The authors have modelled seasonal dust concentrations in Europe using a regional meteorological model with chemical and aerosol transport. Using the results of these model experiments they have produced aerosol concentration independent parameterisations of ice nucleating particle concentrations for general modelling use. This concept goes some way to filling the gap between the increasing understanding of ice nucleation in the laboratory/field and the end user modeller. As such, I hope that this would be the first in a series of parameterisations produced to cover the whole globe.

The manuscript itself is in a good condition, fairly well written and readable, and has no dramatic issues. However I feel that the description of the methodology and statistical processes lacks some detail and I have some moderate concerns, mostly around the choice and determination of the parameterisation. I list these moderate issues below, followed by some minor comments and technical issues. Following consideration/correction of these issues I would recommend this manuscript for publication in ACP.

Moderate issues:

Pg. 32075 and throughout. Data and parameterisations have only been produced for winter and summer. Was there a specific reason for omitting spring and autumn? It makes the parameterisation a bit incomplete and I would recommend including spring and autumn parameterisations or recommendations for which months to use the summer/winter parameterisations.

Pg. 32089, Fig 1. Please explain why INP concentrations decrease so sharply at higher levels, as this is counter intuitive. For example, the summer dust surface area/number concentration shows no significant trend with altitude, but the number of INP goes down at high altitude. Until the fraction frozen predicted by the Niemand et al 2012 and/or Steinke et al. 2014 parameterisations is 1 (i.e. $n_{\mathrm{dust}} = n_{\mathrm{ice}}$) the ice concentration should continue to increase. Please ensure this is a real effect and not a statistical issue as the parameterisation values and shape are based on it.

Pg. 32078. Parameterisations. Were any other forms considered for the parameterisation? I agree that the median INP are a function of concentration and temperature, and hence proportional to altitude. However I would wonder if the multiplication of two lines would be a simpler representation, e.g. an equation representing the fraction frozen vs $z$ multiplied by the trend of dust vs $z$. I also think a temperature dependent form of the parameterisations needs to be provided, as most of the models that would use this parameterisation will carry temperature information. This is especially important since the 4 K range the 25-75 % temperatures cover would result in around an order of magnitude shift in INP concentrations.

Minor issues/comments:

Pg. 32073, line 4-5 and Pg. 32075, lines 10-16. In the same year as Hoose and Möhler, 2012, there was another ice nucleation laboratory review (Murray et al., 2012). In the interests of impartiality, this should also be referenced.

Pg. 32073, line 17. Please do not use 'and references therein' for referencing specific details, especially in a journal that does not have a reference limit.

Pg. 32073, line 21-22. Being pedantic, immersion and condensation are separate pathways. Yes there is debate as to whether the process actually causing ice nucleation is different, but the pathway from dry particle to ice is different.

Pg. 32073, line 25-29. You might also consider referencing the parameterisations produced by Phillips (e.g. Phillips et al., 2008) in here somewhere.

Pg. 32073-32077. Section 2 in general is lacking in detail. A list of specifics is provided below but should not be viewed as exhaustive.

Pg. 32074, line14. Please list the actual dust particle size bins. 5 bins for 0.1-24 µm seems a bit coarse – has an investigation been done to see if there are any effects from this bin scheme?

Pg. 32074, line 17. Is soil temperature taken into account for the dust emission? Significant parts of the domain will be below freezing for large periods of time. Also, how important is the in-domain dust emission compared with advected dust?

Pg. 32074, lines 22-27. Please clarify – were these evaluations performed with exactly the same setup as the model runs used in the current work?

Pg. 32075, line 3 and elsewhere. I would caution against referring to this as specifically desert dust; natural dust or soil dust is likely more appropriate. Of the samples used by Niemand et al., 2012 one is from an agricultural valley (Canaries Island Dust), one is from 50 Km north of Cairo: the Nile Delta ('Saharan' Dust), and one is from an unknown location (Israeli Dust – collected from a dust storm of unknown origin). No other description such as the organic content or mineralogy of these dusts was given.

Pg. 32075, lines 10-16. *'While there are indications that Arizona Test Dust is a more efficient ice nucleus than natural desert dust particles at the higher end of this temperature range, their behaviour is comparable at temperatures below 238 K (Hoose and Möhler, 2012; Hiranuma et al., 2014)'*

I have issues with this statement. It is not borne out by the reference to Hoose and Möhler, 2012 – Fig. 3 of that paper shows that for sub-micron particles ATD and natural dusts are similar above 238 K and ATD much better (i.e. lower RH) below 238 K, whereas in super-micron particles the natural dusts are better at all temperatures. Also, from a purely geological point of view using Hiranuma et al., 2014, which refers to Hematite as a desert/atmospheric dust proxy, is probably not appropriate. See the author response to comments/discussion on the ACPD version of that paper. References to any temperature threshold used, 238 K or otherwise, need to be made in the figures, especially figure 1.

Pg. 32075 line 25 – Pg. 23076 line 25. Please provide more detail on how the different means and medians are calculated and related to each other. For example, is the median potential INP calculated from the median particle surface area and concentration, or using the absolute particle surface area and concentration and then calculating the median using time/spatial data? Is the particle surface area calculated using a spherical assumption with the bin-centre radius? Is the 25-75% range for INP concentration estimated using the 25-75% range of the dust surface area/concentration, the temperature or both? The way these paragraphs are written implies that deposition is more important (i.e. has more INP) than immersion, but the deposition maximums are at a much lower temperature – it would be helpful clarify by adding a comment about the lower temperatures for deposition.

Pg. 32076 line 13-14. Have you checked the residuals to see if infrequent significant dust events is the cause of a high mean? The mean values are generally neglected in the remainder of the manuscript, which should be justified better. If the parameterisations that follow are for background INP might it not be better to remove outliers?

Pg. 32076, line 17. To save your readers from being obliged to look up and read the Bangert paper, please also provide the values.

Pg. 32077 line12. As suggested 2 comments above, the presence of significant outliers can be confirmed by analysing the residuals.

Pg. 32077 line 17. Why was an altitude of 5 km chosen?

Pg. 32077 line 19. This is indeed a remarkable sounding result. Are there any observations to back this up?

Pg. 32077 line 29 – Pg. 32078 line 2. I agree that in terms of latitude the dust concentration is largely constant throughout your domain. However with the prevailing winds and a major source of advected dust in the domain being from the west, I would think that the same comparison for longitude is needed before it can be said that dust concentration is constant within the domain.

Pg. 32078, line 12. Please clarify the identity of $\sigma$. Is this the usual definition of SD (Standard Deviation)? If so I would suggest this is not appropriate for your purpose, as using the standard deviation not only requires a normal distribution (which apparently you do not have) but is also derived from the mean – should it be used in combination with the median?

Pg. 32080, lines 10-14. This is a little confusing/concerning. The INP concentrations in Fig 1 are calculated using the Steinke et al. 2014 parameterisation. The DSF equation 6 is derived directly from the same function. Surely the results from the two should be the same?

Pg. 32081. Data from Conen et al. 2012 and Klein et al. 2010 appear in Fig. 4 but are not mentioned in the discussion and probably should be.

Pg. 32081, lines 9-10. Could a reason for the different temperature sensitivities be speculated upon?

Pg. 32081, line 14. I think that this sentence need to be rearranged - the INP are not at higher temperatures, the observations are.

Pg. 32081, lines 15-18. Discussion on Joly et al. 2014 could be expanded to include implications. Do the authors think this implies that Joly et al. 2014 was actually measuring dust, Niemand et al. 2012 was measuring biological, both, or that composition is not that important?

Pg. 32081, lines 21-27. This discussion is weakened by not including and considering the observational conditions. E.g. DeMott et al. 2010 was in the condensation mode at 240 K, which fits with Fig 4 nicely.

Pg. 32089, Fig 1. It would be helpful to readers who look at the figures before reading in depth if you define what Europe means, either in the caption or with a domain-description figure.

Pg. 32090, Fig 2. The scale in the upper panel runs from $10^1$ to $10^7$, whereas the data fits between $10^2$ and $10^6$. Since the aim is to instruct the reader that there is no significant variation with latitude, adjusting the y-axis would make this clearer.

Pg. 32091, Fig 3. Please consider adding the model data from Fig. 1 to this figure, as it would make it easier to see the quality of the parameterisations.

Pg. 32092, Fig 4. I could not find any reference to the left panel in the text. Could you speculate as to the cause of the wave-like feature in the left panel, and also the horizontal/diagonal bands in the data? Please also consider changing the symbols as they print very badly in the discussions version and are not discernible.

Technical:

Pg. 32074, line 10-27. This section needs more references.

Pg. 32075, line9. Replace 'a' with 'at'.

Pg. 32075, lines 17-21. The concept of potential INP is not new and should be referenced (Murray et al., 2012; Atkinson et al., 2013).

Pg. 32076, line 27, Pg. 32077 line 2. The values for deposition INP ($10^{-3}$ and $10^{-4}$) do not match figure 1, please check.

Pg. 32078, Eq. 2. Should $\varphi(t)\mathrm{d}t$ be $\varphi(z)\mathrm{d}z$? If not, please define $t$.

Pg. 32079, line 24. Insert: …suggested *by* Fletcher…

Pg. 23080, lines 20-25. Please check your use of past-present tense. Specifically: …observations *were* typically… and …DeMott et al. (2010) develop*ed*…

Pg. 32081, lines 19-21. Please provide a reference for this sentence, and clarify if the first half of this sentence refers to the parameterisation results or field observations.

References supplementary to the original manuscript:

Murray, B. J., O'Sullivan, D., Atkinson, J. D., and Webb, M. E.: Ice nucleation by particles immersed in supercooled cloud droplets, Chem. Soc. Rev., 41, 6519-6554, 10.1039/C2CS35200A, 2012.

Phillips, V. T. J., DeMott, P. J., and Andronache, C.: An Empirical Parameterization of Heterogeneous Ice Nucleation for Multiple Chemical Species of Aerosol, J. Atmos. Sci., 65, 2757-2783, 10.1175/2007jas2546.1, 2008.