Atmos. Chem. Phys. Discuss., 9, C3587–C3592, 2009
www.atmos-chem-phys-discuss.net/9/C3587/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Bacteria in the global atmosphere – Part 1: Review and synthesis of literature data for different ecosystems" *by* S. M. Burrows et al.

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Received and published: 5 August 2009

We thank Referee #1 for comments on the manuscript and for the overall very positive evaluation. Our responses to the individual comments follow.

1 Specific comments and responses

In general I find that in spite of the large number of references, there are a number of important papers that have not been mentioned. For example, many papers that deal

with bacterial ice nucleation such as those by David Sands, Cindy Morris, Yankofsky and more have not been mentioned.

In response to the referee's request, we have added several references to the introduction, including references to papers by Sands, Morris, Yankofsky, and Biggs, which deal primarily with bacterial ice nucleation and its potential impacts on precipitation.

In section 2.3 the paper mentions that dead cells and even fragments can still be good CCN and IN. A reference should be given here.

Since the CCN and IN activity depends only on the size and surface characteristics of a (insoluble) particle, we assume that it is not automatically lost when the cell is dead or fragmented. In the case of dead bacteria, they clearly can be IN-active, as when they are used in the commercial product Snomax (TM). For this, we will provided a reference. We were unable to find references specifically demonstrating the other claims. To address the referee's concern, we will revise the statement to read that dead and fragmented cells "may" act as CCN and IN.

In Section 3 it is mentioned that to a first approximation the effects of the meteorology on emissions can be expected to be similar to the effects on mineral dust and sea salt. I am not sure this is completely correct. Over the ocean bacteria could be released by bursting bubbles just like sea salt particles, however, over land, dust is emitted due to breaking of the soil dry upper crust, while on plants bacteria may be emitted by splashing of rain drops and by wind.

We have drawn the analogies to the emissions of mineral dust and sea salt since they could be useful because these types of aerosols are much more common and have been better studied than biological particles. Because many bacteria are found in soils, they will be emitted together with those soils when they enter the air. For an over view of dust emissions and their implementation in global climate models, see for instance Tegen (2003). For dust, emissions have been found to depend roughly on the third power of the surface shear velocity (u^{*3}), with u^* depending on wind speed, surface roughness and atmospheric stability, as well as on factors such as soil type, vegetation cover, soil moisture, snow cover, and maximum wind speeds (i.e. gusts).

Bacteria on the surface of plants may be emitted differently than dust, however, as the referee indicates, there should still be a dependence on wind speed. It is possible that the functional dependence is different than that for dust, and likely that the threshold wind speed for emissions from plants is lower than for emissions from the ground (Jones and Harrison, 2004). We are unaware of any laboratory or field studies specifically investigating how the emission rate for bacteria from leaf surfaces depends on wind speed, although some field studies such as Harrison et al. (2005) examine correlations between wind speed and airborne bacteria concentrations. It is difficult to draw conclusions about the emissions from such a study, since higher wind speeds are associated not only with higher emissions, but also with greater turbulence and therefore greater dilution of the boundary-layer air, leading to lower concentrations. The emission of bacteria from leaf surfaces when exposed to gusts of wind was studied by Lighthart et al. (1993), but this study did not investigate the dependence of emissions on varying wind speed.

As the referee notes, it has been observed that large raindrops falling on a plant can result in increased emissions, probably from shaking of the leaf surfaces. For instance, Robertson and Alexander (1994) observed higher concentrations of airborne bacteria after a simulated rainfall. This effect deserves further consideration; however, we expect it will contribute little to total emissions, because (1) this release mechanism will only occur during the first few minutes of rainfall until the leaf surface is wetted and (2) most bacteria released from leaf surfaces in this way will be subsequently collected by falling raindrops and removed from the air, unless the rainfall is very brief.

We will expand our discussion of emission mechanisms to better address these issues.

The end of Section 6 – the reports of higher concentrations emitted in the summer cannot be universal. There are places where the maximum is observed during the

rainy season (such as autumn, winter or spring). In fact these may suggest a close connection with temperature and/or precipitation. This is in agreement with what the paper says in Section 7.1.

We did not mean to imply that concentrations or emissions of bacteria are always highest in summer, at all locations. In Section 7 we discuss two field studies conducted in Oregon, in which both emissions and concentrations were both found to be highest in summer; however, we agree that this result can not automatically be generalized to all locations, and in fact is contradicted by measurements in other locations, as we mention in Section 7.1. In the revised paper, we will explicitly state that these studies were conducted in Oregon, and that they may not be universally applicable. Also, in Section 7.7, where we discuss seasonal cycles in bacteria concentrations, we will more clearly point out that the concentrations are not always highest in summer. Note that in the one study we are aware of that explicitly links the bacteria concentration to the seasonal precipitation (Rosas et al., 1994), a minimum in concentration was observed during the rainy season, rather than a maximum as stated by the referee. This is mentioned in Section 7.7 of the manuscript.

Section 7.7 –Yankofsky et al reported that in Israel the concentrations of ice nuclei bacteria are actually highest in winter when the temperatures are lowest. However, this may not be the same for the total bacteria concentrations.

We are not aware of and could not locate this particular study; it would be helpful if the referee could provide the exact reference. It is an interesting observation, but we agree with the referee's suggestion that what is true for ice nucleation active (INA) bacteria may not be true for the total bacteria in the atmosphere, the focus of our manuscript. It seems feasible to us that at least two mechanisms could cause an increase in the concentration of INA bacteria in the winter that would not affect the total bacteria in the same way: (1) bacteria with genes for the INA trait may be more likely to express it under low temperature conditions, and (2) bacteria possessing the INA trait may have some selective advantage in winter compared to other microorganisms.

Section 7.4 – there are papers by Sands and by K. Bigg that should be mentioned.

Section 8.1 – measurements of bacteria above the crops should include some of the works of David Sands in Montana.

The papers we know of about atmospheric bacteria by David Sands and E. Keith Bigg deal primarily with the "bioprecipitation" hypothesis and with a potential source of ice nuclei from marine bacteria, respectively. Since the body of our paper is a review of measurements of bacteria concentrations and emissions to the atmosphere, we find that these references are better placed in the introduction, and hope the referee will agree. As stated previously, we will add references to papers by both authors to the introduction.

Small error: end of Section 5 – should by "higher".

We thank the referee for pointing out this typo, which we will correct.

2 References

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 10777, 2009.