CHAPTER 6

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FROM HIERARCHICAL TO NETWORK-LIKE INTEGRATION: A REVOLUTION OF MODELING STYLE IN COMPUTER-SIMULATION

INTRODUCTION

In general, models in science are highly idealized and ignore most of the effects dominating reality. A prominent example is the theory of ideal fluids that neglects the effect of dissipation. Hence, strictly speaking, models in science are unrealistic. They represent an ideal world that is believed to lie behind the diversity of phenomena in the real world. To obtain more realistic models – in our example, the theory of real fluids – additional effects (e.g., the viscosity of the fluid) have to be integrated into the basic model. An adequate choice of effects depends upon the purpose for which a model is built. For example, if one tries to understand the phenomenon of hydrodynamic convection patterns, because of the relevance of dissipation basic principles of thermodynamics must be added to the mechanical equations of hydrodynamics. In this case, integrating new effects into the basic model of ideal hydrodynamics is not a problem, because this takes place under the uniform paradigm of physics. Hence, the integration of new effects is based on reliable theoretical grounds.

In most fields of practice, however, the process of applying science has to face serious problems: All kinds of effects must be taken into consideration in order to gain the relevant knowledge to tackle real-world problems. In this case, integration is no longer possible on the grounds of a common theoretical conception. Various scientific disciplines may become involved, contributing heterogeneous models; questions of instrumentation and technology may arise; and there may well be problems of political regulation, social acceptance, and economic success. These problem areas have to be integrated into an overall strategy of knowledge production without a common theoretical ground and even without the leading role of science. This new form of integration may be called pragmatic integration, because it has to be successful but by no means correct. Therefore, from the opposing perspective, this pragmatic integration can be seen as a fingerprint of application-dominated knowledge production in different problem areas in society.

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Pragmatic forms of integration challenge knowledge production in several fields. There are no general methods for making integration a success. Because of the lack of theoretical paradigms, it must be determined by technical or social constructions: as a kind of plumbing with respect to the methods and as a social networking with respect to its social practice.

In the last decades, computer simulations have become established as a powerful instrument in science and technology for the theoretical solution of complex problems, especially the dynamics of complex systems. Simulation models – traditional ones like differential (difference) equations as well as phenomenological ones like agent-based models – are used as a kind of generative mechanism to imitate the dynamic of a complex system. For this reason, computer simulations are more than classic models in science. They are complex algorithms that open up the possibility of running theoretical models as computer programs on a computer in order to show the internal dynamic behavior of the models.

Because, nowadays, computer simulations are used in different fields of practice, the problem of integrating different contexts of application is a current problem within simulation. What is a realistic simulation of a complex behavior? Is it, for instance, realistic because it uses the basic equations of physics, or is it realistic because all important effects are integrated beyond the theoretical paradigms of these effects? And, if so, what are the important effects? Which ones may be neglected for the sake of simplicity, and which ones not?

Many simulationists argue that the reality approach of simulations cannot be decided on the basis of the quality of the underlying models. On the contrary, it must be decided on the basis of the quality of the result. In other words, a realistic simulation is a simulation that is believed to be realistic. This transition to a new approach toward realism within computer simulation can be demonstrated in the case of climate research. Climate simulation models are, at present, the most complex ones, running only on the biggest computers in the world, involving also high levels of commitment to climate policy. Because of their political context, these simulation models must be realistic and reliable at the same time. This need has fueled a revolution in modeling style – a transition from a hierarchical to a network-like integration of models.

In the following pages, this revolution will be illustrated in two steps: We shall start with the development of hierarchical integration and the essential breakthroughs for the simulation method (1955 to mid-1990s) and then go on to analyze the shift toward a network-like architecture of simulations in climate research driven by the political demands for integration.

SOLVING UNSOLVABLE PROBLEMS

The Simultaneous Birth of the Electronic Computer and the Simulation Method

The development of both the electronic computer and the simulation method took place nearly simultaneously. The time and place of the latter's birth can be located at the end of World War II in Los Alamos, with a couple of applied mathematicians working together in the Manhattan project acting as virtual parents. One of the problems that turned out to be crucial for the development of the atomic bomb was the diffusion problem of neutrons. Although the physical principles had been known for a long time, the underlying equations proved impossible to solve. The electronic calculation machine developed at this time opened up new possibilities for the treatment of such complex problems. However, to use the new electronic machine, new methods of dealing with mathematical problems had to be developed: the Monte Carlo method, modeling via cellular automata, as well as finite difference approaches. All these methods can be seen as an attempt not to solve a system of complex mathematical equations but to imitate the dynamic encoded in the set of equations. These new approaches to complex problems constitute ways to use computer simulations as scientific instruments. The Polish mathematician Stanislaw Ulam played a key role in the invention of a couple of these methods – if one wants to mention a central figure in the invention, or construction, of simulation methods, he is the one.

The Monte Carlo method may serve as an example. This method goes back to the joint effort of Stanislaw Ulam and John von Neumann when working together on the Manhattan project in Los Alamos. Monte Carlo may count as the first simulation method.[1] Because the neutron diffusion problem was unsolvable by analytical methods and because a lot of experimental data were available on the scattering of individual neutrons by atoms, they were looking for a method by which they could obtain the behavior of a macroscopic neutron beam from these individual scattering events. Instead of calculating a solution of the basic equations, a statistical method was employed to imitate the behavior of diffusion. The following example will illustrate this approach.

Imagine that you intend to determine the volume of a certain body via Monte Carlo. You can embed the body into a cube with a known volume. The surface of the body defines an analytical function whose integration would give the so-called primitive. In many cases, this analytical approach is impossible, and the primitive cannot be calculated. The idea is to replace the (unknown) primitive by a ratio that can be determined 'empirically,' or quasi-empirically, by iterating computer runs. The computer determines a point within the cube at random. If this point belongs to the body, the trial is said to be successful. By re-iterating this random choice, one can determine the unknown volume as the ratio of successful trials out of a large number of trials. In other words, the surface function is not integrated numerically. Instead, this process is imitated by a generative mechanism.

During his work in the context of the Manhattan project, von Neumann tackled problems like the propagation of shock waves, another problem that could not be treated with analytical methods. This meant they could not be treated mathematically at all. The important point is that the relevant laws of hydrodynamics are very well known. They are expressed in a system of nonlinear partial differential equations (PDEs), whose solution determines essential properties of the behavior of the system under investigation. But to solve such a system of equations, one would have to find a set of analytical functions satisfying the set of differential equations that make up the integration in the technical mathematical sense. This is (in most cases) impossible for a set of complex nonlinear equations. Computer simulations changed the situation fundamentally – a new strategy for solving this problem had become available.

First, the set of PDEs is replaced by so-called finite difference equations (FDEs). Space and time are endowed with a grid structure reflecting the limited capabilities of a computer – it can only handle discrete objects. The FDEs are calculated at the grid points and evolve step by step over time. Just imagine a kind of approximation: If the grid becomes finer and finer, the FDEs will become identical with the PDEs – at the limit. The point, however, is that this holds only in principle. There is not only a limitation of computer time, which does not allow infinite small grids, but also a problem of truncation errors, because the calculation within the grid is done recursively. It starts from an initial value, and in each time step, the computer calculates a new set of values for the variables at the grid from the former ones. Therefore, truncation errors may evolve in time and the calculation may become unstable.

In general, this strategy of imitating the continuous dynamics of PDEs through a generative mechanism of FDEs makes it possible to treat complex systems, that is, systems in which a theoretical model of the dynamics is known, but the system is intractable for reasons of complexity. There is a wide range of applied problems that meet these requirements: The physical laws are well understood, but the interactions of different processes render the entire system 'complex,' that is, intractable. One cannot hope to achieve a solution of the PDEs with traditional mathematical means.

Nevertheless, both the epistemological and the methodological status of simulations are discussed controversially in the philosophy of science. The common view holds that simulations are more or less calculations that profit from the brute force of the computer. The computer is seen as 'number cruncher.' But there is also a heated discussion about the fundamentally new features that make simulation models and the important new class of models and simulations a new instrument of science.[2] As mentioned above, from the very beginning, simulations were seen as a quasi-empirical, experimental approach. "Broadly speaking, such methods will amount to construction of statistical models of given physical situations and statistical experiments designed to evaluate the behavior of the physical quantities involved" (Ulam 1952: 264).

Whereas Ulam praised this as an inspiring source for mathematics, von Neumann's response was less enthusiastic. He considered the 'experimental' approach to be a kind of trick, not completely appropriate to the mathematical problems of fluid dynamics he was struggling with. But he was pragmatic enough to see that the simulation method might open up a new access. And he suggested meteorology as an ideal case for the application of the FDE simulation strategy.

Skepticism in Meteorology

During the first half of the twentieth century, one rather speculative question in meteorology was which conditions and hypotheses would be sufficient to construct a model of the entire atmosphere that would be able to reproduce its behavior at least in a gross manner. Some achievements of the theory of the general circulation existed, but they pertained to very restricted parts such as to *lateral diffusion* (Rossby in the 1930s), or to the *jet stream* (Palmèn and Riehl in the 1940s). Which kinds of interactions were responsible for the global behavior observed remained simply unknown. The physics of hydrodynamics was well known and commonly accepted, but their nonlinear behavior was completely unknown at that time. Furthermore, it was believed that the simple nonlinear equations could not describe the complex behavior of fluids. The reasons for both irregularities and regularities were seen in the infinite influences coming from the outside world. In short, the hypothetico-deductive method was not applicable, because there was no mathematical instrument available that would allow an investigation of hypotheses or models. Thus it was commonly held "that a consistent theory of the general circulation is out of reach" (Lewis 1998: 42).

Directly after the war, von Neumann set up a working group on meteorology at the Institute for Advanced Studies in Princeton, headed by Jule Charney. The goal was to model the fluid dynamics of the atmosphere and to treat the resulting system of PDEs with the newly developed FDE simulation method. "To von Neumann, meteorology was par excellence the applied branch of mathematics and physics that stood the most to gain from high-speed computation" (Charney, cited acc. to Arakawa 2000: 5).

The design of the computer and that of the problems of meteorology would have to co-evolve, von Neumann suggested. Consequently, and already in 1946, he called a conference of meteorologists "to tell them about the general-purpose electronic computer he was building and to seek their advice and assistance in designing meteorological problems for its use" (Arakawa 2000: 5).

The approach of employing computer simulations on the basis of hydrodynamics – that is, with known theoretical basis but unknown dynamic properties – was to become the starting point for climate research as a modern discipline.

The phenomena of global circulation in the atmosphere show an enormous complexity – different processes interact in a highly nonlinear way. This is the reason why weather forecasts are impossible if one wants to make predictions that go beyond a critical period. Weather is, so to speak, a chaotic system. On the other hand, there are phenomena in the atmosphere's dynamics that are regular for long periods of time. To give an example, the so-called *surface westerlies*, continuously blowing winds north of the equator, have been well-known for centuries and were used when crossing the Atlantic Ocean in sailing ships. This difference – stable global patterns on the one side and unstable chaotic behavior on the other side – represents a major characteristic of complex systems.[3]

A Breakthrough: The 'First Experiment' by Phillips

A path-breaking success changed the skepticism concerning modeling the general circulation, and brought this project right into the center of a new scientific discipline. In 1955, Norman Phillips, working at Princeton's Institute for Advanced Studies, succeeded in his so-called *first experiment* in simulating the dynamics of the atmosphere, that is, in reproducing the patterns of wind and pressure in the entire atmosphere within a computer model (Phillips 1956).[4] The development of a simulation model of the general circulation of the atmosphere was celebrated as a major breakthrough. It surprised the experts, because it had been generally accepted that a theoretical modeling approach concentrating on the hydrodynamic equations would hardly be possible. Namely, it was believed that a model of a complex phenomenon has to be more complex than the system to be analyzed. This first attempt to build a simulation model of the entire atmosphere was considered an 'experiment.' This underlines how uncertain the success of this project was. At the same time, the conception of experiment expresses an important aspect for methodology: In simulations, scientists use their models like an experimental set-up.

The simulation model of the 'first experiment' worked with a very coarse spatial discretization of the atmosphere. In the vertical direction, it exhibited only two layers, and horizontally each grid cell covered more than 200,000 km^2. Phillips had to introduce the physical laws that govern the dynamics of the atmosphere. He used only six basic equations (PDEs), which, since then, have been called the 'primitive equations.' They are generally conceived of as the physical basis of climatology. These equations express well-known physics of hydrodynamics – the surprising thing was that only six PDEs were sufficient to reproduce the complex behavior, and Phillips had the skill and luck to make an adequate choice. This physical basis had to be adapted to the grid. The construction of a discrete model is a typical task of simulation modeling. The global and continuous equations of hydrodynamics had to be reformulated in order to calculate the evolution of the relevant variables in time – pressure, temperature, wind speed – step by step at the grid nodes.

In the first stage of the experiment, the initial state was an atmosphere at rest, with no differences in temperature or pressure, and no flow. In the second stage of the experiment, the dynamics was started, that is, the radiation of the sun and the rotation of the earth were added. The atmosphere settled down in a so-called *steady state* that corresponded to stable flow patterns. The tantalizing question was whether the model would be able to reproduce the global flow patterns of the real atmosphere, for instance, the surface westerlies. The result was positive – everyone was impressed by the degree of correspondence. As mentioned above, the experts were skeptical about the possibility of a global (and not far too complicated) model, but the empirical success was convincing. The decisive criterion for success was the adequate imitation of the phenomena, that is, the flow patterns. Because there was no knowledge about the outcome, Phillips' attempt to use a specific set of equations can be understood as an experiment – an experiment on modeling the equations of motion within a computer.

The continuous primitive equations of the atmosphere were by no means solved (that is, integrated in the strict technical sense) by Phillips' simulation experiment. Instead, the phenomena of the atmosphere were imitated by the generative mechanism of the discrete difference equations. The success of the imitation was judged solely by the correspondence between simulated and observed flow patterns. Hence, the validation of simulation results relies on a quasi-empirical strategy.

The success of the simulation experiment was acknowledged immediately and was judged to constitute a theoretical breakthrough. In the same year, E. Eady, the leading theoretical meteorologist in England, formulated far-sightedly: "Numerical integrations of the kind Dr. Phillips has carried out give us a unique opportunity to study large-scale meteorology as an experimental science" (Eady 1956: 536).[5]

And indeed, this experimental approach via simulations played a major role in shaping the emerging discipline of climate research. A. Arakawa (2000) calls this the "epoch-making first phase" of climate simulation modeling.[6] Experimental access to the climate system is a key for climate science. Another example is a researcher who succeeded recently in showing that the Pacific Ocean can exert a considerable influence on the Gulf Stream in the Atlantic Ocean. He discovered this connection by numerical experiments and described his approach in an interview as follows:

> *Q*: "You feed the model and then you wait and see what happens"?
>
> *A*: "Yes, exactly. That is the case – without simulation I would never been able to obtain this result" (transcript from an interview[7]).

This is not the place to discuss further developments in climate simulation (see, for more details, Küppers and Lenhard 2005). Simulations, of course, spread rapidly to very diverse fields. This development can be summarized by stating that science had acquired a new instrument – the simulation method provided a means of studying complex systems.[8]

HIERARCHICAL INTEGRATION AND THE POLITICAL CONTEXT OF APPLICATION

The Centralized Model of Atmosphere: The Unidirectional Forces of Science and Politics

The 'epoch-making first phase' (Arakawa) assigned a key role to the general circulation models (GCMs). In the 1960s, the next stage began, the 'magnificent second phase' in which climate science evolved as a normal scientific research program, centered around the GCMs and concentrated mainly in a couple of research centers in the United States.[9] Already in 1960 the *Geophysical Fluid Dynamics Laboratory* (*GFDL*), which belongs to a section of the US Department of Commerce, was founded in Princeton to follow up on this approach. This was the first institution with the official task of simulating in climate research. Other typical institutions are the *National Center for Atmospheric Research* at Boulder, Colorado, also founded in 1960, or NASA's *Goddard Institute*. The scientific agenda consisted in refining the GCMs, implementing lattices with higher resolutions, and integrating more subprocesses connected to atmospheric dynamics. In short, the GCMs have been growing more or less continuously for about thirty years.

The GCMs form a class of huge simulation models that run on high-speed supercomputers. This requires a considerable effort in funding, although climate research, having started as a part of meteorology, used to enjoy only limited visibility as a scientific discipline. About twenty years ago, circumstances changed almost completely. The climate system became a subject of hot political debate. The so-called greenhouse effect was discovered and was discussed controversially right from the start. Perspectives on the climate system switched radically. Once seen as a stable system, its potential instabilities and changes now became the topic of discussion and investigation.

The field of climate research became one of the most prominent scientific fields in the media. At the same time, funding rose enormously. Climate research was expected to answer – and was in part defined by that demand – the following questions of utmost public, scientific, and political interest:

- Is there actually a change in the climatic system, or are we observing only random fluctuations; that is, can we *detect* climatic change?
- And if there is a change, are we humans a cause of it; is it an anthopogenic change? That is, can we *attribute* the change to a cause?

Some countries decided that they needed research institutes to tackle these questions. Germany, for instance, founded the *Max Planck Institute for Meteorology*. This institute was built around a GCM (in part imported from the United States, then rapidly developed further) and followed more or less the example of the US institutions mentioned above.[10] GCMs occupied a central role in the scientific enterprise, and this role was assured and fostered by political demands. The goal was to predict the climate system's future state. To be applicable in a political context, such predictions need a high degree of reliability and certainty. The high status of the physical laws that constituted the nucleus of GCMs met this political requirement perfectly.

Besides the scientific efforts, there were remarkable, perhaps unprecedented, global institutional efforts. A joint venture of science and policy was undertaken. The UN and the World Meteorological Organization founded the *Intergovernmental Panel on Climatic Change*, IPCC, a global institution with the official task of delivering an assessment of detection and attribution. Every four to five years, the IPCC publishes an *Assessment Report*, a voluminous compilation of the current state of scientific knowledge. A great number of climate researchers worldwide are involved in this IPCC process. The central tools for analysis and prediction are the GCMs building the backbone of the IPCC's assessment reports. The statements derived from these models serve as a basis for political negotiations and decisions such as the Kyoto protocol.[11]

There is a strong demand for integration for political reasons as well: As is well known, climate change as a political issue instantly attracted opposing parties. Leaving aside considerations about political aims, it is obvious that the *reliability* of knowledge about the climate system became a prominent problem. And that amounts to questions on the validity of the simulation models: Are they really realistic? Are there important subprocesses that have not yet been taken into account? Could these influence predictions of the future development of the climate system?

The policymakers' demand for reliable data on the development of the climate system fostered the efforts to integrate all kinds of effects that were believed to influence the dynamics of the atmosphere. This integration was driven by attempts to make the model more realistic. This was important for climate research as well as climate policy.

Figure 1 should visualize the situation as it is commonly viewed by the community: "basically, it is all physics" (interview), and consequently, the primitive equations of the atmospheric GCM constitute the nucleus – governed by the equations of fluid dynamics. This situation is also reflected in the 'architecture' of the research institutions of climate science. Mostly, they are rooted in physics; for example, the GFDL even bears fluid dynamics in its name. Until recently, the directors were physicists as well.

More and more subprocesses have become attached to the core, that is, are being integrated into the simulation model. Ideally, no essential parts or processes should be

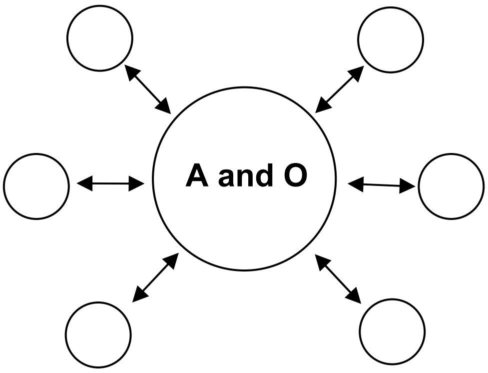

Figure 1. The physics-based atmospheric dynamics at the center. An increasing number of subprocesses are additionally becoming integrated

left out. Integrating aerosols into the GCMs might be seen as a typical success. These gave the models a 'cooling-by-pollution' effect that improved the match with observed temperature patterns.

Far along this line of 'densifying integration,' lies the great achievement of climate modeling in the 1990s: the coupling of atmospheric GCMs and those of oceans (see Figure 2). Both simulation models are centered around hydrodynamic codes – atmosphere and oceans are fluids in physical perspective. This coupling induced no fundamental change in architecture, because physics maintained its position as the theoretical nucleus. The coupled GCMs (CGCMs) once again produce a centralized architecture, now with two centers, resulting in a kind of twin-star image:

A great technical effort was required to couple the two most voluminous simulation models. CGCMs also provided an enriched basis for statistical analyses. The results of CGCM simulations led to a majority opinion that a change of climate can be diagnosed. Moreover, it was a celebrated claim that now, with CGCMs, it became possible to distinguish the so-called 'fingerprint' of human impact.

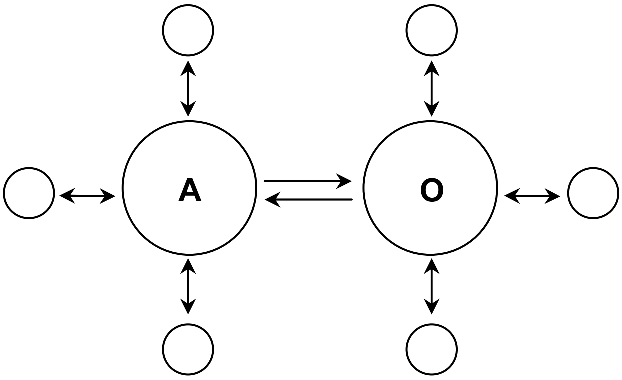

Figure 2. Architecture of coupled atmosphere-ocean generated circulation models (CGCM)

An Episode of Science and Policy: The FA Controversy

While the coupled GCMs were celebrated as a milestone on the road toward a realistic model of the climate system, they gave rise to a heated controversy. The claim to be able to accomplish a more and more comprehensive simulation of the climate system, a simulation drawing on an objective basis of laws of physics, was surely one of the central claims of climate research. For the first time, it became possible to couple atmosphere and oceans. Each system takes the role of a boundary condition for the other. Both systems were calibrated separately to show a steady state similar to the observed phenomena. And now, as the systems became coupled one to the other, the researchers introduced a mechanism to enable interchange while simultaneously guaranteeing that the coupled system would not drift into a new, unrealistic state. In short, this so-called Flux Adjustment (FA) was an 'artificial' mechanism intended to keep the GCMs from leaving their precalibrated (realistic) region.

In some sense, the worst case occurred: In *Science*, one of the most widely read and influential journals, the coupled models were denounced as relying on a "fudge factor" (Kerr 1994). Critics asserted that this flux adjustment was an 'artificial' mechanism without any 'real' counterpart and had been introduced merely to produce the desired results. The coupled models were expected to provide a new and superior integrated basis for predictions, but the criticism of FA challenged this claim. If the results of climate research were not based on 'realistic' models and did not rely on objective laws of physics, would that not question the entire scientific-political enterprise?

The media echo was controversial. The spectrum ranged from 'a blatant scandal' to 'only a storm in a teacup.' Even scientific experts saw things rather differently. We conducted several interviews that also raised this issue. The statements of the scientists ranged from an uncomfortable feeling, because FA was of an artificial nature, across the claim that FA was only a preliminary technique and that the models will be *really* realistic in five years, to the opinion that FA was fully legitimate and comparable to techniques common in simulation modeling.[12]

The heated discussion was accompanied by a critical assessment of the reach of models in general, noteworthy also in *Science* (see Oreskes et al. 1994; see, for a reply defending the modeling approach, Norton and Suppe 2001). None of the sides in this controversy will be taken here. The point is that the incriminated strategy of 'artificial' tools like FA is widely used in simulation modeling and, what is more, belongs to the methodological core of that approach.[13]

Consider, for instance, parameterization in which a complicated mechanism like cloud dynamics is replaced by one or a few parameters that are easier to handle. One could easily extend the criticism against FA to cover parameterization techniques as well – techniques nearly ubiquitous in complex simulation models. Second, the FA affair brings to the fore the hybrid nature of climate research: It is a scientific and political project carried out under the scrutiny of public media. There is a certain tension in the political application of simulation results. Whereas there is no way of treating climatic changes without simulation models, the methodology of simulations seems to cause some tensions with demands for 'realistic' models.

The hybrid scientific-political nature makes it difficult to separate political and scientific motives. The development of 'stars' and ongoing integration up to the twin-star architecture of coupled GCMs can be interpreted in two ways: according to political and according to scientific motives. Hence, the forces of science and policy point in the same direction in this case and result in an ongoing integration. The next section will argue that a fundamental restructuring of the model architecture is presently taking place.

CHANGING THE PARADIGM OF INTEGRATION

From Stars to Networks

The effort to achieve ever greater integration strengthened the star architecture. The emergence of twin stars, that is, coupled atmosphere-ocean models is commonly conceived as a first-rank scientific achievement in the field. Arakawa argues that the 'third phase' of simulation models, that is, integrated modeling, started with this successful coupling.

Up to this point, it is hard to distinguish whether evolution is driven by inner-scientific momentum or induced by political demand. However, the scientific research program of refining and integrating GCMs based on the physics of fluid dynamics has now come to the end of its rope. The paradigm of the centralized model reaches its limiting factors when the processes that are to be integrated have no relation to the theoretical framework. Some leading research institutions are already responding to this by switching to a new architecture of climate simulation models. This new architecture does not deal with integration as an adaptation of additional parts to the dynamic center of GCMs. In fact, one can observe a profound shift in the modeling architecture of simulations in climate science. Roughly speaking, the new approach is to develop models of different, theoretically incompatible fields independently and then to couple them to one another on a merely technical basis of simulation. In this way, it aims at an integration of a variety of models from physics, biology, chemistry, and even economics.[14] Their dynamics can hardly be connected to physics, and therefore the whole architectonic paradigm of a centralized structure seems to be ill-suited for the task of enforced integration. The new architectonic paradigm can be described as a network or grid (see Figure 3).

The most important feature of the new net architecture is that there is no longer one theoretical nucleus. The new nucleus is built by a (virtually theory-free) simulation coupler that is linking the various models. Coupling takes place in a simulation-technical sense (see Winsberg, this volume, who nicely captures the coupling of heterogeneous models as a 'handshake' between them). Each of them has its own theoretical nucleus, thus the net shows symmetry between the models.

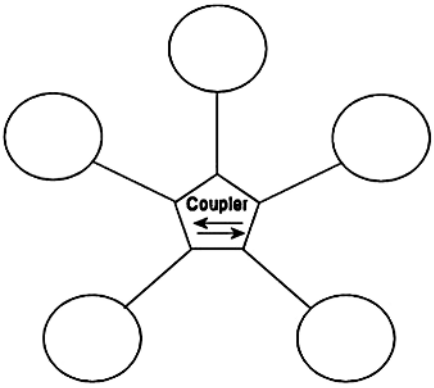

Figure 3. Simulation coupler in the 'void center' integrating various models

The *National Center for Atmospheric Research* (NCAR) has been among the first to implement the new architecture, their program being to realize 'NCAR as an integrator.'[15] The central part of that plan is formed by the so-called *Community Climate System Model* (CCSM) that integrates different simulation models via a hub.

In this organizational structure (Figure 4), a coupler unit controls the exchange of parameter values between independent and exchangeable models. This modeling approach is in clear contrast to earlier attempts at integrating submodels around the center of a physically based GCM.

Thus, the task is no longer to build one all-encompassing model – ideally *the* right model. Instead, researchers construct a model by coupling together different modules that were developed on their own. The coupled network normally presents a

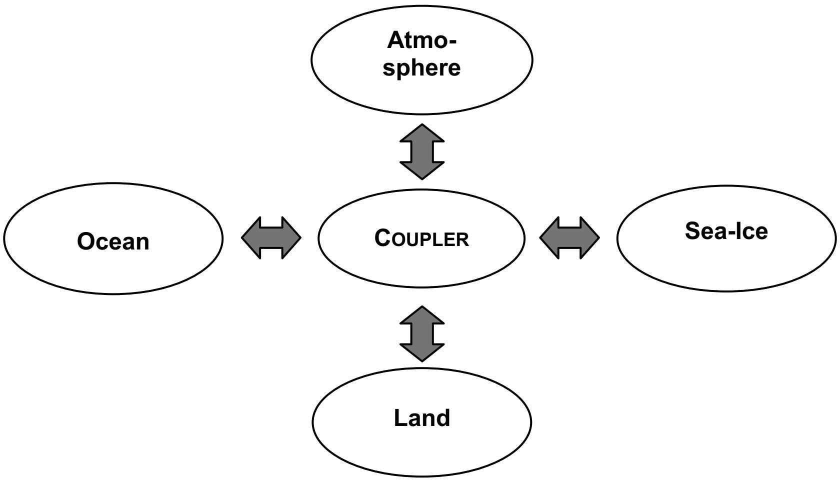

Figure 4. Architecture of NCAR climate simulation modeling

mixture of fully fledged and very basic modules. By replacing some of them, the network can be adjusted to different research questions. The NCAR emphasizes the flexibility of the new net architecture. Figure 4 is explained with the words:

> […] based on a framework that divides the complete climate system into component models connected by a coupler. Individual components – ocean, atmosphere, land, and sea-ice – can be exchanged for alternate models, thus allowing different configurations appropriate for different applications (CCSM 2004).

The shift from a centralized 'star' to a net architecture is being rated very highly in methodological terms. In interviews, it has been called a 'revolution in modeling style':

> I think actually it's symptomatic worldwide, […] we had a modeling framework that we have been using for quite some time, but in the last four, five years we are really pretty much throwing it out the window. We have redone our entire modeling framework from scratch, […] building a proper framework to allow interaction between different physical and biological components, also taking advantage of advances in computer technology to allow the system to be more flexible (transcript from interview).

This amounts to saying goodbye to the fundamental leading role of physics in climate research. In climate change analysis, the physics of fluid dynamics takes at best a position as *primus inter pares*. Under the perspective of ongoing integration, the whole climate system, including all biological, environmental, economic, and other components, is regarded as *one* system. And for this reason, the model architecture can constitute a paradigm for simulations. The analyzed change from hierarchical to network-like integration, which also took place in a social and disciplinary sense, presents a profound paradigm shift – a revolution in modeling style.

CONCLUSION

The foregoing argumentation is oriented toward the case of climate research and the simulation models it employs. It is intended as a contribution to the nuances of the development and the history of the simulation method. Can one draw conclusions beyond that? Are the observations made here also valid in a more general sense? And if so, in what respect? We shall raise three points that conclude our account while also posing new questions:

1. First, the result of the revolution in modeling style can be called pragmatic integration without theoretical background. This allows the integration of theoretically incompatible models. Even models that are distributed, that is, located in different computers can be integrated by this approach. Recently, we can also observe a movement toward so-called 'distributed computing' in somewhat different, although closely related, respects. One important development in this direction is *computational grids*, that is, clusters of computers that are connected but do not execute a central 'program' but contribute their pieces independently. Thus, a huge amount of computational force – but also tons of data that are only available at widely distributed places – can be gathered, and science is making serious efforts to use this kind of resource to manufacture an instrument for the investigation of complex problems that are currently out of reach. Very diverse

projects are conducted 'on the grid' such as climate forecast (connecting more than 25,000 PCs) or pharmaceutical drug development. Those efforts can be summarized under the title 'e-science.' For instance, in 2004, Germany started a research initiative called d-grid to investigate under what conditions a grid architecture can be used effectively. Again, the conditions are of diverse nature: computational, legal, institutional, and many more. As may be obvious, simulation on the grid promises to be one of the major benefits of computational grids. The impact of the new grid architecture has yet to be determined.

2. Second, the simulation architecture indicates a strong application-oriented influence. This results from the need for unifying integration. However, this integration is not achieved in the sense of a unifying theory, but in a pragmatic sense of tinkering the different autonomous models together. "Modeling is a kind of engineering work. We have the components, but they do not fit. And then, we are knocking, or tinkering, them together such that it works" (transcript from interview, see endnote 7).

   This kind of 'tinkering' may be recognized as a quite general feature of science that is under strong pressure from applications, or even dominated by them. Usually, applied problems do not occur at the rare spots that are neatly covered by scientific theories. The lack of a general and common theoretical framework has to be compensated. Whereas hydrodynamic systems are theoretically well understood, when confronted with the real world, they pose complex problems that curtail the range of the theory very strongly. And, moreover, the questions often transcend the theoretical framework – as was observed in the case of the climate system in which science is removing the theoretical nucleus to one of the nodes of a network. This network is connected computationally by a simulation coupler, but theoretically unconnected! There seem to be a plethora of examples in which applied sciences are driven to pragmatic integrations when confronted with the lack of a "rock-bottom of theory" (see Carrier 2004). In the physics of nuclear fusion, for example, the laws have been well known for decades, but the construction of a concrete fusion reactor poses problems that cannot be solved by that theory. Another example, from economics, is so-called innovation networks that should integrate different parts of knowledge on a purely pragmatic level to enable the development of a new product (see, e.g., Pyka and Küppers 2002). Theoretical integration is not the goal. On the contrary, these networks aim at an effective exchange of bits of knowledge, *although* a common theoretical framework does not exist. In sum, tinkering can be considered to be a 'fingerprint' of science dominated by applications.

3. Third, we have argued that a fundamental change, or even a revolution, in modeling style occurred as a change in the *architecture* of simulation models. That architecture may function like a paradigm is due to the fact that simulations are by no means purely theoretical entities. Simulations are different from calculations or algorithms – they work with concrete implementations. Effective implementations limit the possible range of simulations in a pragmatic sense – they have to run on certain machines in reasonable time. Hence, simulations are technologies that have to be investigated on the basis of their application in scientific

> practice (see Humphreys 2004 and the introduction to this book).
>
> This aspect characterizes simulations as scientific instruments. Not only do simulations call for appropriate new mathematics to deal with computational issues, but the scientific *instrument* of simulation and *applied problems* (think of climate prediction) are also intimately related and interact. In the case of the climate system investigated here, the 'pressure' for integration originated from the applied context; and, at the same time, the task of integration was embodied in the model's architecture. In sum, simulations are mathematical instruments with a material basis.

Let us conclude with a last consideration: Until now, the guiding line in our argumentation has been computer simulation and especially computer simulation in climate research. However, points of a more general relevance beyond computer simulation are involved, namely, the role of theory and of scientific disciplines and networks. In all cases in which complexity sets limits to analytical solutions, scientific theory is becoming less important and partly replaced by practical ad hoc strategies in knowledge production. Whereas the empirical basis is simply too weak to back such a general claim, something is definitely going on in the relation between science, theory, and applications. One reason for this dynamics is the increasing complexity of science and technology. For example, the idealizations that could still be made in linear regimes are no longer possible in the nonlinear regimes that many questions and problems demand. But this is only one side of the coin. Although theories may not be predictable and even calculable in the strict sense, they play an important role in finding strategies for practical solutions to a broad variety of problems.

The same holds for the organization of knowledge production. The example of computer simulation, especially in the case of climate models, shows a transition from disciplinary organization of knowledge production to a transdisciplinary form of organization. There is no argument that this transition is caused by the simulation as such – it may be due to complexity. The integration of all kinds of competencies, abilities, and knowledge bases in different fields of modern industrial research and development is a very common observation and shows the same network architecture. However, this does not imply the demise of disciplines.

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NOTES

[1] Richtmyer and von Neumann 1947, based on contributions by Ulam, and Metropolis and Ulam 1949 count as founding documents of the Monte Carlo method. See also the compilation of Ulam's papers 1990 and the accounts of Galison 1996, 1997, and Fox Keller 2003.

[2] See, for example, Humphreys 1991; Rohrlich 1991; and Fox Keller 2003 who stress the important role of a new kind of experiments. For a more detailed epistemological account of simulation as 'imitation of complex dynamics by a suitable generative mechanism' adhering to the second view and discussing the common view critically, see Küppers and Lenhard 2003 and 2004.

[3] This is what in other contexts is called self-organization; see, for more details, Küppers 2002.

[4] For more details of the experiment, see Lewis 1998; for a broader history of ideas on modeling the general circulation of the atmosphere, see Lorenz 1967.

[5] The use of the word "integration" is an indicator of the strong belief in the calculation paradigm.

[6] For a history of climate research using simulation models, see Edwards 2000.

[7] The interviews were performed within a research project conducted by G. Küppers, J. Lenhard, and H. Lücking. The project (2001 until 2004) addressed the epistemic characterization of simulations, and included interviews with researchers at a couple of climate science centers in Germany and the United States. It was part of the research group *Science in Transition* at the IWT, Bielefeld, funded by the Volkswagen Foundation.

[8] The experimental approach to complex systems of PDEs is only one particular instance. The simulation method has shed its skin several times, see Fox Keller 2003, or Schweber and Waechter 2000.

[9] The hegemonial role of GCMs in climate research is commonly acknowledged. It is discussed critically in Shackley 1998 et al.; see, also, the dispute between Demeritt (2001a, 2001b) and Schneider (2001).

[10] For an 'evolutionary tree' of GCMs, see Edwards 2000.

[11] The IPCC process and its character as a hybrid science-policy enterprise have been analyzed extensively in the literature. It is not possible to give an overview here. The anthology of Miller and Edwards (2001) gives an impression of the science studies approach to climate science and is highly recommended.

[12] A comparison of the impact of this discussion on different modeling centers is given in Krück and Borchers 1999.

[13] In modeling terms, the FA is equivalent to 'Arakawa's trick.' For an epistemological investigation and a characterization of simulations as imitations of complex dynamics using artificial mechanisms, see Winsberg 2003; Küppers and Lenhard 2006. See Petersen 2000 for an emphasis on a simulation-oriented philosophy of climate research.

[14] For an account of the intimate relation between the simulation method and (inter-)disciplinary structure, see Lenhard et al. 2006.

[15] Documented in the web: http://www.ccsm.ucar.edu/models/

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