There is clearly a disagreement between the authors and Mike Fromm on the validity of the inferences in this paper. To resolve these I require a number of points to be clarified.

The first point that I'd like clarified is the assertion very early in the paper (p.2, l.13) that the AOT of the Raikoke volcanic aerosol cloud should have been of order 0.005-0.01 at 500 nm in the winter half year over the Arctic. For the given references, Kloss et al showed 0.01 – 0.02 at high latitude for 675 nm, my observations at 355 nm and 52°N showed AOD above 12 km of 0.01-0.02, Cameron et al do not show AOT at 500 nm and Gorkaiyvi et al show the same measurements as Kloss. A minor point maybe but where do the authors get the 0.005-0.1 range? Is this based on previous eruptions?

More important in this context is the definition of AOT. A meaningful comparison between different AOT values requires them to be over the same height range. According to figs 11 and 12, you calculate AOT from 6 to 15 km, with the maximum at 9 km. There is nothing scientifically wrong with this of course, but much if not most of this aerosol is in the troposphere and comparison with stratospheric AOT measurements is not meaningful. Kloss for example presents sAOD, the integrated AOT above the tropopause.

Indeed it would seem to me that much of the dispute between the authors and Mike Fromm comes from the altitude at which the measurements are made. The clear evidence of smoke particles comes from the measured lidar ratios, but in figs 11 and 12 these are only shown between 7 and 12 km. Is there evidence that the layer above 12 km was actually smoke? The smoothness of the profiles could arise from the considerable smoothing used in the data processing, and hide the possibility that the aerosol layer was not homogenous. Could the Raikoke aerosol be above the smoke layer? (That would help with the conceptual difficulty many readers will have of the smoke self-lofting to 17 km which is above the region where it was geographically confined). The authors comment on fig. 4 that the blue colours between 13 and 17 km from 45 to 60 N 'may indicate Raikoke – related volcanic aerosol'. Like Mike Fromm, I would contend that it's 'very likely', not 'may'.

Your estimate on p.13 of the Raikoke volcanic aerosol contribution does not consider the possibility that there were two or more aerosol layers (e.g. smoke below the tropopause, smoke + some Raikoke aerosol 1-2 km above the tropopause and volcanic aerosol above). The 10-15% contribution to the extinction coefficient at 532 nm from volcanic aerosol applies (as I understand it) to the entire layer from 6 km upwards, so it is not at all surprising that it is dominated by the smoke.

I don't understand how the self-lofting argument gets smoke into the stratosphere, if the response in the troposphere is to produce plumes. But there is continual exchange of air between the troposphere and the bottom couple of km of the stratosphere so over a couple of months it is entirely possible that some smoke originally in the upper troposphere would be found in the lowest layers of the stratosphere. However, such mixing would not lift the smoke to 17 km, which is well above the extratropical tropopause.

Fig 4 – please give date and time of Calipso overpass in the caption