BOOKS & ARTS

A challenge to Kyoto

Standard cost–benefit analysis may not apply to the economics of climate change.

Cool It: The Skeptical Environmentalist's Guide to Global Warming
by Bjorn Lomborg
Knopf/Cyan-Marshall Cavendish: 2007.
272 pp./256 pp. $21/£19.99

Partha Dasgupta

Bjorn Lomborg's *The Skeptical Environmentalist* created a sensation six years ago. The author offered figures to dismiss claims that the ecological-resource base in many parts of the world is deteriorating, and argued that the costs of reducing ecological losses are usually higher than the benefits. Never mind that several of the world's foremost environmental scientists expressed more than mere scepticism towards Lomborg's grasp of their science: prominent publications such as *The Economist* promoted the book vigorously and wrote sermons on how scientists should practise their craft. People learning of my own work in developing ecological economics would ask, "And have you read Lomborg?" — implying, "Why have you thrown away so much of your working life?"

Things have changed over the past year. Former US vice-president Al Gore's film *An Inconvenient Truth* and the Fourth Report of the Intergovernmental Panel on Climate Change have given rise to great public concern, and many now regard global warming to be the central problem facing humanity. Lomborg's latest book, *Cool It*, is a response to that change in public perception. He doesn't question the science, which says that rising concentrations of greenhouse gases in Earth's atmosphere are affecting our climate system; he questions whether we should do much about it. If *The Skeptical Environmentalist* was the relentless prosecuting counsel, *Cool It* is the hard-headed but caring economist.

The book is a series of exercises in cost–benefit analysis, interspersed with quotes on climate change from the writings of famous people who should know better than to speak in hyperboles. Lomborg produces figures to show that it would be better to replace the Kyoto Protocol with strategies that encourage economic growth and blunt the harmful effects of climate change. Here is a sample: did you say Kyoto would result in fewer floods? Maybe, but it would reduce flood damage by only US$45 million a year, whereas building appropriate infrastructure could lower it by $60 billion a year. Didn't you also say that global warming would cause additional deaths from heatwaves? Yes, but what about the greater numbers who would not die of cold? Are you worried about deepening poverty in the tropics without Kyoto? You shouldn't be, because Kyoto would reduce the number of undernourished people in 2080 by only 2 million, whereas the United Nations proposes in its Millennium Development Goals to reduce the number by 229 million by 2015. What about more severe hurricanes? Well, Kyoto would reduce the increased annual damage by only 0.6%, whereas taking better precautions could lower it by 250%. And so on.

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Should we be spending more on protecting ourselves against the adverse effects of global warming?

Lomborg reports that Kyoto's annual cost would be $180 billion in foregone output, whereas the smart strategies he outlines, which would include an annual expenditure of $25 billion on research and development in clean technologies, would cost a mere $52 billion a year. By his reckoning, those strategies would limit the rise in concentration of carbon dioxide to 560 parts per million (p.p.m.) and the accompanying temperature rise to 4.7 °C. Smart strategies would cost far less than Kyoto, deliver higher economic growth worldwide, and markedly reduce poverty. From the vantage point of Kyoto, there is a free lunch to be had wherever you look.

You might say that the Kyoto Protocol was misconceived and that the world should develop a bolder programme of action, with much higher carbon taxes, international cooperation to reduce hunger, disease and habitat destruction, and development of clean technologies and ways to sequester carbon. But in Lomborg's view, doing more of a bad deal is rarely smart, so he doesn't countenance going beyond Kyoto. All this is spelt out in such a breezy, engaging style, it's hard not to find the arguments entirely reasonable.

Unfortunately, Lomborg's thesis is built on a deep misconception of Earth's system and of economics when applied to that system. The concentration of CO_2 in the atmosphere is now 380 p.p.m., a figure that ice cores in Antarctica have revealed to be in excess of the maximum reached during the past 600,000 years. If there is one truth about Earth we all should know, it's that the system is driven by interlocking, nonlinear processes running at different speeds. The transition to Lomborg's recommended concentration of 560 p.p.m. would involve crossing an unknown number of tipping points (or separatrices) in the global climate system. We have no data on the consequences if Earth were to cross those tipping points. They could be good, or they could be disastrous. Even if we did have data, they would probably be of little value because nature's processes are irreversible. One

implication of the Earth system's deep nonlinearities is that estimates of climatic parameters based on observations from the recent past are unreliable for making forecasts about the state of the world at CO_2 concentrations of 560 p.p.m. or higher. Moreover, the nonlinearities mean that doing more of a bad deal (Kyoto) may well be very good.

These truths seem to escape Lomborg. His cost–benefit analysis involves only point estimates of variables (interpreted variously as 'most likely', 'expected', and so forth), implying that he believes we shouldn't buy insurance against potentially enormous losses resulting from climate change. His concerns over the prevalence of malaria, undernutrition and HIV in today's world show that he is an egalitarian. There is, then, an internal contradiction in his value system, because if you are averse to inequality you should also be averse to uncertainty.

The integrated assessment models of Earth's system on which Lomborg builds his case are arbitrarily bounded on either side of his point estimates. It can be shown that if those bounds are removed (as they ought to be), even a small amount of uncertainty — when allied to only a moderate aversion to uncertainty — would imply that humanity should spend substantial amounts on insurance, even more than the 1–2% of world output that has been advocated. If the uncertainties are not small, standard cost–benefit analysis as applied to the economics of climate change becomes incoherent, even if those uncertainties are judged to be thin-tailed (gaussian, for example); this is because the analysis would say that no matter how much humanity chooses to invest in protecting Earth from passing through those later tipping points, we should invest still more.

Economics helps us to realize what we are able to say about matters that will reveal themselves only in the distant future. Simultaneously, it helps us to realize the limits of what we are able to say. That, too, is worth knowing, for limits on what we are able to say are not a reason for inaction. Lomborg's seemingly persuasive economic calculations are a case of muddled concreteness. ■

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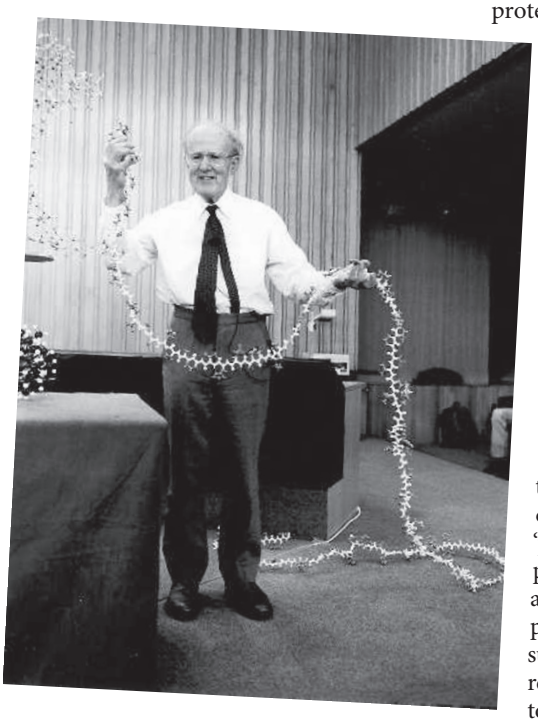
Max in three dimensions

Max Perutz and the Secret of Life
by Georgina Ferry
Chatto & Windus/Cold Spring Harbor Laboratory Press: 2007. 304 pp. £25/$39

Gregory A. Petsko

I have a problem with the title of this book, but it's almost my only quibble with this marvelous biography of one of the least known of the twentieth century's great scientists. By no measure could Max Perutz be said to have discovered the secret of life — a claim that might be defended for Gregor Mendel, or Charles Darwin and Alfred Russel Wallace, or James Watson and Francis Crick. The mechanism of oxygen's reversible binding to haemoglobin, which Perutz elucidated in 1970 after more than 25 years of work, doesn't even apply to most living organisms (and oxygen is deadly to most anaerobes). That said, Perutz did many extraordinary things, including winning the Nobel Prize in Chemistry in 1962 for solving haemoglobin's three-dimensional structure.

It is hard to write a biography of someone only recently deceased (Perutz died in 2002). The biographer must accurately portray someone who was known personally to many readers, yet at the same time expose previously hidden aspects of his or her character. Georgina Ferry achieved both in her biography of Dorothy Crowfoot Hodgkin, and now she has done it again in her engrossing account of the life and work of Max Perutz.



PERUTZ FAMILY

Perutz the showman: Max continued to captivate audiences well into his eighties.

Perutz, whom I knew well for 30 years, can't have been an ideal subject. No scandal colours his career; his personal life was stable and happy; his accomplishments were clear in terms of priority; he made a number of significant mistakes; his major discoveries are not easily understood by the layperson; and he lacked the forceful manner of a Crick or Bernal. But as the father of protein crystallography — arguably one of the greatest scientific advances of the last century — and the founder of the Medical Research Council (MRC) Laboratory of Molecular Biology in Cambridge, UK, his influence was enormous.

Ferry succeeds in bringing what could, in lesser hands, be considered a somewhat drab character sharply to life. As an Austrian Jew born during the First World War, Perutz left for Cambridge in 1936, where he joined the laboratory of one of the larger-than-life figures of modern science, John Desmond Bernal. Together with Dorothy Hodgkin, Bernal had, just two years earlier, taken the first X-ray diffraction photograph of a single crystal of a protein molecule, the digestive enzyme pepsin. This showed that, in principle, the extraordinary power of crystallography could reveal the atomic details of even large biological molecules. Undeterred by the scale of his task, Perutz ventured to use crystallography to unravel how haemoglobin could bind oxygen tightly enough to transport it around the bloodstream, yet release it when and where it was needed.

I doubt whether most people, even today, understand how pioneering this was. Determining the three-dimensional structure of proteins was a goal of Nobel-prize potential for several powerful research groups of that time, but none particularly cared what the protein was. Only Perutz had a greater aim. He wanted to understand the function of haemoglobin, which meant solving all the problems presented by this large, flexible protein. What would he have made of the recent International Structural Genomics Initiative, I wonder, which aims to turn out massive numbers of protein crystal structures without regard to biological or biochemical function?

Perutz's greatest achievement was demonstrating that the method of 'isomorphous replacement', previously used to solve the structures of small organic compounds, could be used to crack the 'phase problem' in protein crystallography. This is the problem of how to deduce a wave's phase component in diffraction patterns. This method made it possible to sum up the scattered X-ray waves in proper registration with each other and therefore to reconstruct the molecule's structure. It opened the way to solving the structure of any large crystalline molecule. Ferry explains the many false starts, and embarrassing errors, that led up to that moment, while allowing the reader to feel the frustrations and joys along the way. It's as good an account of a scientific breakthrough as you will find.

Haemoglobin's structure followed. More