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CMOS Bulletin SCMO
"at the service of its members
au service de ses membres"

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Cover page: Adversarial debate is an integral part of advancing scientific knowledge. However, when dealing with complex systems such as the Earth's climate, the inter-connectivity of nature, science and society must be recognized. Scientific results must be communicated in a language that addresses the public's perspective and concerns. What do mad cow disease, genetic engineering, SARS and climate change have in common? To find the answer and to learn more, read Henry Hengeveld's article on **page 71.** Photograph courtesy of Uri Schwarz, Emeritus Executive Director.

Page couverture: Les débats contradictoires font partie intégrale du processus d'avancement de la connaissance scientifique. Par contre, lorsque des systèmes complexes comme le climat de la Terre sont en jeu, la connectivité de la nature, de la science et de la société doit être considérée. Les résultats scientifiques doivent être communiqués en utilisant un langage qui répond aux perspectives et aux soucis du public. Qu'est que la maladie de la vache folle, l'ingénierie génétique, le SARS et le changement climatique ont en commun? Pour connaître la réponse et en savoir plus, lire l'article de Henry Hengeveld à la **page 71.** La photographie est une gracieuseté de Uri Schwarz, Directeur exécutif émérite.

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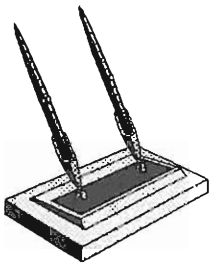
....from the President's Desk

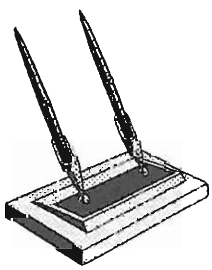


This is my last column, my "last words" so to speak, as President of CMOS and all I can really think to say is "thank you". Thank you to everyone who provided me with comments, guidance, advice, criticism, kudos, ideas, thoughts, inspiration, to-do-lists, food for thought and so much more. I think that every new president comes into this Society with high hopes about how he or she will make great contributions to the professions of meteorology, climatology and oceanography and though I look back and hope that I did give something to the Society, I took away so much more.

I must say, at the risk of using a cliché, that serving as president is a real honour. It truly is - what a fantastic opportunity to get an inside look at the profession and its place in the context of individual professionalism, Canadian politics, and international science. What an amazing array of dedicated professionals I have had the privilege to meet, correspond and share ideas with regarding the current situation and future vision of our professions. They have provided me with insight and inspiration and what a gift that is!

This was a good year for the Society. As an organization, we continued to move forward. And still, all of the accomplishments and all of the progress that we made were only through the individual and collective efforts of many CMOS members across the country. And so once again, thank you for the opportunity to serve in such an exciting year.

Ron Bianchi,
Outgoing President / Président sortant
CMOS / SCMO



I am writing this when most of the country is still waiting for spring and I am thinking about the role of President of CMOS that I am about to undertake on your behalf. Over the past year as Vice-President, I have learned much about how CMOS functions as I have observed Ron Bianchi, our immediate past president, and the other members of the executive carry out their duties. The 'Ontario' executive has successfully piloted CMOS over the past three years and has instituted a number of organizational and administrative changes that promise a successful future for the organization. I thank them all for their contributions and especially Ron Bianchi for his energy, enthusiasm and the hard work that has characterized his term as President.

(Continued on next page - Suite à la page suivante)

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Inside / En Bref

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CMOS exists for the advancement of meteorology and oceanography in Canada.

Le but de la **SCMO** est de stimuler l'intérêt pour la météorologie et l'océanographie au Canada.

As your incoming President, I am leading an incoming 'Nova Scotia' executive. We will all be learning on the job over the next several months. At the same time, the CMOS office will be taking on new administrative responsibilities that had previously been carried out on our behalf by the Canadian Association of Physicists' office. We are presently moving our membership records into a new database system. By the summer's end we will be providing our Chapters and Centres with better information about their members.

In the coming year I will continue the present drive to grow our membership. Growing our membership means more than increasing our numbers. It means working to ensure that all members of Canada's meteorological and oceanographic communities feel that CMOS can play a role in their professional life and that in turn they can help CMOS play its role within Canadian Science

CMOS is largely a volunteer organization that can accomplish great things when we have committed and energetic members behind these initiatives. I shall need your continued ideas, commitment and support in the year to come. I look forward to an exciting year of working for you and with you.

Allyn Clarke
Incoming President / Nouveau Président
CMOS / SCMO

Letters to the Editor

11 March 2003
Subject: Time for Peer Review of Bulletin Letters

Several recent letters to the editor have caused me to ponder whether it's time for the CMOS Bulletin to initiate a peer-review of letter submissions. Perhaps CMOS could start with this letter!

As the official newsletter of the Canadian Meteorological and Oceanographic Society, the Bulletin has a duty to uphold the highest standards with regard to its submissions. For example, peer review of the recent letter by M. R. Morgan, would have caught the fact that there is an error in both the title and first four words of his verbiage. There is no such publication as IPCC Climate Change 2000: It is Climate Change 2001. If the title of the referenced document is inaccurate, one is left with the question as to what else is inaccurate in the Morgan piece.

Further review of the Morgan piece would have pointed out that there is a basic error made as to the causes of ice age cycles. Ice age inception requires changes in seasonality with warmer winters and colder summers. While not something to look forward to, a slowdown in the North Atlantic Deep Water formation could not and would not lead to an ice age. Review of the piece would have pointed Morgan to Berger and Loutre (2002) where it is shown that we should not expect another ice age for 50,000 years!

Further review would have pointed out the oceanographers have been working with meteorologists for decades, and would have further pointed out that the WMO international conference in Geneva did not suggest that GHGs were causing cooling, rather they were discussing tropospheric aerosols. The review of the letter would have pointed out many other blatant and basic errors.

It is a duty of the CMOS Bulletin to uphold the highest standard of publication. Opinion pieces which are scientifically inaccurate are more often found in the pages of newspaper editorials, not in official society publications.

Andrew Weaver, University of Victoria

Reference:
A. Berger and M. F. Loutre, 2002. *An Exceptionally Long Interglacial Ahead?*, Science 297: 1287-1288.

Response from the *CMOS Bulletin SCMO* Editor

The current policy of the *CMOS Bulletin SCMO* is that articles and Letters to the Editor are examined by one or two persons before inclusion in the *CMOS Bulletin SCMO*, to ensure that the material is relevant to the domain of interest of the Society and does not contain offensive wording or statements. The text is either rejected or accepted as submitted, although spelling and grammatical errors are normally corrected. Furthermore, if the Editor notices a minor inconsistency or error which he is unable to correct, the author may be offered the opportunity to make the correction.

To carry-out a peer review, as suggested by Dr. Weaver, would negate three major advantages of the current policy: timeliness, freedom of expression and stimulation of debate. A peer review would be a time-consuming and difficult process to implement in the *CMOS Bulletin SCMO*. Before we change our policy, we would wish to receive more opinions from the membership.

Paul-André Bolduc, Editor, CMOS Bulletin SCMO

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18 April 2003
Subject: Greenhouse effect and Kyoto Protocol

I would like to comment on your recent articles on the Greenhouse Effect, and the necessity of ratifying the Kyoto Protocol.

I am a retired scientist who has specialised in Climate Science over a number of years and recently published a Critique of the IPCC Report "Climate Change 2001" entitled "The Greenhouse Delusion" (obtainable through Multi-Science publishers).

I have been an "expert reviewer" for the IPCC Reports for many years. Thousands of scientists have produced three major reports and many scientific papers on this topic, but have failed to show either that the "globe" is "warming", or that carbon dioxide or other greenhouse gas increases have had any harmful effects.

The scientists have, however, been pressured to provide a series of ambiguous statements which have been interpreted by politicians to imply that increases in greenhouse gases are harmful. One technique was to write a "Summary for Policymakers", agreed line-by-line by Government representatives, which provided statements such as:

- "the balance of the evidence suggests a discernible human influence on the climate";
- "there is new and stronger evidence that most of the warming observed over the last 50 years is attributable to human activities".

The true opinion of the scientists is given on page 97 of "Climate Change 2001":

"*The fact that the global mean temperature has increased since the late 19th century and that other trends have been observed does not necessarily mean that an anthropogenic effect on the climate system has been identified. Climate has always varied on all time scales, so the observed change may be natural*".

Two major pieces of evidence for the harmful influence of increased greenhouse gases are presented in "Climate Change 2001.

The first refers to the supposed "global warming". Measuring the average temperature of the earth should involve distributing thermometers fairly over the earth's surface, 71% over the oceans, 9% over forests, 10% over farmland, and only 1% in urbanised areas where most weather stations are situated. The IPCC have chosen to assume that the weather station average can represent the whole earth's surface. The increase of about half a degree in 140 years in this average is much more likely to be attributable to increased population, energy usage and city building, not to increases in greenhouse gases. Satellite measurements over the past 24 years show only temperature fluctuations attributable to natural changes such as volcanoes, El Niño, and the sun. There is no evidence for the supposed steady upwards influence by greenhouse gas increases.

The second major evidence presented by the IPCC is from computer climate models. The parameters and equations in these models are highly uncertain, and by adjusting them the models can simulate almost any climate sequence including a steady temperature or a fall. No model has ever predicted a future climate sequence successfully. The "confidence" expressed in the models has no scientific basis.

My book analyses the claims of "Climate Change 2001" in some detail, and concludes that there is no evidence for a harmful effect from greenhouse gas increases. Instead, there is mounting evidence that the increases are beneficial to agriculture and forestry.

The Kyoto Protocol is therefore unnecessary. In any case its implementation would have no measurable effect on the climate even if the climate model "projections" of the IPCC are accepted.

Vincent Gray, Wellington, New Zealand

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02 April 2003
Subject: Request for papers, essays and reports

At Indian Meteorological Society (IMS), Kolkata Chapter, it is our continuous endeavour to acquire the necessary and sufficient knowledge to understand various aspects of science, nature and atmosphere. We do not expect all our members to be scientists, but we care for scientific people; people who can think rationally over the events around them. Therefore, to pave its way, Indian Meteorological Society, Kolkata Chapter, has been regularly publishing "Jhar", a scientific magazine distributed free of charge to its members and other eminent scientific people in India.

'Jhar', the newsletter bulletin of IMS Kolkata Chapter has now stepped into its fourth year. With a handful of new features, we have tried to explore the lighter side of meteorology and allied sciences too, with some serious topics as well. A few pages are devoted to astronomy, which include star charts and celestial events of forthcoming months, just to encourage people to look at the night sky. A few regular features are also introduced in this edition like Met. Tit-bits, Met. Puzzle, FAQs (Frequently Asked Questions) etc. A review of weather in recent past is coming into light on regular basis from now.

In this regard, to maintain 'Jhar' as a magazine of high esteem, we invite papers / essay / reports on popular topics from the subjects like Meteorology, Atmospheric Science, Astronomy, Astrophysics, Earth Science, Oceanography, Physical Science etc. etc.

Your kind cooperation in this regard will be solicited.

Utpal Bhattacherjee, The Secretary (Publication)
Indian Meteorological Society

Response from the *CMOS Bulletin SCMO* Editor

It seems to me that both publications have the same goals and that we try to hit the same kind of contribution from various authors. A certain amount of cooperation between both publications and Societies can help us out!

Paul-André Bolduc
Editor, CMOS Bulletin SCMO

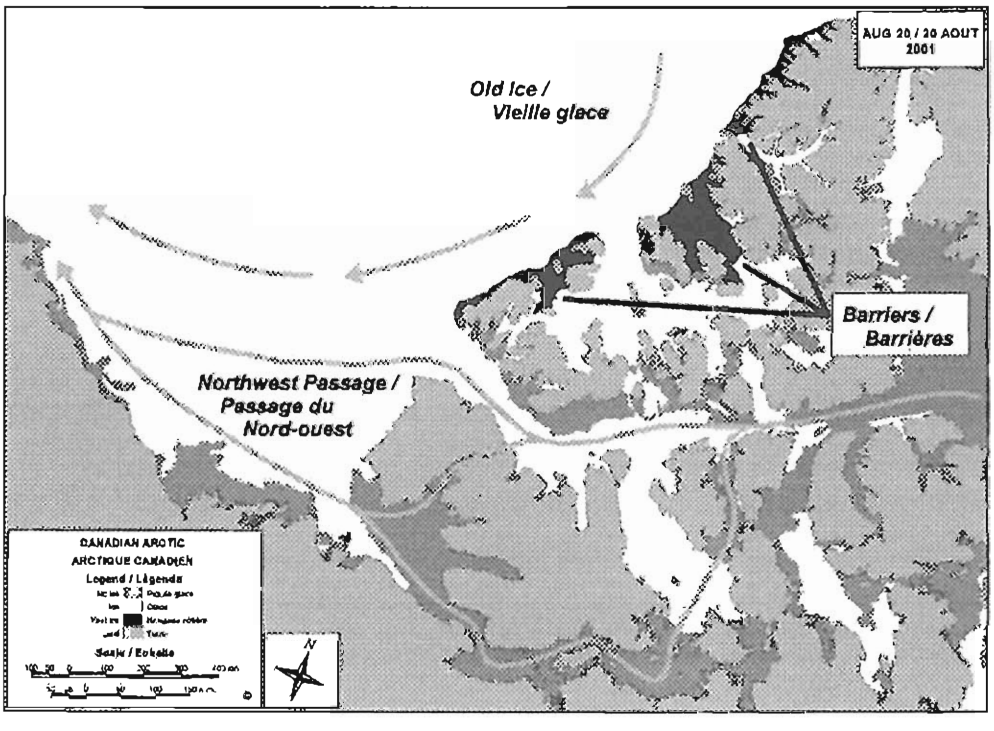
Shipping in the Canadian Arctic: Possible Climate Change Scenarios[1]

by John Falkingham[2], Humfrey Melling[3] and Katherine Wilson[2]

By now most of you have heard the predictions from Global Climate Models (GCMs) that warn, if warming trends continue, by 2050 sea ice in the Canadian Arctic will no longer be present during summer months *(Flato and Boer, 2000)*. This scenario has brought forth much discussion concerning expected increases in marine transportation in Canadian Arctic regions. The Northwest Passage (NWP) lies in the middle of the Canadian Arctic and is a shortcut between Europe and Asia that is some 9,000 kilometres shorter than the Panama Canal route and 17,000 kilometres shorter than the route around Cape Horn *(Falkingham et al., 2001)*. The ever-present sea ice has made this shortcut virtually inaccessible, but a future with less sea ice would mean an increase of shipping traffic through this passage. This, combined with the ability to finally access and exploit large natural-gas reserves within the Canadian Arctic *(Melling, 2002)*, will cause significant impacts on our Arctic environment and its people.

There is observational evidence consistent with GCMs that the amount of ice has decreased during 1971-98 in all the areas of the Canadian Arctic *(Falkingham et al., 2001)*. Military and shipping interests are currently working on future impacts and adaptation strategies based on this scenario of ice-free summers. Yet, this is only one of several other possible climate change scenarios for the Canadian Arctic. The Canadian Ice Service (CIS), as part of its mandate, has monitored ice conditions in the Canadian Arctic for over forty years. Drawing on work by Canadian Scientists and our long experience in providing ice information, we would like to present some other very possible climate change scenarios that will need to be addressed when planning future impacts and adaptation strategies for shipping in the Canadian Arctic.

The lack of solar radiation during winter months in the Arctic means that there will always be at least a winter ice cover and therefore year round shipping will not be possible. Moreover, ice conditions have always been extremely variable with light ice years interspersed with heavy ice years. Thus marine users can not assume consistent ice-free summers and should still expect occasional heavy ice years.



Pack ice in the Arctic Ocean can circulate around the North Pole for several decades *(Colony and Thorndike, 1984)* continuing to thicken by freezing to as much as 3-4 metres *(Flato and Brown, 1996)*. As this ice bumps up against the coastlines of Canada it is broken and heavily ridged, reaching an average thickness of 8 metres *(Bourke and Garret, 1997)* and a maximum of nearly 50 metres *(H. Melling, pers. comm. 2002)*. Normally, old ice occasionally drifts into the channels of the Canadian Arctic

[1] First published in the Northern Climate ExChange newsletter Weathering Change, Volume 1, Number 2. Reproduced here with their authorization.

[2] Environment Canada, Canadian Ice Service, Ottawa, Ontario.

[3] Department of Fisheries and Oceans, Institute of Ocean Sciences, Sidney, British Columbia.

Archipelago freezing across the narrow passages to create a barrier between the Arctic Ocean and the NWP. The old, thick, land-locked ice of the northern Canadian Arctic Archipelago can remain in place for several years.

In 1998, the warmest year on record in the Canadian Arctic, regions of land-locked ice in the Archipelago broke. This also happened early in the 1960s and has occurred at roughly decadal intervals since. This collapse of these barriers has allowed very thick old ice from the Arctic Ocean to drift through the Arctic Islands in subsequent years (*Jeffers et al., 2000; Wilson 2001*).

Evidence of prior incursions of old ice into the NWP has also been found through the analysis of historical data from the Canadian Ice Service *(Falkingham et al., 2002)* and other sources *(Melling, 2002).* The increased incidence of warm summers anticipated with climate warming may cause the ice in the Canadian Arctic Archipelago to break-up more frequently and earlier *(Melling, 2002).* This change will permit old ice to drift more rapidly from the Arctic Ocean into the Northwest Passage, thereby increasing the rate of supply to and thickness of ice within the Northwest Passage. Old ice is extremely strong and dangerous to all ships, even ice breakers, and the increase of this ice in the NWP will result in increased hazards to the marine environment and its users.

Winds and ocean currents can drive sea ice against coastlines and into narrow channels creating high-pressure zones, capable of crushing ships and creating barriers to navigational passageways. Some of these choke points are impassible and icebreaker assistance is usually required. Even in a generally ice free Arctic, small amounts of sea ice could collect at these choke points to create local congested areas. Possible incursions of old ice from the Arctic Ocean will also mean that choke points will continue and become more hazardous with the increased presence of thick old ice.

The most publicized climate change scenario, that of an ice-free Arctic summer by 2050, may lead many into a false sense of optimism regarding the ease of future shipping in the Canadian Arctic. After 2050, there will still be summers of occasional heavy ice conditions, choke points blocking routes within the Northwest Passage, and ice will continue to be a navigational danger. Ships may attempt to travel faster than prudent through what may appear to be an ice-free passage. Even small pieces of old ice can rip holes into hulls of ships, thereby risking human safety, cargoes and the environment.

It is important to remember that with our present imperfect ability to predict future impacts on Arctic sea ice, there are a number of plausible scenarios for the impact of climate change on marine areas of Arctic Canada. These must be acknowledged when planning future impacts and adaptation strategies for shipping in the Canadian North.

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Wilson (2001). CCAF Report: RADARSAT ice motion analysis for the Queen Elizabeth Islands 1998-2000. Chapter 21 *in* The state of the Arctic cryosphere during the Extreme Warm Summer of 1998: documenting cryospheric variability in the Canadian Arctic.

Looking at weather from a native point of view[1]

by Xavier Kataquapi

The weather is an important part of my people, the Cree on the James Bay coast. Many of the elders have a great understanding of changes in the weather. This is due to the fact that during their early lives they lived a traditional life out on the land and had to deal with their environment on a daily basis. It was a matter of survival to have a good knowledge of extreme weather conditions in order to be able to prepare for them.

A good knowledge of the weather can also mean the difference between a good or bad hunt. There are many hunters and trappers in the community and each time they leave, forecasting the weather for the near future determines whether or not one should leave to head out on the land. Good or bad weather also determines what animals will do and where they will go.

Many people I know who still venture out on the land keep a close eye on different signs that forecast the weather. Halos around the sun and the moon can signal cold weather for the next few days and active northern lights that dance across the sky mean stronger winds in the near future. Fire can also provide a clue as to what weather will take place in the near future. If the base of a fire is burning white it means cold weather and if it is red then it will be warmer.

I learned about these methods of predicting the weather from my parents, elders in my community, and others who have a good knowledge of life on the land. It seems that when I was growing up the only way to learn about my people was outside of school.

Thankfully, there are those who want to introduce the accumulated knowledge and wisdom of First Nation people to young students. The Ojibway and Cree Cultural Centre, based in Timmins, has produced a new book for educators to teach the knowledge and experiences of the Cree and Oji-Cree of the Nishnawbe-Aski Nation (NAN) area. It is a unique book, titled Weather, It's Right or Not and was developed by Jim Hollander, Curriculum Writer and Coordinator for the Cultural Centre. The book features the science of climate change. It provides examples of predicting the weather from a First Nation point of view and the ways Native people deal with changes in the climate.

This will be good for young students who do not have as many opportunities to live on the land as our parents and grandparents once did. The new book is an outline for teachers to follow and teaches students about the six seasons of the Cree and Oji-Cree people. These seasons are Spring (See-Kwan), Break-up (Mee-Noh-S-Kah-Mee-N), Summer (Nee-Peh-N), Fall (Tah-Kwa-Kah-N), Freeze-up (Mee-Kee-Ska-Ow) and Winter (Pee-Poh-N). It also provides examples of predicting the weather using traditional knowledge and encourages students to learn more from elders and others in the community.

The teacher resource is a great teaching tool for any school and is designed to be adapted to any curriculum or school. It was created using the traditional knowledge of the Cree and Oji-Cree people in addition to scientific facts about climate change. The book was created for the NAN area but is also available for non-Native schools and educators who can use this resource to introduce their students to the Native culture.

It is nice to know that there are people who are actively working to teach our young people the accumulated knowledge of my people. It is good to see this knowledge being used to keep our students aware of their Native heritage.

A note about the author

Xavier Kataquapi is a 26 year old Cree freelance writer. He was born and raised in Attawapiskat, Ontario on the James Bay coast. He writes mainly for First Nation media and has a column titled 'Under The Northern Sky' which he has been writing since 1999. He writes mostly about First Nation issues and his life growing up on the land on the James Bay coast. He speaks the language of the James Bay Cree people and his second language is English. He enjoys the traditional pursuits of his people and discovering new cultures and places in the world.

Note from the Editor: The Editor wishes to thank Robert de Chancenotte, Meteorologist, Montréal, for pointing out this interesting article about weather prediction and climate within the Native Community in Canada.

[1] Reprinted with permission of the Editor, The Nation (December 27, 2002, page 23).

Communicating Complex Science: Has the Science Community Failed?

by Henry Hengeveld[2]

What do mad cow disease, genetic engineering, SARS and climate change have in common?

Plenty. First, they are all complex scientific issues that involve the well-being of society. Secondly, each is still inadequately understood, and hence subject to major scientific uncertainties. Third, because of the human dimension, each is of immense interest to the public, and hence to policy-makers. This interest not only involves us personally, but also implicates future generations - that is, our children and grandchildren.

The above factors, and others, lead to a fourth commonality. Precisely because, in each case, the science is complex and inadequately understood, scientists are reluctant to talk about it outside of their peer community. This reluctance is, after all, consistent with their training. Yet, society, because of the importance of these issues to the well-being of humans, has questions to which it wants answers, and in terms that it can use and understand. Hence each of the above issues is also complicated by a major communication gap between science and society.

Take, for example, climate change. For a number of decades, scientists have been beavering away to address the various elements of the climate system puzzle and to develop models of that system that can help understand the complex linkages between climate and society. Currently, some 2000 new climate change-related papers are published in peer-reviewed journals each year, and more than 20,000 such knowledge 'pieces' have accumulated over the past 20 years. We have learned a lot! Through the Intergovernmental Panel on Climate Change (IPCC), three major assessments of what we know and don't know have now been prepared. These assessments have each involved several thousand scientific experts (including a number of CMOS members) and produced thousands of pages of information. The fundamental conclusions, achieved with due consideration of the major uncertainties still remaining, are that:

- the climate system is changing;
- humans are the likely cause of most of the changes during the past 50 years;
- without direct policy action, human-induced changes in the Earth's climate during the next century will almost certainly be unprecedented in human history and could achieve catastrophic proportions;
- these projected changes spell 'T-R-O-U-B-L-E', particularly for the developing world and future generations.

Yet, after the release of the second IPCC assessment in 1995, Fred Seitz, past president of the U.S. National Academies of Sciences, publicly voiced his concern that he had "never witnessed a more disturbing corruption of the peer-review process than the events that led to this IPCC report" (Seitz 1996). After the Third Assessment Report was released, Chris DeFreitas (a Canadian climatologist now at the University of Auckland) advised Canadian petroleum geologists that, "by failing to convey a balanced representation of the science presented in the detailed reports, the Summary For Policy Makers, along with the IPCC press releases, have become a tool to drive public hysteria." (De Freitas 2002). Harvard astrophysicist Sallie Baliunas, with Carleton University geologist Tim Patterson and engineer Allan McCrae, wrote in Canada's national newspaper that "...when it comes to climate change, humans aren't the culprits." (Baliunas et al, 2002). On November 25th, just prior to Canada's ratification of the Kyoto Protocol, 27 dissenting scientists, including eleven Canadians, advised our Prime Minster in an open letter that "many climate science experts from Canada and around the world...strongly disagree with the scientific rationale for the Kyoto Accord...The views of dissenting scientists have not been properly heard or considered by the government." (Patterson et al. 2002). Correspondence by CMOS members to the *CMOS Bulletin SCMO* has added to these. Madhav Khandekar, in commenting on the IPCC process, notes that the "very science that brought in the Kyoto protocol is now being excluded from debate" (Khandekar 2002), while Dick Morgan indicates that he is "appalled by the number of IPCC statements which, without qualification, are inaccurate." (Morgan 2003).

Now, if you were a member of the public listening to this, wouldn't you be confused?

I have little expertise on mad cow disease, SARS or genetic engineering. For these, I am just a member of the general public asking the scientists involved whether they are sure that they have properly considered the long-term risks to society, and about the ethical aspects of their work. Perhaps my science background gives me an edge over

[2] Science Assessment and Integration Branch, Atmospheric and Climate Sciences Directorate, Meteorological Service of Canada, Downsview,ON.

others in understanding the logic presented, but I have little more understanding of body chemistry, genetics or diseases than the person standing next to me on the bus.

However, after more than two decades spent trying to bridge the gulf between science and policy/public on the climate change issue, I feel somewhat compelled to join others who have challenged how the science community deals with complex scientific issues, and how we communicate our knowledge, or lack thereof, to a society that is vitally interested and, ultimately, is both our employer and client. My purpose is not to defend the quality of the science, or the IPCC. Others have done so, and – besides – that would take more than a short article. Rather, I would like to take a somewhat more philosophical look at *how* we do and communicate science. Are we failing our clients because the traditional paradigm behind that science, which has served us so well in the past, may be failing us when it comes to complex systems?

The following is an evolution of related thoughts that I presented in a seminar to colleagues at the MSC Downsview facilities on - of all days - April 1st of this year. They have benefited from feedback during that seminar, as well as subsequent comments from colleagues (particularly Elizabeth Bush). I present these thoughts here, not as a statement of facts, but as a mixture of borrowed concepts and personal reflections. Perhaps they can serve as a basis for inviting/provoking other CMOS members to talk about the issue as well.

Normal Science – our heritage, and our solid foundation for public service

All of us were trained in the 'scientific method'. Our reports on science experiments in secondary school were already formatted in a logical sequence of steps that began with purpose and/or hypothesis, followed by a description of method of investigation, then by presentation of observations and results of our experiments. They concluded with discussion and findings. As we moved through our university programs, we were constantly challenged to follow this process in a manner that was objective, open, transparent, imbued with integrity. We were taught to submit our results to peers within our disciplines for internalized debate and critique and, in turn, to critique the results of others. Such critique focused on what we disagreed on or questioned, not on what we could agree on. We were encouraged to use an adversarial approach to debate that sought out weaknesses in the conclusions of our peers. As science philosopher Popper (1979) put it so well many years ago, "our belief in any particular natural law cannot have a safer basis than our unsuccessful attempts to refute it".

This approach to science, because of its reliance on 'objectivity', sought to separate nature and science from society. Hence, under this paradigm, society had no business telling scientists what to study, or how. Conversely, scientists needed training only in how to communicate with peers, not with society. It also used incrementality, where new science was solidly based on the heritage of science previously explored by others. This inherited science, because it had withstood the tests of time, could be used, without much critical thought, both as the foundation and as the guide for further investigation into the unknown. It was part of the paradigm within which we conducted our research. Occasionally, an Einstein might come along to successfully challenge this foundation and cause a scientific 'revolution'. That would cause an earthquake within the system that could take years of rebuilding around a new 'paradigm', but that was also part of the 'method' - seeking truth through proper process. As science philosopher Thomas Kuhn defined it many years ago (Kuhn, 1962), this 'normal' scientific method was a puzzle solving process. In this context, the only real role for society was to keep funding science and to become the rather passive but fortunate recipient of all the good that such puzzle solving could provide. Science communication, to a large extent, involved a one-way transfer of knowledge and wisdom, usually through applied science and engineering, to policy-makers and the public.

This process has worked quite well for many decades, and has indeed helped society immensely. It also worked well for decision-makers. When they needed scientific advice, they would establish committees and conduct hearings where they could invite scientists to testify, assuming that the committee members (generally non-scientists) had the wisdom and understanding to sort scientific truth from fiction. They hired advisors and consultants to help sort out the relevant information and apply it to the question at hand. Occasionally they requested formal assessments from the expert community. *In all of this, the over-riding assumption tended to be that systems and innovations could be assumed safe unless proven dangerous.*

Complex Systems – a new challenge for science and society!

The world is changing. Society is becoming increasingly complex – better educated, more institutionalized, more mobile, increasingly globalized, much more dependent on technology, more demanding. Not surprisingly, science is also becoming more complex. Never before have we been able to manipulate genes of living organisms, transmit diseases around the world within days, or significantly change the composition of the Earth's atmosphere. Furthermore, as the complexity increases, so does the inter-connectedness within and between natural and social systems. In fact, the separation of nature and society, a basic plank in the paradigm of normal science, is no longer possible. Perhaps it never really was! This is where the conventional 'scientific method', though still the foundation of sound science, begins to run into serious problems, and where society no longer fully trusts science as a source of only good. Society now wants - and should have - a voice in why, how and what science is undertaken, and how it is used. Ethics become an integral part of the equation! This is particularly true when the scientific uncertainties are high, but when the stakes for society are also very high. In cases such as these, policy-makers cannot afford to wait until the uncertainties are resolved. They need to make decisions *now*, in the face of these uncertainties but with the best advice that science can offer. Suddenly, scientists find themselves expected to talk about a complex, uncertain issue to a client whose language they don't understand, and who they were never trained to communicate with.

Ravetz (1999, 2002) and others have argued that one of the reasons that normal science has difficulty with understanding and communicating information about complex systems is its reductionist approach to such systems. That is, we take the system apart, and assign different elements of the system to various specialist groups. Each group then explores that element in detail, learning more and more about smaller and smaller pieces of the puzzle. They share this increased knowledge with peers within their specialty. However, because of the complexity of the system and the specialized knowledge required to understand the details of the component parts, they end up knowing less and less about parts of the system other than their own. How can any one keep track, for example, of several thousand new research papers each year on the full range of climate change science and still have time to do their own research?

This works fine as long as the broader community has processes in place to put all the elements together again. Without this process, all the detailed knowledge has little value. This is somewhat analogous to the fable about the three blind men asked to use their hands to identify an elephant. One grabbed the elephant's trunk, felt it move and sway, and declared it to be a snake. Another grasped the tail, pulled hard and stated that he held a rope. The third wrapped his arms around a leg, felt the roughness of the skin, and identified it as a tree. Without properly adding up the pieces, they had lost the bigger picture. Likewise, with complex scientific issues, few if any experts can put together a complete puzzle on their own. They must rely on the help of, and trust, experts working on other parts of the system. Then they must, collectively, tell others about the picture that emerges! Furthermore, they need to recognize that nature-society linkages within the system need to be included.

Powell and Leiss (1997) address another aspect of the challenge of communicating the science of complex systems to non-scientists - that of language and perception. They note that scientists, in keeping with their training, communicate their findings through technical documents published in peer-reviewed, discipline-specific journals and/or as government reports. From time to time, they provide reviews or expert risk assessments that attempt to synthesize their state of knowledge. These documents and assessments use scientific jargon and processes of reasoning, present risks in terms of probabilities and levels of acceptability, and almost always argue for better knowledge before sound conclusions can be presented. Furthermore, the risks are usually described in comparative terms, based on population ratios, and on the principle that a death is a death. In contrast, most of the public obtains its knowledge through media, off-hand remarks by officials and politicians, and from reports prepared by special interest groups. They learn by intuition and comparison with personal experiences. They don't care about probability statistics, but want to know whether or not an event will happen and whether they will personally be impacted. They also don't just care about the risk of dying, but also about how they die. Hence, their perception of risk is vastly different from that of the expert community. This results in a large communication vacuum between science and public. When the uncertainty and the stakes are both high, who is to say which perception is the more appropriate?

Post-normal inquiry: An evolution in science communication?

About a decade ago, Funtowicz and Ravetz (1992) proposed an alternative approach to 'normal' scientific investigation that would help address some of the above concerns – an approach that they dubbed 'post-normal science', or PNS. They argued that normal science, when faced with complex systems, has failed us in many ways. PNS, they suggest, can help redress some of these failures, precisely because it accepts that complex science is value-laden, that nature, science and society are no longer separable, and that scientific uncertainty is not a reason for avoiding relevant dialogue with society (Ravetz 1999). Hence, PNS can help bridge the communication gap between science and policy. Others (Mehta 2002; Dempster, 1998) have welcomed this new concept, although they have in general been less harsh in their criticism of normal science – a perspective I am personally more comfortable with. They note that normal science remains the sound underpinning of how we should do science, but that PNS complements this by expanding the process of peer review to include cross-discipline assessments, inputs from other sources such as traditional knowledge, and critique from stakeholders within society. Thus, while revolutionizing how we communicate science, PNS does not replace normal science.

Needless to say, the concept of PNS leaves many scientists very nervous, and some quite vexed. This paradigm shift in how we approach science, at least at first glance, appears to go against all our formal training on objectivity and separation of nature and society. On the other hand, to the meteorological community, this may not seem like a revolution at all, but business as usual. In some respects, meteorologists have long recognized, and effectively dealt with, the challenge of communicating complex science in terms that the public understands and can use. If a forecaster were to declare that, given the uncertainty in the evolution of the weather system over the next 24 hours, he/she lacked the confidence to provide a prediction, the public would not be well served! Nor would it be helpful to add a number of caveats to the forecast. Rather, the users want to know whether or not they are likely to experience precipitation tomorrow, and if so, when, how much, in what form and with what personal risks. A one liner, please! Over time, by listening to the general public, our forecasters have become quite good in understanding the needs and perceptions of their clients, and they have adapted their forecasts to keep them relevant and comprehensible.

However, most climate system scientists have been less successful in bridging this communication gap between science and the public. Here, as in so many other areas of science, experts have continued to use the reductionist approach to complex systems, and then to rely on models to put the pieces of the system together again. Through testing and experimentation, new pieces and feedbacks are continually added and the performance of the models is enhanced. Give them enough time, and they will come up with a good model that works well. Just don't rush the process and expect any early predictions!

This approach has some serious challenges to overcome. First, the climate system is indeed very complex, involving intricate, non-linear feedbacks between almost every aspect of earth sciences. These take place at temporal and physical scales that range from seconds and micrometers to millennia and planetary. This is no longer a science that can be neatly divided and debated within disciplines, but demands cross-discipline collaboration. Secondly, it is no longer a system that can be analyzed in a truly objective manner, independent of human society and its behaviour. In this system, nature and society are now unavoidably linked. Hence, this linkage needs to be integrated into the simulations and speculations of how the system will behave and change over time. Third, the range of potential climate change impacts on ecosystems and society include the real risk of catastrophe at scales unprecedented since at least the last deglaciation, particularly if the climate system should go into convulsions. For these reasons, *inter alia*, the climate change issue becomes too important to leave to scientists alone. Like with health issues similar to that of mad cow disease and the current concern about Severe Acute Respiratory Syndrome (SARS), policy-makers and the public rightfully need and demand advice from the expert community, including worst-case scenarios, regardless of whether these experts fully understand the problem or not. 'I have no advice', or 'We don't as yet have evidence that there is cause for concern' become unacceptable answers. Furthermore, society demands a voice in how and what science is undertaken and how the conclusions are developed. In other words, the public wants a stake in the science debate, and wants scientists to listen to them as well as to advise them!

In the late 1980s, well before the seminal paper on PNS by Funtowicz and Ravetz, the United Nations General Assembly recognized the complex and value-laden nature of climate change and therefore asked the WMO and UNEP to collaborate in establishing a scientific assessment and advisory process on climate change for its member nations. The result was the IPCC. The IPCC, in turn, developed an assessment process that included full engagement and ownership of both the relevant international science communities and of the international policy-making community. This joint ownership of process both ensured scientific credibility of the assessments *and* allowed the communication of assessment results to decision-makers in an effective and comprehensible manner.

The underpinning of the IPCC process is a rather conventional reductionist approach to science assessment. Lead experts on specific sub-themes, identified on the basis of their publication records and recognition within the science community, are invited to convene a team of peers to undertake a comprehensive literature review of that sub-theme and to prepare a related chapter for the full assessment. The first draft of that review chapter is then submitted to the broader international peer review community for critique and additional input. Those providing significant new information for inclusion are then included as contributing authors. The second draft, having incorporated the responses from this first peer review, is then submitted to the peer science community for a second review. However, it is also forwarded to member governments involved in the IPCC process for review and critique. In most cases, governments circulated the reports to a broader review community, including some non-scientists. The chapter authors then undertake a third re-draft, and add a shorter chapter summary, somewhat similar to an expanded abstract. In the Working Group I contribution to the IPCC's Third Assessment Report (TAR) on the climate system science, the above process involved more than 600 authors from the expert community and 420 additional reviewers. Building upon earlier assessments, the report considered the accumulated results of some 7000 previously published papers and documents, primarily from the peer-reviewed literature. The extended review involved in preparing the assessments is both unprecedented and consistent with one of the key elements of PNS – that of expanded peer and stakeholder review.

However, it is in the development of the Summary for Policy Makers (SPM) and the Synthesis Report that the PNS approach becomes most apparent (Saloranta 2001). For each of the three subsidiary Working Groups, the IPCC Bureau, in collaboration with the lead authors of each chapter, develops an integrated overview of the key results of contributing chapters. The overview involves a major condensation of results. For example, the component of the SPM representing Chapter 3 on climate system observations condenses 60 pages of details into three pages of highlights, much like a paper abstract. This overview is then submitted to countries for formal comments and subsequently debated, line-by-line, in plenary session attended by government representatives to the Working Group (most of whom also have climate science backgrounds). Lead authors for the contributing chapters are in attendance to advise on the intent of the wording of the summary, to identify where it is discussed within the chapters, and to ensure that any re-wording does not alter the intent. The final product thus becomes a bridging of the perspectives forwarded by scientists, using their logical approach to puzzle solving, and non-scientists who approach the science much more in terms of relevance to policy issues. It represents an added-value product that has the approbation and comfort of the policy community in terms of its relevance to governments and of the lead authors in terms of scientific integrity and credibility.

There are also other emerging types of PNS that can contribute to bridging the science-policy chasm. For example, in recent years, some researchers have used the Participatory Integrated Assessment (PIA) approach as an effective way for working with stakeholders and policy-makers (Cohen 1999, Toth 2001). This approach considers not only the uncertainties and complexities of the issue at hand, but the diversity of values that must be considered. It thus takes into account the social context in which scientific and political activities operate. Others have further complemented PIA activities with the use of Integrated Assessment Models and related tools such as Interactive Scenario Scanners or Safe Landing Analyses (Rotmans 2001). These can be employed to undertake theoretical gaming exercises involving both scientists and policy-makers, much like the military often do to better understand the risks and possibilities and develop effective strategies. As with the participatory assessment process used by the IPCC, these approaches encourage interactive dialogue among scientists, stakeholders and policy makers, allowing these communities to listen to and learn from each other. Unfortunately, it is also an area of activity in which very few Canadian scientists are as yet engaged.

It is precisely this post-normal science approach (or whatever we wish to call it) that, I believe, has distressed - even infuriated - some within the science community, resulting in the contrarian rhetoric noted previously. Most (although not all) of these contrarians rather grudgingly accept that the chapters within the IPCC assessments are credible summaries of the science. However, they argue that the collaboration between scientists and government representatives in preparing the SPMs is a corruption of the science involved, resulting in significant biases in the final product. They, in essence, reject PNS as an acceptable process. However, major scientific bodies have provided some welcome and overdue support for the process. For example, the U.S. National Academies of Sciences (2001), in a report solicited by and submitted to the White House, advised President George W. Bush that the full IPCC TAR WG I report is 'an admirable summary of research activities in climate science'. It further noted that, while the SPM puts stronger emphasis on concerns and less emphasis on uncertainties than the full report, all changes in the text were made with the consent of the convening lead authors and that most changes had little impact on the substance. About the same time, Academies of Sciences from 17 other countries (including the Royal Society of Canada) published a joint statement in the journal Science indicating that, in their perspective, 'the work of the IPCC represents the consensus of the international science community on climate change science' (Australian Academy of Sciences et al 2001). They also endorsed the method used by the IPCC in developing this consensus. The American Meteorological Society and the University Corporation for Atmospheric Research have also strongly endorsed the IPCC process (Avery et al 1996).

Some Concluding Thoughts

The conventional scientific method of investigation, including adversarial debate internal to the science community, is an essential and integral part of advancing scientific knowledge, and provides the foundations for sound scientific advice. It helps ensure objectivity, transparency and repeatability. It reduces the influence of personal biases and uncritical assumptions on results, helps build confidence in that which survives challenges by others, and encourages renewed efforts to address that which does not. Hence 'normal' science must remain the fundamental basis of how we pursue climate system and atmospheric sciences.

However, when dealing with complex systems such as those of the Earth's climate, where the effect of human activities is an inherent element of that which is being investigated, there must also be a shift in the beliefs and principles that we apply to our investigations. First, we must recognize the inter-connectedness of nature, science and society, and hence also give society its due role in formulating the science agenda. We must listen to the public's perspectives and concerns, and must formulate our research results in a language and construct that responds to these. As already noted, weather forecasters do that quite successfully. Now climate scientists should as well. Some may call this post-normal science. Others may consider it a terrible distortion of how we really should be doing science. I prefer to think of it as fulfilling our ultimate *raison d'être* – helping to provide sound advice for the well-being of society and the pursuit of sound planetary stewardship. As noted by more than 100 Nobel laureates (including 66 physicists and chemists) in a statement released a little over a year ago, "to survive in the world we have transformed, we must learn to think in a new way. As never before, the future of each depends on the good of all" (Alferov et al 2001).

To do so will demand that scientists move beyond adversarial debate about what they disagree on. They must, from time to time, undertake assessment of collective results obtainable from the many pieces of the puzzle already available and state that which they can *agree* on. That is what the IPCC has done for climate change. Perhaps we also need a similar assessment at the national level (as the U.S. NAS has done), although it is unlikely to differ significantly from an international assessment based on the same data. Finally, while a number of CMOS members already do so, more scientists should be prepared to actively communicate the conclusions of such assessments, as well as the results of their own research, to policy-makers and the public, whether through the media, through letters and op-ed pieces, or through public presentations to their local communities. As noted in a recent commentary in Nature (Willems 2003), that may require both training to de-jargon our information and to learn to use a cognitive language more familiar to non-scientists. It also means addressing many of the institutional barriers that hinder us from doing so.

CMOS, as a scientific NGO, is exactly the kind of organization that is well suited, and therefore ought to be at the front lines in building these bridges. It should help its members to span the gulf between science, the policy community and public on issues related to atmospheric, ocean, hydrological, cryospheric, and other related sciences - especially climate change. It is great to see that various leaders in the CMOS community have already begun to argue for precisely such action (Bianchi, 2002; Strong, 2002).

Lets talk about it!

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Next Issue *CMOS Bulletin SCMO*

Next issue of the *CMOS Bulletin SCMO* will be published in August 2003. Please send your articles, notes, workshop reports or news items at the earliest to the address given on page ii. We have an **URGENT** need for your articles.

Prochain numéro du *CMOS Bulletin SCMO*

Le prochain numéro du *CMOS Bulletin SCMO* paraîtra en août 2003. Prière de nous faire parvenir au plus tôt vos articles, notes, rapports d'atelier ou nouvelles à l'adresse indiquée à la page ii. Nous avons un besoin **URGENT** d'articles.

The Seconded Meteorologists

by Morley Thomas[3]

Meteorologists today may be interested, and perhaps amused, to learn that decades ago, federal government policy encouraged the Meteorological Service to undertake meteorological applications work for other government agencies without cost recovery. In fact, not only did the Service do this but it also loaned or "seconded" meteorologists to some.

In the 1950s and 1960s, a dozen or so meteorologists were seconded to work with other agencies. The arrangement was simple; the meteorologists' salaries were paid by the Meteorological Service while travel and any other expenditures were the responsibility of the host departments who determined the meteorologists' work program. By remaining employees of the Meteorological Service the seconded meteorologists were eligible to apply in competitions for other positions. The host departments were delighted with a program that cost them no salaries or person-years and they repeatedly asked for more secondments.

How did the program get started? In a sense, the concept was first used during World War II when John Patterson, head of the Meteorological Service (called the Meteorological Division and then in the Department of Transport), convinced the Royal Canadian Air Force authorities that they should not attempt to hire or train their own meteorologists but ask the Met Service for professional assistance. Patterson had the complete support of Transport officials and subsequently provided hundreds of civilian meteorologists to the RCAF during the war. But these professionals were not seconded in the true sense since not only pay but also travel expenses, training and postings were handled by the Met Service. On the RCAF stations, however, the meteorologists were part of station operations, reported to the station commanders and had supervisory responsibility for the airmen and airwomen assistants in the Meteorological Section.

During and after the war there were meteorologists posted to the Defence Research Establishment at Suffield, Alberta, and National Defence also requested and obtained the loan of meteorologists for such cold weather exercises as Muskox and the development of radar. But these postings were not part of the secondment program which was to begin after the war.

Who were seconded ?

The idea of seconding meteorologists to civil departments seems to have arisen a few years after the war and by chance I happened to be one of the first involved. In May 1949 I had just completed the MA course when I was asked by Robert Leggett to join the National Research Council as his Building Research Climatologist. I was very interested but since the Met Service had paid my salary for the months when I was on the MA course, I told him I could not just resign and that I would have to discuss the matter with Andrew Thomson, then the Met Service head. Leggett and Thomson soon agreed that the Met Service would second a meteorologist to NRC but a competition would have to be held for the position. I applied and ranked third on the eligible list. When the two more senior meteorologists turned down the position I happily accepted and reported to the Division of Building Research in March 1951.

It appears that the first meteorologist to actually go to a host department was Art Grant who went to the Department of Mines and Technical Surveys in November 1950 to advise on meteorology in aerial surveying. During the next year, besides my posting to the Division of Building Research, G.C.W. Tait went to the Atomic Energy Commission (AEC) in Chalk River, George Robertson to the Department of Agriculture, Hugh Cameron to the Department of Forestry and Jack Turner to the British Columbia Forestry Service. In 1952 Harold Baynton went to the International Joint Commission for air pollution work at Windsor and in 1953 George Gilbert went to the Defence Research Board. In 1955 Jim Bruce went to the Ontario Conservation Branch at Toronto for flood control work.

From the 1950s through most of the 1960s some seconded meteorologists moved on to other positions while additional secondments were made to some agencies. Tait resigned in 1952 and Grant in 1954; neither was replaced. Don Boyd replaced me in the Building Research position in 1953. Stu Edey and Dan Williams joined Robertson at Agriculture in 1957 and 1960, respectively. When Cameron took a new secondment with the Canadian Army in 1954, Les MacHattie replaced him at Forestry where he was joined by Mike Webb in 1964. Ted Munn replaced Baynton at Windsor in 1956 and Don McMullen succeeded Jim Bruce in 1958. The last new secondment was in 1959 when Gord McKay went to the Prairie Farm Rehabilitation Administration offices in Regina.

During these years other meteorologists took postings that were somewhat like the secondments. For example, Ken Pettit flew with the NRC/RCAF "Ice wagon" doing icing research and Don Storr worked on the Alberta east slopes watershed research project. Meteorologists who worked at Suffield after the war included Bill Clink, Olie Johnson, E.J. Kermode, Dick Longley, Walter Halina and Ted Walker. On an administrative/executive level, Des Kennedy was posted in 1946 to National Defence where he became the Director

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of Meteorology and Oceanography. Harry Tucker joined Kennedy at DND in 1953; earlier he had been posted to TCA headquarters where Mac Elsley followed him.

Policy Change
Most seconded meteorologists were engaged in applications, that is the application of meteorology and climatology for the health, safety and economic advantage of Canadians. Both the private sector and government departments were becoming increasingly aware of the value of applied meteorology in the 1950s and 1960s and some meteorologists were hired away from the Met Service. Also, Met Service policy on applications work began to change and new specialist positions were created within the Service rather than seconding more meteorologists to other agencies. For example, by 1960, the Head Office Climatological Section had created and begun to staff hydrometeorological, micrometeorological and arctic units to work alongside the traditional climatological operations units.

In the mid-1960s the Met Service decided to discontinue long-term secondments. The half-dozen or so remaining host agencies were eager to keep their meteorologists and absorbed most of them into their own establishments. By the end of 1969 all secondments had ceased except for Don Boyd with Building Research.

Because of the growing demands for applied meteorology services, each of the Regions had established Scientific Services units by 1973. These flourished for a number of years before government policy changed radically, cost-recovery was mandated and budgets slashed. Until this time Meteorological Service policy was undoubtedly hard on the few private meteorological firms that had begun to spring up. But the "free" service policy begun with the secondment program did introduce an awareness and appreciation of meteorology in several Canadian economic and environmental sectors.

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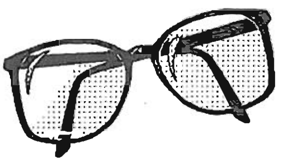
GEOSPHERE-BIOSPHERE INTERACTIONS AND CLIMATE

Edited by L.O. Bengtsson and C.U. Hammer

Cambridge University Press, New York, USA
302 pp. ISBN 0-521-78238-4; US$ 74.95

Book reviewed by Charles T. Schafer[1]



This 2001 publication summarizes the deliberations of a working group that was convened in November, 1998. It was sponsored by the Pontifical Academy of Sciences and the International Geosphere-Biosphere Programme. One of the working group's key goals was to examine relationships between biogeochemical cycles and climate. The book's subject matter is divided into five parts (anthropogenic problems, the human perspective, modelling, paleoclimate, and future strategies) comprising a total of 18 chapters. Most of these have been written by European scientists with critical contributions on atmospheric carbon dioxide variation, carbon sources and sinks, the human side of climate change, long term climate stability, and the greenhouse implications seen in the paleoclimatic record by six American and one Canadian researcher respectively. The work is aimed at researchers and graduate students seeking a comprehensive overview of current understandings and theories on interactions among geosphere, biosphere and climate. However, the precautionary and conservative style used throughout the text makes it a very useful source for a broader audience in distinguishing fact from theory in the ongoing climate change debate.

The format of the 18 chapters tends to vary somewhat; some start with an abstract while others begin with an introduction. There is also quite a range in the length of chapters. Nevertheless, the subject material of even the shorter contributions is clearly germane to the "big picture" of global climate change. Their inclusion in the publication is a credit to the foresight of the editors. The book has been dedicated to the late Hans Oeschger, Professor Emeritus of Physics, University of Bern who became world famous for his work on measuring radiocarbon in very small samples of carbon dioxide.

Chapter 1 stands apart from the five part framework of the book. It deals with the "ozone hole" issue and points out at the start that this feature was not predicted by any model, came as a total surprise to all scientists, and had developed at a "least likely" location. Anthropogenic problems are treated in Part I which covers feedback loops, carbon dioxide variations, modelling, and carbon sources and sinks. Inez Fung's paper explains why future growth rates of atmospheric carbon dioxide will not only follow anthropogenic inputs but will also be influenced by those terrestrial and ocean carbon dynamics that are an inherent feature of a changing climate. Martin Heimann talks about why coupling of the terrestrial carbon cycle to the hydrologic cycle is an important next step in numerical model development. Wallace Broecker cautions that numerical modellers are "a long way from producing simulations that adequately replicate [the ocean's] thermohaline ventilation".

In the "Human Perspective" treated in Part II, Stephen Schneider considers, in the context of policy options, whether society's contemporary actions are causing the climate to change in ways, or at rates, that will threaten natural systems or make human adaptations difficult. As an example of the conservative approach seen throughout this book, Schneider reminds the reader that there are "no analog habitats" in the fossil record for many of the climate impacts that we are witnessing today.

Modelling the Earth's system is the focus of Part III. Its chapters cover the history of the development of Earth System Models and the importance of this tool in understanding how a host of ecosystems-associated properties will change as systems are "forced" by changes to water, energy and carbon dioxide. Andre Berger's sensitivity experiments on the role of carbon dioxide, sea level and vegetation in relation to glacial-interglacial cycles indicate that the concept of the 100 kyr cycle cannot be sustained if these parameters are not taken into account. Thomas Stocker's review of nonlinearities in the Earth's system attempts to demonstrate how they can give rise to multiple equilibria states, and why it is of paramount importance to understand the forces that have imprinted these events in paleoclimate archives throughout the world. The review by J.E. Kutzback on simulating the climate of the Holocene (i.e., the last 10,000 years) argues that, to date, modelling has not been able to simulate the full magnitude or the spatial and temporal structure of Holocene climate change. In his chapter on climate-terrestrial biosphere interactions, Iain Prentice suggests that it is more important than ever to understand how they were regulated naturally in the past. He describes how dynamical global vegetation models or DGVM's are being developed for this purpose.

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Parts IV and V of the book deal respectively with the paleoclimate record and the question of how to meet the challenges of a changing climate. The papers contained in these sections indicate that the climate change problem is not one that we might reasonably expect to be settled on the basis of model results alone, how very long-term evaluations (millions to billions of years) can assist in our understanding of climate change on a decadal to millennial scale, and what links can be seen between the ocean's thermohaline circulation and the carbon dioxide cycle during previous glacial-interglacial cycles. It will be necessary to explain how these features of the Earth's system are related to the Dansgaard-Oeschger and Heinrich events. J. Duplessy remarks that despite all of the currently available historical information, climate change science is still not yet able to satisfactorily explain glacial-interglacial carbon dioxide variations.

Part V of the book offers several interesting perspectives on how to meet the challenges that lie ahead. Its chapters include arguments on mitigation versus adaptation, getting from a "pollution pipe" to a "systems" approach that can deal with interacting biophysical and socioeconomic components, and how the World Climate Research Programme (WARP) is working toward its primary goal "to understand and predict - to the extent possible - climate variability and change, including human influences". The last chapter of Part V is an overview by Martin Heimann of a panel discussion on future research objectives. Panel participants voiced their concerns about several critical issues such as the widespread "downsizing" of established surface-based observational services in many parts of the world and how it seems to be tied to a deterioration of those political and administrative structures that are needed to maintain long term measurement networks.

Although many of the contributors to this publication show an apparent bias toward the International Panel on Climate Change side of the debate, the combination of information on current limitations of climate change modelling, in relation to what is known from contemporary physical measurement networks and fossil archives, makes reading this book a very satisfying and enlightening experience. If time does not allow for a complete read of the rich and timely material found throughout this text, a casual "cruise" of its many carefully selected figures will likely bring the reader back to some of the more detailed messages and ideas that can be found in the writings of the authors.

Geological Survey of Canada Contribution No. 2002185.

The Earth's Plasmasphere

J.F. Lemaire and K.I. Gringauz

Cambridge University Press, New York, USA
Cambridge Atmospheric and Space Sciences Series
ISBN 0-521-43091-7

Book reviewed by Konstantin Kabin[2]

The book by J.F. Lemaire and K.I. Gringauz is the only scientific monograph to-date that is devoted exclusively to the Earth's plasmasphere. Although D.L. Carpenter is listed as a contributor rather than a co-author (as mentioned in the introduction, this was his personal preference), he is the sole author of the second chapter and a co-author of the first chapter. A publication listing on the front page Gringauz and Carpenter, the two famous pioneers of plasmasphere science, clearly deserves attention from anybody interested in Space Physics. I believe that "The Earth's Plasmasphere" will be the standard reference on the subject for years to come.

The first three chapters of the book provide a very extensive review of the experimental results. Although the results are seldom described in detail, the book provides enough information to direct an interested researcher to the relevant original publications. This literature review is particularly valuable because many of the discussed publications are too old to be found in the modern electronic databases. Finding them in a traditional library may easily take weeks of work. The results are presented in a historical order, and as mentioned in the Preface, "this book is much more than a monograph about a scientific topic; it also provides a historical account of the growth of a new field of research". This account is surely very enjoyable reading for the scientists old enough to have participated in the discovery of the plasmasphere; to a younger generation of scientists it gives a unique prospective on how the discoveries were made.

Today's student, who knows about the existence of the plasmapause from the very first space physics class that he took, may find inspiration and excitement in a first-hand account of the controversies and hot debates which surrounded the now-so-familiar concepts some 40 years ago.

Chapter 4 of the book is essentially a summary of the previous chapters. While necessarily repetitive, it provides a good phenomenological description of the physical phenomena related to the plasmasphere and plasmapause. As mentioned several times in the book, not all pieces of the puzzle are yet in place, and there are many gaps in our understanding of plasmasphere which have to be filled. In

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fact, section 4.11 has suggestions for many PhD theses to come. Chapter 5 outlines the progress in modelling the Earth's plasmasphere.

It is only natural that a first monograph on the subject, with different sections written by different authors, has certain personal biases and discontinuities of presentation. For example, while plasma measurements, both in situ by spacecraft and indirectly by whistler wave observations, are discussed in great detail, nothing is said about magnetic field measurements, and magnetic field models are only briefly mentioned on just one page. The "interchange motion" is well discussed in section 5.4, but without any references to observations. The criticism of the fluid theory presented in chapter 5 may not be entirely shared by everybody in the community.

With all the merits this book has, it should not be taken for what it is not, and it is not a student textbook. "The Earth's Plasmasphere" will be very useful for a researcher who is already well familiar with the subject, but a beginner not aware of the principal results and not familiar with the common abbreviations and jargon of the field will likely feel lost.

For an inexperienced reader the historical perspective taken by the authors presents an additional challenge and the whole book may appear to be a baffling mosaic of apparently disjointed facts, only obscuring the large-scale picture of the plasmasphere.

For readers to benefit from this book, they should start with a general picture of plasmasphere already in their mind. Another deficiency of the book, which can be a hurdle to a casual reader, is the concise index and table of contents. Somebody who did not read the entire book may have some difficulty locating the relevant information. Chapter 2 alone has 48 unnumbered sub-sections not reflected in the table of contents!

Over all, the book contains a wealth of material and presents a unique historical perspective. Although not perfect in every aspect, it is clearly a useful addition to the library of any active researcher in the field of plasmasphere physics. "The Earth's Plasmasphere" can be, however, a difficult read for an outsider.

Synoptic and Dynamic Climatology

Roger G. Barry and Andrew M. Carleton

New York: Routledge, 2001. 620 pp., $60.00US
ISBN 0-415-03116-8

Book reviewed by Steven Quiring[3]

The origins of *Synoptic and Dynamic Climatology* can be traced back to an earlier work written by R. G. Barry and A. H. Perry entitled *Synoptic Climatology: Methods and Applications* (1973). In the nearly thirty years that have passed since that book was published, the discipline of climatology has experienced significant growth and numerous theoretical and methodological advances have been made. R. G. Barry and A. M. Carleton wrote *Synoptic and Dynamic Climatology* to serve as a comprehensive and thoroughly up-to-date guide for these two closely related fields of climatology.

Barry and Carleton's book is divided into three main sections. The first section provides an introduction to the global climate system (Chapter 1) and the important space-time scales in weather and climate processes. The first section also covers the collection and analysis of climate data (Chapter 2). Chapter 2 provides an in-depth discussion of satellite climatology that covers the history, the basic principles, and the climatological applications. Chapter 2 also contains a useful overview of some of the basic statistical techniques used to describe and investigate climate data such as probability density functions, time series analysis, empirical orthogonal functions, and interpolation.

The second section deals with dynamic climatology, the field that examines the underlying forces (or factors) that control and maintain the global climate system. The extensive discussion of dynamic climatology covers three chapters (and more than half the book). This section opens with a detailed description of the global climate and the general circulation of the atmosphere (Chapter 3). Chapter 3 outlines the factors that determine the planetary climate and discusses how atmospheric circulation is controlled and maintained. The resulting global climate is then discussed with emphasis placed on circulation cells, the impact of surface geography on the climate, and feedback mechanisms within the climate system. This chapter also includes an excellent presentation of the basic equations used to drive General Circulation Models. The chapter closes with a detailed description of the main features of the general circulation such as the westerlies, the inter-tropical convergence zone, and the important centres of actions. Chapter 4 focuses on large-scale circulation and it

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begins with a discussion of the circumpolar vortex and jet streams. Other topics that are covered include planetary waves, zonal and blocked flows, and low-frequency variability. Chapter 5 describes all aspects of global teleconnection patterns. Not surprisingly, the majority of Chapter 5 is devoted to the El Niño Southern Oscillation (ENSO) and it provides an exhaustive description of its characteristics, global impacts, and predictability. The authors also discuss a number of other tropical and extra-tropical oscillations such as the North Pacific Oscillation, Quasi-Biennial Oscillation, and Arctic Oscillation.

The third section of the book covers synoptic climatology, the field that relates atmospheric circulation to the surface environment, and this section encompasses two chapters. Chapter 6 describes the genesis and evolution of tropical and extra-tropical cyclones. In addition, this chapter includes a discussion of how storm tracks are formed and maintained as well as a section on synoptic scale systems in the tropics. A highlight of Chapter 6 is the section on satellite-based synoptic studies. Chapter 7 provides a thorough overview of the major methodological approaches in synoptic climatology and illustrates some of the main applications of synoptic research. Many of the subjective and objective synoptic typing schemes are outlined and the principal catalogues (e.g., Lamb classification, Groswetter classification) are described. The book closes with a brief summary of the major research advances in the fields of synoptic and dynamic climatology (Chapter 8).

This book is packed with detailed information and covers a staggering amount of ground. Perhaps the book's greatest strength is the comprehensive review of the scientific literature that has been carried out by the authors. Each chapter is supported by numerous references and the book contains more than 100 pages of bibliographic material. Barry and Carleton have done an exceptional job of providing a concise summary of climate theory and placing current climatological research in historical context. All of the major concepts that they cover include a discussion of how the relevant theory and methods have evolved over time as well as references to the seminal papers. However, the depth and style of *Synoptic and Dynamic Climatology* is also a weakness since it produces a work that lacks flow and often leaves the reader without an appropriate introduction to the difficult concepts.

Synoptic and Dynamic Climatology is already somewhat dated since the most recent literature used is from 1998 and early 1999. The authors have attempted to correct this shortcoming by including a list of relevant literature published during 1999 and 2000 (after the main text was written). This book would have benefited from a list of figures, especially since the text is meant to serve as a reference and there are more than 300 figures in the book. In future editions, I hope that the authors will increase the number of colour figures (currently there are only five) because some of the figures (e.g., 2.19, 2.24, 3.44, 3.76, and 6.4) were difficult to interpret.

Synoptic and Dynamic Climatology will provide a valuable reference for those who are already familiar with the subject area. Therefore, this book is most appropriate for graduate students and professionals involved in climate research.

Ionospheres: Physics, Plasma Physics and Chemistry

by Robert W. Schunk and Andrew F. Nagy

Cambridge University Press, 2000, Hardback
0-521-63237-4, 2000, $100.00

Book Reviewed by Richard Marchand[4]

The book "Ionospheres: Physics, Plasma Physics and Chemistry" by Schunk and Nagy is an impressive compendium of knowledge and phenomenology that relates to ionospheres. It is presented in fourteen chapters and several appendices, and it is divided essentially into two parts consisting of a) an introduction and a general presentation of the near Earth space environment, and b) a detailed account of ionospheric observations and phenomenology. Following an introduction and broad description of the solar system space environment in chapters 1 and 2, the basic physical and chemical processes involved in understanding the ionosphere are presented in chapters 3 through 9. These processes include the microphysics of collisions and transport, the chemistry of the upper atmosphere, the ionization/recombination processes taking place in the ionosphere, and some elementary physics of weakly ionized plasmas. Chapter 9 introduces some of the physical models and mathematical techniques used to describe the neutral atmosphere at a mesoscale. These include the Navier Stokes equations in a rotating frame of coordinate, gravity waves, and kinetic processes in the thermosphere and in the exosphere. Chapters 11 and 12 are devoted to a systematic description of the Earth magnetosphere and, to a lesser extent, those of the other planets in the solar system. This is, in my view, the most valuable contribution of this book, as it summarizes in approximately 150 pages, a vast amount of observations and phenomenological models concerning the ionosphere. Finally, chapter 14 presents a number of measuring techniques used in ionospheric research. This chapter is not required for the understanding of the earlier chapters, and it only sketches the essentials of some of the most classical techniques. It is, nonetheless, an interesting complement to the rest of the book, and I am pleased to see that the authors chose to include it. To do justice to the measuring techniques used in the ionosphere and in space

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plasmas, however, would require another book.

Overall, I enjoyed reading this book, and I found it very informative. It is definitely a reference that anyone involved in upper atmosphere or in near Earth space physics research should be looking at. Perhaps just as useful as the chapter contents themselves, the references listed at the end of every chapter also constitute a valuable source of information. This book, however, is not for the casual reader. Some sections are, in my opinion, very dense. They summarize complex phenomena that, without prior knowledge, would require a careful study of the cited literature, in order to be properly understood and appreciated. This is particularly true of chapters 11 and 12. I suppose that some chapters would be suited for parts of graduate courses on upper atmosphere and space physics. The reader should also be cautioned that certain phenomena may not be described with sufficient depth or accuracy. This is the case, in particular, with some topics in the introductory chapters. As an example, in the section dealing with the ion-acoustic wave (chapter 6), the authors derive a dispersion equation using the plasma fluid approximation. They also derive an expression for ion thermal corrections; an effect known not to be described adequately in the fluid approximation. Indeed, a proper account of ion temperature effects on that wave necessitates a kinetic description of the ions. The reason is that ion Landau damping effects (neglected in the fluid approximation) are comparable to the correction terms affecting the real part of the mode frequency. Another omission worth noting is that of the thermal force in the momentum equations discussed in Chap. 5. This effect is negligible in low density nearly isothermal space plasmas. It can be significant, however, in higher density collisional plasmas, capable of sustaining an appreciable ion and electron temperature gradient. In paragraph 2.4.1, it is mentioned that "Mercury is unique among the inner planets in that it has a very strong intrinsic magnetic field". Yet, in table 2.4, the magnetic dipole moment of Mercury is not even mentioned. The fact is that observations[5] indicate a magnetic dipole moment of 2-6 10^{12} Tm^3; that is, at least a factor thousand less than that of Earth. What was meant, I suppose, is that Mercury is the inner planet with the second strongest dipole moment after Earth. Finally, various levels of sophistications are presented for modelling transport in plasmas, including the 5-moment, the 13-moment and the 20-moment transport models. This is done even if, in effect, only the 5-moment approach is practical; particularly for large three-dimensional multi-species simulation codes.

In summary, despite a few shortcomings, this book is a solid and valuable reference on planetary ionospheres. It is worth consulting for its breadth and for the many references that it gives at the end of every chapter.

[5] C. T. Russell and J. G. Luhmann, "*Encyclopedia of Planetary Sciences*", (1997).

OCEAN WAVES AND OSCILLATING SYSTEMS

Linear Interactions including Wave-Energy Extraction

BY JOHANNES FALNES

275 p. Hardbound Cover, Cambridge 2002
ISBN:0 521 78211 2, Price:$75.

Book Reviewed by Madhav L Khandekar[6]

Waves on an ocean surface have always fascinated scientists and non-scientists alike for centuries. Well-known mathematicians of the nineteenth century like Kelvin, Lamb, Rayleigh and Stokes, who were greatly fascinated by water waves and wave motion in fluids, did extensive mathematical analysis and laid the mathematical foundation of the mechanism of wave generation, wave growth and wave propagation on an ocean surface and over large water bodies. The extensive work of Kelvin, Lamb and others is a subject matter of several text-books on wave motion and fluid mechanics which have been published in the last fifty years or more.

The recent thrust in developing new technologies for extracting wave energy from oceans has led to the development of specialized mathematical treatment for the study of interaction between waves and oscillating systems. This is precisely the subject of the book *Ocean Waves and Oscillating Systems* by Prof. Johannes Falnes, whose work on power from ocean waves has earned him a renowned name in recent years.

In his preface, Prof. Falnes mentions the 1973 world oil crisis which prompted him and his esteemed late colleague, Kjell Budall to work on a new project aimed at utilizing ocean wave energy. Their research project led to a series of lecture notes on 'Hydrodynamic theory for wave power plants'. These lecture notes were used by Prof. Falnes for a graduate course at the University of Trondheim in Norway for many years. The present book is a revised version of these lecture notes.

The book deals with gravity waves on water and their interaction with oscillating systems. The discussion on waves is almost exclusively limited to waves of sufficiently low amplitude for linear analysis to be applicable. The low amplitude wave assumption is quite reasonable in most practical situations, for example in wave-power plant

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operations where linear analysis is applicable most of the time.

Following an introductory section, Chapter Two of the book provides a mathematical description of free and forced oscillations in the time domain as well as in the frequency domain. The complex representation (mathematically speaking) of sinusoidal oscillation is discussed and the mathematical connection between complex amplitudes and Fourier Transforms is well presented. Linear systems are discussed in a rather general way and the concept of mechanical impedance is introduced together with a discussion on power and energy relation in oscillating system. The next Chapter Three discusses the similarity between waves on water with other types of waves like acoustic waves or electromagnetic waves. Chapter Three also discusses the transport of energy associated with propagation of waves.

Chapter Four provides a basic discussion on gravity waves on water with various assumptions like inviscid and incompressible fluid flow. The Chapter also discusses real ocean waves and introduces some general definitions like significant wave height, peak period and peak frequency. Also discussed in this Chapter are wave-energy transport and drift forces caused by absorption and reflection of wave energy.

Chapter Five discusses interaction between waves and oscillating bodies. For an oscillating body in water, six modes of motion corresponding to six degrees of freedom are considered, the six modes being surge, sway, heave, roll, pitch and yaw. These six modes of oscillations are well-known in studies on ship motion and ship stability. This Chapter deals with several important aspects of waves and oscillating bodies, namely radiation from an oscillating body (waves radiating as a result of body's oscillation), wavemaker in a wave channel and wave motion due to bodies of different geometry (ex. a sphere or a vertical cylinder). The Chapter also deals with case studies on wave generation due to several bodies, partly or totally submerged in water and interacting with waves.

Chapter Six deals with wave energy extraction and provides an introduction to the concept of wave absorption as a wave-interference phenomenon. The Chapter further develops mathematical formulation for absorption by a body oscillating in one mode of motion, maximum absorbed power and absorption by a system of several oscillators. The main focus of this Chapter is on wave-energy conversion with a single body oscillating in just one degree of freedom. The last Chapter deals with wave interaction with Oscillating Water Column (OWC). The OWC refers to a water column below a water-air interface inside a hollow structure with a submerged opening so as to allow communication between OWC water and water of the open sea. Many of the wave-energy converters that have been investigated so far have an OWC with a power takeoff through a hydraulic machinery or more commonly, a pneumatic power takeoff using air turbines. In the latter case, there is a dynamic air pressure above the water surface inside the OWC chamber and the OWC is referred to as 'Oscillating surface-pressure distribution'. Several examples of OWC with pneumatic power takeoff are considered in this Chapter together with case studies of systems of OWCs and oscillating bodies.

Several problems – some partially solved – are given at the end of many Chapters. These problems provide a valuable guidance in applying various mathematical formulations developed in the book towards practical applications.

All in all, an excellent text book for someone with a good background in fluid mechanics to know all about wave energy extraction and its potential for practical applications. The book appears to be free from minor errors and typos and has been brought out in an attractive hard cover jacket. The contents of the book, in my view, are comprehensive enough that it would remain a leading source of information on mathematics of wave energy extraction for many years to come.

Madhav L Khandekar is a former Research Scientist with Environment Canada and is the author of a monograph 'Operational Analysis and Prediction of Ocean Wind Waves' published by Springer-Verlag, 1989.

Atmosphere-Ocean Interactions, Volume 1

Editor: W. Perrie

Wessex Institute of Technology, 2002 Hardback Cover
1-85312-892-9 Hardbound, 316 pages, $215.00 U.S.

Book reviewed by Paul Myers[7]

This volume examines interactions between the atmosphere and ocean, surveying key interaction mechanisms that are important for marine storms and their development. The book is divided into 3 sections, each of 3 chapters. Each chapter is a contributed paper from a researcher active in that area. The first section discusses basic considerations of marine storms in the coupled atmosphere-ocean system. This leads into the second section, where coupled atmosphere-ocean-wave model simulations are covered. Finally, longer time-scales, including the question of climate change, are considered in the third section.

Considering the chapters in more detail, the first (by E.L. Andreas) is a review of sea spray formulations and how they influence the exchanges of mass, moisture and

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volume between the atmosphere and ocean. The second chapter (by J.R. Gyakum) focuses on the processes associated with the transformation of tropical cyclones into extratropical storms. Chapter 3 (by I. Ginis) examines tropical cyclone – ocean interactions. Chapter 4 (by J.W. Bao et al.) provides a detailed review of the turbulent fluxes of heat, momentum and moisture at the sea surface and suggests parameterizations for high wind conditions. In chapter 5 (by P.A.E.M. Janssen et al.), the authors examine the momentum transfer between waves and the atmosphere and consider the role of wave-induced stress. Chapter 6 (by W. Perrie and Z. Long) describes a regional coupled atmosphere-wave model and stresses the importance of the feedbacks between the atmosphere and the ocean. Chapter 7 (by C. Tang et al.) reviews oceanic and atmospheric boundary layer models. For oceanic mixed layers, turbulent closure models and bulk formulations are considered, as well as mixed layers under sea ice. Chapter 8 (by K.J.E. Walsh) examines the role of air-sea interactions on tropical cyclones and their intensification. Connections are made to the possible implications of CO_2-induced global warming as well as the El Niño/Southern Oscillation. Finally, chapter 9 (by R.E. Tuleya and T.R. Knutson) considers the possible impacts of CO_2-induced global warming on the formation, track and intensity of tropical cyclones.

This is a solid volume for scientists interested in the areas it covers. The papers are for the most part well written and seem more than just copies of papers existing in the literature. Many of them are suitable for non-specialists and do a solid job of explaining their key points in a clear and direct manner. One problem with the volume is that there are a number of gaps of white space (of up to half a page) in a number of articles, as if the publisher had difficulty in inserting the text and figures into the volume. Although not a major problem, they do occasionally detract from the readability of the volume. Additionally, the title may mislead some people, since I was expecting papers on large scale coupled modeling, issues of surface boundary conditions, flux corrections in models, etc., from the title Atmosphere-Ocean Interactions. That said, for those researchers who are interested in (or who wish to learn something about) the details of the air-sea interface and the details of the interactions between the atmosphere and the ocean and the impact they have on marine storms, this is an excellent volume that I would recommend.

Taken by Storm: The Troubled Science, Policy and Politics of Global Warming

by Christopher Essex and Ross McKitrick

Key Porter Books, 2002. ISBN 1-55263-212-1
Price : $26.95.

Book reviewed by Paul LeBlond[8]

This book is about the clash between scientific uncertainty and policy decisiveness. In the words of the authors: "We wrote this book because we got tired of opening the newspaper or turning on the TV news and seeing a river of idiotic alarmist nonsense rushing out at the public." Essex and McKitrick proclaim outrage at the simplification that takes place between the scientific realm, awash in methodological and interpretative contingencies, and the world of policy, which must give unambiguous direction, easy to understand and enforce. This is a feeling that many scientists may have shared when witnessing how nuance scientific advice, for example about fish stocks, gets translated into black-and-white directives.

The authors place most of the blame for the narrowing of options on the often misleading simplifications introduced by interpreters of scientific results, especially environmental advocates, whom they accuse of reducing complex issues to simple-minded cartoons. Again, many researchers will sympathize with these views.

Because they are about to present views contrary to those of most climate scientists and government policy, the authors remind us that science does not stand on authority. "One hundred Nobel laureates CAN be wrong," they proclaim. They even quote Einstein as an authority on the issue! They continue with a chapter entitled "The Convection of Certainty", where they present their interpretation of the societal feedback loops, involving environmental groups, official science and politicians, which are active in creating an illusion of certainty from a basis of uncertain science. They also frankly dissociate themselves from popular concerns about the environment, dismissing as irrelevant and futile individual efforts at recycling and reducing pesticide use. That of course is clearly irrelevant in any discussion of the science, but may yet colour their judgment about the broader issues in the end.

Much of the book is devoted to a critique of climate data, models and their results - mostly Essex's writing, I suppose. The discussion is extensive and sometimes incisive: readers are introduced to the problems of sub-grid-scale parameterization, the dangers of turbulence and

[8] Galiano Island, B.C.

chaotic behaviour, the pitfalls of truncation errors, the perils of averaging, the difficulty of attributing changes to anthropogenic causes, and other issues well known to climate modelers and certainly of interest to anyone who would deepen their understanding of climate studies.

The long catalogue of problems, uncertainties, data gaps and pure ignorance could be interpreted as an impassioned plea for further research, which, in part, it is. However, the presentation is seriously tainted by the very cartooning which the authors deplore in others. The breadth of views of the scientific community is reduced to a few statements: "The Doctrine", an easily identified scapegoat, escorted by "T-Rex", a pseudonym for the globally averaged surface temperature, the popular misinterpretations of which bring forth entire chapters of derisive comments. Climate researchers will wince, as the authors claim to have at environmentalists, at the cartooning of their science. I leave to others the chore of commenting on some of Essex's more eccentric statements. In spite of their avowed intent, the authors' agenda is soon revealed: they are clearly more interested in discrediting the Doctrine than in presenting a reasoned explanation of the problems of climate modelling for the benefit of non-experts. This is not a work of education after all, but one of advocacy.

Although the authors grudgingly admit that, for example, "GCMs are to be admired" and that "because of them, the notion of climate change is plausible and not something that can be casually dismissed," the impression left after reading that part of the book which addresses climate science (the first three quarters) is that model results are useless as future climate projections and that reconstructions of past climates are equally fanciful. They claim that, recent measurements notwithstanding,"The Earth has not experienced the warmest global temperatures in a thousand years" because there is some uncertainty about past climate reconstructions. Simply because there is some possibility that the reconstruction may be in error, does not mean that they can just pick their favourite answer and claim it to be the correct one. Similarly, the conclusion they would rather draw from current model results about global warming, is not that increased CO_2 may produce global warming, but that CO_2 is irrelevant and that ALL model predictions are qualitatively in error.

In the end, a policy maker, the planet manager, say, faced with this story would presumably call in the modelers and ask them to present their response to the criticism. He would then ask the critics what their projections would be with regard to future climates. As the critics cannot come up with credible contradictory model results, he would then have no choice but to consider possible action based on those available. The question is then: what possible action, besides more research? And what may be the economic impact of various options?

This is where McKitrick - that's presumably his contribution - takes us in the last quarter of the book. Because economists have eschewed large models - "they cannot do more than generate speculative projections", writes McKitrick - they argue in terms of basic principles, based as much on philosophical preferences as on empirical wisdom, which leaves them somewhere at the level of Arrhenius discussing the basic physics of greenhouse gases in the 19th century. On the role of science in guiding policy, the authors think that: "There is some science that's good enough for, say, Science magazine, and there is science that is good enough for making policy." While there might be some truth to that, one wonders whether the statement, when applied to the value of economic projections, wouldn't simply be that none at all is good enough for policy. The economic arguments, while elegantly expressed, are convincing only to those who already agree with their premises as well as their conclusions.

What is to be done? The authors' final conclusion is that: "The right thing to do is to muddle along, focusing on basic priorities like economic development, wealth creation, education and the spread of freedom." Their recommendation that: "The best policy is to do nothing unless future information indicates otherwise," makes sense only if you accept the argument developed in the first part of the book that concludes that not enough is known already to take some action.

I found the last chapter most interesting because of the authors' proposal for settling doubtful issues through an adversary, polarized process, similar to that of the courts, rather than through consensual expert panels, such as the IPCC. That process would certainly give more voice to views contrary to the majority. Whether it would work any better, lead to wiser policies, and in the end achieve a broader consensus, remains a matter of debate which should be of interest to our leaders.

"Taken by Storm" will remind climate modelers of some of the challenges of their trade. It will not teach apprentices how to overcome them. It will not provide lay people or scientists in other fields an intelligent understanding of the principles of climate modelling, including some of the real problems mentioned by Essex and McKitrick. Nevertheless, while I am not convinced by their arguments and disagree with their conclusions, I believe that they should be listened to. The authors are a pair of earnest and perhaps somewhat idealistic academics who have made a valiant effort to bring their honest objections to a broad public. Their book is already being used as an intellectual buttress in a vast political debate. Science advisors and policy makers concerned with the outcome of this debate would do well to read "Taken by Storm" and the more technical reviews which will undoubtedly follow.

Books in search of a Reviewer
Livres en quête d'un critique

Emissions Scenarios, Intergovernmental Panel on Climate Change, Cambridge University Press, Paper Cover, 0-521-80493-0, 2000, $44.95.

Climate Change 2001, Synthesis Report, Contribution of Working Groups I, II, and III to the Third Assessment Report of the Intergovernmental Panel on Climate Change, by Robert T. Watson, Editor, April 2002, Cambridge University Press, Paperback Cover, 0-521-01507-3, $40.00US

Scattering, Absorption and Emission of Light by Small Particles, by Michael I. Mishchenko, Larry D. Travis and Andrew A. Lacis, June 2002, Cambridge University Press, Hardback Cover, 0-521-78252-x, $90.00US.

Air Pollution X, Edited by C. A. Brebbia and J. F. Marin-Duque, September 2002, Wessex Institute of Technology, Hardback Cover, 1-85312-916-X, $385.00US.

The State of The Nations's Ecosystems, Measuring the Lands, Waters and Living Resources of the United States, The H. Heinz III Center for Science, Economics and the Environment, Cambridge University Press, Paperback Cover, 0-521-52572-1, $25.00US.

Meteors in the Earth's Atmosphere: Meteoroids, and Cosmic Dust and their Interactions with the Earth's Upper Atmosphere, Edited by Edmond Murad and Iwan P. Williams, Cambridge University Press, Hardback Cover, -0521-80431-0, $80.00US.

Coastal Environment, Environmental Problems in Coastal Regions IV, Editor: C.A. Brebbia, Wessex Institute of Technology, Hardback Cover, 1-85312-921-6, $247.00US.

Ecohydrology: Darwinian Expression of Vegetation Form and Function, Peter S. Eagleson, Cambridge University Press, Hardback Cover, 0-521-77245-1, $110.00US.

Oil and Hydrocarbon Spills III: Modelling, Analysis and Control, Editor: C.A. Brebbia, Wessex Institute of Technology, Hardback Cover, 1-85312-922-4, $245.00US.

The High-Latitude Ionosphere and its Effects on Radio Propagation, by Robert Hunsucker and John Hargreaves, Cambridge University Press, Hardback Cover, 0-521-33083-1, $140.00US.

Sky and Ocean Joined: The U.S. Naval Observatory 1830-2000, by Steven J. Dick, Cambridge University Press, Hardback Cover, 0-521-81599-1, $130.00US.

Innovative Energy Strategies for CO_2 Stabilization, by Robert G. Watts, Cambridge University Press, Hardback Cover, 0-521-80725-5, $80.00US.

Exploration of the Solar System by Infrared Remote Sensing, by R.A. Hanel, B.J. Conrath, D.E. Jennings, R.E. Samuelson, Cambridge University Press, Hardback Cover, 0-521-81897-4, $120.00US.

If you are interested in reviewing one of these books for the *CMOS Bulletin SCMO*, please contact the Editor at the e-mail address provided below. Of course, when completed, the book is yours. The instructions to be followed when reviewing a book for the *CMOS Bulletin SCMO* will be provided with the book. Thank you for your collaboration.

Si vous êtes intéressés à faire la critique d'un de ces livres pour le *CMOS Bulletin SCMO*, prière de contacter le rédacteur-en-chef à l'adresse électronique mentionnée ci-bas. Bien entendu, le livre vous appartient lorsque vous avez terminé la critique. Les instructions qui doivent être suivies lors de la critique d'un livre dans le *CMOS Bulletin SCMO* vous parviendront avec le livre. Merci pour votre collaboration.

Paul-André Bolduc
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Next Fog Conference

Did you know that the next Fog Conference will be held in late 2004 in Cape Town, South Africa. The exact dates and venue are presently being determined. It will be organized by Professors Hannes Rautenbach and Jana Olivier of the University of Pretoria. For more information, please contact Professor Rautenbach at hannes.rautenbach@up.ac.za

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Short News

Bruce Whiffen in Canada and Arkadi Koldaev in Russia are actively working on forecast and safety issues related to fog on highway accidents and in aviation disasters.

NOTICE to MEMBERS and FRIENDS of CMOS

CMOS is undergoing a major change in how we run and operate our business. Our long-term dependence on the Canadian Association of Physicists for management and financial services is coming to an end. The demands we have been putting on their services have exceeded their capacity to provide such services without a major increase in our contract arrangements, in fact a doubling of costs. Your Executive and the Office of the Executive Director decided that we could afford to undertake this responsibility ourselves. Coincident with this decision, was a request from MSC to host a Canada-wide private sector home page referral system. The combination of these two actions has led us to purchase new database software which will allow for a much more accessible data base for membership, subscriptions, web page, congress registration, committees and last but not least the financial management of our income and expenses.

Already, various forms are being designed for the new software system. The turnover is scheduled for the end of May by which time most things should be in place, but if not, bear with us - it is not an easy task to separate from CAP and introduce new software all at once. By the end of the year, we hope to be able to offer most of our database services through the Internet.

Ian Rutherford, Richard Asselin and Bob Jones have been carrying the major load in this transition period. Ian is at the CAP Office handling our accounts and seeing how things are and have been done in the past for us. He, along with Richard Asselin, are also spearheading the specifications for our new software. Dorothy, Uri and I are engaged in reorganizing the office and preparing for a four-day CMOS work week with an additional person in the office. Yes, we are planning to keep the office manned four days a week with all of us sharing our time over the week.

The one request we would like to make to you as members and friends of CMOS is to note and record our new postal address:

The Canadian Meteorological and Oceanographic Society
PO Box 3211
Station D
Ottawa ON K1P 6H7

and as well the following co-ordinates:

Telephone: (613) 990-0300; Fax: (613) 993-4658
e-mail: CMOS@meds-sdmm.dfo-mpo.gc.ca
Web: www.CMOS.ca

Neil J. Campbell
Executive Director

AVIS aux MEMBRES et AMIS de la SCMO

La SCMO effectue présentement des changements importants dans sa façon de conduire ses affaires. Notre dépendance de longue date de l'Association canadienne des physiciens pour les services administratifs et de gestion financière tire à sa fin. Les demandes que nous mettions sur leur bureau ont excédé leur capacité à fournir ces services sans une augmentation importante des termes de notre contrat, en fait le double du prix. Votre Exécutif et le bureau du Directeur exécutif ont décidé que nous pouvions maintenant prendre ces services en main. Par coïncidence, nous avons reçu une demande du Service météorologique du Canada pour héberger des pages web reliées au secteur privé sur notre site. Ces deux conditions nous ont amené à acheter un nouveau système de gestion de banque de données qui nous donnera un accès beaucoup plus facile à nos données sur les adhésions et les abonnements et permettra la gestion de l'information sur les pages web, des enregistrements aux congrès et des comités, en plus de la gestion financière de nos revenues et dépenses.

Nous avons déjà élaboré les nouvelles formules qui seront imprimées à partir de la banque de données et transféré la plupart de l'information sur les membres et les abonnements. Le transfert complet des responsabilités est prévu pour la fin mai, alors que tout devrait être en place. Mais s'il y a des pépins, prenez patience – ce n'est pas une tâche facile de nous séparer de l'ACP et d'implanter un nouveau logiciel de banque de données en même temps. D'ici la fin de l'année, nous espérons pouvoir offrir la plupart de nos services basés sur la banque des données via l'internet.

Ian Rutherford, Richard Asselin et Bob Jones exécutent le gros du travail durant cette transition. Ian s'occupe de notre comptabilité et apprend la routine du travail administratif au bureau de l'ACP . Avec Richard Asselin, il contribue à la mise en place du logiciel. Dorothy, Uri et moi sommes engagés dans la réorganisation du bureau exécutif et la préparation pour une semaine de travail de quatre jours, avec l'arrivée d'une nouvelle personne. Oui, nous planifions de garder le bureau ouvert plus de jours par semaine en nous partageant les journées.

Nous aimerions que vous preniez note de notre nouvelle adresse postale et que vous en fassiez part à vos amis:

La Société canadienne de météorologie et d'océanographie
C.P. 3211
Station D
Ottawa ON K1P 6H7

De même que les détails suivants:

Téléphone: (613) 990-0300; Télécopie: (613) 993-4658
Courriel: CMOS@meds-sdmm.dfo-mpo.gc.ca
Toile: www.SCMO.ca

Neil J. Campbell
Directeur exécutif

Canadian Meteorological and Oceanographic Society

La Société canadienne de météorologie et d'océanographie

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