Clim. Past Discuss., 9, C2478–C2484, 2013
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Climate of the Past Discussions

Interactive comment on "Seasonal changes in glacial polynya activity inferred from Weddell Sea varves" *by* D. Sprenk et al.

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Received and published: 30 October 2013

Summary:

Sprenk et al. present a series of physical and geochemical analyses of a Weddell Sea core, PS1795, to support the hypothesis that winter polynya formation led to enhanced Antarctic-region bottom water formation during the Last Glacial Maximum. The authors describe extensive new data for their core, including wet-bulk density, magnetic susceptibility, total carbon/nitrogen/sulfur, biogenic opal, IRD counts, thin section descriptions, a suite of XRF elemental concentrations, and 14C dates. They also use a few interesting image processing tools to identify particle sizes and laminations.

These data greatly improve the description of core sediment and the character of late

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Interactive Discussion

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glacial period varves that have been previously reported. The authors go on to explain a proposed mechanism of varve formationâĂŤseasonal variations in water velocity due to changes in the volumes of brine formed during the closing of polynyasâĂŤand to further describe the relationship between katabatic winds and polynya formation.

Evaluation:

The authors have clearly gone through a great deal of effort to gather new physical and geochemical data on core PS1795. What is the importance of this data, and how does it influence the interpretation of varve formation, polynya activity, and bottom water formation? It really is a nice dataset, but it is difficult for me to see the importance of the data with regards to the discussion and conclusions. Much of the material in the discussion has been examined previously- Weber et al., 2010 and 2011 give good evidence that the laminations are seasonal varves, and several papers (e.g., Weber et al., 1994; Smith et al., 2010; Weber et al., 2010) discuss polynya activity generating bottom water. The authors therefore need to specify the contribution of their data to their interpretations. Does the new data remove some uncertainties regarding the mechanism of varve formation?

The introduction and discussion cover very similar ideas. It is not clear how interpretations from this study differ from interpretations in previous work. The authors need to be more explicit about what ideas are their own (and new), and what ideas have already been published. What does this study bring to the field?

The data presented here are potentially very valuable, and I believe they should be published. However, further work is needed to a) interpret the data and b) place those interpretations into context of previous studies.

General Comments:

I agree with the first reviewer in their comment regarding the deposition of IRD during winter months. How do the authors reconcile winter IRD deposition with the explanation

CPD
9, C2478–C2484, 2013

that faster bottom waters deposited the silt-rich layers? If there was iceberg rafting of sands, there was likely iceberg rafting of silts. Can the varves be explained simply by invoking seasonal IRD deposition, and without appealing to bottom water velocities? Given that there really is only a slight increase in the percentage of silt-sized particles within the light layers, any input of silt to the core should be considered relevant.

The authors mention a few studies (e.g., Renfrew et al., 2002; Justino and Peltier, 2006) to support seasonal polynya activity, which is central to the authors' interpretation of varve formation. But, there are two issues stemming from this: how seasonal is polynya activity, and how seasonal are the local katabatic winds? Renfrew et al. show some polynya ice production all throughout the years of their study, with the exception of during the very peak of summer. I suspect (although I could be wrong), that with the more expansive sea ice of the LGM, polynya formation and closure could persist throughout the summer. The LGM sea ice extent estimated by Gersonde et al., 2005 would support this, I think. The authors might also benefit from referring to seasons as "warmer" and "colder", to get around the definitions of "winter" and "summer".

While it is likely, judging from the Justino and Pelier (2006) modeling study and from modern observations of katabatic winds on Antarctica, that LGM katabatic winds exhibited some seasonality, I do not think that they entirely ceased for the summer. Katabatic winds during the modern summer are less frequent and less intense than during winter (see Nylen et al., 2004, JGR, figure 4), but they do occur throughout the year. How would this impact the authors' interpretation of the varves? Additionally, there are influences on katabatic winds beyond simple seasonal temperature changes. Yasunari and Kodama (1993, JGR) demonstrate that E. Antarctic katabatic wind strength increased with a weakening in upper level westerlies, and Nylen et al. describe synoptic scale forcings on katabatic wind strength. An explanation of the atmospheric dynamics here is well beyond my expertise, but I'd suggest that the authors consider these influences when making their interpretations. Overall, a stronger case needs to be made for a very seasonal katabatic cycle.

CPD
9, C2478–C2484, 2013

Specific Comments (Page, Line):

What happens to the clay-sized particles during the deposition of the light-colored layers? Are they winnowed away due to the swifter currents, do they settle out farther downstream from the core site, or are they present in the light layers but diluted by the coarser particles? A brief discussion of this would be helpful to understanding varve formation mechanisms.

Why use a 20-63 μm definition for silt size? A brief discussion of this would be helpful, particularly because clay-sized particles (<2 μm or <4μm) aren't really produced by subglacial abrasion (which I assume is the source of most of the core sediment). If the "clayey" layers in the manuscript are actually fine silts, this should be mentioned.

I would like to see a description of the thickness of layers (in particular, is there a difference between average thickness of clay-rich layers and silt-rich layers?).

Is there IRD being deposited within modern polynyas? I know that this information may not exist, but if it does, it would be interesting to discuss as a comparison to your varve mechanism.

Would you predict any downstream (away from the coast) effect of repeated and frequent polynya formation? For instance, would you develop freshwater lenses at the margin of the sea ice resulting from the increased ice flux to the area of melt? There's no need to incorporate this into the manuscript, but it might be interesting to think about.

I'm a bit unclear as to how the orientation of the channels, and the process of overspilling, affect your interpretations. I think this could be expanded on, and incorporated more into your interpretation of varve formation mechanism.

I understand the desire for cautious writing, but you need to finish your discussion and conclusion sections on strong notes, otherwise the reader will disregard what you have said. I would, in fact, remove your final conclusions paragraph.

CPD
9, C2478–C2484, 2013

I agree with the first reviewer that a more uniform name needs to be given to the sediment layers, and that the writing needs to be cleaned up a bit (it's not bad, but there are some grammar errors).

5124, 16, and elsewhere: The term glacial (or glacially) can refer to a glacial period or a glacial process. Reword to clarify.

5125, 5: What is well-known about bottom-water formation (e.g., volume, rate, location)? Citation also needed.

5125, 9-12: Reword to avoid writing verbatim from Weber et al., 2010.

5125, 13: Citation needed for the lack of major floating ice shelves (e.g., Andersen, 2002, QSR)

5125, 29-5126, 6: Interesting, but irrelevant to this study. Remove if you decide not to expand on this in the discussion.

5126, 7-12: This is where you can expand on the motivation for your study. Why do you want this data? What do you hope it to solve? This will help you explain how this study contributes to the understanding of bottom water formation or varve formation or polynya activity.

5126, 14: Cite studies demonstrating that coastal polynyas were active (e.g., Smith et al., 2010; Mackensen et al., 1989, Marine Geology; Mackensen et al., 1994, NATO ASI Series).

5126, 25: What do you mean by "every core site"? Every core site from Weber et al., 2011?

5127, 19: I would label the Weddell Gyre on your Figure 1.

5127, 19-5128, 10: There is a lot of detailed background on ocean circulation here that isn't discussed later in the manuscript. I would remove anything you don't talk about further on. However, you could include a brief background here on atmospheric circu-

CPD

9, C2478–C2484, 2013

lation/dynamics influencing katabatic winds (and their seasonality). Methods section: Avoid using a few of the acronyms that are less frequently used in the manuscript. At least some of WBD, MSCL, MS, TN, TS, TIC, TOC, and TC could just be spelled out, for ease of reading.

5131, 1: Can you cite Weber instead of Sprenk et al., 2013b? Because your 2013 manuscript isn't published, you should cite one of the Weber papers whenever possible.

5132, 12-13: The sentence beginning "Therefore, the dense. . ." is confusing. Reword.

5132, 17-21: This is methods, and can be moved to the methods section.

5132, 26-28: Sentence beginning "One age-model. . ." is confusing. Reword. Also, which age model do you use in the rest of the paper? The one incorporating the hiatus?

5134, 22-5135, 2: This can be in the methods section.

5134, 25: Why aren't the varves horizontal? Is this an artifact of the coring process?

5135, 8-20: Why is the origin and distribution of Si important to this study? Expand on your discussion.

5135, 21-5136, 2: Why is it important that clay-rich layers have higher counts of elements that are found in clay minerals? Does this shed insight onto the sediment source?

5137, 1: Not sure what "yet in some parts even no detected dark particles" means.

5137, 7-8: The LGM age of the varves in this part of the Weddell Sea was established in Weber et al., 2011.

5137, 15-19: Why is the statement beginning "In contrast. . ." important? Does it tie back to sedimentation rate? If so, explain.

5137, 20- 5138, 4: Why is this important to the study?

CPD

9, C2478–C2484, 2013

5138, 11: It took me a couple of reads to understand that sea ice production is increased because of the repeated closing of coastal polynyas. Can you state this explicitly, either here or in the introduction?

5138, 26: You mention cyclones and barrier winds as potentially influencing seasonality of polynya activity, but don't include these mechanisms in the rest of your discussion.

5140, 4: A citation is needed following "Weddell Gyre."

Conclusions: This is a bit detailed for a conclusions section. Remove the methods mentioned here. Focus more on the interpretations, and less on the detailed geochemical results.

References: I think that the Smith et al., 2010 study should be cited more throughout the manuscript, as it the ideas presented in it are quite similar to that of your study.

Table 1: Cut down on the information in the table heading- if it's in the text, you don't have to write it here. Figure 3: Because of the low resolution of B, C, and D, they do not add much to the reader's understanding. Also, in the figure caption, replace "regular" with "typical" ("regular" can refer to either something normal or something with a periodicity, either of which could apply here).

Interactive comment on Clim. Past Discuss., 9, 5123, 2013.