Major Comments

The authors have put considerable effort into answering both referees' queries and the manuscript is much improved over the previous version. However the explanation of the processes generating the polynya is still not quite convincing and needs further work. Certainly plots of salinity need to be presented in Fig. 2, as well as those of temperature and density.

A naive view of Antarctic open-ocean polynya formation is that it is fundamentally a consequence of extra salinity in the surface waters that permits the water to get dense enough to convect while it is still above freezing. Once the convection 'punches through' into the warm salty upper circumpolar deep waters, then it continues bringing up salt and the convection penetrates very deeply and can be sustained for several winters. The warm water in the polynya is the *consequence* of heat being brought up from below. Therefore consideration of the evolution of the salinity structure is essential to understand the process of polynya generation. This is still missing from the MS.

It seems to me from the evolution of density in Fig. 2h that a key driver of the model polynya is the very salty (because it's warm but anomalously dense) water lying at depths of 50–100m in 1985–1986 that becomes entrained into the ML in autumn 1986. But this would be clear if you plotted the salinity evolution.

(i) The inclusion of the water-column trajectory in Fig. 2e is very useful. However, this trajectory starts only in August 1986 (unless it didn't move from March 1985–August-1986, in which case it needs to be made clearer). So where does the data comes from that is used in the Hovmöller diagrams 2f–1i before August 1986?

(ii) Some of the text on p9 (incidentally, where are the line numbers?) seems inconsistent with the revised argument. In para 2, l3 the text mentions 'ice fraction increased by more than 0.2', in June 1985, but at 63 deg S, near the 1986 polynya, which is presumably where the trajectory is at this stage (comment(i)), the ice fraction anomaly is actually *negative* according to Fig. 2a. Similarly Fig. 2b suggests that the MLD anomaly is only ≈10m (or even not significant) there.

(iii) p9, para 2, 3rd line from last. You state that 'the model polynya allows the formation of dense water at the surface due to brine rejection'. You need to justify this in terms of something like ice divergence, because the naive expectation is that in a polynya, where there's no ice, there can be no brine rejection.

(iv) Surface waters will not follow the column trajectory because they are influenced by the Ekman drift. You might want to consider what impact this has on the salt budget of the column.

Minor Comments

p9, Section 3.1 title. Better would be 'Mechanism of polynya formation in the control run'
p11, para 3. 'The warmer the ocean is in summer and autumn 1986, the less sea ice can form, so the larger the polynya in winter 1986'. I just don't think it's that simple—as I stated in my previous review—warmer summer ML *by itself* will just slightly delay the formation of ice the next winter. Enhanced salinity is required.