

# 9 Hot Air: The Psychology of CO<sub>2</sub>-Induced Climatic Change

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Since 1957, the Mauna Loa Observatory has recorded a yearly increase of approximately one-third of 1% in the CO<sub>2</sub> (carbon dioxide) concentration of the Earth's atmosphere (Bacastow & Keeling, 1977). Although CO<sub>2</sub> concentrations are minute (on the order of 330 ppm), they play a major role in regulating the Earth's climate, particularly its temperature. Current projections indicate a doubling of CO<sub>2</sub> levels by the year 2030 with a consequent average warming (at the surface) of 3–4°C. Perhaps more important than the mean change is the differential warming at different latitudes, from 2°C at the equator to 11°C at the poles (Manabe & Wetherald, 1975). Reduction of the temperature gradient between different latitudes will likely mean a substantial reduction in atmospheric and oceanic circulation. One can anticipate marked changes in fisheries and agriculture, with a melting of the polar ice caps a distinct possibility.

Although point predictions are extremely unreliable at the moment, it seems to be a reasonable assumption that CO<sub>2</sub>-induced variations in temperature and precipitation patterns will lead to "better weather" in some places and "worse weather" in others. If, for example, climatological patterns return to those characterizing the Altithermal Period 4000–8000 years ago, when the world was several degrees warmer than it is now, the habitability of the Canadian prairies and the Sahel would increase as the former became warmer and the latter wetter. On the other hand, reduced precipitation in the southern Great Plains of the United States would remove vast acreages from the stock of arable land (Kellogg, 1978). However, even areas with improved climate might be adversely impacted by such changes. The legacy of industrialization and colonialism has been the establishment of highly specialized and relatively inflexible land use and agricultural patterns. An underdeveloped equatorial country might have neither

the time nor the capital to exploit the opportunity to shift from field crops to rubber and coffee; the hybridized grains used so successfully in the central United States might not prove as viable in the thinner soil of the upper Great Plains, which would then enjoy the best temperature-precipitation combination. International complications might prohibit some countries from exploiting their own technical ability to adapt. Iceland might be unable to pursue the fish on which its economy depends if shifts in ocean currents carried them within the 200-mile limits of other countries; power politics and local corruption might impede the flow of food stocks needed to tide some countries over as they adjust their agriculture to new conditions.<sup>1</sup>

Even where adaptations are technically and politically feasible, they may not even be attempted unless reasonable assurances can be given that they will be successful. A key piece of information in this respect is a guarantee that the climate is in fact changing. Unfortunately, the natural variability in climatic patterns is so great that it is difficult to identify either cycles or secular trends even in the absence of perturbations like that introduced by the increase in CO<sub>2</sub>. The impact of changes in CO<sub>2</sub> is deduced with the help of general circulation models (GCM's), which simulate the results of overlaying such changes on approximations of today's climate. Unfortunately, these models must be based upon incompletely verified climatological theories and subjectively assessed model parameters (e.g., about oceanic currents and their interaction with the atmosphere). Not only are both present and future climates insufficiently understood, but they are also unlikely to yield clearly diagnostic signs that changes are afoot and action is needed. Indeed, for the 20 years that reliable CO<sub>2</sub> observations are available, global temperatures have actually been decreasing (*Climate Change to the Year 2000*, 1978; *World Climate Conference*, 1979).

Understanding and coping with the origins of the CO<sub>2</sub> build-up is fraught with the same problems as dealing with its impact. Measurement of the change itself is still part art, part science, although experts seem to be reaching consensus on acceptable procedures. No one is entirely certain that the recent upturn is not one leg of a natural cycle whose previous stages neither were observed nor are retrievable from geologic records. Current accepted wisdom is that the origins of the build-up are international. The two leading culprits are increased use of fossil fuels (CO<sub>2</sub> being a major by-product of combustion) and logging of forests, particularly in the tropics (with both burning of the trees and elimination of a natural CO<sub>2</sub> sink contributing to increased atmospheric concentrations). Because the benefits of the activities leading to the build-up are as unevenly distributed as

<sup>1</sup>A chilling reminder of economic constraints on cooperation in time of need is the fact that the massive starvation in the Sahel during the drought of 1968-1973 reflected a minor shortfall in the quantity of grain available. The needed grain was certainly available in the world market. The missing ingredients were adequate distribution systems and resources sufficient to allow the people of the Sahel to acquire the grain (Glantz, 1976).

the costs and benefits of its consequences, unprecedented international cooperation would be needed to halt the change. Deciding that such a halt was desirable would require not only changes in attitudes toward nations, but changes in attitudes toward nature. One prominent agronomist<sup>2</sup> has stated that even were a CO<sub>2</sub>-induced climate change in the offing, it would constitute "not a problem but an opportunity" to master nature in new ways.

A final complication in establishing what is really happening lies in the interface between the politics of science and of nations. For proponents of nuclear power, CO<sub>2</sub>-induced climatic change is a consequence of using fossil fuels that reduces the relative riskiness of the nuclear option. For students of the atmosphere and oceans, concern over the problem potentially posed by CO<sub>2</sub> promises an outpouring of resources for the study of theoretical problems that have now become quite actual. One can only speculate on the impact of this context on the questions that are posed and the way answers are reported.

## INTERLUDE

Why am I telling you all this? Because at least a portion of the natural scientists dealing with this problem have realized that it cannot be managed without some substantial recourse to social scientists. The origin, consequences, prevention, and mitigation of this problem (if one exists) are all societal. All the natural scientists can do is fill in the gap between CO<sub>2</sub> build-up and physical consequences. Even that they cannot do without some societal analysis indicating by how much human activities are likely to upset the natural CO<sub>2</sub> balance, how climatic changes are likely to affect that imbalance, and what information decision makers need to have as outputs from the climatologists' models.

In fact, the problem is acknowledged to be so complex that psychologists have been asked for help (U.S. Department of Energy, 1980). This chapter is one attempt to assess what psychologists do and can know about how people deal cognitively with this environmental event with social causes and consequences. I would consider it successful if I could (1) make a convincing case that psychologists do have something to offer; (2) identify in this context some issues that our psychology has heretofore missed; (3) help define by example how one might approach related problems; and (4) recruit some of you to work on this particular interface between psychology and the world.

The prospect of CO<sub>2</sub>-induced climatic change poses a series of interlocking decisions to be made by individuals, groups, national, and international bodies. At each level, people must decide whether the problem is worth attending to and, if so, should efforts be made to prevent the build-up from happening (e.g., by

<sup>2</sup>Sylvan Witwer at the Department of Energy-AAAS Conference on CO<sub>2</sub>-Induced Climatic Change, Annapolis, Maryland, April 6, 1979

drastically restricting the consumption of fossil fuels), to implement curative schemes (e.g., massive afforestation programs), to adapt to the new world we are creating (e.g., by developing new crops or moving large populations), or to promote the build-up (for those who hope to benefit from the change). Each decision requires an assessment of what is happening, what the possible effects are, and how well one likes them. The quality of these assessments at one level constrains the wisdom of the decisions made at others. Failure of the U.S. to adopt a coherent policy is likely to thwart any international effort. Absence of international cooperation may lead U.S. consumers to feel "why should we drive less when the Brazilians provide tax incentives for logging out the Amazon?" We are all in trouble if the climatologists seriously understate or overstate how much they know. How such assessments are made, by consumers, legislators, diplomats, or scientists, would seem to be eminently psychological questions.

### WHAT'S TO KNOW?

Obviously, people respond to problems as they see them rather than to problems as they are. The importance of cognitive representations in coping with CO<sub>2</sub>-induced climatic changes is particularly great because the evidence on causes, effects, and intermediary processes is almost entirely abstract. One cannot directly sense what is really important (e.g., CO<sub>2</sub> concentrations, atmospheric refraction), and what one can sense is often misleading (e.g., random weather fluctuations).<sup>3</sup> Both the content and quality of our response hinges on the validity of our (cognitive) understanding of what is happening to us and our world.

This chapter attempts a psychological analysis of the kinds of information that one must understand in order to be on top of the CO<sub>2</sub> situation. These generic types include very low probabilities, conflicts between technical experts, and descriptions of gradual changes buried in noise. A more extensive list appears in Table 9.1. Regarding each type of information, one should ask a series of questions:

1. What are its formal properties?
2. What are its observable signs?
3. How are those signs revealed to the individual?
4. Are they contradicted, supported, or hidden by immediate experience?
5. Do people have an intuitive grasp of such information?
6. To the extent that they do not have such a grasp, what is the nature of their misunderstanding?

<sup>3</sup>According to one leading climatologist, every time there is a major snowstorm in his area, the local news media call him to find out if this is the climatic change he has been predicting (Schneider, 1979)

TABLE 9.1  
Nature of the Issues in CO<sub>2</sub>-Induced Climatic Change

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<i>Properties of the Information</i>	
High level of uncertainty	Critical observations often missing or questionable
	Critical assumptions often unproven
Uncertainty is poorly formulated	
Hard to assess	
Hard to communicate	
Subject to distortion in transmission from experts to nonexperts	
Random error added	
Systematic error added	
Highly diagnostic information rare and unlikely	
Highly technical	
Enormous quantity	
<i>Properties of Process</i>	
Component processes	
Many simple, established causal relations	
Many involve conjecture in the absence of historical or contemporary data	
Complex interactions between components	
Understood only through simulation models	
Future may arrive before models with desired sophistication to simulate it can be developed	
Hard to assess adequacy of theoretical approximations	
Superimposed on poorly understood natural changes and cycles	
<i>Properties of Effects</i>	
Very low probabilities for many of most interesting	
Involve destabilization of entire ecologies	
Secondary and tertiary effects often unidentified (much less measured)	
Resilience of human controls uncertain	
Often completely unfamiliar	
Hard to imagine	
Hard to evaluate	
Long time span for many	
Until they are felt	
Until they can be undone (if not irreversible)	
Benefits and costs distributed at different points in time and to different people	
Incommensurable	

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7. How great are such misunderstandings and how severe are their consequences?
8. Does natural experience provide feedback highlighting misunderstandings and inducing improvement?

If we hope to improve as well as predict performance, we must also ask:

9. Can the understanding be enhanced, for example, by generating better evidence, developing superior presentations, or altering basic approaches to knowledge?

These questions ask how suitable people's cognitive ecology is for coping with the informational ecology within which they live. The accepted wisdom among many students of judgment and decision making under conditions of uncertainty is that the match is far from perfect. In this view, people have neither the cognitive capacity nor structures for coping with complex, probabilistic problems. As a result, they resort to judgmental heuristics or short cuts or rules of thumb that allow them to reduce such problems to simpler and more familiar terms. These strategies are adaptive in the sense that they always produce some answer and often that answer is moderately accurate. They are maladaptive in that they can produce highly erroneous judgments and in that the great facility with which they are applied inhibits the search for superior methods. Identification of these limits might be traced to Miller (1956) and Simon (1956); specification of how people get around them has involved Tversky and Kahneman (1974), among others.

Despite the considerable progress made by cognitive, social, and organizational psychologists in elaborating these concepts, and demonstrating their robustness, it is by no means trivial to apply them to a particular situation. One source of difficulty is that some heuristics might better be described as metaheuristics (Einhorn, in press). They provide not judgments, but ways to produce judgments. Thus, they are given to varying interpretations. For example, the availability heuristic leads people to judge events as likely to the extent that exemplars are easily available in memory or imagination. Deciding what constitutes exemplars, how the memory search is conducted, and how ease is measured requires a detailed analysis of the situation under consideration. A second problem is that we know little about the ecological validity of heuristics (e.g., how often and how badly do they lead us astray?). For example, are more likely events generally more available? If heuristics are valid, we can trust people's intuitive judgments more and our own ability to explicate these processes less. For, there are many ways to explain good judgments and many fewer to explain any particular pattern of errors.<sup>4</sup>

Presumably, there is no general answer to this question; the application and validity of judgmental strategies must depend on the situation. Whenever an answer depends on circumstances, we need a theory of circumstances. Analyzing the psychological details of particular situations like CO<sub>2</sub>-induced climatic change is one path to developing a general capability for applying our theories.

Because there has been virtually no research on many of the kinds of information listed in Table 9.1, it seems most efficient to explore in depth what is and should be known about one kind of information that has been studied somewhat,

<sup>4</sup>Even though Kahneman and Tversky's research has come to be known for its identification of errors, they make no statement about how bad judgment is in general. They focused on errors both in hopes of finding one unique way to explain a pattern of errors than a pattern of success and because suboptimal behavior in conditions encouraging optimality suggests deep-seated cognitive tendencies.

that concerning low-probability events. This is done in the following section. Subsequent sections consider the possible results of similar analyses for other kinds of information, the implications of social realities on cognitive processes, the implications of cognitive processes on social realities, and the role of psychology in all this.

## LOW-PROBABILITY EVENTS

One fortunate feature of our environment is that the most fearsome events happen fairly infrequently. Major floods are confined to small regions and typically are infrequent there; disastrous plagues have been the exception rather than the rule; even the most seismically active areas experience catastrophic tremors at long intervals. In the realm of hazards of human origin, life-threatening endeavors are usually constrained to have a low probability of leading to disaster. Even nuclear power plants, one of the most troublesome of hazards in many people's minds, do not melt down very often (although one might find any epsilon of probability unacceptable).

CO<sub>2</sub>-induced climatic change involves natural effects of human origin. There, too, the worst effects seem to be quite unlikely. If one aggregated all expert opinions into a probability density function for the mean change in the Earth's temperature over the next 75 years, the result might be roughly bell shaped with a mean at + 1.5°C and 98% credible intervals at about -2°C and +7°C. Thus, the most dire consequences seem quite unlikely. The probability of either a new Ice Age or rapid melting of polar ice caps accompanied by inundation of coastal cities is small, although nonnegligible. If the climatologists are to be believed, however, there is a good chance of some regional dislocations due to changed precipitation, increased variability of growing season, reduced need for space heating, and the like.

Although they may have great economic consequences, such changes are unlikely to threaten the viability of a society, particularly as long as other countries and regions can lend support. It is the low-probability-high-consequence events about which one must really worry. Mistakes in understanding them and preventing or mitigating their consequences could push a society beyond the limits of its resilience. Unfortunately, there are both statistical and psychological grounds for expecting such events to be poorly understood.

### What Can Be Known?

At times, it is possible to identify a population of events from which an observed sample may be drawn as a step toward assessing the probability of a particular calamity. Most seismologists might argue that the United States Geological Survey (USGS), has perhaps 75 years of reliable records upon which to base

assessments of the frequency of large earthquakes in various parts of the U.S. The copious records of ice-pack movements maintained in Iceland over the last millenium provide a clue to the probability of an extremely cold year in given future periods. The apparent absence of a full-scale meltdown in the 500-1000 years of nuclear reactor operation may allow setting some bounds on the probability of future meltdowns. Of course, extrapolation from any of these historical records is a matter of judgment. Changes in design, public scrutiny, and federal regulation may render the next 1000 reactor years appreciably different than their predecessors. The new conditions created by increased CO<sub>2</sub> concentrations may artificially change climate variability in a way that amplifies or dampens yearly or daily fluctuations.

Even if experts were to agree on the relevance of these records, a sample of 1000 reactor or calendar years may be insufficient. Given the magnitude of possible consequences, a .0001 chance of a meltdown might be deemed unconscionable, but we will be well into the next century and irrevocably committed to nuclear power and its consequences before we will have enough hands-on experience to assess the probability of a meltdown to the desired accuracy. We know that meltdowns are unlikely (in the present sense), but whether they are unlikely enough may not be known until it is too late, or it may not be known at all.

When no historical record is available upon which to base conjectures, one is left with conjecture alone. In the scientific community, the more sophisticated conjectures are based upon models. GCM's represent one such genre, the fault-tree and event-tree analyses of a loss-of-coolant accident upon which the "Rasmussen" Reactor Safety Study was based (Atomic Energy Commission, 1975) represent another. Each is composed of component processes and interactions between them that are known to some degree of precision.

The fault tree involves a logical structuring of what would have to happen for a core to melt down. If sufficiently detailed, it will reach a level of specificity for which we have relevant experience (e.g., the operation of individual valves). An overall probability of failure for the system is determined by combining the needed component failures. Unfortunately, some components are entirely novel or have never been used in these particular conditions; their performance parameters must be guessed. Furthermore, the logical structure and completeness of the tree are more or less matters of opinion.

GCM's share the same strengths and weaknesses. They attempt to predict the unknown world of heightened CO<sub>2</sub> concentrations on the basis of related observables and their hypothesized interconnections. These are, respectively, recorded atmospheric and oceanographic conditions and generally accepted theories of their dynamic interaction. As with fault trees, some of the data are uncertain and some of the logic is disputable.

Thus, critical low probabilities are often revealed through the filter of formal analyses rather than through direct experience. One's faith in the probabilities so

revealed depends on the success of the analysts in identifying all relevant components, assessing their values, and understanding their interrelations. Recent psychological research suggests some likely bounds on their success and our faith. People apparently have limited ability to recognize the assumptions upon which their judgments are based, appraise the completeness of problem representations, or assess the limits of their own knowledge. Typically, their inability encourages overconfidence (Fischhoff, Slovic, & Lichtenstein, 1977, 1978).

One might hope that the results of this research conducted on laypeople could not be generalized to technical experts, that somehow their substantive knowledge and training would lead to improved judgment when forced to go beyond the available data. Unfortunately, a modicum of systematic data and many anecdotal reports suggest that this is not the case. As a case in point, a high-level peer review found that the Reactor Safety Study had greatly overstated the precision of its conclusions (Nuclear Regulatory Commission, 1978).<sup>5</sup> The unpleasant surprise at Three Mile Island demonstrated that it had not included all pathways to disaster<sup>6</sup> nor even explicitly raised a number of implicit and erroneous assumptions (e.g., that trained personnel would always be available). For their part, GCM's necessarily omit some aspects of the environment believed to be relatively unimportant (for the sake of manageability) and incorporate untested assumptions provided by other disciplines (e.g., that the rate of increase in CO<sub>2</sub> production of the last 20 years will continue unabated in the future, in a world that may have more or less nuclear power, war, recession, and environmental awareness than its predecessor). They seem poorly suited for even providing guesses at their accuracy.

If one reads such analyses and the rare subsequent evaluations with an eye to the psychology of the analyst, there seem to be generic sources of error and omission. These include: (1) failure to consider the imaginative ways in which human error can mess up a system (e.g., the Browns Ferry fire in which the world's largest nuclear power plant almost melted down due to a technician's checking for an air leak with a candle, a direct violation of standard operating procedures); (2) insensitivity to the assumptions an analysis makes about constancies in the world in which the system is embedded (e.g., no major changes in government regulatory policy); (3) overconfidence in current scientific and technological knowledge (i.e., assuming that there are no new chemical, physical, biological, or psychological effects to be discovered); (4) failure to see how the system functions as a whole (e.g., a system may fail because a back-up component has been removed for routine maintenance).

<sup>5</sup>A specific contribution of psychology to improving the practice of formal analysis is suggested by the experimental finding that the elicitation procedure used by Rasmussen's team for assessing failure rates produces judgments with particularly exaggerated precision (see Fischhoff, 1977).

<sup>6</sup>One intriguing limitation of fault-tree analyses is highlighted by the fact that it is something of a moot point whether or not the Three Mile Island sequence was included in the Reactor Safety Study (Whipple, 1979).

### What Can They Tell Us?

Low-probability events reveal themselves to experts through systematic samples and formal models, each with their strengths and weaknesses. They reveal themselves to nonexperts through unsystematic experience and reports from the front by experts, seers, and the media that traffic in such reports.

To make use of what the experts tell us, we must understand both the substance of their message and the qualifications that (should) accompany it. An obvious limit on our ability to understand substance is having the report couched in unfamiliar technical terms. These can mislead (say, when technical terms have common-language counterparts with different meanings), confuse (perhaps leading us to think that we understand when we really do not), and dissuade us from even attempting to understand.

Obviously, most scientific problems afford opportunities for asserting some sort of elite control. However, even well-meaning attempts to inform the public may go astray. CO<sub>2</sub> issues make a terrific chalk talk, but their impact may be lost if care is not taken to draw causal links between the parts (Tversky & Kahneman, 1980), particularly those links connecting human behavior and climatological consequences. Without such explicit ties, a CO<sub>2</sub> crisis may appear implausible as well as improbable. On some level, it may be hard to believe that global cataclysm might be the result of such innocuous and sensible acts as lighting home fires and burning leaves. The CO<sub>2</sub> problem represents a global commons dilemma in which seemingly inconsequential individual decisions combine to produce universally averse consequences in the long run. Although moralizing might lead to more prosocial behavior (Dawes, 1980), it is likely to have little effect until recipients are convinced that a dilemma exists.

Even if people are willing to listen, it may be difficult to present low probabilities to them comprehensibly. Is, for example, the difference between .001 and .0001, so stated, meaningful to people? Scattered evidence suggests that people may ignore or exaggerate probabilities in that range (Lichtenstein, Slovic, Fischhoff, Layman, & Combs, 1978; Slovic, Fischhoff, Lichtenstein, Corrigan, & Combs, 1977). One alternative is to provide a concrete referent in the form of a familiar event with an accurately judged probability of similar magnitude. The efficacy of this (or any other) procedure for communicating low probabilities is still undocumented (Fischhoff, 1977).

As a guide to action, the uncertainty surrounding the experts' best guess may be as important as the substance of the guess. One wants to know "Just how high could it be?" and "Do these experts know enough for me to take their best guess seriously?" A good deal of evidence (e.g., Gettys, Kelley, & Peterson, 1973; Kahneman & Tversky, 1973) suggests that were such qualifications provided, they would not be used properly. In particular, people seem to be as confident making inferences from highly unreliable data as from reliable data, rather than

less confident as statistical theory dictates.<sup>7</sup> If, as previously suggested, there is also a propensity for experts to exaggerate how much they know, one should expect a gap between the credibility afforded to scientific analyses and what they merit.

Another form of credibility problem arises when the integrity of the source is threatened. Most people probably have learned to discount what they see on TV because of its tendency to sensationalize. Whether they are aware of the subtle biases that can enter into scientific analyses may be another question. For example, the very raising of CO<sub>2</sub> questions rather than those surrounding other hazards of potentially greater magnitude<sup>8</sup> may reflect a desire to make life easier for one domestic energy industry (nuclear); not raising them may reflect a desire to obscure international energy issues (the fact that the industrialized countries are enjoying most of the benefits of creating the CO<sub>2</sub> imbalance whose costs will be borne by everyone). Whenever uncertainty is as rampant as it is with CO<sub>2</sub>-related issues, there is ample opportunity to fudge results (say, by making small and, one hopes, unchallengeable changes in many parameters leading to a large overall effect) or manipulate the reported conclusions. For example, the executive summary of the Reactor Safety Study has been found to have limited fidelity to the body of the nine-volume report (Nuclear Regulatory Commission, 1978). Perhaps closer to home, climatologists, like psychologists, may often be tempted to describe their work so that firm conclusions seem close enough to merit funding but just far enough around the corner to prevent rigorous evaluation of the product.

### What Can We Learn by Ourselves?

Unlike nuclear power, climate is directly experienced. That experience may set us wondering about the likelihood of major climatic changes (say, as did the recent West-coast drought and severe Northeastern winters). Once we are interested, that experience may support or contradict what the experts tell us (as the drought and cold seem to do, respectively), with regard to CO<sub>2</sub> projections. Often, personal experience will be all we have to go on.

How good are we at assessing low-probability natural events? A popular

<sup>7</sup>Glenn Schweitzer (1975), Director of the Office of Toxic Substances in the Environmental Protection Agency, has commented: "Too often lawyers and economists seize upon (statistically derived) numerical risk factors forgetting that these experimentally derived estimates may in fact have a very shaky relevance to the real world [p. 73]." See also Lodge (1976).

<sup>8</sup>Despite the enormous destructive potential of earthquakes in the U.S., and the fairly high likelihood of their occurrence, almost no research is going into improving human response. Seismological research designed to develop the capacity for earthquake prediction is, however, well funded, despite some serious suggestion (National Academy of Sciences, 1978) that the expected value of forecasts is negative, once one considers social reactions to them.

experimental design in the 1950's and 1960's had subjects try to learn the relative frequencies of successively presented events drawn from a multinomial distribution (usually binary events). As summarized by Peterson and Beach (1967), these studies found people to be quite good at the task. Unfortunately for the problem at hand, the target events in these studies were never of very low frequency or distributed over long periods of time amidst appreciable noise, as are natural hazards. Moving several steps in the right direction, Lichtenstein et al. (1978) asked people to judge the likelihood of a randomly selected individual dying from a variety of recognizable, but not necessarily common causes (e.g., botulism, tornadoes, cancer). They found that people (1) had a pretty good idea of the relative frequency of most causes of death; (2) substantially underestimated the differences in the likelihoods of the most and least frequent; and (3) persistently misjudged the relative likelihood of those causes of death that are unusually available (e.g., tornadoes) or unavailable (e.g., asthma). Slovic, Fischhoff, and Lichtenstein (1979) found a similar pattern of results in estimates of the fatalities from various technological hazards.<sup>9</sup>

Judgments of fatalities for unlikely events are, of course, not quite the same as judgments of their very low probabilities. In a study asking people about the lethality of some of these same causes of death (i.e., the probability of dying given that one was afflicted), we found that formally irrelevant changes in response mode produced appreciable differences in assessed probability (Fischhoff, Slovic, Lichtenstein, in press). For example, death rates derived from responses to the question "For every 100,000 afflicted, how many die?" were roughly two orders of magnitude greater than those in response to "For every individual who dies, how many are afflicted but survive?" These differences seem due in part to the effect response format has on how people access their knowledge, which is probably not naturally organized along these lines, and in part to variants of the well-known effects that the choice of numbers in magnitude estimation experiments has on the results of those experiments (Poulton, 1968).

Geographers' studies of lay assessments of the likelihood of (rare) natural hazards yield a mixed bag. For example, a survey by Hewitt and Burton (1971) found that residents of London, Ontario, had quite accurate judgments regarding the likelihood of hurricanes and tornadoes, typically overestimated the frequency of floods, and were split between overestimating and underestimating the frequency of ice storms and blizzards. Surveying a variety of such studies in various countries, Burton, Kates, and White (1978) concluded that the accuracy of judgments increases with length of time in the area, regularity of exposure to

<sup>9</sup>They also found that although laypeople agreed with most experts' assessments of the expected fatalities from nuclear power in an average year, they saw much greater likelihood for a major disaster and much greater consequences should one occur. Given the present state of uncertainty regarding nuclear power, it is unclear whose cognitive abilities are to be trusted more.

nature, periodicity of the hazard, and invulnerability from effects of hazard. These factors do not bode too well for responsiveness to CO<sub>2</sub>. Furthermore, even in situations in which people have fairly accurate assessments, their notion of underlying mechanism may be quite different than the accepted scientific view. For example, an atypical period of rainy weather following the first agricultural settlements in the High Plains of the U.S. led to the belief (endorsed by the AAAS) that "rain follows the plow." The more normal and drought years following the breaking of the sod resulted in tragic disruption of lives and loss of topsoil (Kates, 1962; Opie, 1979).

Although many of the climatic fluctuations and meteorological events that may be affected by possible CO<sub>2</sub> changes have some natural, semiobservable frequency, the event itself does not. In fact, one directly sees little or nothing to indicate that some global dislocation may be on the way as a result of commonplace actions taken by all the Earth's denizens. Those who have not heard the cry of alarmed climatologists (e.g., Bryson, 1974; Schneider & Mesriow, 1976; Wigley, Jones, & Kelly, 1980) are doubtless worrying about other things. While everybody is doing something about the weather, no one is talking about it.

### Do We Have to Know All This?

Given the complexity of the problem, one cannot expect anyone to know the future regarding these low-probability-high-consequence effects of a possible CO<sub>2</sub> build-up. Given our cognitive limitations, it may be unrealistic to expect very many people to have *optimal* judgments, properly incorporating all that could be known at present. Then, one must ask whether what we do understand is good enough. Is satisficing satisfactory?

In effect, one needs an error theory predicting the price paid for human frailty. Creating such a theory requires not just a consideration of the suitability of cognitive processes for understanding natural processes, but some idea of what decisions are to be made on the basis of our (mis)judgments. That is, what will be on the test? Without embarking on a detailed analysis of the personal and collective decisions that might be involved in trying to prevent a CO<sub>2</sub> crisis or mitigate its consequences, we might note a few general principles. The same reasons that make it difficult to assess low probabilities make it nearly impossible to evaluate those that we or others produce. As a result, spotting and correcting errors (i.e., learning) might be quite slow (Einhorn & Hogarth, 1978). In a consideration of formal risk-assessment procedures, Kastenbergh, McKone, and Okrent (1976) found that assessments were extremely sensitive to outliers. Thus, the appropriateness of our decisions to take seriously or discount unusual events may be quite important. Von Winterfeldt and Edwards (1973) showed that under quite general conditions, modest inaccuracy in assessing probabilities (or utilities) should not have too bad an effect on decisions with continuous options (e.g.,

invest \$X or increase production by Y%). However, Lichtenstein, Fischhoff, and Phillips (1977) showed that such mistakes can be quite costly when decision options are discrete (e.g., operate/do not operate). When dealing with low probabilities, a modest underassessment may push the event below the threshold of concern; further underassessment may mean not only that nothing is done, but that the topic is not even monitored for future signals. On the other hand, overassessment may leapfrog the event over other low-probability-high-consequence events in our hierarchy of concerns and lead to the neglect of more important issues. Many advocates of nuclear power feel that its risks have been accentuated to the detriment of concern over the effects of fossil fuels (like CO<sub>2</sub>-induced climatic changes).

Insensitivity of decision outcomes to errors would make it hard to go too far astray and hard to learn very much from experience. It is hard to tell when one has done a poor job of assessing very low probabilities; as a result, the skill may be lacking for those few instances in which one must get such assessments right. The difficulties of learning are exacerbated by the tendencies to state probabilities in vague, nonnumerical terms (Lichtenstein & Newman, 1967) and to reconstruct or misremember our own predictions to have been wiser than they actually were (Fischhoff, 1975). In the absence of any explicit instruction in dealing with low probabilities, any cognitive representations or manipulative skills that are not acquired naturally are not likely to be acquired at all.

### RECAP

Thus, there are limits to what our natural and technological environment will reveal about the probability of the unlikely catastrophes it harbors. Not only does such information emerge of its own accord at a slow rate, but the best efforts of historians and modelers will not coax forth the whole story. This partial picture of an obscure environment reaches decision makers, be they legislators, regulators, industrialists, or laypeople, through the filters of science and the news media reporting them. Using that information requires some analysis of the physical limits on the acuity of its sources, as well as the random and systematic error that the filters might induce (e.g., through endemic overconfidence or self-serving biases). Decision makers must also integrate what they are told by the experts with what they are told by their senses and comrades.<sup>10</sup> This task is complicated by the facts that probabilities may be experienced and reported in forms that are hard to compare and that the definitions of relevant events and

<sup>10</sup>With the exception of social-comparison theory (Festinger, 1954), the role of indigenous social-support systems is generally ignored in the decision-making literature. Other people would appear to serve as an important source not only for information but also for decision rules (e.g., Kunreuther, Ginsberg, Miller, Sagi, Slovic, Borkan, & Katz, 1978). Discussion with others would also serve to expand an individual's information-processing capacity by externalizing some of the informational load, akin to human peripheral processors.

universes may be poorly specified. Finally, there is reason to suspect that we lack some of the cognitive repertoire needed to handle such information effectively and that our normal experience is poorly structured to inculcate such skills.

As mentioned, low probabilities were taken as a case in point for how one might develop the formal and psychological properties of a class of information encountered in various guises in CO<sub>2</sub> issues. Some hints at what analogous explorations with other kinds of information might reveal follow. The theoretical purpose of such analyses is to develop a broadly applicable theory of judgment based on the match between informational and cognitive ecologies. Doubtless there are other ways to characterize the situations within which individuals exercise their cognition. Informational types was chosen because of its generality and because it allows the exploitation of the many existing studies of responses to various kinds of information (albeit conducted without reference to their prevalence or distribution). Because psychologists have used information types in the past, one might expect them to be readily able to fill in the research gaps identified by an analysis like the present one. A conceptual scheme that raises answerable questions would seem to have something going for it.

The practical purpose of such an analysis is to anticipate people's perception of a situation as it naturally impinges upon them. This in turn should give us some idea of how and how well they would respond to it in the absence of any intervention. Finally, it should clarify somewhat the efficacy (or wisdom) of various manipulations designed to help people understand what is happening to them. From this perspective, it is not a foregone conclusion that "expert" intervention is superior to people's natural coping strategies. Nonetheless, it does seem that the world in which we, as individuals and as a species, have acquired our cognitive skills may be different from our present world, with its potential for socially induced climatic change and other forms of irreversible environmental degradation.

### WHAT ELSE IS THERE TO KNOW?

Coping with CO<sub>2</sub>-induced climatic change means comprehending a wide variety of information about possible effects and presumed underlying causes. Part of comprehension is an appraisal of the quality of that information. Having a low probability of occurrence is one quality of some possible effects, particularly the more drastic ones. Table 9.1 lists other formal or physical or objective properties of what one must know. The cursory discussions that follow sketch lines along which more protracted analyses might be conducted.

#### Properties of Information

As a guide to action, available information about climatic change has one most regrettable property: It is highly impeachable. This vulnerability stems from the same reluctance of the environment to reveal its secrets that plagues attempts to

assess the probabilities of unlikely consequences. Scientific understanding of what is happening right now is limited by the embryonic stage of some theories, the absence of reliable data on some key parameters, and the small amount of data regarding others. Climatologists face the usual problems of discerning a weak (if ominous) signal amidst noise, created in this case, say, by unreliable measurement and meaningless local variations. Their task is complicated by the presence of systematic as well as random error in their observations. Climate is subject to poorly understood fluctuations that are large in their amplitude relative to the signal in question here. As a result, they can mimic or obscure the effects of CO<sub>2</sub> build-up. Climatologists' faith in eventual warming despite the cooling trend of the last 20 years reflects such a belief in the masking potential of cyclical fluctuations. Historical climatology provides some hope of reducing the heroism in this particular leap of faith by reconstructing past climates on the basis of documentary and physical evidence (Ingram, Underhill, & Wigley, 1978).

Evidence about the limits of evidence might reach nontechnical audiences in a number of ways (if it reaches them at all). One is through explicit statements of uncertainty; the problems of formulating and communicating such statements was discussed earlier. A second is through observation of disagreements between experts. Unless the audience has an appreciation of the naturally disputative and accretive character of science, its resolution of the conflict may not be a balanced and informed weighing of the sides. Alternative resolutions are doubting the probity of the disputants, siding with the most assertive (or colorful or optimistic or certain), or deciding that "anything goes" and that "my guess is as good as yours." Such potential for misleading or confusing the public by contradictory or premature pronouncements poses for climatology quality-control problems that most sciences are ill-equipped to handle. To take an example from another realm, even though most seismologists might agree that earthquake prediction is unfeasible at the moment, they would be hard pressed to cope with a freelancer publishing daily forecasts in the *L. A. Times*. Another path to recognizing uncertainty is through observing the conflict between one's own sensory experience and the pronouncements of climatologists. Our great facility for interpreting short-term fluctuations may seriously restrict the visibility of long-term trends (Fischhoff, 1980).

### Properties of the Processes

Making sense of a natural or social process often means interpreting it in terms of some sort of causal scheme. If carefully explained, many aspects of CO<sub>2</sub>-induced climatic change should make sense to most people. The causal agents are for the most part large, visible objects. Their interaction is through such physical processes as ice fields reflecting sunlight and wind mixing water. One can see how that works. Modest stretches of the imagination may be needed to visualize other aspects—for example, the large negative effects that minute concentrations of

such a common chemical as CO<sub>2</sub> can have or the fact that many innocuous individual decisions can produce such an immense common dilemma. More difficult may be comprehension of causes whose impact has a very long lag time—for instance, the extent to which heavy capital commitment to some production and consumption patterns reduces social resilience and precludes many responses to crises, or the "time bomb" inherent in the enormous number of young people in the world today, with a lifetime of reproduction and overproduction of CO<sub>2</sub> before them.

Still other processes cannot by their nature be understood. GCM's are used because climatologists realize that one cannot simulate in one's head the interaction of a large number of systems, even if they are individually quite simple. After throwing everything into such a model, one must trust the results. What happens when those results are unwanted politically or counterintuitive (i.e., not believed) psychologically? There may be a strong temptation to try it again, adjusting the parameters or assumptions a bit. Any persistent tendency to yield to such temptation could generate a systematic and not easily detected bias in the results of modeling efforts. The debate arising out of Meadows, Meadows, Randers & Behrens' *Limits to Growth* (1972) or Forrester's *World Dynamics* (1973) may have petered out because there are no clear standards on the acceptability of models. Everyone agreed that the models in question were somewhat wrong and somewhat oversimplified, but no one could tell quite what that meant.

### Properties of Effects

Because climate is part of our lives, we should, it would seem, have no trouble comprehending what the outcomes of CO<sub>2</sub>-induced changes are (e.g., what it means to have an average increase of 2°C). There are, however, a number of reasons to doubt this presumption, all of which have analogues in the reasons for doubting the assumption that because we all live in society, we would be able to understand the meaning of a projected shift in one of its parameters (e.g., an increase in the median age or percentage of handicapped or price of fuel). One is that we do not experience our environment directly; rather, we have about us a series of defenses that regulate contacts so as to make them more pleasant and less demanding. Air conditioning and social norms are two obvious examples. We may have little idea of what life would be like if the conditions to which that veneer of civilization was adapted were changed.

A related reason for doubt is that we experience weather not climate, people not society. As a result, we seldom have to confront the complexity of the natural and social ecologies within which we live. We may not realize that an older world threatens the bankruptcy of the social-security system or that a warmer world will eliminate the hard freezes that keep pests from destroying susceptible crops in some regions. Although the connections are straightforward and com-

prehensible when drawn, one should not expect either experts or laypeople to recognize spontaneously the secondary or tertiary effects of projected changes.

Finally, no one knows how well people are able to imagine dramatic changes or, conversely, to what extent they are prisoners of their own experience. Do any of us who have not suffered that unmaskable pain of cancer know what it means? (If we did, would anyone start smoking?) What presumptions about unalterable aspects of human nature constrain our imaginations regarding, say, what awaits us in foreign countries or prison? Can we flesh out projections of climatic conditions outside of our species' experience? Can we really know what it will be like to live in the greenhouse? Without that experiential understanding, can we act appropriately to the possibility? A related argument is used by some foes of nuclear power who say that because we cannot grasp the time span during which some radioactive wastes must be stored, we should avoid the whole business; without basic comprehension, wise decision making is infeasible.

Understanding effects requires not only factual knowledge but also an evaluative assessment. Do we want this to happen? How badly? Such questions would seem to be the last redoubt of unaided intuition. Who knows better than an individual what he or she prefers? When one is considering simple, familiar events with which people have hands-on experience, it may be reasonable to assume that they have well-articulated opinions. Regarding the novel, global consequences potentially associated with CO<sub>2</sub>-induced climatic change or nuclear meltdowns, that may not be the case. Our values may be incoherent, not thought through. In thinking about what are acceptable levels of risk, for example, we may be unfamiliar with the terms in which issues are formulated (e.g., social discount rates, miniscule probabilities, or megadeaths). We may have contradictory values (e.g., a strong aversion to catastrophic losses of life and a realization that we are not more moved by a plane crash with 500 fatalities than one with 300). We may occupy different roles in life (parents, workers, children) that produce clear-cut, but inconsistent values. We may vacillate between incompatible, but strongly held, positions (e.g., freedom of speech is inviolate, but should be denied to authoritarian movements). We may not even know how to begin thinking about some issues (e.g., the appropriate trade-off between the opportunity to dye one's hair and a vague, minute increase in the probability of cancer 20 years from now). Our views may undergo changes over time (say, as we near the hour of decision or the consequence itself) and we may not know which view should form the basis of our decision (Fischhoff, Slovic, & Lichtenstein, 1980).

If asked, people can usually find some way to evaluate almost anything. But if their values are poorly articulated, that way may tap only a part of what they feel and may lead to responses not in their own best interests. As a result, the particular or peculiar way that issues are posed by nature, scientists, politicians, and the media may have great power over just what responses emerge as apparent expressions of people's values.

## COGNITIVE POLITICS

Real problems produce genuine conflicts of interests. Thinking about them might not only help protect our science from unwanted pressures, but also uncover some presumptions that are typically neglected.

One might start by asking "Why would anyone want to ask psychologists for their opinion about such policy issues?" A naive answer, doubtless with some truth, is that decision makers and natural scientists realize that they do not have all the answers; the source and resolution of this environmental problem lie in social and cognitive processes. If that is the case, one might ask "How far will they let us go?" For example, can we work on those topics traditionally in the stranglehold of economics or will our funds be restricted or, more ominously, will we censor our own inquiries to avoid areas in which we are not wanted? Are the natural scientists sufficiently committed to allow their people to work with us or do they maintain the prejudice that interdisciplinary research is the domain of people who cannot cut it in their own field and is thus a no-no for anyone interested in promotion and tenure? If that is the case, does it mean anything more than an ego-weakening rebuff or will restricted communication imperil the validity of our research?

A less naive answer to "Why did they ask us?" is that our basic paradigm embodies a political perspective appealing to some. Perhaps we are seen as contributing to a stratification with a technical elite near the top. Maybe experts are frustrated by the refusal of laypeople to believe their analyses, feeling "the public is crazy. Let's bring in some psychologists to solve this clinical problem." Our focus on individuals rather than social or political institutions may be conducive to some people of power. On the other hand, our interest in facilitating communication with laypeople may cast us as populists concerned with enfranchising and empowering nonexperts by increasing their ability to act in their own best interests. Our focus on what people can be taught to do may make us a healthy antidote to claims of lay incompetence (that stupid, emotive public). What we actually are probably lies between these extremes and depends on how we envision, shape, and disseminate our work. Again, even though names can never hurt us, labels can lead us to define our research task in ways that build in incapacities and blind spots that we could, in principle, avoid.

Although we may be invited at the behest of natural scientists and political decision makers, we should not hesitate to tell them how to run at least a part of their lives. We probably know some things about the foibles inherent in their modes of analysis that suggest altered approaches and reasons for caution (see the section "What Can Be Known" earlier in this chapter). We also may know something about what the body politic wants to know and how it might respond to various messages. Such information could serve as the basis for manipulating opinion or for making big science more responsive to the public that pays for it. For example, we might tell legislators never to order cost-benefit analyses be-

cause it is impossible to provide a clear exposition of their assumptions; we might tell climatologists that instead of trying to understand the full picture of what will happen, they should try to produce one clear diagnostic sign that something is really changing in the climate of sufficient magnitude to merit our attention.

## CONCLUSION

Psychology has learned a fair amount about the minutiae of cognitive and social behavior in the constrained environments common to our various studies. The complexity of behavior and its sensitivity to subtle alterations in those environments has served as a source of both frustration and inspiration. Elaborating the effects of such shifts has led, at least in some cases, to a moderately inclusive understanding of very small worlds. The approach attempted here has been to look at the psychological equivalents of the environmental properties of a very large world. Such analyses might tell us (1) what we know and need to know about the application of existing results to the real world; (2) whether there are important tasks and phenomena out there that have yet to find their ways into our studies; and (3) what we know already with sufficient confidence to be able to contribute to policy making.

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