Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-852-RC1, 2020

Atmospheric Chemistry and Physics Discussions
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ACPD

Interactive comment

Interactive comment on "Opinion: Cloud-phase climate feedback and the importance of ice-nucleating particles" *by* Benjamin J. Murray et al.

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Received and published: 10 September 2020

General Comments

I thank the authors for this stimulating opinion piece. My interest is not only in the topic as a person working in this field, but as someone who also carries a healthy question about how important primary nucleation may be claimed to be in some circumstances, due to observing clouds in many cloud-based field campaigns. Hence, I expected to see secondary ice formation processes brought front and center as a related discussion point, because it may be such an important process in determining cloud properties for radiative transfer and precipitation. To what extent is still unknown

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Discussion paper

in different cloud scenarios, and to what extent primary ice nucleation processes constrain and feed back to influence secondary processes is also uncertain. I imagine that the thinking here is to provide a special focus on primary nucleation as an independent need, or perhaps also that the higher latitudes are not regions where secondary processes can dominate. The former point is valid, and enough reason to support this opinion piece. I think the latter point is being questioned by some recent studies now making it into the literature. In fact, the complex link between the properties of unfrozen and frozen cloud regions in the same systems is really only alluded to very late in the paper (statement on line 316-317 regarding INP removal processes). With a potentially deeper cloud phase that is >$0°$C and greater cloud depth before freezing ensues, a stimulation of secondary processes and stimulation of warm rain processes could occur. Such an outcome is of course not at all clear, and hence, not only do we need a continued ramping up of data collection and process studies related to primary ice nucleation, but secondary ice formation processes may need just as much attention (perhaps not relegated only to a late mention on lines 322-324 of the piece), and interactions between warm and cold cloud phases (i.e., microphysics in general) deserves attention as a feedback. I am not sure where to put that in the paper, but it could be consolidated. I realize that this paper is attempting to remain finely focused on a key and important variable and objective, and section 4 alludes some other needs while justifying the focus of the opinion. Nevertheless, I wonder if it could be unsatisfying to acquire a bundle of new ice nucleation data, but not have a grasp on being able to properly simulate ice evolution due to lack of understanding of other processes.

This possibly sounds more negative than intended, and odd coming from an ice nucleation researcher. I hope not. Otherwise, I can only support many of the contentions here. There are some truly excellent sections and statements made. I have an assortment of specific comments below, the most critical of which deal with expanding the discussion of some needs. A few are simply editorial.

Specific Comments

1) Introduction

Line 58: I looked later in the paper, but did not find this. What will constitute sufficient information about INPs in the regions poleward of 45 degrees? There have been at least two major campaigns in the Southern Ocean region since 2017, the ACE campaign and a suite of studies from 2016 to 2018 that were supported by the U.S. NSF and DOE, and Australian and New Zealand organizations, all including measurements of ice nucleating particles. Some of these are recently published in McCluskey et al. (2018a), Schmale et al. (2019) and Welti et al. (2020), two of which are noted (Welti et al is recently in review in ACP). More measurements are sure to appear. This is not a question of referencing this work though, it is an honest question about the range of spatial and temporal scales that will be needed both in the Southern Ocean and in Arctic regions, where similar campaigns have occurred as referenced in the paper, and more are in the works.

2) The cloud-phase feedback and the importance of ice-nucleating particles

Lines 85-86: While this point about INPs being cloud destroying agents is well-taken, it occurs to me that this paper has mainly considered a uni-directional change in INPs in the future. It could go either way, right? One can imagine either that a warming planet results in increases or decreases in INPs in different regions, and that decreases could be driven by cloud changes as well. That is, the net impact in a remote region like the Southern Ocean is a consequence of gains and losses of INPs, and this is not only affected by source strengths but by scavenging processes.

Lines 93-94: Here one needs to ask if this is a truth or a point for inspection that the INP population controls the amount of ice in most shallow clouds. To some extent this is certainly proven for Arctic clouds, but it is not what has been observed in all clouds over the Southern Ocean, depending on the scales one is referring to. For clouds where secondary ice processes provide the ultimate control on maximum ice concentrations and the distribution of precipitation, this might not be true. A question is to what extent the secondary process cares about the size of the "trigger" imposed by primary ice formation, and to what extent the areas of secondary processes count from the radiative balance standpoint for wide regions where INP concentrations are generally low. It is difficult for me to support this point strongly, due to the fact that some papers are presently in review and without open access. Nevertheless, I wonder if this point deserves some mention.

Line 95: Do we also need to define the areal extent of critical regions where INP concentrations may be relatively higher or lower? As made apparent in a few recent papers, there are broad regions of the Earth where INPs appear to be well-mixed and relatively uniform (Welti et al., 2020; Schrod et al., 2020), just as there are broad regions like the Southern Ocean where the concentrations are markedly different (reduced) compared to continental regions.

Lines 98-106: I believe there may actually be more to say here. For example, the weaker slope for the fertile soil dust may mask a complete difference in the nature of the INPs versus the mineral dust, speaking of their encompassing both microbial components and their byproduct fragments and organic molecules from their action in soils (e.g., Hill et al., 2016). These other biogenic (primary biological particles and molecular organics), and potentially most important INP sources in the higher temperature regime of supercooled clouds (Garcia et al., 2012; Huffman et al., 2013; O'Sullivan et al., 2018; Mignani et al., 2020; Schneider et al., 2020), may also be altered in a warming future world since they depend on environmental disturbances and conditions at the surface of the Earth. Interestingly, it is clear based on the sheer explosion of recent publication submissions, that the community is already taking up the charge to establish INP spectra and types from different sources, which support the statement on line 105. I think the work beyond the growing number of short and long-term assessments may come in being able to piece out the specific contributors in different source scenarios.

3. To what extent is the persistence of supercooled liquid clouds related to ice nucle-

ation?

Line 146: Missing date on DeMott references. Could be 2016?

Line 150: May I request a definition of biological particles? It may be obvious to the authors, but the wider community reading this may not understand if one means microbes or all biologically-derived INPs (i.e., organic molecules). There is growing evidence that the former are not the same as the latter, in likely following different dependencies (e.g., Mignani et al., 2020; Schneider et al., 2020; Suski et al., 2018). See note below also regarding "biogenic" sources. Also on line 150, why "potentially" combustion particles? There seems ample evidence that biomass burning is a clear source (Schill et al., 2020, and references therein), if not necessarily black carbon.

7. What do we currently know about atmospheric INP in the regions important for the cloud- phase feedback?

Line 215: This is the first use of the term "biogenic" INP sources. It think it is important to be clear on definitions here. It is a point we as a community struggle with still.

Lines 219-220: I hesitate to make this comment, but does Fig. 4 need a qualifier regarding "recognizing that results may to some extent reflect both true INP variability and INP measurement capabilities/uncertainties"? Or "assuming no measurement biases" or "assuming perfect and equivalent measurement capabilities in all studies shown"? Perhaps this is the point of many of your notes in the Supplement, which I only noted late in writing these points.

Section 7.2 INP in the southern mid- to high-latitudes: I wonder if Antarctica is the only consideration as a changing source? There are known land regions impacting the broader region, with variations in transports in different areas that have occurred in the past or may occur in the future (e.g., Neff and Bertler, 2015). In a related regard, the work of Bigg (1973), averaged in Fig. 4, suggests a drastically altered INP scenario now compared that present over 50 years ago. This has been at least briefly discussed in some of the referenced recent papers, but you make no note of that here.

8. Important areas of future research

Line 281: Should it be "aerosol" generally, rather than "dust"? At least one of the references noted was not specific to dust.

Lines 298-300: Clearly these may be examples familiar to the authors, but you should perhaps mention that other semi-autonomous instrument developments have been occurring for existing technologies within the community (e.g., Bi et al., 2019; Brunner and Kanji, 2020). Others are underway. Also, note that the Möhler et al. (2020) is now in discussion.

Editorial notes:

Line 42: This may be a language preference, but I think of higher and lower latitudes, so higher than 45 degrees rather than above 45 degrees. Or perhaps "poleward" of 45 degrees?

Section 6: I am sure that the authors now realize that this section repeats section 5.

Line 345: Should it be Fig. 4 instead of Fig. 3? Also, Jessie's name is misspelled.

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-852, 2020.