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Dr. Markus Dyck
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Dear Dr. Markus Dyck

Please find enclosed the comments from two referees who have evaluated your ms E3685 for Ecography.

I am sorry to report that the EiC of Ecography has decided not to accept your ms for publication. I hope the enclosed comments may be of value for you.

Thank you for considering Ecography.

Yours sincerely,

Linus Svensson

2003-06-05

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MS#3685 Dyck et al. ,Polar bears and climate change,

Dear Authors,
two independent referees have now reviewed your paper. I am sorry to inform you that neither of the reviews was favourable to your paper. After familiarizing myself with your paper and the referee statements I have decided to reject your paper from publication in Ecography. The main reason is that, in my opinion, your paper fails to make large enough contribution to our knowledge on the issue because you do not develop your ideas in terms of new analysis. The issue covered is valid and there is merit to critically examining existing literature. Even though you provide some new interpretation of climate data these remain detached from polar bear population dynamics.

I understand that my decision must be a disappointment to you but I certainly hope you will find the comments useful when revising your paper for submission elsewhere.

Sincerely

Mikko Mönkkönen
Deputy-Editor-in-Chief, Ecography

EiC

Evaluation of "Dyck et al. Polar bears and climate change?" Reference number 3685

General evaluation:
General style is not in correspondence with the guidelines given for the journal, nor for papers in general: abstract is lacking as well as a clear division into introduction, methods, results and discussion (see: ftp://oikos.ekol.lu.se/Pub/EcographyGuide.pdf).

The paper is merely a long discussion with pros et contras for the authors ideas that other factors than climate (Stirling et al. 1999) could explain reduction in body size and reproduction (density dependency) in the Hudson Bay polar bears. Even if I agree that so might be the case, I recommend the paper to be extensively revised or published in another journal as a note or "critique" in the effects of climate change debate.

Since the paper mainly is about polar bears in the Hudson Bay, that should also be included in the title.

Specific comments:
In the brief introduction given in the paper, the role of the Hudson Bay polar bears is outlined. However, they put no attention to importance for clothing, handcraft and role of eco-tourism. Also to my opinion, it is lacking some words about what role the polar bears might play in the system.

On page 6, the climate change is described as natural climatic oscillations. At page 9 climate changes has become man-made, global warming by anthropogenic greenhouse gases. I would omit the framing of the observed climate changes and so avoiding that the paper is brought into a unwanted discussion/context.

On page 7 the authors suggest the possible evolutionary scenarios of the polar bear in response to climate change over a couple of decades. To my opinion this paragraph is speculative and really brings the paper out on "thin ice". I would personally have omitted this paragraph.

In the figure Captions: Figure 2 it is stated that that AO for winter is average for January, February and March and spring temperature is March, April and May. In Figure 1 winter is average of December, January and February and Spring is average of March, April and May. If so, and not a writing mistake I have problems understanding why different months are chosen to describe Winter and Spring temperatures.

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Markus Dyck et al. **Ref no. 3685**
Polar bears and climate change?

General comments:
Overall, I find the manuscript somewhat misleading and poorly structured: largely in the biological component. However, I may be biased in my review as some of my own studies are misquoted. Overall, a major restructuring is required and further relevant analyses are required. I find the connection between the various paragraphs to be disjointed and confused: moving from body mass estimation to fasting to issues of formulating testable hypotheses to archaeological issues confusing and the flow and logic is not clear to me. The key issue seems to be that the authors do not believe the results and conclusions of Stirling et al. 1999, Derocher and Stirling 1995, and Stirling and Derocher 1993. As a co-author on some of these papers, I am intimately familiar with the results and the methods. Some of the criticisms made in this manuscript are interesting but several are incorrect, misinterpreted, out-dated, or previously discussed but ignored. Additional structure of the paper would be useful to assist the reader understand what the issue is. In summary, it is useful to criticise the work of others and propose new hypotheses as this is the basis of scientific advancement. However, to make these advancements, the methods and logic must be well developed and while I see merit in some of the issues discussed, I do not see that this manuscript provides an advancement of the issue. The paper falls short of its goal to examine peer-reviewed literature in a sufficiently rigorous manner and the objective to "introduce our analyses of climate data" while interesting, is not directly related to goal of climate change effects on polar bears as currently structured. The main potential of the manuscript, and the exciting point, is the exploration of the issues of AO as they pertain to polar bears. Unfortunately, this goal is not achieved in any meaningful way due to the lack of rigour and consideration of the ecological processes in arctic marine ecosystems.

Specific comments:
Pg. 2: The reference of World Wildlife Fund for Nature 2002 is not a primary source so additional information should be given so the document can be located.

Pg. 2, 3rd paragraph: The reference to Derocher and Stirling 1992 and 1995 incorrectly suggests that the change in the population dynamics and mass of polar bears was related to freeze and thaw cycles. In the abstract of Derocher and Stirling 1992, the last sentence states "....the changes in reproduction, weight, and cub mortality are consistent with the predictions of a density-dependent response to increasing population size." How this can be construed to freeze and thaw cycles is unclear. From Derocher and Stirling 1995, the last sentence of the abstract states "Insufficient information was available to determine the cause of declines in reproduction and body mass." This source addresses possible effects of handling (rejected), climatic anomalies "...such as the lower temperatures in eastern Hudson Bay during the 1980s...", harvest effects, density dependent issues or possible ecosystem shifts. Nowhere is this source definitive about the freeze and thaw cycles so some better reference checking by the authors would be warranted.

Reread Stirling and Derocher 1993 and Stirling et al. 1999 for a discussion about climate change issues.

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Pg. 2, 4 lines up: The authors should consider a more thorough review of the literature and not rely solely on Stirling et al. 1999 for climatic information in the Arctic and in western Hudson Bay. There are numerous papers ranging from permafrost

Gough, W.A. and Leung, A. 2002. Nature and fate of Hudson Bay permafrost. Regional Environmental Change **2**: 177-184.

to sea ice see Model tuning and its impact on modelled climate change response: Hudson Bay sea ice, a case study Gough WA CANADIAN GEOGRAPHER 45 (2): 300-305 SUM 2001

More specifically, see Climate change scenarios for Hudson Bay, Canada, from general circulation models Gough WA, Wolfe E ARCTIC 54 (2): 142-148 JUN 2001 which has the following abstract *"Two generations of a climate model are compared using the impact of a CO2 doubling on the Hudson Bay region as the means of diagnosing differences in model performance. Surface temperature, precipitation, sea-ice coverage, and permafrost distribution are compared. The most striking difference is the response of the sea ice in the two models. In the coupled atmosphere-ocean climate model, sea ice virtually disappears in Hudson Bay. This leads to a substantially higher regional temperature response. We suggest that conductivity of sea ice and thermal diffusivity of seawater are key factors that cause the difference in sea-ice response. It is recommended that a regional model be developed to produce more representative climate change scenarios for the Hudson Bay region."*

There are several projection models available from numerous sources showing the past and future trends for this area. See

Comiso, J.C. 2002. Correlation and trend studies of the sea-ice cover and surface temperatures in the Arctic. Annals of Glaciology **34**: 420-428.

Comiso, J.C. 2002. A rapidly declining perennial sea ice cover in the Arctic. Geophysical Research Letters **29**: 1956 doi 10.1029/2002GL015650.

Parkinson, C.L. and Cavalieri, D.J. 2002. A 21 year record of Arctic sea-ice extents and their regional, seasonal and monthly variability and trends. Annals of Glaciology **34**: 441-446.

A much more thorough coverage of the relevant literature is required. The whole premise of the paper hinges on the issue of refuting climate change or lack of demonstration of this issue.

Pg. 3, first line: It is unclear why the temperature records for Churchill, Manitoba are considered representative of western Hudson Bay. It may be so but some support for this method is required. The polar bears in Hudson Bay rely on an ecosystem that is some 800 x 1000 km in size so using a single data point from the western edge of the ecosystem may be unreasonable. If it is reasonable, then provide suitable documentation of studies that have used a single point for a 800,000 km^2 ecosystem. Granted the bears in this population are western based, this could reduce the area by 50%.

Pg. 3, 2nd line: That detecting climate trends given existing data and large variation should not come as a surprise. This is a universal issue and in some respects seems trivial as an issue.

No trend from 1932 to 2002 while interesting seems to be outside of the scope of the paper which appears largely to discount the conclusions of Stirling et al. 1999. The finding of an apparently significant trend (what do the authors mean by "A clear tendency…"?). The statistical methods and result are not present so if this paper is intended to show no climate change in Hudson Bay then some numbers would be useful.

The ecosystem upon which polar bears depend is governed by much more than April, May and June temperatures so it would seem a more thorough analysis is warranted.

Pg. 3, ca. 11 lines down: The idea that your results confirm the suggestion made by Stirling et al. in 1999 seems to be valid. What happened in the period after this publication is beyond the scope of Stirling et al. as the time frames are different.

Pg. 4, 1st para, last sentence: "…the hypothesis…" The logic here escapes me. The hypothesis of late spring warming was proposed as a possible mechanism of impact for polar bears in western Hudson Bay but why this impact would not apply to other areas is a curious statement and the logic escapes me. It has long been projected that climate change will result in cooling and warming in different areas and perhaps even within a single polar bear population so the issue of "universally extended" to other locations is a trivial finding.

Pg. 4: The analysis of AO is the most significant contribution of the paper in the first 4 pages. That AO and temperature are correlated seems to be off the topic of the paper title unless some clarity or connection is given. No statistics make this a just so story until some results are given. What is mean by the authors for "climatic conditions….. have a close association with the AO circulation index."?

Pg. 5, 1st para, last sentence and in footnote: Stirling et al. are not
Your definition of statistically significant is set at 0.05 if you consider Stirling's 0.07 to be not significant. However, this is user defined. A p-value of 0.07 can be considered significant for a trend of a pattern as variable as sea ice break-up. To hold to alpha levels of 0.05 is simplistic and contradicts your earlier statement about how hard it is to detect a trend. This is particularly difficult to deal with since in your manuscript, no significance levels of any sort are presented.

Pg. 5, 2nd para. The issue of Svalbard reindeer is only remotely related to polar bears given the differences between terrestrial and marine systems. Be careful with the notion of an "introduced Svalbard reindeer… population" as this is a native species in Svalbard but there was an experimental relocation of the animals. My concern is that people may sense that introduced would be similar to red deer in New Zealand. There are several other studies showing the relationship of wildlife to AO and NAO and this would likely be a useful approach to deal with the general issue rather than the specific (i.e., reindeer).

Pg. 6, 2nd para: Flow is missing here. I don't see the logic. The logic of dealing with Frobisher Bay (now officially renamed Iqaluit and please provide lats and longs for locations mentioned e.g., Churchill) is unclear if the intent is to deal with Hudson Bay. What happened when the AO was linked to Churchill temperatures? My guess is that is didn't work or this would have helped your argument. Using the relationship of AO and

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"Frobisher Bay" some 1200 km to the NE seems unlikely to be linked to the western Hudson Bay population of polar bears which appears to be the focus of the study.

Pg. 6, 2nd para, last line: "zero to 5 decimal places" Why not use the standard reporting for statistical results? This excess use of text to say $p < 0.0001$. This is the first statistical reporting in the paper and the relevance is somewhat unclear. Yes, AO and temperature are related but if this is the major discovery then the selected journal is inappropriate and the finding is not new.

Pg. 6, last para: "are really detrimental to biodiversity as suggested." Who suggested this and what is the context of the remark? See Tynan, C.T. and DeMaster, D.P. 1997. Observations and predictions of Arctic climate change: potential effects on marine mammals. Arctic **50**: 308-322. for additional views on marine systems.

Pg. 7, 6 lines down: "true hibernation state" Who says that this is true hibernation? Some references would be useful. My sense is that the consensus is that they are not true hibernators with a substantially lowered core temperature.

The generalities about primary production and such are valid considerations but without consideration of sea ice issues, the relevance of the discussion is with little merit. See Tynan and DeMaster above.

Pg. 7, last para: This discussion is not very insightful and to state on absolutely no new evidence that "It is more likely that what is being observed for WH is caused by density-dependent factors…" is unsupported speculation. The issue of density dependence in this population has been discussed in a variety of publications and to ignore these is a failure to deal adequately with existing knowledge. On what basis do the authors assert that density dependence is the cause? Check the error bars on the available population estimates and assess if there is documentation of a population increase. I think you will find limited data to support your assertion and hence earlier authors have typically been shy to boldly confirm what you have. There is abundant speculation on this issue in the literature but the current discussion does not significantly add to the issue.

Pg. 8, 2nd para: The issue of body mass estimates is not central to the theme of the paper. Recent works (see Derocher, A.E. and Wiig, Ø. 2002. Postnatal growth in body length and mass of polar bears (*Ursus maritimus*) at Svalbard. Journal of Zoology **256**: 343-349. for a different interpretation of this result. The equation of Kolenosky et al. 1989 is from a population that resides in southern Hudson Bay, adjoins the western Hudson Bay population, feeds in the same ecosystem and likely interbreeds with the adjacent population. The issue of population specific equations has been very well covered in the recent literature but in reality, this does not apply to western Hudson Bay studies. Perhaps contact M.R.L. Cattet (University of Saskatchewan in the acknowledgements of the manuscript) for the results of his recent analyses that suggest IF you detect a trend with estimated masses then the trend must be very strong because estimated mass does introduce error and increase variance but in a relatively non-biased manner. The senior author of the manuscript was present at a recent presentation by Cattet where these results were presented. Overall, mass estimation results in reduced statistical power so the argument

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fails: estimated mass would be more likely to show NO trend. Further, the generality of this issue is highly species specific so a generalisation is not possible and should be avoided. Further, if there is no trend in the body mass data then there is little reason for your manuscript. Failure to treat the reproductive data with a similar discussion unbalances the issue. There are changes in both the reproductive characteristics and body mass. Of course, as the authors note, there is some correlation between these two elements but even those studies would be unlikely to be true as they also hinge on estimated mass from morphometrics. The r-square value for the estimated mass of polar bears and the actual weighed mass of the same bears from Svalbard was 0.96 with an error of 8% normally distributed about the mean (see Derocher and Wiig 2002, Figure 1). That weighing a bear has zero error is unlikely as well.

Pg. 8, last para: "we wish to encourage archaeological search for information…" What do the authors imply from this? The studies mentioned are interesting but I don't see the thread. If recent studies are fraught with bias how is it that archaeological studies won't present similar issues? Allusions to developing "testable hypotheses" is a valid conclusion but failure to provide an example is not helpful. Is the testable hypothesis related to AO or the position of the Arctic Front?

Pg. 9, 3 line down: "largely stochastic in nature" Where is the analysis or reference for this statement?

Pg. 9, 5 lines down: see the seminal work by Vibe, C. 1967. Arctic animals in relation to climatic fluctuations. Meddelelser om Grønland **170**: 1-227. that has serious discussions about the issue of decadal timescales. This is not a novel finding.

Pg. 9, 1st para, last sentences: This is interesting and an area that could be developed but as stated, it basically says that nobody knows how humans are affecting climate but you know that late spring temperature is not necessarily useful. The logic here seems flawed. The issues are indeed complex and I agree that the links are not definitive but the lack of comprehensive coverage of the relevant literature about climate change and the arctic leads to incomplete and simplistic conclusions.

Pg. 9, last para.: this part I can agree upon. There are multiple hypotheses available and the paper should have addressed these in full if it were to make a significant contribution. However, concluding that Svalbard, the only non-harvested polar bear population being studied ignores many of the recent papers showing that this population is the most polluted in the world and that population level effects are being suggested. See:

Derocher, A.E., Wolkers, H., Colborn, T., Schlabach, M., Larsen, T.S., and Wiig, Ø. 2003. Contaminants in Svalbard polar bear samples archived since 1967 and possible population level effects. The Science of the Total Environment **301**: 163-174
Skaare, J.U., Bernhoft, A., Derocher, A., Gabrielsen, G.W., Goksøyr, A., Henriksen, E., Larsen, H.J., Lie, E., and Wiig, Ø. 1999. Organochlorines in top predators at Svalbard - occurrence, levels and effects. Toxicology Letters **112**: 103-109.

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Bernhoft, A., Skaare, J.U., Wiig, Ø., Derocher, A.E., and Larsen, H.J.S. 2000. Possible immunotoxic effects of organochlorines in polar bears (*Ursus maritimus*) at Svalbard. Journal of Toxicology and Environmental Health, Part A **59**: 561-574.
Andersen, M., Lie, E., Derocher, A.E., Belikov, S.E., Boltunov, A.N., Garner, G.W., Skaare, J.U., and Wiig, Ø. 2001. Geographic variation in selected PCB congeners in polar bears (*Ursus maritimus*) from Svalbard east to the Chukchi Sea. Polar Biology **24**: 231-238.
Skaare, J.U., Bernhoft, A., Wiig, Ø., Norum, K.R., Haug, E., Eide, D.M., and Derocher, A.E. 2001. Relationships between plasma levels of organochlorines, retinol and thyroid hormones from polar bears (*Ursus maritimus*) at Svalbard. Journal of Environmental Health and Toxicology **62**: 227-241.
As just a few examples of why Svalbard may not be a good model.

The figures are interesting but not really necessary. A couple of examples would suffice, perhaps Figure 1b, top panel along with Figure 2 one panel. A full statement of statistical results would be useful.

Review by Andrew E. Derocher, Department of Biological Sciences, University of Alberta

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