Review of “Wind-driven droplet production and the transport of *Pseudomonas syringae* from aquatic environments” by Pietsch *et al.*

Summary

Pietsch *et al.* present measurements of aerosol droplets produced in a wind-wave flume along with measurements of the transport *Pseudomas syringae* via this mechanism. I welcome new measurements of this sort and it is excellent to see more groups moving into this field. That said this manuscript has a number of major issues that must be rectified before I can recommend publication. It is for this reason that I can only recommend major revisions.

Major points

The authors are not clear in their description of the mechanisms of wind-driven droplet production that are pertinent to the transfer of microbes

There are two production mechanisms for the production of spray particles: mechanical tearing of water droplets from wave crests at high wind speeds, so called spume droplets, and the bursting of bubbles at the water surface which form film droplets and jet droplets. Bubbles form predominantly from breaking wind waves [e.g. Wu, 1981] and these entrain air into the near-surface water column producing a plume of bubbles [Blanchard and Woodcock, 1957]. As these bubbles rise they form regions of foam at the surface known as whitecaps. Making this distinction early in the manuscript (and in the title of the manuscript) is critical for a number of reasons. Firstly, they operate across different environmental conditions - spume droplets are only formed under very high wind conditions whereas bubble bursting operates both at high wind speeds and under lower wind speed conditions (from around $5\,\mathrm{m\,s^{-1}}$). Secondly, they produce droplets of very different sizes (see Figure 1) and thirdly they are likely influenced by the composition of the surface of the water (the so-called surface microlayer) to different extents. These last two points may be very pertinent to the study of microbe transfer given that the surface microlayer is known to be enriched in microbes (the bacterioneuston). Therefore, I suggest that the distinction between different droplet types is made right from the start of the manuscript and that the title is changed to something along the following lines: “Wind-drive spume droplet production and the transport of *Pseudomonas syringae* from aquatic environments”

Following on from this, it is clear that the authors believe their system is also entraining air as bubbles which are subsequently bursting and producing film and jet droplets. While this may be the case, given their setup it seems unlikely and needs verification. I would urge the authors to conduct some measurements of the void fraction in the water. If they cannot do so, the addition

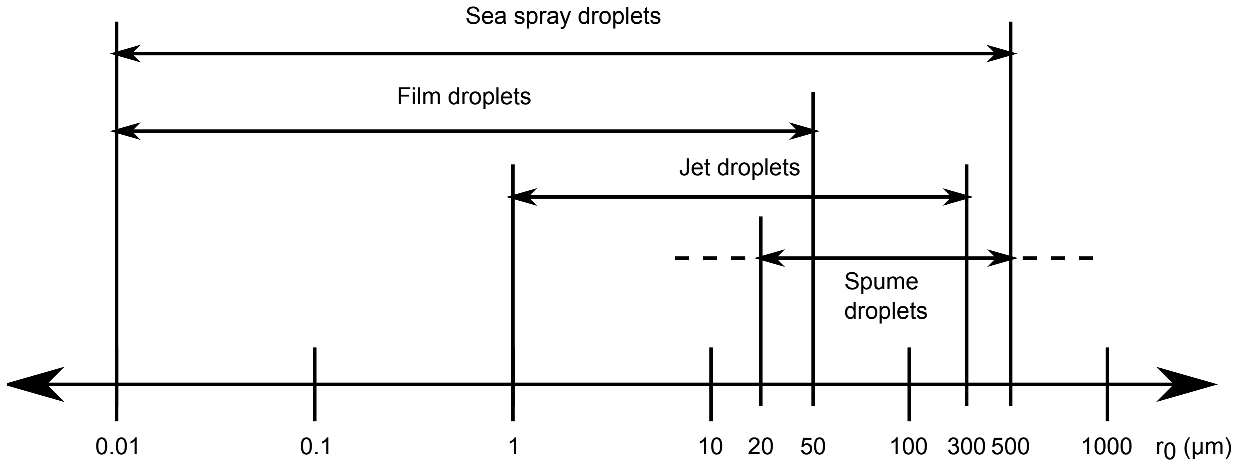

Figure 1: A schematic comparing the sizes of sea spray droplets based on Andreas [2002]. Although specific to sea spray aerosols the relative differences in droplet sizes likely also holds for spray droplets from freshwater environments

of a video showing the process of air entrainment in their flume (from above rather than the youtube video they already provide) should be made to the supplementary material.

I am also intrigued why the authors are interested in studying spume droplets - a mechanism restricted to higher wind speed conditions - in freshwater environments. Do authors think that wind speeds high enough to generate spume droplets are common in freshwater lakes? I would argue that they are not.

A number of important topics are missing from the literature review

Following on from an accurate description of the mechanisms of wind-driven droplet production a number of other topics pertinent to their study are either completely missing from the manuscript or are missing important recent references. For example, in the past 10 years a whole host of studies have investigated the process of spray aerosol production from bursting bubbles using porous glass/metal frits [e.g. Mårtensson et al., 2003], plunging jets [e.g Salter et al., 2014] and plunging waterfalls [e.g. Stokes et al., 2013].

As hinted at in the previous point, the surface microlayer plays a potentially critical role in the transfer of microbes from aquatic environments to the atmosphere. However, I cannot find any mention of the surface microlayer in the manuscript. As such, I point the authors in the direction of a good recent review by Cunliffe et al. [2013].

In addition to this, a series of recent studies have investigated the transfer of bacteria to the atmosphere via aerosolisation [e.g. Hultin et al., 2011, Fahlgren et al., 2015]. Despite this, there is no mention of these studies in the manuscript which suggests to me that the authors have not done a good enough job of reviewing the literature. I strongly urge the authors to go back to the literature and include a section on previous laboratory studies concerned with the transfer of bacteria to the atmosphere via aerosolisation.

A thorough characterisation of the laboratory wind-wave flume is is currently missing

Since the wind-wave setup used by the authors is rather small compared to other similar systems the authors should have conducted a series of experiments to test the sensitivity of the system to changed parameters. For example, how important was the depth of the water to the results the authors obtained? How did the authors decide on the dimensions of the wind-wave flume they have built? Why did the authors decide to forego the use of both mechanical wave paddles and fans - an approach often used in such setups in order to produce trains of heavily breaking waves? Is the wind-wave flume isolated from the room atmosphere and is the air in the flume passed through HEPA filters to remove all aerosols - I can find no mention of these issues in the manuscript. If the authors are trying to determine the flux of aerosols/bacteria from the water surface they need to make sure the room is not contaminating their measurements.

Further to this, several important variables that are critical when referencing spume drop observations were not measured. For example, an important physical reference in spume drop observations is the wave height, typically generalised via the significant wave height, H_s. The authors make no mention of the wave heights generated in their chamber so it is difficult for the reader to understand the state of the surface in the wind-wave tank. It would also be very useful to a reader familiar with aerosol studies focused on bubble-bursting/spume droplet production if the authors could provide an indication of the overall flux of particles from the water surface. Even if the authors do not have a condensation particle counter to hand, very reasonably priced optical particle size spectrometers are now available on the market that can be used to enumerate and size the spume droplets generated by the system the authors describe [e.g. the Alphasense OPC-N2 which was evaluated by Crilley et al., 2018].

From my perspective, a thorough characterisation of the wind-wave flume either as a separate manuscript or alongside the data presented here is a must before this work can be published.

Several variables that may be important to spume drop production were neither measured nor discussed

The authors use a solution of $MgSO_4$ in their wind-wave tunnel. While this may have simplified their experiments from a microbiological perspective I question the relevance of this solution to freshwater environments on Earth. This point is critical because it is well known that the salinity of water solutions affects bubble size distributions and subsequently the droplet size distributions that result when bubbles burst [Mårtensson et al., 2003, Tyree et al., 2007, Zábori et al., 2013a, Park et al., 2014, May et al., 2016]. Although this study only focuses on spume droplets rather than jet and film droplets some discussion of the potential effects of their choice of water solution on spume droplet production is required [see e.g. Fairall et al., 2009]. I would suggest that the authors include some comparison of the droplet sizes generated by freshwater to compare their measurements using $MgSO_4$. If their measurements of droplet sizes without *Pseudomonas syringae* were made in pure water than the authors need to be clearer about this.

It is well known that water temperature also affects the production of droplets from the water surface [Bowyer et al., 1990, Zábori et al., 2013b, Salter et al., 2014, 2015]. As such the authors should report the temperature of the water solution during their water experiments.

An adequate description of the experiments to allow replication is missing

From the description of the imaging experiments, the results section and the figures it is impossible to determine how many experiments were conducted, how many droplets were visualised or how long the experiments were conducted for. This information is critical if attempts to replicate the work are to be made. For example, was a single experiment conducted at each wind speed or were these experiments replicated? During each experiment how many droplets were analysed?

Other minor points

Line 28 - As well as "fragmentation" it is common to refer to those droplets torn from wave crests at high wind speeds as "spume droplets". As such, I suggest that where these droplets are first described both terms are used.

Line 82 - Whitecap coverage is much more nuanced than the discussion the others provide here and there have been more recent advances. I suggest the authors take a look at Callaghan et al. [2008, 2012].

Line 86 - When discussing the role of the bubble population at the surface the authors should include Salter et al. [2014].

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