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Discussions

Interactive comment on "How reversible is sea ice loss?" *by* J. K. Ridley et al.

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General comments:

Ridley et al. assess the reversibility of sea ice loss by ramping up CO2 in the HadCM3 GCM and then ramping it back down again. They find that when Arctic sea ice loss and recovery are plotted against global-mean temperature, there is not evidence of hysteresis. In the Antarctic, however, they find that the ice recovery lags behind the global temperature recovery. Overall, they conclude that there is "no clear evidence" for an irreversibility threshold in the sea ice changes simulated with this GCM.

This is a very timely study, and the results are interesting. The paper is well written, concise, and, except in a few instances described below, very clear. I have a number of concerns which are discussed below. I would recommend the manuscript for publication after these comments are addressed.

Major comments:

(1) There are noteworthy omissions in the literature review. A recent paper by Armour et al. (GRL L16705 2011) uses a very similar methodology to find a very similar result using a different GCM. A detailed discussion of what the current paper adds to the picture presented in Armour et al. would be useful. For example, the magnitude of the time lag in the recovery of sea ice behind climate forcing (p. 2351 line 21) is addressed in Fig S1 of Armour et al. for the GCM they use. Also, I wonder whether the authors want to discuss Winton (doi:10.1175/2011JCLI4146.1 2011), if they find that it added relevant results to the findings of Ridley et al. (2008), when they discuss the range of simulated sea ice sensitivities on p. 2350 line 20. Lastly, I want to mention a very recent paper of my own at http://gps.caltech.edu/∼ian/reprints/Eisenman-2011-accepted.pdf, which addresses physical mechanisms for the range of sea ice irreversibility scenarios, in case it is useful at all when the authors are revising the mechanistic discussions in Sec. 3.

(2) The terms "hysteresis" and "reversible", which are used prominently in the paper including in the title and abstract, are fairly ambiguous here and ought to be defined to clarify their usage. Regarding "reversible", I think most people interpret the question posed in the title ("How reversible is sea ice loss?") to relate to whether the sea ice would return if greenhouse gas levels were reduced. This also appears to be a meaning that the authors use, but they appear to use different meanings as well, such as whether the sea ice returns when the local temperature recovers (p. 2353 line 24). It is easy to imagine a scenario where sea ice and local temperature vary in unison but do not recover when greenhouse gas levels are reduced. Regarding "hysteresis", in a simple linear system in which the response has a characteristic time scale over which it exponentially adjusts to the current rate of forcing, when response is plotted versus forcing, something that looks like a hysteresis loop can occur which disappears when the forcing is varied more slowly. This linear effect, which is not typically referred to as "hysteresis", needs to be clearly distinguished from nonlinear rate-independent

hysteresis involving multiple states. For example, the term "significant hysteresis" in the Abstract (p. 2350 line 10) to describe the Antarctic experiencing "a lag in the recovery of lost sea ice" (p. 2356 line 2) appears to use this atypical definition. Ideally, the authors should choose specific definitions of "hysteresis" and "reversible", make them clear to the reader early on, and stick with them throughout.

(3) What is the significance of the ice area being smaller during ramp-down than during ramp-up in the Antarctic (Fig 3a)? The authors suggest that this is not irreversibility but is instead related to differences in the time lag of the temperature response in each hemisphere. But can bistability and hence irreversibility really be ruled out? It would be interesting to see what happens when the forcing is fixed somewhere near the midpoint (~2.5xCO2) in two separate simulations, one starting from the upper branch of the apparent hysteresis loop in Fig 3a and the other from the lower branch, to see whether they converge on a single state (as was done, for example, in the Armour et al 2011 paper mentioned above).

(4) It was not clear to me what is the significance of the sea ice area vs local temperature plots (Figs 2b, 3b). If you just manually removed the ice from a grid box, the surface temperature would change. Much of the area included in the "local" average experiences a change in sea ice cover in these simulations, and given horizontal mixing in the atmosphere between locations where ice concentration changes and the rest of the "local" region, it seems likely that the surface temperature in the "local" region would be closely married to the sea ice area. If this is true, the lack of hysteresis in Figs 2b and 3b would be expected and not particularly informative. This is in contrast to the discussion in the paper, which makes substantive physical claims based on the lack of hysteresis in Fig 2b and 3b. For example, on p. 2353 lines 10-14 the authors claim that the results in Fig 2b imply that the lag in Fig 2a (ice area vs global temperature) is due to ocean cooling effects rather than some "innate feature of the sea ice", and that ocean vertical structure is not driving the changes.

(5) In Fig 2a, the ramp-down trajectory appears to be above the ramp-up, implying that

Arctic sea ice recovers faster than global temperature. Given this point, is ice area vs global temperature a good measure of reversibility? For example, on page 2356 line 3-5, the lag of Antarctic ice area behind global temperature is described as "a lag in the recovery of lost sea ice", but this same line of reasoning seems like it would imply that the Arctic sea ice preemptively recovered (because it recovered ahead of global temperature, albeit behind the greenhouse forcing).

Minor comments:

(1) It was not clear to me why the two experimental setups (fast and slow) were chosen. Was the difference between the two seen as physically enlightening, or were these seen as two extreme cases bracketing the range of policy-relevant scenarios? Further explanation of the justification for this choice would be useful.

(2) Some (brief) mention of the ice rheology in HadCM3 would be useful when ice advection is discussed in the Methods section (p. 2352 line 5).

(3) On p. 2354 line 11, it says the surface air temperature changes 1.6 times faster in the NH than the SH. Is this based on a figure, or is it from part of the analysis that is not shown? More importantly, exactly what does this mean? The CO2 is ramped up and then down, and the temperature changes. Is a characteristic exponential response time scale derived from the GCM results, or is something else meant here?

(4) In regard to the differing rates of response when climate is warmed and then cooled (p. 2354 lines 10-19), the authors may wish to consider the discussion in Stouffer (J Cli, 2004, "Time scales of climate response"), where the difference between the response time for warming and cooling the climate is investigated in a GCM.

(5) I found lines 1-7 on page 2355 to be confusing, especially the final sentence. It is interesting that the deep Southern Ocean continues to warm in the slow scenario even after the forcing is brought back to 1xCO2 for more than a century. Is this behavior specific to the Southern Hemisphere (compared with the Northern Hemisphere), and if

so, can it be readily explained (e.g., more vertical mixing)?

(6) On page 2355 lines 24-26, surface freshening and mixed layer depth in the Arctic is included in the concluding summary. Was this discussed somewhere above?

(7) I think it would be more clear in Fig 1 if the labels a,b,c,d were marked on the CO2 trajectories rather than as vertical dashed lines. For example, point "b" appears in the plot to apply both to the line at CO2=4 and to the line at CO2=1, but it actually applies only to the latter. In Fig 5, where only one scenario is included in each panel, I think the labels for these points are clear.

(8) In Fig 5, the numbers between the plot and the colorbar in each panel do not appear to be labeled in the figure or identified in the caption. I assume the numbers refer to the simulation year, but some clarification would be useful. Also, if my assumption is correct then there is an error in panel b (or in Fig 1): although the labels "a" and "b" in Fig 5a fall at the same simulation times as in Fig 1, the labels "c" and "d" in Fig 5b fall at considerably later years than in Fig 1.

Interactive comment on The Cryosphere Discuss., 5, 2349, 2011.