning, it's a pure numerical artifact." When simulations serve as teaching tools, they do not necessarily represent plausible scenarios. For example, during a doctoral course in multiscale modelling and simulation, students had to choose a particular modelling project to work on. The climate modelling project was introduced by a climate modeller as a "wild experiment to see how you get complete ice-cover" on Earth. The intention was to make the students "get a feeling" for the simulation model and understand it. Frequently, instructors facilitate the understanding of a simulation model and its output through extreme settings. This pedagogical technique may enhance the curiosity for generating completely unrealistic outcomes.

In his classic work on play as the paradigm of collective life, Huizinga (1948: 224) suggests that modern science is quite unsusceptible to play because it sticks to the strict demands of truth. But when the modelled world behaves in an unpredictable fashion, exploration of strange outcomes that are far from true may take place. One professor in astrophysics suggested that there are diverse ways of dealing with “strange” output.

> If the program starts producing things that look *interesting but strange*, then you can work in different ways. You can say that I don’t think reality is like this so then I have to get rid of this and then you go and look. But you can also do the opposite and say this was funny, I wonder how I can make the program produce even stranger things? And then you trigger this, perhaps find some property of the program or the equations that makes it become very strange although you remove yourself from reality. *You know you remove yourself from reality, but it does not prevent you from trying to study the phenomenon and refine it*. (emphasis added)

To see how one can produce “even stranger things” unravels the properties of simulation models. It is a form of unfolding, albeit without any expectation that the results bear any relation to how natural systems behave. There is no question of conflation, but of a sincere desire to explore the simulation model itself, as a modelled world that does not command attention on the basis of being like the real world (cf. Riezler 1941: 505). The astrophysicist talks about the fascination for strange things just like Turkle ([1984] 2004: 143) discusses how adolescent children delight in spectacular screen effects that computer scientists call “artifacts”. In the astrophysicist’s wording, simulation reminds of play, a feeling of excitement and enchantment about something that is different from ordinary life (cf. Huizinga 1945). In this case, it differs from ordinary science aimed at investigating plausible, as opposed to “strange”, results.

Many scientists actually talked about how they “play around” with computer models (cf. Dowling 1999). For example, illustrating the essential character of play as fun (cf. Huizinga 1945: 11; Goffman 1961: 17), one professor said the following about what started his interest in ocean modelling: “It is really fun, fascinating. What started me out was the whole fascination of having the whole ocean in your computer and playing around with it and learning about it.” This quote also implies that a playful perspective is more likely among doctoral students, or any newcomer to simulations. Even if we consider play and game as different perspectives rather than as stages, play is primarily related to the learning of new activities (cf. Persson 2007: 132, compare the expression hydrodynamical play*school*). One astrophysicist described the work with simulations in the following way: “Simulation is an activity that reminds of play and if you would go over in the corridor where most of my students sit and follow their work for a day, you would see that there is a distinct element of play”. While this astrophysicist first refers to the similarity between simulation work and play in general, he uses doctoral students as examples. He also connects this to their interest in computer models as “virtual” by adding that he has “students who live completely in the virtual”. Interestingly, he uses the term “virtual” – derived from popular culture – to talk about the relation between scientists and their computer models. However, later on the astrophysicist stressed:

> The moment of truth is when you stand there and compare with observations …. But this feeling can be of different intensity. I think that I belong to the … type of researcher that feels strongly you can feel this “show evidence” with your models. Some others are more fascinated with the models and less with this confrontation with reality.

This astrophysicist emphasized the importance of realism, but also distinguished between types of researchers (without particularly distinguishing between doctoral students and research scientists) in terms of what importance they put on confronting the models with reality, in other words, comparing simulation results to observations. The next section discusses the role of observations in relation to the rules of the game.

The Role of Observations in the Game

In simulation modelling, it is customary to test the realism of the model through comparison with observations (see e.g. Heymann 2006; Merz 2006; Oreskes et al. 1994). This is reminiscent of the positivist philosophy of science, in which comparison of theories or models with observations is a fundamental idea and a rule for evaluating science. One should evaluate scientists’ statements about the role of epistemological “rules” of science in relation to working practice with a critical eye, but they represent the scientists’ understanding of their work and their perspective (Sundberg 2006). In this paper, the point is not to discuss the extent to which simulation models are “realistic”, or if comparison to observations is a good measure of this. The issue that interests us here is how computer modellers *relate* to the rules, as an illustration of play or game.

In an earlier study, I showed how meteorological research modellers tend to praise the principle of falsification and comparison of output and observations, but "defend" their models in practice, for example by attributing problems to data or comparison in general (Sundberg 2006). The material from the current study resembles the meteorologists' accounts; for example, it emphasizes that observations include errors. One oceanographer spoke a great deal about the general importance of "observations" early in the interview, whereas his concrete examples later focused on how "data" contain errors and are not always suitable for model comparisons:

> Data doesn't give, isn't the truth, either, really. Because data can contain lots of errors. So when you are modeling, you also have to assess data, and I had a doctoral student … and he looked at something and then he looked at all the existing data and it turned out that they contained lots of errors, so he took away some data and took away some more data and finally, he had very little data that held for the numerical study he was undertaking.

The oceanographers and astrophysicists speak of "observations" as principally synonyms of "the truth", "reality", or "nature" (cf. Merz 2006: 168f.; Knorr Cetina 1999: 52). When it comes to comparison to measurements in practice, existing data do not have the same status as the more general reference to "observations". Comparisons between output and data are also difficult; it is like contrasting "apples and bananas" (sic), as one oceanographer expressed it. Thus, when researchers talk about comparisons with observations in general, they refer to observations as an *ideal* that constitutes the basis of empirical evidence. When they speak of how they use observations *in practice,* the messiness and inadequacy of concrete data enter the picture.

The practical use of observations also depends on whether the simulations are "idealized" or "realistic", a distinction which both oceanographers and astrophysicists make (cf. Sundberg 2005: 139ff.). Compared to "idealized" simulations, "realistic simulations" are more comprehensive. They include more detailed process descriptions that interact in the calculation to better reflect the variability and complexity of real processes. The models often include more dimensions. In astrophysics, they have spherical or cylindrical rather than

Cartesian geometry. As one astrophysicist explained, the reason to “dress up the model” with “all these details” is to enable comparison with observations. Idealized simulations usually have a simpler set-up of initial conditions. They include fewer process descriptions, and they may be based on models with fewer spatial dimensions. Generally, the aim is to understand the relative importance of different physical processes for a particular phenomenon or the underlying mechanisms. The simulationist excludes descriptions of other real world processes that do not have major importance since they just interfere with the interpretation of what is important. Talking about one such simulation, an astrophysicist talked about how he “uses them as experiments, to try to understand the system, then you don’t necessarily match any observations. As in this case, we have no observations of planet in disks, we cannot compare this to anything, but we know that this happens sometime in some system.” This quote also shows that observations of astrophysical relevance are generally sparse compared to oceanographic data.

Data are actually not only used to confront simulation output. Simulations are also seen as *complements* to the scarcity of observations (cf. Winsberg 2006). For example, one ocean modeller complained that “In the ocean there are so many processes and so many regions that are very poorly measured” but also added, “Actually the models help in getting the full picture.“ Oceanographers talk about how simulations “fill in the gaps” and give “more information”, not least because they produce so much output. This illustrates the persuasive impact of the abundance of numbers that simulations generate (Law 2007; cf. Helmreich 1998). In addition, it shows how measurements do not only provide an epistemological test in relation to simulations. Output and data also complement each other as different forms of information.

More importantly, comparison to observations serves a legitimatizing purpose. One oceanographer I interviewed did not *himself* place great importance on comparison to data, but mentioned it in order to legitimatize his research to others:

> A good theoretical article can be based on simulations [you have run] for the oceans … and perhaps you don’t verify, maybe you don’t have so many observations. That’s good research. But there is *always* the *possible critique* about how realistic is this? So what I did as a young

> researcher [was to say] that the next step is to verify this, to *make the reviewers happy*, but you never did this [laughs]. *No one* makes a follow-up. (emphasis added)

This researcher apparently felt that lack of comparison is somewhat controversial. At the same time, the quote indicates that there is a common acceptance of omitting it – as long as you present your work in an approved style and thereby relate to the "rules of the game". The inclusion of the reservation "to make the reviewers happy" illustrates the perspective of game, in which the expectations of the others in the group are recognized and anticipated. To publish in an approved style and know what goes on in your scientific field in a way that reflects the expectations of the group are skills to be learned (Delamont and Atkinson 2001: 103 ff; cf. Campbell 2003: 911). Thus, comparison of models and data is part of the rules of the game to which one publicly adheres. Yet the quote points out that the perspectives of game and play may exist side by side.

The socialized simulationist in astrophysics or oceanography knows how to follow the rules of the game (in which the official striving for "realism" is important). One astrophysicist talked about why he had included a sentence on the importance of making realistic simulations in an article based on some highly idealized simulations: "If you would publish in a mathematical journal or a theoretical physics journal, then you would perhaps not write anything about observations. But because this is *Astronomy and Astrophysics,* then, if you don't have a little bit of connection to observations, then you may be afraid that they [the readers] think that this is not astronomy." [16] I do not claim that idealized simulations manifest the perspective of play and realistic simulations are part of game, but point out that scientific journals function as common communication channels. The choice of journal is therefore a choice of reference group and, consequently, of which expectations you are acknowledging. In Mead's terminology, it is a question of which generalized other's role is to be taken (cf. Shibutani 1955: 568).[17] Most important, it is the publishing *situation* which brings out the perspective of game, even if *everyday work* is to some extent conducted from the perspective and activity of play.

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[16] *A & A* is one of the most important journals in astronomy and astrophysics.

[17] This discussion evokes associations to Bourdieu's (e.g. 1988) theory of fields, the relation between the positions inside of them in general, and his analysis of the academic world in particular. However, it should be clear that the focus here is on the perspectives that the simulationists have in relation to rules.

Concluding Remarks

This article has shown how the practice of numerical simulations gives rise to perspectives that can be understood in accordance with play and game, and are related to, but not exclusively part of, the process of socialization. I have also attempted to show how simulationists handle the everyday situation of dealing with unexpected output and in addition, how this relates to the perspectives.

Unexpected outcomes lead to unfolding of computer models through adjustment of the set-up and investigation into the deepest mechanisms and framing. Observations serve both to check and complement simulation results. While this exploration's primary purpose is to determine if the results are reliable and plausible, there is also fascination for what is strange, particularly for doctoral students. However, learning to evaluate data is part of doctoral studies. An experienced scholar may consider some results as "obviously wrong" or "implausible" results. To investigate results like that is therefore considered to be exploration of an artificial world. The doctoral student who interprets the results differently may think of it as an investigation of their potential realism (cf. Traweek 1988: 82).

In addition, my observations indicate that the activity and perspective of playing is primarily a part of the working situation of doctoral students. Nevertheless, it was the research scientists who spoke most openly about both playing and interest in the modeled, "virtual" world. In fact, they gave their accounts spontaneously during the interviews. They are therefore of high evidential value with respect to the reconstruction of perspectives (cf. Becker 1958: 655). A possible explanation is that the doctoral students I interviewed had begun to acquire the game perspective, but in the position they occupied as not yet fully qualified members of the scientific world, they chose to emphasize the competence of their scientific work to a sociologist and outsider. In other words, their aim was probably to show how well they played the role of scientists. In contrast, a socialized member and participant in the game can afford to give a glimpse of playfulness to the outsider, while remaining a respected scientist among peers. This can be seen as an example of Goffman's (1961: 114f.) point that it is those who feel secure in their role who express distance, whereas newcomers leave the expression of distance to a time when their competence has been proven.

Students who use simulations for their doctoral research work socialize into this practice and the ontology of computer models through development and/or use of simulation models. This presents two “dangers”. On the one hand, the student may become “trapped” in “code physics” and focus too much on the inner workings of the computer model. On the other hand, interpretation of the output without knowing the internal features of the computer code that created it may lead to a failure to distinguish between model physics and real world physics. Yet movements between observations, theory and simulations (through unfolding and framing) are movements in an internally referential system, not about encounters with reality. This is a feature of modern science in general, not a particular feature of simulation-based science (cf. Knorr Cetina 1999: 71; 287 ff.). Nevertheless, this article has made a sociological contribution to the discussions about confusion of simulations, data and the real world by showing how playing with models is related to both situation and position. It has also shown how the focus on the virtual develops from practice. The “virtual worlds” of simulation models may enhance the facility by which the connection of scientific work to playing seems reasonable, but the more general point is that the investigation of scientists’ perspectives enables us to acknowledge that scientific practice is both serious (game) *and* fun (play). This complements the views of science as about power, alliance-building, or reputation (e.g. Bourdieu 1988; Latour 1987) (or mundane work, for that matter).

Finally, this article has focused on similarities between the simulationists we have encountered in oceanography and astrophysics, rather than their differences. Nevertheless, in discussing “rules of the game”, one interesting difference concerns the connection to observations (and analytical results), and touches on the question of whether simulations are about to establish new standards and values of scientific practice (cf. Heymann 2006). Dowling (1999) has shown how simulation modellers make use of the methodological ambivalence of whether simulation is “theory” or “experiment”. While it is problematic for a sociological analysis to draw upon these epistemologically laden categories without exploring the meanings of “theory” and “experiment” in the simulationists’ usage, it is clear that this ambivalence may be useful in everyday work, as we have seen in some of the examples in this article. Yet this ambivalence is not necessarily an asset in relation to, for example, publication, as the last part of the analysis suggests. What is the role of the simulationists’ work? Should it be framed to follow current standards of more traditional scientific knowledge or should it develop new ones? Further sociological investigation is necessary to analyze how the particular “rules of the game” in simulation-based science are about to develop from *within* – rather than as prescribed from *outside* by the philosophy of science, and how the scientific disciplines will negotiate these changes.

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