

***Interactive comment on* “Sources and sinks driving sulphuric acid concentrations in contrasting environments: implications on proxy calculations” by Lubna Dada et al.**

Anonymous Referee #2

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The manuscript, “Sources and sinks driving sulphuric acid concentrations in contrasting environments: implications on proxy calculations,” by Dada et al. describes a new method for estimating gas phase H₂SO₄ concentrations using relatively common measurements. The development of these so-called “proxies” for H₂SO₄ is important as this species is often used in global models for simulating the timing and intensity of new particle formation events. Additional proxies are especially needed for representing regions that were not include in previous attempts (e.g., China) or during time periods that we not considered previously (e.g., nighttime). Thus, this manuscript is potentially valuable and is, in principle, worthy of publication in ACP. I do however, wish to point out a one main item and a few minor issues that I would like the authors to respond to

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prior to recommending publication.

As a major concern: In the abstract of this manuscript and throughout the text the authors claim that the new proxy is “a more flexible and an important improvement of previous proxies.” While that may be true, we only are provided a comparison to the previous proxy developed in a pristine boreal forest atmosphere (the Petaja proxy). Nowhere do the authors compare their new proxy to that developed by Mikkonen et al. First of all, this makes little sense as the Mikkonen model was developed for a broader range of conditions than the Petaja model. If there is a valid reason to disregard the Mikkonen model then the authors should state that, or else they should show model predictions from that on all relevant figures as they did with the Petaja model. Otherwise they should remove the statement that the model is an improvement over other proxies, as they are only comparing to one.

As minor issues: Line 27: Just to be slightly fussy with wording, H₂SO₄ is important in new particle formation for actually two reasons: it has low volatility and also has strong intramolecular bonding abilities. Merely mentioning low volatility misses qualities that make this compound special. Line 64: I suggest that the authors put a sentence or two here to state why it is important to develop a proxy for H₂SO₄. Many readers may be aware of the reason but it's a small thing to do and will be a great benefit to those who would otherwise be left wondering why so much effort is being placed in this. Line 75: I notice that Dr. Mikkonen is a reviewer of this article, so perhaps he will make this point (and I hope he also raises the concern that I express above). While the statement that his parameterization does not include condensation sink it technically correct, I believe that he considered this in his statistical analysis and found that condensation sink, or rather higher aerosol loading, is associated both with the source and sink of H₂SO₄, and that is the reason why on average it does not appear in the parameterization. If true then perhaps more accurate to state it this way rather than to leave the reader to conclude that this model overlooked the potential role of condensation sink. Line 86: I suggest you choose a better word than “goodness” Line 249: this reference to

the Petaja paper seems strange. Why wasn't standard referencing used is referring to Equation 7 in the text (e.g., on line 245)?

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