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Changing the Atmosphere

Expert Knowledge and Environmental Governance

edited by Clark A. Miller and Paul N. Edwards

The MIT Press
Cambridge, Massachusetts
London, England

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Epistemic Lifestyles in Climate Change Modeling

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Climate models allow scientists to combine insights from many disciplines, helping to integrate the understanding of complex natural systems. At the same time, they enable scientists to portray climate as a global system, and human interventions as potentially global in their effects. Climate modeling can thus be portrayed as a form of "world building" (see Edwards, chapter 2, this volume).

In building global climate simulations, scientists make numerous epistemological choices and assumptions. These choices affect their final outputs in a myriad of ways, some trivial, others quite significant. The modeling strategies employed by individual modelers, modeling centers, and even national research and funding communities often vary considerably. This chapter explores several approaches to modeling represented among American and British climatologists, tying their systematic variation to a concept I term *epistemic lifestyles.*

In 1993 I worked for several months at the Hadley Centre, the United Kingdom's premier climate modeling center, as part of a sociological study of climate change science and policymaking. The degree of unanimity and consensus among the scientists concerning the Hadley Centre's aims and ambitions struck me at the time. The scientists there did not appear to disagree very much and argued in rather subdued and implicit ways. A year later I visited several American climate modeling centers, where I encountered much greater dissension and argument (within and between centers) over priorities and use of limited resources, methodologies, evaluations and interpretations of model results, and appropriateness of tools, and so on. The American scientists seemed to enjoy argument and debate in a relatively open fashion.

coordination between researchers in the United States—for example, through development of "consortia" with the primary climate modeling centers (such as the National Center for Atmospheric Research (NCAR) and the Geophysical Fluid Dynamics Laboratory (GFDL)) at the hub. B had been very impressed by the Max Planck Institut für Meteorologie (MPI) in Germany and the Hadley Centre (HC) in the United Kingdom. Both countries had rapidly developed highly organized and coordinated climate-modeling centers, which were increasingly at the forefront of building complex, comprehensive climate models and providing scientific insights to the Intergovernmental Panel on Climate Change (IPCC) and Framework Convention on Climate Change (FCCC). How could the United States be in danger of lagging behind, given that it had pioneered the climate-change field from the start—and still had many of the leading research scientists, and far larger research resources, than Germany and the United Kingdom put together?

The answer to that question for B appears to have resided in the European countries' very centrally managed and coordinated research effort, and their achievement of broad consensus on how to conduct climate modeling and interpret its outputs. B perceived the United States, by comparison, to have a very fragmented and diverse research effort, with multiple centers, multiple funders, and different and sometimes conflicting scientific and research policy objectives and management approaches.

Not surprisingly, C did not agree with B's assessment of his climate model. He wrote a letter in response, again widely circulated, which initiated further bilateral correspondence. C argued that his group's model compared favorably with others. His choice of a coarse resolution partly reflected the need to "perform many exploratory integrations prior to the successful execution of the main experiment." In other words, the final run written up as a paper and presented at conferences rested on many other, shorter, exploratory runs needed to tune the model, test computer code, study the model's drift, and so on. C's model run was distinctive in integrating the model as a control run for a very long time (1,000 years) in order to simulate low-frequency natural variability or "noise." From this baseline, he could then ask how likely it is that the observed climate change of the past 100 years is due to anthropogenic influences rather than natural variation. Priority was given, and resources devoted, to producing a long control run rather than to increasing the resolution or making parameterizations more complex.

C conceded that "obviously, we would have preferred to use a coupled model with higher computational resolution. . . . The choice of the computational resolution of a model should be made by carefully balancing the scientific requirements and available computer resources" (Scientist C, letter, August 23, 1993).

B's interpretation of this statement was that C's group had had to "make a choice so based on limitations of computer resources that, I would argue, you were doing significantly less than what you are capable of and what you 'obviously' preferred to do." The solution for B was to increase available resources. But he implied that in return, funders would require greater coordination of the climate modeling research community. Lack of computer resources had become increasingly serious, according to B:

> The answers [to questions about human-induced climate change] may not yet be certain, but they must be the best we can provide, or we must say clearly that resource limitations are preventing us from doing this. . . . The systems modeling community is experiencing such serious constraints that we do not now have the resources needed to assure that we have a set of system models under development and being used that is at the forefront of current understanding of what could and must be done. We, as a nation, need a range of system models from conceptual to exploratory to definitive that embodies all that we understand as being important to getting better answers; and, when we provide our best answers for important near- and long-term decisions valued in the many billions to trillions of dollars, we need to be incorporating all that is necessary in the models to assure we have the best answers we can provide. (Scientist B, letter, October 19, 1993)

B's concern is related therefore to the perceived needs of climate policymaking and to the collective effort required by the climate science community to meet them. C outlines a different interpretation than B of what are "the best answers we can provide," as explained below:

> I am not comfortable with the suggestion that a model with the highest possible resolution and the most detailed parameterizations should always be used for an experiment just because nature is infinitely detailed. . . . I am not convinced that many current parameterizations successfully incorporate the information from field experiments as you implied. This is because it is difficult to fully understand the details of the process involved and how to aggregate the information from a local field experiment to the grid scale of a climate model. . . . In my opinion,

> a parameterization should be as simple as possible in order to improve our understanding of model behavior, facilitate the tuning of the model, and reduce the computational requirement of the model.
>
> . . . Although a Manhattan Project of climate modeling, such as what you seem to have alluded to . . . can yield a very detailed model, such a model alone may not be very effective in gaining a "predictive understanding" of future climate change. To get predictive understanding, it is also essential to conduct a large number of numerical experiments by using not only the most detailed models but also simpler models and then comparatively assessing the results from these experiments. Whether a model is "state of the art" or not depends upon the objective for which it is used. (Scientist C, letter, August 23, 1993)

C is concerned that centrally coordinated model development and application would reduce diversity in climate modeling. Hence C also writes: "In dealing with such a complicated issue as global change, multiple approaches and strategies are essential."

B responded to C's preference for simple parameterizations as follows:

> While we can never expect to be fully as detailed as nature, I would hope and expect that we could aim to realistically represent the full set of critical aspects and interactions. The USGCRP is investing several hundred million dollars each year . . . to improve understanding of processes and to provide the basis for improving parameterizations. I believe that much progress has been and is being made (and if this is not the case, that is if all this process research is not improving on the simple parameterizations that have been around for 15 years we really need to rethink our research philosophy). . . . The [model] results will be much better received if the most advanced and best tested (not necessarily the most complex) parameterizations are included; clearly there will need to be tradeoffs when very significant increases in computer resources gain virtually no advance in accuracy, but we should not easily give up on requiring accuracy—if modelers are expected to produce more and more accurate results, then we need to demand the resources needed to produce such results. Thus I feel that resources must be insufficient when your simulation is using bucket hydrology, convective adjustment, average diurnal radiation, etc.[1] Further, I do not see the logic of simplifying parameterizations that are working well just because other parameterizations are not working. (Scientist B, letter, September 17, 1993)

In B's account, promotion of more complex parameterizations becomes entwined with the institutional objectives of the USGCRP and its stated ambition of improving parameterizations in climate models (see, e.g., National Science and Technology Council, 1994). B argues that some parameterizations must become more physically realistic to improve accuracy, even if that improvement is incremental and piecemeal, and involves revisiting other parts of the model. While not explicitly disagreeing with B's comments above, C continues to express a preference for relatively simple parameterizations.

> It is very important to quantitatively calibrate a parameterization by use of large scale observation (i.e. satellite observation of radiance, runoff from a major river basin, etc.). Because it is very difficult to uniquely determine a large number of parameters from relatively few variables available from large scale observation, a parameterization should be as simple as possible. . . . A prediction of future climate, which is not based upon satisfactory understanding of model behavior, is indistinguishable from the prediction of a fortune teller. . . . It is not obvious that the use of an improved model automatically reduces uncertainty. (Scientist C, letter, October 18, 1993)

Here C is alluding to the danger that parameterizations will become more complex than is warranted by the data or theory available for confirmation.

On the desirability of a high model resolution, B and C are in closer agreement. In particular, both say that higher resolution improves the accuracy with which the hydrodynamic equations (based on classical physical theory) are computed (letter from C, September 23, 1997). Yet B's requirements vis-à-vis the model resolution are somewhat more stringent than C's. B maintains that climate models used in making predictions should resolve the major dynamic features of the circulation in the atmosphere and ocean:

> Even for exploratory runs, I am not at all comfortable that using coarser resolutions will produce meaningful results (as, for example, in an experiment coupled with a dynamic ocean) in that the major atmospheric circulation systems that drive the ocean are not well located or of proper magnitude. . . . [A] resolution of 3 degrees [in the ocean model] may be adequate if all one wants is the global meridional [equator-to-pole] heat transport . . . but if one wants to represent the largest contributor to ocean variability (at least on interannual scales), namely the ENSO [El Niño Southern Oscillation[2]] cycle, it seems clear that resolution must be less than a degree. In addition, with bottom water formation apparently a small scale phenomenon, it would appear that fine resolution is likely needed to represent processes that may play a role in abrupt climate change.[3] In that your study was looking at variability, the 4 degree resolution just seems to be an unsatisfactory simplification. (Scientist B, letter, September 17, 1993)

It is clear that B wants C's team to be going full-steam ahead on "improving" their model: making the parameterizations more complex, increasing model resolution to improve the representation of dynamics, including new suites of diagnostic variables, and so on. Hence, B wrote: "that [your center] (like some of the other centers) has to make a choice between making simulations and improving the model aggressively is unfortunate."

C proposes a somewhat different "triad" strategy for model improvements, consisting of

1. Reliable monitoring of thermal forcing (i.e., greenhouse gases, aerosols, solar irradiance, and so on) and basic variables of climate (including temperature and salinity in oceans)
2. Prediction of climate and climate change by models of various complexities
3. Comparison of predicted and actual climate change (including exploring the natural variability to detect a signal from the noise)

C notes that:

> Although it may take a very long time, I believe this may be the only way to slowly but steadily reduce uncertainty in our climate prediction. It is, therefore, very urgent to develop a comprehensive plan for the reliable, long-term monitoring of climate with sufficient accuracy and to implement it as soon as possible. Without such monitoring, it would be impossible to demonstrate the credibility of climate models and prediction of future climate. (Scientist C, letter, October 18, 1993)

This plea for appropriate monitoring should be read not so much as a comment on the USGCRP as on NASA's Earth Observation System (EOS), which many climate modelers have felt to be inadequate in terms of providing relevant, long-term data on climate forcings and feedbacks (Hansen, Rossow, and Fung 1993).

Epistemic Lifestyles

In the rest of this chapter, I explore different ways of being a climate modeler, or different epistemic lifestyles. By *epistemic lifestyle* I mean the set of intellectual questions and problems, and the accompanying set of practices, that provide a sense of purpose, achievement, and ambition to a scientist's work life, as well as the more mundane sense of carrying out those activities necessary to "getting the job done."[4] Additionally, an

epistemic lifestyle includes the social networks and connections through which scientists organize their individual and collective work. We can ask, for example, to what extent a scientist is within a well-bounded organization and a highly structured social grouping (see Douglas 1986).

The factors that give rise to different sorts of epistemic lifestyles include many of those identified in sociology of science: disciplinary concerns and practices; institutional culture, structures, and processes; policy and "user" relationships and support; funding sources; peer-group concerns; career trajectories; and so on. The word *lifestyle* indicates the immersion of scientists in an intellectual-organizational setting and trajectory that is only partly consciously chosen and not easily exchanged for something different. Such lifestyles are, of course, only "ideal types," evident to different degrees in any individual modeler or organization.

Climate Seers and Climate Model Constructors

What are the principal motivations and knowledge ambitions of climate change modelers? We can arguably distinguish between the following two groups of modelers:

- Those conducting model-based experiments to understand and explore the climate system, with particular emphasis on its sensitivity to changing variables and processes, especially increasing greenhouse gas concentrations. Such scientists can be termed *climate seers.*
- Those developing models that aim to capture the full complexity of the climate system, and that can then be used for various applications. Such scientists can be termed *climate model constructors.*

The climate seers are most clearly identified with model studies of human-induced climate change, though their interest is more widely on the functioning of the forcings and feedbacks within the climate system and their sensitivity to change (whether induced by CO_2, sulfate aerosols, changes in solar energy input, the effects of a nuclear war, deforestation, desertification, or other factors). The climate seers use climate models because they are regarded as the most appropriate and useful tools for exploring sensitivity to change, including for producing projections of future climate change, and for answering the more fundamental scientific questions about how greenhouse gases (GHGs) change climate processes and with what effects. The seers are usually driven by a desire to understand the scientific processes and feedbacks that will occur in a climate system due to changes in climate "forcing," and to predict the effects of a given change in forcing—that is, something similar to C's "predictive understanding."

The model constructors are much more interested in advancing the actual climate models "for their own sake." The models are an important end in themselves, not merely a means to answer a prior scientific question. The model constructors see their main task as building better climate models of the atmosphere, ocean, sea-ice, and land surface. This, for them, involves higher resolution; better, more physically based, and hence (it is thought) more "realistic" parameterizations; performing better diagnosis; and so on. The assumption behind such development work is that a better simulation of the *current* climate will result. The models can then be coupled to one another without the need for correction factors or excessive tuning (regarded as "fudges"). The model constructors' assumption is that a "best possible" simulation of reality is feasible and desirable. The model that produces it can then be used for a whole range of applications, whether it be to study atmospheric chemistry, climate interactions, land-surface effects, paleoclimatology, or the enhanced greenhouse effect.

Herein resides an important distinction between the seers and developers. For the seer, *which model is "state of the art" depends on the model's intended application* (as C expressed it above). For the model constructor, by contrast, *a single state-of-the-art model exists irrespective of its application.* It is defined as one whose control simulation is close to the observed climate, that uses the most up-to-date observations as inputs and for verification, and that contains the most detailed, most physically realistic parameterizations (sentiments expressed by Scientist B).

Seers, Constructors, and Parameterizations

The difference between seers and constructors is illustrated well by how each group views parameterizations. As we saw above, C, who approaches the ideal-type climate seer, views parameterizations not as realistic representations of natural processes, but rather as means to create models that are effective for answering questions about (human-induced) climate change. This is achieved through tuning the model in accord with the modeler's own judgment of what is a good climate simulation, while keeping the parameterization sufficiently simple that its influence in generating the representation of physical processes, and the response of the model to perturbation, can be understood. Climate seers do not negate the importance of better parameterizations, but rather see little room for improving on them in the medium-term future. For a model constructor, on the other hand, parameterizations should attempt to represent real physical processes as closely as possible. Hence the relatively simple parameterizations developed in the 1960s (convective adjustment, bucket hydrology, and so on) are viewed as inadequate for the contemporary task of building better models. In summary, while for climate model constructors the sole purpose of parameterizations is the representation of physical referents, for climate seers physical referents are only one consideration to take into account alongside other factors related to the model's intended application (tractability, computer power requirement, interpretability, and so forth).

The representation of clouds provides a good example of the difference between these contrasting approaches. The simplest parameterization for clouds entails prescribing the amount of cloud, zonally or globally, according to climatological means. More complicated schemes involve predicting the amount of cloud based on other model-resolved variables. In many early models, for example, whenever the relative humidity in a grid box reached a given quantity (e.g., 95 or 99 percent), clouds immediately formed within that box. In many respects this parameterization is unrealistic. For example, clouds can form at much lower relative humidities than 95 percent, even down to 60 percent in the case of cirrus clouds. Climate seers at GFDL therefore began to use a somewhat more complicated cloud parameterization that takes account of the change of relative humidity with height. Model constructors, however, also wish to represent the influence of different cloud types and their highly complex radiative and microphysical properties; their cloud parameterizations can be quite elaborate (McGuffie and Henderson-Sellers 1997; Rasch and Williamson 1990).

All climate modelers agree that the climate system is very sensitive to relatively minor changes in the radiative and microphysical properties of clouds (such as changes in the ratio of liquid water to ice). But climate seers have been skeptical of parameterizations that aim to predict cloud optical and microphysical properties, because of the difficulty of confirming and evaluating such formulations. Even one of the groups that developed more complex formulations warns that "although the revised cloud scheme is more detailed, it is not necessarily more accurate than the less sophisticated scheme" (Mitchell, Senior, and Ingram 1989, 132). Some climate seers have expressed a concern that complex, but unverifiable, physical parameterizations of clouds are in danger of confusing analysis and understanding of model behavior. "Solving" the problem of clouds is seen by them as a task that will take several decades (interview, April 25, 1994). One seer even likened attempts to represent the physics of clouds in climate models to Don Quixote tilting at windmills!

The dilemma of the climate-model construction approach is that complexity does not increase in neat increments. One scientist expressed this as follows during an interview:

> You go through at least two or three stages of model development. One is where you do produce something very simple and that just has one or two constants which you set and you're done. And then you come back and you can put in something much more complicated, a lot more physical basis to it, but it's now got 27 different numbers you've got to set. And you don't know how to set them, because those numbers depend on more things you haven't done yet. So you've just got to set them somehow. At the next stage you push the problem back a level because now you're modeling the things the numbers are based on. So now you take out the bunch of arbitrary numbers in the first parameterization, but you have arbitrary numbers in what you just put in. So there's a stage where you go from something simple, where you only have one or two numbers that you pick, to something that is complicated, with a bunch of arbitrary numbers in it, and then you go beyond that to predicting what you need in order not to have a bunch of arbitrary numbers any more. . . . At this point . . . we're in that second stage of adding a bunch of arbitrary numbers. (Scientist D, interview, April 15, 1994)

Increased complexity frequently has the counterintuitive effect of reducing the quality of the control climate simulation. This is because the model's parameterizations have been tuned *to each other*, so that if one is changed, the other parameterizations are no longer "in sync" and the quality of the simulation suffers. The solution in the model-construction approach is to open up the other parameterizations and to

improve them by making them more physically realistic as well. The belief is that a better representation of reality in all major parameterizations will eventually come to simulate the real processes of the climate system.

Climate seers tend to see such model developments as a luxury they can ill afford. One compared the different purposes of models to the different purposes of cars in Formula One racing and everyday driving (interview, April 27, 1995). Formula One vehicles have very high performance capabilities but are notoriously unreliable. Like the climate model constructor's ideal model, they have state-of-the-art technologies and incorporate the latest insights, but for routine everyday use they are not so reliable. By contrast, climate models used in CO_2 perturbation experiments need to be reliable enough to operate for very long time periods. For example, some long simulations take many months to compute; hence climate seers have to be reasonably confident that their models are not going to "crash" and can be easily fixed if necessary. What is then required is more akin to the family car, which starts every day on turning the ignition switch and can be easily and cheaply maintained and repaired.

For similar reasons, the climate seers also perceive model development quite differently from the model constructors. Seers tend to be more cautious about changing a model that "works" and is reliable. An incremental reductionist strategy is adopted in changing the model, with the influence of each model component analyzed separately. Only after the model is well understood is it appropriate (in the climate seers' opinion) to add complexity. Additional elements of complexity are then added one at a time, and their implications for the rest of the model are analyzed. This is a much less open-ended and more time-consuming strategy (relative to how much the model changes) than the many-pronged approach of model constructors. Using a simpler model also means that it is possible to conduct multiple runs for the purpose of model testing and diagnosis. Furthermore, climate seers argue that there is little point in introducing a level of complexity in one model component that is negated by simplicity elsewhere in the model.

An example of the latter condition is the parameterization of convection (the movement upward of warm air masses). Climate model constructors are critical of simple convection schemes (such as the moist convective adjustment scheme in the GFDL model) and have developed more physically based approaches. However, these new parameterizations were initially used in models without a diurnal cycle of radiation. The influence of day-night heating on convection far exceeds that arising from the difference between moist convective adjustment and the more complex schemes. The value of the added complexity in a model without diurnal heating was questioned by climate seers (interview, April 27, 1994). The model constructors would instead perceive the case for including diurnal heating in the model to be strengthened by the adoption of the more complex parameterization of convection.

The Heat and the Wind: Thermodynamicists and Dynamicists

Climate is commonly understood by scientists as the product of the interaction of thermodynamics (the movement of heat) and dynamics (the atmospheric and oceanic circulation). Yet some climate modelers appear to give more emphasis to thermodynamics, while others give more emphasis to dynamics.[5] According to the thermodynamically oriented, the climate system is driven primarily by changes in heat fluxes—such as those caused by increased concentrations of greenhouse gases—and associated thermodynamic feedbacks: the ice-albedo feedback, cloud changes (influencing the amount of incident or reflected radiation, and so on) and water vapor feedback.[6] As Scientist E put it: "To a first order, if you think of the whole world as a box—how much heat goes in and out of the box—that [dynamical] stuff doesn't matter" (interview, April 25, 1994). E went on to explain:

> *Scientist E*: My experience with heat has been that if you get the global mean right, you get the rest of the places right too, more or less, as long as you have sea-ice at about the right places.
>
> *Interviewer*: I was talking to Peter Stone at MIT about some of the analyses they've been doing in which they inferred the heat fluxes in the ocean [from AGCMs], and they seem to think that they're often quite in error, with the ocean not transporting enough [heat].
>
> *Scientist E*: But the atmosphere will adjust to it pretty fast. You might not get the split right between the atmosphere and the ocean. But if your polar-equator temperature gradient gets a bit too strong, the winds will blow a bit harder and the atmosphere will transport more heat.

> *Interviewer*: But wouldn't that change the sensitivity of the system to perturbation?
>
> *Scientist E*: It's conceivable.... My view of the world is much more thermodynamic. It's much more important to get the sea-ice edge in the right place, and to have about the right temperature distribution, than it is to worry about what the winds are. It's just my bias. As long as the circulation looks something like the observed ... I forget it.
>
> If you think the transports are what makes everything go then you have to worry about model resolution, because the higher your resolution, the better the transports you're getting because you're doing all the finite differencing better and all that kind of stuff. And if you're a thermodynamicist you worry about where the sea-ice edge is, what your cloud albedos are, stuff like that.
>
> ... The dynamicists are more mathematical and therefore more rigorous in their proofs of things, and therefore more theoretical. The thermodynamicists tend to think more physically based, and [their] arguments are all physical. They don't use equations—they wave their hands a lot more when they talk. (Scientist E, interview, April 25, 1994)

E described how his model transferred a heat imbalance at the top of the atmosphere almost exactly to a heat imbalance at the interface between the atmosphere and ocean. The dynamical properties of the atmosphere between the ocean-atmosphere interface and the top of the atmosphere do not appear to change the heat balance, providing supporting evidence that the system can be modeled, to a first approximation, thermodynamically.

To date, those with a thermodynamic bias have dominated model experimentation on anthropogenic climate change. Dynamicists, however, are still significant to the evaluation and reception of such research because they form a more or less skeptical hinterland of climate modeling. This situation is likely to have influenced many political, media, and policy discussions about the science of climate change. Informally, dynamicists are sometimes critical of the existing models of the leading thermodynamicists, much in the vein of Scientist B.[7]

Dynamicists frequently have a background in numerical weather prediction (NWP) and university-based meteorological research. They are more concerned about the existence of model errors in the control run than are thermodynamicists. Provided they are not large, such errors are not thought by thermodynamicists to influence model response to CO_2 doubling—that is, they believe the difference between the control and perturbation simulations to be acceptably similar to the (unknown) difference between unperturbed and perturbed conditions in the real climate. This is a classic thermodynamic-type assumption, since it treats the climate system as one in which the quotient of the inputs and outputs remains (within limits) more or less constant. By contrast, the dynamicists are more inclined to regard errors in the control run as influencing the simulation's response to a perturbation (such as increasing greenhouse gases) in unrealistic ways. Hence they see the reduction of errors in the control simulation as critical, while thermodynamicists see diminishing returns for such effort.

A good example of a typical dynamicist concern is that to be adequate for climate change experiments AOGCMs should be able to simulate ENSO. Some dynamicists have argued that anthropogenic climate change will have some of its most significant effects through natural modes of variability, such as ENSO; hence model simulation of such processes is critical to understanding the broad-brush pattern of climate change (Palmer 1993).

The categories of "thermodynamicist bias" and "dynamicist bias" should be seen as ideal types. The issue is not whether the climate system is *either* thermodynamic or dynamic, since a key rationale of GCMs is, after all, to represent the interaction of these. Instead, the issue is the relative weight accorded to thermodynamic or dynamic considerations in models for studying anthropogenic climate change.[8] E is closer to the thermodynamicist "ideal type" than most modelers, while F expresses a more common balance between thermodynamics and dynamics in the following interview quotation:

> A lot of the short-term variability in the atmosphere is . . . going to be governed by the ability to represent the atmospheric dynamics, whereas some of the long-term changes like response to CO_2—the overall level of warming—is much more sensitive to the model physics. . . . One of the arguments goes that one of the largest aspects of interannual variability is El Niño; therefore, a change in frequency of El Niño would be a substantial climate change. I think there's some truth in that but I wouldn't want to push it too far. But I would certainly like to be using a model that makes some attempt to produce oscillations that are like El Niño. But I think one can learn a lot even though the current climate model doesn't really simulate El Niño. (Scientist F, interview, March 11, 1993)

This quotation illustrates how scientific perceptions of the appropriate balance of thermodynamics and dynamics depend on the specific research questions posed and the time and spatial scales thereby implicated. Dynamics become more important for climate seers when the research explores short-term variability and regional climate changes.

Most climate seers to date have adopted a balance of thermodynamics and dynamics similar to that of Scientist F. This partly reflects the dominance of research questions devoted to the long-term effects of perturbation by CO_2 at large spatial scales; a quantitative description of the climatic effects of increasing greenhouse gas emissions has (to date) required a thermodynamically biased approach. Thus the commitment to a thermodynamics bias *is partly a function of the objectives and key research priorities and questions of the research* (i.e., a function of being a climate seer). adoption of a thermodynamically oriented approach will have some bearing on what research questions and objectives are thought worth posing and feasible to approach. The research objectives and questions and the adoption of a thermodynamic bias come to mutually reinforce one another (although not irrevocably).

Model constructors appear to be more eclectic in adopting both thermodynamic and dynamic approaches. Perhaps this reflects their desire to build the "perfect" climate model (which would incorporate both these dimensions). However, it does appear that the constructors may orient themselves relatively more toward dynamics, perhaps because so much work has already been devoted to thermodynamics, which is therefore better incorporated into existing climate models. Some of the "big" remaining problems in anthropogenic climate change research, such as developing credible regional climate change scenarios, seem to require much work on representing dynamics. On these issues, the climate seers have made less progress.

Epistemic Lifestyles in Three Climate Modeling Centers

So far I have tried to account for empirically defined ways of being a climate modeler in terms of differences in research objectives, methods, assumptions, and experiences, which (taken together) I have called *epistemic lifestyles*. The following discussion explores these distinctions in three major climate modeling centers that were among the principal groups studying anthropogenic climate change as of the early- to mid-1990s: NCAR, GFDL, and the Hadley Centre (HC) of the UK Meteorological Office (UKMO).

All three centers are large, resource-rich organizations, running complex coupled AOGCMs in transient forcing experiments (i.e., simulations in which CO_2 concentration is increased by an increment each model year to the point of doubling and beyond). However, each center is involved in such research to a different degree.

GFDL, where model constructors are least in evidence, has had a major group devoted to the anthropogenic climate change issue since the 1960s. Such simulations have also been the *raison d'être* of the Hadley Centre since its establishment in 1990 (from a group working at the UKMO since the mid-1970s). However, HC devotes considerable efforts to model development and has many model constructors, who coexist relatively harmoniously with climate seers. At NCAR only a small team (three or four scientists) works in the climate seer mode. NCAR's prime organizational mission is to serve the U.S. academic climate research community through provision, development, and testing of the Community Climate Model (CCM). A large group of scientists at NCAR is devoted to the CCM. Their work includes helping university academics to use the model for basic understanding of climate and to answer more academic" questions, such as studies of paleoclimates, vegetation-climate interaction, and specific parameterizations. This group is much less concerned with immediate policy-relevant questions, such as CO_2 doubling (global warming) experiments. This gives the model constructors at NCAR a strong position within the organization, while they are least dominant at GFDL. Divisions between seers and constructors are more evident in the U.S. centers, where it is not uncommon for criticism and disagreement to bubble up in informal discussions.

U.S. science, it has been noted, tends to be more contentious and fragmented than European science, whose disputes are less evident and possibly hidden from view (Jasanoff 1986). Difference is also more likely to emerge in the United States because of the multiplicity of centers and funding sources. While NCAR is funded by the National Science Foundation, and by a series of "soft" funders, including the U.S. Department of Energy (USDOE), GFDL is part of the National Oceanic and Atmospheric Administration (NOAA) within the U.S. Department of Commerce. Two other major climate change modeling centers in the United States are the Goddard Institute for Space Studies (GISS), part of the National Aeronautics and Space Administration (NASA), and a group at the Lawrence Livermore National Laboratory (LLNL), a USDOE-funded national laboratory whose work was formerly dominated by nuclear weapons design. But there are also many centers and departments in universities that work on climate change using models (U.S. Global Change Research Program 1994, 1995). Hence there are four major climate modeling centers and a host of smaller university departments, all feeding climate modeling results and insights into different parts of the U.S. government and into Congressional hearings. It is perhaps not so surprising that differences in objectives and scientific styles are accentuated in such a system, a phenomenon also observed in the energy modeling field in the United States during the 1970s and 1980s (Baumgartner and Midttun 1987). The pluralistic funding and policy advisory process in the United States encourages competition between centers, reflecting competition between different offices within government. Research organizations are motivated by this diversity (and by peer pressure) to carve out their own "niche" in the climate change modeling field, rather than to compete "head on." Differences in research style are thereby accentuated and embodied in organizations.

GFDL, with its very long control and perturbation simulations, is especially strong in the climate seer role (see the many papers by Manabe and his collaborators, e.g., Manabe 1997; Manabe, Stouffer, and Spelman 1994). NCAR is especially strong in climate model construction and dynamics. The climate seers at NCAR are a distinct group within the organization and have tended to concentrate on more dynamical aspects of coupled models, such as ENSO and the heat-transport effects of a high ocean model resolution (see papers by Washington and Meehl, e.g., Washington 1992; Meehl 1990). GISS, meanwhile, is especially strong in producing new policy-relevant climate model formulations and simulations and in communicating new findings effectively to the policy community and beyond. GISS's rapidly produced prediction of the climatic effects of the eruption of Mount Pinatubo is a good example of such a role (Hansen et al. 1992), while its climate sensitivity studies have demonstrated the strong but indirect effect of particulate aerosols on climate, and the need for new long-term monitoring of climate forcings and feedbacks (Hansen, Rossow, and Fung 1993; Hansen et al. 1997). LLNL has concentrated on the development of new computational techniques, such as massively parallel processing, and on interactions between atmospheric chemistry and dynamics.

The HC is quite different in that it alone constitutes the focus of climate change modeling in the United Kingdom. Hence it is competing internationally but not intranationally. The HC is regarded as the main national source of scientific input on climate change issues not only to the wider scientific community, but also to all departments of government, to industry, and to NGOs. It enjoys a close, hands-on relationship with the Department of the Environment quite unlike what is found in the United States, or indeed in most European countries. Thus competition between centers results in differentiation and specialization in the United States, while the very *lack* of competition for funding and national recognition results in a more comprehensive approach to climate modeling in the United Kingdom. The close connection to government policymaking may also contribute to the cohabitation of epistemic lifestyles. Policy "needs," expressed through a single large government research contract placed at the HC, facilitate this "unified" approach to climate modeling. The contract requires the HC to provide state-of-the-art, authoritative climate projections for the IPCC and to feed into internal negotiations within government on the national position vis-à-vis the FCCC. Modeling to address policy-relevant questions thus becomes a priority for research.

In the mid-1990s the HC responded to policy-led questions about the ability of its AOGCM to reproduce the past century record of global temperature change. The policy mandate authorized research leaders at the HC to request help from climate model constructors in order to build better representations of relevant physical processes, especially of sulfate aerosols, which climate seers could then apply in model runs. The hierarchical culture of the UKMO and HC greatly facilitated the ability of

research leaders to implement a cohesive strategy, given that such leaders at the HC have tended to be climate seers and have exerted considerable control over the deployment of computer and personnel resources. By contrast, GFDL and NCAR are less hierarchically organized. They are characterized instead by distinct research "baronies," competing for credibility and resources.

The HC shares the so-called unified model with the weather-forecasting scientists at the UKMO. The "same" model is adapted and used by scientists with different sets of concerns and interests, and may consequently work as a "boundary object" (Star and Griesemer 1989) usable in exchanges among several different epistemic lifestyles. This contrasts with the situation at NCAR, where one finds separate modeling groups, each with a different funding stream, and a different modeling approach or style, each in some degree of competition with the others. It also differs from GFDL, where a distinct climate seer GCM group coexists with a range of other process specialists and modeling efforts. The climate seers have established a strong, distinctive approach at GFDL and they tend to be unwilling to accommodate the agendas and priorities of model constructors.

The unified-model approach of the HC is perhaps only possible in a cultural context where more hierarchical control and a strongly "hands-on" policy direction of research planning are accepted. Under these conditions research is inevitably less driven by the concerns of distinct peer communities, whether they be seers, model constructors, dynamicists, process specialists, or oceanographers. Another consequence of the more centralized control of collective work is that individual creativity and initiative may be more inhibited and secondary to collective goals. However, the constructors and dynamicists at the HC have other important ways to pursue their interests and objectives. For example, wide-ranging collaborations exist with other centers and universities to develop the parameterizations or other aspects of the climate model. This work occurs simultaneously with, but rarely feeds directly into, the configuration of climate change perturbation experiments. At the HC, the constructors and dynamicists are also closely linked to weather forecasting (NWP) research. Indeed, parameterization specialists work on both the climate- and weather-forecasting versions of the "unified model"; they hold joint appointments in the HC and UKMO. In other words, constructors and seers are accommodated at the HC—despite the overriding influence of the latter—by networks of research and peer-group authority that extend beyond the HC into the high-status UKMO forecasting research (where model construction and dynamics are especially valued) as well as into other universities and research centers.

The differences between the HC and the two American centers lead me to propose a third epistemic lifestyle—the *hybrid climate modeling policy style*—in which the policy-influenced objectives and priorities of the research organization, as defined by its leadership, take precedence over other individual or organizational motivations and styles. Such objectives and work program are decided in negotiations between senior climate researchers and a coterie of policymakers in government. The available resources are then deployed by the organization as a whole in pursuit of those objectives, largely regardless of whether they are more thermodynamic, dynamic, model construction related, and so on. While the other two lifestyles also imply a degree of collective work, this third lifestyle is distinct in the strong boundaries around, and the differentiating roles and status within, the organization. The quid pro quo for consensus surrounding the style is a double identity for some researchers: part and parcel of the collective effort, but also individuals with independent interests, objectives, and research collaborations.

Intraorganizational negotiations are also important in the U.S. centers, of course. There, however, policymakers rarely have the direct influence over decision making about research priorities observed at the HC. NCAR's orientation toward dynamics and model development created a general context in which the climate seers were put somewhat on the defensive. And despite the polyarchy of research baronies at NCAR, internal peer-community pressure toward dynamically oriented model construction appears to have been rather strong. In addition (or as a consequence?), the climate seers at NCAR adopted a management style of reasonable tolerance, leading to negotiation and mediation with the other approaches (on decision styles, see Downey 1992). This intraorganizational context may help account for the rather more "conservative" approach of the climate seers at NCAR, compared with those at other centers, with respect to shortcuts, approximations, and correction factors.[9]

Concluding Comments

I have described three empirically identified differences in ways of doing climate modeling, or *epistemic lifestyles*: climate seers, climate model constructors, and a hybrid climate modeling policy style. The range of factors influencing which epistemic lifestyle has been adopted in different modeling centers included the following:

- Disciplinary/research experience background, especially the historical success or otherwise of thermodynamically or dynamically biased research approaches
- Organizational location, objectives, main funders, main users and customers
- The role of academic collaborators and users of models (e.g., in the case of NCAR, oriented toward model construction and dynamics)
- The role of policymakers in negotiations over research priorities and directions (e.g., at the HC)
- The role of organizational culture, especially along the continuum between centralized research direction and separate research "baronies"
- The opportunities for different epistemic styles to treat the climate model as a boundary object and to develop coexisting alternative axes of interaction and authority (e.g., to NWP research in the case of the HC)
- The role of different national cultures of research, especially the degree to which research becomes fragmented and specialized, or cohesive and comprehensive, because of funding arrangements and connections to policy institutions

These distinctions (among others) provide some insight into the range of different opinions, practices, and priorities within the climate-modeling community. But what of the wider relevance of the existence of different epistemic styles? Do the arguments here shed any light on the political debate over the role of climate change science? A key aspect of that debate, as discussed in this book at several places (see especially chapters 1, 7, and 8), is the crucial role of a high degree of scientific consensus in constituting and reinforcing a set of political and social beliefs concerning the desirability of reducing greenhouse gas emissions. Given that GCMs are, arguably, the major pillar upholding the powerful scientific consensus, any difference of opinion within the scientific community concerning the status and validity of GCMs will have a wider political significance.

Political factions opposed to, or simply wary of, calls for action on greenhouse gas emission reductions will find solace and intellectual support in the critical hinterland of GCM modeling populated by the climate model constructors. The appeal is simple enough to understand: the GCMs typically used by climate seers are perceived by climate model constructors as rather crude tools for simulating the climate system. Can those "crude tools" really be trusted to generate reliable scientific knowledge of climate change and the role of anthropogenic influences? The climate model constructors (knowingly or not) provide much critical ammunition from a powerful "insider" position of knowledge and authority, on which the contrarians draw in challenging the cognitive authority of GCMs.

One wonders, however, whether those "crude tools" will ever become sophisticated enough to win over the contrarians. Are the contrarians assuming that scientific certainty over the question of anthropogenic climate change can be settled once and for all by a statistical level of proof at the 95 or 99 percent confidence level? This chapter has not set out to answer those questions, but the existence of epistemic lifestyles suggests that we should not expect definitive levels of scientific certainty and consensus at such statistical levels to emerge. We may instead have to come to see scientific uncertainty as a multifaceted, multidimensional concept. Statistical levels of certainty may be appropriate under certain assumptions, beliefs, and institutions but not under others associated with a different epistemic lifestyle.

The epistemology of science and the sociological analysis of science in practice illuminates the interweaving of academic disciplines, organizational and institutional objectives and trajectories, policy influences, and the epistemic "spaces" that emerge in our intellectual understanding of the climate system. Epistemic lifestyles is one way of indicating the discrete packages of viable scientific endeavor that emerge from this interweaving. Scientific certainty and consensus cannot simply sit uncontested above epistemic lifestyles, but have to be produced by, and in a negotiation between, those epistemic practices.

The existence of epistemic lifestyles suggests that the climate model constructors' reservations about current GCMs used in studies of anthropogenic climate change are not simply grist for the mill of the contrarian cause. While questions about the GCMs used by climate seers are at the fore in the concerns and motivations of the climate model constructors, this is not the only appropriate level at which to explore the beliefs of climate model constructors vis-à-vis anthropogenic climate change. Rather, a "higher-level" negotiation between the climate model constructors and climate seers appears to find more agreement on the likely role of human-induced greenhouse gas forcing of the climate system than the apparent criticism of current GCMs would suggest.

More research is required to understand the character of negotiations between epistemic lifestyles. A practical message for the climate-modeling community is that mechanisms for promoting negotiation between scientific viewpoints, based on a fuller understanding of where those positions come from, would help to present a coherent position on key scientific and policy questions, which nevertheless acknowledges the vital and necessary role of diversity in the practice of climate science.

Notes

An essay like this is really the joint product of a sociologist and the many climate scientists and other commentators and colleagues who have generously provided assistance. In particular, Ron Stouffer first suggested the distinction between dynamics and thermodynamics, while Syukuro Manabe and Michael MacCracken shared their correspondence with me, gave me permission to use the material, and provided me with feedback on the first draft. James Risbey and Peter Stone also helped in developing some of the ideas here in the context of a detailed study of flux adjustments in coupled climate models (Shackley et al. 1999). Paul Edwards has provided very supportive editorial guidance and feedback. I am also very grateful to the following scientists: David Bennetts, Byron Boville, Kirk Bryan, Ulrich Cubasch, Klaus Hasselman, James Hurrell, Jerry

Mahlman, Gerald Meehl, John Mitchell, Tim Palmer, Philip Rasch, Michael Schlesinger, Joseph Smagorinsky, Warren Washington, and Richard Wetherald. It goes without saying that none of the above bear any responsibility for my interpretations of what they told me. Thanks go to the Centre for the Study of Environmental Change at Lancaster University, to the United Kingdom's Economic and Social Research Council, and to DG XII of the European Commission for providing me with the opportunity to conduct this research. Travel was assisted by grants from the Royal Society (London) and the National Center for Atmospheric Research, and these are much appreciated.

1. *Bucket hydrology* is a simple way of representing the movement of water from the land surface (McGuffie and Henderson-Sellers 1997, 168). *Convective adjustment* is a way of representing the vertical heating in the atmosphere due to the transfer of energy by moist and dry convection; it does not aim to represent realistically the physical process of convection (McGuffie and Henderson-Sellers 1997, 111–113).

2. The *ENSO* is a large-scale fluctuation in ocean and atmospheric dynamics in the southern Pacific, extending also to the Indian Ocean and the mid-Atlantic. An El Niño–Southern Oscillation event starts when the usually cold water off the Pacific coast of South America becomes warm, indicative of a massive change in the direction of trade winds and ocean currents. ENSO creates large changes in precipitation patterns, causing flooding in some places and droughts in others. The global scale and significance of ENSO has only been realized in the past fifteen years or so. For a good discussion, see Glantz 1996.

3. *Bottom-water formation* refers to the importance of the "downwelling" at a few ocean locations of cold and/or dense saline water. In the northeastern Atlantic, for example, the warm saline waters from the equatorial Atlantic cool rapidly and sink. Such downwelling is an important component of the *thermohaline circulation*, a massive ocean-circulation pattern essential to the temperate climate of northwestern Europe. Scientist B is referring to the possible sensitivity of this circulation to the precise position at which deep water forms and, in its extent, to the precise locational input of freshwater in the North Atlantic region (see, e.g., Rahmstorf 1997).

4. The idea of "styles" of scientific thought and practice is well established in the sociology of science. See for example Fleck [1935] 1979; Rudwick 1982; Maienschein 1991: Knorr-Cetina 1991; Hacking 1992.

5. I owe this distinction between the thermodynamic and dynamic style to discussions with Ron Stouffer.

6. The ice-albedo feedback occurs when warming melts ice deposits, rendering the surface darker and therefore less reflective to solar radiation, so promoting further heating. Warming also increases the water content of the air, so promoting further heating through the potent greenhouse gas properties of water vapor.

7. Examples of academic and semipopular literature from dynamicists that promotes a more circumspect view of climate models includes Pielke 1991, 1994; Palmer 1993; Stone 1992.

8. One distinguished climate modeler, commenting on a draft of this chapter, argued that it is impossible for a climate scientist to know whether dynamics or thermodynamics is more important because climate "is sustained through close interaction of these two types of processes." Hence, to distinguish climate modelers in this way is "highly misleading" (letter, September 23, 1997). In response, I have downplayed the distinction, but there still seems to me, and to other climate modelers (see note 6 and the *climate modeling pyramid* of McGuffie and Henderson-Sellers (1997, 44)), something of value here as a subsidiary, lesser distinction to the climate seer/model constructor category.

9. For instance, NCAR climate seers—unlike their compatriots at the HC, GFDL, and Max Planck Institut für Meteorologie in Hamburg (MPI)—have not used flux correction in coupled models of the early 1990s (Shackley et al. 1999). Nor have they used so-called accelerated spin-up methods for getting ocean models into equilibrium, use of which has allowed GFDL and MPI to run very long control simulations. In both cases, the reasons given by NCAR climate seers are similar (if less pointed) to the criticisms of these techniques by dynamicists and model constructors (interview, April 11, 1994).