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ABSTRACT This paper discusses the distribution of certainty around General Circulation Models (GCMs) – computer models used to project possible global climatic changes due to human emissions of greenhouse gases. It examines the trope of distance underpinning Donald MacKenzie's concept of 'certainty trough', and calls for a more multi-dimensional and dynamic conceptualization of how uncertainty is distributed around technology. The certainty trough describes the level of certainty attached to particular technoscientific constructions as distance increases from the site of knowledge production, and proposes that producers of a given technology and its products are the best judges of their accuracy. Processes and dynamics associated with GCM modeling challenge the simplicity of the certainty trough diagram, mainly because of difficulties with distinguishing between knowledge producers and users, and because GCMs involve multiple sites of production. This case study also challenges the assumption that knowledge producers always are the best judges of the accuracy of their models. Drawing on participant observation and interviews with climate modelers and the atmospheric scientists with whom they interact, the study discusses how modelers, and to some extent knowledge producers in general, are sometimes less able than some users to identify shortcomings of their models.

Keywords atmospheric sciences, certainty trough, general circulation models, global climate change, simulation technology, uncertainty

Seductive Simulations?

Uncertainty Distribution Around Climate Models

Myanna Lahsen

'[D]istance from the cutting edge of science is the source of what certainty we have', Harry Collins (1985, quoted in MacKenzie, 1990: 370) has stated, arguing that there is an inverse relationship between the level of certainty attached to any particular scientific construction and proximity to its site of construction. Donald MacKenzie's (1990) analysis of antiballistic missile technology extends Collins' (1985: 145) slogan that 'distance lends enchantment' to technology. MacKenzie (1990: 370–71) proposes a distinction between two 'quite different' kinds of uncertainty about missile accuracy. The first kind of uncertainty is that of the 'alienated and those committed to an alternative weapon system'. The second, 'more private and more limited' form of uncertainty is that of 'those closest to the heart of the production of knowledge of accuracy' (p. 371). Knowledge producers themselves are 'certainly not critics' of their own technology, writes MacKenzie; they do believe that missile accuracy – the ability to render

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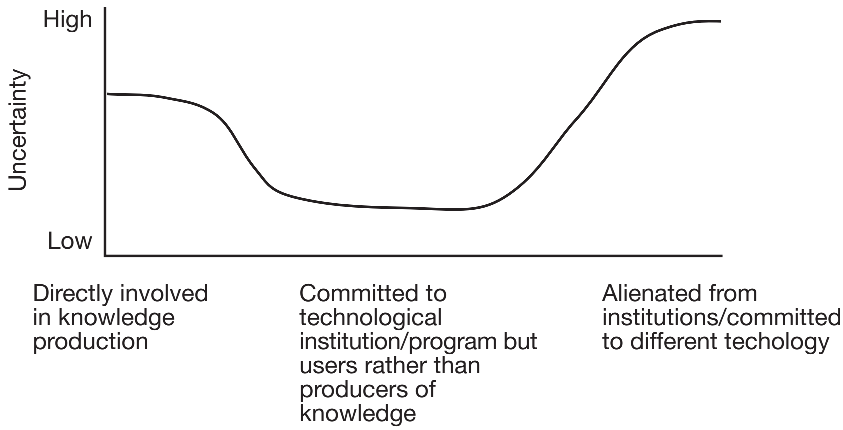
FIGURE 1
Mackenzie's certainty trough (MacKenzie, 1990: 372).



Source: MacKenzie, 1990. © MIT Press.

missiles accurate in their targeting of enemy sites – is in principle knowable. However, because of their intimate awareness of the 'human vagaries' in the production of the missile technology, they are not inclined to defend accuracy as fact (p. 366). Between these two positions are the 'program loyalists' and those who simply 'believe what the brochures tell them' (p. 371).

MacKenzie represents these divergent positions in what he refers to as a 'schematic and impressionistic' diagram in the shape of a trough (Figure 1). The attitude of 'program loyalists' is represented by a dip (thus producing a 'trough') in the connecting line representing the higher levels of uncertainty found among the two other groups, the knowledge producers and the alienated. MacKenzie (p. 371) suggests that this diagram '*might* describe ... the distribution of certainty about any established technology'.

After 40 years of development, computerized models of climate dynamics are an established – albeit always evolving – technology. Such models have become a cornerstone for the atmospheric sciences and for the bodies of evidence supporting claims about human-induced climate change. Although model developers and defenders do acknowledge many uncertainties, as MacKenzie's diagram would predict, I argue that the certainty trough only partly captures the distribution of uncertainty around climate models and their projections of future human-induced climate changes.

MacKenzie may have intended the certainty trough to serve as little more than a heuristic. However, the diagram deserves critical analysis because of the widespread influence of MacKenzie's work, and of the certainty trough in particular an analysis of the social dimensions of computer models (Shackley & Wynne, 1996; Jasanoff & Wynne, 1998; Smyth, 2001; Jasanoff, 2003).[1]

When applied to climate simulation technology, MacKenzie's framework does not capture the complex dynamics shaping when and how climate models are accepted, questioned, or rejected. In what follows, I identify the limited applicability, in this case, of the certainty trough and the trope of distance. I identify four interrelated factors that complicate the certainty trough: (1) General Circulation Models (GCMs) are produced at multiple sites dispersed in time and space; (2) GCM model developers are generally also users, and some other users also may be said to be knowledge producers; (3) identification of some model inaccuracies requires empirical understanding more prevalent among empiricists than modelers; and (4) social and psychological factors can reduce modelers' ability to retain critical distance from their own creations. The implication of (1), (2), and (3) is that there is no single vantage point from which to best evaluate the performance of a single complex GCM. The last point (4) highlights the need to develop the certainty trough and the concept of distance so that the model also accounts for the influence of professional investments, social networks, and the broader political controversy that surrounds the GCMs and the issue of climate change.

The paper draws from ethnographic fieldwork carried out over 6 years (1994–2000) among US climate scientists at institutions where climate modeling is carried out. The research analyzed the social, cultural, and political dimensions underpinning disputes among scientists regarding the present and future reality of human-induced climate change, and the severity of its impacts. The author was based at the US National Center for Atmospheric Research (NCAR) throughout this time, an institution that hosts numerous important modeling efforts, and traveled to other US modeling institutions around the country to study scientists there. The research involved participant observation supplemented by more than 100 semi-structured interviews with atmospheric scientists, about 15 of whom were climate modelers. Some of the latter were interviewed numerous times. Participant observation over the 6-year period involved conversations with many more modelers, and provided the opportunity to engage and listen in on their formal and informal conversations.

General Circulation Models

Computer simulations are central tools in areas of global change science where wholly empirical methods are infeasible (Edwards, 1996). They are part of a broader trend in science towards simulation technology that allows scientific investigation of, and experimentation with, complex systems without some of the time and access constraints of traditional experimentation. Scientists experiment with modeled earth systems in order to understand how human activities may alter the global climate and its associated natural and social processes.

GCMs are simulations used to model climate dynamics.[2] GCMs use computations to simulate complex interactions between the components of the earth system; time-dependent three-dimensional flows of mass, heat, and other fluid properties. The models divide the earth system into three-dimensional grids, mathematically representing the physical movement of gaseous or liquid masses and the transfer, reflection and absorption of energy. They compute these processes at each grid-point at appropriate time intervals and repeat and speed-up these processes to simulate future states of the climate system. The more complex GCMs include anthropogenic effects and couple oceanic, atmospheric and land-surface processes.

GCMs are based on the numerical weather prediction computer models that also provide the forecasts and visual displays commonly shown on television weather programs. Climate models can be programmed to provide similar visual displays of projected temperature changes and other climatological variables mapped onto geographical maps. For global GCMs, such maps represent the contours of land masses and oceans covering the entire globe. As in the case of television weather maps, the global climate projections frequently use colors to represent temperature differences, indicating warm temperatures with red and orange, and colder temperatures with shades of blue and green.

Climate modelers come from a variety of disciplines. The majority of them have degrees in atmospheric science, mathematics, and physics. Currently, two dozen or so scientific groups around the world use GCMs to consider the potential consequences of anthropogenic greenhouse gases in the atmosphere. The resources required to run the increasingly complex climate models are so extensive and expensive that relatively few countries and institutions can afford them. Most of these countries (for example, England, Germany, and Japan) have chosen to focus efforts on a single national model. The USA, however, has numerous modeling efforts, with no single national model. Consequently, model groups compete against each other for access to research funds from national agencies such as the US National Science Foundation, the Environmental Protection Agency, the National Oceanic and Atmospheric Administration, and the Department of Energy.

The Epistemology of Models

> The biggest problem with models is the fact that they are made by humans who tend to shape or use their models in ways that mirror their own notion of what a desirable outcome would be. (John Firor [1998], Senior Research Associate and former Director of NCAR, Boulder, CO, USA)

> In climate modeling, nearly everybody cheats a little. (Kerr, 1994)

Climate models are impressive scientific accomplishments with importance for science and policy-making. They also have important limitations and involve considerable uncertainties. The present discussion focuses on uncertainties about the realism of climate simulations – rather than the models' significant strengths – in order to highlight features of models that are overlooked when their output is taken at face value.

Climate simulations are based on the assumption that nature can be quantified and that it constitutes a sufficiently deterministic system that therefore, in principle, can be forecast far into the future (Shackley & Wynne, 1996). Whether or not long-term climate predictions are reliable is an open scientific question, however (Somerville, 1996). Some skeptics argue that indeterminacies perturb the climate system to the point of always frustrating attempts at long-term forecasting (Wiin Nielsen, 1987).

Modelers generally agree that the climate system is a chaotic system in both a technical and practical sense, rendering short-term weather patterns unpredictable beyond a few weeks. Nevertheless, they do believe that certain predictions can be made for the climate system, and that the models 'get the sign right', meaning that they are accurate in the overall effect they predict: that increased concentrations of greenhouse gases in the atmosphere will result in net warming rather than cooling. Asked about this, one climate modeler simply answered: 'when you heat a kettle it doesn't get cold'. By contrast, at least a few atmospheric scientists I interviewed thought it unlikely, but not impossible, that the models might be getting the sign wrong.

Paul Edwards (1999, 2001) has analyzed the blurred and elusive boundaries between models and observational data in global climate research. Edwards describes the uncertain relation between global climate simulations and the data they integrate and against which their accuracy is checked. Some of these uncertainties are quantifiable and manageable by means of empirical and computational improvements, while others represent 'unquantifiable, irreducible epistemological limits related to inductive reasoning and to the nature of model-based global science' (Edwards, 1999: 439).

To gauge the realism of long-term GCMs that project climate conditions many years into the future, scientists initially run the models for past periods, in order to compare the simulations against empirical data. This practice – often referred to as 'historical forecasting' – faces important challenges as well, due to the lack of independent, reliable, consistent, and global data against which to check the GCM output. In the absence of such data, modelers often gauge any given model's accuracy by comparing it with other models. However, the different models are generally based on the same equations and assumptions, so that agreement among them may indicate very little about their realism.

Model uncertainties are a function of a multiplicity of factors. Among the most important are limited availability and quality of empirical data and imperfect understanding of the processes being modeled. As a result of data limitations, GCMs are used in order to 'massage' the very data sets fed into the GCMs in the first place to render them consistent and broadly applicable (Edwards, 1999, 2001). Thus, the GCMs are used to fill in and smooth data sets derived from geographically dispersed measuring stations, to render them fit for the process of producing and validating

GCMs. This circularity characterizes processes of model production and validation, which ideally should be kept separate.

Unable to calculate atmospheric changes everywhere, because of limited observational data and computer power, GCMs break the atmosphere into a manageable number of blocks (grid boxes) and calculate relevant processes within each block. The region represented by each block ranges from 100 to 500 km^2 in GCMs, and commonly includes about 10 km of the atmosphere above Earth. Each block is partitioned into multiple vertical layers starting with the lowest 2 m of the atmosphere, at the earth's surface, and proceeding above the atmosphere where pressure approaches zero. In the process of dividing atmosphere and oceans into such large three-dimensional grid boxes, GCMs do not resolve climate processes and factors that are smaller than the grid's scale, even though these factors variously cool and warm the climate system. These unresolved factors include key hydrological processes, such as cloud formations and their movement in the atmosphere, as well as eddy currents, evaporation, and surface exchanges.

Instead of being empirically represented, sub-grid and insufficiently understood phenomena are 'parameterized'. Rather than actually incorporating such phenomena into the models based on physical measurements, modelers treat them indirectly by seeking to include their estimated climatic effects. A successful parameterization requires understanding of the phenomena being parameterized, but such understanding is often lacking. For example, the intricate microphysical processes that make up clouds are not well understood, nor is the overall climatic effect of clouds. Depending on their composition – which varies in time and space – the feedback processes created by clouds might trap heat around the earth or reflect radiation away from it, thus either warming or cooling the planet. How best to parameterize various processes is a contentious subject among modelers and model analysts.

When confronted with limited understanding of how the climate system works, modelers seek to make their models conform to how the climate system is expected to behave. The adjustments may 'save appearances' (Baker, 2000) without integrating precise understanding of the causal relationships the models are intended to simulate. For example, a particular tuning or tweaking technique is called 'flux adjustment' or 'flux correction'. It was used in the past for large coupled models at almost all the major international modeling centers. Flux correction has generally been considered a necessary adjustment to get model outputs to conform more closely with reality. In the coupled models (for example, ocean–atmosphere models), the ocean might tend to 'drift' away uncontrollably, a consequence of the linear rather than non-linear feedback structure of the model. In addition, large regions of modeled oceans have at times turned into solid ice.

Oreskes et al. (1994) have noted the difficulty of distinguishing what is and is not based on existent and validated knowledge in exploratory climate modeling. They write that when there is a lack of full access in time or space to the phenomena of interest,

> ... there are certain similarities between a work of fiction and a model: [J]ust as we may wonder how much the characters in a novel are drawn from real life and how much is artifice, we might ask the same of a model; How much is based on observation and measurement of accessible phenomena, how much is based on informed judgment, and how much is convenience? (Oreskes et al., 1994)

As is the case of scientific knowledge generally, models cannot be verified in the sense of having their truth status confirmed with certainty (Oreskes et al., 1994; Shackley & Wynne, 1995; Norton & Suppe, 2001).[3] Identifying model errors is particularly difficult in the case of simulations of complex and poorly understood systems such as the earth's atmospheric system, simulations that sometimes extend hundreds of years into the future. Whereas the accuracy of weather forecasts is established within days, climate forecasts may only be proven wrong in decades or centuries. Moreover, though computer resources have greatly expanded in the last few years, at the time of the research reported here, modelers lacked sufficient computer capacity and time to perform a large number of slightly varied runs of the same complex GCM model, which was required for the creation of error bars, and for testing, diagnosis, and documentation of model characteristics (National Research Council, 1998). For related reasons, computer models (model codes) are seldom subjected to peer review (Bankes, 1993) and large-scale model studies are never replicated in their entirety by other scientists, because this would require them to reimplement the identical conceptual models. Replication in science is generally difficult (Collins, 1985; Collins & Pinch, 1993), and in the field of climate modeling, the exact reproduction of a climate model outcome will never happen due to the 'internal model variability' that results in chaotic dynamic perturbations. The nearest climate models come to close scrutiny of their subcomponents is in the comparison of international peer-reviewed studies. This process is also the closest the models come to peer review. A kind of replication also occurs when model groups 'benchmark' a code to ensure near-exact (replicable) conditions. This practice is performed when model developers integrate code developed by other groups on other platforms (that is, other computers) with their own platform.

The Distribution of Uncertainty Around General Circulation Models

In his historical study of physics, Peter Galison observes that 'the computer began as a "tool", an object for the manipulation of machines, objects, and equations ... [b]ut bit by bit ... came to stand not for a tool, but for nature' (Galison, 1997: 777). His account evokes theoretical discussions of the role of simulations in postmodernity (Poster, 1988; Baudrillard, 1993). Galison does not discuss variations in levels of certainty attributed to simulation technology and output, a challenge taken up by Simon Shackley and Brian Wynne (1995, 1996) in their analysis of climate models and their interface with policy.

General Circulation Model Users

Shackley and Wynne draw from MacKenzie's certainty trough to explain the distribution of uncertainty around GCMs. They credit modelers with being more aware of their own models' uncertainties compared to users, and as inclined to freely discuss these uncertainties, 'at least among themselves' (Shackley & Wynne, 1996). Shackley and Wynne suggest that awareness of flaws, limitations, and uncertainties associated with GCMs and their output diminishes as these technologies travel from the production site to other areas of science and society where they are used. Scientists ('impacters') who use GCM output as input in other models that simulate socioeconomic and environmental impacts of GCMs' projections of future climate change serve as a case in point. Shackley and Wynne describe the impacters in terms of MacKenzie's certainty trough, suggesting that their location further from the site of GCM construction renders them less aware than modelers of the uncertainties associated with the output.

Shackley and Wynne's representation of the distribution of uncertainty resonates with the perspective of model developers. In interviews and conversations during my ethnographic research, modelers typically identified the problem of *users*' misuse of their model output, suggesting that the latter interpret the results too uncritically. Model developers profess that they, as one of them put it, take the models 'seriously but not literally' (Somerville, 1996). Some among them even publicly criticize the most complex modeling efforts. For instance, veteran US-based climate modeler Syukuro Manabe told *The New York Times* that the models have 'gone too far' and that '[p]eople are mixing up qualitative realism with quantitative realism' (Revkin, 2001). His assertion supports the claim that modelers are intimately aware of the limitations of the models they themselves help produce, but leaves ambiguous exactly who the 'people' are to whom he refers: do they include climate modelers or only users – and which types of users, with what kinds of relationship to model development?

GCM developers' discourses support Shackley and Wynne's analysis of the distribution of certainty. In what follows, however, I identify limitations in the applicability of the certainty trough and the trope of distance when applied to simulation technology.

Several important dimensions of climate modeling complicate the applicability of the certainty trough in particular, and the trope of distance in general. First, model developers typically are also model users. Because of the complexity of the models and of the phenomena they seek to represent, GCM model developers build only parts of a model, integrating sub-models and representational schemes ('parameters') developed by other modeling groups. Even scientists ('model users') who are not primarily model developers typically modify the models they have obtained from elsewhere. Moreover, much research in the atmospheric sciences today consists of modifying a subset of variables in models developed elsewhere. This complicates clear-cut distinctions between users and producers of models.

Second, the difficulty with distinguishing developers from users also complicates clear identification of the exact site of production; any complex GCM involves *multiple sites of production.* Any single model developer acts at a distance from at least some of the multiple sites in which models and their components are developed.

Third, increased specialization has reduced the amount of time model developers have to study the atmosphere by means of empirical data. Model accuracy is gauged by checking models against empirical knowledge of how the real atmosphere and larger Earth system behaves. For this reason, some empiricists are perhaps in the best position to identify some model inaccuracies. These empiricists may also be described as users and even, in some respects, as alienated from the GCMs.

Fourth, climate modelers' psychological and social investments in models and the social worlds of which they are a part can at times reduce their *critical* distance from their own creations. Modelers sometimes identify with their own models and become invested in their projections, which in turn can reduce sensitivity to their inaccuracy. Such personal and professional investments are not unique to the field of modeling.

Because of the above factors, model producers are not always willing or able to recognize weaknesses in their own models, contrary to what is suggested by the certainty trough and by analyses of climate models drawing on that framework. Below, I argue that some users – the empiricists who help validate the models – may be better positioned to identify inaccuracies than are model developers.

Multiple Sites of Production; Integration of Model Development and Model Use

The certainty trough best describes innovations with unitary functions and unitary sites of production. It applies poorly to GCMs because they are are not carried out at a singular site and because GCM creators are also commonly model users.

In the first decades of climate modeling (from the 1960s until the 1980s), individual model developers constructed their models from scratch. Today, no scientist single-handedly develops a complex GCM model from the bottom up. In any given modeling institution, there is typically a division of labor. For instance, a coupled ocean–atmosphere model typically involves at least three groups of scientists: one group of scientists focuses on the development of an atmospheric model, another group develops an ocean model, and a third group couples the two together. Moreover, developers of each of the sub-models often build upon and integrate elements from other sub-models (for example, convection schemes) developed by other groups. As a result, no single person deeply knows, or is 'close to', all aspects of a given GCM.

Modelers' empirical knowledge of the physical atmosphere is limited by the fact that they have to devote the vast majority of their time to studying their models rather than independent data sets and actual weather phenomena. MacKenzie's diagram is too simple conceptually to account for the fact that complex GCMs involve a multiplicity of production sites and that model developers also are model users. Especially in the context of 'big science' (Galison & Hevly, 1992; Gibbons et al., 1994), actors are not easily captured by singular and unchanging labels ('knowledge producers', 'users', 'alienated') or characteristics (as knowledgeable, under-critical, or over-critical). Since producer/users *vary in their familiarity with different parts of the same overall model*, no single person intimately knows all dimensions of any given GCM, or the associated uncertainties. In other words, *the overall distribution of certainty varies* depending on the sub-model or issue in focus. Moreover, as I discuss in the sections that follow, there may be variation in the distribution of certainty through time: the same person may think differently of the same technology at different moments in time.

Knowledge Producers – Are They the Most Reliable Evaluators of Their Own Technology?

Even disregarding the fact that model developers often are users as well and work at some distance from a given model's multiple sites of production, the certainty trough does not account for the possibility that knowledge producers may not be the most reliable evaluators of their own work. MacKenzie (1990: 366) indicates some awareness of this when he acknowledges that knowledge producers 'certainly [are] not critics' of their own technology.

However, more subtle indications of knowledge producers' limitations may be overshadowed by MacKenzie's description of them as being intimately aware of the shortcomings of the products of their own labor. Shackley and Wynne reinforce this view when they draw upon MacKenzie's certainty trough to discuss impacters' less critical understanding of the GCMs, while suggesting that modelers openly discuss their models' shortcomings among themselves, and also when proposing that modelers' tendency to downplay uncertainty is a function of strategic choice.

According to Shackley and Wynne, modelers sometimes deliberately present their models in ways that suggest and encourage exaggerated faith in their accuracy. The authors identify a duality in climate modelers' discourse, observing that they shift between strong claims to scientific authority (models as 'truth machines') and more modest claims about models as aids to thinking about the world (models as 'heuristics') (Shackley & Wynne, 1995, 1996). Though modelers lack a conceptual basis for knowing whether the long-term climate is predictable, their discourse often moves from describing long-term global climate predictions as being possible *in principle*, but presently unrealized and uncertain, to suggesting that their models are in fact predictive. My interviews documented this tendency with testimony from modelers themselves, confirming my own observations.

Such oscillation may signal a need for a more radical reconceptualization, but Shackley and Wynne's distinction between how modelers speak among themselves and how they speak to external audiences preserves the certainty trough, as does their portrayal of modelers' shifting representations as a function of strategic choice. They suggest that modelers – keen to preserve the authority of their models – deliberately present and encourage interpretations of models as 'truth machines' when speaking to external audiences.

Shackley and Wynne thus identify an important aspect of modelers' public communications. Like scientists in other fields, modelers might 'oversell' their products (as acknowledged in quotes presented below), because of funding considerations. In a highly competitive funding environment they have an interest in presenting the models in a positive light.

The centrality of climate models in politics can also shape how modelers and others who promote concern about climate change present them. GCMs figure centrally in heated political controversies about the reality of climate change, the impact of human activities, and competing policy options. In this context, caveats, qualifications, and other acknowledgements of model limitations can become fodder for the anti-environmental movement (Gelbspan, 1995, 1997; Helvarg, 1994; Lahsen, 1998b, 2005). Media-propelled political campaigns have erupted regularly since the early 1990s, often with help from public relations companies, politically conservative think tanks, and industry groups with interests in continued high-level dependence on fossil fuels. These organized opponents attack the scientific evidence of human-induced climate change – and hence climate models – to undermine policy measures designed to prevent or mitigate the problem (Lahsen, 1998b).

In such a charged political context, modelers learn to exercise care in how they present their models in public forums. The need for such care is sometimes impressed explicitly upon them by scientists who have experience in national and international climate politics. Speaking to a full room of NCAR scientists in 1994, a prominent scientist and frequent governmental advisor on global change warned an audience mostly made up of atmospheric scientists to be cautious about public expressions of reservations about the models. 'Choose carefully your adjectives to describe the models', he said, 'Confidence or lack of confidence in the models is the deciding factor in whether or not there will be policy response on behalf of climate change.' While such explicit and public references to the political impact of the science are rare (I only encountered this one instance during my fieldwork), a similar lesson is communicated in more informal and subtle ways. It is also impressed on many who witness fellow atmospheric scientists being subjected to what they perceive as unfair attacks in media-driven public relations campaigns, such as the case of the Lawrence Livermore National Laboratory (Livermore, CA) modeler Benjamin Santer, in the controversy over the 1995 Intergovernmental Panel on Climate Change (IPCC) report (Lahsen, 1998b).

It is thus correct to distinguish, as Shackley and Wynne do, between how modelers speak among themselves and how they speak to 'external audiences'. Climate modelers and advisory scientists use strong claims – invoking models as 'truth machines' and downplaying uncertainty – in communications directed 'outside' of the modeling community, and such discourse does not necessarily reflect their more private discussions.

But is it accurate to suggest, as MacKenzie, Shackley, and Wynne appear to do, that the highest level of objectivity about a given technology is to be found among those who produced it? Shackley & Wynne (1996) write that 'at least among themselves', model producers typically acknowledge many uncertainties and indeterminacies in their models. Knowing exactly how modelers speak among themselves is complicated by an 'outsider's' difficulty to directly observe unmediated, informal communication among modelers. However, my participant observation and interview data suggest that model producers are not always inclined, nor perhaps *able*, to recognize uncertainties and limitations with their own models.

With its trope of distance, the certainty trough may inadvertently reinforce – and perhaps draw persuasive power from – a common, idealized construct of scientists as autonomous knowers of truth. Shackley and Wynne use comparative and qualifying terms such as 'likely', 'more' (for example, 'more aware') and 'quite' ('modelers are quite prepared to discuss model deficiencies' [1995: 118]), 'typically' ('Typically, the producers of knowledge acknowledge its many uncertainties . . . ' [1996: 277]), 'may' ('so practitioners may attribute greater certainty to knowledge from another specialty than the practitioners in the first specialty' [1995: 114]). Nevertheless, the trope of distance embedded in their analysis suggests that, at sites where models are developed, people involved in their production know the models' limitations in a broadly consistent and reliable way, even though the latter may be hidden or ignored when the models 'travel' out into the world.

Yet, as discussed earlier, GCMs are developed and modified at a multiplicity of sites dispersed in time and space; they are in fact composites of multiple production processes. As a result, any given part of a larger GCM is transparent only to some of the persons who helped develop it. No single person has deep knowledge of all of the subcomponents and assumptions that make up a given GCM. To function properly, it would seem that the certainty trough should be applied not to a single overall GCM, but to each of the innumerable subcomponents that comprise it.

Similar to Shackley and Wynne's portrayal of modelers as being able to identify and discuss model weaknesses among themselves, Deborah Dowling's (1999) construct of the 'competent professional' aguably serves to preserve an idealized image of how scientists use and relate to simulations. On the basis of interviews with scientists in a variety of fields, Dowling describes how the 'competent professional' relates to the simulation technology they develop and use: they have a 'clear, analytic grasp' (p. 266) of the mathematics built into the program; they 'get into the code' and 'know the strengths, the approximations, the difficult points, and the pits you can fall into' (ibid.). In the words of a pharmaceutical chemist quoted by Dowling, responsible users try to 'break down that sense that, because this number came out on the printed form, it has got to be right' (Dowling, 1999: 266). Dowling does not discuss the possibility that competent scientists at times may fail to live up to this standard. Her account of modelers' relation to their own models recalls Robert Merton's norms of science (Merton, 1973 [1942]), which have been criticized as ideals serving an ideology that promotes scientists' self-interest (Mulkay, 1976).

MacKenzie may also verge on idealizing knowledge producers in his contrast between them, on the one hand, and users and critics on the other. Users' and critics' credibility is undermined in contrast to that of knowledge producers. In the certainty trough diagram, users are presented as under-critical and critics as over-critical, in contrast to producers, who have detailed understanding of the technology's strengths and weaknesses. In this framework, which valorizes closeness to the site of knowledge production, the critics' relatively greater distance from the site of knowledge production also weakens their credibility. MacKenzie portrays 'users' as less knowledgeable than producers, and lumps them with persons who uncritically believe the most idealized representations of the models (those who simply 'believe the brochures').

MacKenzie notes that users sometimes consider knowledge producers to be 'liars' and demand that their results be subjected to elaborate and expensive independent testing (MacKenzie, 1990: 373). This suggests that at least some users maintain critical distance from the technology in question, and that a great deal of heterogeneity is lost when such users are lumped together with others who are uncritically committed to the technological program. The possibility of critical distance on the part of (some) users is not well reflected in the certainty trough diagram. In contrast to the critics' 'alienation' from the technology in question, and their 'commitment' to alternative technology, users are assumed to be predisposed to accept the technology, because of their 'loyalty' to the program. MacKenzie appears to imply that their judgment is based relatively more on external interests and possibly emotional factors, as opposed to intimate, technical, and objective understanding. MacKenzie's description would seem to imply that knowledge producers, by contrast, are not similarly swayed by external and subjective factors.

By contrast, my case study indicates the extent to which the certainty trough overlooks variation over time in knowledge producers' willingness *and ability* to recognize inaccuracies in their own models. In what follows, I present interview data suggesting that modelers do not necessarily like to discuss their models' weaknesses *even among themselves*, and that *in some respects* they may be less inclined and less able to identify them than are some other scientists. I also present evidence of greater heterogeneity in the distribution of certainty within the categories of 'knowledge producers' and 'users' than the certainty trough assumes.

The Power of Simulations

During modelers' presentations to fellow atmospheric scientists that I attended during my years at NCAR, I regularly saw confusion arise in the audience because it was unclear whether overhead charts and figures were based on observations or simulations. In one such presentation about the role of clouds in the climate system, observational data were compared against model simulations. I grew confused as to which charts represented empirical data and which represented simulations. I realized that I was not alone in my confusion when scientists in the audience stopped the presenter to ask for clarification as to whether the overhead figures were based on observations or model extrapolations. The presenter specified that the figures were based on models, and then continued his presentation.

Sometimes such confusion resulted from imprecise communication on the part of modelers. It is understandable that modelers easily forget to preface each of their representations with the words 'simulated' or 'modeled' ('the *simulated* ocean', 'the *modeled* ocean–atmosphere dynamic', and so on). At other times, modelers may have been strategic when alternating between speaking of their models as heuristics and presenting them as 'truth machines'. However, the oscillation also may reflect how some modelers think and feel about their models at particular moments when they fail to maintain sufficient critical distance. In interviews, modelers indicated that they have to be continually mindful to maintain critical distance from their own models. For example:

> *Interviewer*: Do modelers come to think of their models as reality?
>
> *Modeler A*: Yes! Yes. You have to constantly be careful about that [*laughs*].

He described how it happens that modelers can come to forget known and potential errors:

> You spend a lot of time working on something, and you are really trying to do the best job you can of simulating what happens in the real world. It is easy to get *caught up in it*; you start to believe that what happens in your model *must be what happens* in the real world. And often that is not true . . . The danger is that you begin to *lose some objectivity* on the response of the model [and] begin to *believe that the model really works like the real world* . . . then you begin to *take too seriously how it responds* to a change in forcing. Going back to trace gases, CO_2 models – or an ozone change in the stratosphere: if you really believe your model is so wonderful, then the danger is that it's *very tempting to believe* that the way it responds to a change in forcing must be right. [Emphasis added]

This modeler articulates that the persuasive power of the simulations can affect the very process of creating them: modelers are at times tempted to 'get caught up in' their own creations and to 'start to believe' them, to the point of losing awareness about potential inaccuracies. Erroneous assumptions and questionable interpretations of model accuracy can, in turn, be sustained by the difficulty of validating the models in the absence of consistent and independent data sets.

Critical distance is also difficult to maintain when scientists spend the vast majority of their time producing and studying simulations, rather than less mediated empirical representations. Noting that he and fellow modelers spend 90% of their time studying simulations rather than empirical evidence, a modeler explained the difficulty of distinguishing a model from nature:

> *Modeler B*: Well, just in the words that you use. You start referring to your simulated ocean as 'the ocean' – you know, 'the ocean gets warm', 'the ocean gets salty'. And you don't really mean the ocean, you mean your modeled ocean. Yeah! *If you step away from your model you realize 'this is just my model'*. But [because we spend 90% of our time studying our models] there is a *tendency to forget* that just because your model says x, y, or z doesn't mean that that's going to happen in the real world.

This modeler suggests that modelers may talk about their models in ways they don't really mean ('you don't really mean the ocean, you mean your modeled ocean . . . '). However, in the sentence that immediately follows, he implies that modelers sometimes actually come to think about their models as truth-machines (they 'forget to step away from their models to realize that it is just a model'; they have a 'tendency to forget').

The following interview extract arguably reflects such an instance of forgetting. This modeler had sought to model the effects of the possible 'surprise' event of a change in the ocean's climate-maintaining thermohaline circulation. On the basis of his simulation he concluded that the widely theorized change in the ocean's circulation due to warmer global temperatures is not likely to be catastrophic:

> *Modeler C*: One of the surprises that people have been worrying about is whether the thermohaline circulation of the oceans [the big pump that could change the Gulf Stream] shuts off . . . . If the models are correct, the effect even of something like that is not as catastrophic as what most people think. You have to do something really nasty to [seriously perturb the system] . . . The *reality is, it really is* an ocean thing, it is basically an ocean phenomenon; it *really doesn't* touch land very much.
>
> *Interviewer*: But wouldn't it change the Gulf Stream and therefore . . . ?
>
> *Modeler C*: Yes, look right here [*shows me the model output, which looks like a map*]. If the model is right. [*Slight pause*] I put that caveat in at the beginning [*laughs*]. But right there is the picture.

Modeler C struggles to not speak of his model as a 'truth machine', but lapses before catching himself when presented with a question. Though he starts off indicating that the models could be wrong ('if the models are correct'), he soon treats the model as a truth machine, referring to the modeled phenomena as reliable predictions of future reality ('The reality is, it really is an ocean thing'). Catching himself, he then refers back to the caveat, followed by a little laugh.

The following modeler suggests that the above tendencies are pervasive in the field of climate modeling:

> *Modeler D*: There are many ways to use models, and some of them I don't approve of. [*Pause*] It is easy to get a bad name as a modeler, among both theoreticians and observational people, by running experiments and seeing something in the model and publishing the result. And pretending to believe what your model gives – or, even, *really believing* it! [*small laugh*] – is the first major mistake. If you don't keep the attitude that it's just a model, and that it's not reality . . . I mean, *mostly people that are involved in this field really have that, they have the overtone that it is.*
>
> *Interviewer*: They do tend to think that their model is the reality?
>
> *Modeler D*: Or even if they don't think that, they tend to oversell it, regardless.
>
> *Interviewer*: And why do they oversell it?
>
> *Modeler D*: Because people get wrapped up in what they have done. You know, I spent years building this model and then I ran these experiments, and *the tendency is to think: 'there must be something here'*. And then they start showing you all the wonderful things they have done . . . And you have to be very careful about that.

Confirming Shackley and Wynne's argument, modeler D suggests that modelers sometimes 'oversell' their models, strategically associating them with more certainty than is warranted. However, echoing others quoted earlier, Modeler D also suggests that modelers sometimes lose critical distance from their own models and come to think of them as reliable representations of reality.

The increasingly realistic appearance of ever-more comprehensive simulations may increase the temptation to think of them as 'truth-machines'. As Shackley et al. (1998) have noted, there is a tendency among modelers to give greater credence to models the more comprehensive and detailed they are, a tendency they identify as cultural in nature because of a common trade-off between comprehensiveness and error range. As GCMs incorporate ever more details – even things such as dust and vegetation – the models increasingly appear like the real world, but the addition of each variable increases the error range (Syukuro Manabe, quoted in Revkin, 2001).

Some may resist this temptation better than others, and even the same modeler may resist it at some moments better than at others. Modelers' statements suggest that the level of certainty any given modeler attaches to his or her model varies in time; they do not necessarily have a consistent, 'healthy skepticism'. At the 'inner core' of knowledge production – among modelers at the individual and the group level – awareness of uncertainties thus can wax and wane depending on the situation.

This confusion of simulations with real data within the atmospheric sciences may be part of a more general phenomenon: similar conflation of simulations with 'observations', 'samples', and 'data' has been identified in studies of scientists in other fields of research (Dowling, 1999). Simulation techniques may especially encourage such conflation, however. For example, Stefan Helmreich's ethnographic study of artificial life simulators (1998) revealed the powerful effect of simulations on the imagination of their creators and users. Similar to global climate simulations, visual simulations of artificial life afford a 'god's eye view' that contributes to the illusion that they are empirical results. These simulators conflate their artificial worlds with real life, Helmreich suggests: the researchers recognize that the life they simulate is not carbon-based, but they nevertheless consider it to be an alternative, electron-based kind of life, insisting that their admittedly artificial worlds are real life-forms in the sense that they are composed of real matter and energy. It would seem reasonable to assume that attempts to simulate the real world only intensify such conflation.

Downplaying/Ignoring Model Uncertainties and Limitations among Modelers: The Role of Emotional Attachment and Social Worlds.

Modelers' professional and emotional investment in their own models reduces their inclination and ability to maintain critical awareness about the uncertainties and inaccuracies of their own simulations. Shackley and Wynne suggest that modelers talk freely about their models' shortcomings among themselves. However, the following researcher identified a general reluctance on the part of modelers to discuss their models' weaknesses, even among themselves:

> *Modeler E*: What I try to do [when presenting my model results to other modelers] . . . is that I say 'this is what is wrong in my model, and I think this is the same in all models, and I think it is because of the way we're resolving the equations, that we have these systematic problems'. *And it often gets you in trouble with the other people doing the modeling. But it rarely gets you in trouble with people who are interested in the real world*. They are much more receptive to that, typically, than they are if you say 'here, this is my result, doesn't this look like the real world?' And 'this looks like the real world, and everything is wonderful'.
>
> *Interviewer*: Why do you get in trouble with modelers with that?
>
> *Modeler E*: Because . . . when I present it, I say 'this model is at least as good as everyone else's, and these problems are there and they are in everybody else's models too.' They often don't like that, even if I am not singling out a particular model, which I have done on occasion [*smiles*] – not necessarily as being worse than mine but as having the same flaws. Not when they are trying to sell some point of view and I go in there saying 'Hey, this is where I go wrong [in my model], and you are doing the same thing! And you can't be doing any better than that because I know that this isn't a coding error problem' [*laughs*].

This modeler confirmed statements about modelers I encountered in other contexts, who also identified a disinclination on the part of modelers to highlight, discuss, and sometimes even *perceive* problems in their model output.

In a paper consisting of an imaginary dialogue about global environmental science and policy between two sirens of Greek mythology, Shackley & Darier (1998) suggest that when modelers discursively associate their models with unwarranted or unproven levels of accuracy, this may reflect how they also *think* of their models. In response to the first siren's suggestion that such discourses are strategic in nature and intended to seduce outside audiences, the second siren argues that modelers have '(self)seducing powers'. She notes that modelers 'trust' their models, that they have some degree of 'genuine confidence, maybe over-confidence' in their quantitative projections. She comments: 'It is not simply "calculating seduction" but a sincere act of faith!'

These claims about modelers' relation to their own creations remain at the level of speculation in Shackley and Darier's text. They are backed up by a reference to the paper by Risbey et al. (1996) on integrated assessment (IA) modeling. Risbey et al. do not discuss modelers' relation to the models in detail but note that IA modelers do not always 'bear in mind' the distinction between models as heuristics versus truth machines, and that they can fail to 'maintain familiarity' with the limitations of their models (1996: 331). Despite such limited documentation, it would seem that these analysts of model communities have witnessed dynamics of the sort described here.

Modeler E, in the excerpt quoted above, distinguished some modelers from 'people who are interested in the real world'. He thus implied that modelers sometimes become so involved in their models that they lose sight of, or interest in, the real world, ignoring the importance of knowing how the models diverge from it.

Recognition of this tendency may be reflected in modelers' jokes among themselves. For example, one group joked about a 'dream button' allowing them – Star Wars style – to blow up a satellite when its data did not support their model output. They then jokingly discussed a second best option of inserting their model's output straight into the satellite data output.

These psychological and social dimensions of modelers' investment in the accuracy of their models, and the seductive power of simulations in general, are not accounted for by the certainty trough, or, more generally, by the trope of distance. The evidence from the present study suggests a need to revise the certainty trough to indicate greater certainty among climate modelers. Better yet, assuming it could be represented visually, the curve indicating knowledge producers' awareness of uncertainty ought to vary depending on context, as their acknowledgement of uncertainty and inaccuracy waxes and wanes. As I suggest in what follows, levels of certainty among users also vary: *some* users are *sometimes* more aware than producers are of model inaccuracies.

General Circulation Model Users and Critics

Shackley & Wynne (1995: 114) suggest that 'practitioners may attribute greater certainty to knowledge from another speciality than the practitioners in the first speciality would attribute to it themselves.' This is undoubtedly an important dynamic in the distribution of certainty, but it may be the case that practitioners in neighboring fields – or who are in the same field but have different interests and rely on different methods – sometimes see limitations in the models that the modelers themselves can not or will not see.

The complexity of GCMs undermines modelers' ability to gauge the validity of their own models. Computer models have grown so complex and scientists so specialized that modelers spend little time checking their own models against available observations. This weakens their ability to identify where their models fail to accurately represent empirical evidence. A physicist interviewed in a study on computers and identities confirmed that scientists today can come to know more about computer models than about actual biogeophysical dynamics that their computers represent:

> My students know more and more about computer reality, but less and less about the real world. And they no longer even really know about computer reality, because the simulations have become so complex that people don't build them anymore. They just buy them and can't get beneath the surface. If the assumptions behind some simulation were flawed, my students wouldn't even know where or how to look for the problem. So I am afraid that where we are going here is towards *Physics: The Movie*. (Turkle, 1984: 66)[4]

Because modelers' limited empirical knowledge of the atmospheric system reduces their ability to identify shortcomings in their models, more empirically inclined scientists are brought in to help evaluate the models' performance. Some of these empirical scientists, who refer to themselves as 'close' or 'feedback' users, are better able than the knowledge-producers to judge a given GCM's accuracy, at least as far as identifying gaps between the simulations and observations is concerned.

Modeler E noted that theoreticians and empiricists often criticize modelers for claiming unwarranted levels of accuracy, to the point of conflating their models with reality. My fieldwork revealed that such criticisms circulate widely among atmospheric scientists. Sometimes such criticisms portray modelers as motivated by a need to secure funding for their research, but they also suggest that modelers have genuine difficulty with gaining critical distance from their models' strengths and weaknesses. Moreover, they criticize modelers for lacking empirical understanding of how the atmosphere works ('Modelers don't know anything about the atmosphere').

Presentations at NCAR provide a forum for witnessing the tensions that exist between modelers and empiricists. Following modelers' presentations of their work, empiricists frequently feel a need to emphasize the difference between model output and observational data. In interviews, empiricists often voice criticisms along the lines of this one expressed by a meteorologist: 'I joke about modelers: they have a charming and naive faith in their models.'

Such comments were especially common among empirical meteorologists trained in synoptic weather forecasting techniques, who conduct empirical research on a regional or local scale. They have not been centrally involved in the process of model development and validation, and thus may fall within MacKenzie's category of the 'alienated'. These empiricists trained in synoptic methods are particularly inclined to criticize GCMs. Such criticism may have to do with the fact that there is considerable resentment among various subgroups of atmospheric scientists about the increased use of simulation techniques, and such resentment may be echoed in other sciences in which simulations are ascendant (Lahsen, 1998a). Moreover, models and simulations are 'misfits that do not sit comfortably in established categories' (Sismondo, 1999: 253). They exist in an ambiguous space between theory and experiment and occupy an ambiguous social position in science as well.

Synoptically trained empirical meteorologists have particular motivation to resent models. Their methods and lines of work were in important part replaced by global numerical models. The environmental concern about human-induced climate change, and the associated politics, also favored the GCMs and those working with them. The applied aspect of these meteorologists' work was thus being taken over by numerical weather forecasting, pushing them in the direction of basic research. Their comments should be understood as potentially interested instances of boundary-work (Gieryn, 1995) whereby they, as a competing scientific group, seek to promote the authority of their own lines of research in competition with GCMs. This placed them at a competitive disadvantage when national funding priorities changed in favor of research with obvious social benefits, whereas GCM modeling seemed relevant to predicting future manifestations of human-induced climate change.

The emergence of numerical techniques also represented a loss in epistemic status as well as funding for the empirical meteorologists. So-called 'objective' numerical methods resulted in the demotion and relabeling of their more qualitative approach as 'subjective', an unattractive label in the context of a cultural preference for 'hard' science within the scientific community. As a meteorologist jokingly said to me when expressing resentment at the label: 'so I change it. I refer to [synoptic analyses] saying: "these are the intelligent analyses, and those [GCM studies] are artificially intelligent."'

Compared with modelers, such empirical research meteorologists with background in weather forecasting are part of a different social world; these two groups partake in different, albeit overlapping, social networks defined by different scientific orientations and cultural norms. The empiricists are less committed to GCMs or to the theory of human-induced climate change.[5] They manifest skepticism about numerical forecasts in general, especially beyond a period of 10 days or so. These attitudes are part of a common culture among weather forecasters, including scientists who may only have done forecasting early in their careers. This culture also involves humility about the accuracy of forecasts of atmospheric conditions, which they trace to experiences of regularly seeing synoptic and numerical weather forecasts proven wrong. In my judgment, many of these meteorologists are rightly classified as 'alienated' with regard to GCMs.

Nevertheless, they may have important insight into model inaccuracies, both for technical reasons (a function of their expertise) and for social and psychological reasons (a function of their lesser investment in the GCM enterprise). As suggested by Modeler D above, non-modelers may at times be better able to keep uncertainties in mind than the modelers themselves. The fact that some of them are engaged in the process of validating GCMs also indicates that the empiricists in some respects are in a privileged position to discuss inaccuracies; sometimes they become quite knowledgeable about GCMs. Those who help with validating GCMs are not necessarily alienated from the technology, and may have a distinct investment in the models. However, at least some of these users interact closely with empirical meteorologists trained in weather forecasting, and in my judgment they appear to be influenced by the forecasters' 'alienated' attitude.

If some of these empirical meteorologists and self-identified users are properly classified as alienated, this also confounds the axis of distance in the certainty trough diagram. Some empirical research meteorologists are alienated and committed to alternative technology (for example, empirical 'synoptic' methods), but they are not necessarily more distant from the site of knowledge production than are many users.

Moreover, the attitude of the 'alienated' towards the models may be ambivalent rather than consistently critical. They criticize GCMs, and resent the way institutional funding has been drawn away from synoptic approaches.[6] At the time of my fieldwork, such atmospheric empiricists complained about insufficient funding even for *validation* of the GCMs with empirical data, a complaint supported by analysts (Risbey et al., 1996). However, I found that even the meteorologists whose empirical research competed with the numerical models were, overall, inclined to acknowledge the value of GCMs. They considered GCM modeling as an important contribution to science. This underscores once again the weakness of the certainty trough in terms of ambivalence and complexity in the level of commitment to a given technology.

Compared with modelers, the empiricists may at times be in a better position to identify model shortcomings because of their deeper knowledge of empirical processes simulated by the GCMs, and because of their lower psychological and professional investment in the models. This is not to suggest that empiricists are invariably better identifiers of truth. Indeed, their expertise, methodologies, and commitments are also limited. Rather, the point is that scientists tend to become emotionally involved with their own creations. Simulation of complex, uncertain, and inaccessible phenomena leaves considerable room for emotional involvement to undermine the ability to recognize weaknesses and uncertainties.

Empiricists complain that model developers often freeze others out and tend to be resistant to critical input. At least at the time of my fieldwork, close users and potential close users at NCAR (mostly synoptically trained meteorologists who would like to have a chance to validate the models) complained that modelers had a 'fortress mentality'. In the words of one such user I interviewed, the model developers had 'built themselves into a shell into which external ideas do not enter'. His criticism suggests that users who were more removed from the sites of GCM development sometimes have knowledge of model limitations that modelers themselves are unwilling, and perhaps unable, to countenance. A model developer acknowledged this tendency and explained it as follows:

> *Modeler F*: There will always be a tension there. Look at it this way: I spent ten years building a model and then somebody will come in and say 'well, that's wrong and that's wrong and that's wrong'. Well, fine! And then they say, 'well, fix it!' [And my response to them is:] '*you* fix it! [*laughs*] I mean, if I knew how to fix it, I would have done it right in the first place!!! [*Laughs*] And what is more, I don't like you anymore – all you do is you come in and tell me what is wrong with my model! Go away!' [*laughter*]. I mean, this is the field.

Modeler F's acknowledgement of inaccuracies in his model is implied in his comment that he would have improved the model if he knew how. Nevertheless, the interview excerpt is another indication of modelers' emotional investment in their models. Modeler F acknowledges a common tendency on the part of modelers to distance themselves from criticism and react personally to criticisms ('I don't like you anymore'; 'go away!'). One might dismiss the seriousness of this account, because of the joking manner in which Modeler F expresses it. Yet anthropologists identify humor as a common marker of cultural sensitivity and charged emotions. In this context, it is interesting to note other instances (such as Modelers A and C, above) in which modelers laughed when discussing their relationship to their own models, particularly the temptation to perceive them as truth-machines.

Aside from the stakes involved with environmental policy and with modeling groups' need to sustain funding for their work, it is important to recognize model developers' stake in their own creations. Building a good, complex GCM is a daunting task that requires years and even decades of dedication. As a result, model developers' entire professional careers are on the line; the performance of their model will reflect on their careers as a whole. It is thus understandable that they also are sensitive to criticisms and at times may tend to give their models the benefit of the doubt. During my fieldwork, I witnessed prolonged and acrimonious fights in which model developers defended their models and engaged in serious conflicts with colleagues within the larger modeling group who rejected them in favor of other models they deemed more accurate.

The point to stress here is that modelers' desire to produce accurate simulations sometimes weakens their willingness and ability to identify weaknesses and uncertainties. Aside from the general phenomenon in science of competition and conflict, the excerpts presented above reveal emotional aspects of modeling not captured by the certainty trough. Modelers' careers and identities become intertwined with, and partly dependent on, the quality of their models, to the point that they sometimes may be tempted to deny it when their models diverge from reality.

Revising the Certainty Trough

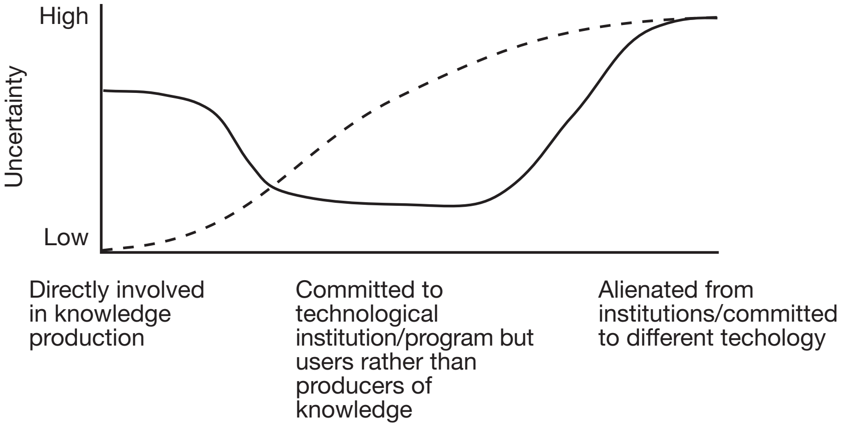
The certainty trough may account for the distribution of certainty at a broad, general scale. Generally speaking, atmospheric scientists are better judges than, for example policy-makers, of the accuracy of model output. However, the distribution of certainty about GCM output within the atmospheric sciences reveals complications in the categories of 'knowledge producers' and 'users', and the privileged vantage point from which model accuracies may be gauged proves to be elusive.

Model developers' knowledge of their models' inaccuracies is enhanced by their participation in the construction process. However, developers are not deeply knowledgeable about all dimensions of their models because of their complex, coupled nature. Similarly, the empirical training of some atmospheric scientists – scientists who may be described as users – limits their ability to gauge GCM accuracies in some respects while enhancing their ability to do so in other respects; and, generally, they may have better basis than the less empirically oriented modelers for evaluating the accuracy of at least some aspects of the models.

Professional and emotional investment adds another layer of complexity. Model developers have a professional stake in the credibility of the models to which they devote a large part of their careers. These scientists are likely to give their models the benefit of doubt when confronted with some areas of uncertainty. By contrast, some of the empirically trained atmospheric scientists, who are less invested in the success of the models, may be less inclined to give them the benefit of the doubt, maintaining more critical understanding of their accuracy.

The distinction between modelers' public and non-public representations of models reflects modelers' investment in the perceived accuracy of their models. The distinction implies that in their interaction with external audiences, modelers at times downplay model inaccuracies because they are interested in securing their authority. I argue that this framework needs to be stretched further to account for limitations in modelers' ability to identify such inaccuracies, limitations that may arise from a combination of psychological, social, and political factors. Attitudes towards technology are affected by these factors, which introduce (lack of) distance of a different kind than what the certainty trough highlights. The notion of 'alienation' in MacKenzie's framework may hint at the impact of emotional commitments on perceptions of accuracy, but such an interpretation is

FIGURE 2
Revised certainty trough. The dashed line (------) indicates uncertainty levels not accounted for in MacKenzie (1990).



Source: MacKenzie, 1990. © MIT Press.

somewhat undercut by the Marxist conception (alienation from the means of production) implied by MacKenzie's usage.

In light of the above, one might revise the certainty trough diagram as shown in Figure 2.

However, even the revised diagram fails to capture the complexity of the dynamics involved. To account for the complexities discussed in this paper, the certainty trough diagram would have to do the following:

- recognize that any given model involves multiplicity sites of production;
- represent greater heterogeneity within the general categories of knowledge producers and users;
- reflect the shifting roles of individual actors involved: model developers are also model users in some ways, users contribute to the production of the technology;
- show that users in some instances may be closely interlinked with 'alienated' groups and share in some of those groups' criticisms (for example, share some resentment toward funding patterns that privilege GCM approaches over more empirical lines of research);
- acknowledge that some users may be better positioned to identify some model inaccuracies than producers of the models in question;
- show oscillation through time in the level of uncertainty and skepticism acknowledged by producers and users of models.

A single, visual diagram seems unable adequately to represent the complex ways in which political, professional, and psychological investment in the models, as well as variations in time and context, affect awareness of inaccuracies and uncertainties. Adequate representation of the complexity would require a more dynamic model capable of showing multiple dimensions and variations through time. It would also have to account for the different combinations of socio-cultural influence – the different social worlds – that shape different actors' relationships to the technology. Modelers and empirical atmospheric scientists are part of different, albeit partly overlapping, social worlds involving different inclinations to question the models' scientific and political value as well as their accuracy. As I have suggested, these actors relate differently to the climate models for reasons not captured by the certainty trough, including reasons unrelated to distance. Other, more obviously social and psychological factors come into play, complicating the meaning of 'distance' and 'proximity'. When we take these other factors into account, it appears that distance lends enchantment, but proximity lends enchantment as well.

Notes

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1. The cited works explicitly discuss and integrate MacKenzie's certainty trough. More general influence of MacKenzie's work is reflected in the significant number of citations it has received, especially in the field of science studies (see for instance the Web of Science database, <www.isinet.com/products/citation/wos/>).
2. For simplicity, I will use the term 'models' and 'climate models' rather than specify in each instance that I am referring to 'GCM modeling'. It should be made clear, however, that scientists of all disciplines and practices use models, and that a wide range of models exists. For example, a mouse may serve as a 'model' in medical research.
3. Stephen D. Norton and Frederick Suppe (2001) criticize the analysis of Oreskes et al. (1994) for singling out models for criticism, when such criticism applies to scientific knowledge in general. They note that scientific claims never can be established with absolute certainty and that Oreskes et al. operate with a criterion of certainty that is 'epistemologically irrelevant to actual scientific knowledge acquisition' (Norton & Suppe, 2001: 103). Norton and Suppe note that models are useful precisely because they simplify the otherwise baffling complexity of the phenomena modeled. As atmospheric scientist Kevin Trenberth has noted, 'All models are of course wrong because, by design, they depict a simplified view of the system being modeled' (Trenberth, 1997).
4. This physicist thus also refers to the common practice of obtaining and using models developed by others. As noted above, atmospheric scientists often modify models obtained from elsewhere, complicating clear identification of users versus producers, as well as the exact site of production.
5. I base this on my years of fieldwork among such meteorologists. This claim is also supported by the proportionally large representation of synoptically trained meteorologists among signatories of petitions designed to weaken policy action on behalf of human-induced climate change. See for instance S. Fred Singer's 'Leipzig Declaration' (Olinger, 1996). See also <www.sepp.org/pressrel/meteorLD.html>.

6. One veteran synoptic research meteorologist expressed his exasperation in a 1999 letter to the Undersecretary of Commerce for Oceans and Atmosphere and Administration of the National Oceanic and Atmospheric Administration (NOAA). Noting his significant record in a particular area of weather forecasting, he criticized NOAA's bias towards numerical climate prediction methodology. He suggested that rival climate forecast methodologies were being squashed to 'prevent embarrassing comparisons'. In an interview with me, he claimed to speak for many fellow colleagues trained in synoptic methods, and my subsequent interviews among synopticians confirmed his claim.

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