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Reflections on the Scientific Process, as Seen in Climate Studies

How do scientists get reliable information about the world? When we are told of an "advance" of science, the language brings up a picture of people marching resolutely ahead. A scientist "discovers" something, as when an explorer of old pressed into an unknown valley. Other explorers push onward, taking knowledge a "step forward." That would be a "progress" in the old meaning of the word, a stately parade advancing according to plan.

In reality, after a scientist publishes a paper with an idea or observation, other scientists usually look upon it with justifiable suspicion. Many papers, perhaps most of them, harbor misconceptions or plain errors. After all, research (by definition) operates past the edge of the known. People are peering through fog at a faint shape, never seen before. Every sighting must be checked and confirmed. Scientists find confirmation of an idea all the more convincing when it comes in from the side, using some entirely different type of observation or line of thought. Such connections among different realms are especially common in a science like geophysics, whose subject is intrinsically complex. Scientists may start with something they learned about the smoke from volcanoes, put it alongside telescopic observations of Venus, notice the chemistry of smog in Los Angeles, and plug it all into a computer calculation about clouds. You cannot point to a single observation or model that convinced everyone about anything.

This doesn't look like an exploring team aiming into new territory. It looks more like a crowd of people scurrying about, some huddling together to exchange notes, others straining to hear a distant voice or shouting criticism across the hubbub. Everyone is moving in different directions, and it takes a while to see the overall trend. I believe this is the way things commonly proceed not only in geophysics but in most fields of science.

In these essays I have tried to show this process by connecting the dots among roughly a thousand of the most important papers in the science of climate change. For each of those thousand, scientists published another ten or so papers of nearly the same importance, describing related data, calculations, or techniques. And for each of those ten thousand, specialists in the particular subject had to scan at least ten other publications that turned out to be less significant—studies that offered minor corroborations, or perhaps distracting errors, or that turned out not to be relevant at all. By pulling the main developments above the tumult, these essays give a clearer picture than scientists could see at the time.

That is the traditional role of historians, whose key problem is not so much to find information as to select and summarize it. This Web site also offers something less traditional. In these thirty parallel essays connected by hyperlinks, I hope to give an account that conforms to the manifold character of scientific research better than could ever come from squashing everything together into a single linear narrative.

Getting coherent explanations is harder in geophysics than in more self-contained disciplines like astrophysics or molecular genetics. Scientists in those disciplines address problems that fall within a well-understood boundary. That boundary is roughly congruent with a social boundary, defining a community. Typically such a community can trace its origins to a few outstanding researchers and teachers, inheriting their specialized knowledge, questions, and techniques. The discipline has developed its own journals, scientific societies, meetings, and university departments. Scientists develop these social mechanisms partly to facilitate their work of training students and raising funds for research. Still more, the social coherence of the discipline is invaluable for their work of communicating findings to one another, debating them, and reaching conclusions about which findings are reliable.

For the process to work, scientists must trust their colleagues. How is the trust maintained? The essential kind of trust comes from sharing a goal, namely, the pursuit of reliable knowledge, and from sharing principles for how to pursue that goal. Integrity in telling the truth is one important principle, but it is not enough: while scientists rarely cheat one another, they easily fool themselves. Another necessary principle is to take things apart—tolerating dissent, allowing every rational argument to be heard in public discussion. A third principle is to put things together—arguing out a consensus on important points, even while agreeing to disagree on others. These principles and more are inculcated in training, and reinforced by daily interactions.

Maintaining trust is more difficult where the social structure is not cohesive. A community in one specialty cannot thoroughly check the work of experts in another branch of science, but must accept their word for what is valid. The study of climate change is an extreme example. Researchers cannot isolate meteorology from solar physics, pollution studies from computer science, oceanography from glacier ice chemistry, and so forth. The range of journals that climate researchers cite in their footnotes is remarkably broad. This sprawl is inevitable, when so many different factors do in fact influence climate. But the complexity imposes difficulties on those who try to reach solid conclusions abut climate change.

In physics, I can say that a coin will fall with precisely such-and-such an acceleration when you drop it. Not everything can be predicted—the physicist normally cannot tell whether it will land heads or tails—but the general movement is predictable with great exactitude. The reliability of such physical laws can be checked by a single person, or at most by one or two teams of physicists. It is otherwise for a question like what the climate will do after we double the amount of CO_2 in the atmosphere. Here we encounter so many nearly chaotic influences that we can know the main facts only roughly. And here the level of reliability can only be established through checks and corrections by a variety of scientific communities, each dealing with its own piece of the problem.

Who made "the discovery of global warming"—that is to say, the discovery that human activities have very likely begun to make the world warmer? No one person, but a dozen or so scientific communities. Their achievement was not just to accumulate data and make calculations, but also to link these together. This was patently a social process, the work of many people interacting with one another. The social process was so complex, and so important, that the last stage was visibly institutionalized: the workshops, reviews, and negotiating sessions of the Intergovernmental Panel on Climate Change (IPCC).

The scientific community also included some skeptics who believed that global warming was not likely at all. They pointed insistently at all the places where climate theory was incomplete. And amid the immense volumes of data now available, they found scraps here and there that supported their views. They believed that "global warming" was nothing but a social construction —more like a myth invented by a community than a fact like a rock you could hold in your hand. After all, the critics pointed out, communities of scientists had often held mistaken views, and then changed their collective minds. You couldn't trust a clique of professionals, they warned, whose very social cohesion might perpetuate the error of a shared opinion. Hadn't experts once agreed to deny that greenhouse warming was possible? Hadn't experts, as recently as the 1970s, warned of a new ice age?

Most scientists found this not persuasive—worse, hardly even interesting. The critics' selected data and arguments looked feeble when they were set against the enormous mass of evidence for greenhouse warming. To be sure, half a century back, most scientists had found Callendar's greenhouse warming proposition implausible. But scientists back then had understood that their ideas about climate change were based on no more than a scatter of uncertain measurements and hand-waving arguments. Callendar's proposition, although it flew in the face of ideas about climate stability that scientists had long taken for granted, was only provisionally set aside. It stuck in the minds of the experts, awaiting the coming of better data and theories. Likewise during the controversies of the 1970s, most scientists had explained time and again that their knowledge was still too primitive to say whether the climate would turn warm or cold. Their main point was that they had now learned enough to give up the old confidence in stability. By the end of the 20th century it was the critics, arguing for a self-regulating climate, who were struggling to maintain a traditional belief.

By that time, not only scientists but most people had reluctantly reached a less comfortable view of the natural world and its relationship with human civilization. The views of the public and of the scientific community had necessarily changed together, each exerting pressure upon the other. On the public side, gloomy news reports hammered home how severely our technologies can change everything, even the air itself. Meanwhile on the scientific side, knowledge of how climate could change pushed ahead in a direction influenced by field observations, laboratory measurements, and numerical calculations. Yet the advance was constrained within limits set by the larger community's commonsense understanding (and by the funding it provided). Eventually scientists arrived at solid conclusions, as witnessed by consensus panel reports. Certainly in a restricted sense one could call the resulting understanding of climate change a product of human society, or as some would say, a social construct. Scientists themselves sometimes spoke in such terms, describing their understanding of climate as a stout edifice constructed from the countless mutually supporting observations and calculations.

But what does it mean to say understanding was "constructed"? That is misleading language, no better than talking of scientific "advance." Scientists were not a crew laying bricks according to an architect's plans. The terms used in the previous paragraph come from traditional models for the scientific research process, relying on metaphors drawn from classical or even preclassical mechanics—a forward push or "advance," a "construction," a process where ideas are swayed by "pressures" or "influences," where observations "support" one another, and so forth. This language sounds like it means something, but it doesn't really.

In recent years, historians of science and technology have increasingly turned instead to the perspective offered by Darwin's insight on evolution through natural selection. The key factors in the Darwinian model are *reproduction, variation*, and *selection*. Where you find those, you may expect evolution—and they are indeed found in scientific research. Scientific ideas can be said to *reproduce* as they pass from one scientist to another, or within the scientist's own thinking from one month to the next. In these passages *variations* commonly arise, different ideas or combinations of ideas. The variants compete with one another within the constraints of their environment—namely, the minds of scientists. Some are *selected* to survive and further reproduce. As the process repeats over time, something quite different may evolve.[1]

Please notice that normal biological evolution likewise operates upon systems of information. We say that traits such as the color of feathers evolve, but the essential thing that changes is the biochemical instructions for making the feathers. These information packets are encoded in DNA molecules, conveniently organized in genes. The difficulty in talking about Darwinian evolution for other systems of information, such as scientific ideas, is that they are not packaged in such neat units. We can talk vaguely about the evolution of anything from a folk song to the design of a paper clip to an entire national culture. Actually biologists have the same problem of specifying just what evolves—they argue over how far natural selection acts on genes, on complexes of genes, individuals, communities, species, and entire ecosystems. If we try to speak of the evolution of ideas, it is even harder to identify just what entities reproduce and vary and are selected.

I believe that it will pay to consider selection at the level of what I will call *"research plans."* A research plan may be defined as a cluster of associated ideas that are the intellectual basis for a research activity that a given scientist (or team of scientists) considers undertaking for a limited period of time. A research plan offers an answer to the scientist's question: "What shall I do next?" Normally the activity is intended to result in some published scientific papers or reports. A research plan comprises a set of more elementary ideas: theories and hypotheses, observational

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[1] What I call a scientific idea is a subset of a general class of distinguishable ideas, vaguely analogous to genes, termed "memes" in Dawkins (1976, 2nd ed. 1989), ch. 11; see also Dennett (1995), ch. 12; for the history of speculations on ideas derived through natural selection, see Campbell (1974); for extended discussions, Hull (1989); for commentaries on Hull the journal "Biology and Philosophy," vol. 3 (1988), and also subsequently, e.g., Oldroyd (1990); for further extensive discussions, Ziman (2000). I am also indebted to Lindley Darden for useful discussion.

and mathematical techniques, knowledge of the results of earlier experiments and calculations, and so forth. If we imagine the research plan as analogous to an individual creature, these more elementary ideas are like the genetic elements that shape its form (although we cannot push such analogies too far). The research plan is a broader entity than a supposedly unitary idea such as “magnetic field.” It is narrower than the “research programme” which may occupy an entire community of scientists for decades.[1]

A scientist’s research plan can *reproduce* when a colleague or student or the selfsame scientist finds it interesting, and uses some of its elements in designing a new, related research plan. Along the way, particular elements of the plan will be modified, or new elements may be borrowed from other research plans; that’s the *variation*. Note that research plans are mortal—even if they succeed, that is itself their end—but they can spawn progeny, new research with variations in questions or techniques.

Productive scientists usually keep a number of possible plans in mind at once. But a scientist can devote effort to only a few of the many possibilities that fertile minds will conceive. Thus research plans undergo *selection* in rigorous competition with one another—first within the originating mind, and perhaps later in the discussions of funding agencies and so forth. It is partly like the process of artificial selection practiced by animal breeders. In research, however, by definition a scientist cannot well predict the outcome of a particular line of inquiry; a plan once started may prove ill-conceived and perish. So there is always plenty of selection operating outside human intentions.[2]

All this selection of research plans takes place purely within a mental environment. The most important feature of this environment is the scientists’ stock of knowledge of the external world. Like a mutant mouse whose fragile bones cannot hold it up against gravity, a scientific idea will not endure unless it can stand up, in scientists’ minds, against what they understand from observations in the field or the laboratory. An idea (unlike a mouse) never encounters natural facts directly, but rather confronts a mixture of the observations, experiments, and theories that scientists at that time accept. A scientist’s selection of a research plan also depends upon more general ideas about which directions of inquiry are more or less promising. And then there are perceptions about whether one can find the necessary material resources: one will not pursue an unfundable project.

Environments set limits, but within these limits many different paths of genetic descent are

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[1] Lakatos (1970); for a revision of the "programme" as a "research tradition," which "evolves" through changes in the multiple and perhaps rival theories it contains, see Laudan (1977), ch. 3.

[2] This is mostly a simplified version of the analysis of Hull (1989), chapters 11, 12. I diverge from Hull by taking not scientists but “research plans” as the “interactors,” i.e., the entities selected through interaction with the environment and one another. This keeps the discussion consistently within the context of a mental environment.

possible. It is a familiar fact in Darwinian evolution that much depends on the particular sequence. To be sure, key components of a successful scientific theory are as rigidly determined by natural facts as the optics of the eye. But there are more superficial characteristics that may be determined somewhat arbitrarily by antecedents, like the color of an eye. These characteristics can hold social importance (as eye or hair color may). Many interesting features of the history of climate research, such as the 1970s warnings of an imminent ice age, flourished on this surface level. It is human history that determines just when particular ideas emerged, and which ones were emphasized or neglected in particular places and times.

The history of the scientific understanding of climate can be described well in Darwinian terms. We can begin by noting the great number and variety of ideas that germinated (like Darwin's countless seeds), very few of which flourished and had progeny. A research program first had to survive contact with “common sense”— for example, what sort of climate change people of the day, whether scientists or the public, thought was possible at all. The ideas driving the research also had to survive tests against observations and calculations of the actual air and sea. In this testing the particular program had to do better than alternative approaches, within the mental environment of the scientific community. For example, studies on the influence of the Sun and of volcanoes were repeatedly compared with one another, as matched against both the historical record of temperature changes and theoretical calculations of the effects of solar radiation and volcanic emissions. Meanwhile, a program had to find sustenance in the material environment —for example, by drawing on the funding that geophysics researchers won through addressing Cold War concerns. When a research program survived all this it could give rise to a new generation of variants. Eventually a few of the ideas in the programs might evolve into conclusions, fitting so well with empirical observations and other accepted ideas that scientists perpetuated the conclusions in consensus panel reports, and finally in textbooks.[1]

Looking at how variant research programs arose, we can even draw an analogy with sexual reproduction, for many of the best ideas came when a research program picked up an idea or tool through intercourse with other programs. Although barriers to communication among the diverse specialties impeded climate research, the best scientists were always alert to developments in other fields. Specific examples of fertile combinations of programs from widely different fields are the work of Wallace Broecker on the role of the oceans in climate change (should we call him an oceanographer or a geochemist?), and the studies of Reid Bryson (a climatologist, a meteorologist or a paleontologist?) on past climate catastrophes. Half a dozen different kinds of projects fed into Roger Revelle’s crucial discovery of low ocean carbon absorption, and a yet wider range of specialized work was indispensable for computer models of the atmosphere. The 700-plus hyperlinks among the essays on this Web site give a striking demonstration of the importance of cross-fertilization in producing what a Darwinian would call new varieties.

Of course, whatever language we use to describe scientific work, we must take care to avoid speaking of ideas as active entities inhabiting passive minds. The actual effective agents are human beings. Research plans do not jump at one another like mating insects: it is scientists who

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[1] For additional discussion, see Weart (1997).

ingeniously test them and combine them. Considering the entire process, in a restricted sense one could surely call the eventual understanding of climate change a product of human society.

We should not call it "*nothing but*" a social product. Future climate change in this regard is like electrons, galaxies, and many other things not immediately accessible to our senses. All these concepts emerged from a vigorous struggle of ideas, evolving through encounters with experiments, observations, and rival hypotheses. Eventually most people were persuaded to agree that the risk of global warming was real, regardless of the social process that had led to the conclusion.

When people said that the prospect of global warming was "real" (or even "true"), they were implicitly promising some level of reliability. The IPCC was pressed to be as explicit as possible about that. When the panel announced in 2001 that the current rate of warming was "very likely" greater than any seen within the last 10,000 years, they responded to criticism of earlier reports by adding a footnote to define "very likely." They said it meant that they judged there was a 90-99% chance that the result was true. The panel further judged it "likely"—by which they meant a 66-90% chance of being true—that the warming was largely due to the rise of greenhouse gases.[1] What it might mean to call a result "true" remained open to debate; philosophers have devoted lifetimes to pondering how a scientific concept might somehow correspond to an ultimate reality. That ageless question rarely troubled climate scientists, who took it for granted that the future climate is as real as a rock, even if their knowledge of this future thing could only be stated within a range of probabilities.

Our human comprehension of climate goes beyond scientific reports into a wider realm of thinking. When I look at a snowless street in January I may see a natural weather variation, or I may see a human artifact caused by greenhouse gas emissions. Such perceptions are shaped not only by scientists, but by interest groups, politicians, and the media. With global warming the social influences run deeper still. Unlike, say, the orbits of planets, the future climate actually does depend in part on what we think about it. For what we think will determine what we do.

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[1] They did not try to define what a "90% chance" of being true might mean. Discussion has continued, e.g., some groups have derived something resembling a probability distribution by comparing results from computer runs covering all plausible assumptions. IPCC (2001), pp. 1, 6, 8, 13.