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Climate
of the Past
Discussions

Interactive comment on "Little Ice Age climate and oceanic conditions of the Ross Sea, Antarctica from a coastal ice core record" *by* R. H. Rhodes et al.

Anonymous Referee #1

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The paper presents geochemical records of stable isotopes (D and 18O) ion chemistry and ICPMS element measurements from a 120m ice core record drilled in a costal site in Antarctica (Mount Erebus Saddle-MES). The cores were sampled with about 2cm resolution using a melter system for discrete aliquots. The seasonal variations of geochemical elements are used for the ice core dating and the record is said to cover 678 years, with $\sim$ 8 years accuracy. From the 1970-2006 period, the seasonal variations are suggested from stable isotopes (with maximum values assigned to summer and taken as reference), on lithophile elements (faint summer minimum), on sodium (summer maximum). The period prior 1850 is compared to the more recent one: on average the mean deuterium content is 7 per mil lower and interpreted by a temperature lower

by 1.75°C; the lithophile elements which are higher by a factor of about 3 are interpreted by a stronger winter katabatic wind. From the MS- ion which is taken as a proxy of the nearby marine biogenic production, the record displays an increase between 1825 and 1875 that may correspond to diatom production seen in two marine records. Finally, the authors suggest that prior 1850, this sector of Ross Ice Shelf was substantially cooler with stronger katabatic wind which would have contributed to maintain a polynia that favored the biological productivity and deep water formation. We have to acknowledge the modern laboratory techniques deployed for an ice core study to produce numerous geochemical data series (sets of almost 6000 data) obtained at high resolution for 2 stable water isotopes by MS and laser, 7 chemical elements by IC, more than 20 elements by ICPMS-while only a few of them is finally used. The value of a climate record relies on quality of the analytical measurements, on the dating of the archive, and on the reliability of the climate proxy at the location. For this paper, while the climatic scenario looks plausible, one may feel some over interpretation of the data. First I feel uncomfortable with the use of the term LIA for Antarctica; to what period this corresponds, and here understood as a probable global cooling event. I am very reserved for the dating of this ice core. Also, the link between dust and winter katabatic wind strength which is based from the seasonal variation is hard to accept. I regret that in spite of the number of data which are produced here, no basic statistical analysis was made to insure the data are (or are not) normally distributed. Finally the link between MS- record and diatom records could be the result of chance. I have some minor concerns on the quality of the data (isotopes and chemistry) and on accuracy of the temperature change evaluation. On overall, the paper is a good technical report with thousands of new data in spite of a minimum statistical study is missing. Due to the dating uncertainties of the ice core and some over interpretation of the data, its scientific significance remain poor to fair.

Here are the concerns: - Ice core dating: the dating relies first on the identification of seasonal peaks of the stable isotopes and the use of a Herron Langway densification model. The concern is the absence of reliable time markers. If one could be ready to accept the tritium increase from ~1970's, this in spite that only 6 measurements have been done, problems are occurring in identifying the seasonal maximum to the year for the period 1970 to 2005 in Fig 6. From the stable isotope we can count 30 years in this 36 year interval and the dating error is already 15%! The authors extended their exercise to the previous period covered by the ice core. They made some allusion to diffusion of the isotope signal and its restitution for the dating, but this without evaluation how important it is? Does the restituted signal have more peaks than the original record? For the time markers to constrain the ice core chronology, the authors suggest the identification of the Tambora event from Bi/Al, Ti/Al ratio, as SO4 from marine origin obliterates the expected volcanic spikes. Because the importance of this marker, we may wonder from Fig 3, what decipher the chemical signal at circa 1817 from the peaks in various elements at circa 1830? In Fig 4, they tentatively proposed the recent anthropogenic input of Pb as time marker as suggested elsewhere. If true one would expect the authors convince us by showing the entire Pb record with the recent increase as a hockey stick-like record. Unfortunately they show only the record back to 1840. Notwithstanding, from the older marker assigned to Tambora event (61m, 200 years old) the method is extrapolated to 59 m more down to 120m depth adding almost 500 years to the ice record. It is well known that glaciological models could be wrong by a factor of two depending on accumulation rate and the chosen ice thinning function. This to conclude that the dating they propose is very disputable and the ~8 year accuracy in age layers is well underestimated (likely by one order of magnitude). This has implications for their discussion, and all time periods identified by year (AD) are meaningless. -On the seasonal variation of various geochemical elements. Fig 5 displays the monthly mean variation from the 1970-2006 period for stable isotope, sodium, aluminum and manganese. For the last 2 elements, the amplitude of the seasonal signal is very low with respect to the variability of concentrations: e.g. for aluminum the seasonal amplitude is only 0.4 ppb while concentrations may vary from 0.09 to 7.6 ppb! With respect to the expected standard deviation (not indicated but obviously » 0.4 ppb) the seasonal variation of dust is meaningless. In other words from

the bulk variance of dust data, the part attributed to the seasonal variation is negligible. As consequence the interpretation which is made in term of winter wind is not hard to accept! This also to point out the lack of statistical studies from the ~6000 data produced. We may wonder for lithophiles elements which display spikes, if concentrations are normally distributed. -The interpretation and discussion in term of climate is confusing. -On the use of Little Ice age (LIA): from the paper one may understand that is a global cooling event as no detail is given. The authors even expect to see the start and the onset of LIA in the MES records... Indeed LIA corresponds to a cold period (between ~1400 and 1850's) well represented in Europe by advance of glaciers (France and Switzerland) the decline of temperature in England and as well as variable responses for surrounding countries and variable regional responses. LIA should not be used for a climate record of the Southern hemisphere. As example, for the Younger Dryas which is recognized in Europe and Greenland, has a corresponding event identified in Antarctic ice core, called ARC, which is not in phase. Because the regional variability, it is usual to explore the link between different records by comparison and to discuss their possible relationship through concept of teleconnexion, or coupling, both of them being either permanent or intermittent. -There is also allusion to the see-saw phenomenon between Greenland and Antarctica which is documented during the last glacial period, possibly occurred during Holocene and LIA. To my understanding this is a bad extrapolation, as climate coupling between latitudes to favor of the meridional links are likely absent during interglacial periods. This seems is of secondary importance, but the paper give us the impression that everything in climate could be present everywhere under any climate. -The isotope-temperature relationship is disputable and especially for the coastal regions in Antarctica. Accuracy needs to be evaluated for the 1.75°C temperature change which is proposed. -On the origin of the dust and the link with wind. It would have been pertinent to observe the dust from filter by optical or electron microscope to evaluate their size and get some basic elemental composition. The link with katabatic wind which is made from seasonal variation is hard to accept, and for the observed spikes, the dust sources may be also involved. At the most it would be better to use the occurrence of meteorological events leading to dust spikes at MES, instead to use the maximum of wind speed recorded in Scott Base. -The link between MS-ion and polynia could be true. What is disputable is the comparison which is made with record of diatoms (bloom between 1600 and 1850), then two selected records are compared with MES record for the period 1825 and 1870 (fig 8)? This is hard to accept because the non-objective choice for the two marine records, as well as the accuracy of the ice core dating. Minor remarks -About IC and ICPMS measurements: we may wonder about Ca concentrations from their blanks (20 and 6 ppb respectively). It is said to originate from ceramic knife. Indeed, ceramic is very stable, and resistant to IC eluents... Ca record is not used in the paper but we may wonder about possible poor analytical cleanness of water system or lab air quality. -Stable isotopes measurements: two different systems have been used; one is the classical MS, one using the laser spectrometry technique. While the two techniques of measurements have been intercalibrated and finally corrected the shift in excess of deuterium unfortunately drastically corresponds to the change of the techniques (Fig A3)! How to be sure that the shift at $\sim$1600 AD is not an artifact?

Interactive comment on Clim. Past Discuss., 8, 215, 2012.