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Interactive comment on "Monitoring Sudden Stratospheric Warmings using radio occultation: a new approach demonstrated based on the 2009 event" *by* Ying Li et al.

Anonymous Referee #1

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This is a well-done manuscript outlining the details of and application of a novel method for detecting and evaluating sudden stratospheric warmings (SSWs). The authors aptly couch their work in the context of the ongoing discussion within the SSW community about SSW definitions. They demonstrate that their method and definitions, at least for the 2009 SSW, agree with established metrics and provide additional objective information. While the context of their work is centered around the use of radio occultation data, the authors show that using a selected model's data results in complimentary analysis, showing that this work may readily be applied to long-term reanalyses.

I find this work to be properly placed in the literature and a novel contribution to the

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Discussion paper

community. I do have a few comments I would like the authors to address prior to publication.

Minor comments

1: My main concern about the manuscript is on how clear the authors are in letting the reader know that the particular threshold choices are determined based on this one anomalous event. I appreciate that they do make this clear in the conclusions section, but that clarity was missing in Section 2.3 where the threshold values are introduced. In particular, I think the paragraph beginning on page 7, line 27 could use an additional statement(s) on this topic.

Along these lines, I think some additional clarity in the statement on pg. 12, line 1 is warranted. Certainly, this SSW is known for being strong, but as-written, the authors seem to suggest that their method is sufficient to determine that this event is strong. Given that the work in this manuscript is based off a single SSW, it's not obvious how that can be determined independently of other SSWs.

I think the authors should critically consider other areas of the text that would benefit from further discussion about this topic.

2: The authors bring up the Butler et al. (2015) requirements for a standard definition of SSWs. Missing from the manuscript is the authors' discussion on how their definition fits these three proposed criteria. These are criteria the SSW community has agreed upon, so providing additional contextualization of their method in light of these should be done.

Specific comments

1: Do the authors report somewhere that bending angle and density are given in normalized units? This is apparent, but the reader would benefit from a definitive statement in the manuscript. As well, please state how the normalization is performed (normalized with respect to what?).

2: Abstract, line 20: recommend “has strong potential.”

3: Abstract, lines 17 and 22: is it necessary to introduce these metrics – the 3 Mio. km^2 threshold and the MSTA-TEA40 metric – here? I'm not sure that the abstract benefits from either the specificity of the former or the raising of the as-yet undefined metric and abbreviation of the latter. I would recommend removal unless the authors have strong objections.

4: Pg. 8, lines 25-26: I'm not quite sure I follow what's being said here. Is it that the specific definitions the authors have proposed may change as more systematic study is performed?

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