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Discussions

Interactive comment on "Cloud responses to climate variability over the extratropical oceans as observed by MISR and MODIS" *by* Andrew Geiss and Roger Marchand

Anonymous Referee #2

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The authors present an analysis of cloud fraction trends as observed by MISR and MODIS satellite instruments over the period from 2000 to 2013. These trends are related to trends in meteorological quantities over the same period as represented in the ERA Interim reanalysis dataset. The analysis is primarily based on a Maximum Covariance Analysis, a method related to principal component or empirical orthogonal function analysis, but based on the covariance matrix between cloud fraction data and the meteorological quantities from ERA Interim.

The analysis is carried out in four ocean basins more or less independently - the North and South Atlantic, and the North and South Pacific. The trend analysis identifies an

increase in low cloud of moderate optical depth in three of the four basins (all but the South Atlantic), and a reduction in clouds of high optical depth in all basins, especially at high altitudes, although the authors raise the possibility that some of the latter trends could be a result of an unaccounted-for drift in the MISR calibration.

The connection of these trends to the meteorological quantities is, however, harder to interpret. Clear decadal changes in the North Pacific are identified over this period, but changes in the other ocean basins are less coherent. And while the covariance analysis identifies relationships between trends in cloud fraction and those in the meteorological fields that seem to be physically coherent, it's not clear to me what light this analysis shed on the trends.

So my main concern is methodological – not to say that it is wrong in any way; indeed it looks to have been quite compentently done, and it is well explained in the text. But the covariance analaysis seems to be creating more problems than it solves, in several senses. Firstly, the multivariate nature of the SVD means that the method is looking for vertical structures in all of the meteorological fields that are consistent across the whole basin. This isn't unreasonable, but it's not obvious to me that these structures should be spatially consistent: for instance the horizontal transport and shears will vary strongly across the basins. This means that you need a bunch of 'modes' in order to decompose a specific signal, which may go some way towards explaining modes like NP1 and NP2, or SA1 and SA2 that look fairly similar to each other in terms of their expression in the cloud fraction space and seem to be 'correcting' each other to some extent. Secondly, it's not obvious how to connect the identified 'modes' to the trends themselves – or how to connect them to the climate modes. Both of these issues might be avoided by using a simpler analysis.

This method may be better suited to looking at month-to-month variability, which will contain much more information than a single spatially varying map of decadal trends.

I think these challenges need to be better addressed for the work to be publishable.

Perhaps the method has advantages that I am not seeing - if so the authors should make this case. However, given that the conclusions drawn from this methodology are rather vague, I'm not sure this is the case.

To be more constructive, one reasonable question to address might be 'Can the cloud fraction trends be 'explained' by the trends in meteorological trends?' Or do we need to look to compositional changes? A more restricted version of this is to what extend can these trends be explained by decadal variability in the identified climate modes. If either of these could be answered clearly this would go some way towards answering the more fundamental question of why these trends are occurring, which the paper much more impactful.

I have a few additional specific comments but they are of relatively minor importance:

Trend significance – has a field significance test been considered? There are many trends being tested for significance here and individual significance tests for each trend may not be fully rigorous. (The trends look robust so I am reasonably convinced that they are not statistical artefacts, but one can be misled in these things.)

Has the seasonal dependence of the trends been considered?

Figures 4 and 5 are not very clearly discussed in the text – they are referred to some extent in the discussion of Figure 3, but there was clearly a lot of effort put into including a lot of information in these figures and the reader is left to extract this information on their own.

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