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Models and Simulations in Climate Change

Historical, epistemological, anthropological and political aspects

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1. Introduction

Since the 1930s, historians and philosophers of science have generally either approached models starting from scientific theories, or taken them to be intermediary entities between theories and real objects. When it emerged in the 1930s, the notion of 'model' took on two well-defined meanings: the first acceptance is described as 'logical' (even though it first appeared in the field of mathematics); the second emerged in the context of the empirical sciences' attempt to conceptualise the relationship between physical, economic and social systems on the one hand and their formal representations on the other. During the 1930s also, models and their role in scientific knowledge became the object of philosophical scrutiny, within the Vienna Circle's reflection on the verification of scientific theories (logical positivism) and their empirical falsification (Karl Popper). In all cases, theory thus provided the starting point for thinking about models, which were primarily understood as intermediary entities, transitional objects between theory and a given empirical object. Philosophers then concerned themselves primarily with the relationship between models and theories, the 'realism' or 'representativeness' of models, and the purity of the methods — analogies and metaphors in particular — used for constructing them.

A look at recent scientific practice, however, especially that generated in the past few decades by computers and numerical simulations, reveals this framework to be largely inadequate for analysing contemporary models. Historical studies have further shown that even as far back as the 1950s many models did not fit this conception.[2] Many examples of models can be found which were developed for very pragmatic purposes, with theory playing almost no part in the process. The Second World War and the Cold War spawned a range of multidisciplinary activities which benefited in the United States from the boom in applied mathematics: in the fields of operational research, systems analysis, and communications engineering (electrical, phone and servo-mechanical communications), not to speak of the first simulations in military nuclear research, or meteorology. It seems as if post-war modelling to a large extent rejected mechanical reductionism, hope in an unified science and the search for universal truth. John von Neumann claimed at the time that the sciences aimed not to explain but merely to build models whose only legitimacy lay in their efficacy.[3] Modelling practices, always pulled between abstraction and application, now found

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[2] See Armatte & Dahan [2004], forthcoming
[3] "Methods in the Physical Sciences", *Works*, VI, p 491.

themselves subjected to another set of contradictory forces: should they be first and foremost predictive and operational, or rather cognitive and explanatory? The tension between understanding and forecasting, between cognitive and explanatory models became a growing source of conflict and compromise-seeking.

From the 1980s, as I will show here in the paradigmatic case of contemporary climatology (but this is a general feature), models can no longer be analysed in terms of the epistemology described above. Devised in a foundational and normative spirit, this framework proves far too restrictive to account for the construction and working of models, the diversity of modelling practices and their significance. Computers have profoundly transformed not only modelling and simulation practices, but also the way in which we conceive of models. Together with this methodological turn came a shift in the objects, phenomena and systems under consideration, itself made possible by the computer's ability to handle increasingly complex systems, to concentrate multiple interactions and feedback, and to integrate multiple scales and temporalities.

In order to understand how scientific practice and knowledge effects relate to each other, the notion of model needs to be historicized through a study of its workings and functions in different historical configurations of scientific research. It also needs to be subjected to sociological analysis: modelling activities should be replaced in their institutional, technical and political environments, without separating the cognitive from the social elements which combine within each model. We need to pay more attention to the actors —the researchers, engineers and users of models— and to actions themselves.

This epistemological and methodological interest in models and simulations has led me, via the case of meteorology,[4] to the issue of global climate change in the Earth System. This field's models and modelling practices illustrate perfectly the need for a new theoretical and reflexive framework.

2. Meteorology in the 1950s

Meteorology provides a good point of entry into climatology. Let us examine the methodology involved in the elaboration of the first numerical prediction models, developed between 1947 and 1953 within the framework of the Meteorological Project. This project, closely tied to the construction of a computer, was launched at Princeton by John von Neumann, with Jules Charney as its scientific director.[5]

The methodology of ' theory pull'

The first task Charney and his colleagues faced was to filter atmospheric phenomena of meteorological interest from other, unwanted phenomena (gravitation, sound wave propagation, etc.). This process of filtering what Charney called *atmospheric noise* had been implicitly carried out for decades by meteorologists formulating theories, but it was brought to the foreground by the introduction of the computer. Charney claimed that output noise could be eliminated in either of two ways: by making sure that the input was perfectly clean, or by building a filtering system into the receiver. In 1947, Charney coined his popular metaphor: 'the atmosphere is a transmitter, the computer is the receiver.' The computer at

[4] See Dahan [2001].
[5] On this project, see Aspray [1990]

once appears as a non-neutral object that not only recorded data but also selected it and thereby played an active role.

Later, Charney and his colleagues adopted an unified approach. They investigated a series of pilot-problems involving increasing numbers of physical, numerical and observational factors, while simultaneously implementing algorithms which integrated the models' equations. Thus, starting from a simplified model whose behaviour could be computed, they compared the model's output with the observed phenomena, and then accordingly modified the model —usually by adding a physical factor whose initial exclusion was thought to have caused the discrepancy between the model's output and the phenomena observed. In 1950, Charney, Fjötoft and von Neumann[6] published an analysis of their equations and a summary of the numerical 'predictions' (or rather post-dictions, made available after the phenomenon had already taken place) obtained over a period of 24 hours, for a few carefully selected days. The model proved satisfactory for periods when the atmosphere behaved barotropically,[7] but it was still unable to predict the formation and behaviour of typhoons. The Princeton group set out to improve it, by successively investigating baroclinic models of one to seven atmospheric layers.

Subsequently, in 1952, Charney wrote that: 'The philosophy guiding the approach to this problem [of numerical prediction] has been to construct a hierarchy of atmospheric models of increasing complexity, the features of each successive model being determined by an analysis of the shortcomings of the previous model.'[8] In the first stage of the computer era, the main trend in meteorology was arguably to 'complexify' the simplistic 'physical models' of atmospheric behaviour. As a giant calculator, the computer made possible the treatment of increasingly complex and evolved equations, to produce better descriptions of the atmosphere and therefore give hope for more precise predictions.

Charney named this approach 'theory pull': generally it is the case that a theory (in practice, a combination of several theories) describing the dynamics of atmospheric phenomena 'pulls' the development of models. Several factors, however, affect the apparent 'purity' of this methodology. First, the 'race against the clock' character of numerical weather forecasting constitutes a major constraint, since the procedures of collecting observation data, feeding it into the model, processing it by the computer, and producing numerical outputs have to be carried out as quickly as possible. The improvement of forecasting has to be counterbalanced with the increased time and work required for numerically processing the model and is therefore a matter of constant negotiation.

It is not the case that this hierarchy of increasingly complex physical models followed a rational logic, whereby simplified numerical prediction equations were derived from comprehensive equations expressing the laws of atmospheric movements. In fact, the algorithms for integrating models depended on computer capacity, which itself evolved constantly. Not only did computer availability vary (since Princeton's machine was still under construction, Aberdeen's ENIAC had to be used), but also memory size, the speed of computational methods, etc. To complicate matters, the algorithms' numeric stability (linked

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[6] Charney, J., Fjörtoft, R. and von Neumann, J. [1950] "Numerical integration of the barotropic vorticity equation," , *Tellus*, 2, 237-254.

[7] A model is called barotropic when pressure at one point depends solely on its location on the earth's surface and not on its altitude; it is called baroclinic when a vertical pressure component is added in the model, that takes into account air circulation and the loss of potential energy.

[8] Meteorology Project Report, quoted in Aspray [1990], p. 302

to the sizes of the time step and the spatial grid respectively) proved particularly problematic and had to be dealt with following a number of strict mathematical conditions[9] which belie any 'logical' succession of increasingly complex models.

W. Aspray has written that in the post-war, computers almost transformed meteorology into a mathematical science.[10] What can be observed, in my opinion, is rather that the technical and numerical possibilities created by computers considerably disrupted the current hierarchy of physical models, turning meteorology not so much into a mathematical science whose methodology unfolded logically and rigorously but into an engineering science, with its typical series of successive adjustments.

During this whole period, computers not only played the role of giant calculators able to process an increasing amount of data and bring much-needed complexity to atmospheric models, but they also acted as true 'inductive machines', to use Charney's 1955 expression, capable of selecting physical hypotheses and of testing their effects.[11]

Models for understanding or for predicting?

Towards the end of the 1950s, when improvements in the quality of meteorological forecasting seemed to have ground to a halt, the models themselves came under debate. Scientists sought either to extend the principles of filtration or to reverse to earlier hydrodynamics equations, taking advantage of the availability of more powerful computers. Both trends concerned physical models, which scientists sought to make more representative of the complexity of the phenomena under consideration. Yet other researchers, including Barry Saltzman and Edward Lorenz, began investigating new avenues based on the computer's use as an inductive machine (i.e. seeking to test hypotheses separately). They examined different kinds of models. These drastically simplified 'laboratory models' were intentionally designed not to *predict* but to *understand* particular aspects of atmospheric behaviour. The use of these models and the question of what could be learned from them caused great controversy and discussion within the community of numerical meteorologists, in particular at a symposium held in Tokyo in 1960.

In the context of this debate, Lorenz, a meteorologist at MIT, first used a simplified twelve-equation model which he shortly afterwards further reduced to a three-dimensional model. The behaviour of the model, which he exhibited in 1963, was chaotic. Did these simplified models display, to use Charney's expression, 'fatal defects? To what extent did they represent a reality on which they only very roughly based? More generally, should the difficulty of long-range forecasting be attributed to computers, numerical methods, to models, or to the atmosphere itself? These crucial questions remained an important preoccupation for Charney and his colleagues, reluctant to renounce the possibility of forecasting altogether. The chaotic turbulence of the atmosphere and the notorious 'butterfly effect' (a sensitivity to initial conditions that shattered the old dream of long-range forecasting) were only gradually accepted. This was the outcome of a complex scientific, disciplinary, but also professional and cultural process that took place in the 1970s, involving not only meteorologists, but also dynamic systems mathematicians, theoretical physicists and many others.[12]

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[9] The so-called Courant, Friedrichs and Lewy conditions. See Goldstine [1977].

[10] Aspray [1990], 152.

[11] Charney, Conference at the Academy of Sciences, 1955, in Aspray [1990], 153.

[12] For a precise diachronic analysis of this story, see Dahan & Aubin [2002].

Before moving on to climatology models, it is important to note that another crucial topic emerged at the Tokyo symposium. In its closing session, Eliassen mentioned as a particularly interesting field of research the possibility of computing the climatology of an atmosphere, thereby opening a general programme which was undertaken by climatologists in the 1980s. Strikingly, he outlined this programme in very abstract mathematical terms: 'given a planet with specific properties, distribution of oceans and continents, elevations, insolations and so on; determine the distribution of climate'. Eliassen suggested not only new methods of mathematical modelling, but also a new philosophy of modelling, expressing tremendous ambition for numeric modelling and an ideal of what science could supply. He further added:

> This should in principle be possible, from weather forecasting techniques by making forecasting for a long period of time and making statistics. This may become of importance for determining the climate of previous geological periods where the surface features of the earth were different from what they are now, and it may also be of importance for determining changes in climate caused by various external and internal changes of the system, changes within the atmosphere or of the earth's surface or of solar radiation. Since mankind is all the time changing the properties of our planet, there are, of course, already artificially produced changes of climate, and one is even thinking of producing such changes deliberately. It is vitally important that we shall be able to predict the effects before we try change the properties of the planet.[13]

Deliberately modifying the climate or the atmosphere has therefore not always been looked upon negatively. In fact, a great deal of research into the modification and control of weather was conducted in the United States from the 1940s to the 1970s: provoking rain, deflecting a hurricane, or even controlling the climate through the selective addition of carbon dioxide were then perfectly legitimate research topics! These were gradually abandoned between 1971 and 1973 due to American society's changing attitudes towards nature, technology and risk in this period.[14]

3. Climate change studies

Before delving into the analysis of climate models connected to the greenhouse effect and contemporary scientific practice, I wish to underline three specific characteristics of this field. These are, to an extent, interdependent and reveal together a set of scientific, political and professional constraints inherent to the development of models.

1) 'Putting the global climate system in equations' is an ambitious undertaking, a new type of big science, which has developed over the past twenty years in parallel with the growing power of computers. It has as its central objective the measurement of the climate's sensitivity to anthropic effects, in particular forcing by the greenhouse effect. Without this issue, the field would not exist, at least not as it stands. Here is yet another instance where the goal of scientific research is not knowledge in itself but rather action and application. It is important to understand that today's climatology faces the particularly difficult methodological and epistemological problem not so much of accounting for the earth's climate, but of identifying the climate's *sensitivity* to anthropic effects, in order to establish whether the greenhouse effect is intensifying. Against the backdrop of enormous natural climatic variability, the lack

[13] Proceedings International Symposium on Numerical Weather Prediction in Tokyo, November 1960, pub. 1962. p. 646.

[14] See C. L. Kwa "The Rise and Fall of Weather Modification: Changes in American Attitudes Towards Technology, Nature and Society", in *Changing the Atmosphere*, C. A. Miller & P. N. Edwards (eds.), 2001, MIT Press, p 135-166.

of a ‘reference trajectory’ (what would be the climate without Man’s doings?) makes the validation of models particularly problematic.

2) This research is also inseparable from the emergence of the greenhouse effect on the international political scene, notably at the conferences held in Rio (1992), in Kyoto (1997), and Buenos Aires (2002), to name only the most significant.

Since 1979, various international scientific and political institutions have been created for which climatic change, in the broad sense of the term (including holes in the ozone layer, acid rain, desertification, etc), is a central concern. One might believe, in keeping with traditional epistemology, that scientific knowledge is independent from and precedes political decisions. In reality, the scientific and political realms have been shown to evolve jointly, even in instances when internal consensus could not be achieved. Climate change studies have emerged a key element in the transformation of the world sometimes referred to as *global governance* (in the Foucauldian sense of a tight combination of knowledge, techniques and power). America’s withdrawal from the Kyoto protocol must doubtless be analysed from this geopolitical point of view and opens a new phase in this governance, although this cannot be further treated here.

The second characteristic feature of climate change studies is that they were from the start constructed as an hybrid political *and* scientific field, whose agenda, research programmes and hierarchy of objectives were partly determined by a geopolitical agenda. This feature has been the source of considerable tension between the scientific logic of the various researchers, laboratories and scientific subgroups involved and the geopolitical imperatives they were expected to serve. The different ways in which nations such as France, the UK or the United States organise and direct research have for instance had direct repercussions on the kind of climate research carried out in these different countries. The varied and complex social and professional landscape of climate change studies has necessarily shaped the way in which scientific stakes and priorities have been defined.

The link between science and politics has taken the form of a peculiar institution, the Intergovernmental Panel on Climate Change, or IPCC, involving several hundred scientists. Created in 1988 by the World Meteorological Organisation (WMO) and the United Nations Programme for the Environment, its task is to inform governments of the state of knowledge in this area. It has progressively formed three working parties studying: 1) the climate’s and the natural biosphere’s physical and chemical system; 2) the impact of climate change on the biosphere and on socio-economic systems (this group also works on the adaptation and vulnerability of ecosystems); 3) the mitigation of climate change. This organisation has contributed to structure the whole field.

Numerical modelling has emerged as the only tool allowing a quantitative projection into the future. In each group’s work and reports (there were three extensive reports in 1990, 1995 and 2001), models played a crucial role and were the main tools used in IPCC expertise. In the first approximation one can say that physicists, oceanographers, natural scientists, etc., are predominant in the first group, while economists have the upper hand in the third. The second group is made up of all sorts of specialists, such as hydrologists, biologists, ecologists, climatologists, and economists.

3) However, the boundaries of the fields defined by these three groups are blurred; the results and objectives of each group are interdependent. This brings us to the field’s third

characteristic, which gradually emerged over the past few years as a result of a growing understanding of the scientific, political and socio-economic factors governing climate change.

What is the IPCC's methodology? Its third Report (2001) charts in considerable detail its climatic projections until the end of the 21st century. These projections were carried out only after the completion of a preliminary and necessary stage: the determination, for the same period, of evolution scenarios of forcing agents such as greenhouse gases and aerosols, a task which comes within the remit of socio-economists.

These drew up a series of possible images of the future, each of which is internally coherent (in terms of demography, type of economic development, social and technological choices).[15] These images can be static snapshots of a particular moment in the future (2050, 2070, 2100…), but they can also reflect possible evolutions of states, hence the use of the more dynamic term *scenario*, to which I will return. All these images are then translated using one single global variable:[16] the concentration of carbonic gas in the atmosphere (or the number of carbon particles per volume, ppcm). The use of this variable enables scientists to focus on the globe's temperature, which has proven much easier to manipulate than other climatic indicators (rainfall for instance), this choice being of course very simplistic.

The IPCC's third report sought to evaluate climatic change in three stages:

a) define economic scenarios of the future evolution of greenhouse gas emissions;

b) use biochemical models such as carbon cycle models to establish, on the basis of emissions, scenarios of future atmospheric concentrations of these greenhouse gases;

c) finally, use models of the general circulation of the atmosphere, forced by these concentration scenarios, to evaluate future climatic change.

But given the importance of interaction and feedback, this simplified, linear methodology proved deceptive, as scientists have begun to realise in the past few years. When preparing medium- and long-term forecasts, the first group, which a priori focuses on the mechanism of the earth's climate system (physical, atmospheric, thermal, oceanographic processes and so forth), cannot ignore the scenarios produced by the third group. But it must also integrate elements of the second group's work (e.g. impacts: how will hydrology be affected? how will vegetation and agriculture be modified during the first decades of eventual warming?). Attention must be paid throughout to the strategies developed at each stage of the process. The models used by each of the three groups must therefore be able to take the results of the two others into account, and so to operate in a loop. The longer-term the prediction aims to be, the more important this feedback is.

These feedback loops, to which I will return, have only just begun to be built. It should be noted that as simplistic as it may be, the choice of a single variable, carbon concentration (or carbon equivalent), is convenient, while the impact of climate on the economy and on man

[15] These scenarios neither take into account the impact of climate-related general politics nor the emission objectives set by the Kyoto protocol, though they do make allowances at different levels for governmental action, e.g. the development of low-energy consuming technologies.

[16] In the third report of 2001, four new variables were introduced, corresponding to the concentrations of greenhouse gases other than carbonic dioxide, referred to as 'carbon equivalent'.

is local, multiple and heterogeneous. Despite a pressing social demand for a study of the local impacts of global climate change, it remains difficult to construct regional models and to interpret the results obtained.

The issue of integration, which is the third characteristic I wish to highlight here, arises within climatic models themselves and in the integration of the climate with the socio-economy. It raises significant methodological, epistemological, but also socio-professional difficulties (connected to the required interdisciplinarity and to the different logic of the actors and sub-disciplinary communities). Integration is necessary, given the systems' complexity and it shapes the way in which models are collectively constructed. This takes us back to the shift I have advocated above, from studying models to investigating modelling practices.

Integration also affects the certainty and reliability of the predictions derived from models. Models which left out this or that major interaction (that is, which did not integrate it) will be less trusted. And uncertainty is closely linked to the political acceptability of all the measures and actions that one could consider implementing in order to fight climate change.

A few words on the new term *scenario*. This keyword has appeared in the modelling jargon of many disciplines (chaos studies, mesoscopic physics, earth sciences), and especially in historical disciplines where numerical experimentation has finally been made possible (paleoclimatology, evolution theory, embryology…). Halfway between a model and a narrative, the scenario allows a smooth transition between what the model produces and the accounts describing this production. It also conveys a certain modesty about the results achieved by models. In the field discussed here, the scenario often expresses a manager's or a political decision-maker's philosophy of modelling, rather than a scientist's. As a consequence, the modelling of many complex systems now privileges prevision and expertise above a deep understanding of the phenomena under consideration.

The three characteristics of the field are interdependent. It is because the object of study is less the climate than its sensitivity to anthropic effects that the question of integration is so decisive. Such an interdependence renders necessary the simultaneous treatment of the different scientific, operational, technical, institutional and political aspects which shape its development. I will however begin by giving an insight into climate model-building processes, which reveal an amazing complexity of practices and methodological problems.[17]

4. Models of the Planet's climate machine

Climatic modelling sits at the intersection of several scientific traditions: an older and strictly climatological tradition using simple or conceptual models,[18] and a meteorological tradition closely linked to numerical meteorological forecasting models, which has greatly developed in the West in the last 35 years.

[17] My research deals mostly with very recent history : it is based on archival reports by French laboratories, as well as on anthropological investigations. For the past 18 months I have lead a research project which aims to a reflexive analysis of scientific practices in the field of global climate change. I also carried out extensive interviews with scientists in this field. Finally, some of my conclusions are based on material collected by Hélène Guillemot, PhD candidate at the Centre Alexande Koyré, Paris, who is working on the history of climate modelling in France ; our discussions have been very stimulating. I want here to thank all the members of our reserach team and particularly Jean-Yves Grandpeix (LMD, CNRS) who helped me to enter in this field .

[18] Kandel [2002], [Nebecker] 1995.

The simple, non realistic, model of the planet's radiation balance

The older climatological tradition created the simple model of the planet's radiation balance, on which claims to the effect of greenhouse gases on the climate rest. Based on Tyndall's 1861 climatic theory of carbonic gas, Arrhenius (1859-1927) had already argued in the early 20th century that the 'average' temperature of the surface of the earth can be calculated using the gaseous absorption (by water vapour, carbon dioxide or methane) of the infrared radiation emitted by continental or ocean surfaces.[19] This model has a simple predictive value: if the level of carbon dioxide rises, the surface temperature rises with it.

Of course, this model is not very 'realistic', in that it accounts neither for local temperature variations nor for atmospheric or oceanic energy transport. I use here the term 'realistic' not in a philosophical sense (in opposition to nominalism or constructivism), but in the sense scientists tend to use it. The degree to which a model is realistic depends on how far it takes elementary processes and observed phenomena into account, or, in other words, how 'representative' it is of reality. However, considering that in the case of climate change the system has never been observed, *realism* cannot simply be equated to conformity with the observations.

General circulation models (GCMs)

In order to obtain increasingly realistic models taking into account an increasing number of processes and able to predict an increasing number of parameters liable to verification, climatology's 'meteorological tradition' rapidly became hegemonic. This was due, firstly, to a potential structural similarity between meteorological forecasting models and climatological models and, secondly, to computing power and the floods of available data —in particular coming from space research.[20]

This meteorological tradition first aimed at the construction of large GCM models, e.g. three-dimensional representations or simulations of the atmosphere's movements and changes in its physical state (in other words, global circulation and related meteorological, chemical, biological and other processes).[21] As early as 1975, Meteo-France for example relied on a meteorological numerical prevision model for the Northern hemisphere. From 1988, the organisation switched to a global model to produce its forecasts. In 1992, with the appearance of a new generation of parallel computers, this model was replaced by the Arpège model (now comprising between 500,000 and 700,000 code lines), which can operate either in numerical prevision mode or in climate mode, depending on what routines are activated. The two versions of the model share the same dynamic core and digital resolution, but they differ in their 'physical parametrizations' (I return to this below).[22]

[19] Where radiative forcing takes place, warming almost always results from a decrease in terrestrial radiation towards space (greenhouse effect), whereas cooling is linked to the increased reflection of solar radiation.

[20] Indeed, an important factor facilitating the emergence of and shaping this field of research was the exponentially increasing amounts of spatial observation data made available by agencies such as the USA's NASA, with its geostationary satellites and various means of observation and teledetection. Seeking to raise their public profile especially after the collapse of the Soviet Union and the end of the Cold War, these agencies began feeding laboratories and atmospheric science centres with vast amount of data. The planetary scale of space science contributed in this way to give a truly *global* character to the issue. See Edwards [2001], pp 31-65.

[21] Some GCMs were first tested on other planets, Mars in particular. Several climate model makers therefore come from astrophysics.

[22] In addition, the compromise between physical precision and calculation costs is different for each model. In climate mode, the model needs to run for years, while it runs for days only in numerical mode. Further, the size

Meteorology's methodology is clearly deployed here to cover the planet with a numeric grid, whereby priority is always given to the numerical resolution of fluid dynamics equations, to the determination of pressure and wind fields and their variations. Climatological models however consider the atmosphere statistically and on significantly longer time-scales than meteorology does.

Although they have been identified, the difficulties linked to scale remain significant. The two (vertical and horizontal) motions of the atmosphere for instance require a distinction between two types of scales (of the order of 0.1 km for surface turbulence and 1 km for convection, up to 1,000 km for the synoptic scale). Further, different phenomena work on different characteristic time scales. We thus have a horizontal grid a few hundred (200-500) kilometres long and a vertical grid covering between 100 metres and 10 or 20 kilometres. Every GCM (there are about twenty in the world) is divided into two parts: a 'dynamic' and a 'physical' part, the latter representing vertical exchange processes on a scale finer than the grid. The relationship between the two is far from straightforward, and is indeed a source of methodological and disciplinary tensions, to which I will return.

To resolve the problem numerically and to calculate variations in the atmosphere's physical state over time and across space, it is necessary to set boundary conditions and specify an initial state (as for any mathematical problem involving partial differential equations). Setting boundary conditions is immensely difficult, especially for the ground surface, where the atmosphere interacts with oceans, great glaciers, ice fields, and vegetation cover. As for the initial state of the atmosphere, it cannot be chosen arbitrarily and is supplied by WMO models which require lengthy data assimilation periods. Numerical prevision is especially tributary of a precise knowledge of the initial conditions (given the chaotic behaviour of the atmosphere); whereas climatological models rely more crucially on boundary conditions. The equations are solved by 'dynamics' (as a function of time), while the source terms (pressure and wind fields, etc.) are determined by physical 'parametrizations', the time step being of a few minutes for dynamics and from ten to thirty minutes for physics.

The dynamics/physics interface, parametrizations

The interface between dynamics and physics in GCMs is always problematic. It is a source of methodological and epistemological tension, of professional and disciplinary disagreement between model makers in the tradition of numerical prevision and scientists interested in fundamental research (in physics, thermodynamics, turbulence studies..). Indeed, all physical processes taking place on a scale much smaller than the model's grid cannot be calculated on the basis of physical laws, and are replaced by parameters. This means they are handled *indirectly* in the dynamic model; their climatic effect is estimated rather than actually calculated. This very complex methodology is referred to as *parametrization* and concerns little-known physical processes escaping dynamics, e.g. average and deep convection, clouds, radiation, the processes taking place in boundary layers such as evaporation by the vegetation cover, etc.

Parametrization involves several steps: 1) a physical conception of the phenomenon is taken as the starting point, to be translated into stable and effective algorithms; 2) these are

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of the numerical grid is not the same. Finally, elements such as stratospheric ozone or ocean variations which do not affect France's weather in the short term are left out in the prevision model, while they have to be included in climatic models.

tested against observation and 3) validated in a climate model using simulations. Problems then frequently arise, for the improvement of one aspect often results in a deterioration of the global result, due to incomplete control over all the feedback and compensation mechanisms at work in each model. For instance,[23] the introduction in a GCM of a new parametrization of tropical convection caused a catastrophe when unknown and inexplicable feedback reactions appeared. Substantial difficulties can also arise from a change of resolution: a parametrization which worked well at resolution T_{21} no longer functions at resolution T_{42}.[24]

Clouds perhaps best illustrate the complexity of the dynamics/physics interface. Establishing the clouds' physical role in the radiation balance, or in connection with steam physics has proven extremely difficult and even principles remain a matter of intense debate. To elucidate them, scientists have sometimes resorted to simple, so-called 'conceptual' models, to isolate and investigate a single fundamental issue. Thus, a numerical simulation — 'the radiative forcing of the nebulosity' which assumes that clouds are instantly transparent to radiation— was launched in 1997 to evaluate the feedback of clouds on the radiation balance.[25] Using such a forcing amounts to isolating one single aspect of cloud feedback and, *ceteris paribus,* evaluating the causal importance of this factor (in particular whether the feedback is positive or negative). Here computers are used as 'inductive machines', as Charney described this practice 40 years earlier, and can only have, at best, a heuristic function. It has failed to convince most specialists, especially physicists seeking to study physical processes closely and gain a deeper understanding of the phenomena.

Since the 1980s two Toulouse-based physicists have been working, together with American colleagues at the National Center of Atmospheric Research (Boulder)[26], on cloud phenomena on extremely small scales (less than 1km). They combine the direct observation of clouds, radar-Doppler data, and simulations, to produce very small scale models of convection phenomena. Through the application of averaging processes, which introduce new parametrization terms, they obtain small 'Cloud resolving models'. But this raises problems of scale shifting: how do these clouds interact on larger scales? How do they behave collectively? The initial purpose of this research on clouds was clearly to 'theorise' physical processes: to find a theory of 'squall lines', to understand how a storm creates a whirlwind, to explain the formation of coherent storm structures (e.g. that recur every three days in Africa), and so forth. As it became increasingly involved in climate change issues, this physicist's logic had to adapt to the more operational objectives of the model builders and developers. It had to focus on the parametrization of phenomena (in particular convection) in order to improve models.

Among the scientists working in the field of climate change, two ideal types can be identified, that are opposed on different levels. The first ideal type is interested in forcing and feedback, while the second aims to build the most comprehensive and realistic models, able to reproduce the earth's climate. The issue of parametrization brought out this opposition: the first[27] believe that while parametrizations may be improved, the main objective is to gain a better understanding of the response to disturbances; for the second, the top priority is to

[23] Interview with J-F Royer, Météo-France, January 28, 2004
[24] Explain
[25] Le Treut [1997].
[26] Interview with Jean-Philippe Laffore on January 29, 2004 ; with J-L. Reidelberger on January 30, 2004. Both physicists stayed over a year in Boulder.
[27] In his study of English-speaking countries Shackley calls them 'seers'.

produce the most realistic representations and to come as close as possible to physical phenomena.

The representation of clouds illustrates well the contrast between these two approaches. All researchers know that the atmosphere is very sensitive to the slightest variations in radiative or microphysical properties. The first believe several decades will be needed to understand the role of clouds, that other approaches are preferable to this Titanic task. These researchers only reluctantly add new levels of complexity, they prefer using simpler models that allow a larger number of numerical simulations and the testing of more models. Despite their reliability and efficiency, GCMs are cumbersome, not unlike Formula One cars, as some researchers have pointed out. For some tasks, smaller models –2CVS— can prove more useful. For scientists adhering to the second ideal type, fundamental research comes first, physical processes should remain the central focus and should be integrated into the model.

Both groups, however, are reluctant to alter a model that works. Given the number of interactions in each climate model and the number of possible error compensations, the addition of an extra level of complexity or the modification of a particular parametrization can disturb all the others and cause the model to diverge in uncontrollable ways.

The opposition between these two ideal types is not a metaphysical one. It is rooted in particular forms of knowledge, know-how and practice. Climate is traditionally understood as a combination of thermodynamic (heat and convection) and dynamic (atmospheric and ocean circulation) phenomena, which can be given different relative importance. Those who privilege thermodynamics consider the earth as a box affected mainly by thermal fluxes and tend to focus on the increasing concentration of CO_2 and its associated thermodynamic feedback –e.g. of ice fields with the influence on albedo, of clouds with the the quantity of incident and reflected radiation, of water vapour. Dynamic properties (of the atmosphere or the oceans) are rarely mentioned in their thermal balance. Dynamicists in contrast are more cautious in assessing and using this kind of work. Their background is usually in numerical weather forecasting and meteorological research. They pay more attention to the presence and control of errors in models, which can decrease the realism of the simulations' response to disturbances. They find attractive the way in which El Niño is currently modelled.

Of course, these two thermodynamic/dynamicist standpoints are ideal types. Every researcher or small group exhibits an intermediary and specific combination of these two extremes. This combination also depends on the chosen field of research. In temperate areas, dynamics govern atmospheric phenomena and clouds evolve on a large scale, whereas in Africa and in Tropical areas, dynamics is subservient to thermodynamics. In practice, scientists always combine relatively simple models with work on more realistic GCMs. This is how they attempt to master complexity.

Coupling with the ocean, Controversies on flux adjustments

In the continuous evolution towards more realistic models, the atmosphere-ocean interaction rapidly emerged as a primary factor, considering the all-important role played by oceans in the process of heat redistribution. Great computer programmes were set up in the 1990s in the United States and in Europe to produce coupled atmosphere-ocean models. This arduous task required several years of research, because the ocean's time scale is significantly larger than the atmosphere's, and because important gaps exist in the available observation data. Further the current methodology, more or less based on the physical laws of propagation and whose mathematical expression is known, no longer applies. In oceans, thermal processes are crucial.

France was then lagging behind due to the presence of two competing atmospheric models, used respectively by Météo-France (Arpège) and by the Laboratoire de Météorologie Dynamique (LMDZ), reflecting the two institutions' antagonism.[28] In addition, their relations with oceanographers were little developed at the time. A scientific computing laboratory, the Cerfacs,[29] was put in charge of studying the question of coupling. In a tense institutional context, it sought to develop a 'universal' methodology, which might be simultaneously applied to both atmospheric models in order to couple them with the CEA's oceanographic model.

Coupling, as Cerfacs director J-C. André stressed, is above all a reflection on the way in which two milieus exchange physical quantities, which will determine their interaction. Models and exchanges then have to be *synchronised*. Beyond the physical understanding of coupling and of the time and space scales involved, technical choices also have to be made concerning the type of machine and communication used, as synchronisation implies that fields (of pressure or of wind) can be switched and interpolated from one model to another. The Cerfacs chose to retain a *modular conception* of modelling – e.g. to preserve the identity of the models to be coupled – and created a fictive layer between the two environments - atmosphere and ocean - to encapsulate both models relative to these environments.[30] Certain variables were then selected from the first environment and sent to the intermediary layer, and the same procedure was repeated from the intermediary layer to the second environment. The first simulations were carried out four or five years later and the coupling programme (OASIS) was finally completed in 1997. Yet, as E. Terray explained, the programme does not allow the actual coupling to take place immediately: its interface first has to be implemented in the (2, 3, or more) models to be coupled —for instance ocean-ice coupling, without any atmosphere or with a simplified atmosphere…

In coupling with the ocean, all little-known processes are again 'parametrized' —they are handled indirectly by estimating rather than calculating their climatic effects. But from the beginning, when coupled atmosphere-ocean models started being used in the United States in the 1990s, 'climatic drift' phenomena appeared, revealing the inability of these models to reproduce a stable climate. Model builders accordingly focussed on parametrizations and on 'flux adjustments' connected to the physical plausibility of such changes. Thanks to this empirical technique, which seems better mastered today, more realistic results can be obtained with a shorter calculation time; but this technique has caused great controversy.

In order to determine the superficial ocean's heat balance, measurement and data collection campaigns were launched to investigate cyclogenesis (the generation of ocean depressions) and the impact of wind and the state of the sea on the quantity of movement exchanges between the atmosphere and the ocean. The Toulouse team was searching for a 'universal' parametrization. But as S. Planton noted, 'a universal parametrization cannot exist! Parametrizations are conditioned by geographical zones. There is too much complexity,

[28] See below for a discussion of these models.
[29] The Cerfacs is a peculiar, hybrid structure, a private law civil company which operates in collaboration with four large organisations: the CNRS, Météo-France, EDF and SNECMA. The climate modelling team is a research unit associated with the CNRS.
[30] Interview with Cerfacs director Jean-Claude André and Emmanuel Terray, responsible for the elaboration of the coupling code for atmosphere and ocean models.

certain processes are dominant in one zone but not in another…'[31]. Parametrization in a numerical model therefore appears as a method of empirically adjusting a physical process, radically distinct from the theoretical representation of this process by a physical law.

According to a survey conducted in 1998-99 by English and American sociologists, scientists have split into two camps with regard to these techniques. Purists are reluctant to use such adjustments and advocate new programmes of fundamental research into real physical processes; while more pragmatic scientists, although they admit the downsides of these techniques, deem them irreplaceable, considering the urgency of the issues at stake and the demands of agencies and governments. Scientists who resorted to such adjustments tended to be connected with the IPCC or decision-makers, and often worked on scenarios. On the other hand, fundamental research programmes and a privileged integration into the academic community tended to remove the need for such adjustments. In 1999, the authors of the report concluded that 'The debate on flux adjustments is a prism through which one can explore the model builders' social, political and scientific presuppositions'[32]

The division of work between different groups —physicists, oceanographers, numericists, university researchers or model builders working in large centres for atmospheric science— can therefore generate different attitudes towards computers, towards the simulations they produce, and towards models.

Towards a generalised science of couplings

After the ocean, the biosphere and the climate need to be brought into constant interaction, an undertaking which has only just begun. How to model the global hydrologic cycle, or couple bio-geochemichal cycles with the climatic system? These are some of the innumerable questions that remain unanswered, and which have enormous implications for the reliability of models and the uncertainty of their predictions. The various phenomena linked to chemical reactions (ozone, aerosols..) in the stratosphere and the troposphere also need taking into account, phenomena which, as we are beginning to grasp, play a crucial role for evaluating the extent of climate change.

Every extension, every coupling and every inclusion of a new phenomenon requires research by specialists, who need to understand each other and co-ordinate with others, in addition to the actual computing and technical work required to harmonise different models. In these transdisciplinary collaborations, instances of mutual incomprehension and disagreement are frequent, made worse by the fact that coupled models tend to fail in their early stages. Indeed, non-coupled models (of the atmosphere, the ocean, ice…) are 'forced' by particular boundary conditions which constitute a kind of implicit modelling of the exchanges with the outside environment. This forcing enables simulations not to stray too far from a given trajectory. When models are coupled, these constraints are lifted and degrees of freedom are added to the system, which can lead to aberrant climatologies or climatic drifts. This discrepancy is connected to the coupling itself and lies at its interface.

5. An overview of recent developments

[31] Interview with S. Planton on January 30, 2004.

[32] Simon Shackley, James Risbey, Peter Stone & Brian Wyne 1999. See also H. Guillemot, 2001 and PhD thesis (in preparation). The French survey displays a similar diversity in scientists' attitudes and standpoints.

Before examining the field's most recent developments, it will be useful to examine its forms of institutional and political inscription and the configuration of the actors involved.

A few remarks on the French configuration

Two scientific institutions currently work on climate change models in France. The first is Météo-France, a Toulouse-based public and national organisation specialised in meteorological prediction and which uses the Arpège model. It is a centralised, operational organisation which, in accordance with its remit, is less interested in fundamental research than in operational aspects. It is staffed mainly by engineers, recruited as fresh graduates from the prestigious Ecole Polytechnique, who chose to enter the Meteorology Corps and become top-ranking civil servants. Bringing an excellent education in mathematics and physics, these engineers receive additional training in meteorology and modelling. In the mid-1980s, Météo-France set up a research centre, the Centre National de la Recherche en Météorologie, for investigating the problems and the models of numerical prevision and data assimilation. Since the early 1990s, the centre has also begun studying climate, urban pollution and seasonal prevision. In the centre, research engineers rub shoulders with development engineers. In the past decade, the Meteorology Corps has diversified its sources of recruitment, and extended it to a few other Grandes Ecoles (e.g. the Ecole normale supérieure, Agronomy..). At Météo-France, the distinction is relatively clear-cut between model makers and developers (who can also work on parametrizations) on the one hand, and model users, who carry out simulations and test the validity of models, on the other.

The second organisation is a federation of five laboratories (including the Laboratoire de Météorologie Dynamique, the oldest in the field), which gathered in the Institut Pierre-Simon Laplace in 1994.[33] These laboratories are all associated with the CNRS and the majority of their members are researchers, engineers and technicians who belong to this organisation. Each of these laboratories handles two or three components of a general circulation model and each researcher (of the LMD, of LODHIC..) works on three, four, sometimes even five, research topics. Models, as we have seen, contain several layers or levels[34] organised like computer networks —phenomenology of appearances; physical, chemical, biological or economic theory; mathematical model; numerical model; computer model—, each element of the system being represented differently in each of these levels. Researchers at the Institut P-S Laplace can work on several of these levels, alternating fundamental research (e.g. on convection or radiation), with the improvement of models and parametrizations, research on data, simulations, work on interfaces and couplings, code writing, etc. Research is not organised along hierarchical lines and the researchers' motivations for doing modelling work are very diverse.

The LMD, which had been working since the 1970s on a 'home-made' model of the planet (made obsolete by the appearance of new calculators), took in 1990[35] the strategic decision to build a new model, the LMDZ, instead of working on Météo-France's Arpège model, then under construction. These researchers refused to adopt Météo-France's more organised and hierarchical working style, fearing that they would become users rather than actors in the development of the new model. They wished to retain the conditions of

[33] The other laboratories include the Laboratoire des Sciences du Climat et de l'environnement (LSCE) and the Laboratoire d'Océanographie Dynamique et de Climatologie (LODYC) linked to the Commissariat à l'Energie Atomique (CEA). It is the latter that constructed the ocean circulation model.
[34] As LMD's J.Y. Grandpeix put it.
[35] Fontevraud seminar, 1989.

independent research and control over their choices. At the time, the scientific authorities did not interfere with this decision, which had the effect of delaying the team, from the point of view of operational efficiency in the international scientific race (especially against the UK and Germany). The French climate community remembers this decision with mixed feelings. France is the only country in Europe to work on two different atmospheric models.

From 1995, thanks notably to the Cerfacs, the first ocean-atmosphere couplings were carried out both with the LMD's atmospheric model and Météo-France's Arpège model. The two organisations involved in climate change models have moved closer to each other, allowing a better agreement on objectives and strategies, more fruitful scientific exchanges, as well as a limited distribution of tasks.

An example of interdisciplinary work:the Meso-NH code

At Météo-France, individual feelings are more rarely expressed. The structure of the organisation and the way in which it functions confer to it an unquestionable operational efficiency. In the mid-1990s, the directors decided to develop a meso-scale research model useful to the whole climate research community for work on small to medium scales, hoping it would prove useful in the long run for local numerical prevision. J-F. Laffore, research engineer at Météo-France, co-ordinated the project for five years. It resulted in an atmosphere and surfaces simulation system, including an ocean section, which can operate at the user's request on scales ranging from 1 cm to 70 km. This system, or 'Meso-NH code', is equipped with all the necessary parametrizations; it can call atmospheric chemistry solvents or be coupled with an aerosol module. Its new and essential element is a surface model which distinguishes vegetation cover from urban areas, ice fields, etc. (see below).

The Meso-NH project required great engineering and management skills in order to enroll and combine different kinds of competence, to co-ordinate research, harmonise the different sub-models' codes, and document everything accurately. Further, a long process of pooling the different sub-communities' concepts proved necessary. Indeed, 'making sure that the words used by each group and for each model have the same meaning, Laffore explained, requires a great number of meetings'.[36] Every project like Meso-NH code which presupposes, on top of the construction of new models and the adaptation of existing models, an important coupling work is first and foremost an interdisciplinary task that requires a huge effort of *translation*.

This project illustrates the cultural differences between both institutions. A CNRS scientist striving to obtain new results and be published in international journals would probably not have been able to devote enough time to it.[37] As a researcher at the Institut Pierre-Simon Laplace said, 'the rules of today's research in France favour ten small disciplinary topics where everyone is his or her own boss rather than a single large transdisciplinary project under a single leader.[38] In short, the French landscape is structured by two poles: on the one side, the CNRS, staffed by researchers who are brilliant, very autonomous (scientifically) but relatively individualistic and fragmented, and, on the other, a very well-structured and increasingly technological organisation, but lacking real researchers.

[36] Interview with J-F. Laforre.
[37] Today, Météo-France is developing the AROME project, the equivalent of Meso-NH for numerical prevision.
[38] Interview by Hélène Guillemot, Autumn 2003.

The introduction of surface models

H. Douville has built up his own 'ecological niche' at Météo-France, where, for the past few years, he has been studying the representation of continental surfaces and their feedback on the atmospheric climate.[39] He focuses on the role of vegetation, soil hydrology, surface water flow, and the flow of large rivers.

Traditionally, climate models did not deal with continents. But the trend towards 'integration' created a need for soil models and in this field too, parametrizations were introduced. A brief description of his project illustrates these scientists' contemporary methods and the networks they belong to. Douville built a surface model based on three available models whose he complexified some elements : the first, ISBA, studies interactions between the soil, the biosphere and the atmosphere, and can run independently; the second, TRIP, deals with the flow of the planet's great fluvial basins (the Amazon or Mississipi rivers…) and was created in Japan; the third focuses on ice fields. The Toulouse scientist, for his part, was mostly interested in soil humidiy and its relation to climate warming.

Douville's model features three hydrologic layers (a surface layer, a shallow layer at root level, and a deep layer) and an additional layer of snow, as well as a water reservoir within leaves. He did not include the size of leaves into this model as, he claims, one has to know when to stop – not a obvious decision to take. As one researcher at the Laboratoire de Météorologie Dynamique[40] explained : 'some want leaf size to be interactive with temperature ; the size of the leaves is a very practical problem, it is important for evaporation, photosynthesis, water balance, etc. We try to include all this in a global model [but then] we longer know <u>what the effects will be</u>. […] we believe the most complex model possible will enable prediction ; one school believes this. Others believe rather that modelling is an object in itself, a tool for understandiing the system, and that the purpose of modelling is understanding.' This tension is shaped by the search for increasingly realistic models, integrating an increasing number of phenomena and feedback.

Douville's methodology consists in 'forcing' surface models with Météo-France's atmospheric model before re-integrating the soil's humidity field in the complete model, which produces a more realistic behaviour. It is therefore an indirect validation of the soil model, via the atmospheric model.

The aim is to predict seasonal anomalies, inertia, or 'climatic memory effects' in the soil. His model is part of an European project to compare surface models —a comparison which reveals such disparity in its results that it requires constant recourse to process physics. Indeed, he notes, a notion such as 'absolute water content' varies so much from one model to another that one wonders whether it is a physical concept at all. He also stresses the need to avoid locking oneself into a single model and to compare the output with observations. The model could be correct, but the data input could be faulty in the absence of systematic measurements of soil humidity, impossible to obtain by teledetection or aerial photography.

Besides, climate warming and soil dryness appear to be linked, but to what extent is the feedback positive? This constitutes an important source of uncertainty, varying from one region to another, doubtless more so for African than for Indian monsoons, where feedback acts differently. In the construction of this kind of model systematic measuring and

[39] An engineer trained as an agronomist who entered the Meteorology Corps in 1991.
[40] *Entretien avec Hélène Guillemot, 11 Juillet 2003.*

observation campaigns (e.g. of humidity, water balance..) must be undertaken. It is useless to overly refine a model if the input's quality is low.

The introduction of soils into models of the earth system's climate machine thus came along with a whole set of new conceptions, practices, actors and models. This is part of the mentioned continuous movement to integrate increasingly heterogeneous elements, complex mechanisms and feedback; but the new consideration for soils also announces a return to more local matters.

Back to local issues?

Indeed, soil models are usually constructed on the meso-scale, and are closely associated with various social demands. The scientific community which deals with such models is heterogeneous, and increasingly so: biologists work alongside agronomists, geologists, podologists, forest specialists, etc.. Given the loop mechanisms, this community largely overlaps with that which studies the impact of climate change on local or regional ecosystems.

Of course, a strong link exists between the way in which nature is represented and the way in which it assessed. Because they work using world-wide data networks, global numerical models, and processes integrated at the planetary level, IPCC scientists do not usually deal with the climate of specific countries. They speak of the global climate system or of the earth system – an object to be explored, understood, described, and managed on a planetary scale. The pragmatic meaning of climate thus tends to be blackboxed, the focus being on global quantification. Climate models cannot exactly predict how climate in this or that area of the globe will be affected.

But the impact of climate change is not uniform and geographical disparities are great; it is predicted for instance that climate warming will affect especially the higher latitudes. Closing the gap between the local and the global is now perceived as an important task. Awareness of and political decision on climate issues are clearly connected to the existence of concrete and local images of climate change; there is accordingly an increasing social need for local models on the part of governments and regions. Météo-France as well as the Pierre-Simon Laplace Institute have accordingly increasingly focussed on regional simulations.

Uncertainties in climate/carbon cycle feedback

The issue of uncertainty weighs considerably on political decisions and the cost that countries are willing to bear to fight climate change. Two very different types of uncertainty related to climate change in the coming century can be identified:

a) those linked to scenarios, i.e. to the choices made by societies this century;

b) those linked to models. For a given scenario, the discrepancy in the results obtained using different models reflects the imperfection, the incompleteness of models and their failure to *integrate* all the feedback loops. Adding further to these uncertainties is the fact that the physics governing certain phenomena, especially on small atmospheric scales, is never totally predictable. In temperature predictions for 2100, all the projections give a rise in temperature between 2 to 6 degrees. This margin of error displayed by the curves of the IPCC's third report is however disputed by many scientists who believe it can be reduced. Half of its magnitude seems to be connected to economic scenarios, the other half to the models themselves. Hence the generalised and growing importance of model-comparing programmes, financed by European or international organisations.

The uncertainty linked to climate/carbon cycle feedback seems significant.[39] The IPCC's methodology (see above) does not yet take into account the possible feedback of climate change on the economy or on biogeochemical cycles. CO_2 is emitted by the combustion of fossil reserves or biomass. Today, about one half of this gas remains in the atmosphere, while the rest is re-absorbed by the oceans and continents. The mechanisms governing these oceanic and biospheric sinks involve a whole range of processes (photosynthesis, the decomposition of organic matter, etc..) which are directly linked to climate and whose efficiency, one can reasonably assume, can vary —in other words, the fraction of released CO_2 remaining in the atmosphere could change.

Two studies (by the British Hadley Centre and the Institut Pierre-Simon Laplace) have brought to light the sensitivity of these mechanisms. They show that, for a given emission scenario, the atmospheric concentration of CO_2 in 2100 (and thus its associated climate change) is higher than if this feedback is left out. The Hadley Centre's model absorbs large amounts of CO_2 in the soil and the speed at which the carbon is degraded is very high; the model simulates a drastic change (a carbon concentration in the soil of 980 ppmv instead of 700 ppmv and no feedback). The reason for this change is that the continental biosphere is thought to become, towards 2050, a source of carbon instead of being a sink, as soil warming activates the decomposition of organic matter and occasions a high release of CO_2. The IPSL model, although less pessimistic (780 ppmv), confirms the effect of feedback. Here is an example of uncertainty linked to the effect of climate change on the carbon cycle, one of many loops which have yet to be *integrated* into climate models.

Links with the IPCC

For scientists, the decision to test (or not) the economic and energy scenarios proposed by the IPCC on the (usually coupled) climate models they work on can signify constraining commitments and painful choices. The scientific community is divided on this issue. European or international programmes have often been accused of squandering precious research time with their frequent meetings and bureaucratic procedures. More fundamentally, researchers blame the IPCC for imposing increasingly unwieldly and sophisticated models. Some scientists at the Laboratoire de Météorologie dynamique believe that, in supporting and favouring research on the integration of an increasing number of interactions, the IPCC is promoting conformism at the expense of the conceptual issues raised by modelling and the overhaul of parametrizations's physical foundations.

Météo-France's research centre, a more intistutional and collective body, has in contrast decided to dedicate half of its work on climate (amounting to 10,000 hours of computing) to run simulations of its models, from 2050 to 2100 ; this with the aim of testing IPCC scenarios and to significantly contribute to the fourth report, scheduled to appear in 2007. This institution is keen to feature in this international scientific and political showcase, considering it crucial for gaining scientific credibility.

[39] See the overview of this issue in Friedlingstein [2003] in Changement climatique: l'état des controverses, Iddri, Paris.

6. A few concluding remarks

In conclusion, and to wrap what has been a rather descriptive journey, I propose a few synthetic remarks. They deal with

1) At an anthropological level, the universe of numerical models and the status of simulations
2) From an epistemological perspective, the generalised methodology of model coupling
3) From a political perspective, the appeareance of a new universe of judgement and its entry into to public sphere

1. the universe of simulations and models

To check whether a given climatic model accurately simulates current climate, the methodology – similar to that of numerical meteorology – ideally consists in comparing the model's output (seasonal maps of temperature, rainfall, winds etc.) with the reality of the observed climate. In the transition from meteorology to climatology, simulations however acquire a stronger and more fundamental status. As noted above, models are not initialised with data drawn from actual observations. The climate data to which the models are compared are heterogeneous and have to be re-calibrated, interpreted and corrected according to models which are themselves used as input for other models.[40] The determination of the atmosphere's 'initial state' is one example. So-called *data assimilation* models run without interruption to supply general circulation models. Climate models are 'data-laden', to quote P. Edwards, echoing Pierre Duhem's claim that data (and hypotheses) are 'theory-laden'. Models thus begin working with physical constants (the quantity of solar energy hitting the atmosphere, the speed of the atmosphere's rotation on its axis, etc.), where data is averaged or supplied by other models. Models then run until they reach a state of equilibrium, which is taken to be a climate model. This simulated climate can then be compared to observed climatological averages of the real Earth, and it can also be used itself to simulate paleoclimates or make projections of future tendencies. The climate recorded over the past fifty years is thus regarded by climate model makers, albeit with certain precautions, as an effective anthropic forcing experiment.

When attempting to detect climate change (against the background of natural variability), information can only be gathered from models and not directly from observations, as scientists often stress. Hence the crucial importance of a rigorous methodology of numerical modelling and a critical and lucid analysis of the relationship between model-builders and their models.

The numerical laboratory, re-analysis, idealised modelling..

The scientists we met and the research we investigated paid explicit attention to methodological factors. L. Terray's team at Cerfacs, which describes itself as a team of 'model users', has for instance worked to define a methodology of the *numerical laboratory*. It consists in establishing an inventory of available models for addressing a given problem and the practical methods for using them; in identifying what preliminary *analyses* are required; in finding out what precise validation principles will be employed, etc.

[40] Edwards [1999 & 2001].

Indeed, researchers usually work not with a single model, but with a hierarchy of combined or coupled models, each with its own resolution grid, possible zoom on specific regions, and solvers which may or may not be called, and so on. The aim is to supply scientists with an interface with different 'boxes' and components, with means of easily launching simulations and visualising results quickly, of analysing, comparing and interpreting them. The aim is also to integrate a software structure on this interface to ease all operations, which are time-consuming without being particularly complex and which are often repeated with small, superficial changes. In short, the team has attempted simultaneously to formalise a methodology and the technical tools to apply and validate it.

Moreover, observations are riddled with errors and the databases have to be corrected using data assimilation techniques. The teams thus 're-analyse' models by using models constrained by observations carried out on a long period in the past, in order to obtain the best initial state to serve as basis for prediction models. Validation experiments are performed on the basis of simulations over the past fifty years (which constitute the reference trajectory) — but such trajectories cannot be taken at face value: tropical regions are relatively deterministic and stable, while the middle latitudes are much more chaotic. Variability therefore does not have the same meaning in both cases.

The model builders are also developing precise methodologies which they sometimes call 'idealised modelling', a somewhat surprising locution: is not every model by essence an idealisation? In their understanding, though, idealised modelling combines within each model a drastic simplification of a given class of phenomena or environment, with more realistic phenomena or environments. The corresponding methodology attempts to formalise the constant interplay of forces between ever more realistic models and conceptual models, i.e. between exploratory and predictive models. Mastering complexity and 'the chain from the simple to the complicated', requires this constant coming and going between a diversity of models and approaches.

Scientists have repeatedly rejected the idea of constructing a single, notably European, model, which might have disrupted the balance of power in the community and given official backing to its previsions. They all reject the idea of a unique, ideal model, representative of the real climate system (and able to reproduce it). Instead, a different kind of project was established, PRISM, which aims to make the different European models compatible, and whose architecture facilitates comparisons. Numerous such cross-comparison programmes exist (for GCMs, chemical models, soil models, different coupled models, paleoclimatic models, etc.) and are supported by international institutions. They result in detailed methodological protocols, regular colloquia where they are compared, and common publications.

A model universe? How to escape this virtual world

In climate change studies, models are collective constructions involving numerous inter- and transdisciplinary collaborations. Its modular structure illustrates the collective and continuous character of this process of construction. Model builders are never isolated, each can only intimately know and develop a small part of the model, and has to trust colleagues for the rest of the model, which amounts for them perhaps not quite to a blackbox, but perhaps to a dark grey box. Yet each model builder develops a quasi-emotional relationship to the model or the part of it he is working on, a mixture of fascination and infinite improvement.

As all specialists know and repeat, models are necessarily more 'polished' than reality, on account of the 'Courant-Friedrichs-Lewy' mathematical conditions, which define the relationship between spatial and time grids, in order for the numerical resolution of the model by discretization to converge and be meaningful. Yet scientists repeatedly point out the dangers of shutting oneself up inside the universe of one's model, to seek to improve it indefinitely, while one should always go back to observations, to improve data and input. 'We create a virtual world, a simulacrum of reality and we must be careful not to become its prisoner', one scientist pointed out.[41]

In short, scientists are clearly lucid and display epistemological maturity. But the mysteries of the numerical box —feedback, error-compensation phenomena, scale interactions— inherent to the computerised treatment of such a complex system, and which remain opaque even to the model builders themselves, make the plunge into the infinite intricacies of the model difficult to resist.

The (still limited) return to local issues, the need for reliable previsions and regional modelling are bringing model builders closer to measuring and observation campaigns. This perceptible development can only help the model builders escape the temptation to lock themselves inside their models.

2. Anatomy of an anti-reductionist methodology

The tension between understanding and predicting is particularly acute in the history of meteorology. In the field of climate studies, this tension is constantly expressed in scientific practices and it is the source of much strain, even if scientists on the whole deal with a large number of models (whether they are cognitive, predictive, conceptual, realistic, etc.) in order to master complexity.

The search for an ever more 'realistic' model of the earth system worries many scientists. In my view, this is a powerful trend, which lies at the heart of the dominant methodology of the past fifteen years.

This methodology seem able, at least in theory, of unlimited development, with its simulation and validation procedures, its comparison protocols, and its interfacing projects. From numerical GCM models and the first (atmosphere-ocean) coupled models, it has extended to coupling generally (ice and ice fields, soils, hydrological cycles, vegetation, etc.) and to modular approaches to such couplings, to their combination with chemical models, etc. Such a development remains still partly 'theoretical', because the methodology is limited by data availability (e.g. incomplete, inaccessible or heterogeneous data) and by computer size. But it is one of the most accomplished examples of a concretely anti-reductionist method. Indeed, anti-reductionism, transdisciplinarity and networking arguably define this methodology's anatomy.

Three new epistemological aspects finally confirm the discrepancy discussed in the introduction between the older epistemological discourse and contemporary model practices:

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[41] Interview with J-C.Lafforre

1) The particularly heterogeneous and disunified basis on which models of climate change are constructed. To overcome such disparities, a growing number and diversity of scientific groups form networks, which exchange modules or model components, translate their notions and concepts, learn to co-ordinate their coupling codes and the interface to other models; and which participate in great common programmes, while pursuing individual research projects

2) Not only computers but also the Web play a central role in practices and methodology, to foster the interdisciplinarity it presupposes and within the professional networks which support this interdisciplinarity. Computers and the Web are crucial to overcome this disparity and to initiate an integrative process.

3) A shift of attention away from models and towards modelling and coupling practices is essential, whose first effect is to introduce the category of actors into modellling processes. All constructed models are the outcome of collective work and each model coupling represents an extension of the community of the scientists involved, as well as an increased complexity of the actors' configuration.

Translated by Charlotte Bigg
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