Review report “Dynamic Spectra of Small-Mass Meteors” by Emma R. Mirizio, Robert G. Michell, and Marilia Samara. submitted to Annales Geophysicae

This article presents the results of a spectroscopic observations of faint meteors made from Arecibo during an observing campaign in May 2012. The scientific interest is reduced because the authors have not clearly identified the origin of the meteors studied, and the paper lacks of clear goals, more than a presentation of results. I also found that the paper lacks of scientific justification for many of the results presented. On the other hand, the manuscript doesn’t include a proper citation of previous literature on the topic. Finally, the scientific discussion and the conclusions are too vague as well. In these circumstances, I think that the authors should rewrite the entire manuscript. In order to help them to make it publishable, let me provide a detailed review with major and minor issues.

First of all, the abstract and the introduction should be significantly improved. I found that some of the abstract statements are too optimistic, and does not correspond to well-sustained findings. Meteor spectra presented in this manuscript have very low resolution, according the very few examples provided (e.g. Fig. 1). In addition, some assessments are too vague. You are not presenting comparisons to demonstrate differential ablation, neither the results seem to suppose “a greater understanding of the composition” of small meteoroids.

The stellar calibration is well done although perhaps the number of stars could be too small in some cases (this should be explained case by case). In addition, I found that the authors are not saying anything about an additional key correction needed. The meteors are moving in the CCD detector at a very different angular velocity than the stars that for these short videos appear static (see e.g. Rendtel, 1993). In consequence, the meteor magnitudes compared to the stars directly are systematically understimated. Our experience indicates that such loss could be so high as 3-4 magnitudes, depending of observing circumstances.

In consequence, if the authors want to infer realistic meteoroid masses, the meteor velocity must be properly quantified, and a correction applied to the stellar comparison in order to get the apparent meteor magnitudes. A case study should be selected and entirely reduced in the results section, before the discussion. In addition, if double station was performed and the distance to the observing station is known, the authors could estimate the absolute magnitude of the meteors at a distance of 100 km. All these details should be provided in a specific table for each meteor, perhaps as an annex.

Concerning the main results, I think that the presentation and discussion of the most relevant ones concerning the bulk chemical elemental variations are not properly made. First of all, the authors need to quantify the spectral resolution (e.g. in nm/pixel) and discuss properly if the spectra contain blended lines (e.g. doublets) and how this affects the results. Absolute calibration of the spectra is not possible for low-resolution spectra without a reconstructed trajectory. A thing that the authors can do is to quantify in each meteor spectrum the maximum intensity of the main emission lines. Then, the results can be presented properly in a ternary diagram showing the main rock-forming elements: Mg, Fe, Ca, etc... (see e.g. Madiedo et al., 2013).

In reference with Table 1, I found that a clear identification of the plausible origin of the meteoroids in missing. The authors should be able to identify if each meteor can be associated with a stream or being sporadic in nature. In the discussion they should also note that the observed chemical variations could be stochastic in nature, given the small sizes so the proportions of minerals could be highly variable (read e.g. Rietmeijer, 2004).

I also found that the authors should revise much more papers from scientific literature that are relevant to discuss these results. Small meteoroids are made by fine-grained aggregates that are built by diverse mineral components that end in low tensile strengths (Blum et al., 2006; Trigo-Rodríguez and Blum, 2009). The random distribution of such mixtures might produce different bulk chemical compositions, and tensile strengths (Rietmeijer, 2004; Trigo-Rodríguez and Blum, 2009). Some moderately volatile elements like e.g. Na are preferentially depleted in space during close approaches to the Sun or during long exposures to interplanetary medium (Trigo-Rodríguez et al., 2004).

The spectral results should be properly presented and compared with previous work of faint video meteors. I must say that the article is deeply biased concerning citations, and they are many papers in scientific literature discussing video spectra that might be relevant to improve the scientific discussion of the results (e.g. Borovička J., 2001; Koten et al., 2008; Vojáček, V. et al., 2019). All these papers could give you useful clues to deal with the reduction of video spectra of faint meteors and obtain valuable chemical clues on their bulk chemical compositions. In addition, you could made a better introduction about the use of video sensors for spectroscopy.

At the end of the paper I found that you should separate the discussion from the conclusions. In fact, you should clearly state as separate points the main findings of this research to demonstrate us that it deserves to be published.

Minor things:

1) The terminology should be applied correctly. For example, in the abstract the authors state “small-mass (2-200 mg) meteors”, but they should write “faint meteors produced by small-mass meteoroids”. Meteor brightness depends on luminous efficiency that is not studied here, so I wonder how they estimate such an accurate mass distribution.
2) Meteor spectra presented in this manuscript have very low resolution. Following previous comments, the authors should state this is the abstract and related sections. Some average values should be better presented and discussed (ternary diagrams)
3) Fragmentation is not explained in the introduction despite that might play a role in the ablation of stream meteoroids (Ceplecha et al., 1993)
4) Table 1 present quite surprising values. For example an extremely rare meteoroid penetrating at 77 km/s (then, from interstellar origin). Please explain the accuracy of the data, and your method to infer velocities. Can you really get for so low resolution imagery an uncertainty of 0.1 km/s? Please revise and choose an example case in the paper, explaining the trajectory reconstruction, and the method to get masses, azimuth angles, and velocities. Some of these values seem to be too precise, so need to be properly justified.

5) For the very fast meteors, have the authors detected the presence of 2nd order ionized emission lines? In positive case, have these lines influence in the meteor magnitude (luminous efficiency)?
6) In page 2 and in the discussion section the concept of differential ablation appears. This should be properly explained with clear citation to the papers describing this physical process (Borovička, 1994; Trigo-Rodríguez et al., 2004).

In consequence, I think that this manuscript should be revised properly before being considered for publication. My recommendation is a major review to satisfy all the above mentioned points.

Additional references

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Vojáček, V. et al. (2019) Properties of small meteoroids studied by meteor video observations. Astronomy & Astrophysics, Volume 621, id.A68, 21 pp.