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## ***Interactive comment on “Arctic ozone loss in Siberia in 2011 and 2012” by V. Dorokhov et al.***

### **Anonymous Referee #2**

Received and published: 17 May 2013

The last sentence of the abstract for this discussion paper states, “The aim of the paper is to describe which and where these measurements were carried out and illustrate their performances by some examples of ozone data measured in Western and Eastern Siberia, Russia such as that which occurred in the winter–spring season of 2011.” These refers to ozone measurements by Brewer, SAOZ, and ozonesonde. The authors are true to their word. The discussion paper consists of paragraphs of repetitive descriptions of various Brewers, with only the location and the model number changed, then the same for the SAOZ instruments and ozonesondes.

Each section begins with a review of the operating characteristics of the instrument, all of which are well known within the community, and have been published, then a list of the various locations of the measurements, then some example measurements, with a little bit of emphasis on the 2011 boreal spring ozone loss, then noting the data

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bases for location of the data, NDACC, or in some case where comparison data were obtained, WOUDC. But the discussion paper has no purpose beyond this, and this reader is left wondering why should this be published?

The NDACC and WOUDC data bases are well known, as are the instruments used. There is no new technology presented, or new measurement comparisons, or new ways to investigate the 2011 spring ozone loss. Several papers on the 2011 spring ozone loss have been published, with, in some cases, author lists including some on this discussion paper. Thus at least a fraction of the data shown here has probably already been published, or referenced, related to the earlier published work describing the unusual 2011 Arctic winter, spring (see the following references in the discussion paper: Balis et al., 2011; Bazhenov and Burlakov, 2011; Manney et al., 2011). Thus I do not see a reason why this discussion paper should be published, and therefore conclude, unfortunately, that it is a wasted exercise.

I do not recommend acceptance of this manuscript. I do not see its relevance to the readership of Atmospheric Measurement Technology. I do not see a way to use this discussion paper as the basis for an improved contribution that could be published. Unfortunately while the authors are true to their stated goal, that goal is not sufficient to warrant a publication in the refereed scientific literature.

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