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Interactive comment on "Brief communication: Organochlorine pesticides in an archived firn core from Law Dome, East Antarctica" *by* M. Bigot et al.

Anonymous Referee #2

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Review of Bigot et al., "Organochlorine pesticides in an archived firn core from Law Dome, East Antarctica" from "The Cryosphere Discussions", doi:10.5194/tc-2016-178.

This paper documents the results of analysis of organochlorine pesticides in a firn/ice core from Law Dome, Antarctica. The core was drilled several years before analysis, and was kept in storage. There are few studies documenting the accumulation of organic contaminants in ice (of any kind) from Antarctica that were transported by the atmosphere. The reason for so few studies are many, including the lack of available cores, which itself is related to the difficulty with drilling, retrieving and storing cores from Antarctica. Another issue is the expense involved with analytical work. The work of the authors in this regard is greatly appreciated.

This paper should be given consideration for publication after addressing the general

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and specific comments made below.

General comments:

In my opinion, the paper addresses a relevant issue within the scope of TCD. The idea presented and tools used in the research are not novel, but the data are new and are a contribution to understanding levels of contamination found in the Law Dome area of Antarctica.

The conclusions are generally sound, although rewriting should be considered. Yes, the results are from as early as 70 years ago, but that results from inherent poor dating resolution of organic contaminants in ice cores. The quantitative difference between OCP amounts found the Antarctic and the Arctic are important, but the authors need to be much more precise in P6L30+ where they state that “deposition rates are orders of magnitude lower than those from Arctic regions”. In that statement, they are suggesting that the Arctic is one uniform region with regard to OCP deposition rates, and the published literature shows that this is not true (some of the publications showing this are not included in the reference list). Within the conclusions, I disagree with the use of the word “in-situ” with respect to melting techniques being supported by the references used. I am not certain that the referenced investigations used “in-situ” melting the way it was done for the current investigation because not all of the references specifically state melting at a field site.

The scientific methods and assumptions are clearly stated, along with some of the limitations. I am a bit concerned that limitations involved with using the KISP have not been identified, including long running times and short battery life. However, these are only a problem when using the KISP in the field, instead of in a laboratory.

The results have not been over-interpreted, which is important. The results here are very limited, but are still significant.

It would likely not be possible to reproduce these results, which is a common issue with

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environmental work on this level. There is very little related work in Antarctica, and it appears that the authors have given credit to previous work.

I have one question regarding the title: Is the DSS core really firn to a depth of 45.6 meters? There is no mention of the transition depth from firn to ice in this paper.

In the abstract, I do not understand what is meant by “nominal modern-use chemical contamination”. How do deposition rates “orders of magnitude lower than those from Arctic regions” support validity? This is not standard QC procedure.

In general, the paper is well organized and carefully written, with the issues noted elsewhere. I note that the use of C. E. in the paper (including the abstract) is not define, requiring that the reader understand what this means. That may not be the case.

Figures: Figure 1 needs additional information, including the dimensions of the unit. It would be useful to know the grade of N2 used and its circulation within the device, and not just that it is a “clean atmosphere”.

There is a significant issue with references used in the paper. Why is the Legrand et al. 1984 reference used for aerosols in a glacier? Why not Murozumi et al., 1969 (GCA, 1969, 33, 1247-)? Murozomi et al. also had data for contaminant lead both from Greenland and Antarctica, and was the first paper to identify contamination of ice cores by long-range transport of an anthropogenic substance. This issue takes on greater relevance with the earlier studies about organic contaminants in the Arctic used as references in this manuscript. One assumes that the Gregor et al. (1995) paper is used as a reference because it was the earliest study on PCB deposition in an Arctic glacier. This assumption arises because of absence of later reference to PCBs in glaciers. So again, why use Legrand et al., 1984, as a reference if it is not the earliest?

There is no reason (or reference) given for the quantification of PBDEs as evidence of contamination. What is the rationale behind this?

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Specific comments & technical issues:

P1L26: Replace “hereby” with “thereby”. The statement made here, to “minimize the environmental and human health hazards that they pose” is overstated. Even though the compounds on the Stockholm list have been banned or restricted, they are still found in the environment. And they are still moving around. The only thing Stockholm can accomplish is prevent the mass of these contaminants now in the environment from becoming greater.

P1L33: Again, Legrand et al., 1984 is not the best reference in this context.

P2L5: The claim that “only one study has documented OCP concentrations in glacial ice/firn from the Arctic” is not correct. I can immediately think of 3 without looking.

P2L6: To say that there are no OCP studies in firn/ice cores from Antarctica is splitting hairs a bit too much for work like this. What about Kang et al., 2012, from the reference list?

P5L9: Again, this limited selection of references is a bit surprising considering those not mentioned. Why are no comparisons offered between the results of Kang et al. 2012 and the results of this manuscript?

P3L4: This repeats P2L2.

P3L13: Apparently it is true that Isaksson et al. (2003) never mention the diameter of the core used. The current manuscript also never mentions the diameter of the Law Dome core.

P3L15: While it may be true that no earlier firn core studies are available to use as a guide for sample volume needed from Antarctica, the authors could have used Kang et al. study on surface snow as a guide.

P3L19: C. E.?

P3L26: Was the system shown in Figure 1 capable of holding 144 L of melt?

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P4L27: The original results were not “corrected to estimate the mean deposition”. It is not a matter of correction, but calculation.

P4L28: In my dictionary, “basal area” is defined as the area of total tree trunks (diamters) as a fraction of given land area where the trees are growing. That does not seem to apply here.

P4L31: What is meant by “dissolved fraction of the melt water”?

P6L7: Reference to Stockholm for ban on Dieldrin is not very good. Dieldrin was banned under other regulations many years before 2004.

P7L8: The authors need to do a better job describing “in situ”.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-178, 2016.