

good deal earlier, with the superb evocation of Tukulito's passage into her own personal 'afterland.' But lives do not often conclude at the most elegant point; by following Kruger into Mexico, Heighton gives his narrative more the structure of a life than a novel.

The author of several books of poetry and short fiction as well as another novel, Heighton has been hailed as a new leading light of Canadian fiction, and those who read *Afterlands* will see why. As my expertise lies in the far south rather than the far north, I can not comment on the thoroughness of his research, or the extent of the liberties he takes with documented material. No doubt those better versed in the history of Arctic exploration will have stronger things to say about this. However, criticism of that sort would seem beside the point in a novel that foregrounds its own provisionality, presenting itself as a series of versions of events — an approach towards emotional understanding rather than historical truth. Such responses to polar expeditions will never replace rigorous, balanced, carefully argued non-fiction accounts; but they will continue to complement and complicate polar history. (Elizabeth Leane, School of English, Journalism, and European Languages, University of Tasmania, Private Bag 82, Hobart, Tasmania 7001, Australia.)

SCIENTIFIC UNCERTAINTY AND THE POLITICS OF WHALING. Michael Heazle. 2006. Seattle and London: University of Washington Press; Edmonton: Canadian Circumpolar Institute Press. xii + 260 p, hard cover. ISBN 0-295-98605-0. £38.95; \$US60.00. doi:10.1017/S0032247407006857

Since 1946 most pelagic whaling has been regulated under the International Convention for the Regulation of Whaling (ICRW), adopted in Washington, DC. The Convention is administered by the International Whaling Commission (IWC), which first met in 1949 and has been the principal forum for debate over the management regime that should be imposed on the industry. From the outset, the intention was that the IWC would be advised by a Scientific and Technical Committee (STC, split in 1951 into a separate SC and TC), so that regulation was based on the best available information about the biology of the quarry species and the status of their stocks.

In theory, such a framework should have guaranteed not only the survival of the world's great whales but their sustainable use for human benefit. In practice, the IWC has struggled since its very first meeting to measure up to its objectives. As is well known, between 1949 and 1970, catches in the Antarctic waters that were the main commercial whaling grounds declined, at first steadily and then catastrophically, and by the time the IWC adopted a moratorium on Antarctic commercial whaling in 1982 the industry had effectively destroyed itself. Many scientists and conservationists regard the ICRW and IWC as an object lesson in how not to conduct international regulation.

But the ICRW and IWC started with in-built weaknesses. First, like other international conventions, they protected the rights of the sovereign States Parties and this meant that to be effective decisions had to be reached by consensus. The ICRW allowed any Party that dissented from a decision to give notice within 90 days of its adoption that it would not be bound by it. This effectively meant that any one of the major whaling nations — at various times Japan, the Netherlands, Norway, the United Kingdom, and the USSR — could obstruct measures that clashed with what they perceived as their national interests (often tied closely to the need for a return on their investment in their whaling fleets). It meant that the quest for consensus led to weak compromises over overall (and later national) catch limits. It even meant in the early years that the Scientific Committee did not bother to propose quotas that it knew were too stringent to be accepted.

This book argues that a further, fundamental, problem arose because although the IWC was required to base its policies and limits on science, it used uncertainty over the numbers, reproductive rates, and survival of whales as a basis for rejecting the advice of the scientists. The arguments are familiar in fisheries today. The scientific consensus is that a stock is vulnerable, and that catch limits should be reduced. Those doing the fishery argue that there are plenty to be caught and that the scientists' figures are too uncertain to justify the economic cost of reduced harvesting. In the post-war period, whaling yielded valuable food oils and meat, and much money had been spent on factory ships and infrastructure. Many jobs were at stake. Each nation wanted to go on whaling until it had either got a reasonable return on its investment or found cheaper substitutes for its products, or both. And not all national scientists were beyond reproach: some indeed appear to have been chosen as national expert representatives on the SC because their views fitted the national political goal.

The author argues that scientific uncertainty was used in two ways during the history of the IWC. While the whaling nations were denying the need for stringent catch limits — at least until they had made as profitable as possible an exit from the industry — uncertainty was used to evade the scientific arguments for tighter catch limits. Later, the balance of proof was reversed and the 'Precautionary Principle,' originally formulated as an argument for preventing the discharge to the environment of potentially polluting substances, was prayed in aid as grounds for only permitting whaling at levels that would incontrovertibly safeguard species and stocks.

This shift in policy came far too late, and only after many years during which strengthening scientific evidence was denied while stocks declined. Methods for determining the age of whales improved. The decline of first humpback and blue whales, and then fin whales, became obvious. Smaller and less valuable sei and minke whales were targeted in turn. More effort was devoted to less return. The SC was strengthened in 1960 by the appointment of a 'Committee of Three' (later four)

statisticians, and from 1964 the Food and Agriculture Organization of the United Nations (FAO) played an increasing part, and all concurred in demanding smaller catches. As the economics of the industry worsened, nation after nation withdrew so that by 1973 only Japan and the USSR were left. Even then, the reduced fleets could not catch the unrealistic quotas allowed by the IWC.

By then the political tide had turned. In 1972 the United Nations Conference on the Human Environment at Stockholm passed a resolution demanding a 10-year moratorium on commercial whaling. The United States, United Kingdom, and others supported the proposal. The IWC responded by adopting a New Management Procedure (NMP), followed by a Revised Management Procedure (RMP) in 1975–76. The latter, at last, treated uncertainty as grounds for more rather than less protection. But it came too late. An increasing number of non-whaling states acceded to the ICRW and supported a moratorium. It was adopted in 1982, against the opposition of Iceland, Japan, the Republic of Korea, Norway, Peru, and the USSR. All except Iceland and Korea lodged objections, and were hence legally entitled to continue whaling, although pressure from the US brought a halt to Japanese activities except as so-called ‘scientific whaling’ in 1988.

This book examines the whole sorry but fascinating tale in great detail. It limits itself to the hunting of baleen whales in Antarctic waters because this accounted for the greater part of the global whaling industry. It focuses on the proceedings and decisions of the IWC, and especially on how the scientific advice it received was formulated, how far it was truly independent of national commercial interests, and on how uncertainty was used to set aside scientific advice that would have imposed economic loss. Its central thesis is that science has little hope of determining policy when decisions rest in the hands of delegates governed by national self-interest, and that when economics and science conflict, economics wins every time. Its sub-plot is that uncertainty has been used to support radically different arguments, as the tide of political advantage turned. As the author says, ‘it is not uncertainty itself that determines or influences policy making so much as how we choose to use it — and that is ultimately determined by political choices about what is or is not desirable.’

Critics may argue that the book concentrates too much on the official record and gives insufficient credit to the world conservation movement, which brought increasing political pressure on governments in the 1970s. The discussions in IUCN (The World Conservation Union), of which most ICRW parties were State Members, are not mentioned, yet from 1978 onwards it supported both a moratorium and the work of the IWC’s Scientific Committee. ‘Protectionist’ arguments, fuelled by public wonder at modern films and sound recordings and by ‘whale watching’ and also by evidence that the methods of killing whales are inhumane, receive scant attention. Some scientists who have acted as advisers to government

will also feel that the book is unfairly dismissive of their influence. Nonetheless, it is a valuable record and analysis of the lamentable failure of what should have been a model international regulatory instrument.

It is clearly written, fully referenced, and well indexed. It will be useful to polar historians, but also to students of environmental policy more generally and to those seeking reasons for distrusting governments. Scientists who already look sceptically at economists will find grounds for even deeper scepticism. Conservationists may regard it as a Solemn Warning. But all should be grateful to the author for setting out his arguments — and his evidence - so clearly. (Martin Holdgate, Fellbeck, Hartley, Kirkby Stephen, Cumbria CA17 4JH.)

DEEP FREEZE: THE UNITED STATES, THE INTERNATIONAL GEOPHYSICAL YEAR, AND THE ORIGINS OF ANTARCTICA’S AGE OF SCIENCE. Dian Olson Belanger. 2006. Boulder, CO: University Press of Colorado. xxxiv + 494 p, illustrated, hard cover. ISBN 0-87081-830-9. \$US29.95. doi:10.1017/S0032247407006869

Dian Olson Belanger’s history of the 1957–58 International Geophysical Year (IGY) in Antarctica and the US military’s ‘Deep Freeze’ operations that supported it is a highly informative and readable narrative account of perhaps the single most striking international scientific endeavour of the twentieth century. That the IGY emerged from and was implemented by an international community riven by Cold War tensions and rivalries makes the story all the more remarkable.

The IGY was, from the beginning, an often-tense mix of science, exploration, occupancy, strategy, and politics. Its decentralised nature (relying exclusively on national programmes), the increasingly obvious value of suspending political rivalries between claimant and non-claimant states, and the small, inexpensive bureaucracy (CSAGI) that assisted in programmatic coordination and data exchange substantially abraded the rivalries and suspicions that each participant brought to the enterprise. As the global value of Antarctic research became obvious, the way was paved to an international treaty ‘based on the scientific cooperation of the IGY’ (page 371). That instrument guaranteed to the present day Antarctica’s unique status as, in effect, a world park beyond and separate from an international community that remains committed to the maintenance of its unique peaceful status.

Five themes dominate and structure the book. First and foremost is the recurrent friction between the US Armed Forces charged with logistically supporting the IGY and an Antarctic scientific community chronically suspicious and fearful that the service people, and the Navy in particular, were pursuing their own separate and antithetical agendas. Such fears were not groundless. Some in the Navy wanted to use the IGY as a front or cover to pursue strategic interests, including further