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## ***Interactive comment on “Hygroscopic behavior of individual NaNO<sub>3</sub> particles” by M.-J. Lee et al.***

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I have several concerns about “Hygroscopic behavior of individual NaNO<sub>3</sub> particles” by M.-J. Lee et al. which I hope the authors, reviewers and editor will consider.

The overall goal of this paper appears to be the determination of the deliquescence and efflorescence relative humidity of sodium nitrate which the authors acknowledge is “extensively studied” (line 26). Indeed, the literature already shows that when a crystalline aerosol is present the deliquescence point is ~75% . The literature also shows that efflorescence is unlikely above ~6% RH unless an inclusion is present (or heating or extensive drying is used). There is a discussion of these results in e.g. Gysel et al. EST 2002 and it is noteworthy this is the same humidity response that has been found for other particle types such as ammonium bisulfate and nitrate.

1. MY MAJOR CONCERN is that the authors essential describe the aerosol humidity

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response within their apparatus which is attributed largely to the presence of heterogeneous surfaces and/or contamination within the particles. While of interest to the users of this apparatus I have to stress THIS IS NOT relevant to the atmosphere (aerosols do not lay on such surfaces and foreign material, while present, is not correlated with what is found in their lab) - which is the purpose of the ACP journal. Furthermore, these results have already been shown in the literature dating back to 2002 (and before). The questions that need to be answered – to which I can find no response in this manuscript – (a) what is the atmospheric relevance of the results and (b) what determined here has not previously been shown in the literature? 2. To my knowledge sodium nitrate aerosol may be created by the interaction of sea salt aerosol with polluted conditions. It is NOT, as the introduction suggests, generated by sea spray in an externally mixed state (i.e., it is internally mixed with the other sea salt components). The term ‘secondary’ in the introduction is not used correctly and suggests there is some process whereby  $\text{NaNO}_3$  aerosol is generated from the gas phase (this is a primary aerosol and heterogeneous chemistry). This section needs to be re-written to clarify this. 3. The authors introduce the term ‘growth factor’ as a change in surface area. This is due to their optical technique which looks at a drop on a surface which resembles a hemisphere. However, there are serious issues with this. First, to my knowledge, GF is defined as a change in RADIUS, not surface area. This is the terminology from e.g. HTDMA instruments. Is there any precedence for this definition regarding surface area (SA)? Second, if GF is defined in this manner (SA) it can be confused by readers since the references in this paper use the radial definition. Third, the authors are viewing a projection of a hemisphere in two dimensions, not a true ‘surface area’. Is any correction made to truly derive surface area (if not this needs to be explicitly stated)? I firmly believe this is an incorrect use of an existing definition that was adapted to agree with this technique and needs to be corrected. At the very least a subscript ‘surface area’ needs to be included in the figures and clarification of if it is a projected SA or assumptions are made of the droplet shape. 4. The terms ‘generated’ and ‘powdery’ are highly subjective. For example an aerosol powder is still ‘generated’ for use in the

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lab and a 'powder' can result when an aqueous salt is dried. Terms such as 'wet dispersed' and 'dry dispersed' would be more correctly used and in agreement with the previous literature. 5. Have the authors considered their grinding of the crystalline material introduced the heterogeneous nucleus? 6. Reiterating the point that this paper is written as a microphysics experiment and atmospheric relevance is not addressed: The conclusions discuss the purity provided by the chemical company from which the material was purchased, not an atmospheric process. Even the interesting topic of heterogeneous efflorescence is essentially ignored as the authors don't know the source, size or composition of the inclusions that presumably cause the nucleation events. 7. I note there are numerous grammatical errors that need to be addressed throughout this paper.

I am aware that this is an unsolicited review and suggests this paper is inappropriate for ACP. I believe this is one of the purposes of a discussion journal and I therefore hope that the authors, reviewers and editor will give serious consideration to these points.

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